

Prevention & Treatment

Prevention & Treatment, Volume 5, Article 36c, posted October 18, 2002
[Copyright 2002 by the American Psychological Association](#)

Commentary on "[On The Social Psychology of the Psychological Experiment:
With Particular Reference to Demand Characteristics and Their Implications](#)"

Demand Characteristics in the Laboratory and the Clinic: Conversations and Collaborations With Subjects and Patients

John F. Kihlstrom
University of California, Berkeley, and
Institute for the Study of Healthcare Organizations & Transactions

ABSTRACT

Martin Orne's notion of *demand characteristics* is related to the cooperative principle in language pragmatics: The need for experimenter and subject to have a common view of the experiment is similar to the need for speaker and listener to establish common ground in a conversation. Where experimenter and subject do not share common ground, experiments may be lacking in ecological validity. Like experimental subjects, clinical patients are engaged in effort after meaning, trying to make sense of their symptoms. In order to make sure that patient and therapist are on common ground, Orne urged clinicians to engage in anticipatory socialization and to mobilize demand characteristics in the service of treatment and prevention.

This article is based on a keynote address presented at the Seventh Annual Convention of the American Psychological Society, New York, June 1995, and dedicated to Martin T. Orne. The point of view represented herein is based in part on research supported by Grant MH -35856 from the National Institute of Mental Health. I thank Marilyn Brewer and Mahzarin Banaji for their invitation to present this material.

Correspondence concerning this article should be addressed to John F. Kihlstrom, Department of Psychology, MC 1650, University of California, Tolman Hall 3210, Berkeley, California 94720-1650.
E-mail: kihlstrm@socrates.berkeley.edu

After 15 years of teaching the introductory psychology course, I have gradually come to the conclusion that an awful lot of psychological science could be presented as sustained, empirical and theoretical meditations on a relatively few pithy sentences. Some of these are

real gems and deserve to be inscribed on wallet-sized cards, bumper stickers, and inspirational posters of the sort you see advertised in airline magazines. For example:

Cogito ergo sum,

I think, therefore I am, from Rene Descartes's *Discourse on Method* of 1641 (Chapter 4). All of psychology begins here: It turns out that the only thing we cannot deny, out of all that might be true in the whole universe, including quarks and punctuated equilibrium and postmodern literary theory, is that we exist; and the reason we cannot deny it is not just that we think but that we are *aware* that we think. Setting aside whether this argument actually works as a bulwark against radical skepticism, or the impossibility of knowing anything for sure, in this passage Descartes establishes epistemology as the primary concern of philosophy, and conscious experience, the self, and the relations between mind and body as the central topics for the scientific psychology that would follow in due course, about 200 years later.

William James had the same sort of thing in mind when he stated, in *The Principles of Psychology* ([James, 1890/1980](#)), that

The universal conscious fact is not "feelings and thoughts exist" but "*I think*" and "*I feel*." (p. 221)

When we unpack a sentence like this, at least if we do it the way James did, we are led immediately to ask certain fundamental questions about the nature of consciousness: whether it makes sense to talk about unconscious mental life; if so, what the relations between conscious and unconscious mental life are; what the nature of the self is; and how it plays a role in consciousness.

But is thinking all there is to psychology? In fact, for both Descartes and James, *thinking* is an umbrella term for consciousness. As Descartes put it, this time in the *Meditations* of 1641:

What is a thing that thinks? It is a thing which doubts, understands, conceives, affirms, denies, wills, refuses, which also imagines and feels. (chap. 2, para. 8)

So what does mental life consist of? About 150 years after Descartes, another philosopher, Immanuel Kant, summarized what quickly became the prevailing view in both philosophy and psychology:

There are three absolutely irreducible faculties of mind, namely, knowledge, feeling, and desire.

This comes from the *Critique of Pure Reason* of 1781 (section 14). There is an awful lot to get out of this one. First, does it make sense to talk about faculties at all? Implied in this question is the continuing tension in psychology between general systems for learning or information processing and specialized mental modules. For the neuroscientifically inclined, this is the enduring conflict between specialization and holism. Are there different mental faculties, or systems, and if so, what are their relations to neural systems? And if there are separate mental faculties, what are they, how do they relate to each other, and to what extent are they cognitively penetrable? If we take Kant's answer as a reasonable approximation, to what extent are our emotional and motivational states under cognitive control; to what extent do our feelings and desires color our thoughts?

Now let us move up another 150 years, from Kant to Sir Frederick Bartlett, and this statement from *Remembering* ([Bartlett, 1932](#)):

The psychologist, of all people, must not stand in awe of the stimulus. (p. 3)

The entire history of 20th-century psychology is wrapped up in this sentence, because the comment was not so much about poor old Ebbinghaus, his nonsense syllables, and the law of repetition, which were the ostensible objects of Bartlett's wrath, as it was about associationism and its evil twin, behaviorism, which—at least in Bartlett's view—tended to "overstress the determining character of the stimulus or of the situation" (p. 6). In a very real sense, this sentence is the first shot in the cognitive revolution in psychology, because Bartlett is arguing that people do not behave in response to objective stimulus conditions. Rather, their interactions with the environment, and with each other, reflect *effort after meaning*—an effort that results in a mental representation of the situation and a plan for acting in accordance with this representation. So now we have to know all about mental representations of the world outside the mind, the relation between representations constructed through perception and those reconstructed through memory, the relations between so-called lower mental processes like sensation, perception, attention, and learning, and so-called higher mental processes of memory, thinking, reasoning, problem-solving, and language.

A couple of decades later, as the cognitive revolution began to build up steam, Jerome Bruner ([1957](#)) picked up on Bartlett's central theme when he wrote that

Perception involves an act of categorization. (p. 123)

Here Bruner expresses his emphasis on *going beyond the information given* by linking the current stimulus situation with what is already known from prior experiences. For Bruner, perception is not complete until we know not just the form, location, and motion of an object but also what sort of thing it is. Categorization allows us to make inferences about unseen properties of objects, and their past and future behavior, so that we know how to deal with them. So now we have to know all about the organization of concepts, proper sets and fuzzy sets, prototypes and exemplars, how concepts are acquired, and the relation of cognitive categories to the natural divisions in the world outside the mind.

Then too, [Bruner \(1957\)](#) also noted that (paraphrasing):

The purpose of perception is action.

In Bruner's view, we do not acquire knowledge for its own sake. We want to know, we *need* to know, so that we know *what to do*. Cognitive psychology often leaves this part out: I recently checked three best selling cognitive psychology texts and found that neither *action* nor *behavior* appeared in the indexes of any of them. *Mind in action* seems more to be the province of social psychology, which has always been concerned with the relations between things like people's beliefs and attitudes on the one hand and their interpersonal behavior on the other. How do beliefs translate into behavior, behavior that in turn creates reality? How does behavior flow from attitudes, and how do attitudes emerge from behavior?

Here is a sentence for personality psychologists, from the *Characters* of Theophrastus (c. 319 BC), Aristotle's cofounder and successor at the Peripatetic school of philosophy in ancient Athens:

I have often marveled . . . why it has come about that, while the whole of

Greece lies in the same clime and all Greeks have a like upbringing, we have not the same constitution of character.

This sentence unpacks itself: How do individuals differ from each other in mind and behavior? Do these differences reside in the people observed or the eye of the beholder? If the former, are the differences best construed in terms of discrete types, continuous traits, or some other notion? What are their origins in heredity and environment?

Many of the sentences psychologists meditate on are quite high-minded. Others, though, are somewhat more mundane. Consider this one, from Noam Chomsky ([1957](#)):

Colorless green ideas sleep furiously. (p. 15)

This sentence has so much wrapped up in it that one hardly knows where to begin unwrapping: the difference between syntax and semantics, and between phrase structure and surface structure; the notion of mental processes operating according to rules; whether language acquisition is different from classical and instrumental conditioning. By the time readers have figured out how novel utterances can be generated and understood, they have dealt with the tension between nativism and empiricism, arguments about the modularity of mind (again), and the nature of human creativity. Not bad for a sentence that doesn't mean anything.

Here's a sentence that does mean something:

In a park a hippie touched a debutante.

Like Chomsky, John Anderson and Gordon Bower ([Anderson & Bower, 1973](#), p. 138) got a whole book out of this sentence—a book all about propositional representations of knowledge, spreading activation and priming effects, episodic and semantic memory, and the question of whether knowledge is stored in nonpropositional, analogue or imagistic, form.

And finally, the sentence that will serve as the basis for the rest of this article and that brings us to Martin Orne and the concept of demand characteristics:

Could you pass the salt?¹

Children who reply to this question with a "yes" get dirty looks from their parents and are immediately branded smart alecks by their teachers, because this is not a question about the listener's physical abilities. Rather, it is an indirect request to pass the salt. It harkens back to Bartlett's effort after meaning, as the listener tries to resolve the inherent ambiguity in the sentence. Chomskian syntax and semantics are not enough for that purpose, it turns out. We also need a set of pragmatic principles that go beyond the information given by syntax and semantics and that govern how people communicate with each other. In the final analysis, a sentence like this reminds us that language is not just a tool for individual thought; it is also a tool for interpersonal communication—or, as the psycholinguist Herbert Clark has put it, language doesn't have so much to do with words and what they mean as it does with *people* and what *they* mean. So, in addition to investigating the cognitive bases of language, we have to understand its social foundations as well; once again, social psychology addresses the use to which cognitive structures and processes are put (for reviews of the social psychology of language use, see [Brown, 1965, 1986](#)).

Common Ground, the Cooperative Principle, and Conversational Rules

So, for example, from analyzing how sentences like this are understood, we learn that in order for the speaker and listener to communicate, they have to establish *common ground*—which [Clark \(1979\)](#) defines as the knowledge, beliefs, and suppositions that speaker and listener share in common. Each must have some sense of what the other person knows, believes, and supposes to be true, and each must use this knowledge in structuring his or her communication. If speaker and listener are not on common ground, they will not understand each other and their interactions cannot go very far.

In order to achieve this mutual understanding, people have to manage their conversations according to what the linguist Paul Grice has called *the cooperative principle* ([Grice, 1975, 1978](#)):

Make your conversational contribution such as is required, at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged. (p. 45)

This principle, in turn, is unpacked in terms of four conversational maxims and their submaxims:

- The maxim of quantity: Make your contribution as informative as is required (for current purposes), and *do not* make your contribution more informative than is required.
- The maxim of quality: Try to make your contribution one that is true; do not say what you believe to be false, and do not say that for which you lack adequate evidence.
- The maxim of relation: Be relevant.
- The maxim of manner: Be brief, and orderly, avoiding obscurity and ambiguity of expression.

Grice and others interested in sociolinguistics, including some psychologists (e.g., [Clark, 1996](#); [Higgins, 1981](#); [Schwarz, 1994](#)), have shown how listeners assume that speakers are following these maxims and how lots of mischief can result when this assumption proves false. What interests me is the possibility that the psychological experiment itself can be construed as a conversation, with the experimenter in the role of speaker and the subject in the role of listener.² It turns out that experimenters violate conversational rules pretty often, in a way that makes things difficult for subjects who are trying to figure out what the experimenter is up to; these violations also make things difficult for experimenters—who, when they do not realize that they are being misunderstood, proceed to misinterpret the results of their own experiments.

Let me give a simple example, taken from the centennial exhibition "Understanding Ourselves, Understanding Each Other," developed by the American Psychological Association and the Ontario Science Centre ([American Psychological Association, 1992](#)). At some point in the exhibit, the visitor came upon a corridor paved in a black and white checkerboard pattern, with a multilingual sign warning them to step only on the black squares. Everybody did this, of course, and when they got to the other end they were given a little lecture on mindless conformity. Except, of course, that there is nothing necessarily *mindless* about such behavior at all. Equally likely, the average visitor went "beyond the information given" in the sign, inferred that there must be a good reason for the injunction, and decided to behave in a cooperative manner. To use a real-world analogy, we hardly ever

demand to know *why* we cannot cross police lines; mostly we figure that there must be a good reason for the barrier: Perhaps there is a hole in the sidewalk; perhaps there is evidence that needs to be protected. Neither case is fairly construed as mindless obedience to authority; both scenarios might well reflect reasoned, cooperative behavior, the equivalent of simply passing the salt if asked whether you could (of course, a really polite person passes the pepper as well).

Ecological Validity and Demand Characteristics

Of course, you do not need to be a Gricean sociolinguist to think about experiments that way. Martin Orne had the same kind of idea in his arguments, which he began to voice in the late 1950s and early 1960s, about demand characteristics and the ecological validity of psychological experiments ([Orne, 1962b](#), [1969](#), [1972b](#), [1973](#), [1981](#)). From Orne's point of view, the purpose of laboratory research is to understand the real world: to make the problem simple so that it can be studied effectively, and to control relevant variables so that important relations, especially causal relations, can be revealed. Unfortunately, generalization from the lab to the real world requires an inferential leap: Its legitimacy depends on the degree of similarity between the conditions that obtain in the laboratory and those found in the real world. In the natural sciences, perhaps, it is safe to assume that the lab is in important respects like life. But in psychology, Orne argued, this assumption is not safe.

The situation is bad enough in animal research—for example, McClintock and Adler's studies of sexual behavior in rats showed how theory can be greatly misled when the experimental situation is not naturalistic (e.g., [McClintock & Adler, 1978](#)). But it is even worse in human research, for the simple reason that human subjects are not reagents in test tubes, passive responders to the experimenter's manipulations. Human subjects are sentient, curious creatures, constantly thinking about what is happening to them, evaluating the proceedings, figuring out what they are supposed to do, and planning their responses. These normal human cognitive activities may interact with experimental procedures in unknown ways. At best, they can obscure the effects of a manipulation, by adding noise to the system; at worst, they can render an entire experiment invalid.

For Orne, the experiment is a unique form of social encounter, with roles and rules that are not found anywhere else (except perhaps in doctors' offices, about which more later). Several aspects of this uniqueness may preclude generalizations from lab to life—which is what Orne means by threats to ecological validity.

1. The human subject is a volunteer ([Rosenthal & Rosnow, 1969](#)) who, in addition to his or her desire for remuneration (whether in the form of cash or research participation points), has an emotional investment in research that stems from three goals: to further scientific knowledge; to find out something about him- or herself; and to present him- or herself in a positive light. Subjects participate in experiments to learn about themselves and to help science. They want to be sure that they do it "right."
2. The experimenter and the subject enter into an implicit contractual arrangement with specifically defined roles: The subject agrees to tolerate deception, discomfort, and boredom for the good of science, and the experimenter guarantees the safety and well-being of the subject—a guarantee that implicitly legitimizes any requests that the experimenter might make.³
3. The experiment is by its very nature *episodic*: In important respects it is divorced from the rest of the subject's life experiences, and in any event, it is expressly time limited and should

have no permanent consequences for the subject.

4. Subjects perceive the experiment in the context of their entire experience. They are engaged in "effort after meaning," trying to discern what the experiment is all about and deciding what to do. In so doing, they pick up on what Orne called the *demand characteristics* of the experiment, by which Orne meant the totality of cues available in the experimental situation that communicate the experimenter's design, hypotheses, and predictions.

Some of these demand characteristics are explicitly present in the experimenter's instructions to the subject, but many of them are implicit in the solicitation materials, campus scuttlebutt, incidental remarks made by the research assistants, and hints communicated by the procedures. The important thing to understand is that demand characteristics are not just communicated by the experimenter. Accordingly, unlike experimenter bias ([Rosenthal, 1963](#)), they cannot be controlled merely by keeping the experimenter blind to the experimental hypothesis. Rather, some demand characteristics are *brought into* the experiment by the subject, whereas others arise as the experiment proceeds; in either case, they are everywhere. In the final analysis, they are internal to the subject, they cannot be predicted in advance by someone external to the experiment, and in principle they cannot be controlled; they can only be evaluated retrospectively.

The point of Orne's analysis is that regardless of the experimenter's intentions and instructions, the subject's behavior in the experiment is determined by his or her perceptions of the experimental situation, perceptions that are formed as the subject goes beyond the information given by the experimenter. For this reason, the subject's perceptions may be at variance with the experimenter's intentions. If this occurs, the experimenter and the subject are literally participating in two different experiments, and ecological validity is lost.

As an illustration, consider an anecdote from the early days of sleep research, often told as a moral lesson by Orne himself. One night a subject arrived for an experiment that was, truthfully, advertised as recording physiological responses while subjects were asleep. One participant dutifully submitted to having electrodes attached to his head and other body parts, crawled into bed, and pulled up the covers; the experimenter told him to go to sleep and turned out the light. Half an hour later, the subject was still awake. When the experimenter asked if everything was all right, the subject replied "yes." The experimenter reminded the subject to go to sleep, and the subject agreed to do so. Half an hour later, the subject was still awake: The scene was repeated, and again half an hour after that, and again half an hour after that. Finally, the experimenter burst into the bedroom in a fit of exasperation and demanded to know why the subject was refusing to go to sleep. To which the subject replied, "You mean the mouse in my bed *isn't* supposed to be there?"

The story may be apocryphal, but the experience is ubiquitous. In the very first group hypnosis session I ran at Wisconsin, employing the Harvard Group Scale of Hypnotic Susceptibility ([Shor & Orne, 1962, 1963](#)), with only inexperienced undergraduates serving as my research assistants, a subject became slightly nauseated just as the tape-recorded induction procedure began—not from the any effects of hypnosis, but from a particularly potent combination of beef stroganoff and beer he had just eaten at the student union. One assistant and I helped the subject out of the room under the watchful gaze of about 125 other subjects, while the session proceeded under the nervous eyes of the other assistant. I was able to return to the experimental room shortly, and the rest of the session proceeded smoothly. However, when the procedure was concluded, we had a devil of a time convincing many subjects that the whole episode had not been staged to see how their responses would be affected. We were just trying to assess hypnotizability; these subjects

thought we were doing something else entirely.

This kind of scenario occurs all the time in traditional personality research, where subjects simply cannot believe that all you want them to do is fill out a questionnaire. Once college students read about the classic bystander intervention studies ([Latane & Darley, 1970](#)), when they come into the laboratory they expect smoke to pour in through the ventilation ducts or a research assistant to fall and break his foot.

So in order to make sense of experimental outcomes, the experimenter must attempt to understand the subject's behavior from the subject's point of view. Unfortunately, this understanding is impeded by what Orne called the *pact of ignorance* implied by the experimental contract. Both parties want the experiment to work; therefore, the subject agrees not to tell the experimenter that he or she has figured out the experiment, while the experimenter agrees not to force the subject to admit that he or she possesses this forbidden information. The situation was often caricatured by Orne in a scenario in which the experimenter, who has already mentioned to the subject that the experiment is part of her dissertation research, pulls the subject's payment out of her purse, puts it on the table in front of the subject, debriefs the subject as to the actual purpose of the experiment, and then asks one last question: Did the subject catch onto any of this? The subject dutifully replies "no" (else he would have just wasted both the experimenter's time and his own), the experimenter breathes a sign of relief (because she won't have to replace the subject), the money changes hands, and both participants go on their respective ways.

In order to break the pact of ignorance, Orne argued, the experimenter and subject must alter their usual roles, concluding the experimental episode and transforming what once was a subject into a genuine coinvestigator, who feels it legitimate to reflect truthfully and dispassionately on what has gone on before. That is what Orne's real–simulator design was all about. In this paradigm, a group of subjects is instructed to behave *as if* they were exposed to the experimental manipulation. It is their job to figure out what a subject is supposed to do, simply by virtue of the demand characteristics of the situation. In order to enhance their motivation, the experimenter is kept blind to who is really in the experiment and who is simulating, but afterward the simulators are extensively debriefed in order to identify salient artifacts in the experimental situation. Simulators are not subjects in the usual sense, because they are only *pretending* to be in an experiment. They are not controlling for demand characteristics, or indeed for any other experimental variable; they are collaborators of the experimenter, helping to evaluate the experimental design.

Demand Characteristics in Hypnosis Research

Orne was famous for applying the real–simulator design to evaluating the demand characteristics of hypnosis experiments—for example, in studies of hypnotic age regression ([O'Connell, Shor, & Orne, 1970](#)); or the transcendence of normal voluntary capacity ([Evans & Orne, 1965](#); [Orne, 1966](#); for reviews, see [Orne, 1970, 1971, 1972b](#)). Two of these studies, both performed in collaboration with Fred Evans, nicely illustrate the point about the pragmatics of experimenter–subject conversations.

First, consider an experiment on whether antisocial and self-injurious behavior can be induced by hypnosis. This question goes back more than 200 years, to the French Royal Commission's investigation of Franz Anton Mesmer ([Gauld, 1992](#); [Laurence & Perry, 1988](#); [Schefflin & Shapiro, 1989](#)), and more recently was even asked by the Central Intelligence Agency ([Marks, 1979](#)). The question is certainly a legitimate one: Hypnotized subjects are highly responsive to suggestions, especially for perceptual distortions, and perhaps this suggestibility gives the hypnotist a special power to coerce antisocial and self-injurious

behavior.

In a classic experiment on hypnosis, Rowland had reported an experiment in which subjects were hypnotized, placed in front of a large, active, diamondback rattlesnake, told it was a coil of rope, and asked to pick it up ([Rowland, 1939](#)). One of two subjects who received this request immediately complied, at which point he struck his hand on a pane of invisible glass that had been interposed between him and the reptile. By contrast, all but one of 42 control subjects, asked if they would do the same thing, refused to go anywhere near the snake. Some years later, P.C. Young, who had been one of Hull's ([1933](#)) students at Yale, replicated Rowland's finding ([Young, 1950, 1952](#)): 7 out of 8 subjects attempted to pick up the snake *and* threw a vial of nitric acid at a research assistant (who was also protected by invisible glass).

Orne and Evans were deeply suspicious of both experiments, because their procedures appeared to violate the basic contract between experimenter and subject (see also [Orne, 1972a](#); [Orne & Evans, 1965](#)). In every experiment, the subject agrees to do what the experimenter wants, and the experimenter agrees in return to protect the subject from harm. The request to pick up the snake or throw the acid has some of the character of "Could you pass the salt?": The subjects go beyond what the words mean to infer what the experimenter means. It is not enough, in the Rowland experiment, that almost all of the un hypnotized controls *said* they would refuse to pick up the snake; they might behave quite differently in the actual experimental situation.

Reasoning along these lines, [Orne and Evans \(1965\)](#), working in Australia, contrived an experiment in which a group of highly hypnotizable subjects were hypnotized and asked to reach into a wooden box for all sorts of things: a two-shilling coin, a piece of chalk, a gecko lizard widely known to be harmless, and a green tree snake, also widely known to be harmless. All of the subjects did everything, except one subject, who fainted at the sight of the snake. The surviving subjects were then asked to pick up a red-bellied black snake, widely known to be extremely venomous (Orne liked to describe it as the "Australian two-step," because that is how far the victim gets after being bitten, but an informal survey indicated that this moniker is unknown to Australians). All five of the hypnotic subjects attempted to pick up the snake, which of course was shielded from them behind a pane of invisible glass. They also were willing to remove a partially dissolved two-shilling coin from a beaker of fuming nitric acid (this is perfectly safe if you do it right, but it is not recommended that readers try it at home), and finally to throw the acid at Evans, a graduate student acting as Orne's assistant, who was also protected by invisible glass. But every one of a group of insusceptible subjects, who had been instructed to simulate hypnosis, also did these things, and for that matter, so did every one of a group of unselected subjects run in the normal waking state. All of them, interviewed later, reported that they felt perfectly safe in the experiment, secure in the knowledge that appropriate safeguards were in place—as in fact they were. Their safety had been clearly communicated by the demand characteristics of the experiment.

A sidebar: In the 1970s, Orne attempted to repeat this demonstration for a documentary on hypnosis being prepared for television. The subject recruited for this purpose, a highly hypnotizable college undergraduate, watched Orne as he dissolved a coin in the acid; and when instructed to throw the liquid at the director she did so. When asked to pick up a large, hissing rattlesnake (supplied by a Hollywood animal firm, with its venom removed), she did that as well. When interviewed later, though, she said that she sensed that Orne himself was somewhat nervous in the presence of the snake and that she wanted to say to him, "Don't worry, Dr. Orne. It won't hurt you. It's only an experiment" (personal communication from Helen Joan Crawford, March 1, 2002).

Beyond Hypnosis—and Back

Reading about such an experiment might lead one to reevaluate the procedures used in many classic experiments in social psychology. Consider, for example, Milgram's studies of obedience to authority ([Milgram, 1963, 1965](#)). As every psychologist knows, Milgram ostensibly recruited subjects in pairs and, ostensibly, randomly assigned one to be the teacher and the other the learner in an ostensible study of the effects of punishment on learning. Of course, nothing was what it seemed: The learner was a confederate of the experimenter, making errors according to a prearranged script. The outcome was that a surprisingly large number of subjects were willing to administer intense punishment in response to the learner's apparent mistakes—even when the highest levels of shock were labeled "Danger" and "XXX," and even when the learner screamed in agony, demanded to be released from the experiment, and then fell silent.

The Milgram experiment is a classic—in fact, that is what the black-and-white checkerboard was all about at the American Psychological Association exhibit. To an external observer, the level of obedience and the dispassionate demeanor of the "teacher" subjects are chillingly compelling, which is why the experiment and the documentary film made about it are a staple in undergraduate courses.⁴ Aside from the theoretical implications of its substantive surroundings, the experiment was also highly controversial on ethical grounds ([Baumrind, 1964](#); [Kelman, 1967](#); but see [Milgram, 1964](#))—and, some say, the immediate instigation for the organization of human-subjects protection committees on college campuses and elsewhere. A survey conducted by Milgram, including psychiatrists, middle-class adults, and college students, predicted that almost nobody would take the shock to the highest levels. The experiment seemed to reveal something surprising, and disturbing, about the human capacity for cruelty and the power of the situation to elicit it.

But in order to properly evaluate the experiment, including its ethical dimensions, one has to ask some questions from the subject's point of view—chiefly, "What am I doing here? If my only job is to administer punishment, why can't the experimenter do it himself, and run both of us as learners? If he's interested in the effects of punishment on learning, why is he in here watching me instead of the learner?" Almost nobody polled by [Rowland \(1939\)](#) believed that subjects would pick up the snake, but between his subjects and Young's ([1952](#)), almost everybody in the experiment actually did, and for reasons having nothing to do with the power of hypnosis to coerce antisocial behavior. The totality of cues present in the situation—even when the experiment is conducted in a run-down building in Bridgeport rather than the hallowed spires of Yale—are more than enough to lead the "teacher" to conclude that things are not what they appear to be and to generate the hypothesis that he, not the "learner," is the actual subject of the experiment. If so, the deception has failed, the experimenter and the subject are in different experiments (in Gricean terms, they have failed to establish common ground), and all bets are off ([Orne & Holland, 1968](#)). In reply, Milgram argued that the power of the experimental situation was an illustration of his points about the power of situations in general ([Milgram, 1968, 1972](#)). Perhaps; but as Orne noted at the outset, generalization from the lab to life depends on the experimenter and the subject being in the same experiment. In order to understand experimental outcomes, we have to understand experimental procedures from the subject's point of view.

The argument from demand characteristics quickly got a reputation as a spoiler variable in evaluations of experiments ([Berkowitz & Donnerstein, 1982](#); [Kruglanski, 1975](#); [Rosnow, 1981](#); [Silverman, 1977](#); [Weber & Cook, 1972](#)), but in my view, many critics of Orne's argument failed to appreciate what it was really all about. Orne rejected the reflexive use of demand characteristics to discount experimental outcomes. Rather, he encouraged experimenters to evaluate them to make their experiments better, more ecologically valid,

more convincing.

A nice example of this principle comes from another set of studies, sometimes known as "the case of the disappearing hypnotist" ([Evans & Orne, 1971](#); [Orne & Evans, 1966](#)). Hypnosis is commonly construed as something that the hypnotist does to the subject and that what the hypnotist "does" to the subject is to place him or her "in a trance." In actuality, this belief is not remotely right; in fact, an argument can be made that all hypnosis is self-hypnosis and that the hypnotist is more like a coach or tutor, who instructs subjects as to how to put *themselves* into hypnosis ([Orne, 1970](#); [Orne & McConkey, 1981](#)). However mythological, the impression of the hypnotic subject as somehow "out of it" is rather commonly held, and it occasionally leads to questions from subjects like "What happens if you drop dead while I'm hypnotized?" Roy Dorcus, a hypnosis researcher at UCLA, contrived an experiment to find out ([Dorcus, Brintnall, & Case, 1941](#)). While in the middle of a hypnosis session, he arranged to be interrupted by his secretary, who loudly reminded him that he had an appointment downtown, at which point Dorcus got up and left the room, without a word to the subject, never to return. Hypnotized subjects remained in the room for an average of 28 minutes, compared with 23 minutes for a relaxation control group. The difference was not statistically significant, but in the final analysis it's not clear what conclusions one should draw from the results. This is because the experimenter's unannounced departure violates the experimental contract and may have been perceived by the subjects to be part of the experiment.

Orne and Evans initially performed an elaborate conceptual replication of Dorcus's study, with hypnotic subjects and simulators ([Orne & Evans, 1966](#)). The subjects were recruited for a study of the physiology of hypnosis, and when they arrived at the laboratory they were connected to a polygraph, hypnotized by a tape-recorded procedure, and administered a series of suggestions while a research assistant observed their responses. In the middle of one of these suggestions, while the subject's eyes were shut, one arm stuck to the chair, tapping his foot in time to hallucinated music, another assistant threw a silent switch, the desk light went out, the tape slurred to a halt, the assistant muttered "The damn fuse," opened the door, noisily ran down the hall, and—apparently—never returned. Actually, the assistant tiptoed back into an adjacent room, where she observed the subject through a one-way screen, and reappeared in the experimental room 30 minutes later.

The results of this experiment were striking: On each of four dependent variables, the simulators *outlasted* the reals. They kept their eyes closed for an average of 25 minutes, continued tapping for 17, kept their arms fixed to the chair for 23. When the assistant returned to the room, 5 out of 6 appeared to be deeply hypnotized. A postexperimental interview clarified what had happened. The room was outfitted with a conspicuous mirror, which was in fact a one-way screen, and while the desk lamp and tape went off, the polygraph and a ceiling light stayed on (some light was required for observing the subject, and the experimenters assumed that subjects would realize that different outlets in the same room might be on different circuits). The simulators clearly smelled a rat, and so they behaved the way they *supposed* a hypnotized subject would actually behave.

In a later study, Evans and Orne used this information to improve their experimental procedures ([Evans & Orne, 1971](#)). This time, when the switch was thrown, *everything* went off: desk light, overhead light, tape recorder, polygraph, everything (in fact, the polygraph leads were fed out the back of the machine, through the wall, and into a second machine in the observation room). A portable blackboard covered the one-way screen, and the experiment was conducted at night, so that the room was plunged into complete darkness. Well, almost complete: Evans and Orne had arranged for a light fixture to be installed in the parking lot outside, so that enough light filtered through the draperies to permit the subject to be observed through a peephole drilled through the wall and covered with a translucent

silkscreen painting. Under these circumstances, the simulators stopped their performance almost as soon as the assistant left the room and resumed their hypnotic posture as she reentered 40 minutes later. This time, the behavior of the simulators clearly indicated that the deception had been successful: The subjects accepted the power failure as real.

Whereas the behavior of the simulators differed dramatically across the two experiments, that of the hypnotic subjects did not, and that is of some theoretical interest. But for present purposes I want to focus on the methodological implications: *All* experiments have demand characteristics, and subjects can be *guaranteed* to pick up on them, threatening the ecological validity of our experiments. We ignore this possibility at our peril as scientists.

"Oh, what a tangled web we weave / When first we practise to deceive." This was the epigram, from Sir Walter Scott's *Marmion* (1808), to Orne's critique of the Milgram experiment ([Orne & Holland, 1968](#), p. 282). Orne was very suspicious of experimental deceptions and cover stories, and his critique has become conventional wisdom for many social psychologists, who often use deception in their experiments. This reliance on deception is why social psychology has traditionally been discussed at the end, but not the beginning, of the introductory course: By the time students learn about experimental deceptions, the social psychologists have already gathered their data. But deception is not the only problem. Even when procedures are not deceptive, subjects will still try to make sense of the experimental situation and do what they think they are supposed to do. In a recent study of causal attribution, subjects who thought they were working with a personality psychologist made more dispositional attributions, whereas subjects who thought they were working with a social psychologist made more situational attributions, about a mass murder ([Norenzayan & Schwarz, 1999](#)).

By contrast, cognitive psychologists have not worried about demand characteristics all that much. One prominent theorist in social cognition, when asked on a job interview what made him a social psychologist and not a cognitive psychologist, replied that he lied to his subjects. But it is not true that cognitive psychologists do not deceive their subjects. Consider the levels-of-processing paradigm in memory research ([Craig & Lockhart, 1972](#)), in which subjects are told that the experiment is about how people make judgments about words, when in fact they are going to be surprised with a memory test. Consider research on explicit and implicit memory ([Schacter, 1987](#)), in which experimenters go to great lengths to convince their subjects that the stem-completion test (for example) has nothing to do with the word list that the subject studied only moments before. An adaptation of the real-simulator design was actually employed to evaluate the demand characteristics of some experiments in mental imagery ([Kosslyn, 1980](#)). In the final analysis, the problem of demand characteristics is not just attached to social psychology or to deception experiments. Because *every* subject is engaged in figuring out the meaning of *every* experimental situation, demand characteristics are a problem for *everyone*.

And because experimenters and their subjects are always engaged in a conversation, the logic of conversations is also an enduring problem. Consider work on judgment and decision making ([Tversky & Kahneman, 1974](#)), in which many of the problems posed to subjects violate Grice's conversational rules ([Schwarz, 1994](#)). When we ask subjects in a consumer preference survey to indicate which pair of stockings they prefer ([Nisbett & Wilson, 1977](#)), given the context they have every right to believe that the question is answerable—in this case, that the items are in fact different; and when asked to justify their choices, they have every reason to reject as unresponsive (if not also impolite) the possibility that the chosen pair was on the right rather than the left side of the display. When we ask subjects to predict whether a person is an engineer or a lawyer and then describe that person as someone who is uninterested in politics and social issues and likes woodworking and mathematical puzzles ([Kahneman & Tversky, 1973](#)), subjects have every right to believe that this individuating

personality description is somehow relevant to their task and to use the information somehow. And when they do so, we have no right to conclude that people are irrational or do not understand normative rules of inference ([Nisbett & Ross, 1980](#)). Maybe they do and maybe they don't, but in their conversation with the experimenter they are only doing what comes naturally: Assuming the experimenter is following the cooperative principle and its four associated maxims, they seek to find common ground, and generate their response from this stance ([Bless, Strack, & Schwarz, 1993](#); [Schwarz, 1994](#); [Schwarz, 1996](#)). To assume otherwise, in the absence of an understanding of the experiment from the subject's point of view, is to risk serious misunderstanding of how human minds actually work.

Demand Characteristics in the Clinic

Demand characteristics are not found only in the laboratory; they also abound in the clinic. Psychotherapy is also a conversation and a collaboration, between participants who have clearly defined roles (see also [Kihlstrom, 2002](#); [Siegler & Osmond, 1974](#)). The doctor's task is to investigate the patient's problems, make a diagnosis, prescribe treatment, and monitor progress; the patient's job is to seek help, cooperate with treatment, and try to adjust if his or her condition proves to be chronic.⁵ Patients are engaged in "effort after meaning" on at least three levels: They are, naturally, interested in understanding their condition, but they are also trying to make sense of what the doctor is doing and attempting to figure out what they are supposed to do. And in so doing, they will make use of the totality of cues available to them in the therapeutic situation, just as research subjects do. For this reason, Orne believed that it was important for clinicians to communicate clearly with their patients.

The point applies to all forms of medical intervention, including encounters that involve little more than the dispensing of medication. In talking with medical students, Orne often pointed out that patients are as interested in what their doctors *do not* say as in what they *do* say. When, during a routine medical examination, the doctor moves his stethoscope over the same area of the chest or back twice, the patient will likely infer that he or she has detected something wrong. Rather than leaving the patient in a state of uncertainty, Orne advised physicians to be candid: "I thought I heard a murmur there, but I rechecked and I was wrong." Informed consent procedures are more than legal cover for doctors and hospitals when something goes wrong; at their best, they also serve to inform patients of precisely what is going to happen, precisely what is expected of them, and precisely what to look for as treatment progresses. When surgeons talk to their patients beforehand, the procedures go more smoothly and the patients recover more quickly ([Taylor, 1995](#)).

Some psychotherapists, especially those of a classically Freudian bent, believe that therapy should be an unstructured enterprise, with the doctor silent and the patients following their free associations wherever they might take them. To the contrary, Orne suggested that patients will profit more if they understand the "rules of the game" from the outset ([Orne & Wender, 1968](#)). He borrowed the term *anticipatory socialization* from the sociologist Robert Merton (1957) to capture the process of explicitly explaining to patients the ground rules by which therapy will proceed and their proper role in the process. Without such a learning experience, at best patients will not know what to do in psychotherapy; at worst, they will approach therapy with an inappropriate, counterproductive set of beliefs and expectations. Accordingly, he proposed that therapists engage their patients in an explicit socialization process carried out by means of a preliminary socialization interview in which the therapist establishes rapport with the patient, explains psychotherapy, clarifies the roles of various participants, and discusses any doubts or resistance the patient might have (see also [Sloane, Cristol, Pepernik, & Staples, 1970](#)).

Of course, it is important for therapists not to communicate *too much*. Structuring patients'

expectations too explicitly may lead them down the path toward the self-fulfilling prophecy ([Darley & Fazio, 1980](#); [Merton, 1948](#); [Snyder, 1984](#)). Julius Wishner, one of Orne's colleagues in the psychology department at Penn, was fond of telling the story of his time as a research assistant in a study of carbon dioxide therapy. After each treatment, it was his job to ask the patients how they felt. After the first session, the patients typically said they were "10% better." After the second session, they were typically "20% better." On a hunch, Wishner observed the treatment sessions: As the gas was being applied, the therapist would say to the patient something like, "You are having 10 sessions. This is your second session. Afterward you will be 20% better."

More seriously, a large part of Orne's criticism of the recovered memory movement in psychotherapy had to do with his sense that the procedure could create a self-fulfilling prophecy. When a patient comes to a therapist with an eating disorder, and the therapist believes that eating disorders are caused by incest and other forms of sexual abuse and carefully explains the concept of repression (or dissociation), the danger that patients will "recover" false memories of childhood trauma is very great ([Kihlstrom, 1998](#)). Patients are looking for expert help with their disorders. When therapists embrace inappropriate theories and communicate them to their patients, it should surprise nobody that the patients will do what is expected of them. In this way, far from collaborating to help patients understand their problems and get better, therapists and patients can be drawn into a *folie a deux* whose harm can extend beyond the consulting room to the patient's family and the therapist's profession.

Orne was a believer in specific treatments. For him, it was not enough that a treatment prove itself in a controlled study of efficacy (or effectiveness). For him, evidence of the pragmatic value of a treatment was important ([Fiske et al., 1970](#); Orne, [1975](#), [1977b](#), [1978](#), [1984](#)), but it was also important that the procedure have a proper scientific rationale and that it have some specific effect on the disorder being treated. One of his complaints about biofeedback therapy for problems such as headache was the lack of evidence for the specificity of treatment effects: It was never clear that biofeedback training was anything more than a placebo (Orne, [1979b](#), [1980a](#), [1980b](#), [1980c](#), [1982](#)). To be fair, Orne had similar qualms about hypnosis, which he expressed from the beginning of his career ([Orne, 1953](#), [1962a](#), [1962c](#), [1974](#), [1977a](#), [1979a](#), [1980c](#), [1980d](#); [Soskis, Orne, Orne, & Dinges, 1989](#)). His doctoral dissertation, which attempted to untangle the specific and nonspecific effects of hypnosis in an experimental setting, spurred his lifelong interest in demand characteristics and their implications in the clinic as well as the laboratory ([Orne, 1959](#)). These issues persist today in the evaluation of programs for the prevention and treatment of mental illness.

At the same time, Orne was clear that demand characteristics could be mobilized in the service of prevention of treatment. While the specificity of a treatment is often demonstrated by distinguishing its effects from those of placebo ([Shapiro & Shapiro, 1997](#)), he agreed with Jerome Frank ([1961](#)) that expectations, beliefs, and hope were important elements in psychotherapy—those of the patient and those of the therapist as well ([Orne, 1968](#)). Orne recognized that in principle, every medical treatment, whether psychotherapy or microsurgery, has a placebo component. In order to be scientific, psychotherapists must show that their treatments are specific to the disorder being treated. But in order to be effective, they must embed their treatments in an interpersonal and social context that will potentiate their effectiveness. For Orne, the social context is a catalyst for treatment. Demand characteristics cannot be eliminated from treatment, and so they should be marshaled in the service of helping the patient get well. To do that, doctors must pay attention to what they say, and to what their patients hear.

Conversations and Collaborations Between Experimenters and Subjects, and Therapists and Patients

Orne was concerned with ecological validity and with the peculiar character of the experimental situation. To a great extent, he thought that demand characteristics were a problem because of motives—to help the experimenter, to learn about themselves, and to look good—that were peculiar to research participants. Grice (1975, 1978) reminded us, though, that there is another motive that subjects display both in the lab and elsewhere in life. Subjects are not just motivated to guess and confirm the experimenter's hypothesis; as *listeners*—that is to say, as people engaged in meaningful social interactions—they are primarily motivated to make sense of any communicative situation in which they find themselves. In that respect, at least, Orne need not have worried, for what happens in the laboratory, and in the clinic as well, is entirely representative of what goes on in the real world. The laboratory is just like the real world after all, and so is the clinic. It follows that in our experimental conversations with our subjects and our therapeutic conversations with our patients, as we establish common ground and collaborate with them in learning about the mind and behavior, in general and in their particular cases, we must be careful to follow Grice's maxims:

- be cooperative,
- be informative,
- be true,
- be relevant, and
- be clear.

References

- American Psychological Association. (1992). *Psychology: Understanding ourselves, understanding each other* [VHS recording].
- Anderson, J. R., & Bower, G. H. (1973). *Human associative memory*. Washington, DC: V. H. Winston & Sons.
- Bartlett, F. C. (1932). *Remembering: A study in experimental and social psychology*. Cambridge, England: Cambridge University Press.
- Baumrind, D. (1964). Some thoughts on ethics of research: After reading Milgram's "behavioral study of obedience." *American Psychologist*, *19*, 421–423.
- Berkowitz, L., & Donnerstein, E. (1982). External validity is more than skin deep: Some answers to criticism of laboratory experiments. *American Psychologist*, *37*, 245–257.
- Bless, H., Strack, F., & Schwarz, N. (1993). The informative functions of research procedures: Bias and the logic of conversation. *European Journal of Social Psychology*, *23* (2), 149–165.
- Brown, R. (1965). *Social psychology*. New York: Free Press.
- Brown, R. (1986). *Social psychology* (2nd ed.). New York: Free Press.
- Bruner, J. S. (1957). On perceptual readiness. *Psychological Review*, *64*, 123–152.

- Chomsky, N. (1957). *Syntactic structures*. The Hague, the Netherlands: Mouton.
- Clark, H. H. (1979). Responding to indirect speech acts. *Cognitive Psychology*, *11*, 430–477.
- Clark, H. H. (1996). *Using language*. New York: Cambridge University Press.
- Clark, H. H., & Lucy, P. (1975). Understanding what is meant from what is said: A study in conversationally conveyed requests. *Journal of Verbal Learning & Verbal Behavior*, *14*(1), 56–72.
- Craik, F. I. M., & Lockhart, R. S. (1972). Levels of processing: A framework for memory research. *Journal of Verbal Learning & Verbal Behavior*, *11*, 671–684.
- Darley, J. M., & Fazio, R. H. (1980). Expectancy confirmation processes arising in the social interaction sequence. *American Psychologist*, *35*, 867–881.
- Dorcus, R. M., Brintnall, A. K., & Case, H. W. (1941). Control experiments and their relation to theories of hypnotism. *Journal of General Psychology*, *24*, 217–221.
- Evans, F. J., & Orne, M. T. (1965). Motivation, performance, and hypnosis. *International Journal of Clinical and Experimental Hypnosis*, *13*, 103–116.
- Evans, F. J., & Orne, M. T. (1971). The disappearing hypnotist: The use of simulating subjects to evaluate how subjects perceive experimental procedures. *International Journal of Clinical and Experimental Hypnosis*, *19*, 277–296.
- Fiske, D. W., Hunt, H. F., Luborsky, L., Orne, M. T., Parloff, M. B., Reiser, M. F., & Tuma, A. H. (1970). Planning of research on effectiveness of psychotherapy. *Archives of General Psychiatry*, *22*, 22–32.
- Frank, J. D. (1961). *Persuasion and healing*. Baltimore: Johns Hopkins University Press.
- Gauld, A. (1992). *A history of hypnotism*. Cambridge, England: Cambridge University Press.
- Gordon, D., & Lakoff, G. (1975). Conversational postulates. In P. Cole & J. L. Morgan (Eds.), *Syntax and semantics: Vol. 3. Speech acts* (pp. 83–106). New York: Academic Press. (Original work published 1971)
- Grice, H. P. (1975). Logic and conversation. In P. Cole & J. L. Morgan (Eds.), *Syntax and semantics: Vol. 3. Speech acts* (pp. 41–58). New York: Academic Press.
- Grice, H. P. (1978). Further notes on logic and conversation. In P. Cole & J. L. Morgan (Eds.), *Syntax and semantics: Vol. 9. Pragmatics* (pp. 113–127). New York: Academic Press.
- Groefsema, M. (1992). Can you pass the salt? A short-circuited implicature? *Lingua Franca*, *87*, 103–135.
- Higgins, E. T. (1981). The "communication game": Implications for social cognition and communication. In E. T. Higgins, M. P. Zanna, & C. P. Herman (Eds.), *Social cognition: The Ontario Symposium* (Vol. 1, pp. 343–392). Hillsdale, NJ: Erlbaum.
- Hull, C. L. (1933). *Hypnosis and suggestibility: An experimental approach*. New York:

Appleton.

James, W. (1980). *Principles of psychology*. Cambridge, MA: Harvard University Press. (Original work published 1890)

Kahneman, D., & Tversky, A. (1973). On the psychology of prediction. *Psychological Review*, *80*, 237–251.

Kelman, H. C. (1967). Human use of human subjects: The problem of deception in social psychological experiments. *Psychological Bulletin*, *67*, 1–11.

Kihlstrom, J. F. (1998). Exhumed memory. In S. J. Lynn & K. M. McConkey (Eds.), *Truth in memory* (pp. 3–31). New York: Guilford Press.

Kihlstrom, J. F. (2002). To honor Kraepelin. . . : From symptoms to pathology in the diagnosis of mental illness. In L. E. Beutler & M. L. Malik (Eds.), *Alternatives to the DSM* (pp. 279–303). Washington, DC: American Psychological Association.

Kosslyn, S. M. (1980). *Image and mind*. Cambridge, MA: Harvard University Press.

Kruglanski, A. W. (1975). The human subject in the psychology experiment: Fact and artifact. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 8, pp. 101–147). New York: Academic Press.

Latane, B., & Darley, J. M. (1970). *The unresponsive bystander: Why doesn't he help?* New York: Appleton-Century-Crofts.

Laurence, J.-R., & Perry, C. (1988). *Hypnosis, will, and memory: A psycho-legal history*. New York: Guilford Press.

Marks, J. D. (1979). *The search for the "Manchurian candidate": The CIA and mind control*. New York: Times Books.

McClintock, M. K., & Adler, N. T. (1978). The role of the female during copulation in wild and domestic Norway rats (*Rattus norvegicus*). *Behaviour*, *67*, 67–96.

Merton, R. K. (1948). The self-fulfilling prophecy. *Antioch Review*, *8*, 193–210.

Merton, R. K. (1957). *Social theory and social structure*. New York: Free Press.

Milgram, S. (1963). Behavioral study of obedience. *Journal of Abnormal & Social Psychology*, *67*, 371–378.

Milgram, S. (1964). Issues in the study of obedience: A reply to Baumrind. *American Psychologist*, *19*, 848–852.

Milgram, S. (1965). Some conditions of obedience and disobedience to authority. *Human Relations*, *18*(1), 57–76.

Milgram, S. (1968). Some conditions of obedience and disobedience to authority: Reply to the critics. *International Journal of Psychiatry*, *6*, 294–295.

Milgram, S. (1972). Interpreting obedience: Error and evidence. A reply to Orne and

Holland. In A.G. Miller (Ed.), *The social psychology of psychological research* (pp.139–154). New York: Free Press.

Nisbett, R. E., & Ross, L. (1980). *Human inference: Strategies and shortcomings of social judgment*. Englewood Cliffs, NJ: Prentice-Hall.

Nisbett, R. E., & Wilson, D. S. (1977). Telling more than we can know: Verbal reports on mental processes. *Psychological Review*, *84*, 231–253.

Norenzayan, A., & Schwarz, N. (1999). Telling what they want to know: Participants tailor causal attributions to researchers' interests. *European Journal of Social Psychology*, *29*, 1011–1020.

O'Connell, D. N., Shor, R. E., & Orne, M. T. (1970). Hypnotic age regression: An empirical and methodological analysis. *Journal of Abnormal Psychology Monograph*, *76*(3, Pt. 2), 1–32.

Orne, M. T. (1953). Hypnosis, hypnotherapy, and medical practice. *Tufts Medical Journal*, *21*, 14–25.

Orne, M. T. (1959). The nature of hypnosis: Artifact and essence. *Journal of Abnormal and Social Psychology*, *58*, 277–299.

Orne, M. T. (1962a). *Medical uses of hypnosis*. Group for the Advancement of Psychiatry Symposium No. 8.

Orne, M. T. (1962b). On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. *American Psychologist*, *17*, 776–783.

Orne, M. T. (1962c). Psychotherapy and hypnosis: Implications from research. In J. Masserman (Ed.), *Current psychiatric therapies* (pp. 75–85). New York: Grune & Stratton.

Orne, M. T. (1966). Hypnosis, motivation, and compliance. *American Journal of Psychiatry*, *122*, 721–726.

Orne, M. T. (1968). On the nature of effective hope. *International Journal of Psychiatry*, *5*, 404–410.

Orne, M. T. (1969). Demand characteristics and the concept of quasi-controls. In R. Rosenthal & R. Rosnow (Eds.), *Artifact in behavioral research* (pp. 143–179). New York: Academic Press.

Orne, M. T. (1970). Hypnosis, motivation, and the ecological validity of the psychological experiment. In W. J. Arnold & M. M. Page (Eds.), *Nebraska Symposium on Motivation* (pp. 187–265). Lincoln: University of Nebraska Press.

Orne, M. T. (1971). The simulation of hypnosis: Why, how, and what it means. *International Journal of Clinical and Experimental Hypnosis*, *19*, 183–210.

Orne, M. T. (1972a). Can a hypnotized subject be compelled to carry out otherwise unacceptable behavior: A discussion. *International Journal of Clinical and Experimental Hypnosis*, *20*, 101–117.

- Orne, M. T. (1972b). On the simulating subject as a quasi-control group in hypnosis research: What, why, and how. In R. Fromm & R. E. Shor (Eds.), *Hypnosis: Research developments and perspectives* (pp. 399–443). Chicago: Aldine-Atherton.
- Orne, M. T. (1973). Communication by the total experimental situation: Why it is important, how it is evaluated, and its significance for the ecological validity of findings. In P. Pliner, L. Krames, & T. Alloway (Eds.), *Communication and affect* (pp. 157–191). New York: Academic Press.
- Orne, M. T. (1974). Pain suppression by hypnosis and related phenomena. In J. J. Bonica (Ed.), *Advances in neurology* (pp. 563–572). New York: Raven Press.
- Orne, M. T. (1975). Psychotherapy in contemporary America: Its development and context. In D. X. Freedman & J. E. Dyrud (Eds.), *American handbook of psychiatry* (2nd ed., pp. 3–33). New York: Basic Books.
- Orne, M. T. (1977a). The construct of hypnosis: Implications of the definition for research and practice. *Annals of the New York Academy of Sciences*, 296, 14–33.
- Orne, M. T. (1977b). The search for specific treatments in psychiatry. In J. P. Brady, J. Mendels, M. T. Orne, & W. Rieger (Eds.), *Psychiatry: Areas of promise and advancement* (pp. 237–247). New York: Spectrum.
- Orne, M. T. (1978). Let's consider the alternatives in light of what the psychiatrist does. In J. P. Brady & H. K. H. Brodie (Eds.), *Controversy in psychiatry* (pp. 43–81). Philadelphia: Saunders.
- Orne, M. T. (1979a). Cognitive factors in the control of pain. In L. Grossman (Ed.), *Mechanism and control of pain. Transactions of the 6th International Conference on Endodontics* (pp. 213–228). New York: Masson.
- Orne, M. T. (1979b). The efficacy of biofeedback therapy. *Annual Review of Medicine*, 30, 489–503.
- Orne, M. T. (1980a). Assessment of biofeedback therapy: Specific versus nonspecific effects. In A. P. Association (Ed.), *Biofeedback: report of the Task Force on Biofeedback of the American Psychiatric Association* (pp. 12–32). Washington, DC: American Psychiatric Association.
- Orne, M. T. (1980b). *Biofeedback: Report of the Task Force on Biofeedback of the American Psychiatric Association*. Washington, DC: American Psychiatric Association.
- Orne, M. T. (1980c). Nonpharmacological approaches to pain relief: Hypnosis, biofeedback, placebo effects. In L. Ng & J. J. Bonica (Eds.), *Pain, discomfort, and humanitarian care* (pp. 253–274). New York: Elsevier North Holland.
- Orne, M. T. (1980d). On the construct of hypnosis: How its definition affects research and its clinical application. In G. Burrows & L. Dennerstein (Eds.), *Handbook of hypnosis and psychosomatic medicine* (pp. 29–51). Amsterdam: Elsevier North-Holland Biomedical.
- Orne, M. T. (1981). The significance of unwitting cues for experimental outcomes: Toward a pragmatic approach. *Annals of the New York Academy of Sciences*, 364, 152–159.

Orne, M. T. (1982). Seventeen perspectives in biofeedback: Ten years ago, today, and . . . In L. White & B. Tursky (Eds.), *Clinical biofeedback: Efficacy and mechanisms* (pp. 422–437). New York: Guilford Press.

Orne, M. T. (1984). Psychotherapy: Toward an appropriate basis for reimbursement. In Z. Taintor, P. Widen, & S. Barrett (Eds.), *Cost considerations in mental health treatment: Settings, modalities, and providers* (pp. 55–60). Washington, DC: National Institute of Mental Health.

Orne, M. T., & Evans, F. J. (1965). Social control in the psychological experiment: Antisocial behavior and hypnosis. *Journal of Personality and Social Psychology, 1*, 189–200.

Orne, M. T., & Evans, F. J. (1966). Inadvertent termination of hypnosis with hypnotized and simulating subjects. *International Journal of Clinical and Experimental Hypnosis, 14*, 71–78.

Orne, M. T., & Holland, C. H. (1968). On the ecological validity of laboratory deceptions. *International Journal of Psychiatry, 6*, 282–293.

Orne, M. T., & McConkey, K. M. (1981). Hypnosis and self-hypnosis. In L. Kristal (Ed.), *The ABC of psychology* (pp. 115–118). London: Multimedia.

Orne, M. T., & Wender, P. H. (1968). Anticipatory socialization for psychotherapy: Method and rationale. *American Journal of Psychiatry, 124*, 1202–1212.

Rosenthal, R. (1963). On the social psychology of the psychological experiment: The experimenter's hypothesis as unintended determinant of experimental results. *American Scientist, 51*, 270–282.

Rosenthal, R., & Rosnow, R. L. (1969). The volunteer subject. In R. Rosenthal & R. L. Rosnow (Eds.), *Artifact in behavioral research* (pp. 59–118). New York: Academic Press.

Rosnow, R. L. (1981). *Paradigms in transition: The methodology of social inquiry*. New York: Oxford University Press.

Rowland, L. W. (1939). Will hypnotized persons try to harm themselves or others? *Journal of Abnormal & Social Psychology, 34*, 114–117.

Schacter, D. L. (1987). Implicit memory: History and current status. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 13*, 501–518.

Schefflin, A. W., & Shapiro, J. L. (1989). *Trance on trial*. New York: Guilford Press.

Schwarz, N. (1994). Judgment in a social context: Biases, shortcomings, and the logic of conversation. In M. Zanna (Ed.), *Advances in experimental social psychology* (pp. 123–162). San Diego, CA: Academic Press.

Schwarz, N. (1995). What respondents learn from questionnaires: The survey interview and the logic of conversation (1993 Morris Hansen Lecture). *International Statistical Review, 63*, 153–177.

Schwarz, N. (1996). *Cognition and communication: Judgmental biases, research methods,*

and the logic of conversation. Mahwah, NJ: Erlbaum.

Searle, J. R. (1979). Indirect speech acts. In J. R. Searle (Ed.), *Expression and meaning*. Cambridge, England: Cambridge University Press. (Original work published 1975)

Shapiro, A. K., & Shapiro, E. (1997). *The powerful placebo: From ancient priest to modern physician*. Baltimore: Johns Hopkins University Press.

Shor, R. E., & Orne, E. C. (1962). *Harvard Group Scale of Hypnotic Susceptibility, Form A*. Palo Alto, CA: Consulting Psychologists Press.

Shor, R. E., & Orne, E. C. (1963). Norms on the Harvard Group Scale of Hypnotic Susceptibility, Form A. *International Journal of Clinical & Experimental Hypnosis*, *11*, 39–47.

Siegler, M., & Osmond, H. (1974). *Models of madness, models of medicine*. New York: Harper & Row.

Silverman, I. (1977). *The human subject in the psychological experiment*. Elmsford, NY: Pergamon Press.

Sloane, R. B., Cristol, A. H., Pepernik, M. C., & Staples, F. R. (1970). Role preparation and expectation of improvement in psychotherapy. *Journal of Nervous & Mental Disease*, *150*, 18–26.

Snyder, M. (1984). When belief creates reality. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (pp. 247–305). New York: Academic Press.

Soskis, D. A., Orne, E. C., Orne, M. T., & Dinges, D. F. (1989). Self-hypnosis and meditation for stress management. *International Journal of Clinical and Experimental Hypnosis*, *37*, 285–289.

Strack, F., & Martin, L. (1987). Thinking, judging, communicating: A process account of context effects in attitude surveys. In H. J. Hippler, N. Schwarz, & S. Sudman (Eds.), *Social information processing and survey methodology* (pp. 123–145). New York: Springer.

Taylor, S. E. (1995). *Health psychology* (3rd ed.). New York: McGraw-Hill.

Tversky, A., & Kahneman, D. (1974). Judgment under uncertainty: Heuristics and biases. *Science*, *185*, 1124–1131.

Weber, S. J., & Cook, T. D. (1972). Subject effects in laboratory research: An examination of subject roles, demand characteristics, and valid inference. *Psychological Bulletin*, *77*, 273–295.

Young, P. C. (1950). The possibility of antisocial uses of hypnosis. *American Psychologist*, *5*, 327–328.

Young, P. C. (1952). Antisocial uses of hypnosis. In L. M. LeCron (Ed.), *Experimental hypnosis* (pp. 376–409). New York: Macmillan.

Footnotes

¹ Apparently this sentence first appeared in articles by [Gordon and Lakoff \(1971/1975\)](#), [Searle \(1975/1979\)](#), and Clark ([1979](#); [Clark & Lucy, 1975](#)). According to Herbert Clark (personal communication, June 26, 1995), there also exists a satirical paper entitled "Can You Pass the Salt?" or some such. See also [Groefsema \(1992\)](#).

² This possibility has also been raised by Norbert Schwartz, Fritz Strack, and their colleagues in a number of articles (e.g., Bless, Strack, & Schwarz, 1993; [Schwarz, 1995](#); [Strack & Martin, 1987](#)).

³ In terms of this analysis, the injunction of the of the *APA Publication Manual*, beginning in the fourth edition, that authors substitute *participants* for *subjects* when talking about human beings, may be politically correct but it is not psychologically correct. Both the experimenter and the subject are participants in the social encounter known as *the experiment*, along with laboratory technicians and even scheduling assistants, and each has his or her own unique role to play in that encounter. The experimenter is the experimenter, and the subject is the subject, and we should call them what they are.

⁴ The experiment was the inspiration for *The Tenth Level*, a 1976 television movie, starring William Shatner as a Milgram-like scientist, that won honorable mention in the 1977 media awards from the American Psychological Association.

⁵ These role relationships characterize what [Siegler and Osmond \(1974\)](#) call the *clinical medical model*, in which the doctor's responsibility is to cure disease in an individual patient. In the *public health medical model*, the doctor is replaced by the public health official, who can enforce his or her prescriptions for the good of the public at large, and the patient is replaced by the citizen, who may occasionally be harmed by the official's decisions. In the *scientific medical model*, the doctor is replaced by the researcher, who has no direct obligation to prevent or cure disease, and the patient is replaced by the subject, who volunteers his or her services in return for remuneration. Each version of the medical model contains its own special collection of demand characteristics.