Cognitive Therapy and Research, Vol. 2, No. 3, 1978, pp. 255–264

The Placebo Effect and the Cognitive-Behavioral Revolution

Irving Kirsch[1]
University of Connecticut

Definitions of the term placebo *are considered in relation to the new cognitive-behavioral trend in psychotherapy and recent research on the differential effects of various placebo procedures. Existing definitions either lead to the inclusion of procedures that are not generally regarded as placebos (e.g., systematic desensitization and rational emotive therapy) or result in an empty set. Thus the placebo construct, while valid in drug research, may not be meaningful in relation to psychotherapy outcome studies. Control conditions should be designed for more specific purposes than controlling for* the *placebo effect. In addition, research directed toward specifying the laws governing the effectiveness of manipulations termed placebos is strongly urged.*

In 1960 Hebb predicted a cognitive revolution in American psychology. In the 1970s the revolution was proclaimed to have occurred (Hebb, 1974; McKeachie, 1976; Mahoney, 1977). While the dawning recognition of a new general direction for a field may be widely welcomed, it also presents new problems. As Kuhn (1970) has suggested, while a scientific revolution may resolve many perplexing problems, it also makes many prior solutions newly problematic. We may be forced to reconsider the meaning of terms that we previously thought we understood. The purpose of this paper is to explore one such problem: The meaning of the term *placebo.*

THE PLACEBO AS A "NONSPECIFIC" FACTOR

In physical medicine, the definition of the term *placebo* is quite straightforward. A placebo is a chemically inert substance that works by

[1]Address all correspondence to Irving Kirsch, Department of Psychology, U-20, University of Connecticut, Storrs, Connecticut 06268.

0147-5916/78/0900-0255$05.00/0

virtue of its presumed psychological effect. This definition seems to work well enough for the purpose of studying the effects of medicines, but if we attempt to apply it to the field of psychotherapy (including behavioral and cognitive-behavioral varieties) we immediately are confronted with a problem. Almost all psychotherapies are chemically inert and are presumed to work by means of psychological mechanisms. Are we then to conclude that all psychotherapy is by definition a placebo? This problem has previously been recognized and has apparently been solved by a redefinition of the term. Shapiro (1960, 1971) has suggested that a placebo may be distinguished from a "genuine" treatment by virtue of the former being "nonspecific." This new definition seems to have been widely accepted, but there have been differences in the interpretations given to the term *nonspecific.* Do we mean that the placebo's mechanism—its means of producing an effect—is nonspecific (see Davison & Neale, 1974), or do we mean that its effect, no matter how specifically produced, is nonspecific, i.e., that its action is so broad that any of a number of effects may be produced (see Ullman & Krasner, 1969)? Let us examine the consequences of each of these interpretations in turn.

To adopt the first interpretation at face value is to assume that the mode of action of a placebo is indeterminate. However, this violates a fundamental assumptive strategy of science. As scientists we assume that all events are completely determined until we are forced by empirical evidence to conclude that there is some specifiable limit to determination that we *in principle* cannot surpass (as in quantum physics). Until such a limit is specified, we assume that any failure in our ability to specify the determinants of an effect is a more or less temporary consequence of our current methods of observation. Certainly we have not reached the stage of sophistication in psychology where we are able to mathematically define the limits of determinacy. It would therefore seem reasonable to take the more traditional scientific stance and to assume that our current inability to fully specify how a "placebo" produces its effects is a temporary practical limitation, and not an intrinsic property of the placebo itself.

Let us carry this argument one step further. If the mode of action of a placebo is indeed nonspecific, then in terms of any given effect, one particular placebo should be equivalent to another. In fact, up until a few years ago, this was an implicit assumption in our research strategies. We spoke (and occasionally speak) of controlling for *the* placebo effect. One may have inquired whether a placebo control group was employed in a specific study, but until Borkovec and Nau (1972) published their results, one seldom inquired as to the adequacy of the specific placebo used. We now know that one placebo is not necessarily the same as another; that different placebos may produce differential effects on the same target behavior

(McReynolds, Barnes, Brooks, & Rehagen, 1973). In addition, we have been able to identify at least one specific component (rationale credibility) that has been shown to be responsible for producing differential effects. Kirsch and Henry (1977) found that credibility ratings made prior to the administration of treatment accounted for 36.5% of the variance in self-reports of treatment helpfulness across three treatment conditions. Thus, if we define a placebo as a manipulation for which the effects are not dependent on any specific component, then there is no procedure that could conceivably qualify as a placebo.

Presumably recognizing the problems inherent in assuming that there are a set of procedures, for which the mode of action is intrinsically nonspecific, some writers have used the term in a more relativistic sense. A placebo, viewed from this perspective, is a treatment that is not currently understood (see Mahoney, 1974). We assume that someday we will discover specifically how a given placebo works, and at that point it will cease being a placebo and will become a therapy. Unlike a rose, a placebo by another name smells much sweeter. Ultimately, we are told, the laws governing the action of all behavioral procedures may be known, at which point the term *placebo* will be a historical curiosity. Indeed, there seems to be some empirical support for this position, as we are constantly reminded that yesterday's placebo sometimes does become today's treatment. It is interesting to note, for example, the striking similarities between the "high credibility placebo" of McReynolds et al. (1973), on the one hand, and Maultsby's (1975) description of rational self-counseling on the other. Subjects in the "dissonance enhancement" condition were presented with a rationale based on Festinger's cognitive dissonance theory, instructed to focus on the irrationality of their fears—especially the contradiction between their rational thoughts (e.g., "snakes are harmless") and irrational feelings (e.g., "I am afraid")—and were provided with imagery exercises supposedly "intended to enhance the interface between thoughts and feelings" (McReynolds et al., 1973, p. 88). Maultsby (1975) also begins with the notion of cognitive dissonance, instructs subjects to focus on the irrationality of their negative feelings, and provides them with imagery exercises to resolve the dissonance in the direction of rational thoughts. A major distinction between these two procedures is that McReynolds's manipulation employs imagery content that is neutral with respect to the specific fear. In this connection, however, it is useful to recall Goldfried and Goldfried's (1977) demonstration that when counterphobic treatments are presented as a self-control skill, relevance of imagery content may not result in differential outcome.

In defining placebo as a treatment that is not currently understood, it must be assumed that we currently understand the mode of action of our existing "treatments"; otherwise we would be forced to consider them

placebos. Now if "understanding" is to be judged by some degree of professional consensus, then we must concede that as of today all of our methods of treatment are, according to this definition, placebos. Behaviorists and communication theorists, for example, would not agree with psychoanalysts that analysis obtains its effect by making the unconscious conscious. Nor would they agree with each other as to the most appropriate alternative hypothesis.

Disagreement of this sort may also be found within a given professional community. In the early days of behavior therapy, behavioral treatments were presumed to work on the basis of known "principles of learning." But can we make the same claim today? Let us take systematic desensitization as an example. It is, after all, one of the oldest, most respected, and most extensively researched behavior therapy techniques. At first, we were told that desensitization worked by means of reciprocal inhibition (Wolpe, 1958). It was then discovered that neither a graded hierarchy nor relaxation training was necessary to a successful outcome (beginning with Wolpin & Pearsall, 1965; Wolpin & Raines, 1966). Since both of these had been posited as necessary conditions, the reciprocal-inhibition hypothesis became increasingly untenable. As a result, some investigators turned to an extinction model, positing "nonreinforced exposure" as the necessary and sufficient condition for successful desensitization. However, Kirsch and Henry (1977) have demonstrated that desensitization may retain its effectiveness even when exposure is reinforced by aversive stimulation. Subjects in their "operant desensitization" condition underwent a treatment that was identical to systematic desensitization except that visualization of scenes from the hierarchy was paired with a painful electric shock, thus violating the requirement for nonreinforced exposure. The improvement of subjects in this condition was not significantly different from that of subjects in standard systematic desensitization (nor from that of subjects in an equally compelling placebo condition). Improvement for subjects in all three of these conditions was significantly greater than that of no treatment, minimal treatment, and simulation controls. A variety of other explanations have been proposed for systematic desensitization, but we are far from reaching a consensus with respect to any of them. Thus the specific mode of action of systematic desensitization is currently unknown. Shall we therefore maintain that desensitization is a placebo (although with further experimental investigation it may someday attain the status of a treatment)?

Similar doubts may be raised with respect to other established behavior therapy procedures. For example, aversion therapies are presumed to be effective by means of establishing a conditioned anxiety response. There are at least two studies which tend to cast doubt on this explanation.

Evans (1968) administered aversive electric shock to snake-phobic subjects while they were imaging contact with a snake. Inasmuch as this procedure was virtually identical to aversion-therapy procedures, one would expect it to strengthen the anxiety response to snakes. However, on the posttest, subjects exhibited significantly greater approach to the snake and reported a significant decrease in anxiety. A second study that leads one to doubt the popular conditioning explanations of aversion therapy was reported by Hallam, Rachman, and Falkowski (1972). Their "successfully" treated subjects typically reported indifference rather than anxiety as the emotion elicited by the "conditioned stimulus" following electrical aversion therapy. Thus, if our current ability to consensually understand how a procedure attains its effect is used as a criterion, we may be forced to conclude that all therapies, including behavior therapies, are placebos.

It may be argued here that the insistence on consensus is not necessary. Perhaps the treatment/placebo distinction may be saved if we require of a "genuine" treatment only that some group of professionals, even a relatively small group, have a theory as to the means by which a manipulation obtains its effect. If no such explanation is proffered, the procedure is a placebo. However, under this criterion, what is the status of a procedure that is deemed to achieve its effects by altering beliefs and expectations, as is the case not only of our current conceptions of placebos but also of rational emotive therapy, Beck's (1976) cognitive therapy, Kelly's (1955) therapeutic goal of establishing more convenient personal constructs, and Rotter's (1954) emphasis on altering client expectancies, to name just a few? Since this problem involves a definition of placebo different from that under consideration here (the placebo as an expectancy manipulation rather than a nonspecific procedure), it is considered in greater detail below.

At this point, let us turn to the second interpretation of a placebo as a nonspecific agent. According to this interpretation, although the method of action may be specific, the effect is nonspecific. The same placebo ought to be equally suitable for decreasing anxiety as for increasing it. We also ought to be able to use it to decrease smoking, produce weight loss, eliminate insomnia, enuresis or inappropriate verbal behavior, or to produce any other desired behavioral effect. Of course, in this sense, most of our placebos (unlike the sugar pills of medicine) are not at all nonspecific. We intentionally design our placebos to be specifically credible in relation to the specific target behavior we are concerned with. The existing research on treatment credibility suggests that it would be folly to do otherwise. Thus, by this interpretation, what is coming to be recognized as an adequate placebo control is not a placebo at all; rather, it is another specific treatment. To the extent that it "works," its efficacy may or may not be due to the same factors that are responsible for the "experimental" treatment's

effectiveness. It is interesting to note that although this interpretation of *nonspecific* is the least common in the behavior therapy literature, it appears to be what Shapiro (1971) had in mind in his definition: "A placebo is . . . used for its nonspecific . . . effect [and] is without specific activity for the condition being treated" (p. 440). In any case, in this sense of the term, none of the "placebos" used in behavior therapy research is nonspecific.

THE PLACEBO AS AN EXPECTANCY MANIPULATION

There is one additional element stated or implied in many definitions of placebo that must be considered. It is often assumed that placebos work by having an effect on a subject's beliefs, expectancies, feelings of confidence, etc. Prior to the advent of the new cognitive-behavioral trend, this might have constituted an adequate definition of a placebo. If a procedure worked by automatic reinforcement or by producing insight into the roots of unconscious conflicts, then it was a treatment. Conversely, if it worked by altering beliefs, expectancies, or feelings of hope and confidence, then it was a placebo. This conception of a placebo brings us back to a problem raised above. The essence of this problem is that while these variables (expectancies, beliefs, etc.) are more or less extraneous to psychoanalytic and radically behavioral paradigms, they are central to cognitive-behavioral perspectives. Expectancy was an important construct for Tolman (1932), the progenitor of cognitive behaviorism, and it plays a central role in at least one important social learning theory (Rotter, 1954). Mahoney (1974) has suggested that belief may be an important topic for cognitive-behavioral investigation. Perhaps of greater importance is the fact that many of the currently existing cognitive-behavioral treatment techniques (RET, stress inoculation, treatments based on attribution theory, etc.) have intentionally been designed to work by changing a client's beliefs and expectancies. It also should be noted that the beliefs targeted for change by cognitive therapists are sometimes quite general or "nonspecific." For example, one of Ellis's (1962) "irrational assumptions" is the belief that people are not able to overcome emotional problems. The task of the rational emotive therapist is to convince the client that this belief is irrational and that he or she is capable of controlling negative emotions. In terms of the definition of placebo being considered, we might conclude that the rational emotive therapist's aim at this point is to produce a placebo effect. The point is, do we wish to label all of these treatment approaches as by definition "merely" placebos?

Implicit in most discussions of the subject is the notion that the placebo effect involves an alteration of a specific kind of belief or expec-

tancy. A placebo is a treatment that achieves its effect by establishing an expectancy for improvement. When change is due to the alteration of expectancies other than the expectancy for improvement, the treatment is more than a placebo. However, when one examines the nature of procedures that are used as placebo controls in most therapy outcome research, it becomes clear that the placebo must involve something more than the establishment of an expectancy for improvement. Following pretesting and assignment to experimental and control groups, an experimenter delivers a treatment description and rationale to his subjects. It is at this point that an initial expectancy for improvement is presumably established. If this were all that were necessary to obtain a placebo effect, it would not be necessary to administer the treatment that has been described. In fact, if we conceive of a placebo as a procedure for mobilizing the subject's expectation of help, the issue of credibility could be easily dispensed with. Placebo control subjects would receive the same treatment description and rationale as experimental subjects and led to believe that they would receive that treatment, but improvement would be assessed *prior* to administration of the treatment.

Of course, the procedure outlined above is never followed. In practice, we find it necessary to actually administer the placebo prior to posttreatment assessment. But once treatment has been administered, subjects should no longer expect that they *will* improve; rather, to the extent that our manipulation has been successful, they should believe that they *have* improved. The experimenter supplies the subject with the major and minor premises of a syllogism and expects that the logical conclusion will effect the subject's behavior during pottreatment assessment. The major premise is supplied by the treatment description and rationale: "If I experience this treatment, then I will change." Administration of the treatment supplies the minor premise: "I have experienced this treatment." Conclusion: "I have changed."

What is the relation of this logical sequence to the behavior required of the subject during posttreatment assessment? The expectancy for improvement was described above in a generalized form ("this treatment produces change"). In practice, we attempt to induce more specific expectancies (e.g., "this treatment produces a reduction of anxiety in public speaking situations"). The conclusion of the syllogism given a more specific major premise is an altered efficacy or performance expectation (e.g., "I am no longer anxious when speaking in public"). Should we then conclude that a placebo may be defined as any procedure that produces an effect by altering expectations of personal efficacy? To do so might force us to consider all treatments based on social learning theory as, by definition, placebos. Consideration of the sources and consequences of efficacy

expectations constitute the most recent addition to Bandura's (1977) social learning theory: "In the social learning view, psychological changes, regardless of the method used to achieve them, derive from a common mechanism.... Psychological procedures, whatever their form, alter expectations of personal efficacy" (p. 79).

CONCLUSIONS

It should be clear at this point that there is no existing definition of *placebo* that does not either include treatments that we would wish to exclude or result in defining the null set. What, then, are we to do with the concept of a placebo, especially given its presumed importance in psychotherapy outcome research? Unless a new definition is proposed, I can see only two logical alternatives. First, we might return to the original medical definition: A placebo is a chemically inert treatment working by psychological mechanisms. In that case, we recognize that any form of psychotherapy is a placebo and that our task is to discover the most effective placebo for particular types of people with given kinds of problems. The second alternative is to conclude that although placebo is a useful concept for physical medicine, it is not a useful or meaningful construct in relation to psychotherapeutic procedures. All psychological procedures aimed at producing positively valued emotional and/or behavioral change are treatments, whether they achieve their goal by altering reinforcement contingencies, producing insight, or changing cognitions.

In many respects, these two alternatives are functionally equivalent. In neither case would we be experimentally comparing a treatment to a placebo. Rather, we would compare two treatments (or two placebos) that differ in some specific respects (e.g., exclusion vs. inclusion of a specific component, credibility, simplicity). In other words, the inclusion of a particular treatment/placebo condition should be for some more specific purpose than to control for *the* placebo effect. Given equivalence of the two terms in respect to how we go about our work, I would suggest choosing the term *treatment* if for no other reason than the fact that *placebo* has strong negative connotations.

There is an additional reason for proposing this alternative. Consider the presumed power of the so-called placebo effect. It has been reported that placebos are capable of reducing pain, altering behavior, counteracting the effects of potent drugs, and producing profound physiological changes (Shapiro, 1971). The reported phenomenon of spontaneous remission in cancer patients suggests that we may not yet have uncovered the limits of these effects. In addition, what might be termed *negative placebos* (procedures that induce the expectation of a negatively valued change) have

been reported to be capable of resulting in death (Seligman, 1975). What effects of similar magnitude may be claimed for "nonplacebo" psychotherapeutic procedures? In fact, we do not seem to have developed any therapeutic technique that has been convincingly demonstrated to be effective beyond its "placebo" characteristics. Note, for example, the fact that five of the six existing studies comparing systematic desensitization to a control condition shown to generate comparable subject expectancy failed to demonstrate the superiority of desensitization (Kazdin & Wilcoxon, 1976; Kirsch & Henry, 1977). Given a factor that appears to be more powerful than potent active drugs, has "profound effects on organic illness, and possibly even on malignancies," and in some studies produces its effect in 100% of the subjects (Shapiro, 1971, p. 442), it seems to me that we may be on the wrong track when we attempt to control for this factor and to focus on what remains when it is removed. The alternative is to focus on the so-called placebo effect, to specify the laws governing its operation, to devise means of reliably producing it and of maximizing its impact. Areas for empirical investigation might include the interaction between treatment rationales and subjects' preexisting beliefs. It seems reasonable to expect, for example, that as a result of prior learning, some individuals would be more receptive to a rationale involving psychodynamic concepts, some to a rationale based on radical behaviorist notions, and others to rationales couched in religious or mystical terms. We might also investigate the extent to which treatment procedures might strengthen or contradict the expectancies that have presumably been established via the treatment rationale. But in investigating variables of this sort, with a view toward the discovery and modification of treatment procedures, why should we burden ourselves with the term *placebo* and with all its negative connotations? Given the presumed nature of this effect, using that label would be self-defeating.

REFERENCES

Bandura, A. *Social learning theory.* Englewood Cliffs, New Jersey: Prentice-Hall, 1977.

Beck, A. T. *Cognitive therapy and the emotional disorders.* New York: International Universities Press, 1976.

Borkovec, T. D., & Nau, S. D. Credibility of analogue therapy rationales. *Journal of Behavior Therapy and Experimental Psychiatry,* 1972, *3,* 257-260.

Davison, G. C., & Neale, J. M. *Abnormal psychology: An experimental clinical approach.* New York: Wiley, 1974.

Ellis, A. *Reason and emotion in psychotherapy.* New York: Lyle Stuart, 1962.

Evans, W. O. *The effectiveness of visual imagery on phobic behavior when accompanied by electric shock.* Unpublished doctoral dissertation, Fuller Theological Seminary, 1968.

Goldfried, M. R., & Goldfried, A. P. Importance of hierarchy content in the self-control of anxiety. *Journal of Consulting and Clinical Psychology,* 1977, *45,* 124-134.

Hallam, R., Rachman, D., & Falkowski, W. Subjective, attitudinal, and physiological effects of electrical aversion therapy. *Behaviour Research and Therapy,* 1972, *10,* 1-13.

Hebb, O. D. The American Revolution. *American Psychologist,* 1960, *15,* 735-745.
Hebb, O. D. What psychology is about. *American Psychologist,* 1974, *29,* 71-79.
Kazdin, A. E., & Wilcoxon, L. A. Systematic desensitization and nonspecific treatment effects: A methodological evaluation. *Psychological Bulletin,* 1976, *83,* 729-758.
Kelly, G. *The psychology of personal constructs.* 2 Vols. New York: Norton, 1955.
Kirsch, I., & Henry, D. Extinction vs. credibility in the desensitization of speech anxiety. *Journal of Consulting and Clinical Psychology,* 1977, *45,* 1052-1059.
Kuhn, T. S. *The structure of scientific revolutions.* Chicago: University of Chicago Press, 1970.
Mahoney, M. J. *Cognition and behavior modification.* Cambridge, Massachusetts: Ballinger, 1974.
Mahoney, M. J. Reflections on the cognitive-learning trend in psychotherapy. *American Psychologist,* 1977, *32,* 5-13.
Maultsby, M. C. *Help yourself to happiness through rational self counseling.* New York: Institute for Rational Living, 1975.
McKeachie, W. J. Psychology in America's bicentennial year. *American Psychologist,* 1976, *31,* 819-833.
McReynolds, W. T., Barnes, A. R., Brooks, S., & Rehagen, N. J. The role of attention-placebo influences in the efficacy of systematic desensitization. *Journal of Consulting and Clinical Psychology,* 1973, *41,* 86-92.
Rotter, J. B. *Social learning and clinical psychology.* Englewood Cliffs, New Jersey: Prentice-Hall, 1954.
Seligman, M. E. P. *Helplessness.* San Francisco: W. H. Freeman, 1975.
Shapiro, A. K. A contribution to a history of placebo effect. *Behavioral Science,* 1960, *5,* 109-135.
Shapiro, A. K. Placebo effects in medicine, psychotherapy, and psychoanalysis. In A. E. Bergin & S. L. Garfield (Eds.), *Handbook of psychotherapy and behavior change.* New York: Wiley, 1971.
Tolman, E. C. *Purposive behavior in animals and men.* New York: Appleton-Century-Crofts, 1932.
Ullmann, L. P., & Krasner, L. *A psychological approach to abnormal behavior.* Englewood Cliffs, New Jersey: Prentice-Hall, 1969.
Wolpe, J. *Psychotherapy by reciprocal inhibition.* Stanford, California: Stanford University Press, 1958.
Wolpin, M., & Pearsall, L. Rapid deconditioning of a fear of snakes. *Behaviour Research and Therapy,* 1965, *3,* 107-111.
Wolpin, M., & Raines, J. Visual imagery, expected roles and extinction as possible factors in reducing fear and avoidance behaviour. *Behaviour Research and Therapy,* 1966, *4,* 25-37.