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DOUBLETHINKING OUR WAY TO “SCIENTIFIC” LEGITIMACY: THE DESICCATION OF HUMAN EXPERIENCE

A multifaceted contemporary movement aims to correct alleged weaknesses in the scientific foundation of psychoanalysis. For both pragmatic-political and scientific reasons we are encouraged to do and/or study systematic empirical research on psychoanalytic process and outcome, as well as apparently relevant neuroscience. The thesis advanced here is that the privileged status this movement accords such research as against in-depth case studies is unwarranted epistemologically and is potentially damaging both to the development of our understanding of the analytic process itself and to the quality of our clinical work. In a nonobjectivist hermeneutic paradigm best suited to psychoanalysis, the analyst embraces the existential uncertainty that accompanies the realization that there are multiple good ways to be, in the moment and more generally in life, and that the choices he or she makes are always influenced by culture, by sociopolitical mind-set, by personal values, by countertransference, and by other factors in ways that are never fully known. Nevertheless, a critical, nonconformist psychoanalysis always strives to expose and challenge such foundations for the participants' choices. The “consequential uniqueness” of each interaction and the indeterminacy associated with the free will of the participants make the individual case study especially suited for the advancement of “knowledge”—that is, the progressive enrichment of sensibility—in our field.

Today I want to discuss a rather extensive and multifaceted movement in our field to correct alleged weaknesses in our scientific foundation.

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Propelled by moral convictions, as well as by economic and political pressures, increasing interest has emerged in systematic empirical research on psychoanalytic process and outcome and in apparently relevant neuroscience. At the same time, we have been reminded emphatically of the familiar scientific inadequacies of traditional case studies and associated clinical theoretical writing. My thesis here is that the privileged status that this movement accords systematic research and neuroscience as compared with in-depth case studies and strictly psychological accounts of the psychoanalytic process is unwarranted epistemologically and potentially damaging both to the development of our understanding of the analytic process itself and to the quality of our clinical work. I am not arguing that systematic research and neuroscience are of *no* value to the practice of psychoanalysis, but only that granting them superordinate status relative to other sources of knowledge, including case studies, is unjustified and potentially destructive.

I am hardly alone in advocating the elevation of case studies within scientific discourse. As often happens, one finds, in the course of developing one's ideas on a subject, that they belong to a larger current of thought that is well under way. Daniel Fishman (2006) writes that "in recent years, there has been a revival of interest in the case study's potential to create viable scientific, psychological knowledge that is not inferior to experimental, group-based knowledge, but rather complementary to such knowledge—especially in the area of psychotherapy research" (p. 1). Over fifteen references follow.

I want to place this issue immediately into a certain philosophical context as it bears on psychoanalysis. My argument here has continuity with the critical perspective that I, along with others—including Donnel Stern (1997), who has introduced this talk and is a leading contributor to this movement, have been advancing over the past quarter-century regarding the objectivism that has prevailed in mainstream psychoanalytic practice. The critical or "dialectical" constructivism that I have been encouraging replaces a diagnostic, knowing, prescriptive psychoanalytic attitude with one that requires responsible, creative, improvised, and collaborative efforts on the parts of the participants *to make something* of the ambiguous, context-dependent reality that evolves in the course of their interaction. In this paradigm, the analyst embraces the existential uncertainty that accompanies the realization

that there are multiple good ways to be, in the moment, and more generally in life, and that the choices he or she makes are always influenced by culture, by personal values, by countertransference, and by other factors in ways that can never be fully known. So my critique of the premises for the privileging of systematic quantitative research and of neuroscience is not accurately lined up with the divide between the psychoanalytic practitioner and the psychoanalytic researcher; it is rather lined up with the broader divide between constructivism and objectivism in psychoanalysis, a divide that can be located *within* the community of non-research-oriented psychoanalytic clinicians (Hoffman 1998).

I understand that some who have been advocates of a certain kind of privileging of systematic research, including Merton Gill (1994), with whom I worked closely for many years (Gill and Hoffman 1982a,b; Hoffman and Gill 1988a,b), have opposed authoritarian psychoanalytic attitudes and have had the same interest I've had in challenging them. However, they have felt that a good way to expose and undermine those attitudes and to offer alternative perspectives is through the carrying out and empowering of systematic quantitative research (Hoffman 1996), whereas my conviction is that that avenue is flawed epistemologically and that it threatens to embody yet a new form of prescriptive, authoritarian objectivism.

I have great regard for the contributions to psychoanalysis of many of those with whom I will be taking issue in what follows. My critique is limited to positions they have taken and specific statements they have made regarding what psychoanalysis requires in order to buttress its standing as a scientific discipline.

THE CONTEXTS OF DISCOVERY, JUSTIFICATION, AND CONSTRUCTION: CASE STUDIES VS. SYSTEMATIC EMPIRICAL RESEARCH

So I want to consider the scientific status of individual case reports as compared with systematic quantitative research. I regret that I do not have the time to offer more than glancing reference to the relationship between case studies and neuroscience.¹ The general consensus is that case reports are akin to "pilot studies" or "hypothesis-generating studies."

They belong to the “context of discovery” in science, not the “context of justification” (Westen 2002, p. 883). The contention is that as anecdotal experiences they cannot yield knowledge that is generalizable until the hypotheses that emerge from them are tested in systematic research. Presumably such hypothesis-testing research, with certain identified variables controlled, can yield “findings” that practitioners can take more seriously and apply more directly as they approach their work with patients. As a result of such allegedly scientific study, a practicing analyst might come to know better, for example, what kinds of patients under what circumstances require what kinds of interventions.

With regard to this account, I agree that there’s a sense in which case studies could be viewed as hypothesis-generating. I would rather say, however, that they generate important plausible *possibilities* for practicing analysts to have in mind in their work. The term *hypothesis* encourages the expectation of “testing” that will make greater generalization possible, and I do not consider that expectation to be warranted (see Specht 1988). On the contrary, systematic, allegedly hypothesis-testing research is not likely to do anything more than generate possibilities for practitioners to have in mind as they work with particular patients. In other words, such research usually accomplishes nothing more in that regard than do case studies and therefore deserve no higher status as scientific contributions. To the extent they are accorded such higher status and authority, which too readily becomes *prescriptive* authority, they pose serious dangers to the quality of any psychoanalytic practice, any psychoanalytic attitude, that they affect.

The alleged hierarchical arrangement of hypothesis-generating clinical experience and hypothesis-testing systematic research is the regnant view of their relationship. Westen, Novotny, and Thompson-Brenner (2004), to their credit, have recently challenged this paradigm and suggested more of an equal and reciprocal relationship between these two kinds of empirical work. In an article reprinted in the *Psychodynamic*

¹Recent advances in neuroscience are compatible with the notion of mind as an open, emergent system, with the relational perspective in psychoanalysis, with the notion of unknown, unrealized potentials that the psychoanalytic process has the power to promote, and with other principles underlying my thesis in this paper (see, e.g., Doidge 2007; Solms and Turnbull 2002). What is objectionable, however, is any privileging of neuroscience over psychoanalytic-psychological inquiry for the discovery and validation of propositions that have meaning in the realm of human experience. Such privileging inevitably entails the fallacy of reductionism (see, e.g., Kandel 2005).

Diagnostic Manual (2006), they propose specific methods for bringing them into a cooperative, mutually beneficial relationship. Nevertheless, it remains the case that many researchers, and even many analysts who are steeped in clinical work and clinical-theoretical writing, often implicitly accept the hierarchical arrangement as it bears on the generation of scientifically grounded knowledge. I believe, however, that questions that are so taboo that they might not enter many people's minds include not only "Do the findings of *any* systematic research on analytic process and outcome accomplish anything more than generate hypotheses, or possibilities, for a particular practitioner to consider in a particular moment with a particular patient?" (in other words, "Do they accomplish anything more than a case study accomplishes in that regard?") but also "Is it *desirable*, clinically, for a practitioner to have a mind-set in which he or she even *aspires* to 'know' what 'standard intervention' to apply in working with a particular patient at a particular moment?" Is that a feature of an optimal analytic attitude?

Many feel it's clear that we need systematic research in order to arbitrate among multiple psychoanalytic points of view and in order to decide which theoretical perspectives are best and which treatments are best for whatever forms of suffering our patients bring to us. The alternative, as Westen (2002), Schachter (2005), Wolitzky (2006), Eagle (2003), Eagle and Wolitzky (1989), Wallerstein (2003, 2006), Kernberg (2006), and others see it, is a radical postmodern relativism in which virtually any theory is as valid as any other. In lieu of objective evidence, some combination of logical argument and the power of the opinion of one or another psychoanalytic authority allegedly determines what practitioners feel they should do with their patients. Wallerstein (2006) recently formulated the hermeneutic alternative to what he regards as the natural-science paradigm for generating and testing hypotheses in our field as a totally uncritical pluralism. In my view, these authors do not address the objections to the natural-science paradigm that are in keeping with the best of the hermeneutic tradition, nor do they consider the alternative perspective on the generation of knowledge that that tradition promotes. Hermeneutic critiques of the philosophy underlying "evidence-based" approaches to treatment warrant close attention. Cushman and Gilford (2000), commenting on the values implicit in evidence-based approaches, note that they entail "abhorrence of ambiguity, complexity, uncertainty, perplexity, mystery, imperfection, and individual variation in treatment" (p. 993).

Westen (2002) has compared the perspective that would naturally govern our approach to the treatment of a life-threatening disease, such as leukemia in a child, with the perspective that governs our approach to emotional problems. He wonders how many of us contending with illness in a child would want the physician “who we believe lacks the expertise of a specialist who understands the pathophysiology of the disorder and knows the latest techniques for treating it.” He continues by asking sarcastically, “how many of us would be content to have our child treated by a physician who says that he resonates with particular theories of leukemia and that, although he has not read much of the empirical literature on it since medical school, he believes cells can be understood from a multiplicity of angles, none of which constitutes the whole truth?” He then asserts that “the situation is, in fact, *no* different for our [analytic] patients, who come to us seeking help for cancers of the soul. It is easy to be post-modern with someone else’s misery” (p. 888; emphasis added).

Westen’s position on the comparability of treatment of leukemia and treatment of “cancers of the soul,” an unfortunate medical metaphor if ever there was one, betrays a serious misunderstanding of the underlying epistemological issues. First, the reality of the ambiguity of human experience requires a creative dimension in the process of “making something” of that experience. In some measure, therefore, the context of “construction” of meaning displaces the contexts of discovery and justification. At the same time, the ambiguity of experience does not mean that “anything goes” with respect to how it can be formulated and understood. By way of analogy, I like to consider the Rorschach test (see Hoffman 1998, pp. 20–26), where the number of plausible percepts and plausible constructions is in principle infinite. However, the number of responses that are insufficiently accommodating of the properties of the ambiguous stimulus—that number—is also infinite! And, indeed, the Rorschach, with all its recognized ambiguity, is commonly used to assess a person’s reality testing. By the same token, to say that many theories have some validity does not mean that critical thinking and empirical considerations don’t play a part in excluding some theoretical propositions altogether for failing to fit the data. Beyond that, moral, pragmatic, and aesthetic considerations are involved in negotiating the relative merits of those propositions that are more “in the ballpark” in terms of plausibility. Catherine Elgin (1989), in her contribution to a collection of essays on relativism writes, “to say that personal predilections are involved in deciding among equally worthy alternatives is quite different from saying that personal

predilections are what make the alternatives worthy. Subjective considerations function as *tiebreakers* after the merit of the contenders has been certified by other means” (pp. 97–98; emphasis added).

In *Re-envisioning Psychology*, Richardson, Fower, and Guignon (1999) outline a series of criteria for evaluating the quality of interpretations without resort to foundational assumptions about truth (pp. 298–302). The grounds for choosing one or another interpretation or theoretical perspective are likely to be heavily influenced by considerations other than validity, such as moral considerations in the broad sense, as Cushman (1995, 2005) among others (e.g., Flax 1996; Richards 2006) have argued. Questions such as “What is a good way to be in this moment?” “Which human motives are most important?” “What constitutes the good life?” are implicitly involved. Such questions cannot and should not be adjudicated entirely by “science.” To the extent that we give the authority of science the power to arbitrate these choices we are falling into the worst kind of scientism, in which moral positions masquerade as scientific “findings.”

In the case of leukemia, at least within our culture, there is not going to be a great range of perspectives on the aims of treatment. Here is where it is important to distinguish between two kinds of uniqueness. Although no two cases of leukemia will ever be exactly alike, the uniqueness of each case is likely to be relatively inconsequential as it bears on the “treatment of choice,” as compared with each instance of emotional and psychological difficulty, where what I would call *consequential uniqueness* is the rule. Moreover, the whole person of the treatment provider, of the analyst, is at the heart of what is relevant in engaging with a person struggling with problems in living, whereas in the treatment of a physical disorder it is specific technical expertise that is likely to matter most. In other words, it is likely that the active ingredients in the treatment of many medical conditions can be safely decontextualized to the extent that their relevant “meaning” does not vary from one practitioner to another.² A certain antibiotic should work with a certain kind of infection no matter who prescribes it. But the meaning of the analyst’s interpersonal behavior—whether listening or interpreting or exploring or self-disclosing or relating in other ways—is context-dependent, and one of the relevant contexts is the whole person engaging in that behavior. That is why the

²I am not speaking here of the relevance of the whole relationship of physician and patient as it bears on overall quality of care (Groopman 2007; Newman 2006; Singer 2007). I am speaking only about the “treatment of choice” for a specific, targeted medical condition.

fact that systematic quantitative studies cannot control for “who the therapist is” detracts substantially from their scientific and pragmatic value. Conversely, the fact that case studies *do* allow for consideration of the person of the therapist as he or she engages in the process goes a long way toward contributing to their special scientific power, notwithstanding whatever limitations they may have.

Of course, the same argument that applies to the analyst applies to the patient and to each moment of the encounter. Systematic empirical studies simply do not control for the consequential uniqueness of the analyst, of the patient, of their relationship, and of the moment. Shedler and Westen (2006) put enormous, conscientious effort over many years into developing the Shedler-Westen Assessment Procedure, a Q-sort method by which patients can be comprehensively described and classified, allegedly without sacrificing the complexity and multiplicity of their personality traits. They report that in a sample of 797 experienced therapists of all persuasions, 72.7 percent agreed with the statement “I was able to express most of the things I consider important about this patient,” and that in another sample, of 1,201 psychologists and psychiatrists, 80 percent “agreed” or “strongly agreed” with the statement “The SWAP-200 allowed me to express the things I consider important about my patient’s personality” (pp. 580–581). That’s quite impressive. But let me ask you, if you were looking for a therapist, whom would you prefer: a therapist who feels that the 200 sorted items of the SWAP captures most of what is true of you as a person, or a therapist in that 20 percent minority who feels that important things about you are missed by this instrument? Think about it. I myself would put my money on the latter. That’s likely to be the therapist who is profoundly respectful of, and intrigued by, what is unknown and unprecedented about his patients and who assumes that he or she might well be challenged by each patient to call upon something in him- or herself that is new and unprecedented in his or her experience.

Alan Kazdin (1998) in his highly regarded *Research Design in Clinical Psychology* enumerates the advantages of case studies, as well as their limitations. Among the advantages is that “they permit the *study of rare phenomena*”; in fact, Kazdin asserts, “Many problems seen in treatment or that are of interest are so infrequent as to make evaluation in group research impossible” (p. 204). Note that Kazdin says not “difficult,” but “impossible.” Well, I would submit that from a psychoanalytic point of view any individual case qualifies not only as a *rare* phenomenon but

as a *unique* one insofar as we think in terms of engaging with a whole person. Then, inescapably, we have this logical implication: according to Kazdin's argument, whether he realizes it or not, "evaluation in group research" of treatment issues pertaining to any individual, regarded as a whole person, is "impossible."

I am reminded of this statement by Hans Strupp (2001) toward the end of a distinguished career devoted to psychotherapy research: "Given the uniqueness of every therapeutic dyad and the multitude of relevant interacting variables influencing the course of treatment, the 'empirical validation' of any therapy is utterly illusory" (p. 613). I know that many then ask, If the principle of consequential uniqueness is so decisive, how can one learn anything from systematic empirical research or, for that matter, from case studies, and how can any "progress" be made in accruing knowledge over time? The answer must be something between, at one extreme, simply applying, in a mechanical way, an approach that seems to have had therapeutic value for another allegedly "matched" patient-analyst dyad or sample of such dyads (or even, as in single-case designs, the same dyad at a different moment in time) and, at the other extreme, flying by the seat of one's pants with no guidelines whatsoever. What is required is as much knowledge and sophistication as one can muster regarding the cogent possibilities to consider in terms of understanding as well as ways of relating to our patients, and, at the same time, the assumption of responsibility for creative participation honoring the uniqueness of each person and each moment. Maybe a way to formulate this challenge is to say that we aspire to a kind of grounded and responsible, yet free and creative, relational engagement. The grounding and the creativity constitute an ongoing dialectic. What can develop over time is a kind of responsive yet also critical and creative *sensibility*.

The fact that many different ways of working might have a certain validity and power to effect change does not preclude critical dialogue and debate generated by reports of clinical work. Such dialogue and debate can foster a transformation of theory, and even the emergence of new paradigms. I think the value of constructive critical dialogue (as represented in the thought of Gadamer, Habermas, Taylor, and others) is vastly underrated by the advocates of systematic research. Rather than simply adding coursework centered on such research to the curricula of psychoanalytic institutes, as Kernberg (2006) and others have urged us to do, thereby uncritically accepting the underlying premises of that work,

I think we need to add interrogation of those premises along with exploration of other, socially constructed routes to knowledge.

The fact is that compelling critiques of traditional approaches to psychoanalytic work *have* emerged and taken hold, as have changes in the way many analysts practice. These changes, which are generally in the direction of some degree of democratization of the analytic relationship and of recognition of the intersubjective nature of psychoanalytic data, owe little if anything to systematic empirical research. They owe more to case presentations and to clinical experience and theorizing, as well as to changes in attitudes and values in our culture.

One factor to remember is that we do have “moles” inside those very confidential clinical situations. After all, many analytic patients are themselves people in the field, and many have been analysands with more than one analyst. So some of the “consumers” are mixed in with the practitioners and are involved in the mounting of critiques of old ways of working and in the development of new ones. It’s easy to forget that developments in clinical theory come as much from consumers of our services as from those involved in their delivery. Right here in this room we have as great a concentration of psychoanalytic *patients* as can be found anywhere.

Those who would privilege systematic research over case studies often point to the subjective bias of the reporting analyst as a serious limitation that must be overcome through the fullest possible exposure of the process to independent judges. Although biased reporting warrants serious consideration, an important counterpoint is that the ambiguity of psychoanalytic data leave them relatively unmanipulatable in the sense of stacking the cards in favor of one or another point of view. The analyst can’t force something ambiguous simply to support the view that he or she advocates. The ambiguity in itself ensures the openness of the “data” to critical review and to multiple interpretations. Such data lend themselves to constructive dialogue among the reporting analyst and others. It’s noteworthy that with all the concern about how the reporting analyst, in the interest of supporting his or her point of view, can skew both the course of the analytic work and the way in which it is described to others, in point of fact the data that are customarily presented do not seem to prevent people from mounting critiques of the work, from suggesting alternative formulations of what went on in the process, and from offering suggestions as to better ways the analyst might have intervened and participated.

Moreover, the analyst’s bias and its influence on the patient in the course of the work itself—another alleged limitation of case studies—is

less of a problem when you consider the aim of the analytic process to include the patient and the analyst collaboratively working out some sense of what the "good life" would be for the patient, rather than reducing its purpose to the mere amelioration of suffering. Patients may "take" to analysts with worldviews, ways of thinking and being, compatible with their own. The analyst and the patient are working together to shape or construct aspects of the patient's life and selfhood, not just to discover their existing features, and the context of *construction*, as distinct from both *discovery* and *justification*, requires a critical, reflective engagement with the analyst's values and commitments, not their elimination.

**ANALYSTS AND PATIENTS AS FREE AGENTS:
DETERMINISM AND TECHNICAL RATIONALITY
AS "ESCAPES FROM FREEDOM"**

Regarding the issue of freedom, I think it's difficult to embrace a commitment to be creative as opposed to a commitment to be competent at certain skills. It's not easy to take the former on as a responsibility, and yet I think it's essential that we do so in order to be responsive to our patients' unique emergent potentials as they come into being in the encounter with our own. And there is some mystery there in the idea of "creating" beyond what is "given" or "known" that I don't think we can deny and that is probably a source of anxiety for many of us, partly because it is beyond what we can fully comprehend. It comes down to the unanswerable question regarding human freedom. How is it possible? Beyond that, of course, there's that other daunting question, What should we do with it? The "escapes from freedom"—to adapt Erich Fromm's phrase (1941)—that human beings have sought throughout history have been myriad. Psychic determinism is one of them. Rank (1945) put it this way:

The causality principle means a denial of the will principle since it makes the thinking, feeling, and acting of the individual dependent on forces outside of himself and thus frees him of responsibility and guilt. . . . Only in the individual act of will do we have the unique phenomenon of spontaneity, the establishing of a new primary cause. . . . So one sees why a natural science psychology denies will and consciousness and in their place must introduce the unconscious Id [today we might add preverbal affective patterning and its underlying neurophysiology] as a causal factor which morally does not differ at all from the idea of God, just as sexuality as a scapegoat is not different from the idea of the devil. In other words,

scientific psychoanalysis gives the individual only a new kind of excuse for his willing and a new release from the responsibility of consciousness [pp. 44–45].

The complement of psychic determinism in terms of “treatment” is technical rationality (see Schön 1983), that is, the analyst claiming to know, on the basis of previous professional experience or empirical “evidence,” what will effectively promote “healthy” change. One of the most important areas of interdisciplinary and intermodal integration that we need—in keeping with the crescendo of calls in our literature for “consilience” (Wilson 1998)—is that between psychoanalysis and existential philosophy, and between psychoanalysis and humanistic-existential psychotherapy. I think there’s a reason that we rarely think about that (see Frie 2002) and have chosen instead to attend primarily to the integration of cognitive-behavioral approaches, affect theory, and neuroscience. I believe that those subject areas are appealing because aspects of them, certain versions of them, seem to have the potential to teach us more about what *has to be*: how the patient *has to be organized* given this or that history or trauma, and what we as analysts *have to do* if we are to affect this or that difficulty or injury. In other words, they are appealing, almost irresistible, precisely because they offer the illusion of necessary courses of action and process while minimizing the daunting burden of responsibility that accompanies our own free agency as we encounter new and ambiguous data and the patient him- or herself as a free agent.

Perhaps not surprisingly, in fact, one can find in the humanistic-existential tradition incisive critiques of the assumptions underlying most psychotherapy research that complement critiques grounded in the hermeneutic tradition. Bohart, O’Hara, and Leitner (1998) argue that the kind of therapy they advocate is automatically disqualified in such research precisely because of the central importance in their approach of certain values that can be characterized as respect for the consequential uniqueness of every dyad and every moment, as well as for the free agency of the participants: “The therapist is a disciplined improvisational artist, not a manual-driven technician. ‘Therapy’ consists of engaging in an ongoing co-constructive dialogue with the client. As the dialogue progresses, specific techniques may emerge as relevant at given points, but these cannot be specified or anticipated in advance” (p. 145). I want to underscore this point regarding openness to specific techniques, which may or may not qualify as “psychoanalytic” in and of themselves. There may be instances in which it can be demonstrated that a certain “symptom” is so commonly responsive to a certain intervention that it would make sense to

give very serious consideration to trying that approach. In fact, ironically, the critical constructivist position to which I subscribe encourages an openness to collaborative consideration by analyst and analysand of more ways of working and understanding—in some instances well-researched—than are likely to be entertained within a more traditional, objectivistic psychoanalytic stance (see Hoffman 2006). There are myriad examples. Studies of the neurophysiology of trauma, to name just one, can be very useful clinically (van der Kolk 1987; Davies and Frawley 1994; Anderson and Gold 2003). Wachtel (1997) and Frank (1999) are among the theorists who have written extensively about the integration of psychoanalysis and other approaches. Nevertheless, some caveats are in order. First, even the relatively direct “application” of a likely “correct” understanding or technique would be only a part of a whole psychoanalytic orientation that would be unique in terms of the full nature of the analyst’s participation. Second, there is always a danger that value-laden purposes will be concealed within diagnostic and prescriptive requirements alleged to be scientifically grounded. The consequence may well be that what is *different* about a particular person’s *version* of an allegedly standard “condition” will be missed, along with the opportunity for the special kind of recognition and responsiveness that that version of the problem calls for.

I recently had an experience with a patient, Neil, a gentleman in his seventies, who struggled with a pattern of behavior that seemed to fit well with the diagnosis of “intermittent explosive disorder.” I referred him to a psychopharmacologist for medication, which he reluctantly agreed might help, given his feeling that his sporadic attacks of rage were over-reactions completely beyond his control. A couple of days later he called to tell me he had just had what promised to be his last attack. He was outraged when, over the phone, the psychopharmacologist told him his fee, exorbitant by the patient’s standards, and he blew up in his characteristic way, an explosion exacerbated by what Neil felt was this doctor’s condescending attitude. He now pledged to manage his temper on his own, without benefit of “any fucking medication.” I said, “But, Neil, I thought it was out of your control,” to which he replied: “I changed my mind.” What a powerful phrase when you think about it: “I changed my mind.” Our language often has more wisdom in it than we notice. Since that time the episodes have been dramatically reduced, to about 20 percent of their previous frequency according to the patient’s report (upon reading this, Neil insisted that I tell you that 2 percent is more

accurate). Had the fee been lower and had Neil liked the psychiatrist better, he probably would have ended up with medication that surely would have been credited with any ensuing improvement. As it turned out, the “treatment of choice” was referral to a psychopharmacologist with an exorbitant fee and a condescending attitude. Should we do a study to see how frequently that works?

ECONOMIC AND POLITICAL PRESSURES AND THE ART OF “DOUBLETHINK”

I realize that all that I am saying here may seem ironic in light of the fact that systematic empirical research has long been the underdog, the voice *not* heard, in the psychoanalytic community. The balance of power can change rather rapidly, however. While it may be true that psychoanalysis considered in isolation is not yet on the verge of being taken over by research-minded “marauders,” as Westen (2002, p. 889) calls them, mimicking the alleged attitude of allegedly threatened clinicians, the danger is real indeed, when viewed in the larger context of the dominant scientism in psychology, psychiatry, and the mental health field generally, that advocates of evidence-based psychotherapy will come to set our “standards” of practice, a takeover far out of proportion to what is justified epistemologically.

In point of fact, by now quite a bit of painstaking systematic research has been done that supports the efficacy of psychoanalysis in various conventionally acceptable ways, work summarized by Wallerstein (2003), Kernberg (2006), and others. As some have pointed out—notably Gary Walls (1999), who has written extensively on the subject—it’s a politically significant reality that these results are often ignored in favor of perpetuating the overstatement, even the myth, that psychoanalysis in recent years has acquired no “empirical” foundation at all in the conventional sense.³

With respect to the current status of, and need for, psychoanalytic process and outcome research bearing on this issue, it’s important to distinguish between what we do and say strategically to appeal to the powers that be and what we genuinely believe. Even with respect to strategy,

³As this paper was being prepared for publication, a metaanalysis by Leichsenring and Rabung (2008) of outcome studies of long-term psychodynamic psychotherapy showed significantly greater benefits for certain groups of seriously disturbed patients from long-term treatment as compared with shorter-term cognitive-behavioral approaches.

the question might be raised whether, in the long run, any compromising of ourselves for practical ends will actually succeed, as compared with “playing our cards face up,” to borrow a phrase invoked by Owen Renik (1999) in another context. The danger, of course, is great that what we invest in for strategic reasons—with funding, with time, with energy, with the encouragement of certain career lines and the discouragement of others—will heavily influence both whom we attract to the field and what we actually come to believe is true, so that we might systematically create a nightmarish, Orwellian social reality in which dissent and critical thinking on this matter becomes so much the object of contempt and derision that it all but disappears from our discourse. And then, gradually, insidiously, in that most pernicious and frightening of all the forms of “successful” cultural and subcultural thought control, it begins to fade even from our most private thoughts.

Orwell (1949) wrote in *1984*:

The Party intellectual knows in which direction his memories must be altered; he therefore knows that he is playing tricks with reality; but by the exercise of *doublethink* he also satisfies himself that reality is not violated. The process has to be conscious, or it would not be carried out with sufficient precision, but it also has to be unconscious, or it would bring with it a feeling of falsity and hence of guilt. *Doublethink* lies at the very heart of Ingsoc, since the essential act of the Party is to use conscious deception while retaining the firmness of purpose that goes with complete honesty. To tell deliberate lies while genuinely believing in them, to forget any fact that has become inconvenient, and then, when it becomes necessary again, to draw it back from oblivion for just so long as it is needed, to deny the existence of objective reality and all the while to take account of the reality which one denies—all this is indispensably necessary. Even in using the word *doublethink* it is necessary to exercise *doublethink*. For by using the word one admits that one is tampering with reality; by a fresh act of *doublethink* one erases this knowledge; and so on indefinitely, with the lie always one leap ahead of the truth [pp. 176–177].

Now consider the following statement by Peter Fonagy (2002) on the “realities” confronting psychoanalysis and what, pragmatically speaking, we should do about them:

It needs to be recognized that objections to research will not win the day. It is unlikely that the prevailing view that places controlled studies at the top of the hierarchy of evidence will change, *no matter what the strength of opposing arguments*. The complexity of the issues surrounding resource allocation, the drive to seek certainty and simplicity at the level of policy making, are such that alternative formulations will not be heard. . . .

The best strategy available to us is to collect all the data available rather than enter an epistemological debate amongst ourselves. The debate is inaudible to those outside the discipline. Further it would sap our energies when these are required for a collaborative effort to make the best case possible for psychoanalysis as a clinical method. Even those of us who are engaged in collecting evidence for the effectiveness of this discipline have major methodological as well as epistemological concerns. These should not be set aside or forgotten about, but nor should they become an alternative focus [p. 58; emphasis added].

I see this as the transitionally “terminal” moment, the moment in which there is, first, total capitulation to the political power of a particular perspective, *regardless of its merit*; second, the recommendation that we “go along,” that we play the game by the prevailing rules, the “science game,” as Strupp (2001, p. 615) called it; and, finally, the suggestion that we silence ourselves in relation to the dominant authorities, regardless of the extent and nature of our differences with them. In true doublethink fashion, we are told that our methodological and epistemological concerns “should not be set aside or forgotten about,” but also that we should not allow them “to become an alternative focus,” and that we should not let them “sap our energies” by talking about them even among ourselves! What are the chances, over a period of years in which we devote our resources to persuading those authorities that psychoanalysis is indeed a “scientifically” valid enterprise, that our *objections* to the assumptions underlying that entire, enormously consuming effort will *not* be forgotten, will actually survive, and will have a significant place—which would have to mean further development—in our own minds, not to mention in our dialogue with others? I think the chances of that are virtually nil.

A whole genre of literature has emerged in recent years in which, via an artful version of *doublethink*, the privileging of controlled studies is justified alongside the articulation of rather devastating critiques of their special authority. The offering of the caveat, the reservation regarding what research can accomplish, is essentially disarming. The very act of admitting the limitations of controlled studies empowers the systematic research advocate and disempowers potential opponents. Indeed, it’s a part of the opponents’ death knell. Relegating the admissions, even the devastating critiques, to the status of “caveats” is an extremely effective method for acquiring and maintaining dominance over other conceptions of psychoanalysis as science and other views as to what psychoanalysis requires to establish its legitimacy.

Compelling examples of doublethink can be found at the heart of the highly touted *Psychodynamic Diagnostic Manual* (PDM Task Force 2006), a work endorsed by several analytic organizations. We find, in the chapter "Personality Patterns and Disorders: The P-Axis," the following statement:

There are many different ways to distinguish psychologically between one person and another. All of them, of course, oversimplifications. Any therapist who gets to know a particular patient intimately finds that over time, that person no longer seems to fit neatly into a category; the person's individuality eventually becomes more impressive than his or her conformity with an abstraction. Nevertheless, especially for purposes of treatment planning and for the therapist's sense of how to proceed in the early phases of therapy, it is clinically useful to consider which personality type or types most closely correspond to the psychology of one's patient [p. 29].

That is quite a striking statement. Because of the uniqueness of each patient, we're down to considering the whole PDM classification of personality types as fundamentally bogus, though possibly useful for the practitioner "in the early phases of therapy." It seems to me it could easily be argued that the therapist would do well, even at the beginning, or especially at the beginning, to overcome his or her anxiety about not knowing just how to categorize this person and therefore not knowing just what would be the optimal thing to do from moment to moment, rather than retreating into an illusion of "knowledge" about the person and about the "treatment of choice." After all, presumably the *patient* is there with symptoms entailing some form of security-enhancing closing-down of possibilities, and what are we encouraged to greet him or her with but a massive institutionalized symptom of our own: our own systematic, security-enhancing closing-down of possibilities, a reality-distorting—so the PDM tells us—categorizing of the patient and associated prescriptions for treatment that absolutely do not do his or her individuality justice (although, to be sure, the taxonomy may help us provide a good-enough lie to a third-party payer).

Now I want to turn to a statement in the PDM that appears in a footnote:

We emphasize again that these "types" [of personality] are prototypes that no individual patient may match precisely. . . . Other than in section headings, we do not refer to diagnostic entities in capital letters (e.g., patients with "Borderline Personality Disorder"), and we avoid acronyms (e.g. "patients with BPD"). People whose personalities are problematic do not have something comparable

to a disease, nor does a personality completely define who they are. By avoiding capitalization and initials, we are also trying to resist a tendency that has crept into the mental health field, under the influence of pharmaceutical and insurance companies, to reify complex syndromes, implying that they exist as discernable “things” rather than as interrelated patterns of cognition, emotion and behavior that are frequently seen in clinical practice [p. 31n].

Well, I couldn't have said it better myself. In this statement, in the “fine print,” as Leslie Brothers (2001, 2002) calls it in her critique of neuroscience, the authors acknowledge that style carries a message and has an effect. In order to overcome the association with the medical model and to help differentiate the approach from that of the DSM, they are going to avoid capitalization and initials to identify types of personalities. But with respect to impressions and appearances, do the authors fail to notice the extent to which the entire *Psychodynamic Diagnostic Manual* (the acronym PDM appears in parentheses on the front cover) creates exactly the impression they claim they want to avoid? The Manual is a “manual” after all, and a “diagnostic” manual to boot. How different is the impression created by the PDM as opposed to the DSM? Perhaps it is noteworthy that the authors risked changing only one of the three letters! So, no one will be tagged with capitalized initials as if they had some reified medical condition. I guess that's good. On the other hand, that gesture, that nod to humanistic, existential respect for the uniqueness and limitless complexity of any person, does not dissuade the authors from making sure that every “diagnosed” patient gets assigned a number! The first adult case example is summed up like this: with respect to personality type, PDM Code: “P107.1, with features of P106.2 and P112.1”; with respect to mental functioning, PDM Code: “M205”; with respect to symptom patterns, PDM Code: “S304.1 (tentative).” What, may I ask, does “tentative” mean? We've already been told, by the authors themselves, that, on a priori grounds, over time, what we are going to find is that neither these categories and numbers nor any others in the PDM will be very good fits for this patient or any other person we get to know well! “Tentative” must therefore mean “ultimately inapplicable.” So whom are we kidding?

A patient of mine who is elderly, ninety-five years old as I speak, is horrified at the prospect of dying. He's also horrified at all the losses he has suffered: parents, brother, wife, one daughter, countless friends, several therapists, all gone. So what is that? Is the anxiety or the horror a symptom or just part of the human condition? The anxiety has a history. It goes way back to childhood, when the patient was physically and

psychologically abused by his mother. Death is coming at him like his mother came at him. So maybe that part of the anxiety can be considered a neurotic symptom. If we can get the prospect of death differentiated from the prospect of being abused by his mother we will have transformed neurotic suffering into normal human misery, to paraphrase Freud's concise articulation of the goal of psychoanalysis. But how will I and the patient know when we have accomplished that? Does anyone know what the healthy quality and quantity of the anxiety should be? How much *should* it interfere with everyday living, relationships, work, recreation? What will I say when the doctor from Utilization Review calls and asks me point-blank, "Well, Dr. Hoffman, have you got it down to pure existential death anxiety or is there still some obsolete neurotic childhood anxiety about being abused mixed in there? There's still some of the latter, huh? How many more sessions do you think it will take to get rid of that? You don't know? Well let me look this up here. Is it a Generalized Anxiety Disorder or a Panic Disorder? Something in-between you say? We don't pay for 'in-between,' so you better pick one. You say Panic Disorder. Okay, that should be taken care of, according to my manual here, in about thirteen sessions, that is, if you're doing it right. By the way, what *are* you doing with this patient, Dr. Hoffman?"

In addition to the difficulty, if not the impossibility, of drawing a clear line between normal and neurotic anxiety, there is another complication. The patient, it turns out, has a certain attachment to his anxiety. He expresses this in a variety of ways. One of them is that he thinks that people try to comfort themselves in facing death through various kinds of defenses, such as religious beliefs. He takes pride in the idea that he tries to face death head-on without fooling himself. So you might say he has a certain narcissistic investment in his own courage. Would anyone dare to make a judgment about what proportion of that narcissistic investment is healthy and what proportion is pathological? Regardless of what might be decided about that, the fact is that this patient has a conflict. When people are hanging on to their so-called symptoms for various reasons, conscious and unconscious, it sure makes it hard to predict how long it will take for them to "get better." That depends to a significant degree on when or whether they *decide* to let go of certain problematic ways of thinking, feeling, and behaving in favor of ways of being that invariably entail other problems.

Another patient, a suburban housewife and mother of five, after describing her attacks of anxiety as the most terrible things she has ever

experienced, told me in about the third or fourth session that there was something about them she had to admit she “liked in a weird sort of way.” In the midst of the attack, she would have a sense of unreality in which she would ask herself “Who am I? Who are these five kids? What am I doing in this house?” It created a sense of detachment that was sort of what she wanted. That is, she wanted to escape from a world in which she felt totally trapped, and this was her way of doing it. So you’ll say, undoubtedly there’s got to be a *better* way. Fine. Let’s say that’s true. But does anyone know what that better way is for *her*? Is there some reasonable time that it should take for the two of us to find it? Because until she finds it she might very well hang on for dear life, for the life of her soul, to those terrifying episodes of detachment.

Our work as analysts often entails joining our patients in their struggles with such conflict, including close attention to associated moral, political, and existential issues. When we as analysands and as analysts (and remember, we are both) resist (as the relational movement encourages us to do) the reification of emotional “disorders” as abnormalities located in us as individuals, we open the door to critical appraisal of the sociopolitical contexts in which we are embedded. We may be complicit in sustaining those contexts, however “disordered,” however destructive, they may be. As feminist theorists have argued with respect to hysteria (Bernheimer and Kahane 1985; Bordo 1993), maybe our own alleged “symptoms” can be construed as in part conflicted unconscious protest against “inconvenient truths,” not only about our childhood histories, and not only about the inescapable existential realities of the human condition, but also about the particular sociopolitical arrangements that constitute our collective, perhaps seriously impaired, “holding environments.” Our attachment to those environments, in which, among other things, extreme consumerism and individualism are prominent (see Cushman 1995), may have much in common with stubborn attachments to “bad objects.” The PDM and the kind of research it encourages offer seductive rewards for the perpetuation of what might qualify as subtle sadomasochistic complicity with certain organizations of power in our society. Separating ourselves from that world and trying to create and attach to something new and better may entail extremely hard and conflictual mutual analytic work—work with the potential, however, for raised social consciousness and, ultimately, constructive political action (see Altman 2000; Cushman 1995, 2000, 2005; Layton, Hollander, and Gutwill 2006; Gutwill and Hollander 2006; Lewis 2006; Samuels 1993; Walls 2006).

The urgent call from many leading figures in our field for programmatic changes in our discipline, including changes in our training programs, in the direction of greater respect for systematic process and outcome research, is not complemented by, among other things, outrage that clinical theoretical writing and theorizing has little or no place in academia, that it has no legitimacy that might warrant public or private institutional funding. This is our taken-for-granted reality, despite the common recognition that such work has been the major source of development of knowledge in our field for more than a century. I believe that the current zeal about the grounding of psychoanalysis as science is at best highly imbalanced in this regard and at worst profoundly mistaken philosophically.

CRITICAL VS. CONFORMIST PSYCHOANALYSIS

In addition to the critiques born out of both the humanistic-existential and the hermeneutic traditions, some veteran students of psychotherapy research have virtually defected, deciding that the enterprise and the paradigm it entails are untenable, in part, because they in fact promote denial of the sociopolitical context of the phenomena being studied.

David Orlinsky, who, writing with Ken Howard and others, has been among the foremost students, collators, and integrators of psychotherapy research for over forty years, has come to the conclusion that in a variety of ways, as he puts it, "the emperor has no clothes." His critique encompasses many points. I will quote him on just one of them, the abstracting of individuals and their traits from the social-historical contexts in which they exist, what he calls, following Berger, "componentiality" (Berger, Berger, and Kellner 1974; see also Flax 1996; Layton, Hollander, and Gutwill 2006; Richards 2006):

[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? [Orlinsky 2006, p. 3].

Elaine Schwager died tragically in May 2006 in a fire in her home in New York City. Sadly, I came to know Elaine only on the occasion of her death, when members of the listserve of the New York University Postdoctoral Program in Psychotherapy and Psychoanalysis informed people about her and directed them to her website, where one can find an extensive interview with her, as well as a volume of her poetry. Elaine, the daughter of Holocaust survivors, was a wonderful published poet, as well as a psychoanalytic therapist and writer. In that interview she gave us this as part of her legacy:

I'm really not interested in "craziness," that is, in labeling behavior as "crazy" or in labeling it or diagnosing at all. I am interested in the uniqueness of people and how they came to be who they are. By imposing theories to explain people to themselves, labels or diagnoses, we deprive people of their dignity, of the very complex struggle each person has with his pain, evil, eccentricities, abnormalities, and the creative ways they themselves express that struggle to tell another who they are. . . .

Obviously, genocide is the final grind of the oppressor's boot-heel, where those in authority isolate one group of people as worthless, crazy, abnormal, whatever, and exterminate them. Psychological theory can exterminate aspects of people's psyche by not giving recognition or value to aspects of the self which are designated crazy, bad, or less central than other aspects [Schwager 2001].

"Less central" is so important to include, because it can mean "marginalized." And we know that "marginalized" people, or parts of people, are one short step from being stripped of rights, which is one short step from virtual or literal annihilation.

I believe we are at a critical juncture in the history of our discipline. Perhaps it is an ongoing struggle in human history generally, and for all of us as individuals, to sustain our tolerance of, even appetite for, complexity, ambiguity, and uncertain moral commitment in the face of a competing constant pull that we *all* feel to gain control, to master, to know, to feel safe and secure. But if we submit to instrumental, black-and-white thinking, which after all seems to surround us now, in our culture and our time, in an especially pervasive and dangerous way, we lose so much of what human experience can be; we participate, in effect, in the desiccation, the destruction of experience rather than in cultivating and celebrating its full potential. Although the tension and conflict has expression within our field, at its best a *critical* rather than *conformist* psychoanalysis should emerge as a bastion in our culture that will stand for human

freedom, for the dignity of the individual, for the meaningfulness of community, and for the sacrosanct integrity of every moment of experience.

And the responsibility begins with ourselves, with each one of us. So with respect to doublethink, for example, I am challenged to find the good in it, the ally to my own convictions. Since the data are unquestionably ambiguous, instead of interpreting contradictory statements merely as conscious or unconscious manipulation, I can choose to see them as expressive of profound, authentic conflict. In the fine print, in the footnotes, in the unintegrated paragraphs of those who seem to champion as the royal road to "knowledge" the privileging of hypothesis-testing, quantitative research, and the denigration of case studies, in those conceptual "parapraxes" we find the subversive, disenfranchised, dissociated yet still passionate, truly psychoanalytic voice. That voice stands up for the full richness, complexity, and mystery of each moment of human experience and for its manifold unrealized potentials. Then I and others can find allies in the hearts of our seeming "adversaries" in this debate, and together nurture hope that psychoanalysis can be newly empowered as a humanizing force in our culture and in the world.

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