"Integration of Behavioral and Analytic Modes:

A First Year Student's Perspective"

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2.10.1 Behavioral Psychology/Treatment Methods
2.10.2 Dynamic Psychotherapy/Treatment Methods
2.12 Training

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The impetus for this paper comes from a variety of sources over a period of time which extends back several years, and includes several different clinical and research contexts. My initial contact with psychology was an intensive dive into Jungian psychology, mostly as an adjunct to the typical spate of Sixties interest in mysticism and Eastern religion. Of course, all intellectual types have on hand some Freudian rhetoric and sketchy inklings of the meanings of oral stage, phallic symbol, and dream interpretation. So, without explicitly articulating it, my bias was already that of a dormant analyst. Then, psychology as psychotherapy, as opposed to psychology as philosophy (prescriptive v.s. descriptive?) became primary when I was hired in my first clinical position. This happened to be in a very behavioral lab and the client population happened to be chronic recidivists. So, I vehemently subscribed to that orientation for awhile, completely rejecting and ridiculing my previous stance. Towards the end of my tenure there, many of the issues of theoretical and practical integration began to interest me.

More recently, I was asked to consult on a case at the Psychological Services Center at UMass; instead of overlaying a patina of behavior modification over the psychodynamic foundation, I tried to get a sense of a unified approach. These efforts will be discussed later, as well. Finally, a recent assignment instructed that I choose several disorders and indicate which orientations could supply the most effective treatments for each. This ran counter to my thinking and prompted this paper.

The notion of rapproachement originated in the form of endless journal debates as to the relative merits of one type of therapy over another, sometimes in the form of bona fide treatment outcome studies employing a vast range of dependent variables, but often simply in a virulent exchange of name-calling. There was also divisiveness in the mode of total disregard of conflicting viewpoints. Paul Wachtel, in his introduction, writes, "Behavior therapy... is a major new trend that has developed largely in opposition to psychoanalysis, and the mutual distruct between proponents of the two points of view is considerable. Psychoanalysts and behavior therapists seem to agree on scarcely anything except the joint conviction that they have little to say to each other and that the two points of view are fundamentally incompatible."[1]

The treatment outcome literature is largely composed of one school, usually behavioral, pitting one of its subgroups against another: behavioral against cognitive-behavioral, or relaxation against biofeedback. This makes sense, since the very notion that it is possible to measure and report treatment outcome variables is itself a behavioral invention. The major interest of the treatment outcome literature lies in what is not reported, in what fails to be accomplished, and what is left univestigated.

The following list of these omissions is germaine to this subject in that the unreported variables in the behavioral literature may just be the areas in which things distinctly <u>un</u>behavioral are transpiring. For example, a Methods section includes the number of therapists involved, their gender, and maybe their level of experience and theoretical orientation. But who can tell what they actually do in the course of treatment? A behavior

therapist may be as emotionally supportive as a Rogerian, as interpretive as an analyst, or as relationship-focused as a Sullivanian. All we are told is that the structured protocol was delivered, not how it was delivered. The obvious interest here is in the so-called nonspecifics of therapy, which could be operationalized in a behavioral mode and analyzed, but are rarely the targeted behaviors. This is one direct result of the development of behaviorism as a reaction to traditional approaches. If the therapist-client interaction was deemed important by the adversaries, then the other side will pay no attention to it.

Not all of the burden can be placed upon any school. Rather, it is academic psychology as a whole, with its emphasis on the necessity that psychology act like a science, that has stimulated several unhelpful developments. The paucity of information given in a typical journal article is largely the result of the strictures of the APA format itself. The Methods sections must include a great deal of information, packed densely and stated tersely. This leaves absolutely no leeway for the vagaries of the therapeutic process, or its discussion.

Second, the rationale for the choice of targeted behaviors is never given. Some observable symptom is supposed to change, then a procedure is administered. Often, the choice is one demanded by in-house research priorities and has little to do with the optimum treatment. Or, as Donald Meichenbaum pointed out at the 1981 meeting of the Association for the Advancement of Behavior Therapy, "We are selective in admitting evidence which will validate our a priori beliefs." Again, we can look to the larger institution for perpetuation of what is essentially an anti-scientific mind-set. The implicit laws of tenure and professional advancement demand that

a great deal of one's personal output as a researcher be of the so-called "quick-and-dirty" variety. It is far easier to remain a specialist in one minute subarea of psychology, and to consequently construct studies which will validate the viewpoints arising from this area. If researchers can develop tunnel vision which does not allow for images from other subareas to develop, how can we expect psychologists to let in illumination from other orientations?

Lest it appear that only the behavioral research journals are behind the theoretical xenophobia plaguing the field today, the analysts should be given equal time. They have actually been very clever about avoiding the treatment-outcome business altogether by invalidating the premise behind it. Wachtel explains: "In psychodynamic therapies, the assessment of the patient's personality and problems in living and the treatment of those problems are hardly distinguishable. To many dynamic therapists the joint effort by patient and therapist to articulate the patient's way of living his life, and to understand how it developed and why it causes problems, is the core of the therapeutic process. In a sense, the effort to understand is the therapy?"[2]

The agreement among the analysts that the therapy alliance is fragile and nearly numinous in character allows for journal articles composed of verbatim transcripts of sessions, and other mysterious and unquantifiable data, such as Rorschach results. Such a hermetically-sealed process not only does not allow the scrutiny of outsiders, but demands an interpreter from the inside. Before the outcome of therapy can be evaluated in a manner acceptable to both parties, the analysts must find some objective, or at least identifiable and measurable variables to examine. On the other hand,

the behavioral psychologists will have to target more global items, if rapprochement is to ever occur.

Before leaving this delineation of obstacles to integration, a few more general barbs ought to be hurled. It is the nature of institutions that they resist change and help perpetuate stasis; the institutions of psychology adhere persistently to this law. In the realm of clinical training, the hardening of the theoretical lines begins at the time in January or February when applications for graduate schools are due. Very few clinical training programs are eclectic, and the student will be guided towards those schools which are in the image of his or her mentor. The analytic schools, as a rule, cluster in the Northeast; the behavioral ones in the Midwest. It is a great loss to the graduate student to receive one of what should be a diverse world of viewpoints, but the greatest loss is incurred in the lack of context into which the student may place these views. Having little or merely narrowly-defined research and clinical experience, the graduate student may not be aware that opinions expressed are expressions of a deep-seated bias.

At this point, many of the other institutions converge in the budding psychologist's field of vision. The clinical faculty has either the behavioral or the analytic journals lying around--almost never both. The student learns either qualitative (descriptive methodology favored by most non-behavioral schools) methods, or is taught to observe and quantify the observable. In either case, the alternative may be derided as fuzzy-brained, outmoded, simplistic, or may never enter into the conversation. Of course, the internships are roughly divided up in this fashion, as well. It is any wonder that behaviorists laugh and nudge each other if a psychiatrist or social worker mentions the unconscious? At the last AABT, Meichenbaum was

nearly shouted down when he mentioned integration of some non-behavioral principles into the behavioral repertoire. Should we be surprised that most analysts believe that behaviorism is nothing more than the alternating administration of electric shocks and M&M's?

Obviously, the obstacles to an integration of the psychoanalytic and behavioral schools are legion, but it is time for the inevitable Hegelian synthesis. A few people have ventured in this direction, and the second half of this paper will address their efforts.

Discussion and formal research in the area of rapprochement has proceeded on several fronts. The first, most popular and least satisfying, area is that of the treatment outcome study, specifically designed to compare treatments across orientations. The second has been the content analysis method, which parses the verbal interactions of the client and therapist. Finally, a couple of theoreticians have taken on the task of comparing the vocabulary and techniques of the two schools and attempted a translation of sorts in order to erase the superficial differences and to highlight the true dissimilarities.

The outcome study considered important in comparative therapies is that done by Gordon Paul in 1966. He targeted anxiety as the focus, and used as outcome measures a large battery of self-report tests, autonomic indices of anxiety and physiological arousal, and a behavioral check list of performance anxiety. There were five therapists, interestingly, none was identified as behavioral in orientation, but were Rogerian, neo-Freudian, and orthodox Freudian. The three treatments consisted of insight, systematic desenstization, and placebo, plus two control groups.

The basic problems with this research concerns the choice of therapists and the inadequacy of quality control; the therapists were asked to record the frequency with which they used a variety of techniques, but, as Hans Strupp noted, "As is well known, most dynamic therapists are not primarily concerned with the alleviation of an isolated symptom and they do not accept patients on that basis. Paul apparently induced them to work toward his goals rather than toward their own."[3]

Paul insured success for the behavioral method in this study by several methodological oversights. First, by using only insight-oriented therapists to deliver the behavioral treatment, he guaranteed that the treatment would not remain pure; no psychologist can unlearn years of training merely to meet a set of procedural expectations. This is part of the reportage problem we addressed earlier; a reader cannot be sure of exactly what the therapists were doing. In this case, that is the all-important question, it is the major focus of this piece of work. Second, the implicit bias of the research is behavioral from the onset. The focus is on a targeted behavioral index of anxiety, which is measured behaviorally. Of course behavioral methods will win out in behavioral research; it's a setup. One wonders why this research was not followed up with a study in which a team of behaviorists is hired to deliver insight therapy.

The other work in comparative treatment outcome substantiates an unpopular conclusion: namely, that this is not the methodology which will foster greater understanding of the superiority of one approach over another, much less bring about an integration. Even within the behavioral subgroups, the variables used favor one of the methods involved. For example, a

study of the relative methods of cognitive therapy and social skills therapy in the treatment of social anxiety (which is, by the way, not a hypothetical study, but one in which I was involved last year) has to employ dependent variables which respond both to cognitive and social skills performance. So what is the outcome? Within the group receiving social skills therapy, there is improvement on the social skills measure, and no change on the cognitive, or perhaps some seepage occurs and there is improvement on the latter, as well. We assume the situation is reversed in the case of the cognitive group. We have learned little about the mechanisms involved in change, and if no significant (that is, statistically significant) differences are displayed, we may never read the study. Apparently, the fact that two treatments are interchangeably effective is of no interest, if we may take journal editorial policies as an example.

A more fruitful area has been that of content analysis. In 1979, Brunink and Schroeder investigated verbal therapeutic behavior of 18 highly-trained analytic, gestalt, and behavioral psychologists and psychiatrists. The therapists were compared along six dimensions: type of therapeutic activity (structuring, exploring, interpreting), temporal focus (immediate present or historical past), interview focus (client, therapist, or their relationship), degree of initiative (weak to strong therapist initiative), communication (the presence or absence of rapport, empathy, or understanding), and therapeutic climate (supportive, neutral, or nonsupportive). They state their results as follows: "Compared to the other therapists, gestalt therapists provided more direct guidance, less verbal facilitation, less focus on the client, more self-disclosure, greater initiative, and less emotional support. Behavior therapists and psychoanalytically oriented

therapists were surprisingly similar in their style of therapy, with the interesting exceptions that behavior therapists provided more direct guidance and greater emotional support."[4]

This is only one study, but it does some damage to the existing stereotypes, while perhaps providing avenues for a closer alliance between analysts and behaviorists. A closer examination of the data reveals no difference between the past-present focus among the groups; the primary context was the here-and-now, with roughly ten percent of the sessions spent on the past. The relational aspect of the therapy was found equally important in both groups. Even the supposed analytic strongholds of neutrality and interpretive remarks were shared by the behaviorists, and with the same average frequency.

In what is undoubtedly the most ambitious theoretical work of integration done to date, Paul Wachtel (who wrote his book in close consultation with the behaviorists at SUNY-Stony-Brook) further reduces the distance between the two orientations through a mixture of common sense and syntactic manuvering. One of his major topics is the transference. Most analytically-oriented therapists would maintain that the extensive assessment and active confrontation favored by the behaviorists would dilute, destroy, or otherwise interfere with the blank screen projection which is the major mechanism for change in the analytic tradition. Wachtel recommends a less rigid definition of the client-therapist relationship, suggesting "(a) that a greater range of permited therapist behavior will lead to a greater range of patient's potential ways of being becoming manifest in the sessions, and

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(b) that at the very least it is necessary to recognize that what is revealed by remaining constant is not "the" true underlying personality, but those aspects of the patient's possible modes of adaptation that are likely to occur in a context of frustration and minimal feedback."[5]

On the other hand, behaviorists will have to acknowledge that something of an interpersonal nature actually occurs in the consulting room, regardless of whether they attend to it or not. The therapist is not merely an instrument for instructional delivery of this or that technique. Part of the difficulty lies in the mystique which has developed around the transference; if the transference could be conceived in a less emotionally-laden term to include all therapist-client interactions, then everyone could begin to speak a common language.

A useful device, expanded but not invented by Wachtel, is that of simultaneous translation of the vocabulary of one school into that of the other. This is not as gratuitous as it may sound; in my consulting, I found it the only method of communication between behaviorists and analysts. The schism has gotten so large that there are literally no terms upon which the clinicians in question can agree. At the Veteran's Administration Hospital in which I worked, it was painful to observe a meeting between the analytic staff psychiatrist and the behavioral staff psychologist. Since they were often assigned to the same treatment teams, this lapse was often troublesome.

According to the Wachtel schema, it need not be. Let us take a simple example first: the reduction of tension. We may immediately associate this with a progressive relaxation technique, and thus chalk

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this area up to the behavioral side. But is the analytic situation itself an in vivo experience of tension reduction? The room is darkened, the voices are calm and quiet, there is considerable silence. Analysts deal with relaxation implicitly, not explicitly. That does not seem an insurmountable obstacle to rapprochement.

Or, consider the probing of the unconscious for absent memories. While it is true that the behaviorists would consider this activity a waste of time, the technique used in psychoanalysis for reaching these stored images is remarkably close to systematic desensitization. In both processes, there is a gradual, temporal move toward an aversive event, or memory, or object. In both, the therapist is continually receiving feedback from the client as to how fast is too fast, in time and in tone. When the goal is reached, a breakthrough of sorts is expected, with a consequent reduction in the perceived aversiveness of the object in the eyes of the client.

Finally, reinforcement is omnipresent in the analytic repertoire. Interpretation is a form of attending to the positive or to a belief held by the client. Restating what the client says serves the purpose of calling it to attention as a potentially useful path. Silence on the part of the therapist can be interpreted as passive acceptance of the previous remark; this is a passive sort of reinforcement.

These equations highlight several similarities in the goals of behaviorists and analysts. First, both seek to increase the constructive aspects of the client's feelings, behaviors, and thoughts, and to decrease the frequency of defensiveness, maladaptive behaviors and resistance to the therapy itself. Second, both seek that which is perpetuating the client's discomfort;

the search is carried on differently, but the goals are identical. Third, both seek to engage the client and keep the client in therapy for as long as is necessary; the latter is achieved by giving the client a progress report every so often. This may take the form of direct compliments or subtle interpretations.

If the feuding is to end soon, this common language needs some work. As psychologists, we will benefit from some cross-fertilization of ideas; already, the behaviorists are showing signs of admitting cognitions into their cosmology and the analysts are looking for more structured therapy methods to reduce treatment time. But they still talk to each other very little, and subscribe to much of the defensiveness and resistance which would be interpreted as signs of psychopathology if they were the clients and not the psychologists.

Notes

1. Wachtel, P. Psychoanalysis and Behavior Therapy: Towards an Integration. New York: Basic Books, Inc., 1977, p. 4.

2. Ibid., p. 105.

3. Strupp, Hans H. Psychotherapy: Clinical, Research, and Theoretical Issues. New York: Jason Aronson, Inc., 1973, p. 695.

4. Brunink, S. A. and Schroeder, H. E. Verbal ther peutic behavior of of expert psychoanalytically oriented, gestalt, and behavior therapists. Journal of Consulting and Clinical Psychology, 1979, 47(3), 572.

5. Wachtel, op. cit., p. 70.

REFERENCES

Brunink, S. A. and Schroeder, H. E. Verbal therapeutic behavior of expert psychoanalytically oriended, gestalt, and behavior therapists. Journal of Consulting and Clinical Psychology, 1979, 47 (3), 572.

Strupp, H. H. Psychotherapy: Clinical, Research and Theoretical Issues. New York: Jason Aronson, 1973.

Wachtel, P. Psychoanalysis and Behavior Therapy: Towards an Integration. New York: Basic Books, 1977.