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## PERSPECTIVES ON COLLABORATIVE RESEARCH IN MATHEMATICS EDUCATION WITH INTERDISCIPLINARY CONNECTIONS

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### Introduction

In this paper, I discuss my experiences with collaborative research and how interdisciplinary research has always been a major part of the collaborations in which I have engaged. Collaborative research has much to recommend interdisciplinary research, but the concept has been distorted in recent documents. For example, to enhance research culture, the College of Education at the University of Georgia has published a strategic plan in which a basic action item is the initiation of strategic research collaborations and partnerships with faculty and organizations inside the College and outside the College, where the Associate Dean for Research is the person primarily responsible<sup>1</sup>. Why I consider this action item a distortion of the concept of collaborative research will become clear as I develop the concept. Initially, suffice it to say that in my experience, it is individuals, not institutions, who initiate and do collaborative research. Although it cannot be institutionalized, collaborative research can and must be supported by institutions when it might occur.

According to Dictionary.com, to collaborate means to work, one with another; to cooperate, and collaboration refers to the act or process of collaborating. In my work with Ernst von Glasersfeld, a world-renowned epistemologist, and John Richards, a philosopher of mathematics, we did indeed work one with another on the project, *Interdisciplinary Research on Number [IRON]*. But in our efforts to collaborate, there were clashes among the members of the interdisciplinary research team that von Glasersfeld (2005) perhaps summed up best in describing our activity when watching video-taped teaching episodes that I had conducted with 1<sup>st</sup> and 2<sup>nd</sup> Grade children.

He [Les Steffe], a graduate student of his [Pat Thompson], the philosopher John Richards, and myself would spend countless hours viewing these tapes and trying to agree on what we gathered from them. We had heated arguments and for all of us it was a powerful lesson, hammering in the fundamental fact that what one observer sees is not what another may see and that a common view can be achieved only by a strenuous effort of mutual adaptation. (p. 10)

I recollect these clashes all too well and offer them as an antidote to those who interpret a research collaboration as a group of faculty members harmoniously working together in nirvana. But, then, what glue holds a research team together in the face of heated arguments and the strenuous effort that is involved in mutual adaptation?

### Preconditions for Collaborative Research

Because of the complexities involved in understanding how the human mind constructs something so complex as mathematics, I consider an interdisciplinary disposition a necessity for

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<sup>1</sup> <http://www.coe.uga.edu/dean/reports/index.html>

researchers to move the field of mathematics education forward. I had already established an interdisciplinary disposition when graduating from the University of Wisconsin in that I worked as a research associate in the newly established Research and Development Center for Cognitive Learning where I worked with Henry Van Engen. Although Van Engen had earned a Ph.D. in analysis from Michigan, he spent his career working in mathematics education primarily because he couldn't find a position in mathematics during the great depression of the last century. Working with Van Engen was in part an affirmation of my understanding of the role of mathematics in mathematics education—one doesn't do mathematics education without a deep understanding of mathematics. But I also learned from him that “knowing mathematics” is not sufficient.

Along with his colleague Mike Roszkopf, who also had a major influence on my thinking, Van Engen read widely in psychology, which was quite common among mathematics educators of that time (Fehr, 1953; Roszkopf, 1953; Roszkopf, Steffe, & Taback, 1971; Van Engen, 1949, 1953). In fact, the cognitive development theory of Piaget and his collaborators had a major impact on the modern mathematics movement of the 1960's in that their genetic structures served as a justification for the emphasis on mathematical structure in the modern mathematics curricula (Bruner, 1960). The decade of the 1960's was definitely a heady time in the history of the field, a time during which interdisciplinarity in the field was transformed from the connectionism and/or behaviorism that dominated the field during the first half of the last century to a much broader and deeper conception of mind.

In what follows, I argue that, in view of the historical relation between mathematics education and psychology, the members of a collaborative research team should either have already established an interdisciplinary disposition prior to engaging in collaborative research or engage in collaborative research to develop such a disposition. The members of the team also should have established problems that they want to solve or goals they want to reach. Further, there may be dissatisfaction with their methods of doing research as well as with the analytical constructs being used in building explanatory models<sup>2</sup>. Finally, members of the team might turn to collaborative research out of dissatisfaction not only with their own progress, but also with the state of their disciplines as a whole. To exemplify these four preconditions, I revisit my efforts to explain children's construction of number prior to working in IRON with von Glasersfeld and Richards.

### **Dissatisfaction with Mathematics Education circa 1970**

When I received my Ph.D. from the University of Wisconsin, my understanding of mathematics education as an academic field had its sources in my study of mathematics and in what I later came to conceive of as a strange attempt on my part to use Fisherian experimental techniques in the study of children's thinking and learning. Other than feeling as if I was doing pseudoscience, there were two events that were pivotal in initiating changes in my concept of mathematics education as an academic field after joining the faculty of mathematics education at the University of Georgia in 1967.

#### **The Research and Development Center for Educational Stimulation**

The first was being asked to join the efforts of the Research and Development Center for Educational Stimulation that was housed in the College of Education. The main research hypothesis of this Center was that preschool educational programs for three, four, and five year old children would accelerate their cognitive development from Piaget's preoperational to his concrete operational stage. The director of this R&D Center, Dr. Warren Findley, was a psy-

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<sup>2</sup> I use the term “model” to refer to, “a conceptual construct that is treated as though it gave an accurate picture of the real world” (von Glasersfeld & Steffe, 1991, p. 95).

chometrician whose work was based primarily on the empiricist assumptions of classical test theory. These assumptions were manifest in the directives to support the teachers in a preschool that was the site of the experiment by supplying materials they could use to teach number and measurement to their children.

“Teaching” number and measurement to preschool children constituted a conflation of genetic and mathematical structures that was prevalent in the attitude of many of the curriculum developers of the modern mathematics programs. Piaget was considered to be an observer rather than a teacher (Educational Services Incorporated, 1963) and it was thought that had Piaget observed the mathematical thought of children who participated in the modern mathematics programs, he would have realized the elasticity of the limits of their cognitive processes. The mathematical knowledge of the children that Piaget and his collaborators had documented in the basic books such as “The child’s conception of number” (Piaget & Szeminska, 1952), “The child’s conception of geometry” (Piaget, Inhelder, & Szeminska, 1960), and “The child’s conception of space” (Piaget & Inhelder, 1963) was essentially discounted and curriculum developers did not regard children’s mathematical knowledge as part of the curricula they developed. Rather, the mathematical knowledge of children that Piaget and his collaborators produced was regarded as belonging to the field of developmental psychology and it had to do with school mathematics only to the extent that it served as a rationale that concrete operational children were ready to and could engage in structural thinking (e.g., Dienes, 1964)<sup>3</sup>.

Although I hadn’t myself taught 3, 4, and 5 year-old children, I just did not accept Bruner’s (1960) famous hypothesis that, “Any subject can be taught effectively in some intellectually honest form to any child at any stage of development” (p. 33), even if “any subject” was interpreted as the mathematics of operational children and the stage of development was the preoperational stage of development of most 3, 4, and 5 year-olds.

I was extremely perplexed by the research project because, although it was well before the English translation of Piaget’s book on reflective abstraction by Robert L. Campbell (2001), I understood that reflective abstraction was the construct that Piaget used to explain the transition from the preoperational to the operational stage.

It is then necessary to suppose that abstraction starting from actions and operations—which we shall call “reflective abstraction”—differs from abstraction from perceived objects—which we shall call “empirical abstraction” (assuming the hypothesis that non-perceptible objects are the product of operations)—in the sense that reflective abstraction is necessarily constructive. (Piaget, 1966a, pp. 188-89)

The source of my skepticism resided not only in the assumption that the limits of children’s cognitive processes are elastic, it also resided in the assumption that spontaneous development could be accelerated by children’s specialized “mathematical” interactions with teachers. That is, I was highly skeptical that the processes that are involved in reflective abstraction that produce the fundamental quantitative operations in human beings could be set in motion by such specialized interactions. There was no acknowledgment of the products of spontaneous development in what we were being asked to do and I construed my involvement as a violation of scientific as well as professional ethics<sup>4</sup>.

<sup>3</sup> The curriculum developers apparently did not understand that Piaget’s genetic structures were his formalizations of what he observed children do. He made no claim that children were aware of the structures that he saw in their mathematical behavior.

<sup>4</sup> I also consider the lack of use of the products of spontaneous development in school mathematics today as a violation of scientific and professional ethics.

**Establishing Problems I Could Not Solve**

Using Piaget's conception of the numerical operations of concrete operational children, I decided to involve myself in teaching preoperational kindergarten children in their construction of number in order to conduct my own personal test of the hypothesis of the R&D Center<sup>5</sup>. Piaget's (1966b) analysis had led him to the following position:

The development of number does not occur earlier than that of classes (classificatory structures) or of asymmetrical transitive relations (serial structures), but there is, on the contrary, a simultaneous construction of classes, relations and numbers. (p. 259)

His minimal criterion for children's construction of number was operative one-to-one correspondence that, in his model, was made possible by the emergence of the arithmetic unit. The stages in the construction of one-to-one correspondence exactly paralleled those of the construction of operative classification and seriation (Piaget, 1966 a & b), so I focused on bringing forth classifying, ordering, and one-to-one correspondence actions in such a way that might engender their interiorization by the involved children. For example, after asking children to make a train of toy railroad cars, I would ask them to find all of the cars that were before a given car, all that were after a given car, which one was just after or just before a given car, or which ones were between two cars. I also supplemented these questions by substituting "how many" for "which ones" when appropriate as well as asking the children to find the next three cars after a given car or the fourth one after a given car, etc. I also asked them reversible questions that involved counting in opposite directions. In the case of one-to-one correspondence, I presented the children with problematic situations like getting as many spoons as there were forks, one for each one. I also experimented with inducing reciprocal and transitive reasoning in a way similar to how I experimented with "before" and "after" as inverse actions in the ordering tasks.

I never reported the results of these experiments primarily because of the major constraints that the children presented to me as I worked with them. It wasn't that I had justified my skepticism about directly teaching number and measurement to preoperational children. Rather, I realized that I had experienced a reality *for which I had no model* and I had no language to speak or write about this reality other than my observations of what the children couldn't do. *By teaching children, I had constructed a problem for which I had no solution.* These children could have been classified as preoperational children in Piaget's framework but that did not solve my problem because it did not supply me with an operational model that I could use to explain their mathematical thinking. In short, I knew what they couldn't do mathematically with respect to Piaget's framework, but I didn't know what they could do as I had no operational model of their mathematical minds.

The constraints that I experienced when working with these pre-operational children constituted the second major event that was pivotal in changing my concept of working scientifically in mathematics education. My experiment failed not because the children did not interiorize their goal-directed actions. *It failed because in most cases I could not even engender the children's goal-directed actions.* I will never forget how inappropriate it seemed to ask the children to use one-to-one correspondence as a means of simply comparing the pluralities of two perceptual collections. So, it goes without saying that making indirect comparisons between two such collections by means of transitive reasoning seemed far removed from the children.

At that point, I realized that Piaget's hypothesis that mental operations are the result of the

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<sup>5</sup> This signaled the beginnings of the constructivist teaching experiment, a methodology distinctly different from Piaget's clinical interview.

interiorization of actions that are introduced by the subject did not necessarily involve those specialized actions that an adult might consider as precursors to the operations. That is, I had no reason to believe that classifying, seriating, or corresponding actions are involved in the construction of number. The upshot of this realization was that I simply did not know what actions or interactions of children are interiorized to produce Piaget's seriated classes or number.

It appears to be extremely difficult to define "mathematical contexts," especially with reference to young children. Given the very general basis for construction of logical-mathematical operations ... almost any situation that can be commented on, asked about, indicated as desirable, etc., can lead to actions, utterances, gestures, or other communicative acts that have something to do with logic or mathematics. (Sinclair, 1990, p. 25)

Sinclair's comment brings Piaget's claim that the mind organizes the world by organizing itself into focus. It seems that an observer would indeed be hard pressed to identify communicative acts or situations that give rise to the operations that Piaget identified. This insight led to the realization that Piaget had not solved the problem I faced of not having a model of the realities that "kicked back" at me when I taught these preoperational children. Although I agreed with how Piaget (1980) regarded spontaneous development, there were few suggestions that would be useful in mathematics education concerning the constructive activities that are involved in the few years that a child;

Spontaneously reconstructs operations and basic structures of a logico-mathematical nature, without which he would understand nothing in school. ... He reinvents for himself, around his seventh year, the concepts of reversibility, transitivity, recursion, reciprocity of relations, class inclusion, conservation of numerical sets, measurements, organization of spatial referents. (p. 26)

### **Dissatisfaction with Research Methods and Analytical Constructs**

My short tenure with the R&D Center for Educational Stimulation did engender a shift in how I tried to use Piagetian theory in the mathematics education of students from using Fisherian experimental techniques to actually teaching children. Given my doubts concerning children's construction of number, a return to the "uninterpreted data" was in order. Professor Larry Hatfield and I had joined the Project for the Mathematical Development of Children, a joint NSF project between Florida State University and The University of Georgia, and we both mounted what to my knowledge were the first teaching experiments in mathematics education in the United States. Along with two doctoral students, I taught two classes of first graders for one school year (Steffe, Hirstein, & Spikes, 1976). There were three major findings of this study.

- The first was that children counted independently of our suggestions to solve their arithmetic situations.
- The second was that there were substantial differences in the tasks that children could solve and in the units that children created and counted in their solutions.
- The third was that we observed children who could indeed count-on but who could not solve Piaget's class inclusion problem.

All three findings were anomalies with respect to Piaget's grouping structures. The first two

were anomalies because the grouping structures did not explain children's counting or the units that are involved. The third was an anomaly because I considered that counting-on indicated that children had constructed number, so the children should have also constructed the class inclusion operation. In retrospect, these three observations were the basis of a progressive problem shift in the sense that Lakatos (1970, p. 118) explained. The challenge was to construct a model of children's numerical operations in which these three observations as well as a host of others were explained. It is important to note that this comment is made in retrospect. At the time, I abandoned my attempts to apply Piaget's grouping structures in the mathematics education of children and instead mounted a second teaching experiment to explore the role of counting in their mathematical education. Little did I realize it then, but this marked the beginning of 15 years of productive interdisciplinary research in IRON.

Not only did I abandon my attempts to apply Piaget's Grouping Structures in mathematics education, but I also abandoned my attempts to use Fisherian experimental techniques in mathematics education research. Merging the practice of teaching with the practice of research was a huge breakthrough for me because my identity as a mathematics teacher and a mathematics education researcher were becoming integrated as essential components in a larger conceptual complex that I constructed over the next fifteen years, a conceptual complex that I am still in the process of reconstructing and extending<sup>6</sup>.

My conception of doing science involved developing and using explanatory constructs, a conception that I developed through my studies of physics in my undergraduate years. This was the reason why I was so attracted to Piaget's work. He offered explanations of children's thinking as well as a methodology for observation that became known as the clinical interview.

The focus of the clinician is to understand the originality of [the child's] reasoning, to describe its coherence, and to probe its robustness or fragility in a variety of contexts. (Ackermann, 1995, p. 346)

But, I had just abandoned my attempts to apply Piaget's grouping structures and I was involved in teaching children for rather extended periods of time to explore the role of counting in children's mathematical education. So, I had abandoned the science that I was trying to apply and the clinical interview could not be my sole methodology. The only explanatory constructs of children's mathematics that were available to me was my own conception of mathematics such as Hausdorff's (1962) theory of cardinal and ordinal number and their interpretations, and Van Engen's (1971) "empty hat" approach to cardinal number<sup>7</sup>. I had no other model of children's counting, so it would have been extremely easy to regress to the curriculum trials that were used during the era of the Modern Mathematics Movement of the 1960's where the curriculum development efforts were based solely on how adults understood mathematics<sup>8</sup>. However, what I was observing did not fit within my own conceptions and ways of operating with cardinal and ordinal number. I was able to construct living, experiential models of children's ways of operating, but I lacked the conceptual tools to construct scientific explanations. This lack of conceptual tools is the primary reason that I turned to interdisciplinary work with von Glasersfeld and Richards.

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6 I thank Professor Larry Hatfield for his substantial contributions to this essential change in my conceptions of what it means to do research in mathematics education.

7 The theory of cardinal number served as a basis for mathematical curricula in the elementary school and Piaget's grouping structures served as its psychological justification.

8 Max Beberman, "UICSM: Fifteen Years of Experimentation" in Beberman, M. & Folder, RS 10/12/1, Box 21, University of Illinois Archives.

### **Elements of a Successful Collaborative Research Program**

I will always be indebted to Professor Charles Smock of the Psychology Department at the University of Georgia for inviting me to a colloquium given by Ernst von Glasersfeld. In the colloquium to which I was invited, von Glasersfeld recounted a story published by Lettvin, Maturana, McCulloch, and Pitts (1959) that at once established a bond between the two of us that has lasted from then on up through the present time. When talking about a frog as a fly-catcher, he commented that:

The system [the frog's visual system] as a whole makes the frog an efficient fly-catcher, because it is tuned for small dark "objects" that move about in an abrupt fly-like way. In the frog's natural habitat, as we, who observe the frog see it, every item that possesses the characteristics necessary to trigger the frog's detectors in the proper sequence is a fly or bug or other morsel of food for the frog. But if the frog is presented with a black bead, an air-gun pellet, or any other small dark moving item, it will snap it up as though it were a fly. In fact, to the normal frog's visual apparatus, anything that triggers the detectors in the right way, is a "fly". (von Glasersfeld, 1974, reprinted in von Glasersfeld, 1987, pp. 106-07)

I was very excited by this story because it clarified why I found it necessary to work as a teacher in mathematics education research and why, as Max Born (1968) said, "Thus it dawned upon me that everything is subjective, everything without exception. That was a shock" (p. 162). The point of the story, of course, is that whatever is perceived is basically a composition of signals generated in our various sensory channels.

We are free, of course, to consider these original signals the effect of some outside causes. But since there is no way of approaching or "observing" these hypothetical causes, except through their effects, we are in the same relation to that "outside" in which the first cyberneticists found themselves with regard to living organisms—that is to say, we are facing a "black box". (von Glasersfeld, 1974, reprinted in von Glasersfeld, 1987, p. 107)

The frog story, which is a result of biological research, immediately resonated with me because it clarified my attempts at building models of the mathematical minds of the students that I taught. For me, or any observer, the mathematical minds of these students were my "black boxes" and I had just abandoned the models of them that I had been trying to use in their mathematical education as well as the methodology that I had been using in the applications. Further, the analogy between the frog and myself was simply staggering—whatever the frog established as a fly was due to the way the frog's perceptual apparatuses were designed, so whatever I established as a model of students' mathematical minds would be due to the way my own conceptual constructs were designed.

#### **The Reality of Children's Mathematics**

When teaching students, it doesn't take long to realize that one runs into constraints in interacting with them.

The constructivist is fully aware of the fact that an organism's conceptual constructions are not fancy-free. On the contrary, the process of constructing is constantly curbed and held in check by the constraints it runs into. (von Glasersfeld, 1990, p. 33)

Another way of saying this is that adults are constrained by students' mathematics in the sense that it "kicks back" at us in a way similar to how von Glasersfeld considers reality "kicking back." How to eliminate these constraints constitutes major problems for the researcher, and their solutions depend on learning how to engender students' solutions of their problems. In spite of assertions by others, von Glasersfeld has never denied reality nor do I deny the realities of students' mathematics even though I consider them as "black boxes".

The constructivist conclusion is unpopular. The most frequent objection takes the form of the accusation that constructivism *denies* reality. But this it does not. It only denies that we can rationally know a reality beyond our experience. (von Glasersfeld, 2007, p. 146)

How to build explanatory models of the "black boxes" that constitute students' mathematics [or the mathematics of the other] is a major problem confronting researchers in mathematics education. One does not engage in collaborative research teams to learn how to *apply* the concepts and methods of another discipline to mathematics education. Rather, one engages in research teams in order to *learn* how those one chooses to work with think, the tools they use in their research, and the significant authors they read so that one may use the concepts and tools of other individuals or disciplines in constructing explanations. *If it is a science at all, mathematics education is not and should not be regarded as an applied science.*

#### **Teaching as a Method of Scientific Investigation**

By means of constructing models of students' mathematics, mathematics education can at least in part be established as an academic field. But those who have attempted to read my accounts of children's number sequences might find it difficult to see how one might use these models when working with children. I can certainly empathize with such attempts because it is much like attempting to understand, say, geometrical transformations by simply reading definitions. By and large, it just doesn't work. And neither does an attempt to understand children's mathematics by means of reading explanatory models without having already constructed experiential models. In my opinion, a major issue in mathematics education concerns the extent to which mathematics educators have constructed living, experiential models of children's mathematics. As I see it, there are at least two major aspects concerning this issue.

**Experimental teaching of students.** The first aspect is the extensive and concentrated efforts that are involved in constructing living, experiential models of students' mathematics. Although it is critical to use teaching as a method of experimenting in order to understand students, it is even more critical to use teaching as a method of experimenting in order to understand changes in their ways and means of operating. It isn't sufficient to observe someone teaching children because the actions and interactions of the teacher/researcher are essential in students' construction of mathematics. More importantly, the actions and interactions of teacher/researchers, when coupled with students' actions and interactions, are essential in teacher/researcher's construction of students' mathematics and how it might be productively affected. So, unlike Piaget's minimal intervention methods, to construct how students construct mathematics teacher/researchers have to be centrally involved in the process.

In the words of the cyberneticist Fred Steier speaking of second-order cybernetics;

Approaches to inquiry  $\frac{1}{4}$  have centered on the idea of worlds being constructed  $\frac{1}{4}$  by inquirers who are simultaneously participants in those same worlds. (Steier, 1995, p. 70)



It is well accepted in cybernetics that a science of observed systems cannot be divorced from a science of observing systems because it is we who observe. The cybernetic approach and the radical constructivist approach converge on this very important point in that both emphasize our own subjectivity as observers. This places a very heavy responsibility on teacher/researchers because it places the *experience of the teacher/researcher* at the front and center in research that aims to construct models of students' mathematics (Steffe & Thompson, 2000a & b).

**Elimination of the duality between children and mathematics.** The second major aspect concerns the view that children and mathematics are two separate entities. Separating mathematics and students in this way has always placed school mathematics outside of the minds of the students who are to learn it regardless of how learning was conceived. Von Glasersfeld (1974) provides an extensive discussion of research that *undermines* the belief that the knower and the things of which, or about which, he or she comes to know are, from the outset, separate and independent entities. The basic research that he draws from is Piaget's account of the child's construction of the concept of an object that has some kind of permanence in his or her stream of experience. Rather than attempt to recapitulate Piaget's research and von Glasersfeld's interpretation of it, I encourage the reader to embark on his or her own journey through their intricate and elegant accounts of how the infant comes to be but one element or entity among others in a universe that he or she has gradually constructed for him-or her-self out of the elementary particles of experience. This powerful insight into the child's construction of his or her ordinary items of experience serves as a "demonstration" that Cartesian duality is untenable: That is, the belief that from the outset the knower and the things of or about which he or she comes to know are separate and independent entities is not viable.

If indeed it is accepted that the student and mathematics are not two separate and independent entities, what does this mean for school mathematics and for the activities that are involved in specifying such (a) mathematics? Ernest (1996) [as well as Janvier (1996)] has argued that radical constructivism has little to offer for selecting the mathematics that constitutes school mathematics.

What selection from the stock of cultural knowledge is valuable to teach? (Here again, I pause to consider whether radical constructivism is even able to pose this question.). (Ernest, 1996, p. 346)

The implicit assumption in Ernest's comment is that "cultural knowledge" is that knowledge produced through the mathematical activity of adult mathematicians. This raises a serious issue concerning the commensurability of the mathematical knowledge of adult mathematicians and children's mathematical knowledge<sup>9</sup>. Given that the Cartesian duality is untenable, a major shift is called for in the problem of constructing a mathematics that constitutes school mathematics.

Rather than focus on the selection from the stock of cultural knowledge that is valuable to teach, we adults should concentrate on learning how to use our mathematical knowledge in mathematical communication with children. Our goal should not be to transfer cultural knowledge to children in the particular way that we understand it. Rather, our goal should be to learn how to engender children's productive mathematical thinking and how to build explanatory models of that thinking. This may seem to be a rather ambitious goal, but we have to remember that children spontaneously construct their object concepts and a universe in which they are but

<sup>9</sup> The issue is analogous to the separation between mathematical structures and genetic structures that plagued the modernist movement during the last century.

one element out of the elementary particles of experience. Children enter school having already engaged in constructive activity of enormous magnitude, so their ability to construct a universe of mathematical concepts of comparable magnitude should not be an issue. What is at issue, however, is our disposition toward children as productive mathematical thinkers, our ability to engender children's productive mathematical thinking, and our ability to construct explanatory models of children's mathematical thinking that portrays it as a coherent and internally consistent mathematics.

### **Conceptual Analysis**

Not everyone on a research team needs to have constructed living, experiential models of children's mathematics when engaging in collaboration with other members of the team. But there needs to be at least one member of a team who has engaged or who will engage in this activity in attempts by the team to construct models of children's mathematical minds and how these minds evolve and change over time in the context of specialized interactions. In IRON, von Glasersfeld initially led the way when we engaged in conceptual analysis of the living, experiential models that I had constructed by means of teaching children. Conceptual analysis is the primary method of building explanatory models of students' mathematics and the constructive activity that produces it. Von Glasersfeld learned conceptual analysis through his work on the analysis of word meaning with Silvio Ceccato in the Italian operational school in Milan, Italy (Accame, 2007). In a conceptual analysis, the question is, "What mental operations must be carried out to see the presented situation in the particular way in which one is seeing it?" (von Glasersfeld, 1995, p. 78). Von Glasersfeld told me that the opportunity to continue to engage in conceptual analysis was what attracted him to the interdisciplinary work of IRON. In his words, "It was my interest in conceptual analysis that made it attractive to watch children get into arithmetic. My paper on Units and Number could not have been written without the clinical interviews with the children" (Personal Communication, May 26, 2010).

In his work in IRON, von Glasersfeld first conducted a first-order conceptual analysis of his own operations that produce units and number (von Glasersfeld, 1981). The goal of a first-order conceptual analysis concerns specifying the mental operations that produce particular conceptions of the analyst. It is an analysis of first-order models, which are models the analyst has constructed to organize, comprehend, and control his or her experience; that is, the analyst's own knowledge. The distinction I am making between the mental operations that produce particular conceptions of the analyst and those conceptions is crucial in understanding how the knowledge of researchers can be used in research programs concerned with exploring the operations by means of which human beings construct their conceptions (Steffe, 2007). It is crucial because these operations are involved in producing second-order models, which are models an observer constructs of the observed person's knowledge in order to explain their observations (Steffe, von Glasersfeld, Richards, & Cobb 1983, p. xvi). Because the goal of the analyst in constructing second order models concerns constructing conceptual operations that explain the observed language and actions or interactions of the observed person, I refer to it as a second order analysis<sup>10</sup> (cf. Steffe, von Glasersfeld, Richards, & Cobb, 1983). The reciprocal relationship between first- and second-order analyses is basic in constructivist research programs because it illustrates that researchers and their ways and means of operating and observing constitute the research programs.

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10 A goal of Piaget's genetic epistemology was to provide an ontogenetic explanation of mathematics, and this is one of my goals as well.

### Epistemic Students

The primary goal of conceptual analysis is to produce second-order models that constitute epistemic students. Without differentiating the epistemic student and the psychological student, embedding studies of mathematical learning in the context of using teaching as a method of scientific investigation might be interpreted as a more or less empirical enterprise and as generating a whole industry of empirical research on mathematical learning, to paraphrase Michael Cole's (2004) comments concerning the training studies of the 1960's that were conducted to prove Piaget wrong. But the emphasis on conceptual analysis and the models of students' mathematics that it produces countermands this interpretation.

It is useful to make a distinction between Piaget's epistemic subject and what I refer to as an epistemic student. An epistemic subject is, "that which is common to all subjects at the same level of development, whose cognitive structures derive from the most general mechanisms of the co-ordination of actions" (Piaget, 1966b, p. 308). The development of which Piaget spoke of was spontaneous development to which four main factors contribute; maturation of the central nervous system, physical experience, self-regulation, and social transmission (Piaget, 1964). Of these four main factors, although the first three are sometime considered in mathematics education, social transmission is often considered within the classroom to be the most important factor. Piaget, however, did not mean social transmission as transmission from an objective social reality to the individual as is common in mathematics education. That meaning would be inconsistent with his reaction to the claim that knowledge and language are performed in society: "The preformation of social characteristics is, as in other contexts, nothing but a common sense illusion consolidated by Aristotelian philosophy of potentiality and action" (Piaget, 1965/1995, p. 340). Instead, Piaget thought of social transmission interactively; "Individuals establish equilibrium among personal schemes of action and anticipation as they interact in mutual adaptation—as constrained by the local limitations of their abilities to accommodate those very schemes" (Piaget 1965/1995; Steffe & Thompson, 2000a, pp. 192-93).

This concept of "social transmission" is basic in the construction of living, experiential models of students' mathematics. The concept is also basic in the processes of the construction of epistemic students. An epistemic student consists of inferred mathematical schemes of action and operation of students and the accommodations in them that are brought forth and sustained by the students' teachers (cf. von Glasersfeld, 1980, for an account of schemes). To offset an interpretation of the epistemic student as a static model, I interpret schemes as instruments of interaction. When I think of children's number sequences, for example, I conceptualize my interactions with the specific children who were involved in my construction of the number sequences. So, to elaborate, for me, epistemic students are dynamic organizations of schemes of action and operation in my mental life. But without living, experiential models of children's ways and means of acting and interacting, the models the epistemic students comprise would have no meaning nor would changes in them. In fact, I find it not possible to construct epistemic students without constructing living, experiential models of students' mathematics. Perhaps contrary to conventional wisdom in doing science in mathematics education, I have always found that building living, experiential models of students' mathematics through intensive and extensive interaction with them precedes the activity of constructing explanations<sup>11</sup>.

But what of the other three factors involved in spontaneous development? They are all

<sup>11</sup> Professor John Richards' goal when joining IRON was to establish a justification for mathematics based on its construction in individuals. In retrospect, he was concerned primarily with the construction of epistemic students.

implicated in the concept of epistemic students. Self-regulation is already implicated by equilibration among personal schemes of action and anticipation as individuals interact in mutual adaptation in the concept of “social transmission”. Further, when considering that mathematics education is concerned with educating children from three to seventeen years of age and beyond—from early childhood through young adulthood—it always surprises me that embryogenesis is rarely a topic of conversation at mathematics education professional meetings even in the case of the Psychology of Mathematics Education meetings. Are we to believe that there are no physiological changes in human beings throughout this age range that open new possibilities for mathematical thinking and learning? In all of the teaching experiments that I have done with children, I have never been able to separate the contributions of spontaneous development and specialized interactions in children’s construction of their number sequences (Steffe, et al. 1983). For example, when children are yet to construct arithmetic units, which I have come to believe is a product of spontaneous development, attempts to engender that construction can be just as futile as were the attempts of the R&D Center for Educational Stimulation to accelerate the construction of Piaget’s concrete operations (Steffe & Cobb, 1988). Finally, Piaget’s physical knowledge refers to products of children’s interactions in their physical environments. The mathematical knowledge that these interactions precipitated has been documented (Piaget, Inhelder, & Szeminska, 1960; Piaget & Inhelder, 1963; Piaget, 1969). So, although I consider human beings as interactive organisms, interaction is constrained by spontaneous development.

The living, experiential models of students that a researcher constructs by means of interacting with them are what family therapists call the internalized other. They are not simply images of how the students look. Rather, they consist of images of students’ actions and interactions that one abstracts. Epistemic students are interiorized others—the dynamic organization of schemes of action and operation in one’s mental life. Further, when interacting mathematically with particular students, I consider my experience of their mathematics as *mathematical experiences*. I consider students as rational beings and the explanations of their mathematics *do belong to mathematics*. These explanations, which constitute epistemic students, must be considered as serious and important mathematics.

### **Progressive Research Programs**

Research teams are not easy to sustain and they do disintegrate for any number of reasons. Von Glasersfeld retired from the University of Georgia circa 1986 and moved to the University of Massachusetts to work with Jack Lockhead, and John Richards left the University of Georgia and launched an educational consulting business.<sup>12</sup> Upon the departure of Richards and von Glasersfeld from the University of Georgia, I had constructed models of children’s number sequences and, in retrospect, perhaps more importantly, I had constructed models of the numerical concepts and operations children construct using their number sequences (Steffe, 1992, 1994). So, I felt ready to attack the intractable problem of children’s construction of fractions in which I had been long interested. However, I was yet to construct epistemic students in this contentual area.

Apparently, the lack of models of children’s fractional concepts and operations was not restricted to myself as attested by a comment by Davis et al. (1993) that, “the learning of fractions is not only very hard, it is, in the broader scheme of things, a dismal failure” (p. 63). I was in a similar position with respect to children’s construction of fractions that I was in with respect to children’s construction of number when working in the Research and Development

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Center for Education Stimulation upon matriculation at the University of Georgia's Department of Mathematics Education. But I had a starting point because I thought that children's number sequences should serve in their construction of fraction schemes. In fact, this became known as the reorganization hypothesis—"children's fractional schemes can emerge as accommodations in their numerical counting schemes" (Steffe, 2009, p. 1). But I had little idea of what those schemes might consist of or how they might be constructed.

Fortunately, Professor John Olive joined the faculty of mathematics education at the University of Georgia and the two of us formed the project that has become known as "The Fraction Project". I regarded it as an extension of IRON, but John had not worked in IRON so it was appropriate to refer to the project with a new title. It was an extension of IRON for me because the reorganization hypothesis was our original hypothesis. It is crucial to stress that we had no models of children's fraction schemes or of their construction beyond the hypothetical schemes that had their sources in our first-order analyses of our own conceptions of fractions. So, along with several doctoral students that included Ron Tzur, Barry Biddlecomb, and Azita Manoucheri, we set out to work with eight children throughout their 3<sup>rd</sup>, 4<sup>th</sup>, and 5<sup>th</sup> grades in a teaching experiment (Steffe & Olive, 2010). I can't emphasize enough the necessity of engaging in deep and sustained interactions with children over extended periods of time in order to construct living, experimental models of their ways and means of operating and changes that one might engender in those ways and means. The primary goal is to learn to think like the children with whom one is interacting and *to find problems that can only emerge in that context*.<sup>13</sup> In doing this, one is able to learn the "lay of the land" and experience regularities and/or constraints both within and across the children with whom one is working without necessarily being able to explain the conceptual mechanisms that might produce the regularities and/or constraints.

At the start of the teaching experiment, the reorganization hypothesis constituted a problem shift. That the hypothesis was confirmed (Steffe & Olive, 2010) is, according to Lakatos, essential to claim that the IRON research program was a progressive program; "Finally, let's call a problem shift progressive if it is both theoretically and empirically progressive, and degenerating if it is not" (Lakatos, 1970, p. 118). For Lakatos, a problem shift is theoretically progressive if a superseding theory is posited that has excess empirical content over its predecessor and it is empirically progressive if some of the excess empirical content is corroborated.

### Final Comments

I have argued that collaborative research in mathematics education should be based in interdisciplinary research. However, two or more researchers working together who are simply in different disciplines does not constitute the interdisciplinarity that is needed to constitute a team as a collaborative research team. For example, it is not the case that a practicing mathematics educator and a practicing mathematician can automatically engage in interdisciplinary research. Mathematics education is often thought of as the practice of mathematics teaching, which qualifies most mathematicians as practicing mathematics educators. It is rare, however, to find practicing mathematicians engaging in mathematics education research just as it is rare to find practicing mathematics education researchers engaging in mathematics research. The two fields are indeed quite different fields of research and even fields of teaching practice. Still, my experience is that it can be very important for mathematicians sympathetic with precollege mathematics

<sup>13</sup> When writing a prospectus for doing research, the main goal is often to state the problems on which one is going to work. I find this goal misdirected when there is an assumption that one can know the results of experience prior to experience.

education to engage in interdisciplinary research with mathematics educators.

Historically, when mathematicians have become engaged in collaborative research with mathematics educators, the goal usually has been to develop such things as curricular standards or even full-fledged mathematics curricula such as was developed during the modern mathematics movement of the last century. In these cases, the mathematicians and the mathematics educators have not been on an equal footing and mathematics as known by the mathematicians has been the norm in constituting school mathematics. What has been missing in these asymmetrical collaborations is a mathematics education that has been constituted as an academic discipline. If mathematics education is to distinguish itself as an academic discipline from, say, science education or English education, there must be a content distinction. In the past, this distinction has been served mainly by mathematics. My argument throughout the paper has been that to distinguish mathematics education as an academic field, we must reach beyond Ernest's (1996) "cultural knowledge" of mathematics produced through the mathematical activity of adult mathematicians to a "cultural knowledge" of children's mathematics that is produced through the model building activity of collaborative research teams. This is basic in any consideration of mathematics education as an academic field, and it can profitably include collaboration with mathematicians.

### **Learning from Others**

The most important thing that occurs in collaborative research is the transformations in the knowledge of the members of the team. More simply, learning from others is the primary reason to engage in collaborative research. When considering that the human mind is necessarily more complex than mathematics it produces, this provides a measure of the complexity of mathematics education. So, the interdisciplinary focus in collaborative research is not something that should be considered as nice but nonessential. Rather, an interdisciplinary focus in collaborative research is essential to move the field forward toward a more adequate mathematical education of precollege students.

I like to think of collaborative research as working together but alone, if that makes any sense. Perhaps restating Piaget's concept of social transmission is helpful in understanding what I mean: "Individuals establish equilibrium among personal schemes of action and anticipation as they interact in mutual adaptation—as constrained by the local limitations of their abilities to accommodate those very schemes" (Piaget, 1965/1995). It is essential that each researcher maintains autonomy in the research team and maintains an equal footing with the other members of the team. Maintaining autonomy does not mean isolating oneself from others. Rather, it means maintaining the viability of one's schemes of action and operation by means of the accommodations in them that follow on from interactions with other members of the team. Perhaps what I mean is best expressed by von Glasersfeld's (2005) comment that I quoted earlier that, "[we] would spend countless hours viewing these tapes and trying to agree on what we gathered from them. We had heated arguments and for all of us it was a powerful lesson, hammering in the fundamental fact that what one observer sees is not what another may see and that a common view can be achieved only by a strenuous effort of mutual adaptation" (p. 10). The heated arguments attest to the local limitations in our abilities to accommodate the schemes we were using in assimilation of not only the video-recorded material, but also the language of each other as we made our own interpretations of the children's language and actions. It is these accommodations that I believe are the most valuable outcome of collaborative research.

### **Viability Rather than Validity**

I pointed to dissatisfaction with the state of the field of mathematics education as well as dissatisfaction with my own progress as a precondition for the collaborative research in which I engaged. For me, both of these sources of dissatisfaction had origins in the concept of truth as contained in the Sifting and Winnowing plaque at the entrance to Bascom Hall at the University of Wisconsin from which I graduated.

Whatever may be the limitations which trammel inquiry elsewhere, we believe that the great State University of Wisconsin should ever encourage that continual and fearless sifting and winnowing by which alone the truth can be found. (Taken from a report of the Board of Regents in 1894)

The text of this plaque has both inspired and debilitated me throughout the years since I graduated. The primary source of inspiration resides in the image about how one might conduct one's professional life. But it was also debilitating because it seemed to me that the truth was always on the other side of the divide between what a human being could know and what was "there" on the other side to be found. I could not understand by what means I could possibly cross that divide and it seemed that truth was unattainable. Upon becoming immersed in Piaget's constructivism, I became aware that the Cartesian duality between mind and reality on which the plaque was based dissipated for me in that we human beings construct our own experiential realities. This was very liberating and heady because I realized that the models of children's mathematics that I constructed didn't need to be true in that they didn't need to be considered as matching what goes on in the heads of children. They did not need to be valid, just viable.

Just as the student and mathematics are not two separate and independent entities, a researcher's models of a student's mathematics and the student's mathematics are not two separate and independent entities. As long as those models fit within my experience of children's mathematical language and actions they are "good enough" not only to explain what might be going on in the heads of children, but also "good enough" to make predictions about where I might try to take children I am charged with teaching and if they would be able to traverse the journey with me. Such predictions are crucial in corroborating the models one constructs in the sense that Lakatos (1970) spoke of corroborating new empirical content. Even more satisfying, however, are those occasions when I meet a child I have never met before and, after a few exchanges, feel that I understand the child's mathematical mind because my experience of the child's mathematical language and actions that I am able to bring forth fit within my models of epistemic students.

### **A New Revolution in Mathematics Education?**

There are many reasons that have been advanced for why the mathematics education of students is not as successful as it might be, but only rarely does one find even an allusion to what teachers make of children's mathematics. What teachers make of their students' mathematical language and actions is a very basic aspect of mathematics teaching, and it is definitely one place where mathematics education is failing worldwide. The teacher must construct the mathematics of the children she or he is charged with teaching by means of interacting with them because it is not given nor can it be given by the usual mathematics texts, curricular guides, or lists of standards. By means of constructing the mathematics of the children he or she is charged with teaching, the teacher *confers* mathematical existence on the children. Without such a conferral, the teacher necessarily proceeds as a solipsist and the only mathematical reality in the classroom is that of

the teacher. In this case, the teacher interprets the children's mathematical behavior relative to the mathematics text, curricular guides, or lists of standards and the children have no mathematical existence as autonomous and self-organizing entities apart from the teacher's projects.

The current standards movement is another source of dissatisfaction with mathematics teaching if for no other reason than it is homogenizing mathematics teaching in the schools. The following comments from a Georgia mathematics teacher for whom I have great respect perhaps serves as a "smoking gun" concerning this movement.

You cannot imagine the insanity of Math 1. Thank God I haven't had to teach it, but it's too much content, and the concepts are too advanced for most of the students. I attend a couple of collaboratives each month and hear the same horror stories from teachers everywhere - so it's not just us. Most of our Math 1 students stay after school for a couple of hours on each of two days a week for extra help - pretty much everybody is having to provide a lot of before/after school tutoring - but it is still just too much for them. Note that what we are trying to do is teach what used to be accelerated "college track" mathematics to everybody. Everyone just is not capable of (or interested in) doing this, and when teachers care about their students and their students' futures, it is heartbreaking - and most of the tears come from a feeling that we are doing something that is, well, basically immoral. We feel like we are destroying these kids' futures. That's the viewpoint of the standard class and with-support teachers. The accelerated teachers are even more upset - the same sense of doom about the future plus we are seeing kids who used to like mathematics decide they'd really rather not pursue any kind of math-centered career. It will be interesting to see what effect, if any, the national standards will have and whether the new state school superintendent will address these issues. It's a mess.

The goal of IRON was to start a revolution in mathematics education to fill the void left by the modern mathematics movement of the 1960's that was being filled by the back to basics movement of the 1970's, a movement that was based on classical empiricism and the continuing persistence of neo-behaviorism applied to curriculum and teaching (Steffe, Richards, & von Glasersfeld, 1981). Perhaps this conference, aimed at initiating three new, broadly-based Research Teams to be supported within the Wyoming Institute for the Study and Development of Mathematical Education, can be regarded as an impetus for starting another revolution in mathematics education?

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