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Psychoanalysis, Science, and Hermeneutics: The Vicious Circles of Adversarial Discourse

The discussion I am presenting here originated as an invited address to the Italian chapter of the Society for the Exploration of Psychotherapy Integration (SEPI). SEPI is an organization devoted to examining potential convergences between seemingly opposed and disparate theoretical and clinical approaches, and much of its discourse entails examining the assumptions, the procedures, and the evidence for these various approaches to see whether there are potential integrative possibilities that have been obscured by differing terminological universes and ideological predispositions. In the first national meeting of SEPI-Italy, however, a different line of demarcation between the attendees seemed to emerge. The introduction to the program of the conference referred to “a deep division between two different modes of understanding psychotherapy in general and psychotherapy integration in particular: the scientific that favors objectivity and the humanistic that privileges the subject.” It was this distinction that I was asked to speak to in my keynote address.

In what follows I will speak to this “deep division,” but I will attempt to show how the issues have been excessively and counterproductively dichotomized in the literature of psychoanalysis and in other arenas of psychological discourse as well. In doing so, I will attempt as well to show how this false dichotomy has in large measure been a product of a vicious circle, in which excessive statements from one end of the artificial divide justify (and perpetuate) excessive statements from the other end.

In thus emphasizing the dynamic role of vicious circles, I am extending a point of view that has been at the heart of my theoretical efforts for a long time. Indeed, my own approach to the question of psychotherapy integration itself, to whether psychoanalysis can be effectively reconciled with other theoretical points of view and combined with them in a synergistic manner, has been centrally rooted in an understanding of vicious circles.

Cyclical Processes in Personality Development

Understanding the basic structure both of neurosis, and of personality development more generally, is greatly enhanced by an appreciation of the role of cyclical feedback processes, what I have called “cyclical psychodynamics” (e.g., Wachtel, 1977, 1982, 1987, 1993, 1994, 1997; Wachtel & Wachtel, 1986). The dynamics of neurosis are, by and large, the dynamics of vicious circles. The dynamics of personality development in general are dynamics of both vicious circles and virtuous circles. In both instances, cyclical feedback processes are involved, often processes in which the responses of “accomplices” in people’s life patterns (Wachtel, 1991) are of crucial importance.

An example from ordinary development, discussed in an earlier publication, will illustrate:

[T]he two-year-old who has developed an engaging and playful manner is far more likely to evoke friendly interest and attention on the part of adults than is the child who is rather quiet and withdrawn. The latter will typically encounter a less rich interpersonal environment, which will further decrease the likelihood that he will drastically change. Similarly, the former is likely to continually learn that other people are fun and are eager to interact with him; and his pattern, too, is likely to become more firmly fixed as he grows. Further, not only will the two children tend to evoke different behavior from others, they will also interpret differently the same reaction from another person. Thus, the playful child may experience a silent or grumpy response from another as a kind of game and may continue to interact until perhaps he does elicit an appreciative response. The quieter child, not used to much interaction, will readily accept the initial response as a signal to back off.

If we look at the two children as adults, we may perhaps find the difference between them still evident: one outgoing, cheerful, and expecting the best of people; the other rather shy, and unsure that anyone is interested. A childhood pattern has persisted into adulthood. Yet we really don't understand the developmental process unless we see how, successively, teachers, playmates, girlfriends, and colleagues have been drawn in as "accomplices" in maintaining the persistent pattern. And, I would suggest, we don't understand the possibilities for change unless we realize that even now there are such "accomplices," and that if they stopped playing their role in the process, it would be likely eventually to alter. (Wachtel, 1997).

Much of my own theoretical work has been concerned with understanding why, in so many instances, the accomplices *do not* stop playing the role; how the dynamic force field of each person's personality pulls for particular reactions from others that tend to be just the reactions that keep that very force field ongoing. This process has certain similarities to those depicted by the concepts of the repetition compulsion (Freud, 1920) and of projective identification (e.g., Klein, 1946; Ogden, 1979), but in its emphasis on the critical importance of real life experiences, the cyclical psychodynamic perspective also differs in certain respects from those two concepts. Cyclical psychodynamics as a theoretical approach highlights the dynamic relevance of daily transactions between people both in the contexts of family and intimate relationships and in the countless other contacts we have with people throughout the day. These transactions both reflect *and* reinforce or maintain the internal dynamics that are the more usual focus of psychoanalytic inquiry and theorizing. Cyclical psychodynamics offers a bridge as well to the larger world of social, economic, and historical forces, illuminating both the role of these forces in the psychodynamics of individuals and the relevance of psychoanalytic thought for understanding those societal-level processes (e.g., Wachtel, 1983, 1999).

In the present paper, I wish to discuss still another way in which vicious circles operate in our lives. Here I aim to explicate the role of vicious circles in the epistemological and methodological debates that have become increasingly central to our field's discourse – debates involving the role of positivism, natural science, or empirical validation of the effectiveness of our methods on the one hand, and postmodernism, hermeneutics, and constructivism on the other.

A Caveat

Much fruitless and misleading debate results from a failure to appreciate that science is not one single thing, but a multitude of methods and conceptual strategies that share certain common features I will discuss shortly. What leads some proponents of humanistic or hermeneutic approaches to reject a "natural science" approach, echoing Dilthey's (1883/1991) nineteenth century distinction between *Naturwissenschaften* and *Geisteswissenschaften*, is a focus on only a very narrow range of the practices that deserve to be referred to as science. Science is a term that refers to a host of practices that are designed to enable us (a) to accumulate

and build upon systematic knowledge; and (b) to do so in a way that is also designed to aid us, at least partially, in overcoming our prodigious capacity for self-deception. Any and all of the practices that human beings engage in to further their knowledge that meet these two criteria seem to me to fully merit the term science.

It is important for the reader to keep clearly in mind as I proceed that science is *not* equal to experiments (though controlled experiments are one of its powerful tools). Nor is it equivalent to positivism, to “objectivism,” to “linear” thinking. These are straw man labels that are sometimes used by analysts to defensively reject the *specter* of science, whose real threat is that it might pour cold water over some of their favorite ideas or, even worse, that it might make it possible to check on whether they are actually helping the people who come to see them.

Vicious Circles and the Epistemological Debate

Divisions between contending positions tend to be heightened when extreme positions in one direction elicit extreme positions in the other, and then each extreme position is experienced as “justified” by the excesses of the other side. What I am describing is, of course, a vicious circle – a phenomenon that, as I noted above, is at the center of my efforts to understand the nature of personality dynamics and development as well.

One of the first things one must realize about vicious circles is that it is very difficult to determine when or how they begin. As family therapists have pointed out, the “punctuation” of a repetitive sequence is often one of the key points of difference and contention between participants in that sequence (e.g., Hoffman, 1981). Each side says the other side started it – “I am just reacting to what *they* are doing.” Of course, the other side says precisely the same thing. And each side keeps reacting to the other in a way that *keeps* the other side doing still more of the same – thus justifying their own continuation of *their* provocative behavior still again. This kind of pattern, repeated over and over, can be seen in couples, in families, and in the personality dynamics of individuals.

The same dynamic operates outside the clinical realm as well. I have previously examined, for example, how similar circular patterns can be seen in the way that societies rush madly after economic growth even while the pursuit of that growth creates social disruption and severe ecological damage, and even fails to provide very much of the satisfaction that people think will follow from having more. To understand why we are constantly frustrated, constantly feeling we need “more,” and constantly failing to be satisfied with that more when we get it (and then think that somehow *still* more will do the trick), we need to understand the dynamics of the vicious circle (Wachtel, 1983)..

I have seen a similarly circular pattern as well in an extensive study of race relations in the United States that I completed a few years ago (Wachtel, 1999). In that realm we may also see particularly clearly the all too common mutual *blame* part of the circle. “*We* are only doing what we do because *they* are doing what they do.” And then both sides act in a way that ensures that the other side will *continue* doing the very things they complain about – and the very things that elicit their own participation in the destructive pattern still again. In this realm at least, the perpetual motion machine seems to have become a reality. Sadly and frighteningly, we now see the same pattern in the current struggles between the Western world and radical Islam. Vicious circles abound here too, with each side seeing their behavior as just a reaction to the excesses of the other – and each side *reacting* to those excesses in a way that elicits them once again.

Attention to very much the same kinds of vicious circles helps to understand how our field has gotten so divided over methodological and epistemological questions, especially the issue of science and the humanities or science and hermeneutics. As with all such circular patterns, the moral of the narrative depends on where one begins the description. Through the eyes of the *critics* of psychoanalysis, the narrative begins with years of psychoanalytic practice that offered very little in the nature of systematic evidence for

either the assumptions underlying psychoanalytic work or the effectiveness of the treatments offered. In this version of the story – the story written by the advocates of “empirically validated treatments” and “evidence-based practice” – these defenders of science and the testing of claims in the crucible of empirical evidence were reacting to a degree of self-indulgent irresponsibility in the psychoanalytic community and reacting as well to a stultifying conservatism in which new ideas were supported, ultimately, not by *evidence* for their validity but by whether some respected *authority* endorsed them. (And it must be acknowledged that in psychoanalytic journals, the backing for ideas more often entails citing Klein or Winnicott or Kernberg or whomever than citing systematic observations.

From the vantage point of this narrative, then, the proponents of “science” are the brave insurgents, protecting the public by systematically evaluating the claims of the psychoanalysts and by introducing newer, briefer methods that actually have evidence that they work.

But there is another narrative that, we might say, is a version of this one through the looking glass. This opposite version of the story of “they’re bad and we’re good” *starts* with the demands by the “science” party that nothing be practiced except what has been “empirically validated.”[1] This demand, all too often, derives from a version of science that looks more like an obsessive-compulsive symptom than the creative and disciplined application of human intellect. It insists that nothing can be regarded as having been empirically validated unless it has been tested in a randomized controlled clinical trial. None of the other ways that science operates, none of the hundreds of kinds of methodological procedures and safeguards that are used in the wide range of sciences – from quantum physics, to cosmology, to geology, to cellular biology, to paleontology, to countless other legitimate sciences – none of these are relevant. Only randomized clinical trials.

Imagine if Darwin’s theory had to be tested via randomized controlled clinical trials. We would have to find a granting agency that had sufficient funds (and patience) to fund a project over several billion years! We would have to find a sample of numerous alternative planets (the equivalent of patients in the “randomized clinical trials” paradigm) to randomly assign to pre-specified conditions. Indeed, taking still another of the hallmarks of the extreme scientism camp of clinical ideology, we would have to develop a *manual* for the planets’ evolution. For even if, playing along with the absurdist science fiction scenario I have just created, we found that planets in condition A evolved one kind of life and planets in condition B another, without a manual these zealots would not be satisfied. “How do we know,” they ask, “that planet A *actually practiced* being close to a warm star and having abundant water or that planet B *actually practiced* having a meteorite hit just when its dinosaurs were thriving? Unless we can spell this out in advance, create a manual, train the planet in compliance with the manual, and make sure the planet doesn’t covertly practice any funny stuff, we can’t have much faith in the findings, and the proposition cannot be labeled as “empirically validated.”

In a great irony, the anti-evolution voices of so-called creationism in my own country have an implicit ally in the researchers and psychotherapists who insist that no procedure that has not been tested in randomized clinical trials should be viewed as validated. Evolutionary biology, like many sciences such as geology or cosmology, obviously cannot be approached through randomized trials or, in large measure, through controlled experiments altogether. Thus, the position taken by the extreme wing of the “empirical validation” forces in our field lines them up with the anti-science forces of the creationists. What they have in common is a very creative capacity to ignore evidence that doesn’t fit well with their preconceptions and to declare doubtful and inadequate the evidence that exists.

For the absence of controlled clinical trials does not mean the absence of solid evidence. The sciences I just mentioned (geology, cosmology, etc) do not utilize any equivalent of randomized clinical trials, but this does not mean that they cannot be investigated by very precise, methodologically sophisticated, or genuinely scientific research. The same holds for psychotherapy and for the theories on which it is based. There are many ways to do valid empirical assessment, and it is the totality of evidence we should be addressing, not just randomized trials.

I am sometimes struck by the use of the term “gold standard” to refer to randomized clinical trials. We should remember the story of King Midas. Turning everything into gold is not always wise.

The Preoccupation with Manuals in Clinical Efficacy Research

I do not disagree that, all other things being equal, evidence of efficacy via randomized clinical trials is perhaps the strongest evidence – even if by no means the *only* relevant evidence – for the efficacy of a clinical approach. What I most forcefully do not agree with is the idea that manuals are an essential component. I certainly understand the general logic behind the manualization movement in psychotherapy research. If we are comparing two therapeutic approaches, we need to be able to be confident that what characterized the two groups was in fact the two treatments purportedly being offered. But manualization is by no means the only means of doing this, and in many instances it is highly inappropriate.

To begin with, the manual is only a means toward an end, not the end itself. Even with a manual, there need to be compliance checks to ensure that the therapist is *following* the manual. But if there are compliance checks, then *they* serve – with or *without* a manual – as the means of evaluating. If the therapy being investigated is not a “manualized” therapy, then practitioners of the particular approach being evaluated can look at the work being done – without knowledge of the outcome, so that their judgments will not thereby be biased – and judge the degree to which the therapy approximates that approach. If there is reliability in these judgments by experienced expert practitioners of the two approaches as to which practice sample, judged blindly as to outcome and purported label, belongs to which approach, we can be reasonably confident that the different conditions represent the essential and distinctive features of the different approaches.

After all, whether the therapist is in compliance with the manual is *also* ultimately a matter of judgment, also ultimately in the eye of the beholder – because fortunately, not all manualized therapies are quite as trivial as the “manual” concept might imply. So evaluating whether the therapist has faithfully followed the manual *also* involves a good deal of judgment and choice on the evaluator’s part. Each application of the particular approach is likely to be somewhat different with each patient, and hence, the judgment of whether the therapist is complying with the manual is not a simple checklist judgment but a *real* judgment. Indeed, it is a judgment that is not that different from the judgement that would follow from judging if the therapist is following a treatment approach that is *not* manualized.

What the movement toward manualization in psychotherapy research reflects – a movement whose strength derives very largely from a “gun at the head” enforcement of it through the awarding (and not awarding) of research grants – is two things. First, it reflects a rather mechanical view of science, a “break it into pieces” approach that *has*, it must be acknowledged, worked very well in quite a few areas of inquiry, but that is by no means synonymous with the scientific method. When it is employed not because it *happens to be appropriate* to a particular subdiscipline or a particular problem (which, again, it often is) but because it reflects an across-the-board insistence that without it there is no science at all, then what we have is, again, an obsessive-compulsive symptom masquerading as science.

Second, the emphasis on manuals (along with the insistence that one study a narrowly defined DSM or ICD diagnostic group) is a tendentious, thinly disguised effort to legitimize some approaches at the expense of others, a maneuver in the economic market place much more than an honest effort at science. It is not just coincidence that the primary proponents of this approach to empirical validation are cognitive-behavioral and that it is an approach that *by its very definition* renders the competition not just unvalidated but *incapable of being* validated. For if one criterion for supposed empirical validation is the employment of a manual in the research, then *by definition*, a therapeutic approach that does not employ a manual cannot be “empirically validated.” This is politics, not science.

Science and Intuition

Let us take a closer look at the “break it into pieces” approach to science that is reflected in the insistence on manuals. Some concepts are actually best measured not by criteria spelled out in advance in great detail, but by broad, quasi-intuitive judgments. If they are *then* checked in other ways – for example, using the standard canons of evidence to determine whether the judgment can be reliably replicated by *another* judge who approaches the judgments independently – they are as much “scientific data” as are more molecular measurements on a detailed checklist.

The mistrust of intuition that is reflected in the insistence on manuals flies in the face of much evidence about the actual nature of scientific inquiry, even in the so-called “hard” sciences. The classic writings of Michael Polanyi, a prominent scientist working on the borders between physics and chemistry as well as a philosopher and historian of science, makes this very clear. Polanyi (1966) has discussed in detail, as an essential part of the scientific enterprise, the “tacit dimension” of subtle observation that can be sensed but not yet articulated, and he has shown that it plays a prominent role in the so-called “hard” sciences as much as in our own field. It is the failure to appreciate or acknowledge this tacit dimension, the ideological mistrust of intuition, that I am referring to when I describe certain visions of the essential nature of science as obsessive-compulsive.

One early example from my own work illustrates how the power of intuition can be harnessed to the methodologies that modern science has evolved for checking on that intuition. It illustrates as well why it is scientifically impoverishing to insist on a manual or any other “break it into pieces” methodology when that approach is not suitable to the phenomenon being studied. In the study I am referring to, conducted together with the late Jean Schimek (Wachtel & Schimek, 1970), we investigated various effects of the emotional impact of incidental stimuli. Subjects in the study free associated and made up stories to TAT cards. Unbeknownst to them, part of the study included a sound that was coming from the office next door. The sound – for half the subjects it was the sound of an argument and for the other half of light music and laughter, sounding like a happy party – was at a volume such that the words could not be made out. Later debriefing of the subjects made it clear that the subjects did not recognize that these sounds were part of the study. Moreover, although almost all of the subjects experienced the sound as incidental and did not focus on it, they could, when later questioned about it, accurately identify whether it sounded like the people next door were angry or were in a good mood.

I will not go into detail about the findings – the general thrust was that the emotional tone of the incidental stimuli did affect the emotional tone of the subjects’ free associations and TAT stories; there were more angry themes in the stories when the “angry” tape was playing next door. What I do want to emphasize is that our efforts to create a “manual” for our scoring of the protocols were not only extremely time consuming (and, indeed, obsessional feeling), they were unsuccessful. That is, we were not able to create a piecemeal set of specific behaviorally manifest items to check off that effectively differentiated the response to the two different incidental stimuli. But we were able to achieve high levels of inter-rater reliability and to detect meaningful differences between the responses to the angry incidental stimulus and the pleasant incidental stimulus, when – instead of relying on a manual – we relied simply on our ability to perceive, in a direct and global manner, an emotional meaning in the subject’s material. When, instead of creating a manual for detecting anger in the associations and stories, we simply made a global judgment of how much anger was present (blind as to which condition the stories and associations came from), we got much better and more meaningful results.

Excesses on the Other Side:

Defensive Dismissal of the Scientific Canons of Evidence

This little story from the early stage of my professional career has, I think, interesting implications for both sides of the artificial divide that has been created in our field. On the one hand, it points to the importance of transcending obsessional criteria like manuals. Explicitly spelled out criteria sometimes are the best path to knowledge, but sometimes they are an obstacle. But it is *also* important to be clear that although I have been rather hard on the proponents of “empirically validated” treatments thus far, there are equal and opposite excesses to be found among their opponents. What I have said thus far should not be taken as giving wholesale license to the self-indulgence that is the evil twin of scientism. That is, the message is not – or should not be – simply “trust my judgment.” We made the effort in our study to assess the *inter-rater reliability* of our judgments for a reason.

What I mean by self-indulgence is well illustrated by a posting on a psychoanalytic list serve I belong to that was discussing the necessity for empirical validation of psychoanalytic ideas and the effectiveness of psychoanalytic work. The writer was a fairly well-known psychoanalytic author. I will protect him from himself by not mentioning his name. But I will quote his point of view. In joining the fray on the side of clinical observation and against systematic empirical evaluation (which, again, should not be equated with manuals or randomized clinical trials), this author said the following:

How about this for a kind of research evidence: I now have 14 books documenting ins and outs of the creative psyche in psychoanalytically oriented work behind closed doors. What speaks through these books is the voice of authentic psychical reality – subject to correction, amplification, further work. But what is there is real. Reality speaks.

Interestingly, this same author, in the same message, informs us of his love of and fascination with science. This is a frequent, and comfortably self-congratulatory, expression of bland piety toward science, expressed by many participants in the list serve exchange I am referring to, and it seems to express a love of science as long as it doesn't interfere with their beliefs. The motto seems to be, “science for the masses, intuition for *moi*.”

Put differently, rejection of the *narrow* and *political* criteria for empirical validation is appropriate, but often it disguises a rejection – sometimes smug, sometimes defensive, sometimes both – of *all* systematic efforts to validate the clinical efficacy of psychoanalysis or the ideas on which it is based. Behind banners such as postmodernism, constructivism, or hermeneutics, and employing tiresome straw man depictions of “positivists,” these defenders of the faith ironically ignore some of the most fundamental implications of psychoanalysis itself – that we are extremely prone to self-deception; that this self-deception is often motivated and generally not noticed; that it serves to keep us more comfortable, to not notice what would ruffle our psychic feathers; that our very perceptions, our convictions about what we have “seen with our own eyes,” are subject both to motivated and to unmotivated skews and distortions; that our memories too are suspect, and hence it is dangerous to trust them if we rely on after-the-fact recollections (the mode in psychoanalytic papers) rather than systematically recorded accounts. It is precisely the scientific method that has developed to deal with this state of affairs. Science, in all of its various methodological forms, is essentially a means of attempting to observe in a way that at least partly addresses our proneness to see what we want to see and remember what we want to remember.

Indeed, we might say that it is our *clinical* observations and theories that, most of all, alert us to be suspicious of those very observations and theories if not evaluated in ways more systematic than the accumulation of reports by analysts of what they remember went on in their offices last week. If our clinically based theories have any value at all, they point to an extraordinary capacity of our species to deceive ourselves, and hence to the vulnerability of purely clinically based theories without some further effort to control for this tendency. We are stuck in a closed circle if we do not have a way of evaluating our observations that does not rely on just “trust me; I saw it in my office.”

This does not mean the dictatorship of one particular methodology. Science is a term whose precise meaning is very difficult to pin down, but whose spirit is easy to detect if one is both honest and reasonably aware of the way our perceptions and convictions can lead us astray. We do not have to abandon our concern with nuances and subtleties of subjective experience or with unconscious influences on thought, feeling, and behavior in order to be “scientific.” Yes, it is true that the subtleties of affect and experience can be *difficult* to capture in systematic studies, and that many published studies are concerned with trivialities or superficialities that seem to working clinicians quite irrelevant to the work they do. But if we have a less ritualistic or obsessive-compulsive understanding of what science is, there are possibilities of harnessing its safeguards while also employing the same empathic and perceptive capabilities of the clinician that she relies on in the consulting room. Remember, as one small example, the study I mentioned earlier, in which the “mechanics” or trappings of science did not work but the systematic employment of ordinary emotional sensitivity proved to be both meaningful and reliable. In a similar vein, we may note Westen and Weinberger’s (2004) demonstration that the old dichotomy between clinical and statistical methods is a false one, and that often what is most effectively combined in statistical fashion are not simplistic checkmarks on self-report instruments or the evaluations of lay observers but the sophisticated judgments of experienced clinicians (but bear in mind as well their powerful arguments that *combining* what clinicians do well and what statistical analysis does well yields the most useful and reliable knowledge).

Viewpoints and Methodologies

No one point of view “owns” science – not cognitive behavior therapists, not neuroscientists, not practitioners of controlled clinical trials. Science is simply a way of keeping ourselves honest and, often, of amplifying our perception via new methodologies and technologies – whether they be the telescope, the microscope, the particle accelerator or, for that matter, the tape recorder.

The tape recorder, that by now humble instrument, much more likely to be found in the rooms of children and teenagers than in high-tech laboratories, is still an insufficiently tapped resource for turning clinical process and clinical intuition into scientific data. To begin with, audio or video recording allows others to see the same material and make their own independent judgments (though there are of course differences between seeing a tape and actually being there in the affective field with the patient – no solution is perfect; there are always compromises). Second, tape recording permits the therapist him or herself to check on what has been remembered. It is striking how different a sequence can be when one watches it on tape from what one has remembered (and the subtle differences are just as important as the dramatic and obvious ones). Third, sometimes it is only after watching something many times that we can see something that has, in essence, been lying there waiting for us to notice all along. In one of my very first published papers, concerned with what is communicated by body language (Wachtel, 1967), I described a pattern I did not see until I had looked at the tape more than fifty times. But once I finally noticed it, it “jumped out” at me and became rather obvious.

We still have not exhausted the potential of such simple methodological innovations as the audio and video recorder, not to mention the further possibilities for enhancing our observational acuity represented by methods of analyzing such data in a frame by frame way (e.g., Beebe & Lachmann, 2002; Stern, 1985; Tronick, 1989). This work, along with, for example, the systematic research on attachment and reflective function, which utilizes transcripts of tape recorded interviews (e.g., Fonagy, 2001; Fonagy, Target, Steele, & Steele, 1998; Main & Goldwyn, 1998; Hesse, 1999) shows how *clinically meaningful* findings can derive from new observational methods that enable us to notice phenomena that we otherwise might miss, including phenomena that are close to the heart of the interests of psychoanalytic and other more “clinical” theorists.

Utilizing tape recordings or transcripts derived from them is just one example of a “scientific” advance over just reporting what one remembers from one’s sessions (memories often written down only at the end of the

day or even days or years later looking back on the case). I mention the tape recorder precisely because these days it is a rather humble instrument, available even to children, and yet it represents such a signal advance over the traditional case report, one Freud could not really imagine when he first began his work. I mention it as well because it is a method that basically retains the usual focus of the psychoanalyst. That is, it is directed to the same kind of material that the traditional case study is directed toward, recording the patient's and therapist's words and the therapist's effort to be empathically attuned to the patient's affect state. It is not a diversion from or an avoidance of those data and that effort, not a method that is restricted to checkmarks on questionnaires or to what is immediately consciously accessible or "objectively" measurable. Doing research (or even clinical supervision) from an audio or video tape still requires a good deal of inference and interpretation; it is not always "straightforward." But the conclusions reached are more publicly accessible, more capable of being evaluated by the professional community without having to simply take the reporting analyst's word for it. Moreover, even the analyst's or therapist's subjective experience in engaging in the exchange is not excluded, since it can be provided by the therapist just as it is in the "trust me" case report. And indeed, it may be a *richer* report of the therapist's subjective experience, since it is offered, while the therapist watches the tape, with more reminders of what transpired, in response, we might say, to "thick description" (Geertz, 1973).[2]

More complicated or technologically advanced ways of improving on what we can know just from sessions are, of course, also available, often in the form of some kind of physiological or neurological recording, but including many other methods as well. The point is by no means that physiological evidence is more "real" or "solid" than psychological. After all, how do we even know what a particular pattern of neural firings in the brain *means* unless we relate it to behavioral or experiential data. The "hard" physiological indicators are only as good as the "soft" psychological indicators to which they are correlated. If we do not have good, differentiated indicators in the one realm, we will not in the other. But these physiological indicators can improve our understanding very considerably nonetheless when they are combined with sensitive attention to the psychological realm. What is done under the best methods of inquiry is a kind of mutual bootstrapping. What is going on psychologically, after all, is often hard to discern or articulate or know how to organize into patterns. That is what is meant by saying that psychological matters are complex (a claim often made by those contending that they are too complex to approach via "science.") But just as we can understand a previously not appreciated theme or thread in a patient's narrative by noticing that the (not yet fully developed or articulated) content is interrupted by a long pause or a puzzled or distressed look – part of what good clinicians do naturally – so too can we understand more fully the psychological meanings, the conflicts or unacknowledged desires or fears that the patient is struggling to express if we have, as an aid, information about what brain areas light up on an fMRI.

Obviously, at least as psychoanalysis or psychotherapy are presently practiced, the latter situation refers more to the laboratory than to the clinical consulting room, but it nonetheless refers to the illumination of *psychological meanings theretofore not understood or appreciated*. In both instances, what enables us to make progress is active and sensitive inquiry. In the case of the patient who becomes silent or manifests a noteworthy facial expression or shift of posture, what we do to pursue the not yet fully manifest meaning is to ask what was going through the patient's mind during the pause or just before the appearance of the particular facial expression. Similarly, in the hypothetical laboratory situation, one similarly *asks* the patient what was going on when the physiological indicator lit up. The signal from the brain scan alerts to ask about something that might have slipped past our notice and gone unappreciated or uninterpreted. In turn, the increased differentiation and subtlety of our psychological understanding enables us to improve our methodology for brain scans, providing a new and better platform for still further cues to the investigator to notice still other potentially overlooked and subtle psychological phenomena. In principle, this mutual enhancement from one realm to the other can go on over and over, and with each iteration we have a new platform for further advances, each building on the other.

In a different realm, closer to what today is clinically possible, the work of Luborsky and his colleagues on what he calls the symptom-context method (Luborsky, 1996) illustrates this kind of investigative bootstrapping very well in the "real world." Luborsky's psychological inquiries are triggered by particular,

characteristic and repeated symptomatic acts or events (they can be somatic, such as a pain or a twitch, or they can be psychological, such as an experience of forgetting what one was going to say). Using those events as the starting point, he systematically investigates the context of meaning that precedes and follows the target event. By looking very closely at material that has been tape recorded, one can notice sequences and connections that are easily overlooked as the material flies by in “real time.”

The point, both in the real examples by Luborksy and in the hypothetical ones I am imagining vis a vis brain scans and other “technologically assisted” methods, is that the pursuit of meaning, the hermeneutic quest, is not antithetical to the process of inquiry pursued from an “external” vantage point. Rather, if the inquiry is pursued creatively and with openness to diverse sources of illumination, rather than reflecting what might be called dichotomous “antithesizing,” we find that the dichotomies often turn out to be a product of our preconceptions and biases, and that in fact attention to the “alternative” point of view deepens our understanding of each.

Concluding Comments:

The Contribution of Theory and the Pervasiveness of Vicious Circles

In the process of mutual influence and feedback I have been describing, in which affective and intuitive immersion in the immediacy of the clinical situation and attention to the findings of systematic research provide mutual bootstrapping, the advance of theory is a crucial third dimension. I am very much in agreement with Kurt Lewin’s (1951) contention that “there is nothing so practical as a good theory.” Having a theory that pulls together observations and findings from diverse realms of observation enables us to notice things we didn’t see before and to anticipate and look for phenomena we have not yet encountered. Many of the most significant new discoveries of science – the encountering of phenomena that were not even dreamed of by the best minds of previous centuries – were spurred by theories; theories that told us to look for something we would not have looked for without them.

In the arena of human behavior and experience, however, the dominant theories and disciplines have been hampered by a tendency toward building theory upon the foundation of only a limited range of observations. In the psychoanalytic realm, for example, aspects of the theory that evolved from the process of free association led to theoretical ideas (neutrality, anonymity, and abstinence, for example) that long perpetuated free association or closely related methods as the only appropriate means for psychoanalytic investigation and hence for psychoanalytic theory-building. The consequence was a restricted observational field in which observations that might prove contradictory to received ideas were unlikely to be encountered. Received theory thus appeared artificially or artifactually adequate, but the *growth and change* of theoretical ideas through the challenge of unanticipated observations was limited.

There has, of course, been change in psychoanalytic thought and practice over the years, but that change has still proceeded within a bounded set of possibilities. Preoccupation with whether an idea or method was genuinely psychoanalytic took precedence over whether it was an accurate and sufficiently comprehensive account of human behavior and experience or of whether it enhanced the possibilities for providing patients with the help they sought. Many innovations that passed the second set of criteria were rejected because they didn’t pass the first (that is, they were deemed “unanalytic.”). In recent years, the accelerating impact of new relational ideas has been associated with new psychoanalytic *methods*, and this in turn has brought into focus new observations that require still further theoretical and procedural modifications (see, for example, Mitchell, 1988, Aron, 1996, Mitchell & Aron, 1999). But even these advances have been constrained by a tendency to draw a boundary (even if now a somewhat more relaxed and expanded boundary) around “psychoanalysis” and to exclude via that boundary observations that came from proponents of approaches such as family systems and cognitive-behavioral theories.

To be sure, proponents of these latter approaches have been equally narrow. Cognitive-behavioral therapists and theorists in particular have tended to be blind to the importance of the observations that have accrued from psychoanalytic work for over a century. SEPI, the organization to which I alluded at the beginning of this paper, has been a relatively rare venue for psychoanalysts, behavior therapists, family therapists, and others who wish to learn from outside the artificial boundaries of their home orientation to exchange ideas and build more comprehensive models.

In my own efforts to recast psychoanalytic ideas in ways that can enable them to assimilate as well observations that derive from non-psychoanalytic sources, the concept of the vicious circle, discussed in various ways throughout this paper, has been central. Attention to vicious circles not only illuminates the dynamics of personality more fully and comprehensively, it also permits us to see how each of the major competing schools tends to focus its attention on only a part of the larger pattern that characterizes our lives, a pattern that includes both “inner” and “outer” influences, the influence of the stored impact of the past and the influence of the new impact of the present context. Indeed, by attending to the larger pattern of vicious circles, we find that the very dichotomy between the “inner world” and the “external” environment is a false and misleading one. If we understand fully and properly the so-called inner world of hidden wishes, fantasies, conflicts, self-and-object representations, and so forth, we come to see that these are not just contents or forces in a separate realm “inside” the psyche, but reflect the alive and active ways we *respond to the world*. Looking closely, we see that the persisting unconscious fantasy, the transference inclination, the seemingly archaic desire or fear are part of a pattern of responding to the actual world of events and experiences that we continually encounter. But the world to which they respond is not composed of the ‘independent variables’ of the classical psychological experiment. The so-called “external” world is a function of our “internal” world as much as the internal world is a function of the external environment. Based on our wishes, fears, and fantasies, we *act*. And when we act, others respond to our actions in turn, providing input from the “external” environment that is itself a function of our “inner” proclivities and that, moreover, is further worked over and interpreted in light of those proclivities (Wachtel, 1973, 1981, 1997).

This is much more than just what psychoanalysts have called enactments. Enactment is a concept that, although it illuminates what we actually do in the world, still usually prioritizes the internal. It is the already existing, *internal* engine of behavior that is emphasized, and the context in which it is displayed is simply the stage on which a play already written in childhood is performed. In contrast, the circular process I am emphasizing highlights the *bi-directional* nature of causality in the psychological realm. The psychological structures we discover in our therapeutic work are not simply residues from childhood. They have their *origins* in childhood, but they evolve and change in response to new circumstances. At the same time – and here is the ironic heart of the vicious circle – the circumstances are themselves largely (though by no means completely) determined by the psychic structures and inclinations we have already developed and the behavior in the real world that they generate. We do not understand people very well if we are overly inclined to see their present behavior and experience as determined by their childhoods rather than by what is actually going on in the present. But, at the same time, we do not understand very well what is going on in the present without understanding the longstanding fears and desires, the conflicts, fantasies, and subjective representations, the perceptual and cognitive proclivities that have evolved in the course of development. Neither present environmental contingencies *nor* inner representations of earlier experiences are primary. Indeed, neither has much meaning apart from the other. Such is the seamless unity that the various theories in our field have sliced up and fetishized parts of .

I have attempted, in various publications, to illustrate how this process works and what its implications are for clinical practice (e.g., Wachtel, 1993, 1997, in preparation). I cannot here elaborate further on this conceptualization, but I hope I have provided a reasonably clear picture of how I envision these circular patterns in all of our lives and how they are manifest still again in the debate about science and hermeneutics that rages in our field today.

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Notes:

[1] I should note here as an aside that the terms used in this debate keep shifting, as the tendentious and clearly political dimension of the contending claims begin to be exposed. So changes in terms from “empirically validated” to “empirically supported” to “evidence-based,” to what have you keep appearing. The differences are trivial. The same self-serving arguments just keep getting repackaged.

[2] Some people, of course, argue that to record a session totally changes the configuration of what is transpiring. I believe that to be a self-serving rationalization for not exposing either one’s clinical skills or one’s ideas to this kind of scrutiny.

Bio:

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