

Change Mechanisms in Placebo Procedures: Effects of Suggestion, Social Demand, and Contingent Success on Improvement in Treatment

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Investigated the treatment effects of three social influence variables frequently implicated in psychotherapy placebos. Socially anxious male Ss participated in an experimental treatment for reducing dating anxiety. Ss were either given or not given specific suggestions for decreasing social anxiety, placed in conditions of high or low social demand, and received feedback indicating either high or moderate success on the therapy task. Results support the importance of social influence variables in therapeutic change. Contingent success had its greatest impact on personal attributes; suggestion affected skill behaviors; and social demand effects were found in the self-evaluation of heterosocial performance. Various social influences appear to mediate change differently and do not exert the generic effects commonly assumed to be characteristic of therapy placebos. Implications for outcome research are discussed.

Variables associated with placebo effects have received much attention as alternative explanations for what causes therapy to work (Kazdin, 1979), but relatively little is known about how they mediate change (Bootzin & Lick, 1979). Conceptual confusion over the placebo construct has impeded meaningful investigation on the treatment influences of such variables as suggestion, social demand, and persuasion (Crittelli & Neumann, 1984). Wilkins (1986) noted that it is counterproductive for psychotherapy research to dismiss these variables as nonspecific artifacts because they actually constitute and should be studied as legitimate social psychological models of change. The particular social psychological variables implicated in placebo procedures will be referred to as social influence factors because they derive their impact primarily from the social processes between therapist and client rather than from any learning experiences or skill development resulting from treatment.

Research on placebo procedures has focused on the credibility of the treatment rationale (Kazdin & Krouse, 1983), the style by which the treatment is administered (e.g., therapist directiveness or attention), and the type of procedures used to implement the therapeutic tasks (e.g., homework assignments; Jacobson & Baucom, 1977). However, several other social influences may play prominent roles in the efficacy of psychotherapeutic interventions.

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Social Influences in Treatment

Response-Contingent Success

In therapy clients eventually succeed in tasks that require some meaningful effort on their part. Achievements vary from a meaningful insight to the completion of an anxiety hierarchy, but the common ingredient involved is a success experience as a result of one's responses. Proponents of various psychotherapies have assumed that the learning of specific therapeutic content constitutes a necessary and critical agent of change in therapy. An alternative explanation is that this learning may not be crucial, and change is more directly the result of contingent successes wherein clients come to view themselves as self-efficacious in managing their problems. This enhanced sense of control and mastery leads to new and persistent efforts to cope with such problems, which in turn increases the likelihood that an adaptive response will occur (cf. Bandura, 1977). The potential number of therapeutic tasks that clients can successfully perform increases with treatment complexity. This may account for findings in which complex treatments tend to outperform simple ones. Response-contingent success may also explain the superiority of certain treatments over attention placebo controls. Few placebo controls have been designed to control for the number of mastery achievements that a client experiences during treatment. Interventions emphasizing mastery or perceived success experiences have been effective in treating tension headache (Holroyd et al., 1984), acrophobia (Williams, Turner, & Peer, 1985), and heterosocial anxiety (Haemmerlie & Montgomery, 1982). The Holroyd et al. study is particularly impressive because the investigators manipulated perceived success independent of actual biofeedback-induced electromyographic (EMG) change. Significant headache reduction was associated with the former but not with the latter.

Suggestion

There has been much speculation about the role of suggestion in placebo procedures (cf. Bednar, 1970; Trouton, 1957).

Loose conceptualizations of this construct and the lack of empirical studies have precluded delineating the specific nature of its effects in therapy. Council, Kirsch, Vickery, and Carlson (1983) found that a highly credible placebo achieved equivalent degrees of hypnotic responsivity compared with trance and skill induction methods. Their results suggest that hypnotic inductions essentially establish strong expectancies that certain hypnotic responses will occur. It is possible that many placebo procedures involve similar suggestion mechanisms that generate strong response expectancies. Mischel (1973) noted that response expectancies typically become highly discriminative in nature because social outcomes tend to vary across situations. Attention placebo procedures often have not operationalized suggestion influences in psychotherapy. The suggestions generated involve broad, normative behavioral predictions (e.g., therapy will reduce the client's problems in interacting with others). They lack the specificity and situationality needed to exert important effects on behavior. Given these diffuse suggestions in which clients are allowed to project their own idiosyncratic response expectancies, it is not surprising that systematic change seldom has occurred.

Social Demand

Inherent in the psychotherapeutic relationship are subtle social pressures on the client to behave in certain prescribed ways. The type of social demand invoked depends on situational cues available to the client (Rosenthal & Rosnow, 1969), efforts made by the client to ascertain standards concomitant with the role of the "good patient" (cf. Orne, 1969), and opportunities to model after the therapist (Chaves & Barber, 1974). Previous attempts to manipulate social demand compared identical treatment procedures, except that one is administered in the context of an experiment, whereas the other is presented as a therapy (e.g., Miller, 1972; Persely & Leventhal, 1972). However, removal of the treatment context may reduce attention to the training procedures such that the experimental condition bears little phenomenological resemblance to its therapeutic counterpart. Attentional focus is a prerequisite for most types of learning and psychological change (cf. Carver & Scheier, 1981). It is questionable whether clients in a purported experiment attend as closely to the relevant learning tasks as clients in treatment. Rosenfarb and Hayes (1984) were able to vary social demand within the context of treatment by manipulating the public nature of therapy. They evaluated two variants of self-statement modification and informed subjects that the experimenter either knew or did not know which treatment they had received. Regardless of the skill treatment implemented, treatments in the public context were more effective than private context treatments, which did not differ from a public control condition.

In view of the difficulties in previous research of operationalizing social influence variables and the need to more directly examine their effects in a treatment context, this study was conducted. Each social influence variable, contingent success experience, suggestion, and social demand, was operationalized under conditions that more closely approximated their

actual operation in psychotherapy. For example, specific behavioral suggestions for change were made as opposed to diffuse, global ones. Rather than changing attentional focus to study the social demand effect, the social or public nature of the person's progress in treatment was varied. To better account for the actual change processes that occurred in treatment, the investigation assessed manipulation effects, actual success on the therapeutic task, procedural reliability, treatment credibility, and the quality of the client-therapist relationship.

Method

Design Overview

After a pretreatment assessment highly socially anxious college-age men with minimal dating experience were randomly assigned to one of eight conditions in a 2 (high vs. moderate contingent success) \times 2 (suggestion vs. no suggestion of specific behavioral changes) \times 2 (high vs. low social demand) factorial design. The subjects volunteered to participate in a study presented as an experimental treatment for reducing dating anxiety. Social influence variables were varied within the context of subconscious reconditioning, an empirically derived, credible, placebo procedure involving the purported subliminal presentation of antifear messages by means of a tachistoscope. An EMG biofeedback task was included in the treatment to provide subjects with a contingent success experience. Treatment consisted of one 2-hr session. Two to four days after treatment, the posttest was administered, and the subjects were debriefed. If at that time they wished to seek extensive clinical services for their social anxiety, they were referred to the student counseling center. Heterosocial anxiety was selected as the target behavior because it has been validated as a clinically relevant behavior for analog research (Borkovec, Stone, O'Brien, & Kaloupek, 1974; Dow, Craighead, & Borkovec, 1983; Heimberg, 1977).

Subjects

Subjects were 48 nonmarried, male students and university staff members recruited from introductory psychology courses and by announcements in the campus media at the University of California, Los Angeles. Subjects were selected if they indicated that (1) their anxiety often interfered with their dating; (2) this anxiety was a significant concern of theirs whenever they interacted with women; and (3) they were very interested in participating in a program that might decrease heterosocial anxiety. Table 1 lists the relevant subject characteristics of the study sample and comparisons with a university sample that was used in another social anxiety study but that was not screened for high heterosocial anxiety and minimal dating. The results indicate that the experimental subjects in this study constituted a more socially anxious and unskilled sample of the college-age male population. Sample characteristics also closely corresponded to the criteria most frequently used to select highly anxious male subjects for research on heterosexual-social problems (Wallander, Conger, Mariotto, Curran, & Farrell, 1980).

Subjects responding to campus advertisements ($n = 32$) were significantly older than those from psychology courses ($n = 16$), $t(46) = 4.45$, $p < .001$ ($M_s = 24.8$ and 17.9 , respectively), but there were no significant differences on the dating frequency and self-report anxiety measures in separate multivariate analyses of variance. The two groups were combined in all subsequent analyses. Overall, subjects had a mean age of 22.5 years ($SD = 6.0$, range, 17–40), were

Table 1
Subject Characteristic Comparisons of Experimental Sample with University Sample

Characteristic	Experimental		University		<i>t</i> (84)
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	
Dates in 4 weeks	1.10	2.34	5.08	4.65	-4.85***
Dates in 6 months	6.56	10.23	22.38	16.27	-5.22***
Average dates/month	1.12	1.49	6.10	6.52	-4.62***
No. different women	2.19	2.08	6.12	7.78	-3.01**
Amount of dating ^a	2.79	1.56	4.12	1.18	-4.33***
Anxiety on date ^a	5.50	1.58	3.75	1.56	4.98***
Skill on date ^a	3.42	1.51	4.95	1.65	-4.63***
Want to date more ^a	6.52	1.32	5.18	2.00	3.46***
Survey of Heterosexual Interactions	75.44	15.89	94.22	15.33	-5.56***
Social Avoidance and Distress	15.56	6.56	5.97	4.79	7.02***

Note. For each measure, a higher score indicates a greater quantity of that variable.

^a Subjects reported the degree to which they experienced or engaged in each item on a 8-point scale with higher scores reflecting greater intensity or quantity of that variable.

** $p < .01$. *** $p < .001$.

undergraduates (69%), and came from predominantly Caucasian (77%) or Asian/Pacific (15%) ethnic groups.

Measures

Self-report measures. The Social Avoidance and Distress scale (SAD) is a 28-item true-false questionnaire whose items refer to discomfort around people (e.g., "I tend to withdraw from people") and thereby serves as a general measure of social anxiety. The SAD has an internal consistency reliability of .94 and is associated with behavioral indexes of social anxiety and social avoidance (Arkowitz, Lichtenstein, McGovern, & Hines, 1975; Watson & Friend, 1969). It was standardized on a population of college men and women.

The Survey of Heterosexual Interactions (SHI) consists of 20 situation-specific items assessing the subject's ability to initiate heterosexual interactions. For each situation the subjects respond on a 7-point scale ranging from *unable to respond* to *able to carry out interaction*. The scale is appropriate for men only and was developed and normed for a college-age population. The SHI had a split-half reliability coefficient of .85, a 4-month test-retest reliability coefficient of .85, was related to self-report and self-monitoring measures of social anxiety ($r = -.42$ to $-.69$) and social skill ($r = .79$ to $.84$; Twentyman & McFall, 1975), and proved sensitive to changes in treatment (Gormally, Varvil-Weld, Raphael, & Sipps, 1981; McGovern, Arkowitz, & Gilmore, 1975).

The subjective Expectancies Inventory (SEI) measures the utility or one's expectancy that desirable outcomes will occur when initiating heterosexual interactions. For each of the four situations (e.g., starting a conversation with a woman while waiting in line for a basketball game), a respondent provides two affective responses (ranging from $-100 = \text{horrible}$ to $+100 = \text{elated}$) that reflect how he would feel if the woman accepted his overture and if she rejected his overture. Also, he indicates the probability that she would reject him on a 5-point scale (0%, 25%, 50%, 75%, and 100% rejection). The SEI discriminated between anxious and confident men and was reliable ($r = .85$) over a 4-week test-retest period (Gormally, Sipps, Raphael, Edwin, & Varvil-Weld, 1981).

Behavioral measures. Social anxiety and social skill measures were derived from the subjects' responses to two social interactions, an unobtrusive waiting room situation and a pizza parlor role-play. Various molecular and global measures were selected because the specific behavioral components associated with social skill and social anxiety have not been consistently identified. Trained observers rated

both interactions on a modified version of the Timed Behavioral Checklist for Performance Anxiety (Paul, 1966). Global ratings of anxiety and social skill were also made on an 11-point scale (Curran et al., 1982). Subject talk time (in seconds), confederate talk time, facial gaze, hand gestures, you-statements, open-ended questions, and smiles (Conger, Wallander, Mariotto, & Ward, 1980) were recorded.

The judges were trained on 21 practice videotapes from another social anxiety study to a criterion reliability of .90. Twenty interactions (10 waiting room and 10 pizza parlor) were randomly selected and rated by two judges to check the reliability of the global skill and anxiety measures. Intraclass correlations were .89 for social skill and .70 for anxiety. Similar reliabilities were found when waiting room and pizza parlor ratings were analyzed separately (social skill: waiting room, .94, pizza parlor, .91; social anxiety: waiting room, .71, pizza parlor, .73). For behavioral measures involving counts and duration, Pearson correlations were computed between two raters on 12 randomly selected cases. The following reliabilities were found: smiles, .97; open-ended questions, .67; close-ended questions, .99; compliments, .96; facial gaze time, .99; time subject spoke, .99; time confederate spoke, .98. Kappa coefficients were computed for each behavior rated in the Timed Behavioral Checklist and for hand gestures because each measure required the judges to indicate the occurrence or nonoccurrence of that behavior. The following coefficients were found: hand gestures, .83; repetitive hand movements, .93; rigid, restricted hands, .96; repetitive feet movements, .97; repetitive torso movements, 1.00.

Self-evaluation and self-monitoring measures. On completion of the extended role-play, subjects rated how anxious and effective they were in the situation on a 7-point scale ranging from *not at all* to *very*. A daily self-monitoring measure assessed the subject's social behavior outside of treatment but was not used in the subsequent analyses because one-fourth of the sample failed to complete this measure.

Control measures. Measures were used to check the equivalence of experimental conditions on potentially confounding variables in both the assessment and experimental phases of the study. The subjects rated the degree to which they found the confederates attractive, responsive, friendly, and likable for dating on 10-point Likert scales ranging from *not at all* to *very*. Time the confederate spoke during the interaction was also recorded. The subjects also rated the skillfulness and concern of the therapist and whether they felt relaxed and comfortable in session on 5-point Likert scales. The first two items were averaged to reflect a measure of therapist skill, and the last two items were averaged to reflect a measure of session comfort.

They also completed a three-item treatment credibility measure with a 5-point Likert scale format (Holroyd, 1976).

Frontal EMG activity was monitored by electrodes placed above the center of each eye approximately 2.5 cm above each eyebrow. A baseline averaged over a 3-min period was taken after electrodes had been attached and the subjects were comfortably seated before the tachistoscope. Twelve 3-min samples of average EMG activity were recorded during the subsequent 36-min subliminal conditioning period. A subsequent 3-min learning period was recorded in which subjects lowered frontal muscle tension without biofeedback.

It is possible that treatment differences can actually arise from differences in the extent to which therapists have complied with or carried out the therapeutic tasks (Collins, Martin, & Hillenborg, 1982). This source of treatment confounding involves problems in procedural reliability. *Procedural reliability* refers to the extent to which the treatment has been implemented in accordance with the experimental plan. A trained judge rated whether each major procedure delineated by the training manual for the experimenter had been implemented. In a similar fashion the judge rated the procedural reliability of the researchers carrying out the pre- and posttreatment assessments. A procedure was defined as a sequence of behaviors designed to accomplish a particular subgoal of the treatment (e.g., explaining the rationale for using subliminal conditioning or asking for questions to clarify points). Most procedures were easily identified as they often involved adhering to a specific script stated in the either the assessment or treatment manual. Given the relative ease of the rating task, one judge proved sufficient for the procedural reliability ratings, and he was trained to a criterion reliability of .95 by using 20 practice audiotapes obtained from the pilot study. To obtain procedural reliability 12 treatment and 20 (10 pre and 10 post) assessment sessions were randomly selected and rated.

Manipulation checks. The subjects evaluated their performances after the treatment session on two 9-point items assessing (a) how well they did on the biofeedback task and (b) their confidence in being receptive to the subliminal signals. The two items were averaged to provide a measure of perceived success. Perceived social demand was ascertained by averaging two 9-point Likert items that indicated the extent to which subjects believed the therapist knew (a) exactly how they had performed on the biofeedback test and (b) the amount of progress they were making in treatment. The subjects also listed the number of behavioral target areas they could remember after treatment. The number of areas listed (maximum of 8) reflected the extent to which the behavioral suggestions had been retained.

Procedure

Assessments. When the subject arrived for the study, he was met by the experimenter. A moderately attractive woman posing as another subject arrived a few minutes later. Subjects and confederates were sent to separate rooms to complete self-report questionnaires. Next, the experimenter brought the participants together and informed them that the purpose of the experiment was to study social interactions and that they would be role-playing a certain social interaction. The type of interaction to be role-played would be selected on the basis of their concerns indicated in the questionnaires they had just completed. Thus, they had to wait for a few minutes while the experimenter reviewed their questionnaires to select an appropriate social interaction. The ensuing 4-min waiting period was videotaped by a hidden camera.

Confederates were trained to: (a) initiate interactions by asking one of seven sequentially ordered questions following any silence of 30 s, (b) limit their response duration to approximately 10 s, and (c) maintain moderately attentive, nonverbal behavior (e.g., occasional eye contact during silences and frequent eye contact during verbal

interactions). This situation was selected because of its concurrent validity under both unobtrusive and obtrusive conditions (Kern, Miller, & Eggers, 1983). The deceptions apparently were successful. In standardized debriefings 96% of the subjects indicated that they thought the confederate was another subject, and 94% did not know the waiting room was being videotaped. After the waiting room situation, subjects and confederates were instructed to interact in a 4-min extended role-play. The role-play involved conversing in a pizza parlor while on a first date. The subject's task was to acquaint himself better with his date. Posttreatment assessment was identical to the pretreatment session except that the subject interacted with another confederate. The same two confederates participated in all assessments. Two female research assistants conducted the pre- and posttreatment assessments. Assessment research assistants, confederates, and raters were unaware of the purpose of the study and the condition to which each subject had been assigned.

Experimental procedures. Studies have shown that the subconscious reconditioning placebo (SRP) is perceived by subjects to be as credible as systematic desensitization and cue-controlled relaxation (Lent, Crimmings, & Russell, 1981). SRP was also as effective as the other treatments in reducing smoking (Sipich, Russell, & Tobias, 1974), test anxiety (Russell & Lent, 1982), and speech anxiety (Lent, Russell, & Zamostny, 1981). The placebo rationale emphasized that social anxiety is often caused by subconscious thoughts and feelings. Subjects were told that the treatment would be directed toward reconditioning the subconscious by presenting antifear messages at subliminal levels. In actuality, these messages were nonsense syllables that appeared to be words when flashed at high speeds with a tachistoscope. To enhance the treatment's perceived application to social anxiety, the treatment focused on "reconditioning" progressively more anxiety-provoking situations. Subjects were told that eight specific behavioral areas had been selected on the basis of their assessment data, but the same behavioral targets were used for all clients. All clients received the same pattern of progress up the behavioral-target hierarchy. Consequently, subjects felt they were working directly on social anxiety difficulties with a systematic and progressive approach.

Biofeedback task. A key methodological problem for the development of placebo procedures has been the lack of tangible tasks so that subjects can experience response-contingent successes. The subjects were told that because reconditioning involves the processing of visual stimuli, a person can become more responsive to subliminal presentation when they can reduce competing, visual distractions. They also were informed that as a person focuses and reduces visual distractions, it has been found that muscle tension around the eye decreases. With the aid of biofeedback subjects would learn to decrease such tension, thereby, maximizing their responsiveness to treatment. Within the context of this rationale, the following experimental manipulations were implemented.

Contingent success. Bogus graphic displays of EMG activity were developed. Two graphs were displayed together and were shown to the subjects to provide them with feedback of their performance on the biofeedback task. One graph showed dramatic reductions in EMG activity in each progressive 3-min block. The other graph showed relatively small reductions across blocks. During the initial blocks the two graphs overlapped somewhat but progressively diverged as treatment continued. Subjects in the high success condition were told that the graph displaying large reductions reflected their performance, whereas the graph displaying smaller reductions reflected the average performance of other subjects participating in the study. Subjects in the moderate success condition were told the opposite. The graph showing the smaller reductions was attributed to their performance. Moderate success subjects were also informed that whereas the graph indicated that they have achieved some tension control, their performance was below that of the average subject. The moderate success

feedback was designed to highlight the subject's poor performance but not discourage him so much that he failed to remain involved in the study (Holroyd et al., 1984).

Suggestion. The suggestion factor was varied by the presence or absence of specific behavioral changes associated with greater social skill and lower social anxiety. In the suggestion condition the specific behavioral target (e.g., smile and make eye contact) on which the subject was working was constantly shown on the screen in between tachistoscope flashings of the purported subliminal messages. The experimenter also announced the target behavior before switching slides. Thus, these subjects were always looking at and hearing about one of the eight behavioral changes associated with better heterosocial performance. In the no-suggestion condition subjects simply saw a lettered sequence (e.g., Sequence A) denoting the fact that they were working on one of the target behaviors in the hierarchy.

Social demand. Social demand was varied by having subjects review their progress in a public or private context. In the high social demand condition, (a) the experimenter explicitly told the subjects that he would be monitoring their progress on the biofeedback task; (b) the subjects reviewed their EMG performances with the experimenter conspicuously present at their side explaining the results; and (c) the experimenter asked the subjects for their graphs because the researchers were interested in determining if the subjects' receptivity as indicated by their progress in session matched how they did at posttreatment. Low social demand condition subjects were not told the experimenter was monitoring their progress, reviewed their graphs in private, and were allowed to keep their graphs. An information sheet explained the graphs to low demand subjects by providing clear examples of each type of possible outcome. Pretesting indicated that the information sheet was equivalent to the oral presentation of results in comprehension.

Two male experimenters (Nolan W. S. Zane and a research assistant) conducted the treatment sessions. Both experimenters and assessment research assistants followed procedural manuals that included explanatory outlines and scripts for administering the treatment or assessment procedures.¹

Results

Except where indicated, experimental effects were analyzed by a set of three-variable (Contingent Success \times Suggestion \times Social Demand) multivariate analyses of variance (MANOVA) or covariance (MANCOVA with pretreatment scores as the covariates). If the multivariate analyses revealed significant effects, three-variable univariate analyses of variance (ANOVA) or covariance (ANCOVA that included all covariates used in the given MANCOVA) were used to interpret the findings. This multivariate adaptation of Fisher's protected t test guards against inflated setwise Type I error rates and also keeps both F and t tests relatively powerful (Cohen & Cohen, 1975). To better examine different aspects of treatment outcome, the outcome measures were grouped into the following sets for analysis: (a) self-report measures, (b) self-evaluation measures, (c) global and behavioral measures of social anxiety in the waiting room, (d) global and behavioral measures of social skill in the waiting room, (e) global and behavioral measures of social anxiety in the pizza parlor, and (f) global and behavioral measures of social skill in the pizza parlor. In this way, the critical outcome evaluations were reduced from 25 to only 6 multivariate tests of significance to minimize the Type I experimenterwise error rate. EMG data were analyzed

with a four-variable repeated measures ANOVA (3 Experimental Variables \times Trials) with trials as the repeated measure.

Experimenter and Confederate Differences

Separate four-variable (Experimental Variables \times Experimenter) MANOVAS were conducted on therapist skill, session comfort, and the six sets of outcome measures to determine if experimenters differentially affected the treatment experience. No main or interaction effects involving the experimenter variable were found. Because one confederate was used slightly more than the other in the pretreatment role-plays (60% vs. 40%), separate four-variable MANOVAS (3 Experimental Variables \times Confederate Order) were conducted on confederate attractiveness and responsiveness at both pre- and posttreatment. No significant differences emerged. Similar null effects were found both at pre- and posttreatment on the outcome measures and time confederate spoke. Subsequent analyses were collapsed across both experimenter and confederate variables. Overall, the subjects found the confederates to be in the moderate range (4 = *somewhat* to 6 = *moderately*) on attractiveness ($M = 6.45$, $SD = 1.25$), desirability for dating ($M = 5.75$, $SD = 1.86$), responsiveness ($M = 5.90$, $SD = 1.29$), and friendliness ($M = 6.44$, $SD = 1.17$). It appears that confederates were perceived as intended and followed the response guidelines set fourth in their training.

Pretreatment Equivalence Among Conditions

Separate MANOVAS on dating behavior, self-report, self-evaluation, waiting room anxiety, waiting room skill, pizza parlor anxiety, and pizza parlor skill measures found no significant differences among experimental conditions ($ps > .09$). Thus, experimental conditions were equivalent at pretreatment.

Control Measures

Electromyographic activity. It was assumed that if the biofeedback task was meaningful and subjects were involved in it, significant reductions in EMG activity would occur. At the same time it was important that these reductions be independent of the experimental manipulations, otherwise, they would constitute a confounding source of outcome variance. The four-variable repeated measures ANOVA found a significant main effect for trials, $F(13, 28) = 5.28$, $p < .001$. No other main effects or interactions were significant. Subsequent t tests revealed that at all training trials (M range, 1.20–1.39, SD range, 0.51–0.83) and at the learning trial ($M = 1.22$, $SD = .63$), EMG activity was significantly lower than it was at baseline ($M = 1.77$, $SD = .88$), $ps < .001$. Among training trials there was a tendency for trials later in treatment to have lower EMG levels than trials earlier in treatment. Three-variable ANCOVAs with baseline EMG as the covariate

¹ Copies of treatment and assessment procedure manuals as well as the assessment instruments can be obtained from Nolan W. S. Zane.

were performed on EMG activity during training (averaged across all 12 training trials) and at the learning trial. Both analyses found no significant effects. Results strongly suggest that subjects engaged in a meaningful task during treatment, achieving significant reductions in EMG activity during the session. There was also no evidence to indicate any actual performance differences among experimental conditions.

Ratings. A three-variable ANOVA on credibility revealed no significant effects indicating that the experimental conditions were perceived as equally credible. The mean credibility rating was 3.71 ($SD = 0.67$) on a 5-point scale, which suggests that subjects found the treatment to be moderately credible. Likewise, no significant effects were found for session comfort; subjects reported being relaxed and comfortable in session ($M = 3.95$ on a 5-point scale, $SD = 0.61$). An ANOVA conducted on therapist skill approached significance for the suggestion variable, $F(1, 40) = 3.95, p < .06$. Contrary to expectations, subjects who received suggestions tended to perceive the experimenter as less skillful ($M = 4.33, SD = 0.55$) than subjects receiving no suggestions ($M = 4.62, SD = 0.47$).

Procedural reliability. On the average, 92% of the nine assessment procedures were implemented. A three-variable ANOVA conducted on reliability rate found no significant experimental effects. Treatment session reliability was 89% (of 20 procedures), and all experimental manipulations were invoked. Most of the incomplete procedural implementations in treatment involved not asking for questions after an explanation and not repeating certain instructions. No significant effects were found by the three-variable ANOVA on treatment session reliability. The findings indicate that both experimenters and assessment research assistants implemented the respective procedures as planned. Moreover, procedural reliability across experimental conditions was equivalent.

Manipulation Checks

Subjects receiving high success feedback rated themselves as more successful ($M = 4.00, SD = 0.72$) than subjects receiving moderate success feedback ($M = 3.40, SD = 0.61$), $F(1, 40) = 9.76, p < .01$. The three-variable ANOVA found no other effects on the success measure. High social demand subjects felt that their therapist knew significantly more about their progress ($M = 4.06, SD = 0.84$) than low social demand subjects ($M = 3.44, SD = 0.81$), $F(1, 40) = 6.68, p < .05$. No other experimental effects were found on the demand measure. Subjects who received suggestions recalled almost twice as many behavioral directives ($M = 6.02, SD = 1.75$) compared with subjects who received no suggestions ($M = 3.29, SD = 1.83$), $F(1, 40) = 26.86, p < .001$. No other experimental effects were found on the suggestion measure. Thus, it appears that the experimental manipulations were successful and effectively dissociated from one another.

Outcome Measures

Table 2 lists the pre- and posttreatment means of outcome measures for the total sample. Subjects across experimental conditions made significant changes in many outcome domains. Reliable gains were apparent in self-reported personal

attributes, self-evaluations of social performance, and observed social skill in the less obtrusive waiting room interaction. However, there was no evidence of significant treatment change for observed anxiety in either interaction setting.

Self-report. A three-variable MANCOVA revealed a significant main effect for contingent success, $F(3, 35) = 2.85, p = .05$, but found no other main effects or interactions. As shown in Table 3 subsequent univariate tests indicate that subjects who received high contingent success experiences significantly differed from those who received moderate contingent success experiences on all three self-report measures. The former reported feeling more confident in interacting with women, less anxious around people, and anticipated better outcomes when making social overtures to females than the latter. It appears that contingent success produced significant treatment gains in self-reported personal attributes.

Self-evaluation. The multivariate analysis revealed a significant three-way (Contingent Success \times Suggestion \times Social Demand) interaction, $F(2, 37) = 4.72, p < .05$. ANOVAS found a significant three way interaction for self-evaluated anxiety (following a heterosocial interaction), $F(1, 38) = 9.64, p < .01$, but not for self-evaluated skill, $F(1, 38) = 0.94$. Simple effects analyses involving tests of significance between two adjusted means (Edwards, 1972) indicated that under conditions of low social demand, high success subjects receiving suggestions (adjusted $M = 3.31$) felt significantly less anxious than high success subjects receiving no suggestions (adjusted $M = 5.57$), $t(38) = 2.27, p < .05$. However, the reverse is true for moderately successful subjects. Moderate success, no-suggestion subjects (adjusted $M = 3.94$) felt significantly less anxious than moderate success, suggestion subjects (adjusted $M = 6.41, t(38) = 2.46, p < .05$). Under conditions of high social demand, there were no significant simple effects for the two-way interaction.

Observed anxiety. No significant experimental effects were found with MANCOVAs on observed anxiety measures for either the pizza parlor or waiting room situation. As indicated in Table 2, there were no appreciable changes in observed anxiety from pre- to posttreatment.

Observed skill. Significant changes were evident on the skill measures. Although subjects showed significant gains on social skill behaviors in the pizza parlor situation (see Table 2), these could not be attributable to social influence variables as the multivariate analysis found no significant experimental effects. The MANCOVA performed on measures of social skill in the waiting room revealed a significant main effect for suggestion, $F(9, 23) = 2.75, p < .05$. ANCOVAs indicated that this effect was primarily due to significant differences on two variables, subject talk time and smiles. The subjects who received behavioral suggestions spoke for a longer period of time to their female partner and smiled more frequently than the subjects who did not receive such suggestions (see Table 3). On the average, the subjects given suggestions spoke 29% longer than their no-suggestion counterparts.

Discussion

The results of this study support the importance of directly investigating social influence variables as mediators of thera-

Table 2
Pre- and Posttreatment Means on Outcome Measures for Total Sample

Outcome Measure	Pre		Post		<i>t</i> (47)
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	
Survey of Heterosexual Interactions	75.44	15.89	83.06	16.08	-3.57**
Social Avoidance and Distress	15.56	6.56	12.48	7.32	3.69**
Utility	1.31	1.90	1.62	1.94	-1.19
Self-evaluated anxiety	6.81	2.35	5.06	2.14	5.23***
Self-evaluated skill	4.04	1.75	5.56	1.60	-6.39***
Waiting room situation					
Observed anxiety	5.17	1.33	4.87	1.28	1.36
Timed Behavioral Checklist	13.10	7.19	13.33	8.05	-0.19
Observed skill	4.92	1.64	5.50	1.70	-2.31*
Smiles	4.50	2.76	5.40	2.75	-2.04*
Open-ended questions	.54	.92	1.67	1.29	-3.25**
Close-ended questions	6.60	3.67	7.65	3.78	-1.59
Compliments	1.62	1.58	1.88	1.77	-1.09
You-statements	6.23	3.34	7.98	3.44	-2.95**
Gaze	80.21	43.26	110.92	45.83	-4.15***
Subject talk time	107.23	34.90	115.46	39.95	-1.46
Pizza Parlor Situation					
Observed anxiety	4.88	1.23	4.73	1.32	.71
Timed Behavioral Checklist	13.31	7.57	12.42	8.54	.65
Observed skill	6.17	1.49	6.21	1.40	-.18
Smiles	5.77	2.70	6.17	2.93	-1.06
Open-ended questions	1.27	1.81	2.19	1.79	-2.78**
Close-ended questions	7.19	3.72	7.88	3.98	-1.06
Compliments	2.33	1.89	2.40	1.67	-.19
You-statements	7.29	3.44	8.40	3.55	-1.85
Gaze	130.58	47.86	151.46	49.54	-2.81**
Subject talk time	126.67	30.82	125.00	37.04	.34

Note. For each measure a higher score indicates a greater quantity of that variable.

* $p < .05$. ** $p < .01$. *** $p < .001$.

peutic change. Significant improvements in outcome were associated with contingent success and suggestion, whereas social demand was involved in an interaction of the three factors. These changes were not directly confounded with time, retesting, and actual performance effects. Moreover, the specificity of these effects lends empirical support to the argument (cf. Critelli & Neumann, 1984; Kirsch, 1978) that influences implicated in placebo procedures do not have

similar, generic effects. Contingent success had its greatest impact on personal attributes; suggestion affected skill behaviors; and social demand effects were found in the self-evaluation of heterosocial performance. Different social influence variables mediate change in somewhat different ways.

Some of this specificity may be attributable to the particular manner by which an experimental variable was operationalized. Contingent success experiences were linked to more

Table 3
Pre- and Posttreatment Means on Outcome Measures with Significant Experimental Effects

Experimental variable	Level of experimental variable						<i>F</i> ^b
	High			Low			
	Pre		Adjusted post ^a <i>M</i>	Pre		Adjusted post ^a <i>M</i>	
	<i>M</i>	<i>SD</i>		<i>M</i>	<i>SD</i>		
Contingent success							
SHI	71.12	14.92	88.22	79.75	15.95	77.91	6.07*
SAD	17.04	6.76	10.66	14.08	6.12	14.30	4.23*
Utility	1.31	1.57	2.29	1.31	2.21	0.96	6.79*
Suggestion							
Smiles	4.38	2.52	6.63	4.62	3.03	4.16	11.12**
Subject talk time	106.00	33.95	129.13	108.46	36.52	101.79	8.05**

Note. For each measure, a higher score indicates a greater quantity of that variable. SHI = Survey of Heterosexual Interactions. SAD = Social Avoidance and Distress scale.

^a Adjusted for all covariates used in the corresponding multivariate analysis of variance. ^b For contingent success effect, $df = (1, 37)$; for suggestion effect, $df = (1, 31)$.

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

global types of expected improvement (e.g., decrement in initial anxiety, more relaxed so that the subject can communicate better with women), whereas suggestions presumably created expectancies for molecular behavior changes. Specific behavioral directives were used here because they have been most consistently linked with suggestion-induced therapeutic changes in previous studies. Perhaps, a more interesting alternative would have been the use of suggestions invoking changes in personality attributes or self-constructs. Having each social influence factor targeted for change at the same level of behavioral specificity may have resulted in a more sensitive examination of possible interactions among these variables. Social demand was involved in a complex three-way interaction. Subsequent research is needed to replicate this finding because the interaction was confined to only one outcome measure. Suffice it to say, some evidence suggests that social demand mediates change in treatment, although the role of this type of social influence is still unclear.

Anxiety reduction was one primary goal of treatment, but no significant reductions in observed anxiety were associated with experimental effects in the two heterosocial interactions. In fact, observed anxiety measures showed no appreciable change from pre- to posttreatment. These findings are in contrast to changes observed for skill ratings and self-reported anxiety. It may be that subjects' anxiety actually did not change and that skill and self-report changes were simply more susceptible to demand characteristics of the experiment. This seems unlikely in view of the lack of significance for the social demand effect on these latter measures. A more plausible explanation is that subjects may actually experience a decrease in anxiety, but this decrease is not reflected in anxiety-related behaviors such as nervous hand and leg movements. Such behaviors may have become so habitual that they persist for some time after a change in experiential state. Hodgson and Rachman (1974) observed how desynchrony is often found between self-report and behavioral measures of anxiety.

Replications are necessary because of the analogue nature of the study. Treatments were only moderately credible, probably because the intervention was brief and the treatment rationale was delivered under nontherapeutic conditions. Although subjects were similar to persons typically involved in social anxiety outcome research, it is questionable whether they constitute a clinical sample. However, these aspects may actually enhance the significance of the findings. Social influence effects associated with placebos appear most potent when treatment is conducted under highly credible conditions with patients who are in an acute state of distress (Frank, Nash, Stone, & Imber, 1963). When such effects are still found in a moderately credible, nontherapeutic context with moderately distressed subjects, it seems reasonable to assume that such influences are also important in therapy. Added to this is the fact that the study was conducted on social anxiety, a problem considered to be impervious to general demand and retesting effects (Borkovec et al., 1974).

A more serious limitation is the brief treatment period used in the design. It is unclear if such social influences persist over longer periods. Possibly, as suggested by Frank (1974), social influence variables are crucial in the early stages of treatment

to get clients to renew their coping efforts and reduce their avoidance behaviors. As treatment proceeds such variables may become less critical than certain skills, insights, or cognitive shifts that result from core learning experiences instigated by therapy. On the other hand, for certain clinical problems such as simple phobias social influence variables may be sufficient (Lick & Bootzin, 1975). The effects found were reliable from a statistical standpoint, but replications are required to determine if more powerful and prolonged interventions that highlight social influence variables can actually produce results of sufficient magnitude to be clinically significant as well. Finally, it is unclear how gender differences may mediate social influence effects because only men were studied.

What follows are certain methodological refinements implied by the study's findings that could prove important to outcome research. These may be considered tentative pending replications that address the previously cited limitations. There is a need to design placebo control procedures that represent specific social influence variables. The findings suggest that different social influences affect treatment outcome in different ways. Thus, care should be taken to determine which social influence variables a particular placebo procedure operationalizes. For instance, does the placebo manipulation represent demand for improvement or a suggestion variable or both? The assumption that one experimental condition can automatically account for all social influence variables is no longer tenable. It is based on the misconception that placebos are nonspecific influences. Rather, they are specific social influences that tend to be shared by most therapies (Frank, 1959, 1961). Outcome therapy designs can only study a limited number of such influences at one time.

At the same time, there is no need to control for all social influences typically associated with placebo procedures. Instead, placebo procedures ought to be directly derived from a particular treatment approach. Each procedure must reflect social influence variables that constitute the most plausible alternative explanations given the particular model of treatment change under investigation. Placebo conditions are no longer "controls" but represent legitimate rival change agents. In this way, selection of the specific social influence variables for study is directly tied to theory development. From a methodological standpoint the operationalization of placebos as social influence variables rather than nonspecific artifacts rebalances the technical tasks with respect to identifying viable change mechanisms in psychotherapy. Similar to the study of treatment variables that represent unique learning experiences, social influence variables implicated in a placebo procedure must be specified, operationalized, and empirically tested. In this manner, the social influence model is subjected to the same tests of falsification applied to hypotheses based on other explanatory constructs in outcome research.

Response contingent success proved to be an important mediator of personal construct change in this study. However, outcome studies investigating change mechanisms seldom have controlled for this variable. Note that contingent success experiences cannot be independent of effective treatments. If a treatment is successful, the learning of adaptive coping skills or crucial insights inevitably involves contingent success ex-

periences. As Kazdin (1979) pointed out, the task here is not to demonstrate that theory-specific learning operates independently of social influence variables but to identify which aspects of treatment actually contribute to change. Holroyd et al.'s (1984) evaluation of change in tension headache treatment represents a good example of how to directly account for contingent success experiences in outcome research. Similar approaches in other clinical areas are needed to refine the evaluation of critical change mechanisms in treatment.

In any research associated with placebo effects, ethical issues must be considered. Many of the ethical concerns raised about psychotherapy placebo research are based on a misunderstanding of the psychotherapy placebo as an inert treatment. As noted by several investigators (Bootzin & Lick, 1979; Wilkins, 1986) and found in this study, the social psychological variables implicated in psychotherapy placebos are not inert or ineffective. Recognizing that these factors have real therapeutic effects, Critelli and Neumann (1984) stressed the importance of distinguishing between social influence variables and other variables that have traditionally been associated with placebo procedures such as measurement artifacts (e.g., practice effects in testing). When this distinction is made, it becomes clear that serious ethical questions have occurred more in response to the measurement artifact aspect of placebo procedures.

However, several ethical considerations remain important. First, this type of research still involves deception, and subjects must be carefully debriefed. Besides offering additional treatment, this investigation had research assistants directly inquire about any distress experienced by subjects on discovering the actual intent of the experiment. The investigator, a trained clinician, recontacted subjects who the assistants felt were still distressed after the debriefing; this was necessary for 1 subject. Second, it is still unclear how relatively effective and long-lasting social influence effects are in psychotherapy. It was for this reason that a clinical analogue was used rather than a complete treatment. Because the change parameters of social influence variables remain to be established, future investigations must carefully consider the possible adverse effects on clients of a prolonged treatment on the basis of these influences against the social responsibility to critically evaluate interventions in such a manner that the useful ingredients of a particular psychotherapy become known.

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