

When Tactical Pronouncements of Change Become Real Change: The Case of Reciprocal Persuasion

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In 3 experiments, Ss' public statements of attitude change conformed to the norm of reciprocity, in that the most change on a topic was accorded to a persuader who had yielded to the S's earlier persuasive attempt on a prior topic, and the least such change was accorded to a persuader who had resisted the S's persuasive attempt. This tendency was unaffected by perceptions of the persuader's likability and intelligence, personal relevance of the topic, and strength of the arguments. Private change matched the pattern of public change only when the arguments Ss received were strong, and Ss could (mistakenly) attribute much of their reciprocation-induced change to the cogency of the arguments. Implications are discussed for the internalization of socially desirable conduct.

There is good evidence that a rule for reciprocity governs much of human experience: We report liking those who report liking us (Byrne & Rhomey, 1965; Condon & Crano, 1988); we cooperate with cooperators and compete with competitors (Braver, 1975; Rosenbaum, 1980); we self-disclose to those who have disclosed themselves to us (Cunningham, Strassberg, & Haan, 1986); we try to harm those who have tried to harm us (Dengerink, Schnedler, & Covey, 1978); in negotiations, we make concessions to those who have made concessions to us (Axelrod, 1984; Cialdini et al., 1975); and we provide gifts, favors, services, and aid to those who have provided us with these things (see Cialdini, 1988, for a review).

Of course, as with any form of action, there are variations in the way that the rule for reciprocity manifests itself. For instance, in certain long-term relationships, such as families and close friendships, the pure, tit-for-tat version of reciprocation may not occur. In these "communal" relationships, what is exchanged reciprocally is not a precise set of actions but, rather, the willingness to provide what the other needs, when it is needed (Clark, Mills, & Powell, 1986; Mills & Clark, 1982). It is noteworthy that even in these types of exchanges, then, there remains a fundamental commitment to reciprocity.

Impressed with its generality across behavioral domains and societal groups, some social scientists (e.g., Berkowitz, 1972; Cialdini, 1988; Gouldner, 1960) have accorded the rule for reciprocity the status of a social norm (one that obligates individuals to return the form of behavior they have received) that is said to maximize the outcomes of the individual who abides by it and of the societal group that enculturates it (Axelrod, 1984). Indeed, certain anthropologists have called the obligation to give back what we have gotten a central (Tiger & Fox, 1971) or the central (Leakey & Lewin, 1978) characteristic of being human.

It is odd, then, that there is no evidence showing that the obligation to reciprocate applies to one major and heavily re-

searched form of human behavior—reported persuasion. There are no data to our knowledge indicating that, if someone reports being persuaded by us on Topic A, we will act on an obligation to accede to that person's persuasion attempt on Topic B. One reason for the dearth of evidence in this regard may be the difficulty in documenting that such a reciprocal reaction on our part was due to a desire to reciprocate, rather than to other factors. For example, using average college students as subjects, Cialdini and his associates (Cialdini, Braver, & Lewis, 1974; Cialdini & Mirels, 1976) have demonstrated that persuaders come to view a target who has yielded to their persuasive arguments as more intelligent and likable as a consequence. Thus, if we found ourselves yielding to the arguments of someone who had yielded to ours on a prior topic, our acquiescence could be attributed not to the desire to reciprocate the act but to the increased favorability of the communicator in our eyes.

There is a crucial difference between the two explanations. If our shift is based on the perceived credibility of the communicator, then the change we exhibit should be genuine, manifesting itself not just on our public report of persuasion to the communicator but on more private measures as well. If, on the other hand, our shift is based on the more tactical and self-presentational desire to conform to the dictates of the reciprocity rule in our culture, then the effect should appear principally in our public report of attitude change to the communicator who had previously yielded to us.

The possibility that statements of opinion would be structured to meet tactical goals of a social nature is consistent with a long-standing (though never prominent) recognition within the discipline that attitude expressions serve multiple functions, only one of which may be to accurately represent genuine feelings (Cialdini, Levy, Herman, & Evenbeck, 1973; Cooper & Jones, 1969; Jones, 1964; Kelman, 1961; McGuire & Millman, 1965; Tedeschi, Schlenker, & Bonoma, 1971). In addition, more recently, theorists have begun to insist that reports of attitude change can only be fully understood through formulations that consider the reporter's interpersonal motives (Chaiken, Liberman, & Eagly, 1989; Johnson & Eagly, 1989; Leippe & Elkin, 1987).

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The purpose of the present research was twofold. First, we wished to test whether reports of attitude change appear in a reciprocal pattern, such that individuals admit to greater change in response to the persuasive arguments of a communicator who has yielded to the individuals' persuasive attempts on an earlier topic. To this end, we arranged for subjects to deliver a persuasive message on an initial topic to a fellow subject (actually an experimental confederate) who either publicly yielded to or resisted the persuasive attempt or who (in a control condition) did not respond to it. Then, on a second topic, all subjects received a (rather weak) persuasive communication from the confederate and were asked to report to the confederate the extent to which they changed their own opinions in response to it. These statements of change constituted the major dependent measure of the research and were expected to show a reciprocal pattern such that the greatest admitted change would be reported to communicators who had previously yielded to the subjects' arguments, whereas the least such change would be reported to communicators who had previously resisted them.

The second purpose of this research was to determine whether the predicted pattern of reported change (if obtained) could have been uniquely caused by the tendency to abide by the reciprocity norm. That is, we wondered whether this pattern would appear when other possible causes—such as genuine persuasion resulting from differing perceptions of communicator positivity—were eliminated. To attempt to answer this causal question we took three steps. First, we provided all subjects with prior, equivalent information about the intelligence and likability of the confederate. It was hoped that this procedure would hold constant subjects' perceptions of the communicator's positivity. Second, we added to the design an independent variable—topic relevance—that would not be expected to affect subjects' statements of change if those statements were merely public claims of a tactical sort but that would be expected to influence those statements if they reflected genuine opinion shifts flowing from perceptions of the communicator's positivity. That is, Petty and Cacioppo (1986; Petty, Cacioppo, & Goldman, 1981) have shown that perceptions of a communicator's positivity shifted subjects' opinions only on topics of low personal relevance to the subjects. By manipulating topic relevance in the present research, we could observe whether subjects' tendency toward reciprocal persuasion appeared only on a low-relevance topic (in keeping with a communicator positivity mediator of the effect) or appeared on both high- and low-relevance topics (in keeping with the rule for reciprocation). Third, in addition to recording subjects' attitude change statements made in the presence of the communicator, we included a measure taken in private. In this fashion, it was possible to compare the amount of persuasion reported with the communicator present and absent.

On the basis of the earlier reviewed literature indicating that the obligation to reciprocate is a powerful motivator of human social conduct and on the basis of considerable research indicating that individuals frequently make attitude statements to achieve social goals, tactical goals, or both (Braver, Linder, Corwin, & Cialdini, 1977; Cialdini et al., 1973; Cooper & Jones, 1969; Davis & Florquist, 1965; Johnson & Eagly, 1989; Jones, 1964; McGuire & Millman, 1965), we made the following predictions: First, subjects would report the greatest amount of

change to a communicator who had yielded to their arguments on a prior topic and would report the least such change to a communicator who had resisted their arguments. Second, this basic pattern would be similar for topics on which the subjects felt either high or low personal involvement. Third, the pattern of attitude statements made by subjects in private would not conform to the pattern of public statements.

Experiment 1

Method

Subjects

Forty-two introductory psychology students at Arizona State University (ASU) participated in partial fulfillment of course requirements. Data from 5 of the subjects were omitted from the analysis because of accurate suspicions regarding the experimental ruses or hypotheses.¹ These 5 subjects were spread about evenly across the levels of the major independent variable of the study, such that 2 were in the *yield* condition, 2 were in the *control* condition, and 1 was in the *resist* condition.

Design and Procedure

After appearing at a laboratory room to participate in an "interpersonal interaction" experiment, the subject and a same-sex experimental confederate (posing as a fellow subject) were seated in desks separated by a partition that prevented visual contact but allowed verbal exchange.

Initial attitude assessment and manipulation of topic relevance. The subject and confederate first filled out a questionnaire assessing their attitudes on eight topics that they were told were "currently under consideration by various national, state and local legislative bodies." One of the topics concerned a proposal to require comprehensive senior examinations at their university, either in the next year (high relevance) or in 10 years (low relevance). This manipulation was patterned after the procedures used repeatedly and successfully by Petty and Cacioppo (1986) to vary the degree of personal relevance of the senior comprehensive exams issue. A second topic in the initial questionnaire concerned a proposal to lower the drinking age in Arizona to 18 years. Subjects indicated on 9-point scales how favorable or unfavorable they felt about each of the eight proposals.

After collecting the questionnaires, the experimenter informed the participants that they would be assigned one topic apiece and would be asked to write three arguments supporting their opinions on that topic. Allegedly at random, the subject was always assigned to the drinking age issue and the confederate to the senior comprehensives issue. The experimenter explained that the participants would be reading each other's arguments, but because one purpose of the study was to explore how having different amounts of information might influence interpersonal interaction, one of them would be receiving some biographical information about the other. A rigged drawing was staged, which invariably determined that the subject would receive the biographical sketch. The sketch consisted of bogus feedback from a

¹ The determination of whether a subject's data would be dropped from the analysis was made on the basis of his or her written responses to a pair of postexperimental questions inquiring into suspicions about the nature of the experimental hypotheses and ruses. A judge (Robert B. Cialdini) read these responses while blind to subjects' experimental conditions and eliminated data from subjects whose suspicions were sufficiently accurate to discredit their data.

number of previously administered tests,² showing the confederate to have scored in the average range for intelligence, likability, and anxiety. This information was provided in an attempt to equate across conditions the subjects' perceptions of the confederate's positivity. While the experimenter went to "obtain the biographical information off the computer," the subject and confederate were each to write three arguments supporting their positions on their assigned topic.

First interaction and manipulation of prior persuasion. The experimenter returned and gave the biographical sketch to the subject, who read it and returned it to the experimenter. The experimenter then gave the subject's arguments on the drinking age issue to the confederate and instructed him or her to read them carefully before summarizing his or her opinion on the topic for the subject. Before leaving the room to allow the participants to interact on the topic, the experimenter (except in the control condition) pointed to a large 100-point scale affixed to the wall, the end points of which were labeled (1) *disagree totally with proposal* and (100) *agree totally with proposal*, saying that because some people are more visually than verbally oriented, the participants might want to use the scale on the wall to help them convey their opinions to one another. When the experimenter left, the confederate read the subject's statements and responded in one of three ways designed to manipulate the subject's success in persuasion.

In the yield condition, the confederate stated that, despite initially holding an opinion opposed to that of the subject, the subject's arguments made sense, and he or she now agreed more with the subject. Using the scale on the wall, the confederate indicated that his or her initial attitude had been about 10 scale points from midscale in the direction opposite to the subject's position, but now it was about 20 points from midscale in the same direction as the subject's position. For example, if the subject's statements were in favor of lowering the drinking age, the confederate would say, "Well, I read your arguments, and they make sense. In fact, I would say that before I read your arguments, I was about a 40 on the scale, but after reading your arguments, now I'm about a 70," thereby showing that the subject's arguments successfully changed the confederate's view.

In the resist condition, the confederate indicated that his or her opinion on the scale had not moved from being 10 scale points from midscale in the direction opposite to the subject's. For example, if the subject favored lowering the drinking age, the confederate would say, "Well, I read your arguments, and they make sense; but I really don't think I've changed my mind any. I'd say that before I read your arguments I was about a 40, and I still am."

In the control condition, the experimenter did not instruct the participants to interact in any way. Therefore, no verbal exchange took place on the drinking issue.

Ratings of the confederate's positivity. The experimenter returned and handed out a Personal Assessment Questionnaire, consisting of ten 7-point scales, five of which assessed likability (*likable, good, friendly, warm, and enjoyable*) and five of which assessed intelligence (*worthy of respect, intelligent, wise, perceptive, and knowledgeable of current events*). The participants were instructed to use the scales to rate their perceptions of the other.

Second interaction and public report of persuasion. In the second interaction, the subject was given the confederate's arguments supporting the proposal to require senior comprehensive exams at ASU, either in 10 years (low relevance) or in the next year (high relevance). These arguments, designed to be weak in order to reduce argument-based attitude change, were (a) By the time you're a senior, what difference does one more test make? (b) They have senior comprehensives at other schools, I don't see why we shouldn't have them at ASU; and (c) They couldn't make a senior comprehensive at ASU too hard, so I don't think it would be a big deal to have one.

On leaving the room, the experimenter instructed all subjects to summarize their opinions on this issue to one another by using the

scale on the wall. After allowing the subject time to read the arguments, the confederate asked the subject to indicate where "you would have put yourself on the scale before reading my arguments, and where you would put yourself now." The number of units of change the subject indicated was secretly recorded by the confederate and constituted the measure of publicly reported attitude change.

Measure of persuasion taken in private. On returning, the experimenter began to hand out "a final questionnaire" and discovered that there was only one left, which was given to the confederate to complete while the experimenter left the room to retrieve another questionnaire. The confederate had finished the questionnaire by the time the experimenter returned and, consequently, was dismissed, allowing the subject to complete the questionnaire in private. This sequence of events was staged to ensure the subject that the confederate would not have access to the subject's answers, thus facilitating honest responding to the attitude-related items on the questionnaire. The final questionnaire contained 12 opinion statements concerning the topics that subjects had responded to on the initial attitude questionnaire. Three of the statements involved the topic of senior comprehensive examinations: "Senior comprehensives should be used because they ensure that the students have retained what they have learned"; "Students who have passed their previous classes do not need to take comprehensive exams"; and "No student should have to undergo the added stress of comprehensive exams in order to graduate from college." Subjects indicated their agreement or disagreement with each statement by responding to a 9-point Likert-type scale, ranging from *totally agree* to *totally disagree*. The average of subjects' responses to the three senior-exam-related statements constituted our index of privately measured persuasion. Averaging subjects' responses over these three statements was deemed warranted by high interitem correlations among them ($\alpha = .87$). On the basis of pilot work, we felt comfortable that this three-item index was comparable in sensitivity to our single-item measure of publicly reported attitude; the correlation between control subjects' comprehensive exam attitude scores on the two measures was $r(12) = .90$, $p < .001$.

Results

Analyses to determine the impact of subject sex on responding within our paradigm produced no significant effects. Consequently, all subsequent analyses did not include that variable.

Public Change

Table 1 shows the average number of units of public change reported in the various cells of the design. An analysis of variance (ANOVA) was performed on subjects' reports of attitude change to the communicator on the senior exams topic. In keeping with our first prediction, subjects declared the greatest amount of change to a communicator who had yielded to their arguments on the drinking age topic (26.92) and declared the least amount of change to a communicator who had resisted their arguments on the drinking age topic (6.25), with control subjects reporting an intermediate amount of change (16.17), $F(2, 31) = 5.44$, $p < .009$. Simple effects tests within the significant main effect showed that it was composed of two margin-

² The plausible availability of such information was made possible by a battery of tests that had been administered to all introductory psychology students in the 2nd week of the semester. It was from this mass testing session that the biographical sketch was said to have come.

Table 1
Mean Units of Publicly Reported Change: Experiment 1

Topic relevance	Prior persuasion		
	Yield	Control	Resist
Low			
<i>M</i>	27.50	10.00	7.50
<i>n</i>	6	7	6
High			
<i>M</i>	26.43	24.80	5.00
<i>n</i>	7	5	6
<i>M</i>	26.92	16.17	6.25

ally significant components: yield versus control, $t(34) = -1.74$, $p < .09$; and resist versus control, $t(34) = 1.57$, $p < .12$.

There was no significant main effect of the topic relevance manipulation ($F < 1$). More important, consistent with our second prediction, there was no significant interaction between the prior persuasion and topic relevance factors, $F(2, 31) = 1.13$. Within the low-relevance conditions, the prior persuasion effect was marginally significant, $F(2, 31) = 3.04$, $p < .062$; whereas, in the high-relevance conditions, it was conventionally significant, $F(2, 31) = 3.60$, $p < .04$. Tukey tests performed on the set of six means found no two means to be significantly different from one another.

Private Change

To compute a measure of private change on the senior exams topic, we subtracted each subject's score on the final attitude measure (taken in private) from that subject's score on the initial attitude measure (taken at the outset of the experiment). An ANOVA on those change scores produced no significant main or interaction effects (all F s < 1.08).³ However, the means for the prior persuasion conditions did approximate the shape of the pattern found on the public measure of change: yield = 1.87; control = 1.11; and resist = 1.03.⁴ To ensure that the significant reciprocal pattern obtained on the public measure of change was truly different from the nonsignificant reciprocal pattern found on the private measure, we performed an analysis in which the public and private measures of change were treated as repeated measures. In other words, we performed a $2 \times 3 \times 2$ mixed ANOVA, with a within-subjects factor labeled *type-of-change measure* (public or private). If that analysis generated a significant interaction between the prior persuasion factor and the type-of-change measure factor, we would have evidence that the effects of prior persuasion on the two kinds of change were truly different, as predicted. That analysis did produce a significant Prior Persuasion \times Type-of-Change Measure interaction, $F(2, 31) = 5.37$, $p < .01$; in short, the significant reciprocal pattern found on the public-change scores was significantly different from the nonsignificant reciprocal pattern found on the private-change scores. No other interactions approached significance; however, because of a metric difference between the two types of change scores, there was a significant type-of-change measure main effect, $F(1, 31) = 38.45$, $p < .01$.

Additional Measures

We also submitted subjects' perceptions of the confederate's likability and intelligence to ANOVAs to determine if we had succeeded in equating these perceptions across conditions of the experiment. Evidence that we did succeed in this regard comes from the fact that no main or interaction effects proved significant for either analysis. Furthermore, an examination of the means relative to the crucial prior persuasion factor indicated that for both sets of perceptions a pattern emerged that was distinct from that of the public attitude-change measure: For likability, yield = 5.75, control = 5.17, and resist = 5.50; for intelligence, yield = 5.50, control = 4.97, and resist = 5.32.

Discussion

The results of our first study were quite congruent with our predictions. First, in keeping with the expectation that admissions of persuasion would be tactically generated to conform to the rule for reciprocity, subjects' statements of attitude change appeared in a reciprocal pattern, such that the greatest change was reported to a persuader who had yielded to the subjects' arguments on a prior topic, and the least such change was reported to a persuader who had resisted the subjects' arguments. Moreover, this basic reciprocal pattern (a) was not different for topics of high or low personal relevance, (b) was not significant on the measure of private change, and (c) did not match the pattern of subjects' perceptions of the persuader's intelligence or likability. This last result, especially, seemed to reduce the likelihood that the reciprocal change effect was mediated by perceptions of the communicator's positivity. To provide further evidence in this regard, we conducted an analysis of covariance (ANCOVA) on both the public- and private-change measures using likability and intelligence as covariates. The results were identical to those of the original ANOVAs we had conducted. That is, no effects approached significance except the prior persuasion main effect on the public-change measure, $F(2, 28) = 6.84$, $p < .001$.

Despite the overall support for our hypotheses in Experiment 1, we saw several reasons to replicate and extend our findings. First, Experiment 1 included no manipulation check on the personal relevance variable, thereby reducing the certainty with which we could make the claim that the reciprocal public change effect occurred both for issues of low and high personal relevance. Second, the cell *ns* of that initial study were quite small, further reducing confidence in the generality of that ef-

³ An alternative analysis was performed in which the initial attitude measure was covaried on the final attitude measure. In this study and all subsequent studies, this analysis of covariance approach produced results that were invariably comparable to those of the change-score analysis. Consequently, we have chosen to present the more intuitively accessible change-score results.

⁴ Although no significant differences were found within the design on the private measure, for the sake of completeness we present the means for each of the six experimental cells herein, with the low-relevance mean appearing first within each level of the prior persuasion factor: yield = 2.22 and 1.57; control = .86 and 1.47; and resist = 1.22 and .83. Tukey tests performed on these six means found no two significantly different from one another.

fect. That is, it is possible that the failure to obtain an interaction between the personal relevance factor and the prior persuasion factor was not due to the robustness of the reciprocal change effect but was due to inadequate cell sizes that resulted in a weak statistical test of the interaction. Finally, an alternate conceptual explanation remained conceivable for the findings of Experiment 1. Perhaps, having their persuasive arguments accepted, not commented on, or rejected put subjects in different moods, respectively, that temporarily affected the way that they viewed the persuader's arguments. If, as seems plausible, the subjects who had succeeded in convincing the confederate were feeling most happy, whereas those who had failed were feeling least happy, the results of Experiment 1 could be explained as mere mood-based effects. One research tradition has shown that, after exposure to positive mood induction procedures, people react more favorably to a variety of stimuli (Clark & Waddell, 1983; Howard & Barry, 1990; Isen, Shalke, Clark, & Karp, 1978; Manis, Cornell, & Moore, 1974), including persuasive appeals (Razran, 1938). It seemed possible, then, that the reciprocal pattern observed on the public-change measure was not mediated by a tendency for reciprocation at all. Instead, it may have reflected the current mood states of the subjects and the corresponding degree of favorability they accorded to any stimuli they processed while in those experimentally modified moods; and, perhaps the reciprocal pattern appeared only on the public-change measure because, by the time subjects were administered the private-change measure, the mood state differences among the three groups of subjects had dissipated. To compensate for these three weaknesses of Experiment 1, we undertook a second study.

Experiment 2

The alternative account of our earlier findings that we wished to test required that mood differences be present among the yield, control, and resist condition subjects at the time of the subjects' exposure to the persuader's arguments. Therefore, we reasoned that, if we could eliminate any mood differences (that may have been produced by the manipulation of prior persuasion in our experimental setting) before subjects got the chance to hear the arguments and to publicly report attitude change, and if the reciprocal pattern nonetheless appeared, we would have evidence against the mood-based interpretation of our prior results. To this end, in Experiment 2, we interpolated a mood-neutralizing activity between the manipulation of the prior persuasion factor and subjects' reports of attitude change to the confederate. We also included a mood-check measure to assess the degree to which the activity succeeded in removing any potential mood differences. In addition, to ensure that our effect was not unique to the scale that participants used to report their public changes, we substituted a 9-point scale for the 100-point scale we used in Experiment 1. Finally, in an attempt to enhance confidence in the generality of the reciprocal change effect for both low- and high-relevance issues, we added a manipulation check on personal relevance, and we greatly increased our cell sizes so as to allow for a more powerful test of the interaction between the personal relevance and the prior persuasion factors.

Method

Subjects

Participants were 102 introductory psychology students at ASU, the data from 7 of whom were removed from the analysis because of accurate suspicions regarding the experimental ruses or hypotheses—2 from the yield condition, 2 from the control condition, and 3 from the resist condition.

Procedure

The procedure was identical to that of Experiment 1, with the following exceptions. First, in the initial interaction, yield condition subjects heard the confederate admit to 2 units of change in their direction on a 9-point scale (e.g., from a 4 to a 6), rather than the 30 units of change on a 100-point scale used in Experiment 1; subjects in the resist condition heard the confederate admit to remaining unchanged (e.g., at 4). Second, to allow for the dissipation of any mood changes associated with having been yielded to or resisted during the first interaction, after subjects rated the confederate's likability and intelligence, they (along with the confederate) rated the pleasantness of a series of 26 nature photographs. This photograph-rating task was designed to equate subjects' moods by providing a uniform, mildly pleasant activity that would dissipate existing mood differences by virtue of distraction and the passage of time (Isen, Clark, & Schwartz, 1976). Immediately following the task, participants rated their current moods on a set of eight 7-point scales—labeled as *happy*, *anxious*, *sad*, *low spirited*, *feeling good*, *elated*, *agitated*, and *feeling low*. The scales were anchored by the terms *not at all* (1) and *very much* (7). The final procedural modification involved the introduction of three questions, after the private attitude measure had been administered, designed to check on the success of the experimental manipulations. Two of the questions checked whether the topic relevance manipulation had been properly implemented. One asked subjects to indicate how "personally relevant" the senior exams issue was to them (on a 9-point scale); the other asked what year the exams were proposed to begin. The third question asked subjects whether the "other subject" had changed his or her mind on the drinking age proposal during the first interaction; this constituted the check on the prior persuasion factor.

Results

Public Change

An ANOVA generated only one reliable effect for subjects' public reports of change, the main effect of prior persuasion, $F(2, 89) = 7.52, p < .001$. As can be seen in Table 2, the pattern

Table 2
Mean Units of Publicly Reported Change: Experiment 2

Topic relevance	Prior persuasion		
	Yield	Control	Resist
Low			
<i>M</i>	2.00	0.84	0.86
<i>n</i>	16	19	14
High			
<i>M</i>	1.69	1.19	0.71
<i>n</i>	16	16	14
<i>M</i>	1.84	1.00	0.79

of the means appeared in the same form as in our prior study (yield = 1.84, control = 1.00, and resist = 0.79). As in Study 1, the main effect of prior persuasion was composed of a pair of influences—the twin tendencies to reciprocate yielding and resisting. However, in the present study, simple effects tests demonstrated that prior yielding produced a substantially greater reciprocal response than did prior resistance, which by itself was not significant: yield versus control, $t(92) = -3.1$, $p < .01$; control versus resist, $t(92) = 0.8$.

Neither the main effect for topic relevance nor its interaction with prior persuasion approached significance ($F_s < 1$). Within the low-relevance conditions, the prior persuasion effect was significant, $F(2, 89) = 5.53$, $p < .01$; whereas, in the high-relevance condition, it was marginally significant, $F(2, 89) = 2.74$, $p < .07$. Tukey tests performed on the set of six means found only one pair of means to be significantly different from one another at the .05 level—the low relevance–yield and the high relevance–resist cell means.

Private Change

Consistent with the results of Experiment 1, an ANOVA produced no significant effects on the measure of private change (all $F_s < 1.20$). The means for the yield, control, and resist conditions, respectively, were 0.94, 0.68, and 1.38.⁵ Although the pattern of the means for the private measure of change did not match that for public reports of change, we again performed a $2 \times 3 \times 2$ mixed ANOVA, using type-of-change measure as a within-subjects factor to ensure that the effects of the prior persuasion factor were truly different for the two types of measured change. That analysis produced only one effect that even approached significance, the expected Prior Persuasion \times Type-of-Change interaction, $F(2, 89) = 4.73$, $p < .01$.

Additional Measures

Three questions served as checks on the experimental manipulations. Two of the questions assessed the effectiveness of the topic relevance manipulation. The first asked subjects to indicate the extent—from *extremely* (1) to *not at all* (9)—to which the senior exams issue was relevant to them. An ANOVA found a significant effect for the topic relevance factor such that high-relevance condition subjects rated the topic as more personally relevant (4.07) than did the low-relevance condition subjects (5.71), $F(1, 89) = 12.07$, $p < .001$; no other effects approached significance ($F_s < 1.35$). A second check on the relevance manipulation asked subjects to state the number of years before the senior exams were proposed to be put into place; all but 1 of the subjects answered correctly. A third question, functioning as our check on the prior persuasion manipulation, asked subjects to recall whether the “other subject” had changed on the drinking age issue in the first interaction; once again, all but 1 subject responded correctly. We interpret these findings to indicate that our experimental manipulations did work as intended.

An ANOVA was conducted on the combined eight adjective scales designed to measure subjects’ mood. There were no differences among subjects on the mood measure (all $F_s < 2.5$). The combined mood-score means on the prior persuasion fac-

tor were 5.30, 4.87, and 5.10 for the yield, control, and resist conditions, respectively.

Finally, we analyzed the likability and intelligence ratings of the confederate and, unlike in Experiment 1, found main effects for prior persuasion, such that control condition subjects rated the confederate as less likable (5.20) and less intelligent (4.98) than did the yield condition subjects (5.62 and 5.31, respectively) or the resist condition subjects (5.64 and 5.59, respectively). For likability, $F(2, 89) = 3.95$, $p < .025$; for intelligence, $F(2, 89) = 4.66$, $p < .012$. Although reliable, the pattern of means for these measures is different from that of the public-change measure; thus, perceptions of confederate likability or intelligence cannot be easily evoked as explanations of the reciprocal form of publicly reported change. Additional evidence in this regard comes once again from an ANCOVA using likability and intelligence ratings as covariates. That analysis produced the identical pattern of effects generated by our original ANOVAs. That is, only one effect proved significant—the main effect for prior persuasion on the public measure of change, $F(2, 87) = 6.36$, $p < .01$. Our best guess as to the reason that control subjects rated the confederate least positively is that, at the time they made their ratings, they had had the least interaction with the confederate, who had offered polite and reasonable responses to the subjects’ persuasion attempts (only) in the other two conditions.

Discussion

Our intent in Experiment 2 was to gain confidence that the reciprocal form of publicly reported persuasion found in Experiment 1 would (a) apply whether the personal relevance of the issue was low or high and (b) appear when we eliminated the explanatory relevance of possible mood differences among our subject groups. In the first regard, despite remaining in the uncomfortable position of trying to support the null hypothesis, we found no hint of a Prior Persuasion \times Personal Relevance interaction, even though our personal relevance manipulation check was successful, and there was considerable power in the statistical test of the interaction. In the second regard, through the inclusion of a mood-neutralizing task activity and a mood measure, we were able to ensure and document that no differences in mood existed across our subject groups prior to their public reports of attitude change. Yet the reciprocal relationship appeared again in those public reports, rendering a mood-based interpretation of that relationship improbable.

Experiment 3

In support of our argument that the predicted reciprocal pattern of publicly reported change observed in our subjects re-

⁵ Although no significant differences were found within the design on the private measure, for the sake of completeness we present the means for each of the six experimental cells of Study 2, with the low-relevance mean appearing first within each level of the prior persuasion factor: yield = 1.10 and .77; control = .47 and .92; and resist = 1.24 and 1.52. Tukey tests performed on these six means found no two significantly different from one another.

sulted from a tendency to conform to the rule of reciprocity, we have never found the reciprocal pattern emerging to any significant degree in subjects' private reports of change. That is, if subjects' public statements of change only reflected tactical attempts to live up to the norm of reciprocity (rather than reflecting genuine changes in attitude), we expected that the public changes would not be matched by private changes. That expectation received support in both of our reported studies, as well as in extensive pilot work conducted preparatory to those studies.

With that point safely made, however, it seemed beneficial to consider the circumstances under which public changes of the sort our subjects asserted would become internalized as private changes. For example, there is good reason to believe that if, in our experimental paradigm, subjects were exposed to strong arguments on the senior exams issue (rather than to the weak arguments we had been using), they would show the reciprocal pattern on both the public and the private measures of change. What is more, that reasoning suggests that the private changes would not spring directly from the persuasive power of the strong arguments but from attributional processes that took argument strength into account.

There is a substantial body of research to indicate that individuals see as causal those features of a setting that are salient (see Fiske & Taylor, 1991, for a review). It is our view that in Experiments 1 and 2, the manipulation of prior persuasion made salient the norm of reciprocity for our subjects, who then conformed to its dictates on the measure of publicly reported change. Furthermore, because the arguments the confederate used were quite weak, subjects had little basis for attributing anything to themselves but tactical motives for those public changes. Therefore, when the time came for subjects to record their genuine attitudes toward the senior exams topic, there was no good reason for them to suppose that they actually believed what they had reported. Accordingly, we found no effects of our experimental manipulations on the private-change measures in either of the two experiments using weak arguments.

A very different attributional sequence would apply, though, were subjects to be confronted with strong arguments. Because of the power of the reciprocity norm, it would still be our expectation that subjects would show a reciprocal pattern of change on the public measure. However, when asked to reflect on their true attitudes in private, subjects would now find another salient and plausible cause for their public changes—the presence of strong arguments. Thus, in assigning any causal weight for their public changes to the cogency of the arguments, subjects would be expected to attribute to themselves corresponding private changes. To use a somewhat different theoretical language, subjects exposed to strong arguments prior to their publicly reported changes could discount (Kelley, 1972) the exclusive role of the reciprocity rule in bringing about whatever changes occurred and could assign at least some of the causality to genuine persuasion.

We saw three benefits of testing this expectation. First, we would be able to observe whether our basic finding would appear outside of the weak-argument settings of the prior experiments. It was conceivable that when the arguments were strong, they would carry the day and would overwhelm the role of reciprocity as an influence on subjects' public reports of

change. Second, in addition to examining the generality of the reciprocal effect, conducting a study that used strong arguments would provide valuable theoretical information. As discussed previously, we could identify certain conditions under which tactically reciprocated change statements would become internalized as private changes. A last benefit would be methodological. That is, a critic could argue that the lack of a significant effect on the private-change measure in either of the prior studies may be best accounted for, not by the theoretical arguments we have suggested, but by the possibility that our private-change measure was too insensitive to detect real change. Should we find the significant reciprocal change pattern predicted in Experiment 3 on the private measure, however, such an explanation would become improbable.

Method

Subjects

Participants were 101 introductory psychology students at ASU, the data from 5 of whom were removed from the analysis because of accurate suspicions regarding the experimental ruses or hypotheses—1 from the yield condition, 2 from the control condition, and 2 from the resist condition.

Procedure

The procedure was identical to that of Experiment 2, with two exceptions. The first was implemented because pilot testing of the strong arguments under control condition circumstances indicated that these arguments produced an average of two full units of publicly reported change—an amount of change equivalent to that declared by the confederate in our yield condition. Therefore, the possibility existed that, when strong arguments were used, a ceiling effect would artifactually obscure genuinely reciprocated public change. That is, if we used strong arguments in our experimental paradigm, yield condition subjects could reciprocate perfectly (the two units of change the confederate had earlier ceded to them) and yet show no more change than the control condition subjects. Consequently, a true act of reciprocation among yield condition subjects could not be observed from the data. To eliminate this potential ceiling effect problem, yield condition subjects in Experiment 3 heard the confederate change four units in the subjects' direction on the drinking age topic, thereby allowing subjects to exhibit an amount of change (in return) greater than that produced by the cogency of the strong arguments alone.

The second modification was to substitute three strong arguments for the three weak arguments that had been used in all prior experiments. These strong arguments were adapted from those developed by Petty and Cacioppo (1986) to offer powerful support for the senior exams proposal.⁶

⁶ Those strong arguments were (a) I just read an article about how at Duke University when they started using senior comps the overall GPA went up about 30%, so maybe if we had them at ASU, it would make people study more and get better grades; (b) The article also said that at graduate and medical schools they give preference to students that have comprehensive exams, so I think that if we had them here, it would make ASU graduates more competitive; and (c) I know students at most Ivy League schools have to take senior comps, and I think if we had them here, then ASU would become more prestigious and lose its reputation as being such a "party school."

Results

Public Change

As in the earlier studies, an ANOVA generated a significant main effect for prior persuasion on the public-change measure, $F(2, 90) = 9.35, p < .001$. The means displayed in Table 3 document that, once more, evidence for reciprocated change appeared in these public reports (yield = 3.29, control = 2.27, and resist = 1.71). Simple effects tests demonstrated that the greater part of this effect was attributable to the tendency of subjects to reciprocate yielding: yield versus control, $t(94) = -2.7, p < .01$; control versus resist, $t(94) = 1.51, p < .12$.

Neither the effect for topic relevance nor its interaction with prior persuasion approached significance ($F_s < 1.35$). Within the low-relevance condition, the prior persuasion effect was significant, $F(2, 91) = 8.56, p < .01$; whereas, within the high-relevance condition, it was marginally significant, $F(2, 91) = 2.26, p < .12$. Tukey tests performed on the set of six means found two pairs of means to be significantly different from one another: low relevance–yield versus low relevance–resist; and low relevance–yield versus high relevance–resist.

Private Change

An ANOVA produced only one significant effect on the private measure of change, the main effect of prior persuasion, $F(2, 90) = 3.31, p < .04$. As predicted, the pattern of means for that effect was similar in form (yield = 2.48, control = 1.51, and resist = 1.34) to that of publicly reported change.⁷ Indeed, the parallel form of the public- and private-change measures even extended to the simple effects within the prior persuasion main effect. That is, as with the public-change measure, the greater part of the main effect for private change was due to a tendency for subjects to reciprocate yielding: yield versus control, $t(93) = -1.95, p < .055$; control versus resist, $t(93) = 0.43, ns$. The similarity of the two change patterns was further affirmed in a $2 \times 3 \times 2$ mixed ANOVA using type-of-change measure as a within-subjects factor. The crucial Prior Persuasion \times Type-of-Change interaction was far from significant ($F < 1$), as were all other interactions.

Additional Measures

The items that documented the success of our topic relevance manipulation in Experiment 2 showed evidence of comparable success in the present experiment. That is, high-relevance condition subjects rated the topic of senior exams as more personally relevant (3.02) than did low-relevance condition subjects (5.20), $F(1, 89) = 20.48, p < .001$; and 96% of the subjects remembered correctly the number of years proposed for the exams to be implemented. Similarly, our check on the prior persuasion manipulation showed that 97% of the experimental subjects remembered correctly whether the confederate had changed on the drinking age issue. On the basis of these data, we felt confident that the independent variables of Experiment 3 had been properly manipulated.

The combined measure of subject mood was also submitted to an ANOVA and, as in Experiment 2, showed no significant

Table 3
Mean Units of Publicly Reported Change: Experiment 3

Topic relevance	Prior persuasion		
	Yield	Control	Resist
Low			
<i>M</i>	3.71	2.20	1.57
<i>n</i>	17	15	14
High			
<i>M</i>	2.89	2.33	1.82
<i>n</i>	19	15	17
<i>M</i>	3.29	2.27	1.71

effects (all $F_s < 1.68$; $M_s = 5.48, 5.06$, and 5.10 , for the yield, control, and resist conditions, respectively).

As in Experiment 2, separate ANOVAs on the likability and intelligence ratings each produced only one effect that approached significance, the main effect of prior persuasion: for likability, $F(2, 90) = 4.16, p < .02$; for intelligence, $F(2, 90) = 3.35, p < .04$. However, the pattern of means for these effects (for likability, yield = 5.66, control = 4.99, and resist = 5.32; for intelligence, yield = 5.55, control = 4.94, and resist = 5.21) did not conform to those of either of the measures of attitude change. As with Studies 1 and 2, ANCOVA analysis using likability and intelligence ratings as covariates produced the same pattern of effects as did our ANOVAs on the public- and private-change measures. That is, only two effects proved significant—the main effect for prior persuasion on the measure of public change, $F(2, 89) = 8.09, p < .001$; and the main effect for prior persuasion on the measure of private change, $F(2, 88) = 3.24, p < .05$.

Discussion

Aside from replicating for a third time the reciprocal pattern of reported attitude change, the results of Experiment 3 enhanced confidence in the robustness of this effect. That is, it appears that not only does this basic reciprocal relationship emerge whether the topic is of low or high personal relevance but whether the arguments received are weak or strong. It was not the case that exposing subjects to powerful arguments from a communicator overwhelmed and rendered insignificant the tendency to reciprocate announced persuasion. In fact, when comparing the size of the reciprocal effects found across our three experiments, it seems that the introduction of strong arguments produced no diminution of effect at all.

⁷ Means for each of the six cells were as follows, with the low-relevance mean presented first in each level of the prior persuasion factor: yield = 3.31 and 1.77; control = 1.51 and 1.51; and resist = 1.69 and 1.06. The Prior Persuasion \times Topic Relevance interaction was nonsignificant, $F(2, 90) = 1.27$. Within the low-relevance conditions, the prior persuasion main effect was significant, $F(2, 90) = 4.15, p < .02$; within the high-relevance conditions, it was nonsignificant, $F(2, 90) = .56$. Tukey tests on the set of six means found only one pair that differed significantly from one another—the means for the low relevance–yield and the high relevance–resist cells.

Of more theoretical interest was the emergence for the first time of corresponding patterns on the measures of public and private attitude change. It appears that subjects exposed to cogent arguments for attitude change were led to overestimate the causal role of those arguments in bringing about their (primarily reciprocation-motivated) public reports of change; consequently, they attributed to themselves amounts of genuine, private change proportional to their tactical announcements of public change. What the results of Experiment 3 may reveal, then, is a case of one classic form of misattribution, wherein individuals whose responding is due to a particular motivational factor are led to mistakenly assign causality for their responses to the action of some other salient and plausible cause (L. Ross, Rodin, & Zimbardo, 1969; Storms & Nisbett, 1970; Valins & Ray, 1967).⁸

Before making too much of this attributional interpretation of our findings, it would be wise to recognize a pair of reasons for withholding strong confidence in it at this point—aside from the fact that argument strength was not manipulated directly within the three studies. First, we have provided no internal evidence that subjects actually found the strong arguments of Study 3 to be more persuasive than the weak arguments of Studies 1 and 2; we relied, instead, on the work of other investigators who validated the strength of these arguments on students at a different university at a different time (see Petty & Cacioppo, 1986). More important, to this point we have provided no evidence for the major premise of our attributional account—that subjects publicly announcing change in response to strong arguments would be more likely to attribute their shifts to the inherent cogency of the arguments, whereas subjects publicly announcing change in response to weak arguments would be more likely to attribute their shifts to social factors.

To provide evidence in each of these arenas, a separate study was conducted with 33 students at ASU who participated for class credit. Each student received a questionnaire containing a set of the three weak arguments concerning comprehensive examinations that we had used in Studies 1 and 2, as well as a set of the three strong arguments we had used in Study 3. After rating each argument in a set on 7-point scales along the dimensions strong-weak, convincing-unconvincing, and persuasive-unpersuasive, subjects were asked to imagine that the set of arguments had been presented to them by a fellow student and that they had announced to the student that they had become more favorable toward comprehensive examinations as a result.

At this point, subjects were asked to rate on a 9-point scale the extent to which their announced change was likely to have been due to argument quality or social factors; the scale was anchored by the statements *I was truly influenced by the arguments' quality* (1) and *I was responding to social factors in the situation* (9). This question was asked both after subjects had rated the strong set of arguments and after they had rated the weak set of arguments; the order of presentation of the strong and weak argument sets was counterbalanced across subjects.

The ANOVAs supported both of the previously untested assumptions of our attributional formulation. First, as expected, the strong arguments were rated as stronger, more convincing, and more persuasive than the weak arguments, all $F(1, 32) > 55.82$, $ps < .001$. Second, subjects attributed more of their conceived public change to the quality of the arguments (and less to social factors) after reading the strong argument set ($M = 3.8$)

than after reading the weak argument set ($M = 6.0$), $F(1, 27) = 15.01$, $p < .001$. It appears, then, that the findings of this study lend additional plausibility to the attributional account of our earlier results; although by no means do these findings ultimately confirm that account, as considerable further support is required before strong confidence can be had. Nonetheless, our preferred attributional explanation offers an array of intriguing implications for future work, as is discussed in the following section.

General Discussion

Despite an absence of prior evidence, it appears from the present program of work that the rule for reciprocity does indeed govern public declarations of persuasion. In all three of our experiments, subjects publicly reported the greatest persuasion from another's arguments on a topic if that other had yielded to the subjects' persuasive attempt on an earlier topic, whereas they reported the least such persuasion if the other had resisted their earlier attempt at influence. Furthermore, this tendency for publicly reciprocated persuasion was powerful enough to occur in a basic form that was unaffected by such factors as perceptions of the persuader's likability and intelligence, personal relevance of the topic under consideration, and strength of the arguments received.

It is worthy of note that in all three experiments, the tendency to reciprocate public attitude change was stronger in the yield conditions than in the resist conditions. In keeping with our focus on the social functions of attitude expressions, we favor an impression-management interpretation of this difference. That is, prior research has indicated that individuals are aware that they will be judged as less intelligent and likable by a persuader if they resist the persuasive attempt (Braver et al., 1977). Thus, it appears that, although they invariably reported the least public change to a persuader who had previously resisted their appeals, our subjects may have tempered the degree of their own stated resistance out of a desire to manage their positivity in the persuader's eyes. Nonetheless, the tendency of resist condition subjects to admit less change than control subjects was a reliable one that proved significant when the respective effects were combined across the three studies, $Z = 2.2$, $p < .03$.

The Indirect Role of Argument Strength in Producing Private Change

The factor of argument strength is worth special consideration, as it provides insight into the circumstances under which purely tactical, public pronouncements of change may become internalized as genuine change. Only when subjects were exposed to powerful arguments on an issue did their privately recorded changes conform to the reciprocal pattern of their

⁸ One potential implication of our attributional account of the private change in Experiment 3 is that the reverse pattern should have occurred in Experiments 1 and 2. That is, resist condition subjects' relative lack of public change in the face of the combination of weak arguments and prior resistance could have led them to attribute the least private change to themselves. Such an attributional pattern seems unlikely, however, given evidence that individuals rarely make confident attributions based on the lack of action (L. Ross, 1977).

publicly reported changes. However, it appears that when the correspondence of public and private change did occur, it came about as a side effect rather than as a direct effect of argument strength. That is, because subjects heard equally cogent arguments from yielding and resisting persuaders, it is not possible to assign subjects' differential private changes simply to the power of the arguments they heard. A more satisfactory, though admittedly still speculative, account of the role of argument strength in our findings is one that gives it the status of a discounting cue.

Recall that in all three experiments, our subjects publicly yielded to a persuader in accord with the norm of reciprocity. However, in Experiments 1 and 2, when the persuader's arguments were inherently weak, the subjects had little basis for assuming that those public changes were anything but simple responses to normative prescriptions; therefore, as is suggested by the results of our questionnaire study, they likely inferred no genuine persuasion from their public declarations of persuasion. This finding is consistent with the results of research showing that individuals who behave in a normative fashion typically will not make internal attributions to themselves on the basis of that behavior if it occurs in public. For example, Cialdini, Eisenberg, Shell, and McCreath (1987) found that elementary school children who made a commitment to help other children came to see themselves as more altruistic if the commitment had taken place in private, but not if it had occurred in public.

In Experiment 3, subjects once again changed publicly in accord with the dictates of the reciprocity norm; but, for the first time in our research program, they had a salient and plausible cause other than reciprocity for that change—the cogency of the persuader's message. Thus, when asked to register their true attitudes in private, subjects could no longer dismiss their public assertions of change as plainly and purely tactical; instead, as our questionnaire study suggests, the situation required them to allow for the causal role of genuine, argument-based persuasion—hence, the high degree of similarity between the patterns of public and private change.

Speculative Implications for the Internalization of Desirable Conduct

Certain insights that can be derived from the present research may be applied to the problem of arranging for individuals to incorporate desirable behavioral tendencies into their self-concepts. One difficulty in getting another to adopt a particular behavior pattern is that extrinsic pressures used to instigate the desired behavior may often backfire. That is, these pressures can undermine one's attitude toward an action as well as one's subsequent performance of it by convincing a person that he or she did not engage in the act for any intrinsic reason (e.g., Fazio, 1981; Lepper & Greene, 1978; M. Ross, 1975). A standard solution suggested for this problem is to minimize the size of the extrinsic pressures designed to instigate the behavior. However, this may not always be feasible or desirable.

For instance, in many situations the target of influence may be unwilling to perform the desired action when low levels of personal, material, or social pressure are applied, requiring an intensification of extrinsic pressure to produce any compliance. The implications of our research suggest that under these

circumstances, an influence agent who felt forced to use such heightened pressures to generate compliance would be well advised to use a discounting cue as well. For example, a supervisor who felt required to "pull rank" to get a subordinate to improve job performance should do so in conjunction with a set of additional reasons for that improvement (e.g., that it is consistent with the subordinate's past behavior or personal traits or long-term interests).

Similarly, a parent who has to assert his or her authority in pressing a child toward prosocial behavior through the use of extrinsic forces should not abandon the use of intrinsic reasons in the process (e.g., "You're a good boy, Timmy, and good boys share"). The implication from our research is that, even if an intrinsic reason is not a sufficient motivator of desired conduct, it may still operate as an internalizer of that conduct by serving as a discounting cue that undermines the perceived influence of the extrinsic cause. Thus, in a reversal of the traditional overjustification effect (Lepper & Greene, 1978), it might be possible to undermine the child's extrinsic interest in an activity by presenting a superfluous but plausible intrinsic reason for it. As long as the intrinsic reason remains present and salient in the situation for the child, it retains the potential, through the process of misattribution, to affect self-view. We would expect this to be especially true if, later, the parent focused on the intrinsic reason as the functional cause (e.g., "I knew you'd share your toys because you're a generous boy").

This focusing process might also be effective in leading adults to make dispositional attributions for their socially desirable actions that have arisen through the external pressures of societal norms. That is, if as Cialdini, Reno, and Kallgren (1990) have argued, norms affect behavior powerfully only when they are strong and salient, it could be counterproductive to reduce the magnitude of these extrinsic influences in situations where normative conduct is desirable. Yet, to the extent that the external normative pressures are sizable, they are more likely to be perceived as causal, and the favored conduct is less likely to be internalized. To avoid this dilemma, an influence agent might plan to focus individuals on normative forces prior to the opportunity for desirable action (e.g., to give blood or conserve energy or preserve the environment) but to refocus the individuals on intrinsic reasons for that action after it is done and is thereby ripe for causal analysis. Research indicating that individuals assign causal weight to those factors that are focal in their attention (see Fiske & Taylor, 1991, for a review) would support the effectiveness of such an approach. Of course, additional work designed to test this speculation directly would be required to provide heightened confidence in it.

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