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Science, Not Scientism: The Robustness of Naturalistic Clinical Research

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*How can we know the dancer from the
dance?''* —*W. B. Yeats*

Naturalistic clinical research has been denigrated as inferior science by those who espouse scientism, the belief that the experimental group design or its approximation in single-subject research represents the method of choice in the human sciences (Bhaskar 1989). However, in the postpositivist heuristic paradigm of research, which I have described in detail elsewhere (Heineman [Pieper] 1981; [Heineman] Pieper 1985, 1989; following: Bhaskar 1989; Kuhn 1977; Simon 1966; Wimsatt 1980, 1986), there is scientific warrant for harnessing the robust possibilities of naturalistic research to study clinical practice in its full complexity (Adler and Adler 1987; Bronfenbrenner 1979; Ruckdeschel 1985). The heuristic paradigm conceptualizes science broadly as a systematic inquiry into some aspect of reality that is communicated in a way that will allow an interested person to make an informed evaluation of the process of inquiry and its conclusions (Cronbach and Suppes 1969). The heuristic paradigm recognizes that not all scientists will be able to agree on the precise meaning of words such as *systematic, reality, communicated, informed,* and *evaluation* (Manicas and Secord 1983).

Although naturalistic research has unique strengths and has as much scientific merit as interventionist research, it has been overlooked and underutilized owing to the scientism that has colored the thinking of social work researchers and practitioners since the 1950s (Blenkner 1950; Fischer

1981; Geismar and Wood 1982; Greenwood 1952, 1955; Hudson 1982; Kogan 1960; Thyer 1987, 1989), when social work embraced positivist assumptions and exalted (and, therefore, overused and misapplied) the experimental method. The experimental method entails prospective rather than retrospective studies, control groups, operational definitions, randomized subjects, and data gathering by tape or video recorder, by third-party observers, or by structured instruments (Nelsen 1981, 1985). The experimental method (and the modified form in which it is applied to single-subject studies) is most effective when applied to closed systems and nonhuman subjects; it is often incompatible with the resources and the values of casework, that is, with clinical practice as it occurs naturally. Fortunately, naturalistic research offers an alternative, well-founded approach to the study of clinical process.

Naturalistic research entails the systematic study of clinical practice that is not intentionally altered for research purposes. Naturalistic research is distinguished from Naturalism, which is the philosophical notion that the human sciences can best be studied by the methods of the natural sciences (Bhaskar 1989). The contrast between naturalistic and interventionist research in no way implies the naive view that the participant/subject can be studied apart from interactional researcher effects (LeCompte and Goetz 1982). Rather, the categories of naturalistic and interventionist research refer only to the intentions and practices of the researcher. The interventionist researcher intentionally alters clinical practice for research purposes. In contrast, the naturalistic researcher is a practitioner who aims to minimize research intrusiveness into practice. The focus here is on research into clinical practice, which is why the researcher is referred to as a practitioner. In naturalistic research on an organization, a culture, or a subculture, the naturalistic researcher would be a member of the group under study—not an outsider. Research done by outsiders, no matter how skilled in minimizing research intrusiveness, is always interventionist. Naturalistic research on treatment excludes methodologies that for research purposes dictate, for example, that the client take personality inventories or fill out questionnaires before and during the treatment process, that the treatment process be artificially shortened or lengthened, or that the therapeutic relationship be recorded by third-party observers or electronic recording devices. Data gathering in naturalistic research takes the form of anamnestic process recording.

The unwarranted and largely categorical dismissal of naturalistic research by proponents of scientism is fueled in part by a pervasive category mistake whereby issues of qualitative vs. quantitative data and group vs. single-organism designs are conflated with the more fundamental distinction between naturalistic and interventionist research, with the result that this

distinction has been obscured and neglected (Allen-Meares and Lane 1990; Cook and Reichardt 1979; Lincoln and Guba 1985). Qualitative research is frequently equated erroneously with new (postpositivist) research paradigms, while quantitative research is used mistakenly as a synonym for the standard (positivist) research paradigm (Allen-Meares and Lane 1990; Taylor and Bogdan 1984). This conceptual error occurs when issues that pertain to data analysis are confused with issues that relate to data gathering. To illustrate, just as interventionist methods can produce qualitative data (an example is the videotape of a family therapy session), naturalistic designs can generate quantified data (for example, anamnestic process can be coded for the purpose of executing a chi-square test). Moreover, many single-case designs, such as experimental designs and most change process designs, are deemed naturalistic, when they should be categorized as interventionist because they involve manipulations of the treatment process (Berlin, Mann, and Grossman 1991; Bloom and Fisher 1982; Davis and Reid 1988). Research-motivated manipulations of treatment can range from setting uniform limits on client service to using tape recorders or third-party observers (Dean and Reinherz 1986; Nelsen 1985).

I would emphasize that most authors mistakenly define naturalistic research to include interventionist strategies, such as research-driven data gathering by self-report instruments, personality inventories, electronic recording devices, and/or third-party observers (Lincoln and Guba 1985). This mislabeling rests on the unrealistic notion that participants/subjects forget or adjust to research-determined interventions and behave as if they were not there. The fact that subjects do not complain or comment should not be taken to indicate that they are behaving exactly the way they would in the absence of research-determined instruments of observation or inquiry (Bronfenbrenner 1979).

One consequence of the failure to consider the interventionist nature of electronic recording devices, third-party observers, questionnaires, research-motivated adjustments to the therapeutic process, etc., is that the ethical questions raised by research-motivated interventions are rarely if ever considered. By definition, research-motivated interventions introduce nontherapeutic motives and experiences into the treatment relationship and, therefore, always reduce the quality of the service being offered. An example is when limits are placed on client visits for the sole purpose of standardizing the treatment for research purposes. Sometimes authors confuse therapeutic and research aims and argue that research-motivated interventions are helpful to clients, but this argument is irrelevant to the ethical questions raised by the use of interventions that are introduced purely for research purposes (Ivanoff, Blythe, and Briar 1987:418–19). When a given practice theory prescribes ongoing testing or mechanical recording for diag-

nostic or therapeutic purposes (such as allowing clients to see themselves on videotape), then naturalistic research would obviously encompass use of the data produced by these therapeutically motivated instruments and recording devices. However, because some social work treatment modalities have been developed precisely because they were "researchable," and these models consider interventions therapeutic because they are research driven (Reid 1983; Reid and Epstein 1972). For this reason, I would argue that research on these practice modalities is interventionist rather than naturalistic, even though the research imposes no additional data collection measures.

Treatment quality to serve research purposes should be lowered only after a careful consideration of ethical issues and a weighing of competing values, especially because clients whose treatments are adversely affected are usually both desperate and disadvantaged and lack the means to avoid research protocols by choosing among private service providers.

Ethical issues are also raised by so-called unobtrusive measures, such as hidden cameras, which depend on deception and conflict with both humanistic and social work values. In contrast to interventionist research, naturalistic research raises neither ethical nor privacy issues, because the quality of service is not affected and deception is not an issue.

The position that the only incontrovertibly scientific way to study clinical process is by manipulating that process not only raises ethical questions but also is conceptually flawed because of its unwarranted claim of privilege for its positivist ontology (theory of reality) and its positivist epistemology (theory of how to know that reality).

I have written a number of articles critiquing the scientism that colors the literature on social work research (Heineman [Pieper] 1981; [Heineman] Pieper 1985, 1989) and proposing that social work adopt a more contemporary, sound, inclusive, and promising paradigm of scientific research, which I have termed the heuristic paradigm (Simon 1966; Wimsatt 1980, 1986). The heuristic paradigm is a metatheory—a set of principles that regulate the significance attached to any specific ontology and epistemology. This metatheory recognizes both that researchers' judgments are inherently no more objective or trustworthy than practitioners' trained judgments, and, more importantly, it allows social work research to incorporate social work values. It is important to remember that social work values had never been in conflict with social work research until an erroneous definition of science misled our profession into thinking that its fundamental values represented an unscientific, undesirable, and eradicable type of bias and subjectivity (Tyson, in press).

A heuristic is a problem-solving strategy whose goal is reasonable utility rather than (unattainable) certainty. Nobel laureate Herbert Simon observes

that heuristics are inseparable from and indispensable to scientific activity. He emphasizes that science is a problem-solving process and not a neat, uncomplicated process of theory confirmation by means of deducing a predicted event from specified initial conditions (Hanfling 1981). Simon employs the notion of a heuristic to designate the shortcuts that all of us use all the time to solve one of the important problems of living, namely, that we encounter more information than we can possibly assimilate or employ. To illustrate, sensory input, which positivism takes as a direct, uncontaminated recording of external reality, in fact acquires meaning only through strategies that can be termed functional heuristics. These strategies allow us unconsciously to categorize and to edit the wealth of information registered by our senses. The regulatory role that these heuristics play in our perception of reality can perhaps be most clearly recognized when the process fails. For example, in optical illusions our visual strategies, or heuristics, cause us to draw erroneous conclusions about what we are seeing (Segall, Campbell, and Herskovits 1966). If participants/subjects in a dark room observe a light in motion, they will all assume that the light is moving, even when an experimenter has arranged things so that the light actually remains stationary and the subjects and the floor are in motion. Clearly, we would be paralyzed and unable to function if every time we saw motion we stopped to ask if some unseen hand were holding external reality still and moving us. The point is that what we take for direct, unmediated perceptions actually represent interpretations, e.g., conclusions, which are shaped by our experiences and expectations as well as by sensory input.

Every research methodology represents a heuristic by virtue of the fact that it selectively and arbitrarily organizes experience. As a result, every methodology, including the experimental group method and the change process single-case design, comes packaged in its own peculiar bias. In the heuristic approach, bias ceases to be a pejorative word, but, rather, becomes an accepted, inevitable component of all knowing (Wimsatt 1980, 1986). The goal becomes, not the elimination of bias, but the recognition of it (Gadlin and Ingle 1975; Mishler 1979; Orne 1964; Rosenthal 1980; Rosnow and Davis 1977; Wachtel 1980). For example, rather than view the researcher sitting behind a one-way mirror as an unbiased observer, we will want to look at the nature of the biases this intervention introduces, such as the effect on what thoughts and feelings the client willingly shares or unknowingly reveals.

Let me reiterate that I am not replacing one prescriptive paradigm with another by arguing that naturalistic research is inherently superior to interventionist research. The heuristic paradigm emphasizes both that there is no cookbook approach to science and also that there is no intrinsically superior methodology for getting at truth. Rather, any number of equally valid

scientific methods are available, any one of which may be especially appropriate for researching a given problem in a specific setting (Hartman 1990). One of the exciting aspects of the heuristic paradigm of research is that it makes the researcher an active decisionmaker and thinker rather than an unreflective follower of obsolete rules (Saleeby 1979; Siporin 1989).

On the other hand, in adopting the position that no single methodology is inherently superior to any other at producing useful, scientific knowledge, the heuristic paradigm is not endorsing relativism, which is a type of epistemological skepticism. Relativists argue that the untenability of the claim of superiority for any one methodology results from the impossibility of a well-founded conviction that a mind-independent reality exists. Consequently, relativists abandon the pursuit of knowledge that reflects or corresponds to reality and instead aim at explanatory coherence. An example is the increasingly popular use of narrative coherence as a therapeutic tool. Advocates of a focus on narrative structures believe that the client lacks and needs a coherent self-narrative and that the curative element is the coherence of the narrative, not the knowledge of the primary causes of the client's psychological dynamics, which they believe to be unattainable. Both relativism and realism (the view that sound reasons exist to posit a mind-independent reality, which is amenable to study) are heuristic choices and, as such, are encompassed within the heuristic paradigm, although they neither define nor exhaust it.

Although the heuristic paradigm does make room for any ontology whose adherents are engaged in doing science, as I have made clear elsewhere, my own preferred ontology is a qualified realism (Pieper and Pieper 1990). That is, my personal preference is the position that external reality exists and can be known, even though this knowledge will always be partial, imperfect, and colored to some extent by the researcher's heuristics (Bhaskar 1989).

The positivists' type of realism is much less qualified. They believe that if we look through the lens of their preferred type of data gathering, reality can be known with a high degree of certainty. The positivist view is that researchers (or their electronic surrogates) but not practitioners can make unbiased observations of events (facts), which reflect the closest possible experience of a direct, empirical, unmediated contact with reality and, therefore, that these observations can confirm or disconfirm theories (Ivanoff, Robinson, and Blythe 1987). The positivist researcher argues that theories confirmed by these "immaculate perceptions" (Hanson 1969:74) can be considered to be "grounded" in this unproblematic reality. Beginning about 1950, the social work research literature repeatedly advocates empirical, atheoretical, and grounded research and condemns naturalistic research, which is deemed old-fashioned, anecdotal, soft, and unscientific

(Austin 1978; Epstein 1986; Siegel 1985). A representative positivist assessment of the scientific status of social work is that although social work "has long claimed to be one of the scientifically based professions, it has not produced more than a handful of clinical scientists" (Briar 1979:132). This erroneous judgment rests on the belief that when data are gathered by the researcher or the researcher's electronic agent—the tape recorder or video camera—they are empirical and unbiased because the data gatherer is neutral and objective. This view reflects the misuse of the word *empirical* by social work authors (Blythe and Briar 1985; Gleeson 1990; Siegel 1985) and explains why positivist authors have mislabeled my position "antiempiricist" (Glisson and Fischer 1987:51). The terms *empirical* and *empiricism* traditionally refer to any experience that originates extracranially. Positivist authors fallaciously apply these terms only to data collected in a manner compatible with the positivist paradigm. In fact, a comparison of theories, an anamnestic process recording, and a client's self-report of the impact of a program or treatment are just as empirical as the data brought to us by a video camera.

Contributing to the confusion is the conflation of a misleading definition of accuracy with the term *empirical*. Certain methods of data gathering, such as the electronic or third-party recording of a participant/subject's words and gestures, are considered to mirror reality accurately and, therefore, to be free from subjectivity. Data that fit this definition of accuracy are assumed to exhaust the category of empirical. The problem is, of course, that accuracy is a construct that reflects a heuristic choice of data rather than an unalterable, one-to-one, uncontaminated correspondence with reality. Therefore, not only are there many viable types of accuracy, but also the choice and pursuit of one kind of accuracy makes other kinds of accuracy more difficult or impossible to attain. For example, to obtain an accurate recording of the exact sequence and details of a client's speech and behavior, one sacrifices an accurate knowledge of what the client would say or do without the research intervention that introduces an electronic recording device.

There is an ostrich-like quality to the definition of "unobtrusive" measures as data gathering that requires "observers to be inconspicuous in their observing role and to guard against disclosing to subjects the specific nature of the data collected" (Allen-Meares and Lane 1990). Those who argue that clients soon "forget" about electronic devices or observers depreciate the intelligence of their clients and mistake compliance for habituation. Even after he spent an entire school year in a California classroom, Jackson (1990) noted that he remained enough of an outsider that when he happened to sneeze, members of the class turned around, whereas the sneezes of teacher and students went unremarked.

In addition, the focus on a single type of accuracy blinds researchers to the biases introduced by their preferred methodologies. For example, references to taping or the presence of third-party observers are made only in passing and the effects on clients of being subjected to numerous rating scales and to repeated evaluations of their treatments are dismissed or never mentioned (Cooper 1990; Davis and Reid 1988; Dean and Reinherz 1986; Nelsen 1985). When clients do express concern about research intrusiveness into their treatment, positivist researchers do not take these concerns seriously. In one instance, a client's anxiety about having her symptoms tape recorded is dismissed as a psychopathological "dysfunctional assumption" that needs correcting (Berlin, Mann, and Grossman 1991:10).

Similarly, since positivist researchers are unaware that many different but equally useful types of accuracy exist, they dismiss other methodologies, such as anamnestic process recordings, as less accurate and, therefore, less desirable than their preferred methods of data gathering. The authors of the influential *Nonreactive Measures in the Social Sciences* deprecate human beings as "low-fidelity observational instruments" (Webb et al. 1981:241). Kazdin (1981) reflects the positivist disapproval of anamnestic process, which he deprecatingly refers to as anecdotal: "scientific inferences are difficult if not impossible to draw from anecdotal information. Indeed, it is the anecdotal information that is the problem rather than the fact that an individual case is studied" (185). However, since in actuality each method of data gathering has its own strengths and limitations, a tape recorder or third-party observer is inherently no more accurate than the trained clinician. Clearly, tape recorders introduce bias. Imagine interviewing a frightened pregnant teenager who is hiding the unwelcome news from her parents, with and without a tape recorder present in the room. In the presence of the tape recorder, the teenager's remarks may be accurately recorded, but the nature of her remarks undoubtedly will differ from, and represent her state of mind less accurately than, the statements she would make in a naturalistic data-gathering process—i.e., in the absence of the tape recorder and in the presence of a practitioner she knows and trusts. Put differently, third-party observers and tape recorders, no less than practitioners, are participant observers, because each one affects, and therefore causes reactions in, the participant/subject (Bronfenbrenner 1979).

Akin to the positivists' misunderstanding of accuracy is their overvaluation of reliability, which is the precept that a practitioner–researcher's perceptions about a client or treatment process lack scientific value unless they are correlated with another qualified person's perceptions about the same or a similar client or process (McVicker Hunt 1959). I suggest that it is more meaningful to focus on credibility than on reliability. Reliable observations are not necessarily credible; no matter how many people tell us they saw the same flying saucer, we are unlikely to find them credible. Clinical credibility—our conviction of the scientific value, i.e., fundamental correctness, of the practitioner-researcher's interventions and theoretical understanding—can rest on the comprehensive, detailed, well-conceptualized presentation by a single practitioner of her/his conduct and understanding of a specific treatment process.

Researchers who confuse reliability and credibility tend to adopt the absurd position that it is acceptable to trust clients' lives and well-being to trained practitioners but that the judgments of these same practitioners about their clients lack scientific merit. Since it has been amply demonstrated that no truly neutral measures exist—that all methods of gathering data introduce their own biases—we are free once again to accord scientific status to (to rely on) the practitioner's informed judgment. In the early days of social work research, social work knowledge often advanced by means of the case study reported by the experienced practitioner. No one who has read Towle's brilliant, humanistic studies (Towle 1940) can doubt that social work's knowledge base was well served by this method (Tyson 1992). In fact, experienced clinicians' understanding of their treatment process will contribute meanings that will be absent from the observations and conclusions of a researcher who has no prior knowledge of the client and who has a professional identity that is antithetical to the development of therapeutic involvement with the client. If experience and involvement count for something in real-life clinical situations, they should also be worthwhile in research situations.

Another impediment the positivist research paradigm places in the path of the naturalistic study of clinical process is the requirement that valid scientific investigation must always be prospective (this is known as the rule against ex post facto research). Scientism has exalted the experimental method, and the experimental method requires that clinical practice must be altered for research purposes before it is undertaken. This insistence on the inherent superiority of interventionist, prospective research designs has seriously damaged the cause of naturalistic research. For example, an agency with limited resources may institute a program that it deems extraordinarily successful. Positivist standards for science dictate that if an agency wants its innovation to be taken seriously, it cannot simply report on its new approach but rather must institute a prospective study in which clients are randomly chosen and assigned into experimental and control groups. This stricture rests on the erroneous belief that to predict an event correctly confirms the power of one's theory to explain it. However, prediction and explanation are entirely different activities and serve unrelated functions (Kim 1981; Salmon 1971). For example, while we can confidently predict that if the sun disappears below the horizon in the evening it will appear at

the opposite horizon the next morning, when our investigation assures us that the predicted event has occurred, we are no closer to a causal understanding of planetary motion than we were before. In fact, in addition to the human sciences, many natural sciences, such as Darwinian evolution and seismography, are in principle unable to make specific predictions; yet they yield useful explanations of important events. If the mystique of the prospective study is dispelled, an agency or practitioner can publish the description of a treatment approach that seems to have merit without senselessly expending scarce resources to repeat the treatment for the sole purpose of avoiding the positivist censure of naturalistic research. Agencies and practitioners can make scientific evaluations of ongoing programs or clients and need only to expend monies to reinstitute a program or treatment when this will serve a substantive, clinical purpose, such as the extension of promising services to other client groups.

Another reason that social work's embrace of the positivist paradigm resulted in the disparagement of naturalistic vis-à-vis interventionist research is that the positivists sought universally applicable truths. As a result, they regarded situational or contextual factors as, at best, irrelevant and, at worst, as annoying distraction. However, the search for context-free knowledge has been abandoned as quixotic and unproductive by prestigious researchers in other fields (e.g., Campbell [1975] and Mishler [1986] in psychology; Blalock [1979] and Coser [1975] in sociology). The problem focus is always arbitrary because no level of reality is inherently more important or informative than any other. The decision to call one aspect of what we are studying the problem and to define all other aspects as context is always a heuristic strategy (Wimsatt 1980). As such, it should be determined by consideration of what factors are deemed most important to the question being asked, what resources are available to study the problem, and what the most promising means of studying the problem seem to be. In contrast, positivist researchers prescribe a problem focus that is predetermined by their assumptions about reality; that is, they prescribe small, simple, easily measured units, which they mistakenly believe they can divorce from contextual factors. Conversely, the researcher who uses the heuristic approach realizes that no fixed rules exist for separating the problem under study, or system, from the environment, or context (Witkin 1989; Witmer and Gottschalk 1988). For this reason, the problem environment is best defined as that part of the problem one does not study.

The awareness of the arbitrary nature of problem boundaries leads heuristic researchers to reject quixotic attempts to generate universal truths and to focus instead on the nature and effects of their assumptions and the relevance of their problem definitions to real-life practice. Consequently, researchers gain a new awareness that the determination of the system

boundary of what they are studying has sweeping consequences and implications and, concomitantly, that smaller is not necessarily better when it comes to choosing a problem focus. To illustrate, the approach to tuberculosis in which the relevant system was the tuberculosis bacillus made it possible to ignore the poverty and poor health care that allow tuberculosis to discriminate among classes of people (Levins and Lewontin 1985). A similar problem is highlighted in the aphorism, "when the rich get a cold, the poor get pneumonia." In other words, illness can be seen as a social, economic, and class problem, as well as a medical problem, and this redefined focus has far-reaching consequences for our intervention strategies.

Unwanted teenage pregnancy presents an analogous complexity. Whether we see the system, or problem focus, as the teenager, the teenager and her boyfriend, the teenager and her parents, the teenager and her peers, the teenager in the context of her socioeconomic class, the teenager and the opportunities available to her for a satisfying career, or the teenager and the racial and class prejudices of the society in which she lives, will determine not just how we study unwanted teen pregnancy but also the kinds of information our research provides and the interventions we are likely to attempt. In other words, if we study only the teenager and her knowledge of contraception, social inequality will not be part of the equation and cannot appear as a cause of teenage pregnancy that needs to be addressed.

The assertion that value judgments should have no place in scientific activity is a corollary of the positivist argument that optimal science should admit only a narrow range of data, which positivists (mistakenly) believe to be perceptible in unmediated form and, therefore, to appear the same to every observer not brain damaged. From the 1950s until and including the present, in the literature on social work research we see repeated demands that social work research be both atheoretical and value free. We also see constant criticisms of research in which the researcher's values are manifest. Sadly, social work has a rich heritage of meaningful values on which it has turned its back in the pursuit of the unattainable and already outdated belief in the possibility of an objective science based on unmediated facts (Tyson, in press).

Scriven characterizes the positivist researcher as one who has a severe case of "valuephobia," the fear of including value judgments in scientific research (Scriven 1983). In a typical instance from the social work literature, one author insists that "Evaluators must avoid the role of advocate . . ." (Gibbs 1983:90). Actually, positivist researchers evaluate service by goals chosen to conform to their own research values, rather than because they reflect clients' needs or socially useful or prudent solutions. This tacit value system often means that positivist researchers design program

evaluations that uncritically accept the goal definitions set by program managers. Scriven argues that researchers instead should incorporate social values and match the effects of programs to "the needs of those whom they affect" (Scriven 1983:235).

It is apparent that the belief in the possibility of value-free science both shackles science and also makes it a handmaiden of the status quo. Today it is neither radical nor socially disruptive to study the extent of teenagers' knowledge about contraception; it is much more radical to research the degree to which the epidemic of unwanted teenage pregnancies is a function of broad social, gender, and racial inequalities that give some teenagers a sense of hopelessness at an age that is usually characterized by boundless optimism. In addition to being a chimera, the goal of scientific objectivity is itself a value, that is, a preference, and the value of objectivity has been exalted over social work's more traditional values, such as the precept of respecting the client's rights and needs.

In addition to the conceptual difficulties with portraying the researcher as a passive recorder of unproblematic data, there is the practical consequence that this depiction of the researcher is antithetical to the erstwhile role of the researcher as the handmaiden of social change, and it is largely responsible for the split in social work between advocate and researcher. Positivist researchers belittle the advocates for being too involved with their clients and, thereby, for falling prey to an unscientific subjectivity. Once they recognize that the ideal of the value-free, atheoretical, neutral researcher is an impossible fiction and also exalts only one of many competing values, the researchers are free to ally themselves with the advocates and to gather facts unapologetically with the aim of supporting and furthering social work's traditional humanistic concerns.

One traditional social work value that has been subverted by the quixotic attempt to eliminate values from social work research is respect for clients' right to self-determination, that is, the right to participate fully in their own treatment process. Researchers routinely bypass clients' judgments and observe them as though they were as unthinking and unfeeling as a bacterium under a microscope! When treatment is driven by the value of satisfying the requirements of the positivist researcher, the value of making treatment client centered is abrogated. An example is the single-case design (Berlin, Mann, and Grossman 1991; Davis and Reid 1988; Thyer 1987). When positivist research principles took hold of social work, it seemed that the traditional case study, as reported by social workers like Richmond or Towle, would never lend itself to the experimental method and, accordingly, could not be a part of good science. For example, in applying an experimental treatment to one client, there are, by definition, no control groups for comparison with the experimental intervention. The presupposi-

tion that, to be scientific, case studies had to conform to experimental methodology led researchers to design an experimental version of the single-case study in which the client would serve both as experimental group and also as control group (Hersen and Barlow 1976). In one common variant of the single-subject experimental method, the therapeutic intervention being studied (for example, giving a young bed wetter a toy every morning he slept through the night without wetting his bed) might be withdrawn, or reversed, to see whether the symptoms returned. In this example, if the child's bed wetting recurred after the reward was withdrawn, the researcher would conclude that the reward caused the symptom to improve.

Because positivist researchers assume that science can and should be value free, few question the practice of putting the value of attempting to control for extraneous variables over the value of doing no harm to the client (Hersen and Barlow 1976:92–100; Kazdin 1981, 116–219; Nelsen 1985). Ironically, putting research values over treatment values lowers the quality of the treatment and, therefore, of the research (Lucente 1987). When we include the researcher in the system under investigation, it is clear that the researcher's arbitrary refusal to reward the child for successfully remaining dry all night would confuse the child and disrupt his budding self-confidence in his capacity to regulate his own body. If the bed wetting returns, it is as likely to suggest that the child's bed-wetting behavior is vulnerable to the researcher's cruelty as it is to indicate that material rewards cure bed wetting.

Positivist researchers choose and develop the social work treatment modalities they believe to be the most researchable. The treatment methods that fit most easily into the positivist criteria for research are brief, easily standardized, and focus on readily measured overt behavior rather than on the client's subjective experience. On the other hand, treatment modalities that are long term, focus on helping the client to feel better, and take into account the client's unconscious motives as well as the client's manifest behavior are ridiculed as products of a primitive past, which cannot be studied scientifically. The unfortunate consequence is that our profession's preoccupation with achieving (a spurious type of) scientific respectability dovetails with the government's aim to cut spending and reduce services. Even though we now realize the therapeutic limitations of brief, behavior-oriented treatment, it has become nearly impossible to get funding for long-term, open-ended treatments that focus on the client's subjective experience.

I have argued that social work is committing professional suicide by endorsing the outmoded philosophy that service quality should be narrowly defined by adherence to one value—fiscal accountability measured quantita-

tively and behaviorally and determined only on the basis of the study of atomistic, simplified interactions ([Heineman] Pieper 1985, 1989). The corollary is that our clients increasingly receive only atomistic, simplified, rigid, short-term interventions. For years we have undermined our standing to propose creative, open-ended, flexible service. Increasing numbers of outpatient clinics that receive public funds are eliminating relationship-oriented, psychodynamic treatment approaches and providing their clients only drugs and behavioral or cognitive therapies. The rationale is not that these modalities have been shown to have superior therapeutic value or that clients do not want a more involved, open-ended therapeutic relationship or help with nonspecific but troubling psychic pain, but rather that the mandated treatments fit better with a value system that defines accountability solely in terms of the value of spending the smallest amount of time and money with any given client. Sadly, social work researchers who espouse scientism intentionally or unintentionally support this limitation of services. And once again the poorest clients, who are dependent on public funding, get the cheapest and most meager services.

In summary, if social work adopts the heuristic paradigm, researchers will cease the single-minded pursuit of the chimerical goal of neutral, value-free science and will be able to integrate the more attainable values of the recognition and regulation of bias with social work's traditional values (such as respect for the client's self-experience, sensitivity to gender and racial discrimination, and concern with social injustice) into their scientific activities (Goldstein 1983).

Further, both the effort of critiquing the positivist claim for the superiority of interventionist research and the concomitant argument for the scientific standing of naturalistic social work research are matters of great concern, because for so many years unwarranted positivist strictures have limited the range of data that are considered legitimate, which in turn restricts social work's ability to study clinical practice in all its complexity. To illustrate, one researcher makes the frightening assertion that "If you cannot measure the client's problem, it does not exist" (Hudson 1982).

Naturalistic research is just as scientifically respectable and able to produce legitimate, helpful, relevant, generalizable knowledge as interventionist research is. If social work were to embrace the heuristic paradigm, one significant consequence would be that research-motivated interventions in casework services, such as the introduction of third-party observers, electronic recording devices, and client instruments, would no longer be misperceived as nonreactive and unobtrusive. These service manipulations would cease to be implemented unthinkingly but would have to be justified both in relation to their potential to contribute significantly to the research being undertaken and in terms of the negative effects they may have on a particular service modality and the degree of their compatibility with the broad spectrum of social work values.

Some of the advantages of naturalistic clinical research are that the values of putting the client's interest first and of doing no harm are respected, that treatment is studied in an undisturbed form from an experience-near perspective, and that the practitioner's trained understanding and assessment of the treatment process are highlighted. The resurrection of naturalistic research will encourage practitioners to leave the sidelines and to participate comfortably in relevant, significant, helpful, humanistic, science (Sherman 1987). Practitioners who for the last forty years have unjustly been made to feel that their experienced and educated judgments are unscientific and, therefore, unimportant can join the effort to devise creative and productive ways to study and shed light on the complex, multifactorial, overdetermined problems that plague us all.

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