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WERE LIONS TO SPEAK, WE WOULDN'T UNDERSTAND[1]

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The topic of this paper is criteria for evaluating clinical research in mathematics education. The thesis is that any set of criteria must be consistent with the underlying epistemology from which research of a particular type is carried out. In developing this thesis, I will explore the notion of "world views" in mathematics education research and show how two in particular, environmentalism and constructivism, contrast in their implications for how research is to be carried out and interpreted. Finally, I will offer a set of criteria applicable to constructivist clinical research in mathematics education.

In 1978, the Research Advisory Committee of the National Council of Teachers of Mathematics published a set of criteria for evaluating research proposals and reports (Coburn, 1978). It immediately generated a controversy—one that continues today. The RAC's criteria were decidedly slanted toward empirical studies of a classical mold: purpose, objectives, and procedures must be clearly specified; variables studied must be identified; research hypotheses are concise and logically derived from some theory or related problem; the research design is appropriate to the problem; populations and samples are clearly described; controls for sources of errors are described and are appropriate; samples are of sufficient size and are representative of the populations; methods of analysis are valid and appropriately applied; statistical assumptions are satisfied; and so on. These criteria were intended to be applied across experimental, clinical, and organizational studies, while each of these categories had additional criteria unique to itself. Additional criteria for clinical studies were: the investigated phenomena are clearly identified; interviews and observation guidelines are related to key elements of the study; and the methodology for recording interviews is appropriate.

Objections to the RAC's criteria surfaced immediately. Wheeler (1978) chided the RAC for an apparent narrowness of perspective, and wondered whether Piaget's or Wertheimer's research would have survived scrutiny by reviewers who wore the lens of the guidelines. While admitting that "normal

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research" requires some set of procedural rules in order to establish a basis of "trust" in what is being reported, Wheeler went on to point out that research involves much more.

> What saddens me more, however, is . . . the lack of awareness shown by the RAC—awareness of the activity of research. It is really not aware that definition, precision, and clarity are products of the hard central processes of research and not among its preliminaries? Can all kinds of research be described as having "probable" outcomes stemming from "plausible" hypotheses?

Fennema (1978) responded to Wheeler on behalf of the RAC by pointing out that it intended the criteria to be used as "a format for the judging and reporting of research studies—not the doing of research" (p. 398).

In my opinion, Fennema missed a central point implicit to Wheeler's objection, namely that any research enterprise is carried out from the perspective of a "world view", and that criteria relevant to one view may be inadequate for, or wholly irrelevant to, another. Webb (1979) and Lester and Kerr (1979) came close to this position in their responses to Fennema. Webb attested to an uneasiness with the RAC's criteria in that it is suggestive of a restrictive view of what constitutes research in mathematics education. Lester and Kerr argued that laboratory and naturalistic studies have different functions: laboratory studies (manipulation and control of the environment) are aimed at verification of existing theory; naturalistic studies are aimed at generation of theory. They reiterate a point made by Snow: statistical techniques may be legitimately applied in areas such as agriculture where the subject is passive, but they are inappropriate to education where subjects are "...active, flexible, adaptive processors of information available in a probabilistic, partially redundant environment" (Snow, 1974 as quoted in Lester & Kerr, 1979). Lester and Kerr went on to point out, however, that non-experimental methodologies have inherent limitations: ". . . data collected from non-experimental procedures can be difficult to interpret due to the lack of control of variables, thereby causing internal validity to be suspect" (p. 230).

Both Webb's and Lester and Kerr's responses circumscribe, yet fail to address, what I contend is the basic issue—that of differences in world views. In fact, Lester and Kerr, in noting that the internal validity of non-experimental studies is inherently suspect, confound two views that I will later argue are largely incompatible. Webb's remark suggests the mistake of the RAC was in not taking a broader view of research in mathematics education, meaning a view that would encompass both laboratory and naturalistic studies (to use Lester and Kerr's classification) as valid instances of scientific research. I will argue instead that what is called for is not a broader view, but an acknowledgement of a multiplicity of views of research in mathematics education, quite possibly each being irreconcilable with the others.

The idea that there may be different world views and not just differences of experience, perspective, or focus is certainly not new. Benjamin Whorf formulated this thesis most eloquently in his comparison of English and Hopi temporality. Time, in an English speaker's view, has duration and is thought of as extending over past, present, and future. In a Hopi speaker's view, however, time exists only now; to speak of, say, "days gone by" as objects in and of themselves is nonsense. Rather, there is the overtaking of events. The ontological status of a past event is that of an item of recollection; a future event is an item of expectation; a current event is an item of experience. In Hopi thought, as characterized by Whorf, only current events exist in time. Whorf saw this as having direct implications for science.

> The categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face—on the contrary, the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds...We cut nature up, organize it into concepts, and ascribe significances as we do largely because we are parties to an agreement to organize it in this way . . . the agreement is, of course, an implicit and unstated one, BUT ITS TERMS ARE ABSOLUTELY OBLIGATORY [sic] . . . This fact is very significant for modern science, for it means that no individual is free to describe nature with absolute impartiality but is

> constrained by modes of interpretation even while he thinks himself most free. (Whorf, 1956, pp. 213-214).

Kuhn (1962) gave substance to the notion of world views in the physical sciences, and illustrates again and again their infestation at every level of scientific activity—conceptual, theoretical, instrumental, and methodological. At the conceptual and theoretical levels, a scientist's world view constrains both the nature of the problems he poses and the nature of admissible solutions. At the instrumental and methodological levels a scientist's world view (including his view of what science is) acts as a constraint to his selection of instrumentation and the nature and method of his explanations. A short example: a physicist and chemist were each asked if an atom of helium is a molecule. The chemist responded affirmatively; the physicist negatively. To the chemist, a helium atom behaves as a molecule according to the kinetic theory of gases; to the physicist, it behaves unlike a molecule in that it displays no molecular spectrum (Kuhn, 1970, p. 50).

Petrie (1972) developed a weak form of the world view hypothesis—the theory dependency of observation. In doing so he gave an in-depth analysis of an experiment originally discussed by Campbell (1954) with the aim of establishing the point that researchers committed to different theories may generate different "facts" from what appears to be the same phenomenon. The experiment was that a conditioned finger movement was obtained through pairing a shock and a tone. The shock could be removed by an extensor movement of the finger. The hand was turned over, and the experiment repeated. With the hand in its new position a finger retraction rather than extension was required to terminate the shock. Ninety percent of the subjects immediately retracted their finger upon the sound of the tone. What facts had been established? To most behaviorists, they are that ninety percent of the subjects learned that a shock accompanied the tone, that a finger extension would alleviate the shock, and that a transfer had been made to the muscle group responsible for finger retraction. To most cognitivists of an information processing mold, the facts are that the subjects learned that a shock accompanied the tone, and that a finger withdrawal would alleviate the shock—that is, that putting distance between original and secondary positions of the finger will terminate the undesirable effects of the shock, and the relevant pathways to the goal-state depend on the position of the hand. The behaviorist and the information-processing cognitivist see different facts, and they, qua facts, are irreconcilable.

WORLD VIEWS IN MATHEMATICS EDUCATION

The above discussion is meant to establish the viability of the notion of worldviews. To bring the point closer to home, I will focus on two fundamentally different worldviews that I see operating in mathematics education research. These are what I will call "environmentalist" and "constructivist" views of mathematical knowledge. They hold quite different implications for what constitutes acceptable problems, procedures, and methods of research in mathematics education, as I will attempt to show in the following discussions.

The environmentalist view of a student's mathematical knowledge is that it is a function of his environment, and the way to investigate what children know is through manipulation of their environment. Implicit in this view is a form of realism (at least naive; possibly Fregian). What the researcher isolates as the child's environment is the child's environment, or at least constitutes a part of it. Treatments, problem structures, teacher behaviors, and tasks are common items isolated by researchers of mathematical learning and teaching.

The constructivist view of a student's mathematical knowledge is that it is a function of what the student constructs out of his own activity. It is admitted that a student's environment influences these constructions, but it is the student's environment that exerts any influence and not what the researcher isolates. That is, the constructivist takes as a fundamental position that a student's experience, qua experience, is wholly inaccessible to an observer, and hence that there need be no correspondence between what the researcher and the student see as the student's environment.

From an environmentalist's view, a constructivist has put himself into a hopelessly solipsist position; from a constructivist view, an environmentalist builds castles on sand without being aware of it. But each, committed to his own view, sees researchable problems relevant to his interest in mathematics education.

Even at this global level we can see a divergence in the nature of research that might be carried out from these views. In, say, the area of improving mathematical learning, an environmentalist's task is to isolate features of students' environments that are salient to learning mathematics. A constructivist's ultimate task is to build models of what students' environments might be like, but in order to do that he must build models that would substitute for the mathematical student so that he (the researcher) may perceive the world of mathematics through the (modeled) student's eyes. From an environmentalist's view, the path to improved mathematical learning is through the manipulation of key features of the environment; from a constructivist's view it is through the manipulation of his environment so that salient features of the student and his (the student's) environment are put in a position to make it likely that he will make the desired construction. Of course, the only thing a constructivist has to go by are his models. (The term "model" itself has different meanings according to whether it is used in a realist or constructivist sense. Used realistically, it refers to an object having an ontological status independently of any particular knower; used constructivistically, it refers to a conceptual system held by a particular knower at a particular time.)

Let me examine an implication of the discussion for evaluating research. An absolutely essential ingredient of acceptable experimental research from an environmentalist's view is control of the environment. Without control, there is no way to separate "signal" from "noise." Common attempts at environmental control include standardization of procedures, manipulation of problem or task structure, factorization by treatment level, manipulation of problem difficulty, and various statistical controls. From a constructivist view, "noise" and "signal" are relativistic terms, and refer only to a state of knowledge or model development. The aim of constructivist experimental research is that the models are sufficient and viable with regard to the data they address—sufficient in the sense that were the models substituted for the subjects and given the same inputs ("same" from the researchers point of view), the outputs would correspond; and viable in the sense that the models are not in conflict either with the data, with related models, or with his general epistemology of mathematical knowledge.

A constructivist and an environmentalist judging the same experimental study would quite likely make distinctly different evaluations. Where the environmentalist sees an explanation ("the treatment caused the difference because everything else was controlled") the constructivist sees none ("What is the relationship between the 'inputs' and 'outputs'?"). Where the constructivist sees an explanation ("the model accounts for the data"), the environmentalist sees none ("how did you separate 'signal' from 'noise'?"). Borrowing an expression from Kuhn, where one sees a rabbit, the other sees a duck.

The difference between environmentalist and constructivist views also pervades conceptualization of research questions. A particularly illustrative example can be found in research on mathematical problem solving. To an environmentalist, a problem is an objective entity that exists independently of any one problem solver. To a constructivist, a problem is of necessity idiosyncratic to the person solving it, and is entirely of his own making. An environmentalist takes the position that one may investigate a problem by examining it oneself; a constructivist would agree that one may oneself investigate a problem, but would not admit any necessity to a correspondence between the problem the researcher has in mind and the one his subject constructs. The constructivist asks: "What is the problem that this student is solving, given that I have attempted to communicate to him the problem I have in mind?" This is a legitimate research question to a constructivist; to an environmentalist it most assuredly is not.

The ontological status of problems is seldom discussed in an environmentalist research tradition, for the issue makes little sense from a realist perspective. When it is discussed, such as in the context of Simon and Newell's (1971) and Newell and Simon's (1972) explication of task environments, it proves to be elusive. Simon and Newell take task environments, and hence problems, as objectively accessible to the researcher as well as being the dominant factor in the solver's construction of a problem space. That is, they take the position that the researcher and solver "see" the same problem, and differ only, if at all, in problem spaces. However, from a constructivist's view, Simon and Newell's characterization of a task environment as "the omniscient observer's" conception of the problem (Simon & Newell, 1971, p. 151) creates an awkward situation: a mythical observer's conception of the problem becomes the dominant factor in the solver's construction of a problem space.

From a constructivist's view, it is absolutely essential that the researcher keep in mind that what he sees as "the" problem imposes nothing of necessity upon the problem solver. The constructivist aims at uncovering the task environment of a problem, but if forced to "locate" it he would point to the problem solver's head. That is, to a constructivist the task environment that he elaborates is part of his model of the problem solver.[2]

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[2] It is interesting that Newell and Simon considered the position that a task environment is part of the researcher's model of the problem solver as a viable route to theory construction, yet opted for an "objective" analysis that would give a description that includes 'all possible problem spaces'. (Newell & Simon, 1972, p. 64).

The divergence of views on the nature of problems creates conditions rife with possibilities for misunderstanding and miscommunication. I recently discussed with a prominent problem solving researcher a taped interview of a child solving a missing addend problem, and commented that his behavior led me to believe that it was an ill-structured problem. The other party couldn't understand how I could say that, since a missing addend problem is certainly well-structured. I remarked that from his perspective it may be, but the problem that this child was solving seemed to be ill-structured. The discussion soon turned to epistemology: his argument was that if we don't maintain the objectivity of problems (meaning, I assume, that a problem has an independent ontological status), then we lose all hope of being scientific—we've nothing of interest left to control. My argument was that (1) it is tremendously egocentric for anyone to think that his vision is so clear, and (2) that there are tremendous epistemological difficulties inherent in the position that problems are objective—if we were to take it seriously, then I would see little hope of ever explaining how individual children ever come to see what the problem "is". Having located the source of disagreement, we saw the fruitlessness of the debate and agreed to disagree. Had we not uncovered this fundamental divergence of views, we would have continued speaking past one another.

A similar divergence in conceptualization can be found in research on teaching. For convenience I shall focus on research on teacher clarity. In an environmentalist's view, clarity is an objective phenomenon. If it can be objectively determined that a teacher is clearly presenting the subject matter, then clarity per se is a bona fide candidate for investigation as to its saliency to teacher effectiveness. In a constructivist's view, clarity is a communicatory, and hence subjective, phenomenon. A teacher is being clear when the student to whom he is addressing himself understands in the way the teacher intends. Thus, the environmentalist feels fully justified in recording instances of teacher clarity without regard to what the students understand. The constructivist is compelled to ask: "What did the teacher intend? How was he understood?" These in themselves could be legitimate research questions for the constructivist, and answers could only be given on the basis of models of the participants in the discussion.

The divergence between environmentalism and constructivism also has broad implications for how one views the role and function of psychometrics in mathematics education research. Traditionally, psychometricians have interpreted a score on an item or task as being composed of a "true" score and a quantity due to error. An underlying assumption necessary to make this interpretation tenable is that there is a lineal cause-effect relationship between the item or task and the resulting behavior of the student. This assumption may be made clearer in a discussion of the notion of reliability. In psychometrics, a task is said to be reliable if one can demonstrate that each subject's response to it contains little or no error (Formulation A). That is, if each subject responds consistently over time (duration being limited by theoretical considerations), then the task is reliable. A set of tasks is reliable if there is little error introduced by task selection (item sampling— Formulation B). Both conceptions of reliability aim at determining the degree to which the results of testing a given sample are due to systematic sources of variation. The former addresses response variability while the latter addresses task variability. Both conceptions are based on the notion that a task is a constant stimulus across subjects which elicits a response on the part of a subject according to the degree to which he holds some competency or set of competencies. Formulation A rests upon the notion that at a given time a person has a particular response to a particular stimulus according to the dimensions of the stimulus. If response variability is large, then there are dimensions to the task for which the researcher has not accounted, e.g., task structure, administration techniques, or setting. Formulation B rests upon the notion of stimulus equivalence. If the criteria for task selection and administration are well formulated, then variability between the stimuli in the equivalence class will be small and variation in the sample will be due to variation between the competencies held by the subjects (assuming response variability to be small). If one constructs a set of tasks which are reliable according to both A and B, subsets of which measure various dimensions of a construct, then variation in the sample is due to the variation of the tasks along the dimensions of the construct.

The concept of reliability, as formulated above, can be seen to play a major role in research carried out within an environmentalist research program. From an environmentalist's perspective, tasks, items, dimensions, etc. are objects to be manipulated—it is through these that he controls the environment of his subjects, and hence uncovers the reality of their knowledge. From a constructivist's perspective, there is no such thing as a "constant" stimulus— each student constructs each task for himself, and it is the constructive process and construction itself that are manifested in (what the researcher takes as) behavior. If an environmentalist were to point out that each student was presented with the same set of words, figures, etc., the constructivist would reply that such a characterization unwittingly imports invariant features of a standard observer—one that can at least read and comprehend; and that if by "words" it is meant ink blots on paper or sound waves in air, then we are no longer speaking of a task.

Now that I've painted the world in bold strokes of black and white, I'd like to say that what I've offered are intellectual Archie Bunkers. No one consistently holds the radical positions described here, but I maintain that each of us holds either of them often enough to make environmentalism and constructivism useful conceptual categories in trying to grasp the essence of a piece of research.

There are other ways in which I considered characterizing world views. Along with environmentalism seems to come positivism, realism, Platonism; with constructivism comes essentialism, idealism, and skepticism. They were each relevant to the discussion at various points, and yet when I attempted to incorporate them into my remarks they drew the discussion away from the central point— that each researcher, as a psychological subject, takes (often unwittingly) an epistemological stance concerning the nature and genesis of mathematical knowledge, and that this stance exerts a strong influence on what he or she takes as acceptable research in mathematics education.

Another approach I might have taken would be to draw from Lakatos' (1962) idea that modern mathematical philosophy is deeply embedded in general epistemology—relating it to research in mathematics education by focusing on where it seems researchers assume "truth" to be injected into their research programs If I had taken this path, then I would have argued that classical research in mathematics education displays qualities of the three major traditions that Lakatos identified—Euclideanism (deductive application of mathematics tells us what is true about the environment), Empiricism (observational statements consist of perfectly well known terms and are incapable of refutation), and Probabilistic Inductivism (theoretical statements may be affirmed by showing the truth of their negations to be highly improbable). The difficulty with this approach was that constructivism doesn't fit anywhere in Lakatos' scheme—there is no assumption of "truth value injection" made in a constructivist research program. There are only judgments of the viability of models (von Glasersfeld, 1979).

CRITERIA FOR JUDGING RESEARCH

A major implication of the idea that each researcher brings to bear a world view both in conducting and evaluating research is that there exists a great potential for miscommunication and misunderstanding, especially when the research is carried out from a different world-view. Examples abound: Piaget's genetic epistemology was assimilated into behaviorist research programs for decades before it became recognized that the assimilated version of his theory was no longer what Piaget intended. This situation continues today: I can only characterize Ausubel's (1968) interpretation of Piaget's position as "neobehaviorist" as a clashing of world views; similarly with Brainerd's (1979) interpretation of Piagetian stages as measurement sequences. Another example is the great difficulty the group working under Steffe at the University of Georgia has had in getting others to understand their work (Steffe, Spikes, & Hirstein, 1976; Steffe, Thompson, & Richards, 1981, von Glasersfeld, 1981). Theirs is a radically constructivist world view, and the problems, constructs, methods and models emanating from it can "make sense" only when viewed in that context. At least they make little sense when viewed from an environmentalist perspective.

Given the argument that criteria for evaluating research must emanate from within the world view in which it is carried out, we must have (at least) two sets for clinical research in mathematics education. Criteria for environmentally oriented clinical studies can be drawn largely intact from that developed over the last fifty years for classical empirical research: how well has one controlled the child's environment, and how well has one operationally defined the observational categories used? Criteria for constructivist research are less clear, largely because of the novelty of the position to American mathematics education and psychology. To this topic I'll devote the remainder of my paper.

There has been a trend toward looking to the social sciences for methodological guidance. Shulman (1969) outlined a number of approaches used in sociology and social anthropology that might prove useful in investigating teacher effectiveness. A number of recent articles (e.g., Lester & Kerr, 1979; Becker, 1981) have cited Glaser and Strauss (1965, 1969) or Snow (1974) as sources of methodological inspiration. To these I might add Schatzmann and Strauss (1973) and Smith (1978). In reading each of these I was most assuredly struck by the thoughtfulness of their positions, especially the fundamental importance they give to the fact that, in doing research, the researcher constructs explanations of the phenomenon under investigation, and that their methodological observations were devoted to enhancing the viability of these explanations. I was especially taken by Smith's (1978) account of her transition from a logical positivist position to her current, more humanistic one. In fact, I was so impressed that I began to think that I had no business writing this paper. As I went on, however, I began to realize that sociologists address a qualitatively different set of problems than we who are investigating questions of understanding. The sociologist and social anthropologist focus upon social interaction and genesis, whereas we are most often concerned with individuals and their respective cognitive compositions. To be sure, many of these authors' suggested methods and techniques are pertinent to constructivist clinical research. Glaser and Strauss' (1967) technique of defining and redefining observational categories as the study progresses, and Smiths' (1978) strategy, which she attributes to Beitell (1973), of multiple passes, each at a more theoretical level than the previous are but two. Nevertheless, the character of constructivist clinical research is quite different from what these authors have in mind, if only because we are not participant-observers, but interloper-observers. We, purposely, drastically modify the phenomenon of interest so that we may look for its boundaries—we probe the limits of a student's understanding so that we may in return get an idea of what it was like in the first place.

In an earlier paper (Thompson, 1979) I maintained that the aim of any clinical research in mathematics education should be the construction of models. I still maintain that today. What I didn't specify however was what constitutes a model, nor how one goes about constructing one. Since that time I came across Maturana's (1978) excellent piece on the nature and role of the observer in scientific enquiry, and it helped tremendously in clarifying my thinking. First, by "model" I mean a conceptual system held by the modeler which provides an explanation of the phenomenon of interest, in this case a student's behavior within some portion of mathematics. The conceptual system held by the modeler, when applied to a particular student as an explanation of his behavior, is a model of that student's understanding. Maturana puts it thus:

> As scientists, we want to provide explanations for the phenomena we observe. That is, we want to propose conceptual or concrete systems that can be deemed intentionally isomorphic to (models of) the systems that generate the observed phenomena. (Maturana,1978, p.29).

In implementing this idea in my own research, I have made a distinction largely along the lines of Lin (1979). When attempting to communicate the components out of which I construct explanations of children's behaviors, I characterize the conceptual system, qua system, as a framework; models arise when one applies the framework to a particular child. That is, there are no models in the abstract. A model always has a prototype (cf. Richards, 1979, for an opposing view). This obviates the need to address the question of what a model is modeling when it is characterized as a system per se.

The second question—how does one construct a model—becomes quite easy to answer when we look at it from a psychological viewpoint. One constructs a model just as any other conceptual system— by reflectively abstracting and relating operations which serve to connect experientially derived states. Here I am applying Piaget's notion of reflective abstraction (Piaget, 1950, 1952, 1970) to the researcher. As he or she watches a student ease through some problems and stumble over others, or successively ease and blunder through parts of a problem, the researcher asks himself "What can this person be thinking so that his actions make sense from his perspective? What organization does the student have in mind so that his actions seem, to him, to form a coherent pattern?" This is the ground floor of modeling a student's understanding. The researcher puts himself into the position of the student and attempts to examine the operations that he (the researcher) would need and the constraints he would have to operate under in order to (logically) behave as the student did. This is reflective abstraction.

One does this for each student in the investigation, and as soon as one begins to see a pattern in ones mode of explanation, the job must be expanded to reflectively abstracting the operations that one applies in constructing explanations. When the researcher comes to the point that he is reflectively aware of these operations, and he can relate one with another, he has his explanatory framework (of the moment). That is, he has isolated the components and relationships among components which allow for explanations of individual children's behaviors. The picture is certainly more complex, in that there are interactions between hypothesizing operations and schemas and collecting data, constructing explanations and abstracting a framework, and possible abstracting a framework and collecting data. But in principle, I feel the above captures the essence of the process of constructing models and frameworks. To do an honest job of this, however, I would examine specific researchers with the aim of characterizing their respective task environments, and develop explanations of what I see as their behavior, a framework for these explanations, etc.

Questions for judging reports of constructivist clinical research in mathematics education can now be made more precise. These are:

1. Does the report specify a framework for constructing models? If it does not, then the author has put the reader in the hopeless position of trying to understand where the author's explanations (models) come from, as well as making it well nigh impossible for him to attempt to test the viability of the author's mode of explanation for himself.

2. Are the prototypes made clear? The author's models can only be judged if the reader has a clear picture of what they attempt to explain. The only way to accomplish this, that I see anyway, is to include key excerpts from the interviews.

3. Is the framework grounded in data? In the process of reflectively abstracting operations that would produce prototypic behavior, the modeler may "slip" and incorporate a portion of his own knowledge which is clearly inappropriate imputed to the student. Resnick (1979, p. 15) does this when she explains a second-grader's transformation of "one", "two", and "three" into "ten", "twenty", and "thirty" as multiplying by ten, when it would have been more appropriately described in terms of linguistic transformations or subitizing.

4. Are the models viable? Does the author's framework, when applied to particular students, result in input-output relationships that are contradicted by the data? If so, how critical is the contradiction? The degree of "critical-ness" depends on the extent to which the framework would have to be modified in terms of organization and structure.

5. Are the models sufficient? If all prototypic behavior is accounted for by the models, then they are entirely sufficient. However, this is hardly ever the case. One must decide if there is any significant unexplained behavior. If a student was to solve a problem one way and an isomorphic problem (from the reader's perspective) in an entirely different way, and this was unexplained by the author's model, then it must certainly be judged insufficient.

These five criteria are offered as a starting point. They are aimed at the end product of a research investigation. Some researchers might argue that an author should also give an historical account of the development of his framework (e.g., Stake, 1978) I would agree that this would be helpful for the reader to come to an understanding of the author's framework, but should not enter into judgments of the report itself, given one does understand it. It may be strategic to include such an account, for if the reader doesn't understand a framework he can hardly judge the author's report. However, I don't see it as being necessary.

Finally, by these criteria, very few reports of clinical research would be judged acceptable; those to which they apply have come largely from the University of Illinois, under the direction of Davis, Easley, or Stake, and from the University of Georgia under the direction of Steffe. However, the Zeitgeist of the moment seems to be a trend toward examination of fundamental positions implicit in the status quo, and as a result I expect constructivist clinical research to become more prominent in mathematics education. Before such a shift may come about, though, there are fundamental conceptual and methodological problems that must be addressed. This was the purpose of our symposium.[3]

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[3] The phrase "our symposium" refers, of course, to the AERA session at which this paper was presented.

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