Comments on the State of Psychotherapy Research (As I See It)

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Note: This essay was written in response to an invitation by Chris Muran, North American SPR regional chapter president, to contribute my views on the current state of psychotherapy research for the past-president's column of the NASPR Newsletter. It appeared, sans references, in the January 2006 issue. Comments on the essay are welcome at <d-orlinsky@uchicago.edu>.

I must start by confessing that I don't really *read* psychotherapy research when I can help it. Why? The language is dull, the story lines are repetitive, the characters lack depth, and the authors generally have no sense of humor. It is not amusing, or at least not intentionally so. What I do instead of reading is *scan* or *study*. I do routinely *scan* the abstracts of articles as issues of journals arrive to assure myself there is nothing I need or want to know in it, and if the abstract holds my interest then I scan tables of results. Also, at intervals of years, I have agreed to *study* the research on psychotherapy systematically, usually with a specific focus on studies that related process and outcome (Howard & Orlinsky, 1972; Orlinsky & Howard, 1978, 1986; Orlinsky, Grawe & Parks, 1994; Orlinsky, Rønnestad & Willutzki, 2004). I have been doing this for 40 years more or less, and on that basis (for what it is worth) here is what I think about the state of psychotherapy research.

I think in recent years that psychotherapy research has taken on many of the trappings of what Thomas Kuhn (1970) described as "normal science"—meaning that research by and large has become devoted to incrementally and systematically working out the details of a general "paradigm" that is widely accepted and largely unquestioned. The research paradigm or standard

model involves the study of (a) manualized therapeutic procedures (b) for specific types of disorder (c) in particular treatment settings and conditions. This is very different from the field that I described three decades ago (Orlinsky & Howard, 1978) as "pre-paradigmatic," and in some ways it represents a considerable advance. However, I refer above to the "trappings of normal science" as a *double entendre* to suggest that the *appearance* (trappings) of normal science with its implicit paradigmatic consensus may also represent *entrapment* (trapping) in a constricted and unrealistic model.

The paradigm is familiar. It holds that psychotherapy is basically a set of specific and specifiable *procedures* ("interventions" or "techniques") that can be taught, learned, and applied; and that the comparative potency or efficacy of these procedures in treating specific and specifiable psychological and behavioral disorders defines more or less effective forms of psychotherapy—if patients are willing and able to comply with the treatment provided by a competently trained therapist.

In this process, therapists are assumed to be active subjects (agents, providers) and patients are assumed to be reactive objects (targets, recipients). Researchers may well believe *theoretically* that patients as well as therapists are active subjects, and that what transpires between them in therapy should be viewed as interaction, but *in practice* the paradigm or standard research model that they typically follow implicitly defines treatment as a unidirectional process.

Evidence of these implicit conceptions of the patient, therapist, and treatment process is to be found in experimental designs that randomly assign patients to alternative treatment conditions, just as if they were 'objects' (rarely bothering to inquire about *their* preferences) whereas they never assign therapists to alternative treatment conditions, randomly or systematically (because it seems essential to consider *their* subjective treatment preferences). The

consequence is that comparisons between treatment conditions reflect treatment-x-therapist interaction effects rather than treatment main effects—as Elkin (1999) and others have made clear—but it is an embarrassment that is conveniently ignored by all (as in the tale of the emperor's new clothes).

In addition, the dominant research paradigm constricts our view of the phenomena that psychotherapy researchers presume they are studying by focusing on certain *abstracted* qualities or characteristics of patients and therapists. The target of treatment is not actually the patient as an individual but rather a specifically diagnosed *disorder*. Other personal characteristics of patients are presumed to be "controlled" either through random assignment (another embarrassing myth, since the effectiveness of random assignment depends on the law of large numbers, and the number of subjects in a sample or of replicated samples is rarely large enough to sustain this), or controlled statistically by using the few characteristics of patients that are routinely assessed in studies as covariates. The covariates most typically are *atheoretically* selected demographic variables assessed for the purpose of describing the sample—age, gender, marital status, race/ethnicity, and the like—since there are no widely accepted theories to guide the selection of patient variables. (More recently, "alliance" measures have been routinely collected from patients, reflecting the massive accumulation of *empirical* findings on the impact of therapeutic relationship.)

Psychotherapists are likewise viewed in terms of certain *abstracted* qualities or characteristics. The agent of treatment studied is not actually the therapist as an individual but rather a specific set of *manualized treatment skills* in which the therapist is expected to have been trained to *competence* and to which the therapist is expected to show *adherence* in practice. The few other therapist characteristics that are routinely assessed—professional background, career level, theoretical orientation, and perhaps gender and race/ethnicity—are used largely to describe

the sample or, occasionally, as covariates. Again, this is because there are no widely accepted theories, or extensively replicated empirical findings, to guide the selection of therapist variables.

The constricted and highly abstracted view of patients, therapists, and the therapeutic process in the dominant research paradigm is supported by cognitive biases in modern culture that all of us share. One of these was well-described by the sociologist Peter Berger and his colleagues as *componentiality*. This is a basic assumption that "the components of reality are self-contained units which can be brought into relation with other such units—that is, reality is *not* conceived as an ongoing flux of juncture and disjuncture of unique entities. This apprehension in terms of components is essential to the reproducibility of the [industrial] production process as well as to the correlation of men and machines. … Reality is ordered in terms of such units, which are apprehended and manipulated as atomistic units. Thus, everything is analyzable into constituent components, and everything can be taken apart and put together again in terms of these components" (Berger, Berger & Kellner, 1974, p. 27).

This componentiality is reflected in the highly individual and decontextualized way that we think about persons. We tend to think of individuals as essentially separate, independent and basically interchangeable units of 'personality' that in turn are constituted by other internal, more or less mechanistically interacting components—whether those are conceptualized as traits that may be assessed quantitatively as individual difference variables, or more holistically but less precisely as clinical components of personality (e.g., *ego*, *id*, and *superego*). Thus when researchers seek to assess the (hopefully positive but sometimes negative) impact of psychotherapy on patients, they routinely focus their observations on componential individuals abstracted from life-contexts, and on the constituent components of individuals toward which therapeutic treatments are targeted—symptomatic disorders and pathological character traits. They do not generally assess individuals as essentially embedded in sociocultural, economic-

political and developmental life-contexts. A componential view of psychotherapy and of the individuals who engage in it is implicit in the dominant research paradigm, and produces a comforting sense of cognitive control for researchers—but does it do justice to the *realities* we seek to study or does it distort them?

Another widely shared bias of modern culture that complicates and distorts the work of researchers on psychotherapy and psychopharmacology (and medicine more broadly) is the implicit assumption of an essential distinction or dichotomy between *soma* and *psyche* (or matter and mind), notwithstanding the efforts of modern philosophers like Ryle (1949) to undo this Cartesian myth. Because of this, findings that psychological phenomena have neurological or other bodily correlates (e.g., using MRI or CT scans to detect changes in emotional response) are viewed as somehow amazing and worthy of note even in the daily press. The materialist bias of modern culture also fosters a tendency to view this correlation in reductionist terms, so that the physiological aspects of the phenomena studied are assumed to be more basic, and to *cause* the psychological aspect.

Thanks to a conversation at the recent SPR conference in Montreal among colleagues from different cultural traditions (Bae et al., 2005), I became aware of how unnatural the body-mind dichotomy (with its consequent distinction between 'physical health' and 'mental health') appears from other cultural perspectives, and of how grossly it distorts the evident *psychosomatic continuity* of the living human person. When this basic continuity is conceptually split into 'psyche' and 'soma', a mysterious quality is created as the byproduct (much as energy is released when atoms are split)—a mysterious quality that is labeled (and as much as possible viewed dismissively) as "the placebo effect." This effect, mysteriously labeled in Latin, is viewed as a "contaminant" in research designs—but, struggle as researchers do to "control" it (rather than

understand it), they typically fail in the attempt because the 'effect' reflects an aspect of our reality as human beings that *cannot* be eliminated.

The reality, as I see it, is that a person (a) is a *psychosomatic unity*, (b) evolving over time along a specific *life-course trajectory*, and (c) is a *subjective self* that is objectively connected with other subjective selves, (d) each of them being *active/responsive nodes in an intersubjective web* of community relationships and cultural patterns, a web in which those same patterns and relationships (e) exert a formative influence on the psychosomatic development of persons.

The reality of psychotherapy, as I see it, is that it involves (a) an intentionally-formed, culturally-defined social relationship through which a potentially healing intersubjective connection is established (b) between persons who interact with one another in the roles of client and therapist (c) for a delimited time during which their life-course trajectories intersect, (d) with the therapist acting on behalf of the community that certified her (e) to engage with the patient in ways that aim to influence the patient's life-course in directions that should be beneficial for the patient.

Neither of these realities seems to me to be adequately addressed by the dominant paradigm or standard research model followed in most studies of psychotherapeutic process and outcome. Instead, the dominant research paradigm seriously distorts the real nature of persons and of psychotherapy (as I see them). Why then does this paradigm dominate the field of psychotherapy research, and why do researchers persist in using it if it is as uncomfortably ill-fitting a Procrustean bed as I have claimed?

The answer is *partly cultural*, as the paradigm neatly reflects the componential, psycho/somatically split, materialist cognitive biases of Western culture. It is also *partly psychological*, with supporters of the paradigm becoming more militant as a result of cognitive dissonance generated by the incipient failure of the paradigm's utopian scientific promise (see,

e.g., Festinger, Riecken & Schachter, 1956). It is *partly historical* too, as the field of psychotherapy originated and initially evolved largely as a medical subspecialty in the field of psychiatry—as well as the field of *clinical* psychology that overlapped with, imitated, and set out to rival psychiatry. Again, the answer is *partly economic*, since it is necessary to please research funding agencies (the *real* 'placebo' effect) in order to gain funding for research and advance one's career by contributing publications to one's field and reimbursement for "indirect costs" to the institution where one is employed.

It may be ironic that the paradigm adheres so closely to the medical model of illness and treatment at a time when the psychiatric profession which historically represented medicine's presence in the field has largely (and regrettably) withdrawn from the practice of psychotherapy (Luhrmann, 2000). The apparent solidity of the paradigm that survives is based (a) on the fact that psychotherapeutic services still are largely funded through health insurance which had been politically expanded (after much lobbying) to include non-medical practitioners, and (b) on the fact that psychotherapy research still is largely funded through grants from biomedical research agencies. Although there is no for-profit industry promoting psychotherapy and supporting research on it as Big Pharma does with the psychopharmacologic treatments of biological psychiatry, most of the money that can be had in psychotherapeutic practice and psychotherapy research comes from sources that implicitly support a medical model of mental health. As ever "they who pay the piper call the tune," though perhaps it is more subtle and accurate to say that pipers who need and seek financial support (therapists *and* researchers) play their tunes in ways that they hope will be pleasing to potential sponsors. Necessity drives us (always), but we (all) have an uncanny ability to persuade ourselves that advantage and merit coincide.

A sociology-of-knowledge confession: I know full well that I can say these things mainly because I am privileged by having an old-fashioned, tenured, hard-(but small)-money position in

an arts and sciences faculty, and because I am not really in the competition for funds. As a producer of psychotherapy research, I am free to go my own way through my work as participant in the SPR Collaborative Research Network; but as a consumer of psychotherapy research, I have serious misgivings about the state of the filed stem from a perception that the prevailing paradigm which permits research to pursue their studies in the manner of "normal science" represents a risky *premature closure* in understanding the actual nature of psychotherapy and the people who engage in it. If it is not overtly corrupting (as may be true of some research on psychopharmacological treatments funded by pharmaceutical firms), it is nevertheless constricting in ways that seem to me highly problematic.

If we are indeed to have evidence-based psychotherapies grounded in systematic, well-replicated research (e.g., Goodheart, Kazdin & Sternberg, 2006), and evidence-based training for psychotherapists (e.g., Orlinsky & Rønnestad, 2005), then it would be very nice—in fact, I would think essential—for that research to be based on a standard model or paradigm which more adequately matches the actual experience and lived reality of what it presumes to study. I don't know what that new paradigm or model for research will turn out to be. Constructing it is the task of the next generation—but from it will come the sort of psychotherapy research I think I would *like* to read.

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