Clinical Innovation and Evaluation: Integrating Practice with Inquiry

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We explore the complex interplay of clinical discovery and controlled evaluation, demonstrating how experience in the applied arena provides invaluable insights and ideas about the complexity of the human condition and of ways to intervene effectively. Case studies have features that earn them a firm place in psychological research, and to ignore their potential contributions is to limit severely the kind of knowledge that can be generated by more systematic modes of inquiry. Some limitations of group designs in comparative therapy research are also reviewed, again highlighting the importance of idiographic analyses of single cases. Innovation and creative advancement are most readily nurtured via immersion in clinical/applied work, but at the same time the nature of that work is inevitably shaped by theories and hypotheses that clinicians bring into the applied setting. These abstractions are themselves influenced by the clinician’s interpretations of data, which interpretations are molded by theoretical and metatheoretical preconceptions. In this complex and interactive fashion, clinical innovation is part of a nonlinear network of forces that includes personal biases, professional allegiances, epistemological assumptions, theoretical preferences, and familiarity with and use of certain bodies of data.

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At a recent professional meeting, a colleague was discussing a new and unusual clinical procedure that its proponents had reported the previous year in a clinical case study—a technique that, it was claimed, could eliminate long-standing fears that had proven recalcitrant to other interventions. "How could such a thing be possible?" we asked with the utmost skepticism and even derision, all the while harboring the hope that the clinical results were valid and replicable. The response from our trusted friend: "Look, who knows if it works or not, or how it works if it does work? But I do know two things: First, we can't afford to ignore a promising finding; and second, the people making the claims have their heads screwed on straight.' Continued our friend: "So I've begun doing some controlled laboratory research on the procedure. I'll call you next year when my results are in." Our own reply to him: "Fine, Tom, we're impressed that you're impressed enough to spend some time on it. If your experiments tell us that the clinical reports are probably true or at least believable, we'll be after you to teach us how to do it."

There are a couple of points we wish to make with this little exchange. First, innovations by clinicians are the lifeblood of advances in the development of new therapeutic interventions. Second, whether or not attention is paid to a discovery—especially if that discovery borders on the unbelievable—depends in large measure on a prior pro hominem judgment we have made about the integrity and standing of the creative clinician. And third, clinical innovations are often (though definitely not invariably or even primarily) investigated by more experientially minded workers whose subsequent findings may persuade others that the previously unbelievable technique is worth a closer look.

In what follows we try to make explicit the nature of the complex interplay between clinical innovation and controlled evaluation. The importance of attending to the idiographics of individual cases is also explored in a
discussion of the inherent limitations of group designs in
sheding light on how particular patients change in ther-
apy research. Our sometimes abstract discussions should
be considered in the light of the highly personal judg-
ments that clinical workers and scientists make about
what is worth a closer look. This article is not a review
of comparative outcome studies leading to conclusions
about the superiority of one theory or technique over
another. Like an earlier effort (Lazarus & Davison, 1971),
it is written from the perspectives of both authors and
is therefore biased in the choice of examples and in the
preference for a cognitive-behavioral approach.

CLINICAL INNOVATION AND CONTROLLED
RESEARCH
Many regard the laboratory and the clinic as opposite
ends of a continuum. Research is said to be precise, con-
trolled, and uncontaminated. The ideas that flow from
applied settings are often regarded as woolly, riddled with
bias, purely anecdotal, and even useless. Our abiding
belief is that the path between the laboratory and the
clinic is a two-way street (Woofolk & Lazarus, 1979). We
aver that most new methods have come from the work
of creative clinicians. Furthermore, as we hope to show,
the process of discovery that is carried on within the cli-
ナル practices of some therapists is the equivalent of
research.

Scientists and practicing clinicians can each offer
unique contributions in their own right and can conceiv-
ably open hitherto new and unsuspected clinical-
experimental dimensions for research and practice. Ideas
tested in the laboratory may be applied by the practitioner
who, in turn, may discover important individual nuances
that remain hidden from the laboratory scientist simply
because the tight environment of the experimental test-

ground makes it impossible for certain behaviors to
occur or for certain observations to be made. Conversely,
ideas formulated in the clinic, provided that they are
amenable to disproof, can send scientists scurrying off
into laboratories and other research settings to subject the
claims of efficacy to controlled tests. Cases in point are
cited further on.

While it is proper to guard against ex cathedra state-
ments based upon flimsy and subjective evidence, it is a
serious mistake to discount the importance of clinical ex-
perience per se. There is nothing mysterious about the
fact that repeated exposure to any given set of conditions
makes the recipient aware of subtle cues and contingen-
cies in that setting which elude the scrutiny of those less
familiar with the situation. Clinical experience enables a
therapist to recognize problems and identify trends that
are usually beyond the perceptions of novices, regardless
of their general expertise. It is at this level that new ideas
come to the practitioner and often constitute break-
throughs that could not be derived from animal analogues
or tightly controlled investigations. Different kinds of
data and differing levels of information are obtained in
the laboratory and the clinic. Each is necessary, useful,
and desirable.

CLINICAL INNOVATION AND EXPERIMENTATION
A valuable perspective upon the clinical research enter-
prise should follow from a concerted effort to make the
process of clinical experimentation and innovation more
explicit. Innovation is the outcome of experimentation,
for it is when we try new things that true innovators have
the capacity to appreciate relationships that may go unno-
ticed by less resourceful, less observant workers.

Most practitioners, for instance, have discovered how
difficult it is to console many individuals immediately fol-
lowing an important loss in their lives (such as death of a
friend, dismissal from a job, rejection by a lover, or a simi-
lar deprivation). The ensuing depression often remains
unaltered by reassurance. Supportive therapy over several
weeks, notwithstanding the ever-present risk of suicide in
these cases, often heralds the diminution of the patient's
reported misery. In the present context, the clinical inno-
vator is the person who addresses the problem of what
can be done to facilitate rapid recovery from such a
depression. This attitude demands some form of experi-
mentation. The actual experimental operations usually
are determined in part by the therapist's own theoretical
orientation. For instance, a therapist with a proclivity for
organic notions will be more inclined to search for an
effective combination of drugs or some other biological
mode of intervention. The psychologically oriented ther-
apist will search for effective psychosocial procedures.
The cognitive theorist might look for newer and deeper
mediating belief systems that mitigate subjective misery
rather than for novel means of psychomotor expressiv-
ness, which those who espouse various experiential theo-
ries might be inclined to develop. Occasionally, a sense
of desperation may lead a clinician to make a response
that fits neither his or her theoretical preconceptions nor
his or her more usual empirical resources. Many clinical advances are preceded by what we might term a frustration-observation sequence.

Let us consider the practitioner who has expended energy, time, and effort to alleviate the suffering of a somewhat depressed but extremely demanding individual. The therapist's fund of methods and techniques has been exhausted to no avail. Despite attempts to intervene at the level of family relationships, to tap underlying guilts and hostilities, to assess for and alter biased thinking and dysfunctional beliefs, and to ply the patient with appropriate medication and inspiration, the net result is a demanding and threateningly dependent person whose desperation evokes anxiety in the therapist. At this stage, the harassed and perplexed practitioner may advocate a course of action dictated solely by pragmatic convenience rather than by theoretical confidence. Out of keeping with his or her usual practices, the therapist may confine the patient to bed for 10 days and forbid any patient-therapist communication during this period. In all candor, the therapist's principal motive might simply be to "get the patient off my back" for a while. Ten days later, the patient is seen again and, quite remarkably, reports feeling much better.

Unplanned or unexpected clinical improvements are often dismissed as "fortuitous events" or "spontaneous remissions," but the clinical innovator is the one who carefully notes a variety of possible cause-effect sequences and thus discovers therapeutic levers that less inquisitive colleagues are apt to overlook. A propitious clinical outcome might stimulate innumerable questions. In the hypothetical case already mentioned, one might simply pose the obvious question: "Of what value might enforced bed rest be for certain cases of depression?" The clinician might then look for an additional case presenting with similar problems and try the bed-rest intervention, this time not out of frustration but in an increasingly systematic effort to evaluate its potential effectiveness. If favorable changes are again observed, discussion at professional meetings and publication of case reports can set the stage for comparing experiences with other practitioners and conceivably stimulate experimental design, in its most rigorous sense, to reconcile identical empirical facts within divergent theoretical models. The efficacy of the aforementioned "bed-rest hypothesis," if empirically established, will be...
explained organically by organicists, psychoanalytically by psychoanalysts, behaviorally by behaviorists, and so forth. All too often a useful method will be employed by practitioners of different theoretical persuasions only if it can be "explained" according to their own favorite theories.

A common avenue of clinical experimentation consists of the development of techniques arising out of the therapist's predilections. This was the route followed by most of the psychoanalytic offshoots. Very often, although departing from his teachings and generating independent hypotheses of their own, Freud's former pupils did not deviate very widely in matters of technique-free association, dream interpretation, and analysis of transference retained their preeminence. The differences revolved around points of emphasis, timing, and content of interpretations. The respective deviations in technique were usually dictated by the different theoretical views that the Freudian revisionists espoused (although none of them systematically evaluated the effects of their innovations).

It stands to reason that a theorist who believes that emotional disturbances arise out of feelings of inferiority might develop and use different methods and techniques than a therapist who holds to a theory of unconscious sexual repression. The grave error is then to assume that if a technique proves successful in achieving its desired results, the process that gave rise to it is thereby necessarily strengthened or confirmed. For example, a Rankian might have reasoned that a depressed individual is actively reliving the birth trauma and craving an intrauterine respite. Employing enforced bed rest as a symbolic return to the womb, and then discovering a clinical improvement in X number of patients, the committed Rankian is most likely to resist the notion that the clinical outcome might be unrelated to Rankian theories about the basic therapeutic process. Techniques may, in fact, prove effective for reasons that do not remotely relate to the theoretical ideas that gave birth to them.

There is another side to the theory/practice issue, however, that we feel is sometimes dismissed. When selecting therapeutic techniques it matters very much which theoretical notions a clinician espouses during the conduct of all clinical activities. For example, if one assumes that a given phobic reaction is best conceptualized as an anxiety-avoidance gradient, and furthermore is not secondary to a basic underlying condition that is the proper focus for treatment, one is more likely to employ, with confidence, a technique like desensitization or other exposure-based interventions (Wolpe, 1958). Conversely, if one holds to a view that all phobias are adaptive to the extent that they protect the individual from libidinal impulses that would be devastating were they allowed expression, it would seem that the clinician might choose to dwell upon the presumed unconscious conflicts and ignore the manifest phobia. This is not to say that only one particular theoretical stance will lead to a particular intervention; rather, it is to say that the "set" with which a clinician approaches a problem determines the clinician's own clinical behavior and view of what occurs. This is one reason why we advocate caution, tentativeness, and empirical testing when adopting any theoretical position. Often such positions harden commitments rather than facilitate discovery, which is the real purpose of theories.

Once one has assimilated certain theoretical constructs, it is necessary to apply these nomothetic principles to an idiographic case (Levine, Sandeen, & Murphy, 1992). Gordon Allport (1937) was identified with the so-called "nomothetic-idiographic controversy," but Maher (1966) made a convincing argument against a necessary incompatibility between these two approaches. The application of a general principle in a particular case depends not only on a familiarity with the principle but also on an accurate assessment of the given case. The example Maher uses is that of an engineer who must build a bridge across a particular river: "In order to build a bridge over a certain river, we must know the details of the soil mechanics, water flow, prevailing winds, topography, traffic usage, availability of labor and materials, and so on. When we consider all these, the total picture might not be like any other bridge that has ever been built. Nevertheless, none of the principles or assumptions that go into the final decisions could be made in contradiction to the laws of physics, economics, and the like" (p. 112). We return to this important issue later.
use the resultant methods and findings to communicate what we mean by clinical experimentation. Other endeavors (e.g., Kazdin, 1994) concentrate on a wide range of research and design issues in clinical research and in many ways complement our own efforts.

When creative clinicians learn new things from patients and invent new procedures to resolve difficult problems, they are conducting a form of research. There is usually a series of clinical trials or experiments in which reactions of a patient or several patients to the procedure are observed. How can clinicians make these discoveries, and how can they learn about therapeutic change and invent techniques for promoting it? A complete awareness of what information can and cannot be derived from case studies constitutes the first essential step toward the achievement of any meaningful clinical innovations that deserve to be labeled "research."

As we articulated in an earlier publication (Lazarus & Davison, 1971), there seem to us to be several characteristics unique to case studies that earn for them a firm place in psychological research. We outline them below and then elaborate on each one:

1. A Case Study May Cast Doubt Upon a General Theory.

   The successful handling of a particular case may underscore an important exception to a theory. For example, a given theory may hold that a certain kind of problem is untreatable. If a therapist succeeded in making an impact upon the recalcitrant problem, this would cast doubt upon the tenets of the theoretical viewpoint under consideration.

   A particular theory may also predict that certain methods will prove antitherapeutic. For example, when a fairly elaborate case history was presented to two different audiences—one made up mainly of psychodynamic practitioners, the other comprising clinicians who espoused a family systems perspective—both groups predicted rapid relapse for different reasons (Lazarus, 1989a). The case was that of a 32-year-old man with multiple complaints, including generalized anxiety, depression, obsessive-compulsive problems, somatization, agoraphobia, and social isolation. The treatment followed a broad-based cognitive-behavioral approach (multimodal therapy; Lazarus, 1989b). Because certain psychodynamic issues were not addressed or resolved, the practitioners who identified themselves with psychodynamic theory asserted that the unresolved conflicts would render the treatment gains impermanent and ephemeral. One spokesperson made the dire prediction that the client might decompensate and end up in a mental hospital within 3 to 5 years. The family systems therapists correctly pointed to the powerful mother-son enmeshment that was a fundamental part of the client’s problems, and they contended that the improper management of this dyadic system (from their theoretical standpoint), plus other triangulating and scapegoating issues that were bypassed, heralded only a temporary remission. The fact that a 7-year follow-up revealed that the client had maintained and further extended his therapeutic gains brings some of their respective theoretical notions into serious question. Let it be remembered that only one clearly negative instance is sufficient to cast doubt on any general hypothesis.

2. A Case Study May Provide a Valuable Heuristic to Subsequent and Better Controlled Research.

   Case studies in clinical psychology are probably best known for suggesting new directions that can be pursued systematically by laboratory investigators. Examples are legion. The research in systematic desensitization that virtually exploded in the late 1960s into the 1970s probably would not have been undertaken without the clinical successes being reported by several pioneering practitioners (e.g., Wolpe & Lazarus, 1966). The cognitive-behavioral therapy movement of the later 1970s that extends into the present is derived largely from the clinical reports and theoretical propositions first propounded by Ellis (1962) and Beck (1967). The role of small-N or single-case studies is thoroughly described in a volume dedicated exclusively to case study methodology (Yin,
The limitations of comparative group designs are discussed in a later section. It has been argued that if we are ever to discover what aspects of a particular technique result in therapeutic change, and exactly how this is achieved, global outcomes must be broken down into a series of small interrelated changes. Single-case methodology and designs are ideal for this purpose.

Let us turn to an example of a case study that led to subsequent and well-controlled research. In a book of Lazarus's collected papers, Dryden (1991) included the account of a 19-year-old woman with a severe hand-washing compulsion. She had been treated in the early 1960s, and Dryden comments: "It is interesting to note how Lazarus struggled to find a means whereby the client's compulsive washing could be eliminated. In retrospect, he finally administered a form of 'response prevention', which is now considered an indispensable condition in the amelioration of most compulsive habits" (p. 36).

Initially, some of the best researched and well-documented methods of the 1960s had been applied in this case with great diligence but without success—progressive relaxation, systematic desensitization, and covert sensitization. Finally, the use of a portable faradic shock unit that delivered an unpleasant but entirely safe and nondamaging electrical impulse to her upper arms, was brought into play whenever she overdid any hand-washing. This resulted merely in a temporary hand-washing avoidance, but did not lessen her desire or urge to wash. It was only when the procedure dramatically increased the latencies between her hand-washing urges and the act of self-cleansing (i.e., protracted response prevention), that significant clinical gains accrued. Subsequently, "avoidance of the reinforcing habit" was identified as the active treatment ingredient. Thereafter, in similar cases, the "aversion therapy" was discarded, and response prevention became the mainstay (see Spiegler & Guerremont, 1993). More recent therapies for obsessive-compulsive disorders point to the synergistic impact of adding medication (e.g., clomipramine) and cognitive restructuring to exposure and response prevention (Franks, Wilson, Kendall, & Foreyt, 1990).

Davison's research on Articulated Thoughts in Simulated Situations (ATSS) (Davison, Robins, & Johnson, 1983) is another example of the way in which case studies can provide a valuable heuristic to subsequent and better controlled research (see Davison, Navarre, & Vogel, in press; Davison & Neale, 1994).

3. A Case Study May Permit the Investigation, Although Poorly Controlled, of Rare but Important Phenomena.

Human beings are capable of harming themselves, and others, in the most unusual ways. It is the practicing clinician who is most likely to encounter the vagaries of human conduct. Unusual case reports from "field observers" can add to clinical and experimental knowledge. For example, one of us was consulted some years ago by a young married couple who practiced a peculiar sexual ritual. As a prelude to sexual intercourse, the young man would draw blood by cutting a small incision on the palm of his wife's right hand. She would then stimulate his penis, using the blood of her right palm as a lubricant. Normal intercourse would then ensue, and the moment the wife felt her husband ejaculating, she was required to dig her nails deep into the small of his back or buttocks. Each sadomasochistic act was followed by guilt and remorse. But the most serious consequence of these abnormal practices was the fact that repeated septic wounds tended to develop. The couple in question were both intelligent college graduates who had indulged in these gory activities intermittently for 3½ years. They were vague and uncertain about the beginnings of their strange behavior. Signs of delusions, hallucinations, concrete or overinclusive thinking, and other psychotic behaviors were not present.

Knowledge, in cases such as this, can be advanced in two ways. First, a detailed study could conceivably throw light on the genesis of the problem and thereby add new insights into human aberrations. Secondly, a successful treatment strategy applied in this case may have relevance for overcoming other deviant behaviors. In regard to the first consideration, only vague and very tentative inferences could be drawn. At best, there appeared to be some associations with menstruation (probably safe vis-a-vis possible pregnancy), but the husband's connection between blood per se and sexual excitement remained obscure. It is possible that an extended analysis of the couple may have proved enlightening, but a remarkably simple remedy proved so successful that they were no longer motivated to undergo further therapy. When it transpired that they had desisted from engaging in their sadomasochistic acts since commencing treatment, but
that the husband had been completely impotent, the wife was advised to obtain a harmless red dye and add it to any transparent lubricant. With the use of this mixture, the husband's potency was restored and the need for actual bloodletting was obviated. The same mixture applied to the husband's buttocks at the commencement of the practice was recommended in place of a painful and harmful routine. For reasons unknown, the birth of their first child put an end to their unusual practices altogether. Here again there is tremendous potential for clinical discovery.

4. A Case Study Can Provide the Opportunity to Apply Principles and Notions in Entirely New Ways.

The clinical setting affords the opportunity and challenge to develop new procedures based on techniques and principles already in use. It is a truism that one will look in vain for the "textbook case." Clinicians are often faced with problems for which existing procedures seem unsuitable or insufficient. At the same time, certain aspects of a particular clinical problem may call for a new way of relating old principles and procedures to the resolution of the problem. This issue is related to Point 6 below, but nevertheless seems worthy of separate illustration here.

In one of our early case reports (Davison, 1966), some "tried and true" procedures were employed in a novel context. The use of deep-muscle relaxation has an extensive history in medicine, clinical psychology, and psychiatry. The many and varied applications of relaxation probably share the implicit or explicit purpose of reducing subjective feelings of anxiety. In the case described below, it was possible to use relaxation in a different way to handle a problem that was hitherto considered unapproachable by relaxation training. Clinical innovation implies the discovery that "old" methods can be applied to new problems, as well as the discovery of new methods for overcoming common but seemingly intractable syndromes.

The case was that of a middle-aged, male hospital patient diagnosed as "paranoid schizophrenic," primarily on the basis of his complaints about "pressure points" on his forehead and in other parts of his body. These so-called pressure points were believed by him to be signals from outside forces impelling him toward certain decisions. The man had received treatment for 2 months without any change in these pressure points. In fact, he had even managed to have the medical staff approve the removal of a cyst over his right eye in the hope that this might remove the pressure points. Unfortunately, this had no effect upon his paranoid delusions. Because of their theoretical orientation, the psychiatrists and residents had been restricting their clinical investigations to his past history and, not surprisingly, were finding events in his past to which they assigned considerable etiological significance. Nonetheless, the pressure points remained unabated. The therapist met this man in a Grand Ward Round in a psychiatric hospital, during which he inquired of the patient whether he would describe himself as a "tense" or "anxious" individual. This aspect of the clinical picture had been largely ignored by the presenting physician. When the patient reported that he was indeed very anxious, the therapist agreed to attempt therapy with him as a demonstration case.

During the first session, the therapist concentrated on clearly delineating those situations in which the man became particularly aware of his pressure points. The patient was able to identify several such situations that were, at the same time, clearly anxiety-provoking. For example, being a truck driver, he would often get the pressure points when he was lost and late with a truckload of goods. He then saw them as helpful in deciding how to reach his destination. This led the therapist to inquire whether decision-making situations of any importance were, in general, anxiety-provoking. Indeed they were, and indeed they set the occasions for the most frequent occurrence of his pressure points.

Having satisfied himself that there was a close relationship between anxiety and the pressure points, the therapist asked the patient to extend his right arm, clench his fist, and slowly bend the wrist downwards so as to bring the closed hand toward the inside of his forearm. The intent was to produce a feeling of severe muscle tension in the forearm, and this is precisely what the patient reported. He reported, also, however, that it felt very much like a pressure point. The therapist, believing that he had a good enough relationship with the man to avoid being thrust into the patient's delusional system by disagreeing with him, suggested an alternative interpretation of the pressure points: perhaps they were simply a consequence of his becoming tense and anxious in particular kinds of situations. It was suggested to the man that, in
the absence of a naturalistic scientific explanation, he, like other people, tended to explain strange occurrences in somewhat supernatural or mystical terms. The patient agreed that the merit of the therapist's hypothesis was that it seemed amenable to an empirical test. The means would be to train him in deep muscle relaxation and then to determine whether the relaxation could control the occurrence of the pressure points. The man consented to this, and relaxation training was undertaken. Conventional relaxation therapy extended over several weeks. Outside of therapy, the man was instructed to pay careful attention to the occurrence of the pressure points and to confirm or weaken the assumed connection between anxiety, especially in decision-making situations, and the emergence of troublesome pressure points. The man cited enough occurrences to confirm the hypothesis, and as he was becoming more and more proficient in relaxation, he also reported some degree of control over the intensity and even the persistence of the pressure points by means of differential relaxation. After eight additional sessions over a 9-week period, the man was beginning to refer to the pressure points as "sensations," and his conversation was generally losing its "paranoid flavor."

What we have here is the application of differential relaxation as a means of testing a nonparanoid hypothesis about bodily sensations. Clearly, there is much more to the case than can be explained by relaxation principles alone. For instance, it is likely that new cognitions were induced simply via persuasion. Nevertheless, a functional analysis of the man's clinical picture led to the hypothesis that the pressure points were part of a general anxiety reaction to specific kinds of situations. While it is possible that the pressure points had complex symbolic meanings for the patient, relaxation was effective in controlling the sensations. This helped the patient to account for the sensations in terms of a tension reaction rather than as a product of external forces. That the man became less paranoid as therapy proceeded does suggest that the use of differential relaxation in conjunction with what was called "cognitive restructuring" was indeed an important element in the therapy. Furthermore, having the patient create his own pressure points and then apply learned relaxation skills to reduce them as a way to alter their meaning is similar to an important component of Barlow's empirically validated therapy for panic disorder. By spinning in a chair or repeatedly climbing up and down a step, the patient learns that sensations hitherto interpreted as an impending panic attack are actually controllable by relaxation or other coping skills and therefore nothing to fear (e.g., Craske & Barlow, 1993). There is a growing body of research attesting to the clinical efficacy of Barlow's treatment.

5. A Case Study Can, Under Certain Circumstances, Provide Enough Experimenter Control Over a Phenomenon to Furnish "Scientifically Acceptable" Information.

Thus far we have at least implicitly accepted the commonly held view that case reports are intrinsically uncontrolled. However, one can look to the work of the Skinnerians in both laboratories and clinical settings for disproofs of this point of view. As has been documented in many places, one can establish a reliable baseline for the occurrence of a given behavior in an individual case and then demonstrate changes that follow the alteration of a particular contingency. Then we may return the behavior to its original level by changing the contingency once again. This is the familiar A-B-A design; numerous and ingenious variations on the basic reversal design have been described elsewhere (e.g., Barlow & Hersen, 1984; Hayes & Leonhard, 1991; Kazdin, 1982).

Much of the earliest controlled work in behavior therapy was conducted within the Skinnerian framework. Especially noteworthy were the efforts of Wolf, Bijou, and Baer at the University of Washington, and of Ayllon and Azrin (1968) at Anna State Hospital in Illinois in the 1960s. To illustrate the work of the former group, we describe a case in which it became necessary to reinstate walking in a 6-year-old autistic child who had regressed to the point where he crawled around on his hands and knees more than 80% of the time. This was achieved by instructing his teachers to offer him candy and social reinforcement (attention and praise) intermittently for walking, while completely ignoring him when he was crawling. Within 2 weeks the child walked normally and seldom crawled. One of the teachers questioned the relevance of the reinforcement contingencies and maintained that it was merely noncontingent love and approval that altered the child's behavior. To test this hypothesis, the teachers were again directed to offer "love and approval," only this time to make it coincide with crawling while ignoring the child when he was walking. In less than a week the child had reverted to pretreatment levels of crawling. Finally, by reversing the contingencies once
more, he stopped crawling and resumed normal walking. The control of the child’s crawling by reinforcement contingencies constitutes reliable scientific data on the nature of this behavior (Harris, Johnston, Kelley, & Wolf, 1964). (Under many circumstances it would be unethical or impractical to reverse contingencies in order to reinstate problem behaviors. There are alternatives in single-subject research, such as multiple baseline designs, that eliminate the need for reversals.) Present-day corroborations and extensions of these paradigms have been presented by Butterfield and Cobb (1994).

6. A Case Study Can Assist in Placing "Meat" on the "Theoretical Skeleton."
The reader will recall our earlier suggestion that the theoretical notions to which clinicians subscribe bear importantly on the specific decisions they make in a particular case. Clinicians in fact approach their work with a given set, a framework for ordering the complex data that are their domain. But frameworks are insufficient. The clinician, like any other applied scientist, must fill out the theoretical skeleton. Individual cases present problems that always call for knowledge beyond basic psychological principles.

Illustration of this point can be underscored by referring to desensitization procedures in general and phobic reactions in particular. The general technique of desensitization has been detailed quite specifically (e.g., Wolpe, 1990). In the management of less simple and straightforward cases, however, the mechanistic sequences may not hold up. In these instances, the "meaty" issues involve decisions about precisely what idiosyncratic variations to place on the hierarchy, whether desensitization is even appropriate to the case, and if so, whether crucial dimensions of anxiety have been properly spelled out. Apart from relaxation and positive imagery, what other easily applicable antianxiety responses can be employed within a desensitization framework? This question became fairly urgent when it was found that several people who were resistant to relaxation procedures were also having difficulty in conjuring up vivid images (Lazarus & Mayne, 1990). In placing further "meat" on the "theoretical skeleton," various cognitive procedures have been added to the usual desensitization sequence (Davison & Neale, 1994, pp. 556-557). These additions probably would not have arisen if difficult individual cases had not called for revisions, refinements, and extensions of existing methods. Here again it is likely to be the practitioner who is compelled to amplify theories and techniques in order to accommodate individual variations that expose deficiencies in our existing areas of knowledge.

LIMITATIONS OF GROUP DESIGNS AND THE NEED FOR OBJECTIFIED SINGLE-CASE STUDIES
Clinicians are usually concerned with particular cases. Since group designs, such as those used in the usual comparative outcome studies, provide information on averages, therapy researchers have long appreciated their limitations in informing the practitioner about how to proceed in the individual case. As alluded to earlier, this dialectical tension between the nomothetic and the idio graphic has been a theme in psychology at least since Gordon Allport's classic writings on personality (e.g., Allport, 1937). The pros and cons of single-case methodology have been thoroughly analyzed by Hilliard (1993).

There is, however, an important limitation of group research that is seldom if ever discussed. Consider the simplest of all therapy studies, involving an experimental group and a placebo control group. We have become accustomed over the years to expect some degree of improvement in placebo groups, sometimes even to the degree that within-condition changes are significant. The researcher, of course, hopes that any such improvement will be exceeded by positive changes in the experimental condition. But consider the following frequently encountered situation: Subject A in the experimental group improves significantly, and Subject B in the placebo control group improves to the same degree. Can we attribute the improvement of Subject A to a particular feature of the experimental condition? Another way to put the question is as follows: Given that Subject A improved in the experimental condition, can we say he would not have improved to the same degree if he had been assigned to the control condition (for Subject B showed the same improvement, and it is common to find some degree of improvement even in placebo conditions)? Furthermore, since placebo elements are admitted a part of the experimental condition—hence the inclusion of a placebo control group—can we say with confidence that Subject A’s improvement was not due to the placebo elements inherent in the experimental condition? We suggest that the answers to these questions is no.

Reports of comparative outcome research at least imply that improvements in experimental subjects are
due to something particular about that condition vis-a-vis a control group, even though there is always variance in change scores in both groups. But consider this: As Bergin (1966, 1970) alerted us long ago, there is usually some deterioration among some subjects in experimental conditions, even when the group on average improves significantly with respect to pretreatment status and more than control conditions. How frequently do authors attribute this worsening to something special about the experimental condition? Our answer: Never. With these limitations of group designs in mind, it is important to consider the legitimacy and importance of a research model that is based not upon variation between patients, but upon different averages or responses within a patient (Hilliard, 1993).

Individual patients may be studied in two ways. First, they may be used as "their own control." In this connection, individual patients are studied more carefully than is usual when group comparisons are under investigation, but the findings can be added to hypotheses that still center around group norms. Second, in the truly intensive individual clinical design, each subject becomes his or her own laboratory, and hypotheses that arise are tested solely with reference to that particular individual. In the latter instance, the patient’s variability and reaction patterns may be studied minute to minute, hour to hour, day to day, session to session, and so on. Statistical probabilities can be computed, and experimental design in its most rigorous sense can be applied. The patient’s behavior can be described in terms of a multidimensional or multivariate probability distribution, and therapeutic progress can then be assessed in relation to these probability distributions. Symptom frequency and symptom intensity can be woven into the measures obtained and form part of the overall evaluation of treatment effects.

Much greater precision in these studies has followed the use of recordings, films, and videotapes. Since any given clinical observer is subject to a personal within-rater variability, this factor seems less likely to distort and influence results when cases are evaluated by different raters (notwithstanding problems of inter-rater reliability). Advances in telemetry and other electronic recording devices have added further impetus to objectivity and quantitative accuracy.

The general trend in clinical research is in the direction of greater specificity. Broad questions such as "Is psychotherapy effective?" are now considered meaningless and have been replaced by the standard scientific question: What specific treatment is most effective for this individual with that particular problem working with this therapist of that orientation, and under which set of circumstances? (See Paul, 1967; Strupp and Bergin, 1969). Yet, when aiming for specificity, the major drawback of extensive statistical designs is, as just shown, the fact that they yield only group norms and probabilities, and do not tell us very much about a given individual in the group. Only fine-grained study of individual cases permits us to relate therapeutic effect to specific patient characteristics.

When an individual therapeutic effect follows a sequence of treatment methods within an appropriately controlled framework, numerous patient-therapist characteristics in whose context the effect took place can be specified. One can thus narrow down the particular patient and technique variables involved. Strictly speaking, specific inferences are valid only with respect to the individual case itself, but if one relates the particular individual’s most relevant characteristics to similar attributes in other people, general theories can be formulated in terms of these common characteristics. One does not focus upon identical cases (since everyone is unique, there are no completely identical cases), but there are often sufficient similarities and obvious dissimilarities to permit the evaluation of treatment effects on the basis of these related and unrelated features. The basic emphasis is upon the documentation of clinical research, with special reference to objective ratings and the statistical study of the course of a given patient’s treatment, in relatively concrete and operational terms.

CONCLUSION

Because of our therapeutic bias, we have emphasized cognitive-behavioral experiments and clinical trials that are directly related to treatment and that are intended to partially alleviate emotional suffering in a field where so many people seek help and so few ever really find it. Nevertheless, the field of clinical experimentation as research need not be involved with treatment per se or restricted to any particular theoretical orientation. Studies on intermittent reinforcement, for example, were not conducted with treatment applications in mind, but the results are certainly relevant to clinical experimental investigations. Many areas of investigation are carried out with normal subjects. The basic aim of clinical experimentation is to be able to predict changes following specific experimental operations. Our own interests favor those kinds of experiments that endeavor to provide a
framework of scientifically based knowledge for the treatment of specific abnormalities of behavior, or that are intended to explore particular techniques in order to either examine their validity or improve them and specify limiting conditions for their application. We believe that the kinds of continuing interactions detailed in this article between innovations in the applied arena and controlled inquiry in research settings represent promising strategies for enhancing conceptual and procedural knowledge in what might properly one day become the clinical sciences.

NOTES
1. This couple was seen more than 25 years ago, well before the AIDS crisis. Clearly this kind of bloodletting sexual ritual would be regarded as more dangerous today than it was then.

2. We wish to acknowledge that there are problems in transferring the medical concept of placebo to psychotherapy. The medical referent denotes belief and expectancies that patients can bring to a drug treatment situation and that have been shown to lead to therapeutic change even when the medication provided is biochemically inert. There is no analogous situation in the field of psychotherapy, because patients’ ideas and expectations are an integral part of any psychotherapy. Some have argued that nonspecific factors is a preferable term, viz., "... to what extent does a given treatment produce therapeutic changes over and above the changes that would result from the presence of these nonspecific factors alone (Kazdin, 1986, p. 50). But, as Kazdin cautions, this reconceptualization is also fraught with difficulties. Our use of the term placebo control, therefore, may bother some readers, but our argument goes beyond the controversies of placebo conditions in psychotherapy research.

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