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The Relative Value of Empirical Evidence

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Arguments between adherents of different therapeutic models are often based on the assumption that empirical evidence is ultimately the judge of the merits of the respective approaches. The gathering of empirical evidence, however, is programmed by epistemological premises that in turn are not tested by "objective" demonstration but rather by their congruence with prevailing sociocultural values. The epistemological premises behind individual- and systems-oriented models are incompatible, and respectively congruent with currently coexistent and opposite sociocultural values. Adherence to a particular model is thus based on epistemological-axiological positions rather than neutral objective evaluation.

Approximately half of the June 1978 issue of *Family Process* is devoted to an interesting polemic between Gurman *et al.* (4, 5, 6) and Jacobson and Weiss (10) about conceptual and empirical issues of behavioral marriage therapy. The sequence of the argument, which includes replies and counterreplies covering the various levels of the polemic, points to an escalation of disagreement and a strengthening of the respective positions, as suggested by two symmetrical statements: "[Gurman *et al.*'s] critiques of the behavioral approaches ... no doubt will be influential among non-behavioral audiences" (10, p. 149); "[Jacobson and Weiss'] comments will undoubtedly be hailed by their behavioral colleagues. ..." (6, p. 166).

Whatever their differences, both groups of authors seem to agree on the basic assumption that eventually the empirical evidence will put an end to crucial aspects of the disagreement. "In the final analysis," say Jacobson and Weiss, "the theoretical controversies regarding behavioral marriage therapy (BMT) will be decided in the empirical arena" (10, p. 157). Their opponents had already stated that "research evidence *to date* does not *yet* support... a near-wholesale and exclusive endorsement of a behavioral approach to marital problems" (4, pp. 121-22; italics added).

The basic purpose of this article is to challenge that assumption, a widespread one among discussants of psychotherapy who are understandably concerned about the "scientific" foundations of their field. The point to be developed is that the value of empirical evidence is relative to the epistemological context in which it is gathered and that the fate of such premises—their success or failure—depends largely on sociohistorical factors rather than on "objective" demonstration.

Some of the epistemological premises behind the three models discussed in the Gurman *et al.* vs. Jacobson and Weiss polemic—the behavioristic, the psychodynamic, and the systemic—will be examined, as well as some of the practical implications of adhering to each one of them.

Because the opposition between individualistic and systemic thinking seems to be currently the most relevant, the similitudes between behavioral and psychodynamic models will be more emphasized than their differences.

"Reading" Reality

A first indication that "empirical evidence" does not seem to "speak by itself" but rather is subject to often conflicting interpretations is the debate between Jacobson and Weiss (10, pp. 157-59) and Gurman *et al.* (6. pp. 173-77) about which is the "right" way to categorize and analyze outcome studies—whether it is "right" or not to contrast behavior therapy with eclectic approaches, and so on.[1]

As Bateson (1) noted, there is no such thing as a "neutral" or "uncontaminated" grasping of "reality" but rather a patterned approach to it after a set of categories that regulate both our perception of and our action on reality. (Even this statement, including concepts such as "perception of" and "action on" reflects a certain categorical system.) Unlike Kant, who thought these categories to be universal absolutes, Bateson pointed to their cultural conditioning. Gurman and Knudson (4) allude to these sets of categories—in Bateson's terms, "epistemologies"—as "a series of assumptions, philosophies, and points of view" (p. 125)

A basic epistemological position in our scientific and everyday thinking is to "read" reality as consisting of *sequences* of discrete *events.* Some alleged clairvoyants challenge this epistemology by attributing their eventual "success" to a perception of reality that is not patterned along time categories; thus, they say, it is not that they "predict the future" but that they are reading a more complex and rich "present" (although "present" is here a translation of our usual categories of time).

Another usual component of our current epistemology, namely, the attribution of a *cause-effect* relationship among events that "occupy" different "positions" in a sequence, has eventually brought trouble to systems thinkers, who can properly be described as trying to develop a new epistemology on the available concepts—and are often aware of this frustrating limitation.

General agreement around the events-in-sequence kind of epistemology does not preclude further disagreements. Events, if they are to be conceptualized at all, need to be *punctuated*. For instance, describing a sequence as consisting of the two events, "A's behavior"—"B's behavior" requires ascribing a "beginning" and an "end" to each one of the "behaviors." Different observers may select different beginnings and ends. Does A's behavior end when she stops talking? Does B's behavior start at that point? Or when *he* starts to talk? *Whose* behavior is the intervening silence? Or is silence not a behavior? Or maybe each grammatical sentence should be considered a separate behavior. But then again, it could also be that *nonverbal* manifestations should be considered as beginnings and ends. If so, does a gesture by A in the middle of B's statement mean that B's behavior is in fact two separate behaviors? Jacobson and Weiss may feel that their "commitment to viewing behavior as learned, to development of testable hypotheses, to understanding behavior by means of systematic control" (10, p. 150) affirm them on solid ground, but the ground looks a bit more shaky if one stops to consider just what "behavior" means, or, as Gurman and Knudson (4) point out, that "*the* cause of primary discriminative stimulus of another person's behavior does not exist unto itself, but is 'chosen' by a patient or therapist" (p. 128; italics in original).

There is no objective, "empirical" answer to these questions. The closest we can get to objectivity is to view "behavior"—if we are to keep this concept descriptive of reality—as a continuum. A's behavior does *not* stop when B's starts. But researchers—not to say practitioners—need to conceptually organize this fluidity if they are to operate on it, so *some* answer needs to be provided. Even at this modest level, the collection of "empirical evidence" turns out to be organized by non-empirical decisions.

Punctuation, however, is just one of the epistemological problems to be considered. After a "beginning" and an "end" have been decided, the considerably more complex matter of what the behavior is or consists of remains. As was suggested above, the "what" may be considered to be verbal aspects and/or nonverbal ones. More specifically, there is room here for content of speech, number of words, speed in speaking, positioning, movements, gestures, tone of voice, physical proximity, etc. Because it is practically impossible to cover in a single observational act the whole variety of phenomena that could potentially be observed and because it is certainly impossible to give them "objective" differential weights so that one can objectively decide "what kind of behavior that one is," a process of selection is necessarily involved. As a result, A's behavior as described by observer X is not necessarily the same as when described by observer Y—even if X and Y started and finished their observations at the same points—unless X and Y have previously agreed on their "coding" system.

"Operational definitions" have been advanced as the answer to the problems presented by punctuation and selection of events. However, while they are necessary in order to make public the way in which reality has been punctuated and selected, they do not answer the question of whether the punctuation and selection are "correct."

Operational definitions are in fact *meanings* ascribed to behaviors, in the sense that they put together under the same concept a variety of events (behaviors or details of behaviors) that "mean" the same thing for the observer. Thus, if a behaviorist defines "aversive behavior" as, for instance, "any behavior of A that B rates as unpleasant," then all behaviors of A complying with that characteristic will have, for the observer, the "meaning" of aversion. In this sense, Gurman and Knudson (4) are wrong when asserting that, for the behaviorist, the behavior "does not mean" (although it is true that sometimes behaviorists seem to think that "their" meanings are "objective" and in no need of interpretation). The appropriate question is not *whether* behavior means or not, but *what* is the behavior meant to mean. "Aversive" behavior, when observed and selectively described by a psychoanalyst or a systems thinker, might respectively "mean." "projective identification" or "one further step in the escalation process." Conversely, the same *concept* will be operationally defined in different ways depending upon the theoretical position, as is the case with "control" or "conflict" in the Gurman *et al.* vs. Jacobson and Weiss controversy.

Operational definitions are not isolated; they are part of the last step within a process of hypothesis and deduction that begins at a more abstract level of concepts. Operational definitions are part of and have been developed within a given theory or model—a structure of explanation. In other words, the conceptual structure of a model governs the observation of reality—the collection of "empirical evidence." As Minuchin *et al.* (14) put it: "The investigator's point of view, or governing concept, is his blueprint. It determines the selection of events to be studied and also the methods to be used. Data that are significant to the governing concepts are highlighted. Other data are overshadowed or excluded. Identical observations thus yield radically different working formulations when they are organized according to different conceptual frameworks" (p. 74).

It is not just a different institutional setting, as Gurman and Knudson (4, p. 125) suggest, that persuades a therapist that certain assumptions are "misguided in the overwhelming majority of cases" but mainly the therapist's epistemological setting. Clinical experience is experienced within a certain conceptual framework.

Consider, for instance, the concept of resistance. "Resistance" is one way to read some of the transactions between therapist and client; as Jacobson and Weiss (10, p. 160) correctly suggest, the same transactions may be alternatively read as "therapist's ineffectiveness."More than that, if the conceptual framework of behavioral therapists organizes them not to "touch" certain areas of the clients' experience (as may be the case if the problem is "framed" as lack of skill, as opposed to dubious motivation), then they probably will not see any resistance to their work. Of course a psychodynamic therapist might then contend, *from his own conceptual framework,* that a relevant aspect of the clients' experience has been left aside and hence no real change can possibly occur.

Let us go back to the conceptual structure of a model. It is a hierarchical structure, i.e., it includes different levels of abstraction. The higher the level of abstraction for a given hypothesis, the lower the possibility of having it "verified" or "rejected" through empirical evidence. A statement such as "In situations type A, positive reinforcement is more effective to promote the learning of behavior X than negative reinforcement" is far more testable than "All behavior can be positively reinforced."

Gurman *et al*.'s discussion about the empirical observations of distressed couples (6, p. 170) provides a more concrete example. The observation that " ... partners in distressed marriages behave quite differently (utilizing less positive and more aversive control strategies) when interacting with their spouses than when interacting with strangers" is interpreted within a behavioral epistemology to prove that "marital behavior is based (at least nearly entirely) on the immediate present behavior of the other spouse." Gurman *et al.* contend that the very same observation may "prove" the psychodynamic view of characterological deficits emerging more easily in intimate as opposed to superficial relationships. From a systemic perspective, the observation would illustrate the operation of different sets of *rules* within different systems.

At the highest level of a model's conceptual structure what we find are epistemological premises that cannot be proven true or untrue by "empirical observations"—precisely because those observations are punctuated and selected according to the premises. A classical example is the dualistic organization of human reality around the separate entities of body and mind. Once the separation was incorporated into Western thinking, different theories and hundreds of research efforts were dedicated to elaborate and test hypotheses about how body and mind *interact,* which one of them takes (causal) precedence over the other, and so on. Although all this activity resulted in the collection of a considerable amount of "empirical evidence" that could then be reviewed, compared and fought about, the basic epistemological assumption was not—could not—be challenged by the outcome. In pure methodological terms, such a premise is an untestable hypothesis.

"Testing" Epistemologies

The fact that epistemologies cannot be tested through empirical evidence by way of carefully planned experiments does not mean that they are *invulnerable.* Their vulnerability lies in their relative *effectiveness,* which in turn is decided by historical, sociocultural factors. An epistemology is efficient, i.e., allows for a satisfactory account and handling of "reality," within a certain sociocultural context and censes to be so under a different context.

One example of this is to be found in the origins and development of psychoanalysis. When Freud started his practice as a neurologist, the current medical epistemology required that an "illness" be traceable to "organic" causes in order to be considered as such. A subderivation of the body-mind epistemology was the premise that paralyses, for instance, were the product either of uncontrollable organic dysfunctions or of conscious willful decision. Hysterical paralyses, then, were "read" as simulation and left out of the reach of scientific medicine. (An alternative subderi-vation of the same body-mind epistemology was the older understanding of hysterics as possessed).

Freud's conviction that hysterics were *not* simulators required a new epistemology, and in fact it led to the "discovery" of the unconscious as a source of uncontrollable yet not organic causes of illness. Although Freud did not challenge the basic body-mind epistemology, he did offer an alternative epistemological subderivation.

The acceptance of psychoanalytic epistemology as a way of understanding and modifying human behavior evolved from initial rejection to a sort of imperialistic domination that only in recent years has begun to be challenged in the field of mental health. The reasons for such a lasting success are to be looked for in an area of reality that greatly transcends the limits of both the experimental laboratory and the clinician's office. A possible but not unique explanation is the (socioculturally relative) fitness of the psychoanalytic epistemology to a world that, as the century progressed, gave increasing signs of irrationality. Whatever the explanations, they are not to be found in the persuasive effect of "empirical evidence" collected under controlled situations. Freud's "demonstration" of the existence and "causal" relevance of the unconscious (the phenomenon of posthypnotic suggestion "proving" that an individual may do things without being aware of the reasons) did not shake the convictions of early opponents, no matter how intense their commitment to empirical observation was.

Just as psychoanalysis appeared to be successful in accounting for vast areas of problems that had not previously been considered as such—and for which, then, no need for conceptualization was seen—its epistemology is now in turn being attacked as insufficient to deal with a different set of problems. A typical example of this questioning is the contention that psychoanalysis fails to account for social factors. Social factors, of course, are not a new reality—they were always there—but rather a way of understanding "reality," or "problems" and "solutions," that has been gaining increasing influence among political leaders, administrators, professionals, the media, and the common person. Once more, it is against this perception of reality, rather than against an accumulation of "objective" empirical evidence, that psychoanalysis has been and is being "tested."[2]

Conflicts between models, then, are not resolved through carefully designed observations as much as they are through sociocultural evolution. This is, of course, particularly true for models with a high degree of direct impact on social life, as is the case with models of psychotherapy. In this field, alternative models reflecting different epistemologies are aimed at solving different problems—as defined from the model. It is the consistency of these (epistemologically defined) "problems" with the (historically relative) sociocultural definition of where and what the problems are that decides the fate of a given epistemology.

A concrete situation that is familiar to clinicians working in institutions will further illustrate this point. Current requirements for medical records include the need for information on historical background to be included in the initial evaluation report. This requirement, deriving as it does from an epistemology of "history as relevant cause," poses specific restrictions on the operation of the therapist during the initial evaluation and is in contradiction, for instance, with the systemic epistemology of family therapists who see their first encounter with a family as the opportunity to assess the structure of transactions. Although an experienced practitioner can eventually "explore the history" without hampering the evaluation of current transactions, such an integration is practically impossible for most beginning and intermediate therapists. Meyerstein (12, p. 491), in a related context, has pointed to the constrictions that record standards pose on the training of junior practitioners. Sociocultural factors act here through the implementation of a certain policy regulating the standards to which practice must conform. The issue of the relative institutional power held by sustainers of different epistemologies becomes decisive.

As anthropology has taught us, "problems" and "solutions," "pathology" and "therapeutic goals" are socioculturally relative concepts. The hysteria example shows that a problem does not exist as such, and a solution is not attempted, unless the culture makes room for them. Statements on problems and solutions are ultimately *value* statements. In the early years of psychoanalysis in my country, Argentina, the psychoanalytic subculture was mainly concerned with the "problem" of *inhibition.* The Freudian injunction, "*Lieben und arbeiten,*" was construed to mean maximum freedom from "neurotic" inhibitions. The subculture, as expressed for instance in (separate) parties of psychoanalysts and their patients, placed a high value on having fun, and social talk consisted to a great extent of boasting about sexual accomplishments and making money. When a second generation of psychoanalysts embraced the more British epistemology of the Kleinian model, with its emphasis on the healthiness of the "depressive position" as opposed to the "paranoid" one, a new style of parties emerged with a more quiet mood. Dancing was replaced by melancholic and wise dialogues about deeply insightful elaboration of painful realities. Among couples, expressions like "We cried a lot, and it did us a lot of good" took the place of "We have a terrific sex life." A sociologist would probably be able to correlate the developments in practice and conceptualization with the vicissitudes in the socioeconomic expectations and realities of society as a whole—or of consumers of therapy as a special group. Anyway, an instructive point in this somewhat trivial example is that the same kind of behavior may be considered a solution (freedom from inhibition) or a problem (manic denial) depending upon one's epistemology.

Of course, the relation between sociocultural values and therapeutic epistemologies is not a one-way linear process, as attested to by the pervasive effect of psychoanalysis in shaping Western twentieth century values. In fact, Bateson's model for the evolution of contexts (1, p. 155) may be applied to a further understanding of the evolution of psychoanalysis. The achievement of social success by this theory was accompanied by a generalization of its epistemology of "problems," "causes," and "solutions." Originating in a rather circumscribed set of clinical situations, the model expanded to a general understanding and treatment of human behavior. One of the implications of this development was that eventually an enormous number of situations—more than the psychoanalytic practice could encompass—had been culturally defined as "pathological" and in need of "treatment." Issues of time, money, and availability of well-trained practitioners became critical. In other words, the epistemology was increasingly insufficient to cope with the problems that *it had helped to define.* Group- and brief-therapy emerged largely in the context of this paradox, whose evolutionary nature is further underlined by the fact that long, "interminable" analyses have been mainly a post-Freudian development.

Family therapy is another attempt to deal with the self-promoted limitations of the psychoanalytic epistemology (or, for that matter, of any individual-oriented epistemology). It was precisely the extended and persistent implementation of psychoanalytic premises that helped to "discover" the *problem* of family influence on individual pathology and therapy. Sixty years ago, someone who cannot be suspected of an antipsychoanalytic bias complained about a kind of trouble that "presents a gloomy prospect for the effectiveness of psychoanalysis analysis as a therapy":

In psychoanalytic treatments the intervention of relatives is a positive danger and a danger one does not know how to meet. One is armed against the patient's internal resistances, which one knows are inevitable, but how can one ward off these external resistances? No kind of explanations make any impression on the patient's relatives; they cannot be induced to keep at a distance from the whole business, and one cannot make common cause with them because of the risk of losing the patient. ... [One finds that] the patient's closest relatives sometimes betray less interest in his recovering than in his remaining as he is. When, as so often, the neurosis is related to conflicts between members of a family, the healthy party All not hesitate long in choosing between his own interest and the sick party's recovery [Freud, 3, p. 459].

Expansion vs. "Revolution"

The insufficiency of individual-oriented epistemologies when applied to the understanding of multi-individual situations has resulted in two different types of attempts to account for the newly "discovered" reality. The first type is the *expansion* of the individualistic model, typically through *combinations* of individual explanations.[3]

Consider, for instance, Gurman and Knudson's account of a "contract" in a couple: "A necessary element in the process of *projective identification* is the unconscious *collusion of the second person*, whereby the object of the projections does not disown them but acts upon the message" (4, pp. 126-7; italics added). The reality of the couple is here understood as a combination of A's projective identification and B's collusion (where "collusion"—in fact a "dyadic" concept—is redefined in individualistic terms and predicated on "the second person").

A similar expansion is attempted through behavioristic propositions, such as "aversive stimulation in interpersonal relationships tends to *produce* reciprocal behavior in the second person," complemented by the understanding that "marital conflict *results* from the use of aversive control tactics" (4, p. 122; italics added), where each individual behavior is a function of the other individual behavior, and the reality of the couple—in this case a pattern of conflict—is accounted for as a *combination* of S-R equations. In the specific case of Jacobson and Weiss' behavioristic model, the expansion modality is reflected in their assessment instruments, with their emphasis on obtaining "intrinsic" interactional values ("pleasing," "displeasing," "problemsolving," "positive," "facilitating") for *each individual behavior being observed.* (Whether the judgment is passed by a trained observer or by the other member of the couple is irrelevant in this context, the main point being that the assessment of the pattern is broken into mini-assessments of individual behaviors.) It may be true, as Jacobson and Weiss contend, that the relationship is their patient—but the patient seems to be suffering from *individual pathologies.*

In the expansion type of attempt, then, explanation of the couple's functioning is *reduced* to the individual level in a process that is structurally identical to the explanation of "life" as a *combination* of chemical phenomena.

The second type of attempt in conceptualizing multi-individual events is provided by *systemic models.* From this point of view, a couple is defined as a system, and its patterns system's of interaction explained as a function of the system's *rules.* Rather than conceptualizing a system as a combination of individuals, the systemic approach goes the other way around and conceptualizes individuals as components of the system. Rather than postulating individual causes for systemic effects (even if the causes are meant to be reciprocal, or interactive in any other way), the systemic model postulates homeostatic functions, equifinality, balanced and unbalanced states, etc. Even "circular causality"—if used to describe a cycle of interactions within the system—is *to be explained* in terms of the homeostatic function that it serves; and, in fact, the spiral, rather than the circle, is an adequate image for describing many systemic processes, as in the case of *escalations.*[4]

Thus, once a level of phenomena is delimited (e.g., "interaction") the systemic approach will look for explanations at a higher level ("rules governing interaction," "systemic constraints on interaction") rather than reducing the phenomenon to its "molecular" (individual) components.[5] In this sense, the systemic approach is *revolutionary* in relation to the preexisting, individualistic one.

Conceptualization of change naturally follows explanation. The whole discussion around the comparative desirability and feasibility of "fair contracts for change" (Jacobson and Weiss) as opposed to an "unconditional commitment to change" (Gurman *et al.*) is based on the shared assumption that changes at the level of the couple will come from a juxtaposition of individual changes. The common question that both groups of authors answer differently is how to motivate the individuals to change so that the "resulting" interaction is also modified.

If a couple's problem is "reduced" to a combination of intrapsychic processes, change will be predicted on the correction of the distorting effect that these processes have on perception and behavior. Combined interpretations of the respective behaviors, complemented with an explanation of how the two intrapsychic realities fit each other, will be called for, and simultaneous *insights* will, it is hoped, result. If, on the other hand, the couple's problem is "reduced" to improper learning, a consistent approach will be "to provide couples in distress with various component *skills* in accomplishing marital tasks" (10, p. 152; italics added), the function of the therapist being to *train* the couple in abilities that are lacking.

But if, on the contrary, the problem is conceptualized in a systemic way, no reduction to individual intrapsychic or learning problems is necessary (or allowed). The systemic rules that govern the interaction are the problem. The discussion whether changes in feelings are a prerequisite for changes in behavior, as psychodynamists claim, or if the opposite behavioristic view is true, turns out to be largely irrelevant. Haley (8) exemplifies this with a situation in which the rules call for someone to take the "responsible" part and the other to be "irresponsible." Should the positions be reversed (with the implication, we could add, of substantive changes in the participants' "behaviors" and "feelings"), the basic *rule* would however remain unchallenged (p. 158). As Protinsky (16) pointed out, "The cognitive and behavioral sequence serves the function of maintaining the present structure of the system. That is, there is some systems purpose for these cognitions and behaviors" (p. 87).

Within a systemic view, therapeutic interventions will aim at removing those systemic constraints. Rather than detouring via individual change, the therapist will move directly toward "changing the rules of the game." Blocking, unbalancing, creating boundaries, establishing therapeutic alliances—as described by Minuchin (13)—are examples of systems-oriented interventions, as opposed to behavioral training and psychodynamic interpretation.

The differences in the positioning and activity of the therapist are the most critical practical implication of epistemological affiliations. While a behavioristic model tends to position the therapist as a neutral technician—who eventually increases distance from patients by putting questionnaires in the way of the relationship—the psycho-dynamic one has a tendency to value the therapist's personal input as important. But even then the prescription is either to function as an impartial mirror[6] (in order to control an undesirable variable), or, on the contrary, to invest one's own *personality* in the therapeutic relationship (in order to make full use of an unavoidable variable)—which merely adds a new individual line of causality to the analysis. A systemic epistemology, on the other hand, focuses on the different *positions* that are available to the therapist within the therapeutic system. Thus, while the behavioristic perspective imagines the therapist as a peripheral trainer, and the psychodynamic one places him in the center of a star-like structure—from which interpretations are provided with more or less of a personal touch—the systemic perspective positions the therapist as an active and mobile participant in the system, the structure of which he is trying to modify through structurally relevant interventions.

Further analysis of the above examples indicates that there are at least three different *contexts* involved: the individual, the dyad and the *triad*. While individual-oriented epistemologies explain the more complex by the less complex, systemic epistemology goes the other way. Thus, from a systemic perspective the description of a dyadic pattern of interaction may provide enough context to understand the individual behaviors of A and B, but an understanding of the *pattern* itself requires a move to a higher-level context. *Systemic rules accounting for the couple's interaction are ideally predicated on a broader system than the couple itself.*[7] Some examples from the Gurman *et al.* and Jacobson and Weiss articles will help to illustrate this issue.

1. The statement that " ... marriage is predicated on the relative advantage of relatedness over nonrelatedness" (10, p. 152) implies that reasons for staying together or splitting are self-contained (in the individual motivations of the partners)—a consistent statement with the exchange-theory model as quoted by Gurman *et al.* (4: "...social behavior in a given relationship is maintained by a high ratio of rewards to costs" (p. 122). Extra-couple constraints, i.e., those set by the broader system in which the couple is a subsystem (for instance, but not only, the family unit), are not included in the explanation. To conceptualize the *dysfunctionality* of a couple as a *function* of family-system constraints requires a systemic reading that is not available within an individualistic epistemology. From a systemic point of view, to quote Haley again, a relationship between two is predicated on the exclusion of a third. "If one is thinking in units of three people, a marriage does not exist as an independent unit" (8, p. 151).

In an actual case (7), a daughter was seen to have the position of "keeping parents together." Whenever she moved toward independence, the parental couple would fall into "distressed interaction." At one point the couple announced that they were considering separation. A systemic assessment of this move pointed to the homeostatic implication that, the girl was now to become "closer" in order to save the couple. The therapist then *blocked* the separation by indicating that it was necessary for the couple to stay together until treatment was over—thus making unnecessary the "rescue" maneuver by the girl. In this case, the couple's move toward separation was not read in terms of relatedness being less advantageous than nonrelatedness, but in terms of its meaning for the homeostatic dance of the whole family system.

2. "The behavioral view of distress," say Jacobson and Weiss (10, p. 154), "states that a couple's attempt to bring the exchange of benefits into alignment has shifted to reliance on aversive control procedures." Because, as pointed out before, the reasons for this unfortunate development are traced back to the individuals' shared "lack of interactional skills," no effort is made—nor can be made—to understand the *context* under which the couple makes such an attempt or shifts into such a pattern. The possibility of a child moving out of the home and thus ceasing to function as a detouring resource—which had until then prevented the couple's conflict from becoming open—is not conceivable outside of a systemic perspective.

3. Gurman *et al.*'s discussion of the concept of conflict is also illustrative of disregard for context. In questioning the behavioristic definition of conflict, they espouse the idea that conflict may exist even if none of its "usual signs" are present. But then, in order to cope with the dilemma of a conflict that does not appear as such, they have to introduce the concept of "simultaneous existence of multiple levels *of the relationship"* (4, p. 129; italics added). A systems approach, while consistent with the notion of a multi-leveled reality, is in a more comfortable position to infer "hidden" conflicts, because it does not look just at the couple's relationship but at a broader context. Under these conditions of observation, *triangulation* of a child (or for that matter, of the therapist) in the form of opposing parental demands provides the indication of a couple's conflict, even if (as usually occurs) they don't argue overtly. A common experience among family therapists is the sequence in which father says, "Sit still," and mother adds, "Would you like to play with the blocks?."

The Relativity of "Outcome"

It is no doubt obvious from the previous discussion that my own affiliation is with systems theory. Certainly there are not enough outcome studies to support this affiliation. But as indicated by the above discussion on the relative value of "empirical evidence," no affiliation can be really supported through those studies. In order to hope that carefully designed outcome studies will eventually give an answer to the question of "which model is best," at least four assumptions are needed:

1. That outcome studies will eventually become methodologically unquestionable, i.e., no question will possibly be raised as to the adequateness of operational definitions and control of variables, and the two basic interrogations of "What do you mean?" and "How do you know it?" will be unequivocally and satisfactorily answered.
2. That either a convincingly crucial experiment will be designed or revisions of research literature will become as unquestionable as the studies themselves, so that no question will remain as to the conclusiveness of the studies.
3. That the validity of outcome criteria (such as, for instance "talk-time equality," "spontaneous agreement," "rated helpfulness") in measuring a condition such as "a smooth relationship," for instance, will be some day established beyond doubt.
4. That the condition measured by the criteria (in our example "a smooth relationship") will be consensually *valued* as a "solution" to the "problem."

Now the actual accomplishment of these assumptions faces trouble that runs from extreme difficulty to impossibility. Consider, for instance, the issue of control of variables when assessing the effectiveness of the behavioristic "communication-skill training." From a systemic perspective, several questions can be raised. What is the *reframing* impact on the couple of an approach that "blames the problem" on inadequate training? If this definition of the problem is—as Gurman and Knudson contend—more consistent with the "rational side" of the couple, what is the impact on homeostasis of such "siding" by the therapist? What is the effect on the system of a *therapist-sanctioned* specific technology for the solution of conflict? Does the fact that penalties for not fulfilling contracts are an "aversive stimulus" prescribed by the therapist make a difference as compared with aversive stimuli spontaneously interchanged before therapy? Gurman *et al.* may be right in suggesting that, insofar as behavioral therapists are doing more than the model prescribes, the reason for their success could be ascribed to the nonspecific variables; and, then again, this may *not* be the case.

Even granting that the control of variables and other methodological troubles listed under assumptions (1 and 2) are theoretically bound to an eventual solution, and leaving aside the seemingly infinite impracticality of such a solution, assumptions (3 and 4) ask for too much in the way of erasing epistemological differences; in particular, the possibility of achieving consensus around values is in conflict with our pluralistic society. Gurman and Knudson's (4) distress about a "furniture-for-sex" kind of contract (p. 126) and their questioning of "adulthood" as a sole criterion for health (p. 127), as well as Jacobson and Weiss' questioning whether "repression" is good or bad (10, p. 151) are typical examples of value judgments about outcome. Here again, as in the example of the two psychoanalytic subcultures, what is considered a "solution" from one position turns out to be a "problem" for the other.

Thus, claims of success for a particular therapy are bound to be attacked from an epistemology different from that of the claimers. As Jacobson and Weiss correctly say, "to repudiate one ideology with another is largely polemical" (10, p. 160). But apparently they fail to understand that ideologic (epistemologic) polemics—as opposed to "empirical proof"—are unavoidable when it comes to discussing the issue of effectiveness. The answer to their crucial question, "Are behavior therapists in any way crippled clinically as a result of this emphasis on observables?" depends on *value* judgments about what a "good" outcome is. Their hope for "empirical evidence" as the final judge is inconsistent with their acknowledgment that "models of marriage therapy can be distinguished by their ideological (value) statements as well as by their choice of substantive (empirically relevant) concepts" (10, p. 150). The reason for this inconsistency seems to be their failure to realize how ideological statements guide the choice of "substantive concepts." Thus, if it be true that evaluating therapy models along "substantive dimensions" is, as Jacobson and Weiss contend, "more useful than an ideological critique" (10, p. 151), the implication is that a useful evaluation will be that which accepts the boundaries of the epistemology whose outcome is being assessed. But then such an evaluation will not, as Jacobson and Weiss hope, "facilitate an integration of various models."

"Integration" and Eclecticism

The feasibility of integrating different models—an underlying theme in the Gurman *et al.* vs. Jacobson and Weiss polemic and an explicit recommendation in the case of the first group of authors—needs to be evaluated from the point of view of "epistemological conflict," as exposed earlier in the present article. A *real* integration between models—the simultaneous acceptance of the *complete* models, epistemological assumptions included—is impossible if the models are epistemologically conflicting, unless a third, new epistemology is developed in order to decide when each one of the now subordinated models is to be applied. Short of this, what usually goes under the name of "integration" is that one model—or more specifically some isolated concepts or techniques—is subsumed under a second, prevailing one. Thus, Gurman and

Knudson's "Psychodynamic-Systems Analysis and Critique" of behavioral marriage therapy (4) adopts in fact the form of subordinating systemic concepts to a basically psychodynamic approach, as reflected both in the differential weights assigned to them in the argument and in the definition of systemic realities as intrapsychic combinations. The behaviorist's acceptance of nonbehavioral concepts *if* and to the extent that they can be "redefined" in behavioral terms and Minuchin *et al*.'s inclusion of behavioral techniques within their systemic model for the treatment of anorexia nervosa provide other examples.

In cases like these, the function assigned to the foreign concept within the espoused theoretical framework (or to the technique within the espoused therapeutic context) is modified in accordance with the prevailing model. Thus, it is incorrect to assume that the use of certain specific techniques unequivocally characterizes a model; for instance, Gurman and Knudson's statement (4, p. 125) that a "task-oriented, data-oriented, and ahistorical approach" is necessarily correlated with assumptions of rationality, dysfunction awareness, nonresistance and commitment to therapy does not take into account the fact that these assumptions are not present—rather the opposite is true—in other task-oriented, data-oriented and ahistorical approaches, such as Haley's (8). The difference is not in the use of any specific techniques, but in their function within a different framework; in Haley's case, tasks, for instance, are not prescribed with a "skill-training" model in mind, but rather within a systemic model in which relevance is attached to the interactional processes triggered by the therapist's prescription and the success or lack of success in "learning" the task is conceptualized as irrelevant per se.

This incorporation and redefinition of concepts and techniques from one model into a different one is to be discriminated from the eclectic attempts to keep affiliation with two or more (epistemologically) incompatible models. Gurman and Knudson, for instance, suggest that a behavioral therapist should be able to shift to an "alternative strategy" if a couple "will not 'buy' a behavioral approach" rather than refer them "to a therapist of a theoretical orientation that offers a better 'fit' with the *values and perspectives* of the couple" (4, p. 132). The implication is that therapists not only "should be able to intervene flexibly and in varied ways with different clients" (something that can be done within the limits of a given model), but rather to shift from one epistemology to another, from one model to another, and indeed from one set of values to another in order to accommodate to the clients' "values and perspectives.rdquo;

Such a position, based as it is on the myth of the axiologically neutral therapist, poses at least as many ethical dilemmas as the ones it tries to solve. Because some of the "values and perspectives" of the couple may be part of the "problem," who is to decide which of them should be changed and which of them should dictate a change of model? After reading Gurman and Knudson's attack on the ethics of "mutual control," one finds it hard to believe that a couple committed to those ethics could possibly obtain from Gurman and Knudson the prescription of a reinforcement schedule just because "it fits the couple.rdquo;

Client "fitness" to a particular approach is not an absolute concept, but one that is relative to a therapist's conceptualization. Consider, for instance, the case of a primarily psychodynamic therapist who is "open" to a shift to a nonpsychodynamic model if clients are too resistant to interpretation. Depending upon how strongly convinced the therapist is about the value of "resistance" as both an explanatory ("this couple *is resisting* change") and therapeutic ("change will require working *on* the resistance") concept, he will be able to stand *more or less* frustration before shifting to a nonpsychodynamic model. Just as stronger affiliations lead to fewer occurrences of eclecticism, the reverse is also true, inasmuch as models grow as a function of consistent and persistent application.

Which one of the two situations is more helpful for the clients is again a matter of value judgment. While some will argue that accommodating to the clients' "values and perspectives" is being respectful of their individuality, others will claim that it is disrespectful of their right to outgrow wrong, pathogenic values or perspectives. While some will argue that dogmatic therapists try to impose their own ideas, others will answer that eclectic therapists are fostering erratic changes or no change at all.[8]

In the specific case of the psychodynamic and the systemc models—associated by Gurman and Knudson in the title of their article (4)—we have seen that their incompatible epistemologies about the definitions and "explanations" of problems lead to equally incompatible therapeutic strategies and interventions. An attempt to think psychodynamically *and* systemically at the same time, to see the locus of pathology both in the intrapsychic and in the system's rules and to look for explanations both in deep-rooted introjects and in current systemic dynamics can only lead to confusion. An "alternation" pattern seems theoretically possible, but then a "second-order" model will be needed whose function will be to prescribe the differential applicability of the two models in specific situations. Such a model—if possible at all—has not yet been explicitly stated, though certainly there must be idiosyncratic, implicit models operating behind the strategic decisions of eclectic therapists.

Conclusion

Given the axiological pluralism that characterizes the current sociocultural scene, it is predictable that parallel and opposite therapeutic epistemologies will continue to survive and grow. Psychodynamic, behavioristic, and systemic models reflect the coexisting and largely competing values of full development of personality potential, continuous technological advance, and ecology-wise decision-making. In point of fact, a systemic understanding of cultural evolution leads one to predict that at any point in time there will be at least two competing epistemologies, each one supported by its (own way of collecting) "empirical evidence."

Discussions around the issue of *control* are most relevant in this context. Behavioristic "contracts" are an expression of an ideology that advocates positive mutual control as preferable to the negative kind. Gurman *et al.* (6) challenge the very definition of the problem: "An alternative that seems to be missed by behavioral therapists is the possibility of a noncontrolled and noncontrolling relationship" (p. 169), and they go on to say that "in healthy intimate relationships, people *should* behave in ways that are *not* controlled, whether positively or negatively, by the desires and expectations of others" (italics in the original). If the behaviorist's enthusiasm about the "scientific" control of behavior may sometimes raise the ghost of a "Brave New World," a proposition such as Gurman *et al.*'s suggests the anarchic implications of ultraliberalism—although the authors are careful to restrict this to "intimate relationships." The following statement is particularly significant: "We hope that our interventions and implicit philosophy *lead to a belief* in our patients that they have the ability and the right to be who they are and to do what they want (responsibly) in their intimate relationships" (p. 169; italics added). Without the parenthetical "responsibility," the implementation of this philosophy would lead to the jungle law of the survival of the stronger; by inserting the qualification, though, the authors reinstate control, as responsibility cannot be defined without reference to "the desires and expectations of others"—if not a spouse, in any case somebody (the therapist? society?) who would then be controlling the "intimate relationship" from the outside. Note that by acknowledging that they try to *lead* their patients to *believe* (a more naïve or less honest therapist would have said "help to discover"), Gurman *et al.* do in fact reintroduce the control issue.

By contrast, a systemic perspective cannot think of an "uncontrolled" alternative. Systems are controlled by definition—their rules constrain the behavior of their members. For the systems thinker, to deny control as a necessary ingredient of human reality is as naïve as the behaviorist's denial of "resistance" from the psychodynamist's point ot view. But again, his understanding of "control" differs from the behaviorist's. Control will be seen as "negative" for instance, not when husband and wife interchange "aversive stimuli," but when the systemic rules governing their behavior prevent their mutual satisfaction and individual fulfillment. If a wife indulges in more sex than she would like and her husband in less sex than he would like, the underlying rule may be one of protective overconcern for each other; and particularly if a psychosomatic child is at hand, no interchange of "aversive stimuli" will be recorded by the finest behavioristic instrument. In a situation like this, a systemic therapist may focus on strengthening boundaries around the couple, blocking attempts to detour through other members of the family, and in general on facilitating—via directives, tasks, and enactments in the session—the development of new rules. The epistemological assumptions will resemble those of the ecologist's strategy for "recovering" a polluted environment.

Psychotherapists are constructors of reality (15). Construction materials and techniques may be tested—to the extent that they are more or less effective in building a certain reality. But the reality to be constructed is a matter of sociocultural value. Therapists should not expect that outcome studies will spare them the responsibility of taking an epistemological and ultimately axiological stand as to which problems are to be faced, how they are to be approached, and in what direction the solution is to be attempted.

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[1]This, by the way, is structurally identical to Gurman *et al.*'s observation that the meaning of A's behavior is not absolute but dependent upon B's perception (4, p. 129), which makes their assumption of the absolute value of empirical evidence particularly inconsistent.

[2]Levenson (11) offers a lucid examination—from the point of view of French structuralism—of the sociocultural impact on psychoanalysis.

[3]I have criticized elsewhere (2) a more gross extrapolation of individualistic concepts, namely the attribution of a "psyche"—complete with ego, superego, and id—to a group, as performed in some group-therapy models.

[4]It is interesting to note that individual models that postulate asystemic explanations for multi-individual events may be more "systemic" when applied to their original target. Thus, Freud's individual is regulated in his or her functioning by such a "homeostatic" concept as the principle of constancy.

[5]The very term "inter-action," by the way, carries a reductionistic epistemology.

[6]In this context, Gurman *et al.*'s contention that "'intrapsychic' therapies focus fundamentally on *dyadic* interaction and have since Freud identified 'transference'" (6, p. 167; italics in the original) is a bit puzzling, inasmuch as "transference" happens to be one of the most blatantly individualistic concepts in psychoanalysis—denying as it does any "personal" influence of the psychoanalyst on the patient's attitudes toward him and attributing those attitudes entirely to intrapsychic causes. So much so, that psychoanalysis itself eventually needed the parallel concept of *countertransference*—which again is in line with a definition of dyads as a juxtaposition of two individuals.

[7]Haley (9, 8) has lucidly described his own transition from a dyadic to a triadic understanding of couples—a result of his consistent commitment to systems thinking.

[8]In this context, it is important to note that eclecticism per se is not a warranty of "flexibility"—still another value. An eclectic therapist may be organized around rigid categories of "what-when-for what" and in fact become more harnessed than a "purist" colleague.