

SUGGESTED READINGS

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Chapter 11

Factorial Studies



Experiments that introduce more than one qualitative independent variable (factor) are called **factorial studies**. At a minimum, two factors are introduced. You could study the effect of four types of therapy and gender on self-esteem, in the design shown in Box 11.1.

Box 11.1 Design of a Two-Factor Study

	Assertive (J1)	Behavioral (J2)	Cognitive (J3)	Rap (J4)
Males (K1)	J1K1	J2K1	J3K1	J4K1
Females (K2)	J1K2	J2K2	J3K2	J4K2

Note: There are four levels of J (therapy), two levels of K (gender), and eight groups (combined levels of each factor).

Although factorial studies require many participants, depending on the number of levels of each factor, they are time-savers by virtue of the information they yield in a single experiment. We learn not only whether each separate factor has an effect but also whether the effects differ when both factors are combined. Separate or unique effects of each variable are called **main effects**. These refer to the average effects of each

independent variable. When main effects are significant, we generally can make blanket statements about the effectiveness of those variables—statements without qualification. For example, we might be able to conclude that a type of therapy is equally effective for males and females or that males end up with higher levels of self-esteem, regardless of the type of therapy they receive. Combined effects let us know whether the effects of

each variable simply add together, **additive effects**, or whether the effect of one variable depends on the level of the second variable. When the latter occurs, the combined effect is called an *interaction*; this implies that a qualification or stipulation has to be added to the description of effectiveness. For example, suppose we introduce three levels of reinforcements for three tasks of increasing difficulty and measure correct responses. We require three *F* ratios: one to let us know whether there is an overall effect of magnitude of reinforcement on number of correct responses (main effect), one to let us know if there is an overall effect of task difficulty on number of correct responses (main effect), and one to let us know whether the combined effects of both variables differ from the separate effects (interaction). If interaction is significant, the ultimate conclusion we might reach, after performing multiple comparisons, is that the highest magnitude of reinforcement is most effective—but only for the simplest task.

The type of analysis performed on the data depends on the design of the study. We have several alternatives, although we will always rely on three *F* ratios (for a two-factor study) to learn about main and interaction effects. (With more than two factors, the main effects and possible interactions increase in number and complexity.) If participants are independent groups, then a two-way (or three-way, etc.) analysis of variance (ANOVA) is appropriate. Most often, each of the independent variables consists of deliberately selected levels (e.g., particular drugs,

levels of one independent variable) and $K - 1$ (where K = number of rows or levels of the other independent variable), whereas that for interaction equals $(J - 1)(K - 1)$. Degrees of freedom (*df*) for the error term, which is random error, for testing main and interaction effects equals $N - JK$. With a random-effects model, error for testing both main effects is the interaction and its $df = (J - 1)(K - 1)$,

whereas interaction is tested by random variability as the error term and df is $N - (J)(K)$. If a mixed-effects model applies, the error term for the fixed-effect variable is interaction and has $(J - 1)(K - 1)$ df , that for the random-effect variable is random variability and has $df = N - JK$ for error, and interaction is tested by random variability and has $df = N - JK$. These are summarized in Box 11.2.

Box 11.2 Appropriate Error Terms for Two-Factor Studies

Model	Component	<i>df</i>	Error
Fixed effect	Main effect (<i>J</i>)	$J - 1$	Random error
	Main effect (<i>K</i>)	$K - 1$	Random error
	Interaction	$(J - 1)(K - 1)$	Random error
	Random error	$N - JK$	Random error
Random effect	Main effect (<i>J</i>)	$J - 1$	Interaction
	Main effect (<i>K</i>)	$K - 1$	Interaction
	Interaction	$(J - 1)(K - 1)$	Random error
	Random error	$N - JK$	Random error
Mixed effect	Fixed main (<i>J</i>)	$J - 1$	Interaction
	Random main (<i>K</i>)	$K - 1$	Random error
	Interaction	$(J - 1)(K - 1)$	Random error
	Random error	$N - JK$	Random error

In addition, each model has its own statistical assumptions. The assumptions for the fixed-effects model are the same as for one-way classification studies (see Chapter 10). For random-effects studies, because results generalize to the population, the assumption of normal distributions of scores for the different levels of each variable and combined levels is crucial, as is the assumption of homogeneity

of variances for each treatment level and their combinations (separate cells). And the same assumptions apply to the mixed-effects studies. Commonly used statistical packages include tests of each, and results should be reported. ANOVA, however, is a fairly robust test, provided that *ns* are reasonably large and approximately equal within each of the cells (combined levels).

In many instances, factorial studies employ the same participants in more than one condition, and some ANOVA for repeated measures is appropriate for determining main and interaction effects. In rare instances, the same participants serve in all conditions of the study. Most frequently, however, different groups of individuals (one of the independent variables) participate in all levels of the second independent variable (or more for a higher order factorial study). In this instance, a two-way ANOVA with repeated measures on one factor is appropriate. Again, the analysis will let you know whether there is a main effect of the different groups (between-group), a main effect of the different treatments or levels (within-group because all participants take part at each treatment level), and interaction between the two effects. When you want to check on validity of results, df for each effect depend on whether the circularity assumption has been met (see Chapter 10). The assumption does not apply to the variable that consists of separate groups. Here, we assume that variances for the separate groups are homogeneous. Therefore, $df =$ number of groups $(K - 1)$, for the group factor, and number of participants $(S) - K$, for the error term. The circularity assumption does apply to the within-group factor; the different levels of treatment to which all participants were exposed. For each separate group, the variances of differences between all pairs of treatment scores are assumed to be equal. And if we were to get average values for each level of treatment by combining the group scores (called

pooling), the variances of all possible differences between pairs of the pooled, average values also are assumed to be equal. If the assumptions are met, df for the within-group factor and its error term equal $J - 1$ and $(J - 1)(S - K)$, and interaction effects of both factors has $df = (J - 1)(K - 1)$ and $(J - 1)(S - K)$ for error. The assumptions, however, seldom are met, and you should be alerted to some statement about adjusting df to guard against a Type I error. With no indication and F ratios declared significant at $p < .05$, you can evaluate the conclusions by comparing the F ratios against tabled values associated with $df = [(1/J - 1) \times (J - 1) = 1]$ (for the numerator) and $[(1/J - 1) \times (J - 1)(S - K) = S - K]$ (error) for the within-group, repeated measures, factor and $[(1/J - 1) \times (J - 1)(K - 1) = K - 1]$ (for the numerator) and $S - K$ (error) for interaction. If F ratios are no longer significant, a Type I error looms as a possibility.

From the point of view of design, you need assurance that participants were randomly assigned to the various groups. Of course, this is not possible if one of the variables is an individual characteristic (e.g., gender, personality disorder); these individuals should be randomly selected and then randomly assigned to levels of the other variable. Because attrition almost always occurs—especially if testing occurs on more than one occasion—you need assurance that the loss is not selective. And, as with all studies, there should be guards against experimenter effects as well as some assessment of the independent variables—that is, you want assurance that manipulation of the

factor is some progressive event, such as trials or time, there should be an indication that levels of that factor were presented in counterbalanced order. Caution factors for a factorial study are presented in Box 11.3.

We begin here by evaluating a separate groups factorial study together, and then we'll do the same for a mixed-design study with repeated measures on one factor.

Box 11.3 Caution Factors for Factorial Studies

Between Groups

- Participants were randomly selected or randomly assigned to various groups.
- Administered tests were reliable and valid.
- Multiple tests were presented in counterbalanced order.
- Test administrator was unaware of research hypotheses.
- Attrition did not result in a selective loss.
- Control participants could not learn about experimental treatment.
- Statistical assumptions of ANOVA were met.
- Degrees of freedom (df) are correct.
- Correct error terms were used.

Repeated Measures

- See all of the previously listed factors in the Between Groups section.
- Levels of treatment were presented in counterbalanced order.
- Circularity assumption was tested.
- Degrees of freedom (df) were adjusted if necessary.

STUDY EXAMPLE 11.1: "WHEN AN ADVERSARY IS CAUGHT TELLING THE TRUTH: RECIPROCAL COOPERATION VERSUS SELF-INTEREST IN DISTRIBUTIVE BARGAINING"

This experiment represents the simplest extension of a between-groups design, a 2×2 design. It introduces two levels of each variable. In the present study, participants in a bargaining situation acted either as buyer or seller of an old car. This was the first independent variable.

They bargained with confederates who either were truthful with them about their alternatives (price offered by a potential buyer or their need for parts of the car to rebuild another one) or did not disclose their alternatives. This was the second independent variable. The experimenters were interested in the extent to which the participants would bargain honestly in each of the four conditions. A footnote thanks the nine confederates and another individual for coding and entering the data.

The Study

Paese, P. W., & Gilin, D. A. (2000). When an adversary is caught telling the truth: Reciprocal cooperation versus self-interest in distributive bargaining. *Personality and Social Psychology Bulletin*, 26(2), 79–90. Copyright © 2000 by Sage.

Consider a hypothetical negotiation between a buyer and a seller, with yourself in the seller role. The year is 1970, and the negotiation involves the possible sale of a 15-year-old car. The only issue to be negotiated is that of price. . . . You and the buyer do not know each other, and you do not expect to interact in the future. Both of you have a firm alternative to which you can turn in the event of impasse; in your case, a previous buyer has made you a standing offer of \$300 for the car. As luck would have it, you stumbled on some valuable inside information. . . . prior to the negotiation. . . . The buyer intends to disassemble your car and use its parts to rebuild another car, and . . . the buyer's only alternative is to purchase these parts elsewhere for \$600. . . . There is a \$300 surplus between your alternative and that of the buyer. You also happen to know that the buyer (a) has no knowledge of your alternative and (b) is completely unaware of the fact that you have an information advantage.

Under these circumstances, any settlement you negotiate will depend greatly on how you portray your own alternative to the buyer. . . . Honestly portraying your alternative is likely to result in a settlement somewhere in the middle of the \$300 to \$600 bargaining zone, whereas exaggerating its value (e.g., "I have another buyer who's willing to pay \$500") is likely to give you a much more favorable settlement. . . . Assume that you are fully aware of this going into the negotiation.

Given that you already know the buyer's alternative, his or her portrayal of it will have very little effect on the final settlement—you could call the buyer's bluff if he or she tries to exaggerate. Of course, the buyer does not realize this so you suspect that he or she might try this strategy. Suppose, however, that instead of exaggerating, the buyer truthfully discloses his or her alternative. You must now decide how to portray your own alternative, knowing full well that the buyer has done so honestly. What would you do under these circumstances? . . . You could

safely exaggerate and claim most of the surplus or you could portray honestly and give much of the surplus away; either way, the buyer won't know the difference. . . . On the other hand, the buyer has clearly made an honest disclosure, undoubtedly in the hope that you will do the same. Perhaps you should reciprocate this cooperative gesture. Note that if you do reciprocate, your cooperation will benefit the buyer at a cost to yourself. Not only would you be giving him or her a portion of the surplus [but] the buyer would not even realize, much less appreciate, that you have made a concession on his or her behalf. . . .

To what extent will bargainers cooperate under these circumstances? . . . We conducted an experiment in which participants were put in the exact situation [previously] described. . . . (actually, half of our participants were put in this exact situation; the other half were put in a mirror-image situation in which the participant's role was changed from seller to buyer). . . .

Reciprocity and Self-Interest

The reciprocity rule is a social norm that pervades human culture. . . . In its simplest form, the reciprocity rule merely states that "we should try to repay, in kind, what another person has provided us." . . . One consequence of this rule in bargaining contexts is that a concession by one party will usually cause the other party to concede in return. . . .

. . . Results suggest that a discernibly cooperative move by one's opponent . . . is likely to be reciprocated to at least some degree. (Note that a cooperative move . . . is clearly discernible only when one knows the limit of one's opponent.)

. . . The opponent in our study was ignorant of the participant's limit, and participants knew this. . . . Because of the information asymmetry in our experiment, participants could give the appearance of reciprocating without really doing so. For example, by revealing their own alternative and exaggerating its value, our participants could lead the buyer to believe that they had made a reciprocal disclosure when in fact they had not. Thus . . . one might expect self-interest to override any reciprocal motive to cooperate in the present experiment.

Indeed, recent evidence would seem to support this latter expectation. . . .

. . . The key finding in . . . research [involving the prisoner's dilemma] is that, in comparison to conditions in which the opponent's choice was unknown, the frequency of cooperation did not increase when participants knew their opponent had already cooperated. In fact, just the opposite occurred. . . . Results suggest that bargainers in the seller role are unlikely to reciprocate the buyer's cooperation.

Additional . . . experimenters led participants to believe they were playing a one-shot prisoner's dilemma with an anonymous opponent and varied . . . whether participants knew their opponent's choice prior to making their own decision to

cooperate or defect. . . . The frequency of cooperation did not increase when participants knew the other party had already cooperated. These results cast further doubt on the possibility that bargainers will cooperate under the circumstances of interest here.

At this point, it should be noted that the experimental contexts . . . were distinctly nonsocial. . . . The opponent was not physically present, and there was no communication between players. . . . Under these conditions, the social norm of reciprocity may be unlikely to be activated.

In contrast, participants in the present research negotiated face-to-face with a scripted opponent (a confederate) posing as a participant. In half of these negotiations, the confederate initiated the discussion by making an honest disclosure about his or her alternative (in the other half, the confederate initiated the discussion but did not make an honest disclosure). . . . Our participants actually observed the other party cooperating and then proceeded to bargain with him or her. Under these conditions, the reciprocity rule may be more likely to be activated.

. . . We expected that our confederate's unsolicited cooperation would create at least some feeling among participants that they should cooperate in return. However, given that our participants could give the appearance of cooperating, we were particularly interested in the extent to which they would cooperate genuinely versus merely pretend to do so.

. . . Each participant also was given an incentive to maximize his or her individual outcome. Consequently, our participants could not cooperate genuinely without potentially endangering their own self-interest. Under these circumstances, we expected that participants might attempt to resolve this conflict (between their own self-interest and the obligation to reciprocate) by feigning cooperation. . . . Thus, we attempted to measure both genuine and feigned cooperation in the present study.

1. What was the rationale for the study?

In bargaining situations, sellers and buyers have alternatives. If the seller is aware of the buyer's alternative, but the buyer is unaware of the seller's knowledge, the seller can cooperate with the buyer by presenting an honest alternative and thereby lose money, or the seller can present an exaggerated (dishonest) alternative and lose nothing. In that case, the seller would be acting out of self-interest.

In prior research, it was found that knowledgeable participants (akin to sellers) are not likely to cooperate with an opponent (akin to buyers). The situations, however, were nonsocial; bargainers did not have face-to-face contact, leading to less likelihood of cooperation. In the present study, participants, in the role of buyer or seller, bargained with confederates who disclosed or did not disclose their alternatives. It was anticipated that because participants observed confederates cooperating

(disclosing alternatives), they would feel obligated to reciprocate cooperation. But they also had incentives to maximize their outcome (increased self-interest) and could feign cooperation to resolve the conflict between reciprocity and self-interest.

Hypotheses

The preceding considerations give rise to two hypotheses. . . . We expected to find support for either or both of these hypotheses. The first of these is formalized as follows:

Hypothesis 1: An unambiguously cooperative move at the outset of a distributive negotiation will cause the opposing bargainer to truthfully reveal more information, exaggerate his or her alternative less, and make a less demanding offer than he or she would in the absence of such a move.

We refer to this as the genuine reciprocation hypothesis. . . .

The second hypothesis, referred to here as the feigned cooperation hypothesis, has the following form:

Hypothesis 2: An unambiguously cooperative move at the outset of a distributive negotiation will cause the opposing negotiator to disingenuously reveal more information while exaggerating just as much and making offers just as demanding as he or she otherwise would.

. . . To the extent that our confederate's disclosure activates a secondary motive to cooperate in return, it seems likely that bargainers will want to appear cooperative while pursuing their self-interest.

2. What was the purpose of the study?

On the basis of past research, two hypotheses were tested. The first hypothesis stated that genuine cooperation would be evident when the opponent cooperated at the outset of bargaining. The second hypothesis stated that feigned cooperation, because of self-interest, would be evident when the opponent cooperated at the outset.

Method

Design

Participants assumed the role of either buyer or seller in a negotiation simulation involving the sale of a used car. The roles were written such that participants

► (This aspect of instructions to confederates is very sticky. On one hand, the random assignment of them to all conditions rules out differential effects of the confederates. On the other hand, having each serve in all conditions makes it more likely that, contrary to their instructions, some hypothesis might be formed about the purpose of the study. And the more sessions each takes part in, the more likely it becomes.)

Prior to any actual data collection, the confederates rehearsed each script by playing the role of participant for one another. Live practice sessions also were conducted. . . .

3. Describe the participants and confederates.

There were 44 female and 33 male undergraduates who participated for course credit and a chance to earn extra money. They were about 26.4 years old, and 86% worked full- or part-time. Confederates, also undergraduates, were told that the study was exploratory with no particular hypotheses being tested. They were instructed to focus on their scripts and not try to formulate their own hypotheses. All were thoroughly trained to deliver their script lines in the same tone of voice and participated in live practice sessions.

Participant Roles

. . . Participants in both roles were told to assume that the time was 1970 so that the negotiation would not be influenced by current car prices.

Buyer role. The buyer description put participants in the role of an amateur mechanic who is interested in buying a 1955 Street Streaker for purposes of removing some of its parts to rebuild another car. The buyer is told that he or she could buy these parts new for \$600 and that this is his or her only alternative to settlement. Inside information about the seller's no-agreement alternative was written into the buyer role. . . .

[Precise instructions inform the buyer via a friend that someone, the seller, was offered \$300 for just such a car.]

Seller role. The seller description put participants in the role of a parent who is interested in selling a 1955 Street Streaker that belongs to his or her son who is overseas in the military. The seller is told that a local used car dealer has offered him or her \$300 for the car and that this is his or her only alternative to settlement. Inside information as to the buyer's no-agreement alternative was written into the seller's role. . . .

had a clear alternative of their own as well as accurate information about the other party's no-agreement alternative. A confederate posing as a participant assumed the opposite role and followed a detailed script. The script was varied such that the confederate either did or did not disclose his or her no-agreement alternative at the outset of the negotiation. Thus, the design was a 2 (no disclosure vs. disclosure) \times 2 (buyer vs. seller) between-participants factorial.

Participants

Seventy-seven undergraduates (44 females and 33 males) from courses in business and psychology participated in the experiment for course credit and the chance to earn some money. The mean age of participants was 26.4 years ($SD = 7.6$). Of the participants, 86% were working either full- or part-time. There were 18 to 21 participants in each of the four conditions.

► (Note that research participation is a common requirement in many universities, provided that students can choose the experiment[s] of their choice. Also, the fact that 86% work full- or part-time explains the large mean age of these undergraduates and also suggests that they would have little chance to talk to one another about the experiment. Finally, although we know that the four cell sizes ranged from 18 to 21, it would have been helpful to know exact numbers and numbers of males and females in each condition. We know that two of the cells had 18 and 21. The remaining 38 participants could have been divided evenly, or there could have been 18 in one and 20 in the other. But nothing is said about attempts to equalize males and females in the cells. If there was an even split of females—11 in each—this could have resulted in a range of 7 to 10 males in the cells, which may or may not make a difference. We need to be on the lookout for a test to assure no differences between the sexes in the results.)

Confederates

Five female and four male undergraduates served as confederates . . . required to learn four scripts, one for each cell of the design. In training the confederates, great emphasis was placed on delivering the scripted lines the same way in the no-disclosure versus disclosure conditions. That is, they were instructed to use the same tone and inflection in both conditions, even though some of the words and phrases differed. They were also told that (a) the research was exploratory, (b) there were no specific research hypotheses, and (c) they should avoid forming any hypotheses of their own and instead concentrate on delivering the scripted lines the same way each time.

[Written instructions inform the seller, via a friend, that an expected caller, the buyer, just wants the parts of the car, which can be purchased new for \$600.]

Confederate Scripts

... There was one script for each of the four conditions. Each script contained an opening remark followed by seven blocks of lines. The confederate delivered the first block of lines at the very outset of the negotiation, before any offers were made; this block was varied such that the confederate either did or did not disclose his or her alternative. Each subsequent block was contingent in the sense that the script had two tracks, one for responding to stingy offers from the participant and the other for responding to generous offers.... It should be noted that the confederate tried to elicit a monetary offer from the participant after each block. That is, if the participant's response to a given block did not include a monetary offer, the confederate asked the participant to suggest a price before proceeding to the next block.

4. What were the buyer and seller roles of the participants?

All participants assumed that they were negotiating the price of a car in 1970. Buyers acted as mechanics trying to buy the car for its parts. If negotiations failed, they could buy new parts for \$600. The buyer also knew that the seller had been offered \$300 for the car. Sellers acted as a parent trying to sell a son's car. If negotiations failed, they could get \$300 for the car. The sellers also knew that the buyer wanted to buy the car for its parts, which would cost \$600 new.

5. What were the confederate roles?

If paired with a buyer, the confederate was a seller; if paired with a seller, the confederate was a buyer. In the disclosure condition, the seller revealed the offer of \$300 for the car, whereas the buyer revealed that the cost of new parts would be \$600. In the nondisclosure situations, they revealed nothing.

Procedure

Two to four participants were run in each experimental session, and each session was randomly assigned to one of the four conditions. At the outset of each session, participants were told that they would be required to assume the role of either buyer or seller and that they would attempt to negotiate the sale of a used car with another participant in the opposite role.

► (Note that with four participants per session, there would have been a minimum of 19 sessions and probably more because the range was two to four. The variations are likely due to a population of working students who are not readily available for testing. It would have been useful to know when sessions were conducted and over how long a period of time. Random assignments will tend to reduce the effects of different times, but the different times could increase variability and account for unexpected nonsignificant results.)

Participants were told that two \$5 prizes would be awarded, one for the buyer who negotiated the lowest price and one for the seller who negotiated the highest price.... Participants always competed against either two or three others for a \$5 prize. Informal observation indicated that this was a strong incentive. Not only did participants want to earn the money [but] they also sought the distinction of being the "best."

... A single sheet containing the role description was handed to each participant while a written copy of the script was given to each confederate for reference. The two sheets were identically formatted so as not to arouse suspicion.

► (Note that this is a good feature of the design. Presumably, one of the authors greeted the participants and could have unintentionally influenced them if instructions were verbal. Using written descriptions greatly reduced this risk.)

Prior to assigning negotiation pairs, the experimenter grouped the participants by role and directed each group to its own private room. She entered the participants' room and verified that they knew their own alternative and that of the other party by quizzing them and restating these values aloud.... In addition to this knowledge, it was necessary that participants believe the other party was ignorant of the participant's alternative....

► (She gave a brief speech assuring the participants that the other party did not know their alternatives. Note that questions to the participants and her speech served as manipulation checks. If the participants did not fully understand their alternative and were not convinced that the opponents did not know their alternatives, effectively, there would be no independent variables operating.)

The experimenter then went into the confederates' room to give the appearance that she was instructing them as well. After returning from this room, she called everyone back into the main room where she paired each participant with a confederate and assigned each pair to a room.

Participants were given up to 20 minutes to complete the negotiation. They were told that they should keep a running record of every monetary offer given and received during the negotiation as well as comments made by each party. Separate sheets were given to the buyer and seller...

Immediately after the last negotiation was finished, everyone was called back into the main room and a postexperimental questionnaire was administered.... After all questionnaires were completed, the experimenter determined which buyer and seller negotiated the lowest and highest price, respectively, and paid each \$5. The confederates walked away from the room... but returned discretely and completed another questionnaire regarding the participant's responses in the negotiation.

6. What was the general procedure?

Two to four participants were run per session and were informed that they would act as buyer or seller of a used car and that \$5 would be awarded to the best negotiators. Roles were randomly assigned to each session. Participants and confederates initially were in separate rooms, after each had been handed written descriptions of the role or script (confederate). After participants were assured, by the experimenter, that the opponent did not know their alternative, the pairs were assigned to separate rooms to negotiate for up to 20 minutes. Participants and confederates were reassembled, each filled out questionnaires, and awards were given to lowest buyer and highest seller prices.

7. What were good aspects of the design?

Each session's participants were randomly assigned to one of the four conditions, confederates were trained and served in all conditions, and the experimenter had minimal contact with participants.

8. What were questionable aspects of the design?

Because confederates served in all conditions, they may have caught on to the purpose of the study. And, there is no description of distribution of the participants in all conditions by gender. If there is a gender difference in negotiating, it could affect results.

Measures

Demandingness. Of particular interest here were the price offers made by participants in response to each block of the script as well as the final settlement

price. Given that the buyer and seller limits were \$300 and \$600, respectively, a buyer's offer of, say, \$250 for the car would be numerically equivalent to a seller's offer of \$650 (i.e., these offers are equidistant from \$450, the midpoint of the bargaining zone)... Each offer was converted to an index of demandingness (D)... by subtracting each buyer offer (B) from \$450 ($D = 450 - B$) and subtracting \$450 from each seller offer (S) ($D = S - 450$)... The larger the index, the more demanding the participant's offer.

Exaggeration. In every negotiation, the confederate asked the participant to disclose the value of his or her alternative (i.e., "How much were they asking?" or "How much did they offer you?")... If the participant gave a numerical response to this question, an index of exaggeration (E) was computed for that participant. Specifically, each buyer's response (B) was subtracted from the true value of the buyer's alternative ($E = 600 - B$), whereas the true value of the seller's alternative was subtracted from the seller's response (S) ($E = S - 300$)... The more a participant exaggerates, the larger the E index.

Participant questionnaire.... One item [asked] them to rate how well they thought they had negotiated compared to others in the same role and one item [asked] them to indicate whether their counterpart had negotiated effectively and why they thought so... And two items [pertained] to the participant's previous negotiation experience. Of these latter items, one asked them to describe any on-the-job bargaining experience and the other asked them to describe their negotiation experience in contexts other than work.

Confederate Questionnaire 1... asked them to record demographic information about the participant, the duration of the negotiation, and ratings of the extent to which (a) they were able to follow the script without needing to ad lib, (b) the participant appeared to suspect their confederate status, and (c) the participant said things during the negotiation that deviate[d] from his or her role description....

Confederate Questionnaire 2... required them to refer to, and elaborate on, ... the concurrent record they had kept of the participant's responses....

Truthfulness ratings. We were interested in the extent to which participants made truthful statements, particularly about their alternative, as well as the overall amount of lying they exhibited. Therefore, the second author and an assistant independently read the participants' statements (as transcribed by the confederates) and rated the extent to which they (a) disclosed truthful information about their alternative and (b) told lies in general (both 7-point scales where 1 = not at all and 7 = to a great extent). Raters were blind to condition. Interrater reliabilities were .86 and .81 for items a and b, respectively. A composite rating was formed for each item... referred to as the truth and lie composites.

seller role, demanded an extremely high price in each of the first four bargaining rounds. . . . Each of these prices resulted in a *D* index that was more than twice as large as the next largest *D* index in that round. This participant was therefore excluded from the analyses of *D1* to *D4* (this participant was in the no-disclosure condition . . .).

► (Note that a check for at least one of the assumptions of ANOVA was made. It is assumed that variances probably were homogeneous among the four cells. Again, the test is fairly robust with respect to homogeneity provided that cell numbers are reasonably large, which they are in this case, and are relatively alike, which they are. Finally, there is good justification for eliminating the outlier from the analyses.)

One participant refused to state a price in the first round of bargaining, leaving a total of 75 participants for the analysis of *D1*. By the second round, this participant no longer refused to state a price. However, two participants settled immediately after the first round, leaving 74 participants for the analysis of *D2*. Ten additional participants settled right after the second round, so there were only 51 participants for the *D4* analysis. Last, 5 participants failed to reach a settlement, leaving 72 participants for the analysis of *Dfinal*.

The ANOVAs on *D1*, *D2*, and *D3* each revealed a main effect for the disclosure manipulation (see Table 11.1). . . . Participants in the disclosure condition made less

Table 11.1 ANOVA Summary Table for Dependent Variables

Variable	Source of Variation	df	F	η^2
<i>D1</i>	Disclosure	1	13.02**	.16
	Role	1	1.06	.02
	Disclosure × Role	1	2.39	.03
	Error	71		
<i>D2</i>	Disclosure	1	9.91**	.12
	Role	1	.75	.01
	Disclosure × Role	1	1.18	.02
	Error	70		
<i>D3</i>	Disclosure	1	5.41*	.08
	Role	1	2.25	.04
	Disclosure × Role	1	2.97	.05
	Error	60		

(Continued)

Experience ratings. The two raters also read each participant's description of his or her bargaining experience and rated the participant accordingly (on a 7-point scale where 1 = no experience and 7 = great deal of experience). The interrater reliability for this item was .93, and a composite rating, referred to here as experience, was formed for this item as well.

9. What measures (dependent and other variables) were used?

Demandingness was an index of the difference between \$450 and the buyer's offer (450 - B) and the seller's price and \$450 (S - 450). The higher the index, the more demanding the participant. Exaggeration was an index of the difference between a buyer's quoted alternative and the actual quoted price (\$600 - B) and between the seller's quoted offer and the actual offer (S - \$300). The larger the index, the greater the exaggeration.

Participants' questionnaire contained several questions, including their past experience in negotiating on-the-job and in other situations. There were two questionnaires filled out by the confederate. One focused on the extent to which the participant appeared to believe the role played by the confederate. The second focused on elaborating notes they made about the participant response during the session.

Truthfulness ratings on a 7-point scale of participant's statements (noted by confederate) were made independently by the experimenter and a "naive" assisstant and combined into a composite truth and lie score. Experience ratings were made (also independently) of the amount of prior experience in negotiating had by each participant.

10. What additional measure might have been made?

Each confederate might have been asked, at the end of the study, to state the probable hypothesis being tested, to ensure that each had not caught on to the purpose of the study.

Results

Demandingness

Our first set of results concerns the price offers made by participants in response to each scripted block as well as the final settlement price. A 2 (no disclosure vs. disclosure) × 2 (buyer vs. seller role) ANOVA was conducted on each *D* index. . . . Each *D* index was plotted and found to be normally distributed, with the exception of one outlier on indexes *D1* to *D4*. This participant, assigned to the

note that if there had been no loss of participants from one round to the other, the data could have been analyzed by a $2 \times 2 \times 5$ ANOVA with repeated measures on the last factor, representing the rounds and final price. But the analysis would have been more complex, with unequal ns in the cells, and the status of the circularity assumption would be questionable.)

There was no role effect in Rounds 1 to 4, and there was not a Disclosure \times Role interaction on any of the D indexes (see Table 11.1). There was, however, a significant role effect on the final price; participants in the role of buyer ($M = 66.5$) demanded more than those in the seller role ($M = 31.4$). Although unanticipated, this finding is consistent with previous laboratory experiments in which buyers have been found to outperform sellers. . . . Indeed, there was a role main effect on the duration of the negotiation, $F(1, 72) = 5.63, p < .05, \eta^2 = .07$, [η^2 is equivalent to a coefficient of determination and indicates the percentage of variability in, in this case, duration of negotiations associated with role of the participants] with participants in the buyer role ($M = 10.3$) negotiating longer than those in the seller role ($M = 8$ minutes). This suggests that buyers may indeed have held out longer near the end of the negotiations. There was no effect for disclosure or the Disclosure \times Role interaction on duration.

11. What model was assumed in performing the ANOVA (i.e., what type of independent variables were introduced)?

Both variables were deliberately manipulated; therefore, a fixed-effect model would be appropriate.

12. Analyses were performed to determine whether disclosure of alternatives by confederates had any effect on subsequent offers. Degrees of freedom (df) for each bidding round successively decreased. Did these represent selective losses of participants?

In a way, yes. Those situations in which the confederate disclosed his or her alternative (offer of \$300 for the car or \$600 to buy the parts new) were more likely to reach an early settlement on the sale or purchase of the car.

13. What were the major findings regarding demandingness?

For Rounds 1 to 3, there was a significant main effect due to disclosure: Those participants paired with a confederate who disclosed a selling offer or cost of new parts were less demanding; that is, they were more likely to offer to sell the car at

Table 11.1 (Continued)

Variable	Source of Variation	df	F	η^2
D4	Disclosure	1	2.43	.05
	Role	1	1.12	.02
	Disclosure \times Role	1	1.96	.04
	Error	47		
Dfinal	Disclosure	1	9.04**	.12
	Role	1	14.76**	.18
	Disclosure \times Role	1	2.53	.04
	Error	68		
E index	Disclosure	1	11.74**	.23
	Role	1	.01	.00
	Disclosure \times Role	1	7.70**	.16
	Error	40		
Truth	Disclosure	1	9.65**	.12
	Role	1	.41	.01
	Disclosure \times Role	1	2.28	.03
	Error	68		

Note: ANOVA = analysis of variance; D1 = the index of demandingness for the participant's first offer; D2 = the index of demandingness for the participant's second offer; D3 = the index of demandingness for the participant's third offer; D4 = the index of demandingness for the participant's fourth offer; Dfinal = the index for the final settlement price; E index = the exaggeration index.

* $p < .05$; ** $p < .01$.

demanding offers than participants in the no-disclosure condition in Rounds 1 through 4. This effect was largest in Round 1 and decreased over rounds. By [Round 4], the difference was smaller and nonsignificant. . . . This lack of effect in Round 4 . . . was probably nonrandom attrition; that is, most of the participants who settled early were in the disclosure condition. . . . The disclosure manipulation also had a significant effect on the final settlement price. Again, participants in the disclosure condition made lesser demands than those in the no-disclosure condition. . . . Thus, it appears that the confederate's disclosure elicited genuine, as opposed to feigned, cooperation in the form of less demanding offers and settlement terms.

► (Note that the report of loss of participants from each round explains why the df for the error terms change in each consecutive round and lets you determine that they all are correct. But F ratios cannot be checked because means are not reported in the table [they are shown in a figure but were hard to read]. Also

a price well below \$600 or to buy the car well below \$300 than were participants paired with a confederate who did not disclose their alternative.

14. Did the role of the participant make any difference with respect to disclosure by the confederate?

No, none of the Disclosure \times Role interactions were significant.

15. Did the role of the participant make any overall difference?

Yes, only in terms of final bid. Those who were sellers demanded less (offered to sell at prices closer to the midpoint bargaining range of \$450) than did buyers (offered to buy at prices further from the midpoint range of \$450). If we use the demand formulas with rounded-off means, buyers offered (demanded) \$450 – \$66 = \$384 for the car, which resulted in a profit for the seller of only \$84. But sellers offered to sell their cars for \$31 + \$450 = \$481, which saved the buyer \$600 – \$481 = \$119.

16. What other evidence suggested that buyers were more demanding?

They spent a longer time negotiating than did sellers.

Exaggeration/Truthfulness

Participants' responses to the confederate's question, "How much were they asking?" or "How much did they offer you?" were analyzed for evidence of feigned cooperation. Recall that prior to asking this question, confederates in the disclosure condition had already revealed their alternative truthfully. Thus, if participants in this condition wanted to feign cooperation, they could easily do so by describing their own alternative in monetary terms (thereby returning the confederate's favor) while still exaggerating its true value.

Six participants refused to answer this question (4 of these were in the no-disclosure condition). Of those who did answer, 44 described their alternative in monetary terms. The majority (27, or 61%) of these participants were in the disclosure condition, but this difference was not significant. . . .

A 2 (no disclosure vs. disclosure) \times 2 (buyer vs. seller) ANOVA on the E index revealed a main effect for disclosure (see Table 11.1). Participants in the disclosure condition did exaggerate the value of their alternative ($M = 163.9$) but not nearly as much as did participants in the no-disclosure condition ($M = 337.6$). . . . there was a significant Disclosure \times Role interaction. . . . Further analysis indicated that the

disclosure main effect was much larger for sellers than for buyers and that this was the source of the interaction. Specifically, the mean E index for sellers was 94.2 in the disclosure condition and 405.5 in the no-disclosure condition, whereas for buyers these means were 228.6 and 261.3.

► (Note the meaning of this interaction. Buyers in the disclosure position claimed to have been quoted \$600 – \$229 = \$371 for new parts, whereas those in the no-disclosure position claimed to have been quoted \$600 – \$261 = \$339 for new parts. Sellers in the disclosure position claimed to have been offered \$300 + \$94 = \$394 and in the no-disclosure position to have been offered \$300 + \$405 = \$706. These results are rather interesting because in the end, the buyers, not the sellers, were more demanding.)

. . . . Eight participants disclosed the true value of their alternative . . . and all 8 of these participants were in the disclosure condition. Moreover, even when these 8 participants were removed, there was still a main effect for disclosure on the E index. . . .

In view of the fact that some participants in the disclosure condition cooperated by answering the confederate's question about other options yet still exaggerated the true value of their alternative, we can infer that these participants feigned cooperation to some extent. . . . However, these participants were significantly more truthful than those in the no-disclosure condition. . . . None were completely truthful in the no-disclosure condition. Thus, the present data indicate that . . . in general the confederate's disclosure caused participants to be more genuinely cooperative than they would have otherwise been.

To further examine the truthfulness of participants' responses, 2 \times 2 ANOVAs were conducted on the truth and lie composite ratings as well as on the questionnaire item in which confederates rated the extent to which the participants said things during the negotiation that deviated from their role description. Only one significant effect emerged in these analyses: a main effect for disclosure on the truth composite (see Table 11.1; there was missing data for 5 participants in this analysis. . . .). Participants in the disclosure condition ($M = 2.89$) revealed more truthful information about their alternative than did those in the no-disclosure condition ($M = 1.97$). . . . (the correlation between the truth composite and the E index was $-.61$, $n = 44$, $p < .001$).

17. What were the major findings regarding exaggeration and truthfulness of participants?

Participants who were asked about asking price for new parts (of buyers) or offers for the car (of sellers) either refused to answer, gave truthful answers, or

feigned cooperation by exaggerating the quotes (majority). Of the exaggerators, those in the disclosure condition were more truthful (exaggerated less) than those in the no-disclosure condition. Moreover, the interaction term was significant:

	Disclosure	No-Disclosure
Sellers	94.2	405.5
Buyers	228.6	261.3
	163.9	337.6

The summary table indicates that the difference between means of sellers and buyers to the disclosure condition ($228.6 - 94.2 = 134.4$) was not much lower than that for the no-disclosure condition ($405.5 - 261.3 = 144.2$). But the difference between means for sellers ($405.5 - 94.2 = 311.3$) was much higher than that for buyers ($261.3 - 228.6 = 32.7$). Therefore, the sellers in the no-disclosure condition exaggerated an offered price much more than did the remaining groups. Further analyses revealed that participants in the disclosure group were more truthful about their alternatives than were those in the no-disclosure group.

Further Analyses

We suspected that participants with the least negotiation experience may have been the most susceptible to the disclosure effects [previously] reported. . . . Therefore, we conducted 2×2 [analysis of covariance] ANCOVAs on the *D*, *E*, and truth measures, with the experience composite ($M = 2.75$, $SD = 1.69$) serving as the covariate.

► (In such an analysis, the effect of the unwanted variable is partialled out, and the analysis effectively is on participants as if all had equal experience.)

The covariate accounted for little or no variation, and the analyses otherwise revealed the same effects as before. Thus, negotiation experience did not moderate the effects observed here. We also tested for possible gender and/or confederate effects, and there were no such effects on any of the dependent measures.

► (Note that this is very useful information and answers points brought up earlier with respect to gender differences and the confederates. But we still need to look for any possibility that confederates caught on to the hypotheses being tested.)

When asked if they thought their counterpart had negotiated effectively, 88.3% of the participants responded affirmatively, and this percentage did not differ by condition. In explaining why they thought so, none of the participants gave any indication that the confederate had acted [in a manner that was] unusual, suspicious, or odd in any way. . . . The confederates also indicated that, for the most part, they were able to follow the script without having to ad lib.

► (Note that the question about confederates is answered. On the basis of participant reports, it is unlikely that the confederates caught on to the real purpose of the study.)

18. What did the remaining analyses reveal?

These were very important: in removing additional variables as possible explanations of the results. There was no effect of experience of participants in past negotiations on their performance here. Moreover, gender differences were investigated, and no effect was found. Finally, there was no unwanted effect due to the confederates.

Discussion

In the present research, we examined a distributive bargaining situation in which the participant's counterpart either did or did not make an unambiguously cooperative move at the outset of the negotiation. . . . Given the strength of the reciprocity rule in face-to-face interaction, . . . we expected that participants would feel as though they should cooperate in return, at least to some extent. . . . Because (a) cooperation ran counter to self-interest and (b) participants could get away with pretending to cooperate while still pursuing their own interests, we anticipated only that they would display more seeming cooperation that could in reality be genuine, feigned, or some combination of both.

One possibility we envisioned was that participants in the disclosure condition would reveal a monetary limit significantly more often than those in the no-disclosure condition, yet would exaggerate this limit to the point of making equally small concessions. As it turned out, . . . those in the no-disclosure condition gave a monetary response almost as often as did those in the disclosure condition, and although the latter exaggerated somewhat, they did so to a much lesser extent than did those in the no-disclosure condition. Moreover, participants in the disclosure condition made less demanding offers and settled for significantly less profit. Thus, it appears as though the confederate's unsolicited cooperation caused participants to cooperate genuinely in return.

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We contend that these effects occurred because, at some level, the reciprocity rule was activated. This is only an inference, however, because reciprocal impulses were not measured here. . . .

Alternative Explanations

. . . It is important to consider other explanations. One possibility . . . concerns participants' awareness that the experimenter knew they had an informational advantage. . . . Some participants in the disclosure condition would have used the inside information to its fullest advantage held back because they did not want to be viewed by the experimenter as someone who would use this advantage to exploit an honest opponent. However, . . . any effect of this awareness should have been the same across conditions because the experimenter presumably did not know what was being said behind closed doors. . . . For this reason, it is very unlikely that the effects observed here are due to participants' knowledge of what the experimenter knew.

A second possibility concerns participants' certainty about the value of their opponent's alternative. . . . Going into the negotiation, all participants were fully aware of this information. . . . If . . . uncertainty did arise, it would not have remained equivalent across conditions. This is because once the negotiation began, the value of the other party's alternative was verified in the disclosure condition. . . . Consequently, those in the former condition would have become more certain about this value, whereas those in the latter condition would have remained less certain. To the extent that they were less certain, participants in the no-disclosure condition might have thought there was an outside chance that their opponent's alternative was actually weaker than what they had been told and that more extreme offers were therefore warranted. . . . This could explain why participants' offers and settlement terms were more demanding in the no-disclosure condition. . . . [and] could explain why they were less truthful when the confederate inquired about their alternative.

. . . It should be noted that none of the participants gave any indication—to the experimenter, [to] the confederate, or on the postexperimental questionnaire—that they questioned the veracity of information provided. . . . In fact, most participants appeared to be so caught up in the competition for the money prize that they never thought to question the validity of the information provided. For these reasons, it seems unlikely that differential certainty could have produced the sizeable effects observed here.

Yet another alternative explanation is that our confederate's disclosure, rather than activating a reciprocal motive, created an expectation on the participant's part that the confederate would continue to cooperate in the negotiation. . . . As a result

of this expectation, participants in the disclosure condition may have been less concerned about making a demanding first offer that would guard against any demanding counteroffers the other party might subsequently make. In other words, these participants may have become less defensive, and this reduced defensiveness may have resulted in less demanding offers and a greater willingness to disclose information truthfully. . . . Of the three alternative explanations considered here, this latter possibility is probably the most plausible.

Conclusions and Future Directions

. . . Bargainers in the present experiment became more cooperative, even though there were no incentives for cooperation, when their opponent made an unambiguously cooperative move at the outset of the negotiation. . . . None of our participants knew their opponent. . . . The negotiation was purely distributive, and bargainers were given an incentive to maximize their individual outcomes. Under these conditions, reciprocal cooperation ran counter to participants' economic self-interest. Nevertheless, they reciprocated by making less demanding offers, disclosing more truthful information about their own alternative, and settling for less profit. Thus, it appears as though our confederate's cooperative move may have activated the reciprocity rule (or perhaps an expectation that the confederate would continue to cooperate, as suggested by the alternative explanation discussed earlier).

To the extent that the reciprocity rule was indeed responsible for the effects here . . . self-interest does not appear to dominate bargaining behavior as much as previous research would suggest. Moreover, these results suggest that, in distributive negotiations, an unsolicited cooperative move may invite less exploitation than one would expect.

19. What conclusions did the authors reach?

In this face-to-face negotiating situation, participants who were bargaining with cooperative confederates (disclosure condition) reciprocated by being cooperative: They did not exaggerate their alternatives as much as those in the no-disclosure situation, and they made less demanding offers; they settled for smaller profits even when self-interest (\$5 award and title of best negotiator) was at stake. Their cooperation was due to the confederates' cooperation (reciprocity). Another less preferred explanation is that participants in the disclosure situation became less defensive and cooperated because they expected the confederates to continue to be cooperative.

and, by doing so, seemingly disavow . . . any responsibility for harmful effects resulting from the tease. If, indeed, people are just kidding, then why do high school students list as their primary fear the fear of being teased? . . .

Teasing is difficult to define and study because it is a multifaceted phenomenon—some aspects of which have very positive connotations and some aspects of which have very negative connotations. . . . Because of its positive and negative connotations, teasing can include benign attempts to joke with others and poke good-natured fun or it can be used aggressively to hurt another person's feelings or to ostracize an individual from the group. . . . Depending on the target's current mood and the reactions of others who hear the tease, a particular taunt may be perceived as enjoyable on one occasion but unpleasant on another. . . .

. . . Part of the ambiguity underlying teasing arises from the target's difficulty in understanding the motives of the person initiating the tease. Is the teaser just joking around, or is he or she attempting to ridicule or humiliate? . . .

Teasing can have both positive and negative effects on the instigators and the targets of the tease. On the positive side, teasing may be used to demonstrate camaraderie and to strengthen social bonds with the target of the tease. . . . On the negative side, teasing may be a means of strengthening social bonds with people other than the target by ostracizing the target. . . .

So, when does teasing become aversive? When are teases labeled "good" or "acceptable" teases, and when are they labeled "bad" or "unacceptable" teases? . . . Teasing appears to become aversive when it is perceived to indicate relational devaluation. . . . The relational devaluation may be unintentional on the part of the teaser, but the feelings of embarrassment, identity challenge, and in some instances, exclusion are enough to suggest its presence to the target of the tease. Thus, teasing that implies interpersonal rejection and social exclusion (i.e., bad teasing) is perceived very differently from teasing that promotes camaraderie and social inclusion (i.e., good teasing).

Victims' and Perpetrators' Perceptions of Teasing

If there were no ambiguity in the perceived motives behind teasing, and if teasing never involved relational devaluation, perpetrators and targets of teasing might not be expected to differ in their perceptions of a teasing interaction. However, research on other aversive interactions . . . suggests that victims and perpetrators frequently form very different perceptions of the interaction. Relative to victims, perpetrators minimize the negative impact of their behavior, view their behaviors more benignly, perceive the behavior as motivated by rational motives, and see the consequences of their behavior as more limited in scope. . . .

Yes. This was a well-designed study, and all potential confounds were controlled or shown to be ineffective.

21. To what population do these results generalize?

Results generalize to undergraduate college students, about 26 years old, who are enrolled in psychology and business courses and who work full- or part-time.

STUDY EXAMPLE 11.2: "I WAS ONLY KIDDING!": VICTIMS' AND PERPETRATORS' PERCEPTIONS OF TEASING"

We will review this study together because the design differs from that of the first one, although both are two-factor experiments. The study reviewed previously independent groups of participants who were randomly assigned to the four conditions. The study we are about to review included male and female students who wrote essays on their perceptions of teasing when they were the perpetrators and the victims (within-group factor). Therefore, some of the data were analyzed by 2×2 ANOVAs with repeated measures on the second factor.

The Study

Kowalski, R. M. (2000). "I was only kidding!": Victims' and perpetrators' perceptions of teasing. *Personality and Social Psychology Bulletin*, 26(2), 231–241. Copyright © 2000 by Sage.

► (The author includes a footnote thanking five individuals for helping in data collection.)

In the course of everyday life, people do a lot of mean and unpleasant things to one another. They embarrass each other, betray one another, complain and nag, hurt each other's feelings, make each other feel guilty, and inflict many other sorts of psychological distress. Collectively, behaviors such as these have been termed *aversive interpersonal behaviors*. . . . One of the more enigmatic of these aversive interpersonal behaviors is teasing. . . . Ironically, no matter how threatening a tease is to the recipient, the teaser can always claim that he or she was "only kidding"

The frequency with which people tease and their reactions when teased may depend on the person's early experiences with teasing. . . . People who have had favorable experiences with teasing in the past will likely respond positively to other teasing encounters. On the other hand, people who have been mercilessly teased or who have been adversely affected by teases in the past will respond more negatively.

Personality variables also moderate perceptions of teasing behavior. . . . In addition, because of differences in the ways in which men and women are socialized, gender differences in perceptions of teasing behavior would be expected. . . . Women's teasing tends to be more playful and relationship-enhancing, whereas men's teasing is harsher and more likely to be relationship-demeaning. Because of . . . differential patterns of socialization, men's and women's feelings about teasing someone else or about being the victim of teasing would be expected to differ.

The present study was designed to examine differences in victims' and perpetrators' accounts of teasing episodes. Because of the ethical considerations involved with teasing participants in a laboratory setting, we relied on the use of autobiographical narratives. The efficacy of this methodology has been demonstrated. . . .

In the present study, each participant was assigned to write two autobiographical narratives, one that described an incident in which they were teased by someone else and another in which they perpetrated the tease. . . . It is important to note that each participant assumed both a victim and a perpetrator role. . . . Relative to victims, perpetrators were expected to downplay the adverse effects of their teasing and to see the situation as more humorous. Furthermore, the long-term effects of teasing were hypothesized to be more serious for victims than for perpetrators.

► (Note that there were several ways in which the study could have been performed. Teasing could have been introduced as a variable, with one group as perpetrators and another as victims. However, ethics prevents introducing any variable that might be potentially harmful. The author could have shown films of teasing and being teased and asked participants to describe feelings of the actors or actresses. But the same potential for harm exists. Therefore, the author did the next best thing by having participants describe their feelings when they actually were in both roles.)

1. What was the rationale for the study?

Teasing is among aversive interpersonal behaviors. It can be seen as a joke, a way to establish camaraderie to strengthen a social bond; or it can be seen as aversive, an attempt to ridicule someone to devalue a relationship. The way in which teasing

is perceived depends on the motive of the perpetrator. Perceptions and frequency of teasing depend on past experiences with teasing. Those with positive experiences will respond positively, whereas those with negative experiences will respond unfavorably. Males and females also react differently. Female teases are playful and relationship-enhancing. Male teases are harsher and relationship-demeaning. Thus, feelings about being teased and about teasing are expected to differ.

2. What was the purpose of the study?

This study investigated the perception of teasing by individuals who wrote autobiographic descriptions of themselves both as perpetrators of a tease and as victims of a tease. Perceptions were predicted to be more negative and longer lasting for victims.

Method

Participants

Fifty female and 22 male undergraduate students participated in partial fulfillment of a course research requirement. The mean age of the participants was 22.4 (range 18.44), and more than 96% of the individuals taking part were Caucasian. Students participated in groups ranging in size from 10 to 30. To ensure privacy, participants did not interact with one another at any time during the session.

► (Note that there were more than twice as many females as males. The author has a note indicating that males refused to take part in the study because it would be embarrassing to report on teasing occasions. One then has to wonder whether the males who consented to serve differed from those who did not. For this reason, it is fortunate that each participant served as his or her own control. Moreover, we don't know about where the study took place (e.g., a large room with tables and chairs, an auditorium), especially with as many as 30 participants at a time. Furthermore, noninteraction does not ensure privacy. Unless participants were out of each other's view, there is potential eye contact or mere looking at someone. Finally, number of sessions could have been a minimum of three or a maximum of five.)

3. Who were the participants?

Seventy-two mainly Caucasian undergraduates (50 females and 22 males) took part to fulfill a course requirement. They were about 22 years old but ranged in age from 18 to 44 years. Testing was in groups of 10 to 30.

Procedure

After signing a consent form, each participant was instructed to write two narratives. The order in which the narratives were written was counterbalanced. In one narrative, participants wrote about an event when they were teased by someone else. The instructions were as follows:

Please write a true story from your life about a time that you were teased. Nearly everyone has experienced being teased on more than one occasion; please choose an especially powerful and memorable experience of being teased. Be as thorough and complete as you can, and tell the full story about the instance in which you were teased.

The other narrative focused on an incident in which they teased another individual. The instructions for this narrative resembled those for the victim narrative in every respect except that instead of writing about being teased, they wrote about a time when they were the teaser. Thus, in one story, participants were the victims of a tease, and in the other, the participants were the perpetrators.

► (Note that nothing is said about how instructions were given, verbally or in writing, and, therefore, how counterbalancing was effected.)

After writing each narrative, participants completed a questionnaire examining their perceptions of the experience. To examine participants' views of the positive features of the teasing situation, they were asked to rate how humorous they perceived the situation to be and the degree to which they thought their self-esteem was raised as a function of the teasing event. These questions were answered using 12-point scales with five scale labels (*not at all*, *slightly*, *moderately*, *very*, and *extremely*). Participants also indicated how positively they viewed the other individual and how positively they perceived they were viewed by the other person. Responses were again made using 12-point scales with five scale labels (*not at all positively*, *somewhat positively*, *moderately positively*, *very positively*, and *extremely positively*).

Because of the negative features that some people assign to teasing incidents, a second set of questions examined participants' perceptions of the negative features of the incident recounted. Specifically, participants indicated how negatively they felt about the experience, how negatively they perceived the other person to feel, the degree to which their self-esteem was lowered by the teasing encounter, how annoyed they felt, and how guilty they felt. Participants responded to each of these items using 12-point scales with five scale labels (*not at all to extremely*).

A third set of questions examined participants' prior experience with teasing. They also were asked how frequently they were teased by others and how frequently they teased other people. Twelve-point scales with five scale labels (*not at all to extremely*) were again used.

► (Note that the questionnaire started with a measure of positive reactions and then negative reactions, which might or might not make a difference on the next autobiography. Half the time, they were followed by the essay of a victim (which might exaggerate the negative reactions), and half the time they were followed by the essay of a perpetrator (which might temper the positive reactions because of possible guilt). Or they might make no difference at all. Although presumably, they were filled out twice, we know nothing about time intervening between the end of the first questionnaire and the second essay, during which time some forgetting might occur to lessen any potential carry-over effect.)

4. What was the procedure?

Participants met in groups of 10 to 30 per session and did not interact. Half wrote an autobiographical account of a powerful teasing incident when they were victims and then another essay about an incident when they were teasers. The other half had the reverse procedure. Each account was followed by a questionnaire that measured positive and then negative effects in both roles and the prior teasing history of participants.

5. What were good features of the design?

Good features included counterbalancing of autobiographies, instructions of noninteraction among participants, and use of independent assistants to gather the data.

6. What were questionable aspects of the procedure?

We don't know how much privacy each participant had when writing; we don't know how instructions were delivered; we don't know time limits for writing and for filling out questionnaires, nor the interval of time before beginning the next autobiography; and the positive and negative questionnaires were not counterbalanced.

Results

Content Analyses of Narratives

All narratives, both victim and perpetrator, were content-analyzed along two dimensions: content of the tease and the relationship between the victim and perpetrator. . . . Two raters independently coded the narratives. Interrater reliability for both content and relationship exceeded .80.

► (This was a good feature of the study.)

. . . Seven categories were derived to classify the content of all of the teases: relationships, . . . body parts/appearance, behavior, . . . intelligence, medical conditions, stereotyping/social group, . . . and other. . . . Each narrative was coded as belonging to only one content category.

7. What did preliminary analysis involve?

Scripts of victims and perpetrators were coded for relationship between the teased and teaser and for content of the tease.

8. What were the results?

After ensuring high interrater reliability between coders of the scripts, the author analyzed content into seven categories, including relationships, body parts, behavior, and so on.

Victims. . . . More than 45% of the victim narratives focused on physical appearance. . . . followed by relationships (11.2%) and behavior (11.2%). . . .

. . . A chi-square analysis by gender conducted on the content areas revealed a significant gender difference, $\chi^2(6) = 15.01, p < .02$. For women, by far the largest percentage of teases dealt with body parts/appearance (52%). Men also were teased frequently about their appearance (27.3%) but were teased equally often about their relationships (27.3%).

The relationship between the teasing victim and perpetrator was coded as falling within one of four categories: romantic partners, friends, schoolmates, and relatives. . . . In more than half of the teasing episodes recounted by victims, the victim and perpetrator were schoolmates (55.7%). The teasing episodes recounted by male and female victims did not differ significantly in the relationship between victim and perpetrator, $\chi^2(3) = 1.77, p > .62$. Both male and female victims were teased more frequently by schoolmates.

► (Note that except for romantic partners, presumably of the opposite sex, the remaining categories (friends, schoolmates, and relatives) could be of the same or opposite sex and that impact of a tease may be a function of this factor as well as the relationship per se.)

9. What were the major sources of teasing for recipients?

Most females were teased about their appearance, whereas most males were teased about their appearance (27.3%) and their relationships (27.3%). Moreover, most of the teasers were schoolmates, followed by friends, and these results applied equally to male and female victims.

Perpetrators. The seven categories used to content-analyze the victim narratives also were used for the perpetrator narratives. . . . The largest percentage of teases dealt with behavior (30.6%), followed closely by body parts/appearance (23.6%). The teasing episodes of male and female perpetrators did not differ significantly in the type of tease instigated, $\chi^2(3) = 9.53, p > .14$.

► (Note that df are reported to be 3 instead of 6 [(7-1) (2-1)]. If you check the 9.53 in a chi-square table, you'll see that it has a p of about .14 with 6 df but would be significant for df = 3. Therefore, the reported 3 is a typographical error.)

In general, the narratives in which participants reported teasing another individual involved people with whom the individual was either a friend (34.3%) or a relative (26.9%). When examined by gender, however, this overall pattern changes, $\chi^2(3) = 13.53, p < .003$. For men, the relationship between the victim and perpetrator was usually a friendship (65.0%), followed by schoolmates (20.0%). Women, on the other hand, were more likely to tease relatives (36.2%) than schoolmates (25.5%) or friends (31.9%).

10. What were the results regarding the content of teasing by perpetrators and their relationship to the victims?

For male and female perpetrators, most teases were about behavior and appearance (body parts). Males, however, were more likely to tease a friend, whereas females were more likely to tease relatives.

Linguistic Analysis

A text analysis was conducted on each of the participants' teasing narratives using the Linguistic Inquiry and Word Count [LIWC] strategy. . . . This computer

software analyzes the affective, cognitive, and structural elements of written text on a word-by-word basis. Sixty-one output variables are produced. . . . We were interested in this study in the composite variables of negative emotionality, positive emotionality, self-references, and other references.

The negative emotionality profile is calculated based on words such as *angry*, *ashamed*, and *worthless*. Positive emotionality, on the other hand, reflects words such as *excitement*, *peace*, and *security*. Split-plot analyses of variance with sex (male/female) as the between-participants factor and role (victim/perpetrator) as the within-participants factor were conducted on these composite variables. A main effect of role was obtained on the negative emotionality variable, $F(1, 70) = 5.34, p < .02$. Interestingly, participants' narratives reflected more negative emotionality when they wrote about teasing another person as opposed to being teased. Part of this may reflect the remorse and guilt that many participants reported when they wrote about teasing others. No significant effects were obtained with the positive emotionality variable (see Table 11.2).

. . . Participants used more self-references when they wrote about being teased compared to teasing another, $F(1, 70) = 15.28, p < .001$. Conversely, more references to others were used when writing about teasing another than

Table 11.2 Main Effects of Role (Victim/Perpetrator)

Item	Victim	Perpetrator
How negatively did you feel about the experience?	9.1 (3.3)	4.9 (3.5)
How negatively do you think the other person felt?	2.9 (2.8)	8.3 (4.0)
To what degree was your self-esteem raised?	2.1 _a (2.3)	3.3 _a (3.1)
To what degree was your self-esteem lowered?	7.5 (4.1)	3.1 (2.8)
How annoyed were you by the experience?	10.0 (3.1)	3.6 (3.3)
How guilty did you feel about the experience?	2.6 (2.8)	5.5 (3.8)
How humorous did you perceive the experience to be?	2.8 (3.2)	7.2 (3.7)
How positively do you view the other individual involved?	4.8 (3.6)	7.4 (3.9)
How positively do you think the individual views you?	5.8 (3.5)	7.1 (3.7)
Negative emotionality (LIWC)	3.5 (2.5)	4.5 (3.4)
Positive emotionality (LIWC)	2.2 _a (1.7)	2.1 _a (1.8)
References to self (LIWC)	11.3 (2.6)	8.8 (3.9)
References to other (LIWC)	3.3 (3.4)	8.3 (3.7)
Total word count (LIWC)	103.7 (57.5)	87.6 (42.5)
Exclamation marks (LIWC)	.41 (1.3)	.12 (.47)

Note: LIWC = Linguistic Inquiry and Word Count. Means in a single row sharing a common subscript do not differ significantly, $p > .05$. Standard deviations (SDs) are reported in parentheses.

when writing about being teased, $F(1, 70) = 47.72, p < .001$. (Means are reported in the next table.) However, this effect was moderated by the gender of the participant, $F(1, 70) = 12.22, p < .001$ men ($M = 5.1, SD = 4.6$) used significantly more other references than did women ($M = 2.4, SD = 2.5$). In addition, women used more other references when their narratives concerned teasing others ($M = 8.8, SD = 3.9$) than when they focused on being teased ($M = 2.4, SD = 2.5$), $ps < .05$.

► (Note that circularity is not an issue in split-plot studies. Degrees of freedom (df), too, are as low as they can be. Furthermore, the results can be seen in a table to clarify them. Marginal means are in the following table, and three cell means are given. Only the mean for male perpetrators is missing, but we can calculate it by knowing ns in each group, the corresponding cell mean for females, and the overall marginal. We set up an equation, $22(X) + 50(8.8) = 72(8.3)$; $22(X) = 597.6 - 440$; $X = 7.1$).

Therefore, the table and all means are as follows:

	Males (22)	Females (50)
Victims	5.1	2.4
Perpetrators	7.1	8.8

From a structural perspective, participants used significantly more words when writing about being teased than about teasing, $F(1, 70) = 6.90, p < .01$. The proportion of exclamation marks used also was greater in narratives dealing with being teased than when teasing, $F(1, 70) = 4.40, p < .04$.

11. Scripts were analyzed for their structure, affect, self-references, and other references. What were the major findings?

Perpetrators used more negative words than did victims, but there were no differences between the two in the use of positive words. Victims used more self-references than did perpetrators, whereas the teasers used more references to others. Male victims, however, used more "other" references than did female victims. But the main effect of use of other references was due to females: Female perpetrators used more other references than did female victims. Finally, victims used more words and more exclamation points in their narratives than did perpetrators.

Postnarrative Questions

Two-by-two split-plot analyses of variance with sex as the between-participants factor and role as the within-participants factor were conducted on the remaining dependent variable measures. Overall, participants felt more negatively about the experience when they were the victim of a tease than when they perpetrated the tease, $F(1, 69) = 47.63, p < .001$. (Means for the role main effect are reported in the previous table.)

► (Notice that apparently, one participant did not complete this part of the questionnaire. The df went from 70 to 69.)

A main effect of gender obtained on negativity ratings revealed that men ($M = 5.8, SD = 3.6$) felt less negatively about the experience than did women ($M = 7.6, SD = 3.3$), $F(1, 69) = 7.20, p < .01$.

The analysis of the question that asked how participants perceived the other person felt as a result of the teasing incident revealed a main effect of role, $F(1, 68) = 77.04, p < .001$. Relative to victims, who perceived that the perpetrators felt mildly negative about the teasing event, perpetrators rated victims as feeling very negative about the experience.

... Analysis of the self-esteem enhancing effects of the experience revealed an interaction of sex and role, $F(1, 69) = 6.03, p < .02$. When recounting an event in which they were teased, men ($M = 3.3, SD = 3.1$) reported that their self-esteem was raised more than was reported by women ($M = 1.6, SD = 1.6$). However, women reported that their self-esteem was raised more when they perpetrated a tease ($M = 3.5, SD = 3.2$) than when they were teased ($M = 1.6, SD = 1.6$), $ps < .05$... Overall, participants reported their self-esteem to be only slightly affected in the positive direction by the experience.

The negative effects of teasing on self-esteem were more pronounced. First, women's ($M = 5.8, SD = 3.4$) self-esteem was lowered more than men's ($M = 4.4, SD = 3.5$), $F(1, 69) = 4.32, p < .04$... Victims of teasing reported more detrimental effects on their self-esteem than were reported by perpetrators, $F(1, 69) = 38.07, p < .001$.

Main effects of role also were obtained on questions examining how annoyed participants were by the experience, ... how much guilt they felt, ... and how humorous they found the experience to be... Participants were more annoyed when they were the victims as opposed to the perpetrator. On the other hand, although participants not surprisingly reported finding the experience more humorous when they perpetrated the tease than when they were teased, they also experienced more guilt as the perpetrator than as the victim (see Table 11.2).

... Whereas men's reports of the amount of guilt experienced did not differ as a function of whether they were the victim ($M = 3.5, SD = 4.0$) or perpetrator of the tease ($M = 4.0, SD = 3.7$), women reported significantly more guilt when they initiated the tease ($M = 6.0, SD = 3.7$) than when they were the victim ($M = 2.2, SD = 2.1$), $ps < .05$.

Main effects of role also were obtained on ratings of how positively participants viewed the other individual, $F(1, 69) = 12.95, p < .001$ and how positively they thought the other individual viewed them, $F(1, 69) = 4.94, p < .03$... Perpetrators viewed the targets more favorably than victims viewed perpetrators. On the other hand, perpetrators perceived that they were viewed more favorably by victims than victims thought they were viewed by perpetrators (see Table 11.2).

12. What were the results regarding the questionnaire responses?

Victims felt more negative than did perpetrators, but this was more true for females than males. Victims, however, rated perpetrators as feeling mildly negative about teasing, whereas perpetrators rated victims as feeling very negative. Positive self-esteem was slightly affected by either experience, but males reported higher self-esteem after being teased than did females, whereas females reported higher levels after teasing than after being teased. In contrast, females felt lower self-esteem than males (or at least were more willing to admit it) after being teased, and both sexes felt lower self-esteem after being teased than after teasing. Victims also found teasing more annoying, whereas perpetrators found the situation more humorous, although they experienced more guilt about teasing. The experience of guilt, however, depended on gender. Males felt equally guilty as victim and perpetrator; females felt guiltier after teasing than after being teased. Finally, victims viewed perpetrators less favorably than teasers viewed victims, whereas teasers felt that they were viewed more favorably by their victims than victims felt about how they were viewed by the teaser.

Discussion

Consistent with predictions and with previous research, victims and perpetrators perceived teasing episodes differently. Relative to victims, perpetrators not surprisingly perceived the incident to be more humorous. In addition, whereas perpetrators did not perceive the teasing encounters to be particularly annoying, victims reported being very annoyed by them. Perpetrators also reported higher feelings of guilt relative to victims. Perhaps reflecting these feelings of guilt,

physical appearance represents a readily observable feature, it is an easy target for teasing. Physical appearance is also a primary factor influencing perceptions of social approval and acceptance. . . . Because people who are physically attractive are perceived as more sociable, intelligent, psychologically adjusted, and skilled . . . teases about appearance perhaps more than any other type of tease may be used by perpetrators to convey dislike, relational devaluation, and social exclusion.

(Keep in mind that victims were teased mainly by schoolmates and then friends, whereas perpetrators teased mainly friends and then schoolmates. Moreover, it doesn't seem plausible that a teaser would view a victim positively and yet want to convey dislike or devalue a relationship.)

Gender differences that were observed in relation to the content categories . . . although consistent with what one might expect, should be interpreted cautiously. These differences may reflect discrepancies between men and women in the salience of particular types of teases rather than the frequency with which those teases are actually perpetrated. . . . When being asked to retrospectively recall episodes of teasing, those related to appearance may be more salient to women than to men, in large part because of society's emphasis on women's physical appearance. . . .

. . . Relative to relationships, about which men were teased most frequently, people have less control over their body parts and their physical appearance, about which women were teased most frequently. Because of the less changeable nature of the content of the tease, women may be more adversely affected by teasing than are men. . . . Women's self-ratings of attractiveness and self-esteem are highly correlated, which suggests that they may be particularly susceptible to negative evaluations of appearance. . . .

Relationship Between Victim and Perpetrator

All of the teasing incidents involved victims and perpetrators who were acquainted with one another. One reason for this is that teases, whether motivated by benevolent or malevolent concerns, miss their mark with strangers. . . .

Consequences of Teasing

The long-term, primarily negative impact of the teasing incident on the victims was clear in victims' ratings as well as in the narratives themselves. Many of the victims wrote about the teasing incident as being something that they will never forget or as an event that they "vividly remember." . . . Statements . . . convey victims' feelings that teasers do not value them or their relationship. . . .

recollections of episodes in which the participant perpetrated the tease contained more negative emotionality than did victim accounts.

. . . Victims emoted that they were viewed less positively than perpetrators said they viewed them. Perpetrators, on the other hand, thought they were viewed more favorably than victims actually perceived them. Four explanations may account for this. First, victims may have misinterpreted the motives of the perpetrator and viewed the tease as more malevolent than was the case. . . . In other words, they perceived the perpetrator's behavior to indicate relational devaluation. . . . Second, perpetrators may have teased because they disliked some aspect of the target but were unwilling to admit so in the experiment. Third, even if perpetrators actually viewed the target negatively at the time of the incident, the passage of time and personal feelings of guilt may have led them to regard the target more favorably. Fourth, victims' negative evaluations of perpetrators may have become more negative with the passage of time. The content of some of the narratives, particularly those written by victims, suggested that many of the victims had ruminated about the teasing incident since its initial occurrence. These ruminations might have magnified the event in the victim's mind, thereby enhancing negative feelings that he or she had about the incident and the individual who perpetrated it. . . .

. . . Victims reported more negative effects on their self-esteem than did perpetrators. Teasing may be aversive because it is threatening to the self. . . . Teasers characterize the victim in negative ways and often impose an identity on the victim that is inconsistent with his or her self-perception. If the tease is perceived by the victim as an indication that the perpetrator does not adequately value his or her relationship with the victim, then the victim's self-esteem is likely to be affected. . . .

Also of note is the finding that perpetrators were well aware of the negative effects of their teasing. . . . Victims rated perpetrators as feeling mildly negative about the incident, whereas perpetrators rated victims as feeling very negatively about being teased. Perpetrators' knowledge of the negative feelings induced by the teasing may have facilitated the feelings of guilt that perpetrators reported.

► *(Note the inconsistency in reports by perpetrators. They perceived negative effects of teasing and felt guilty. That is consistent. But they also viewed the situation as more humorous than did victims and believed that victims perceived them favorably.)*

Content of Teases

The overwhelming majority of the victim narratives and a notable percentage of the perpetrator narratives focused on body parts and physical appearance. . . . Because

Perhaps more surprising than the long-term effects of teasing on the victims were the negative effects recounted by perpetrators. After thinking back on the instances in which they teased someone else, many perpetrators reported feeling guilty or wondering what happened to the person [who] they teased. . . .

Many of the victim [and] perpetrator findings from this study parallel those of other studies in finding that victims not surprisingly view the experience more negatively than do perpetrators. . . . Rarely did perpetrators in the present study deny that their teasing had any negative effects. . . . Many of the episodes recounted occurred during middle and high school. Thus, maturation alone may account for some awareness on the part of perpetrators regarding the negative effects of their teasing. In addition, . . . teasing is almost never justified. The victim of teasing rarely if ever deserves to be teased. . . .

Limitations

. . . Certain limitations need to be acknowledged. The methodology relies on narrative accounts of teasing episodes that were "especially powerful and memorable." . . . Thus, we cannot be sure that the results presented here will generalize to recollections of more mundane teasing episodes.

. . . The narratives in which an individual teased and those in which they were teased reflect different events. This leaves open the possibility that the victim and perpetrator narratives are qualitatively different from one another, thereby undermining any conclusions that might be drawn. . . . We believe that the events recounted in the victim and perpetrator narratives are qualitatively similar.

In addition, the unwillingness of some men to participate in the study leads to the question of whether those who did participate represent a different group of individuals from those who chose not to be included. Descriptive statistics showing approximately equal variances for the male and female participants suggested that this was not the case.

► (Not necessarily so; these statistics show that males and females who volunteered for the study were similar in the statistical descriptions. They do not show that the males who refused to volunteer were the same or different. That is, their descriptive statistics might well differ from the males and females who volunteered.)

The gender effects examined in this study were based solely on the gender of the participants. Thus, we do not have information related to the frequency with which men and women were teased by members of each gender.

13. What were the major conclusions?

Perpetrators and victims perceived a teasing situation differently. Perpetrators perceived it as more humorous and with less annoyance but with more guilt. And their narratives contained more negative words, perhaps a reflection of the guilt. Perpetrators viewed victims more positively than victims felt they were perceived. But they also felt that they were perceived more favorably than victims perceived them. Perpetrators also rated victims as feeling more negative about the tease than victims rated themselves.

Because physical appearance was the biggest source of teasing, particularly for female victims, and social acceptability is correlated with physical attractiveness, teasing may be used to convey dislike and relational devaluation.

Negative impacts of teasing are greater on victims than perpetrators; however, many perpetrators feel guilty about having teased the victim.

14. Are the conclusions justified?

Because the narratives were written by the same individuals who had been victimized by powerful teases and, likewise, had victimized others in the same way, the conclusions about the findings per se probably are justified. Order of narratives was counterbalanced, and narratives were of events that took place at about the same time.

Although testing conditions may have been less than ideal, they should have had equal effects on both narratives. The major stipulation relates to the postnarrative questionnaire, in which sets of questions were not counterbalanced. These questionnaires focused on positive aspects of teasing, negative aspects, then prior teasing history. A reminder of negative aspects could have affected a victim report by emphasizing them and could have had the opposite effect on perpetrator reports. Or the questionnaire could have had no effect. Moreover, there were inconsistencies in narratives of perpetrators, but these temper the explanations of the findings, which may be open to debate (e.g., relational devaluation as an explanation of teasing). Moreover, we might be missing some valuable information from males who refused to participate. Nonetheless, their biggest loss is on external and not internal validity.

15. To what population can these results generalize?

Results can generalize to college undergraduates willing to write about powerful teasing episodes that took place in middle school, high school, or both and involved mainly schoolmates or friends.

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Chapter 12

Quasi-Experimental Studies



The major characteristics of a true experimental design, like those considered in the preceding chapters, include random assignment of participants to the various experimental and control conditions of the study. This procedure makes it more likely that groups initially are equivalent and therefore eliminates alternate explanations of results. In many instances, random assignment is not possible or not permitted by an administrator when, for example, special programs in institutions, medical settings, or mental health clinics need to be evaluated. Participants, such as those being treated after attempting suicide, abused children, drug addicts, and so forth, would not be amenable to random assignment. For these reasons, experimenters sometimes are forced to work with intact groups. The net effect is that the various groups cannot be assumed to be equivalent before treatment. Therefore,

the studies are not considered to be true experiments but instead are referred to as seemingly experimental or **quasi-experimental designs**. A variety of such designs are available. Keep in mind that we are dealing with intact groups, not groups that have been randomly assigned to different conditions. Therefore, the important issue is to work with these possibly nonequivalent groups in such a way that we may still be able to draw cause and effect conclusions.

There are two basic categories of these designs. Repeated measures types obtain a number of measures of the dependent variable before and after the intervention (independent variable) on the same group of participants. Any change in behavior after intervention is less likely to be contaminated by the usual confounds. This is the rationale behind the **interrupted time-series study**. It is suitable for behaviors that can stabilize but still have room for improvement or