INTEGRATING PSYCHOTHERAPY RESEARCH
AND PRACTICE: MODELING THE CHANGE PROCESS

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Despite the fact that leading psychotherapy researchers have persistently endorsed the use of intensive analysis procedures over the years, the field has been slow to act upon these recommendations. This article identifies a number of factors responsible for this slowness: the uncritical utilization of conventional research methodologies, the failure to distinguish between different types of intensive analysis designs, the assumption that intensive analysis is necessarily unscientific, and the fact that the behavioral single-case experimental design are inappropriate for studying other forms of therapy. The task-analysis approach to psychotherapy research is presented as a methodologically rigorous alternative to the behavioral single-case experimental design.

There has been a long history of theorists and researchers in the psychotherapy field commenting on the inadequacy of the extensive analysis research paradigm and recommending that it be either replaced or supplemented with intensive analysis procedures (Barlow, 1981; Barlow & Hersen, 1984; Bergin & Strupp, 1972; Chassan, 1967, 1979; Gottman & Markman, 1978; Greenberg, 1980; Kazdin, 1982; Kiesler, 1983; McCullogh, 1984a,b; Rice & Greenberg, 1984; Shapiro, 1966; Strupp & Hadley, 1979). A recent working conference on psychotherapy change process research convened by the National Institute of Mental Health unanimously endorsed the use of intensive analysis research methodology (Kiesler, 1983). Despite the persistent urging of leading voices in the psychotherapy research field that we replace or at least augment the conventional reliance upon extensive analysis with the use of intensive analysis procedures, researchers and granting agencies have been slow to absorb these recommendations. The purpose of this article is to identify some of the factors responsible for this gap between recommendations and practice and to clarify the fashion in which a particular form of intensive analysis—the task analytic approach to the study of change events (Greenberg, 1975, 1980, 1984; 1986a,b; Rice & Greenberg, 1974, 1984; Rice & Saperia, 1984)—can contribute to our understanding of psychotherapy process.

The extensive analysis research paradigm is the one which is most typical of psychological research. It is the research paradigm conventionally taught in graduate statistics and research design courses and the one which has for many people become the sine qua non of experimental design. The focus in the extensive analysis research design is on the performance of aggregates or groups of individuals rather than on the performances of individuals. The experimenter hypothesizes that the average subject will respond in a certain way in response to a specific experimental manipulation. He or she then tests this hypothesis by comparing...
the average performance of subjects in the experimental group with the average performance of subjects in the control group. In contrast to the extensive analysis research paradigm, the intensive analysis research paradigm studies the performance of individual subjects in detail, and then attempts to generalize to the performance of other subjects through multiple replications (Chassan, 1967, 1979). While the extensive analysis approach tests hypotheses with respect to groups of subjects, the intensive analysis approach tests hypotheses within patients.

**Limitations of the Extensive Analysis Paradigm**

The extensive analysis design has the merit of increasing generalizability of findings, since inferences can be made about the population of interest if the experimental sample is a representative one. It is important, however, to bear in mind that in this research paradigm the data represent the averaging of the performances of a group of subjects rather than the performance of any given subject. It is thus impossible to infer backward from the group performance to the performance of the individual (Bakan, 1967; Barlow, 1981; Chassan, 1979). The fact that the average subject behaves in a certain way thus does not guarantee that any one of the individual subjects within the group has actually performed in that fashion. This constitutes a substantial problem in the context of psychotherapy research.

The first concern here is that even though a given extensive analysis study demonstrates that a specific treatment is effective for the average subject, the clinician has no guarantee that the treatment will be effective for any particular patient. It is this concern that prompted Kiesler (1966), over two decades ago, to suggest that psychotherapy researchers conduct factorial studies in which patients are clustered into a number of homogeneous categories and then examine the interaction between different treatment interventions and different patient categories. While Kiesler's suggestion is certainly a methodological advance over approaches which subscribe to the uniformity myth that all patients are the same and will benefit equally from the same treatment (Kiesler, 1966), the logistics of assembling a sample large enough to represent a number of theoretically relevant homogeneous patient categories are virtually insurmountable.

However, even if they were not insurmountable the practice of averaging results over a number of patients creates another substantial obstacle to psychotherapy research. The extensive analysis approach, even if it incorporates a factorial design, limits the researcher to drawing inferences about effective interventions on the basis of *intersubject* variability. It does not allow the researcher to draw inferences on the basis of *intrasubject* variability.

Thus, as Barlow & Hersen (1984) point out, in the extensive analysis design:

If one depressed patient deteriorates during treatment while others improve or remain stable, it is difficult to speculate on reasons for this deterioration if the only data available are observations before and after treatment. It would be much to the advantage of the clinical researcher to have followed this one patient's course during treatment so that the beginning of deterioration could be pinpointed. In this hypothetical case the patient may have begun to improve until a point midway in treatment, and deterioration began. Perhaps a disruption in family life occurred or the patient missed a treatment session, while other patients whose improvement continued did not experience these events. It would then be possible to speculate on these or other factors that were correlated with such change. (p. 36)

In other words, an important source of information about the factors that mediate treatment efficacy is unavailable if our source of information is limited to intersubject variability data. The extensive analysis treatment outcome study operationalizes psychological treatments such as "psychoanalysis" or "cognitive therapy" or "interpersonal therapy" as if they were unchanged packages, and administers them to patients who are either heterogeneous or clustered together on the basis of traits which are presumed to be stable. The clinician however, is constantly monitoring variability within the patient he or she is treating, and modifying interventions in response to this variability (Barlow & Hersen, 1984; Rice & Greenberg, 1984).

This limits the relevance of the conventional extensive analysis outcome design to the clinician. As Barlow (1981) has remarked, "At present, clinical research has little or no influence on clinical practice." This concern is borne out by the results of two recent surveys in which an extremely low rate of psychotherapy research utilization was reported by clinicians (Cohen et al., 1986; Morrow-Bradley & Elliot, 1986).

Dissatisfaction with conventional psychotherapy research paradigms is not new. Psychotherapy researchers have consistently lamented the irre-
The automatic equation by many psychologists of the scientific method with aggregate designs and inferential statistics fails to take into account the fact that some of the more important advances in neurophysiology and the so called "harder" areas of psychology (i.e., learning and cognitive psychology) have come from the intensive observation of single cases (Dukes, 1965). Barlow & Hersen (1984) point out that the seminal works of Broca on brain physiology, Ebbinghaus on verbal learning, and Pavlov on classical conditioning were all based on single case observations. In the fields of cognitive and cognitive developmental psychology researchers such as Luria (1968), Piaget (1954), and Newell & Simon (1972) (whose work we will discuss in greater detail later) have made important contributions, using intensive analysis approaches.

Another problem, however, has been the absence of clear-cut alternatives or guidelines for methodologically rigorous intensive analysis in psychotherapy research. Researchers often equate intensive analysis research with the uncontrolled case study. It is possible, however, to design intensive analysis research which is methodologically rigorous. An example of conducting methodologically rigorous intensive analysis research can be found in the behavioral literature on single-case experimental design. In the next section, we will evaluate the relevance of this literature to psychotherapy research in general.

A Critique of the Operant Single-Case Experimental Design

A major alternative to the extensive analysis approach to psychotherapy research is the operant approach to single-case design. Researchers, for example, Barlow (1981), Barlow & Hersen (1984), and Kazdin (1982), have recently argued eloquently in favor of intensive analysis research and have done an excellent job of discussing the type of single-case experimental designs which have emerged from the operant paradigm. There are, however, a number of assumptions basic to the operant approach to single-case experimental design which are inadequate, when psychotherapy is conceptualized from a nonbehavioral perspective. McCullough (1984b) has outlined four such assumptions. They are as follows: 1) the independent variable(s) and dependent variable(s) must be observable stimulus events occurring contiguously with one another; 2) in order to demonstrate that treatment effect has occurred, the independent variable must produce rapid changes in the dependent variable; 3) dependent variables must be demonstrably influenced by the presence or absence of the controlling stimulus; and 4) the dependent variable must show stability before a treatment is introduced. In this section we will amplify upon the concerns of McCullough (1984b) that we believe to be of greatest significance and discuss some additional ones that we believe to be important.

A first problematic assumption is that the important changes which take place when an effective intervention is administered will decrease or disappear when the intervention is withdrawn. This assumption is basic to both withdrawal designs and reversal designs. Although this assumption may make sense when we assume that the important changes are behavioral in nature, it makes less sense if one assumes that the relevant changes are more complex patterned psychological processes rather than simple frequency counts. For example, once a patient in cognitive therapy has developed an ability to monitor his or her automatic
thoughts, this ability will not necessarily disappear if the therapist no longer works with the patient to monitor automatic thoughts in the session. Another example is that once an adequate therapeutic alliance is developed between the therapist and the patient, the patient is likely to be more trusting and open with the therapist than he or she was before the alliance had developed. An alteration in the conditions which facilitated the development of the alliance in the first place would therefore not immediately result in a loss of trust by the patient. Thus, more subtle psychological variables are not controlled by the presence or absence of a specific independent variable in the same fashion that behavioral operants are.

A second assumption is that an intervention can be administered, withdrawn, and then readministered, without introducing confounding effects. This assumption is basic to the ABAB withdrawal design. However, this assumes that the dependent variable of interest is a simple behavioral operant. Consider the following example. If the intervention is active empathy and the dependent variable is an increase in Experiencing level (Klein et al., 1969) the first withdrawal of the intervention may engender a guardedness in the patient which will not be overcome by its reintroduction.

A third assumption is that the introduction of an intervention will result in a fairly rapid increase or decrease in the intensity of frequency of the relevant behavior. As McCullough (1984b) has argued, this assumption, while adequate in the domain of operant research, is inadequate when investigating psychological processes, such as attitudes or beliefs, which may change more slowly.

A fourth assumption is that interventions do not have compounding, interactive effects. This assumption is basic to reversal designs. Consider the following example. An interpretation delivered in the context of an empathic intervention which preceded it may have a different impact than one delivered outside this context. It is thus impossible to evaluate the effects of interventions independently.

A final assumption is that behaviors which are subject to therapeutic change are independent of one another and are not interconnected by some mediating process. This assumption is fundamental to the other major class of behavioral single-case experimental designs, that is, the multiple baseline study. In this design it is assumed that the causal efficacy of an intervention can be evaluated by applying it to one behavior and then monitoring both the relevant behavior and other behaviors. If only the targeted behavior changes, then causal efficacy is attributed to the intervention.

However, any mediational theory of psychotherapy change assumes that behaviors are in fact interconnected through mediating cognitive processes. Indeed, psychodynamic therapies are explicit about the fact that problem behaviors are merely symptoms of underlying psychological conflicts. The resolution of the underlying psychological conflict may thus result in a change in a number of symptoms. Even in cognitive behavioral theory it is assumed that cognitive processes mediate behavioral change. Bandura's self-efficacy theory, for example, would predict that a change in one problem behavior might result in a change in another problem behavior through enhancing the patient’s perception of his or her own self-efficacy. Thus, the basic assumption that is essential to the logic of the multiple baseline design is inadequate from the perspective of anything but the most behavioral of theories.

In sum, intensive analysis designs are needed which do not have to make the overly restrictive operant \( n = 1 \) assumptions and which can deal more veridically with more complex mediational events.

Understanding the Psychotherapy Change Process

A major tenet underlying all the above assumptions is that the objective of the psychotherapy researcher is to achieve functional control of behavior. The assumption here is that this strategy will lead to the prediction and control of behavior, and that predictive power is the ultimate criterion of science. While it is true that prediction is an important goal of science, the behavioral and logical positivist emphasis on prediction fails to recognize the central roles that explanation and understanding play in the “hard” sciences (Polanyi, 1958; Weimer, 1979).

But as Holt (1978) argues:

Actually, all of the highly developed sciences aim at prediction and control through understanding; the goal is compound and indivisible. Most scientists, as contrasted with technologists, are themselves more motivated by the need to figure things out, to develop good theories and workable models that make nature intelligible, and less concerned with the ultimate payoff, the applied benefits of prediction and control that understanding makes possible. But it is only in recent years that the dual nature of the scientific enterprise has been made clear by philosophers of science, the fact that it has hypothesis-forming and hypothesis-testing phases; moreover, methodologists have
The behavioral approach to single-case analysis is problematic in that 1) it makes a number of assumptions about change which are inadequate given a nonbehavioral meta-theory; and 2) it attempts to develop a science of psychotherapy by empirically establishing control and predictability, without undergoing the crucial stage of developing understanding. This type of understanding can only come from a more fine-grained analysis of psychotherapy process than is afforded by the behavioral single-case experimental design. This type of fine-grained analysis of therapy process should be able to provide the clinician with information that can actually be useful to him or her in therapy sessions. Conventional psychotherapy outcome research treats both psychotherapy interventions and psychotherapy patients as static entities. As any clinician knows, however, patients are living, human beings who change over the course of therapy and over the course of sessions. The skilled clinician modifies interventions in response to subtle changes in the patient’s psychological state. As Rice & Greenberg (1984) have argued, experienced clinicians appear to have implicit or intuitive maps which guide their interventions. They are constantly making “process diagnoses” on the basis of which they intervene. Rice & Greenberg use the term “when-then” performance events to capture the flavor of complex interactional sequences between patients and therapist in which specific therapist interventions are linked to specific patient process states on a moment-by-moment basis.

It is this type of subtle interactional sequencing that is not captured by traditional psychotherapy research paradigms, and that is difficult for the experienced clinician to convey to students, precisely because of the fact that this type of knowledge is often tacit in nature. As Polanyi (1966) maintains, it is the rendering explicit of this type of knowledge which is at the heart of true scientific endeavors. The recognition by advocates of the operant single-case design that it is important to capture intra-subject variability is an important one. However, it does not go far enough. The clinician requires information, not only about more molar sources of intra-subject variance which lead to success and failure but also about more molecular in-session changes in subtle psychological processes.

How does the psychodynamic therapist know when to make a transference interpretation and when not to? How does he or she know when to make a genetic transference interpretation and when to restrict the interpretive link to current extra-therapy situations? How does the cognitive therapist know when to challenge the patient’s cognitive distortions and when to explore the deeper underlying subjective meaning for the patient? How does the experiential therapist know when to promote the deepening of a specific emotional experience and when to bypass one feeling to explore an underlying feeling? Experienced clinicians know that apparently identical interventions can lead to very different outcomes depending on the context. This type of knowledge is often referred to as clinical wisdom and the associated skill is referred to as timing. The central premise of this article is that a vitally important task for psychotherapy research should be to systematically articulate this wisdom and operationalize the principles guiding the timing.

We hypothesize that there are a series of mediating shifts in patients’ psychological state (Horowitz, 1979) that form the context for the intervention and determine if it will be effective. The skilled clinician recognizes these shifts in patient state and is able to modify the intervention appropriately in response to them. Patients undergo a transition through these different operations or states, both within and across sessions, as they move through the process of change.

The process of therapy can thus be seen as a chain of patient states or suboutcomes that are linked together on the pathway toward ultimate outcome (Greenberg, 1982, 1986a; Pinsof, 1980). These suboutcomes can be conceptualized in relatively more molar or molecular terms. An important question to raise is: What is the best level of analysis at which to conceptualize therapy outcome? We believe that psychotherapy research should be conducted at the level that will be most useful to the clinician. The findings that high levels of experiencing are associated with change in client-centered therapy (Klein et al., 1986) or that changes in automatic thoughts are associated with change in cognitive therapy (Persons & Burns, 1985) are too molar to provide the clinician with the type of subtle “when-then” information that is required. This does not mean that more molar suboutcome findings are not important. This type of information is important for establishing an intermediate point of linkage between molecular in-session changes
and ultimate outcome (Safran et al., 1987).

The view of psychotherapy process research we are describing is similar in some ways to the stage process model (Cashdan, 1973) advocated by McCullough (1984a) and by Hudgins & Kiesler (1987), as an alternative to the operant single-case experimental design. In the stage process model:

Therapy process is structured so that the client is guided through a series of sequential stages, each of which contains its own operationalized therapist’s roles and clients’ performance goals. Each treatment stage builds upon and presupposes skills applied during earlier stages. Mastery of the stage goals means the patient is automatically moved into the next stage. Termination of treatment means that the client has mastered all the stage goals in the program. (McCullough, 1984a, p. 387)

While there are parallels between the stage process model and Rice & Greenberg’s (1984) approach, there are some important differences. The first is that the stage process model described above conceptualizes performance goals as rather gross, learned skills (e.g., learning a situational analysis technique). For Rice & Greenberg (1984) however, the performance goals or suboutcomes are shifts in more subtle psychological states.

A second difference is that the stage process model specifies the different stages of treatment in advance and then evaluates the impact of different interventions upon the criteria which indicate the completion of different stages. The psychotherapy process research paradigm proposed by Rice & Greenberg (1984), however, stipulates that the relevant stages or suboutcomes must be discovered empirically. This is precisely the type of information which we currently do not have and that a true science of psychotherapy might provide for us. A research methodology that could provide us with this information would in theory yield the type of understanding of psychotherapy process that could truly impact on the clinician’s practice.

What type of methodology could be employed to empirically map out these transitions in patient states which lead to positive ultimate outcome? A methodology is required that is able to assess patterns of intrasubject variability and that is sensitive enough to pick up subtle transitions in psychological states. Not only would it then have to use intensive analysis in order to pick up intrasubject variability but it would also require a “discovery” component (Reichenbach, 1951) to illuminate subtle patterns of change, which are not already apparent. A more conventional extensive analysis hypothesis-testing design would be inadequate for this type of task, since the mapping of subtle psychological change patterns is neither a hypothesis-testing nor an intersubject variability assessment task. There is nothing intrinsically unscientific about the task, however, and if a conventional extensive analysis hypothesis-testing design is not appropriate, it would seem that we require an unconventional design.

We have argued that although the behavioral approach to intensive analysis recognizes the importance of assessing intrasubject variability, many of the assumptions fundamental to this approach are inadequate to a more cognitively oriented perspective on psychotherapy. An alternative to the types of assumptions intrinsic to behavioral intensive analysis can be found in the cognitive sciences. In particular, the type of work that Pas- cual-Leone (1976a,b) has done on modeling children’s solutions of Piagetian tasks and the work that Newell & Simon (1972) and Ericsson & Simon (1984) have done on modeling human problem-solving activity, provides a useful methodological lead (cf. Greenberg, 1975, 1984). In the next section some of the assumptions that are basic to this type of approach will be discussed.

The Task Analysis of Problem-Solving Performance

Newell & Simon’s (1972) classic work on human problem-solving provides an excellent example of the fashion in which a research methodology can be both discovery oriented and based on the intensive analysis of individual cases, and yet scientifically respectable. They argue that the information-processing paradigm of the cognitive sciences lends itself naturally to the intensive analysis of single subject performances. According to Newell & Simon (1972), an information processing theory is not restricted to studying generalities about Man. With a model of an information processing system, it becomes meaningful to try to represent in some detail a particular man at work on a particular task. Such a representation is no metaphor, but a precise symbolic model on the basis of which pertinent specific aspects of the man’s problem solving behavior can be calculated. (p. 5)

The assumption here is that in order to model the cognitive performances of people engaging in problem-solving tasks it is essential to focus intensively on the individual. As they state at the outset of their book:

There is no lack of orientation towards the data of human behavior in the theory presented in this book. Yet we employ little experimental design using control groups of the sort so familiar in psychology. Because of the strong history-depen-
of the phenomena under study, the focus on the individual, and the fact that much goes on within a single problem-solving encounter, experiments of a classical sort are only rarely used. Instead, it becomes essential to get enough data about each individual subject to identify what information he has and how he is processing it. (p. 12)

As Rice & Greenberg (1984) have pointed out, the basic approach that Newell & Simon (1972) use, involves the intensive analysis of cognitive performance strategies of individuals engaging in problem solving in specific task environments. Through the analysis of individual verbal protocols and the qualitative inspection of similarities and differences between different protocols a general model of human problem solving in specific task environments is developed. The general approach employed by Newell & Simon is referred to as the task-analysis approach. In this approach, the researcher begins by hypothesizing or essentially charting out various alternative pathways through which a specific task may be accomplished or a specific problem may be resolved. This map of alternative problem-resolution pathways is referred to as the problem space. It is a map of the fashion in which the human subject is likely to represent the problem internally and it specifies the boundaries within which the actual performance strategy is likely to be found. This problem space is mapped out on the basis of rational analysis of what steps might be involved in solving a specific problem. It is then refined on the basis of available knowledge about human information-processing characteristics and limitations. A particular problem-solving strategy that may be feasible logically may be psychologically unfeasible for the human information process. Through this type of process, Newell & Simon construct a clearly articulated sequential model of the problem-solving process—a problem-solving program. The adequacy of this initial problem-solving model or program is evaluated on the basis of its ability to reproduce the performance of interest (i.e., solving the specific task) and to take into account various things we know about the human information-processing system.

It is at this point that the full implications of the information-processing paradigm for research in psychotherapy become realized. The traditional extensive analysis design evaluates the validity of a theory by statistically comparing the average performances in experimental versus control groups. In contrast, the paradigm of intensive analysis emerging from information-processing research adopts the equally valid alternative strategy of evaluating a theoretical model, first and foremost, in terms of its ability to explain, predict, or reproduce performance in a specific environmental context for a specific subject. Crucially, in contrast to the extensive analysis design, which is never subjected to the ultimate test of whether or not it can successfully model the performance of the individual, the intensive analysis paradigm we have been describing concentrates heavily on judging a model on the very basis of its ability to predict individual performances.

This is a radically different intuition of how one goes about developing and testing scientific models. This difference in intuitions must be appreciated before the more discovery-oriented intensive analysis design can be evaluated in an informed fashion rather than dismissed out of hand because it does not conform to a more conventional conception of how scientific models are developed and tested.

Once the problem space has been defined, problem-solving protocols are then obtained for individual subjects by having them “think aloud” while solving a specific task. These protocols are coded for the types of psychological operations which are taking place and then compared on an individual basis with the preliminary problem-solving model or program. Through the careful inspection of individual protocols and comparison with the initial model, revisions in the model are made to bring it in line with the real phenomenon.

It might be argued that this type of theory-guided observation contaminates the data, and that the observations are not “pure.” Data out of context of theory are meaningless, however, and there are no “pure” observations (Harre et al., 1985; Weimer, 1979). As Harre et al. argue:

Only theories make it possible to understand actions; and the understanding gained leads, in its turn to the elaboration of theories and to their confirmation or disproof. The explanation of behavior is, therefore, an interplay between the formation of theory and the collection of empirical data, best used as in the physical sciences to illustrate the theory. The intimate interplay between theory and fact, each dialectically engendering the other, is the most characteristic feature of the natural sciences and responsible, above all, for their development. (p. 113)

Intrinsic to the task-analysis approach is the notion of progressively refining the fidelity of the model to the phenomenon of interest through a process of successive approximations (Greenberg, 1975, 1984; Rice & Greenberg, 1984). The extensive analysis methodology postulates a single hypothesis which is either refuted or not refuted,
depending upon whether or not group differences are found. In contrast, the task-analysis procedure employed by Newell & Simon (1972) involves the progressive development of a heuristic model of human performance which is tested and refined in an iterative fashion on the basis of its ability to model specific individual performances. A further refinement of the task-analysis strategy can be found in the work of Pascual-Leone (1976a,b). In his study of children’s performances on Piagetian tasks, Pascual-Leone has employed what he refers to as a process-structural approach to task analysis, in order to combine rational and empirical strategies for purpose of modeling microprocesses in subjects’ performances.

This type of task analysis is uniquely suited to the analysis of the psychotherapy change process (Greenberg, 1975, 1984; Rice & Greenberg, 1984). In trying to understand the nature of the psychotherapy change process we are not interested in testing a specific psychological theory such as attribution theory, cognitive dissonance theory, or learned helplessness theory. We are interested in modeling the ongoing performances and information-processing activities of individuals in specific situations. If the potential implications of the cognitive sciences and information-processing theory for the study of human functioning are to be fully realized, it must be recognized that the use of artificial intelligence methodologies can extend beyond the practice of conceptualizing psychological processes using computer metaphors. It can extend to the practice of actually modeling human psychological performances using what is essentially an artificial-intelligence approach.

The Task-Analysis Approach in Psychotherapy Research

In their research model, Greenberg (1975, 1984) and Rice & Greenberg (1974, 1984) have combined Newell & Simon’s (1972) ideas and Pascual-Leone’s (1976a,b) more fine-grained task-analytic approach and adopted them to psychotherapy research through the following conceptual tactic. They reason that if we can operationalize the therapeutic intervention in a relatively standardized way, we can then consider the intervention to be a task environment in which the patient attempts to solve a particular cognitive-affective task.

A research methodology which is able to model the types of cognitive operations that lead to change in a specific therapy task environment will potentially provide the clinician with information about what type of patient operations are necessary in order for a therapeutic intervention to be absorbed. If on the basis of this research the therapist can learn to discriminate between patient operations which interact with his or her intervention in a therapeutic fashion and patient operations which do not, the therapist then will have important information about when to apply specific interventions and when not to apply them.

In order to treat a therapy intervention as a stable task environment, however, Rice & Greenberg (1984) suggest as a basic prerequisite that it should be administered in a relatively standardized fashion. Fortunately, this requirement is consistent with the growing trend in psychotherapy research toward manualization of therapy interventions and the development of rating systems to evaluate whether or not therapists are adhering to protocol (Luborsky & DeRubeis, 1984).

There is, however, as Rice & Greenberg (1984) point out, an important difference between the task environment in a logical problem-solving task and in a therapy task. The psychotherapy patient is not attempting to resolve a logical problem. The tasks which the psychotherapy patient attempts to solve are more personal and affective in nature. Moreover, in contrast to the types of problems investigated by Newell & Simon (1972), the problems that the psychotherapy patient attempts to resolve do not have finite or unambiguous solutions. They are thus ill-defined problems (Greenberg, 1975, 1984).

The validity of the psychotherapy patient’s solution to a given psychotherapy task cannot be evaluated by comparing it with some a priori criterion, but rather against a more subjective criterion. A task in psychotherapy can be considered to be accomplished when the patient achieves a cognitive-affective sense of resolution (Greenberg, 1975, 1984).

Thus, unlike the context of logical problem solving, there is an intrinsic indeterminacy to the type of problem solving that takes place in psychotherapy. This indeterminacy creates methodological problems for the researcher which do not exist in the context of logical problem solving.

In logical problem-solving research the heuristic value of the preliminary model which is developed is evaluated on the basis of its ability to reproduce the correct answer. Since there is no “correct” answer as such in the psychotherapy context, this verification step cannot be built into psychotherapy process research, in the same fashion that it
can in logical problem-solving research. Since the heuristic value of the preliminary model in psychotherapy process research cannot be evaluated against some a priori criterion, there is no way of evaluating whether the initial modeling attempts are on track.

In logical problem-solving research there are a number of pieces of evidence which together constitute a compelling case for the heuristic value of a problem-solving model. One is that it can reproduce the performances of specific individuals; the other is that it generates the correct solution. Since the psychotherapy researcher has no a priori criterion against which to evaluate the correctness of his or her preliminary program, he or she is faced with an important methodological problem at the outset.

A second source of indeterminacy results from the fact that the psychotherapy researcher cannot chart out the initial problem space for a specific task environment on the basis of logic. The logical problem-solving researcher can be guided by logic in his or her initial attempts to map out the problem space. The psychotherapy researcher can be guided by rational speculation about what might be involved in solving a specific psychotherapy task, but this is not the same thing as logical computation. In addition to rational speculation, available psychotherapy theory, and theory and research from general psychology, the psychotherapy researcher must use a kind of empathic process to chart out the problem space and to establish the preliminary model (Greenberg, 1975, 1984; Rice & Greenberg, 1984). In other words, he or she must be able to "walk in the moccasins" of the psychotherapy patient in order to develop hypotheses about potential pathways for problem resolution. The critic might object that empathy is a poor tool for generating hypotheses about human psychological functioning. However, the difference between psychology and the nonsocial sciences is precisely the fact that we do have empathic access to the relevant phenomenon, that is, human psychological processes. To reject the use of this empathic ability as a tool for generating initial hypotheses would be foolhardy.

It does, however, introduce additional indeterminacy into the process. The number of problem-resolution pathways leading to a specific solution which can be generated on the basis of logic are finite. In contrast, the number of alternative pathways that could lead to an ill-defined problem and which must be generated on the basis of theory and empathy are indeterminate.

Because of the sources of indeterminacy intrinsic to the psychotherapy task, the researcher must make a number of methodological innovations. These involve: 1) establishing performance criteria, 2) operationalizing and validating hypothetical constructs, and 3) hypothesis testing. We will discuss each of these in turn.

Establishing Performance Criteria

The psychotherapy task-analysis strategy suggested by Rice & Greenberg (1984) begins by developing a preliminary or "idealized model" to guide the observation of psychotherapy sessions. This can be likened to the preliminary problem-solving programs developed by problem-solving researchers. The researcher does this on the basis of a number of different sources of information. These include: relevant psychotherapy research and theory, his or her storehouse of past experiences as a therapist and/or patient, empathizing or attempting to place himself or herself into the mind of the patient in the task situation, and observation of relevant psychotherapy sessions or task performances.

As indicated previously, however, psychotherapy research has no a priori criterion for picking relevant task performances or identifying correct solutions. In a psychotherapy task a correct solution is one that ultimately leads to good therapy outcome (Greenberg, 1975, 1984; Rice & Greenberg, 1984). The psychotherapy researcher who is investigating change within session at a relatively molecular level, requires measures of suboutcome to serve as intermediate points of linkage between molecular in-session changes and ultimate outcome. These intermediate measures of outcome must be established on the basis of theoretical grounds and on the basis of empirical research. Different measures of suboutcome may be appropriate for different forms of therapy. For example, automatic thoughts may constitute a good measure of intermediate outcome in cognitive therapy, while level of experiencing may constitute a good measure of intermediate outcome in client-centered therapy.

Although theory should play an important role in the establishment of performance criteria, it must also be established on the basis of empirical research. For example, Safran et al. (1987) found that in cognitive therapy sessions, change in automatic thoughts (as rated from both therapist and patient perspectives) was able to predict patient ratings of change in degree of problem resolution.
at the following session. There are a number of substantial methodological and conceptual problems involved in establishing performance criteria, and we believe that tackling these problems is one of the key challenges facing psychotherapy researchers at present. As Kiesler (1983) states, “The study of what transpires between patient and therapist in their sessions is of little heuristic value unless it directly addresses the raison d’être of the transaction itself: patient change and improvement” (p. 2).

Operationalizing and Validating Hypothetical Constructs

Once a preliminary model has been established Rice & Greenberg (1984) suggest a process of cycling back and forth between the model and the phenomenon. This cycling process permits the researcher in a rigorous and iterative fashion to evaluate and refine the model on an ongoing basis as he or she observes discrepancies between the model and the observed phenomenon.

At the same time as this process of model construction and refinement is taking place the research team works on developing converging procedures to measure the hypothesized patient operations and states in a reliable fashion. Since, as indicated earlier, the performance strategies of psychotherapy patients are really shifts in subtle psychological states rather than logical computation strategies that can be easily identified in verbal protocols, it may require considerable ingenuity on the researcher’s part to measure them. For example, Greenberg (1983) in a study we will describe in greater detail later, employed Client Vocal Quality (Rice et al., 1979), Experiencing (Klein et al., 1969) and Structural Analysis of Social Behavior (Benjamin, 1981) measures in order to verify the existence of a hypothesized process in which the patient softens his or her self-criticism in the two-chairs task in Gestalt therapy.

The process of specifying which procedures can be used to measure the hypothesized operations helps the researcher to further refine and clarify what precisely he or she means by the hypothesized operations (Rice & Greenberg, 1984). For example, if we hypothesize that it is important at a particular point in the therapeutic process for the patient to access a core rather than a peripheral belief (Safran et al., 1986) the process of stipulating what measurement procedures can be used to assess whether or not a belief is indeed core demands that we specify precisely what we mean by core. If one hypothesizes that transmuting internalizations play an important role in psychoanalytic change (Kohut, 1984), the process of operationalizing this construct for measurement purposes can help to clarify what it means. Luborsky’s (1984) research on the core conflictual relationship theme provides an excellent example of the way in which developing a procedure for measuring a construct (in this case transference) can provide direct clinical payoff by deepening our understanding of its true meaning. The measurement procedures which are employed can be a combination of both standardly used instruments, when relevant (e.g., Elliott, 1984; Greenberg, 1984), as well as observational categories which are developed specifically for the research (e.g., Rice & Saperia, 1984).

The performance strategies employed to solve psychotherapy tasks should be thought of as hypothetical constructs. Their presence must be assessed with converging measurement procedures. The researcher attempts to establish construct validity of his or her process measures and observational categories by demonstrating that those measures designed to assess the same operation are more highly correlated with one another than to those which are not (Rice & Saperia, 1984). This construct validation functions as a type of bootstrapping process through which the empirical establishment of a theoretically meaningful network of findings validates the measures and establishes the scientific utility of the hypothetical construct by linking it in a theoretically predictable fashion to observables (Cronbach & Meehl, 1955; Rice & Kerr, 1987).

Hypothesis Testing

Rice & Greenberg (1984) suggest that once a refined model has been constructed and reliable, valid measurement procedures have been generated (see Rice & Saperia, 1984, for an example of this type of construct validation), the adequacy of the model can be evaluated, using an extensive analysis design. Because of the various sources of indeterminacy discussed earlier, a verification phase, using aggregate data, is important. Even though the psychotherapy researcher can establish clear-cut performance criteria for selecting “good” sessions, there is still no one “correct” solution to the psychotherapy task. Moreover, despite the psychotherapy researcher’s attempts to operationalize his or her hypothetical constructs, the performance strategies of psychotherapy patients are not as unambiguous as the performance strategies
of problem-solving subjects. The finding that a psychotherapy process model appears to capture the performance of a specific psychotherapy patient is thus not as compelling as the finding that a problem-solving program successfully models a subject's performance.

For these reasons Rice & Greenberg (1984) suggest that it is important to empirically demonstrate that the performance model is generally able to predict good psychotherapy outcome. While hypothesis testing using aggregate data is essential, it is important that we do not engage in premature verification attempts or hypothesis testing. As Meehl (1978) has stated, one of the central problems with research in psychology has been a tendency to engage in hypothesis testing before the researcher has hypotheses worth testing. This concern is central to grounded theory researchers in sociology (Glaser & Strauss, 1967) who maintain that one of the problems with contemporary research in sociology has been a misplaced emphasis upon the verification of hypotheses to the exclusion of the vital stage of the development of theory which is grounded in the phenomenon of interest.

The type of rigorous modeling procedures we have been describing allow the researcher to make predictions not only about what patient operations lead to favorable ultimate outcome but also about the precise sequencing of operations that should be important. As previously argued, this can provide the type of information that the clinician can really use in therapy. It is also significant from a scientific perspective.

As Meehl (1978) has argued, "It is always more valuable to show approximate agreement of observations with a theoretically predicted numerical point value, rank order, or function form, than it is to compute a 'precise probability' that something merely differs from something else" (p. 825). In other words, to the extent that one can test a hypothesis which predicts a particular pattern of events rather than one discrete event, one has subjected the hypothesis to grave danger of refutation (Popper, 1959). Demonstrations of this type are thus much more compelling than simple demonstrations of differences in group means and have the potential for contributing to the development of new knowledge rather than confirming the obvious. As Weimer (1979) asserts, "Scientific hypotheses, if they are good informative ones, are highly novel, bold conjectures that are as logically improbable as probable. . . . The aim of science is to learn new things—not to enshrine reality" (p. 38).

Verification can come in various shapes and forms. The researcher can evaluate one part of the model, while keeping in mind other parts of the model. Alternatively, hypotheses can be generated with respect to the model as a whole. The important point to remember, however, is that this verification stage should not come until a reasonably good fit between the model and reality has already been established through a process of cycling back and forth between model refinement and intensive observation of the phenomenon. This cycling process involves the sequential observation of single cases, to evaluate whether or not the fit which is observed between model and phenomenon in one case generalizes to a second case. At this stage in the research process it is essential that we make detailed observations of single cases rather than testing hypotheses using aggregate data. The latter course of action would potentially obscure important recurring patterns of change which can only be observed through the intensive observation of single cases.

However, once a refined model has been developed through this rigorous, iterative cycling process, formal hypothesis testing can take place. Greenberg (1983), for example, compared 14 Gestalt "two-chair dialogue" conflict-resolution performances with 14 nonresolution performances to evaluate the validity of a proposed three-phase sequential model of conflict resolution. He used Benjamin's (1981) Structural Analysis of Social Behaviour as well as the Experiencing Scale (Klein et al., 1969) and the Client Vocal Quality Scale (Rice et al., 1979) to assess the nature of the interaction between the two chairs at various phases of the resolution sequence. The pattern of results within the resolution group supported the proposed three-phase model of conflict resolution. It indicated that in all the resolution performances the two sides of the conflict appeared to go through a stage of opposition and then enter a merging phase in which the critical side softened its attitude. In the final phase it was found that both chairs or sides of the conflict became simultaneously more autonomous and affiliative, and engaged in a negotiation.

In a second study Greenberg & Webster (1982) classified Gestalt therapy patients into "resolvers" and "nonresolvers" on the basis of in-session process indicators that were hypothesized to be relevant on the basis of a refined model of client performance. The results revealed that the process indi-
cators which were derived in this fashion were indeed predictive of treatment outcome.

The above-cited studies provide but two examples of the fashion in which hypothesis testing can be conducted at various stages of the research process and model development. It is important to emphasize that although formal hypothesis testing comes at a somewhat later stage of the research process, it by no means constitutes the final stage. The model continues to be refined, developed, and subjected to further tests on the basis of the results of verification studies. We conceive of this activity as a never-ending process of model refinement. A scientific theory or proposition can never be established as true. It can only be established as more or less heuristic at a given point in time depending on its ability to make interesting predictions and to survive critical tests (Kuhn, 1970; Lakatos, 1970; Popper, 1959; Weimer, 1979).

Implications for Psychotherapy Practice and Training

The task-analysis approach outlined here has both long-term and immediate implications for clinical practice and training. As we have emphasized to this point, the type of fine-tuned understanding of the change process potentially emerging from research of this type can play an important role in the long-term development of psychotherapy as a science. Just as important, however, is the immediate payoff to the clinician who is actively engaged in the research process.

The activities of explicitly articulating a preliminary model of the change process and intensively observing psychotherapy sessions in order to refine the model sharpen one’s observational powers and sensitize one to subtle shifts in client process which might otherwise elude detection. The continuous process of cycling back and forth between model and phenomenon as the model is refined forces the clinician to explicate the sometimes hidden assumptions that guide intervention and to evaluate them against empirical reality. The activity of developing measures to operationalize hypothesized client operations and states also refines the clinician/researcher’s understanding of these operations and helps to develop a greater ability to perceive subtle shifts in client operations.

The very process of conducting this type of research can thus play an important role in developing and refining the clinical skills of the researcher. This can help to reduce the traditional tension between psychotherapy research and practice and to bring out the intrinsic complementarity of the two activities.

Guidelines for Evaluating Task-Analysis Proposals

Although influential psychotherapy researchers have been advocating the use of intensive analysis procedures in psychotherapy research for years, there is currently a lack of consensus as to what exactly is meant by intensive analysis. As we have discussed, intensive analysis research can range from the uncontrolled case study to the operant single-case design to the stage-process model (Cashdan, 1973; Hudgins & Kiesler, 1987; McCullough, 1984a, b) to the task-analysis approach described in this article.

Since there is no consensus in the field regarding the nature of an appropriate intensive analysis design, it is no surprise that there are a lack of clear guidelines for evaluating the adequacy of intensive analysis research. This constitutes a particular problem when it comes to the evaluation of intensive analysis research proposals, since the criteria for evaluating the adequacy of an extensive analysis proposal do not apply to the intensive analysis design.

Applying more conventional extensive analysis experimental design criteria to the evaluation of proposed intensive analysis research thus discourages execution of the type of research consistent with the recommendations of leading psychotherapy researchers and endorsed by the NIMH Committee on Psychotherapy Process Research (Kiesler, 1983). It also discourages the type of investigation that has the potential for remedying some of the problems of contemporary psychotherapy research that are responsible for its lack of impact on practicing clinicians (Barlow, 1981). For this reason it is important to provide researchers and granting agencies with alternative research-design evaluation criteria which are more appropriate to the task-analysis research paradigm outlined by Rice & Greenberg (1984). Guidelines for this will be described under the eight headings: preliminary model development, linking process to outcome, preliminary intensive analysis, maintaining a stable task environment, specification of process measures, construct validation, verification plans, and programmatic research plans. These serve as a translation of the different stages of Rice & Greenberg’s task-analytic model (Rice
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& Greenberg, 1974, 1984) into design evaluation criteria.

Preliminary Model Development

The first criterion is that the researcher should be able to present some evidence of preliminary model development. He or she should be able to ground the preliminary model in the relevant literature in a convincing fashion and to explicitly articulate the theoretical assumptions which have guided the development of the preliminary model. Wherever possible, experimental findings relevant to the construction of the preliminary should be cited. The researcher should thus be able to fully outline the chain of reasoning which has led to the development of the preliminary model. This would include both a clear description of the task environment (i.e., the therapeutic intervention) as well as a preliminary description of the problem space (i.e., the client’s internal representation of the task environment). The problem space should consist of a number of different search paths which are potentially available to the patient in attempting to solve the therapeutic cognitive-affective task.

According to Newell & Simon (1972) there are two questions which must be asked in order to develop a theory of problem solving. The first is “How can a problem be solved?” The second is “What makes a problem difficult?” The psychotherapy researcher using the task-analysis approach should thus be able to speculate in an informed fashion about what might be involved in resolving a therapeutic task as well as what the obstacles to task resolution might be. For example, Greenberg (1984) in an initial attempt to develop a preliminary model of task resolution in the Gestalt therapy two-chairs environment speculated that resolution in the two-chairs task environment might occur in three different ways:

1. resolution by some form of integration;
2. resolution by a release of previously unexpressed feelings;
3. resolution by a change in perspective that makes the conflict no longer relevant.

Furthermore, he identified two potential obstacles to task resolution:

1. talking about the conflict rather than experiencing it;
2. avoiding the conflict by minimizing its significance.

Safran & Greenberg (1982a) and Safran et al. (1986) argue that the effectiveness of the inspecting evidence intervention in cognitive therapy is at least partially dependent on an initial process in which the client fully accesses the relevant cognitive distortion. This hypothesis is based on a growing theoretical literature suggesting that enduring changes in therapy results from modifications in core cognitive processes or cognitive processes linked to an individual’s fundamental self-esteem (Guidano & Liotti, 1983; Kelly, 1955; Mahoney, 1982; Meichenbaum & Gilmore, 1984).

Safran & Greenberg (1986) speculate that patients may have difficulty accessing relevant cognitive processes if they are not in a mood which is representative of the emotion they experience in the relevant problematic situation. This hypothesis is based on the empirical and theoretical literature suggesting that the accessibility of cognitive processes is mood congruent (Bower, 1981; Lang, 1983; Greenberg & Safran, 1984a,b, 1987; Safran & Greenberg, 1982a,b, 1986).

Weiss et al. (1987) hypothesize that patients undergoing psychoanalysis are able to acknowledge warded-off affect when their pathogenic beliefs about dysfunctional relationships are disconfirmed through the therapeutic interaction. According to them, this process takes place if the therapist is able to pass the transference test to which the patient unconsciously submits him or her.

As these examples illustrate, it is possible to outline a network of assumptions and hypotheses about potential pathways and obstacles to task resolution on the basis of theoretical and empirical literature, relevant to the interface between a specific task environment and our understanding of the human problem solver. The researcher should ground the preliminary model in this type of network and chart it in graph form (Greenberg, 1984; Newell & Simon, 1972).

Linking Process to Outcome

The researcher should have an adequate plan for evaluating good and bad psychotherapy sessions. As we have argued, since there are no a priori criteria for evaluating the correct solution to a cognitive-affective problem in psychotherapy, establishing criteria for discriminating between task resolution and nonresolution sessions is of the essence.

For those therapies in which valid and predictive measures of session outcome are not available
(and this includes most therapies), the refinement of session-selection criteria should be an ongoing part of the research program. As we have discussed earlier, research designed to establish proximal outcome at a more molar level of analysis (e.g., a change in cognitive distortion in cognitive therapy or an improvement in therapeutic alliance in psychodynamic therapy) is required to establish intermediate links between the more molecular in-session changes in patient state and ultimate therapy outcome.

**Preliminary Intensive Analysis**

The researcher should be able to show that he or she has observed a few cases intensively and to demonstrate the applicability of the preliminary model to these cases, using clinical transcripts. It should be readily apparent to the research evaluator that there is a good degree of fit between the preliminary model and the clinical transcript. Although this is a subjective, qualitative judgment rather than a quantitative one, it is no more qualitative than the types of judgments which are made by researchers who are attempting to evaluate the degree of fit between a logical problem-solving model and a subject’s problem-solving protocol (Ericsson & Simon, 1984).

As Ericsson and Simon (1984) argue in the context of logical problem-solving research:

> In many cognitive tasks the exact verbalizations of the subject are not determined uniquely by the task, or even by the subject’s strategy. Under such circumstances, assessing whether a protocol fits the predictions of a model is a little like judging whether a sentence is grammatical. Linguistics has traditionally relied on naive informants for making judgments of grammaticality. Only very recently has it been possible to attempt such assessments automatically with the aid of computers. In the same way, most analyses of protocol data are carried out by investigators who have had extensive experience with the cognitive processes and their verbalization for the task under study. Comparing a verbal protocol with the predictions of an information processing model involves a judgment not dissimilar to the judgment of grammaticality. Its validity depends at least partly on the consensus of the judges. (p. 348)

Assessing whether or not the clinical transcript fits the predictions of the researcher’s preliminary model involves a judgment similar in some senses to the judgment of grammaticality. The judgment of the extent to which a model fits clinical transcripts, however, cannot be made by naive raters. It is thus essential that the research evaluator be an expert in the relevant form of psychotherapy. The research proposal evaluator must also be able to offer some judgment as to how representative the clinical transcript is of the relevant form of therapy.

**Maintaining a Stable Task Environment**

The researcher should be able to provide assurance that the task environment to be researched will be a consistent one (Greenberg, 1980, 1984). It is thus required that he or she specify the training that the therapists will have and also indicate the procedures that will be used to monitor whether therapists are adhering to the therapy protocol.

**Specification of Process Measures**

The researcher should be able to specify tentatively what process measures will be employed to assess the presence of the hypothesized patient operations. The measures that are proposed must have adequate reliability and must at least have face validity as measures of the hypothetical constructs the researcher is proposing. Adequate procedures must be proposed for assessing reliability of the measures in the study. If the researcher intends to develop coding categories specific to the therapy to be investigated, evidence of preliminary development and plans for further refinement should be provided.

**Construct Validation**

The researcher should provide a plan for construct validation of the proposed measures. This should involve making tentative predictions regarding the fashion in which different process measures and observational categories will intercorrelate.

**Verification Plans**

The researcher should be able to outline a general strategy for the verification or hypothesis-testing phase of the research. The absence of a clear-cut experimental hypothesis is a significant weakness in a hypothesis testing–extensive analysis design. Clearly, however, in the early phases of the task-analysis research program the researcher will not be able to articulate specific hypotheses. However, he or she should be able to offer some tentative examples of hypotheses to be tested (depending on the outcome of the earlier phases of the research), and be able to outline a general strategy for testing these hypotheses, using extensive analysis designs.

**Programmatic Research Plans**

The researcher should demonstrate a clear grasp
of the task-analysis research paradigm and be able to outline in a methodical form, the sequential phases that will be involved in the research. The research proposal should be able to demonstrate the programmatic nature of the research with a clear awareness of what the various phases are in that program.

Conclusion

In this article we have argued that the failure of psychotherapy researchers and granting agencies to follow the persistent recommendations of leading researchers in the field regarding the use of intensive analysis procedures, can be traced to a number of sources. One source is the tendency to see the extensive analysis approach as the sine qua non of scientific methodology and to assume that intensive analysis procedures are necessarily unscientific. Another source is the general lack of familiarity with different intensive analysis designs and a failure to recognize that there are in fact different intensive analysis designs, each with its own strengths and weaknesses.

Another source is the fact that the most methodologically rigorous intensive analysis design with which the research community is generally familiar is the operant single-case experimental design. While this design is becoming more and more popular among behavioral researchers, we have argued that a number of its assumptions are inadequate to deal with psychotherapies and psychotherapy theories which are more cognitively oriented. As an alternative to the behavioral single-case experimental design, we have briefly described the task-analysis approach to psychotherapy research (Greenberg, 1984; Rice & Greenberg, 1984) which has its roots in experimental cognitive psychology. This is an approach which combines both intensive and extensive analysis designs and discovery and justification approaches to scientific research (Reichenbach, 1951).

We have argued that it is an approach to research that has the potential to provide clinicians with the type of finding that can truly impact on clinical practice. It can thus serve as a much-needed correction to the current situation. We are not arguing that extensive analysis designs have no place in psychotherapy change process research. Instead, the position we take is that once specific and scientifically interesting (Popper, 1959) hypotheses are developed using the type of intensive analysis procedures we have described, their generalizability across individuals can be tested using extensive analysis research designs. What we wish to make clear, however, is that the type of discovery-oriented intensive analysis approach we have described should be a pivotal part of the scientific process. It is a conceptually demanding, methodologically rigorous, and labor-intensive process which should not be relegated to the status of “pilot work” which takes place before the “real research” begins.

An important obstacle to conducting this type of research is constituted by the fact that the standard criteria for evaluating the adequacy of more conventional extensive analysis research are inadequate, and there are a lack of clear-cut alternative guidelines. To remedy this situation we have outlined a number of guidelines that can be used for evaluating the adequacy of task-analysis research proposals.

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