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A reaction to "A Critique of the Selection of 'Mathematical Objects' as Central Metaphor for Advanced Mathematical Thinking" by Confrey and Costa

I would like to thank the International Journal for Computers in Mathematical Learning for suggesting that I submit a response to the article by Confrey and Costa (1996). My work in mathematics education has been an attempt to implement my belief that it is absolutely necessary to combine theoretical analysis with classroom practice and the gathering and analysis of data. As Confrey and Costa point out, there is a "real need for a widely respected theory of advanced mathematical thinking" and in the last decade, "...a body of work has emerged that sets out as its goal the description of advanced mathematical thinking."

I think that Confrey and Costa have provided a service to the community by thematizing the set of activities concerned with building and applying theoretical analyses so as to make these activities themselves into objects of our attention. It is important that we develop our theories in concert with discussions of the strengths and weaknesses of various perspectives. Therefore, I would like to offer this essay as a contribution to the discussion which Confrey and Costa have begun.

Confrey and Costa begin their discussion by catching the essence of recent collegiate mathematics education research and curriculum development in observing that its central theme has been "a discussion of the relationship between 'process' and 'object' in learning mathematics." I would go further by noting that this work has focused on the learner's *construction* of *mental* processes and objects and their relationships in order to use these constructs in making sense out of mathematical problem situations. But my adjustment is minor and may amount to no more than saying the same thing in a different way. I am in complete agreement with Confrey and Costa on this point.

However, within a few lines, they present a very different point (which echoes their title) in the following two statements.

> "In this paper, we wish to discuss the intellectual gains and costs of selecting mathematical objects as the central metaphor for a theory of advanced mathematical thinking" (Confrey & Costa, 1996, p. 140).

and,

> "We will argue that while the theory's presentation has been interesting and its goals admirable, selecting mathematical objects as the critical metaphor in the theory may be retrogressive for the field of mathematics education."

I think it is fair to say that these latter two statements characterize the entire paper of Confrey and Costa and that raises some questions because these two quotes say something very different from the first quote. The difference is between focusing on *mathematical objects* vs. investigating the *relationship* between *mental processes* and *mental objects*. There are three questions to which I will respond: (1) Is it a good idea to focus on mathematical objects in the reform of education in advanced mathematics? (2) Is it a good idea to investigate the role of construction of mental processes, mental objects and their relationships in working on this reform? and (3) Which of these two positions more accurately characterizes the work Confrey and Costa are considering?

In the first part of this essay, I will respond to all three of these questions. In the second part, I will consider a number of epistemological issues that are raised by various points made in the paper and state some views that differ from their positions. In the third part, I will mention some additional points on which I think these authors are perhaps mistaken and finally, I will add a few comments about the tone in which this discourse might take place.

Disclaimers and terminology

Before proceeding on this program, I want to say a word about the relation of my remarks to the views of the other people Confrey and Costa are talking about.

On the first point, let me note that Confrey and Costa list several authors and state that their work in this area is based on a sufficiently common set of ideas to justify analyzing the entire set of research under a single rubric. Although there is a large measure of commonality, there are also important differences and I am not convinced that they are insufficient to justify separate treatments. But this might be only my own bias. It is unfortunately the case that we are often more passionate and more enthusiastic in our disputes with those with whom we have a large measure of agreement than with those we totally oppose. Perhaps my feeling of a slight discomfort with the lumping is just this effect. In any case, it is not my intention to pursue further in this essay the question of commonality of ideas amongst those whose work Confrey and Costa are reviewing.

Nevertheless, there are a number of instances in which my views are very different from the views that Confrey and Costa ascribe to the people they are talking about. I suspect that the others in their list may have a similar reaction, but I prefer to let them speak for themselves.

Therefore, I will give examples mainly from my own publications, referring to the work of other individuals only when it is essential that I do so. Although this will lead to a preponderance of my own works in the references, I hope the reader will understand that this is only because I want to speak for myself and not for others.

1 Responses to the three questions

Now let me consider the above three questions. I can deal fairly quickly in the following overview with (1) and (2), but the third is more complicated and I need to explain why it needs considerable elaboration.

1.1 An overview of my responses

Let me begin with Question (1) and state my complete agreement with Confrey and Costa that basing a reform of mathematics education on mathematical object as central metaphor would not help in bringing about the very necessary improvements that Confrey and Costa (along with most of the mathematics education community) are calling for.

Confrey and Costa have made a strong argument that Platonism is not a reasonable basis on which to develop pedagogical strategies, that mathematics should not be taught as a rigid hierarchical sequences of ideas based only on a logical analysis of the content, that although the development of first rate mathematicians is an important goal, the real issue is about raising the level of mathematical knowledge and understanding amongst the entire population, and that mathematical knowledge should derive from actions (both physical and mental) that take place in a social context.

In my view, Confrey and Costa are absolutely right on all of these points. Furthermore, although they pay less attention to the matter in this particular essay, I think that Confrey and Costa would agree that the answer to Question (2) is: yes, it is a good idea. In fact, an elaboration of this affirmative response may serve as a description of my own work of the last decade. It is tempting at this point to take the opportunity to present in some detail the theoretical perspective under which that work takes place and discuss how it can be a useful tool in research and curriculum development. The main reason for not doing this is that such a presentation has recently appeared in print in Asiala et al (1996). and I see little value in repeating it here.

Turning, finally, to Question (3), I think that these authors are profoundly mistaken, surely with respect to my own position and I think the same holds for the other people that they refer to, in suggesting that we are selecting mathematical objects as central metaphor, as opposed to studying the construction, relationships, and roles of mental processes and mental objects in learning secondary and post-secondary mathematics.

Having said that, it may be that the right thing to do would be to stop here. I think that if Confrey & Costa had focused on a theoretical analysis of why making object the central issue can be harmful, then I would tend to agree it would be enough to say: "You are right, that should not be done and I (we?) are not doing that."

But this is not quite what they do. I think they try to show that the specific work of the people they are talking about leads to some of the difficulties we should be avoiding. To mention only a few examples, I note that they assert that various aspects of the work they are reviewing "...fails to legitimate and take into account the richness and diversity of mathematical practice", "neglect(s) the use of tools", and "...may serve to reinforce a narrow perspective of the mathematics community". Therefore, I think it is necessary to consider some of that work and see that it actually says something different from what Confrey and Costa report. The problem is that these authors seem to base their characterization of the work on only a small and unrepresentative portion of it. I think it is possible to have a different view of the totality of this work.

1.2 A different view of the work being critiqued

I would like to consider individually a number of important points on which the description of the work given by Confrey and Costa appears to be at variance with what has been published by the people who have done this work. There are quite a few major issues for which this seems to be the case.

In comparing what Confrey and Costa say with what can be seen in the literature, I will take the opportunity to elaborate to some extent on the ideas, results and points of view of some of the people being discussed. Perhaps presenting it in the context of this dialogue will help make clear what is, and what is not, in the work Confrey and Costa are discussing.

Let me turn now to specific issues. They can be grouped into four categories: 1) the role of mathematical objects vs. mental objects and processes; 2) mathematics as a narrow, rigid, hierarchy of topics determined strictly by logical analysis; 3) the sources of ideas about the development of mathematical knowledge; and 4) the relation of epistemological ideas to pedagogy.

1.2.1 Objects

Confrey and Costa raise two general issues related to objects: the alleged selection of mathematical objects as central metaphor for advanced mathematical thinking, and the question of whether objects have a separate existence. I will consider them separately.

Are objects the central metaphor? It seems clear that the strongest criticism Confrey and Costa make is their objection to selecting mathematical objects as central metaphor for advanced mathematical thinking. In fact, the work they are discussing neither focuses on mathematical objects nor does it attempt to place any kind of object in a central role.

Since this assertion is repeated so often, I feel compelled to go to some length to refute it. Let me begin with a statement that I have offered, in various forms, in many papers and talks as a brief description of the nature of mathematical knowledge and its development in[1] an individual.

> An individual's mathematical knowledge is her or his tendency to respond to perceived mathematical problem situations by reflecting on problems and their solutions in a social context and by constructing or reconstructing mathematical actions, processes and objects and organizing all of these constructs in schemas to use in dealing with the situations. Asiala et al (1996), Dubinsky (1992, 1994).

The reader will notice that this statement contains many components and that "object" is only one of them. Also, although the adjective "mathematical" is applied to what is constructed, I think it is clear from how these constructions are treated that they are only mathematical in the sense that they have some relationship to standard mathematical concepts, but that, being constructed by the individual, they are as much mental as mathematical. Often, I will actually use the term "mental" explicitly as in the section in Asiala et al (1996) immediately following, and elaborating on this description, which has the heading, "Mental constructions for learning mathematics".

Most important is that even if we focus just on the constructions described in this statement, objects are not the only thing being constructed and so objects are neither the central metaphor nor the main focus. In fact, according to this theoretical perspective, an individual's mathematical knowledge begins with actions that are mental or physical transformations of objects. These actions are interiorized to processes. That means that an internal mental operation is constructed to do the same job as did the more external[2] action. Finally, when it is necessary or

[1] Very early on, I believe I have said "acquisition by" rather than "development in", but this is an error I hope I have long corrected.

[2] The term "external" is not intended to indicate anything separate from the individual. Rather it has to do with phenomena to which access is mainly through the senses and which the individual does not interpret as being under her or his control.

useful to apply an action to what has been a process, this process is encapsulated to become a mental object.

Thus we see that objects play an important role in this theoretical perspective, but a role that is only a part of the total picture. Objects exist in order to have something to which actions can be applied and the sources of objects are processes. Furthermore, I, and others, have often stated that one of the most important (and difficult) general mathematical activities consists in encapsulating processes to objects and de-encapsulating objects back to the processes from which they came, Sfard (1987).

It is very difficult for me to see how one can characterize such a complex interplay of several kinds of constructs as selecting one of them to be the central metaphor. In fact, our central metaphor and total focus is on *all* of actions, processes, objects, schemas, and the mechanism for going from one to the other. At the very least, I think what I am quoting here from my various publications contrasts strongly with that which Confrey and Costa claim is what I and others believe:

> "According to the proponents, progress in mathematics depends on the successful acquisition of mathematical objects." (Confrey & Costa, p. 143.)

What I do believe is that this progress depends on many things and the construction of cognitive objects is one of them. But that it is the *only* one is not supported by what has been published.

Although I have said I will focus on my own work and not that of other people, there is one quote taken from Sfard (op. cit., p. 145) which seems to support directly what I am saying. The quote is offered as evidence that "Sfard and others choose the metaphor of object..." I find it hard to understand how Confrey and Costa can characterize as focusing on object a statement which, in only 14 lines, refers to "operational and structural", "...perceive mathematics in this dual way", "actions performed", "...cannot be separated from the processes...", "procedures", and "...objects are ... the result of processes..." It seems difficult to imagine what else an author might write in such a short passage to establish that the idea is about the duality of processes and objects and not a choice of objects as central metaphor.

It is just after this passage that Confrey and Costa write that

> "Over and over, an image of a mental space filled with objects is communicated, yet the relationship between the knower and the known is exclusively about the knower's ability to move the object into the mental space, neglecting other rich types of relationships."

Having presented evidence that I believe tends to contradict the claim that the work is exclusively concerned with objects, I turn now to the suggestion that the work expresses a belief that mathematical objects have a separate existence from the knower and that the only difference between a mathematical and a mental object is that the latter resides in "mental space".

Where do objects come from? I hope that I have made clear my view that objects do not result from moving something that already exists from some external source to an individual's mental space. Mental objects are, according to the theoretical perspective in which I work, constructed by encapsulating processes and thematizing schemas. Mathematical objects (and processes and schemas as well) result from a set of individuals (mathematicians) interacting in a structured society (the mathematical community) and communicating about their respective mental constructions. Although far from identical, the two kinds of objects are, of course, closely related and elaborating that relationship is one of the goals of research.

I believe that the totality of my published work is consistent with this view.

1.2.2 Hierarchy, logical analysis, and sequencing

On page 144, Confrey and Costa begin to elaborate on a list of what they call the individual components of the theoretical perspective they are critiquing. At the top of the list is the assertion that "Mathematics is strictly hierarchically ordered." They elaborate by saying that the people they are referring to "rely over and over again on the typical sequence of content development and assert its necessary ordering. This assumption is explicit in nearly all treatments and discussions." In support of this statement they offer one quote about which they say , "Sfard and Linchevski refer to this emphasis on hierarchy as a 'logical analysis' ".

I would like to argue here that this work does *not* rely solely on hierarchical, logical *or* historical analysis, although they all play an important role. The work does find certain apparently stable sequences, but more in cognitive mechanisms than in the specifics of knowledge topics. Regarding the latter, my own approach not only tends to reject any uniqueness of sequencing in learning mathematics, but in fact questions whether mathematics is learned sequentially at all. I will try to explain in the next few pages how one might perhaps characterize the description of content development that emerges from my work in two, somewhat contradictory, ways. On the one hand, it is a sequence that is sometimes the same and sometimes different from what is derived from logical analysis. On the other hand, it is not a sequence at all.

Hierarchy and logical, historical analysis — a partial reliance. I think it is important to give the full quote of Sfard and Linchevski from which some phrases were extracted:

> "Although much caution is advisable, logical analysis should not be dismissed altogether as a potential source of insights about the process of learning. After all, mathematics is a hierarchical structure in which some strata cannot be built before another has been completed." Sfard & Linchevski (1994).

I think this single shred is a little weak to support the strong statement of Confrey and Costa. The first sentence only asks what is surely unobjectionable, that logical analysis be considered. It does not call for a dominating role. As for the second sentence, interpretations may differ, but I took it to describe how mathematics is organized after it is constructed (either in inital discovery or in learning), not that this structure should direct the learning process. For myself, I would say that in my own work and in the work of Piaget on which my ideas are based, the point of view is about as far as one could imagine from what Confrey and Costa say Sfard & Linchevski believe.

What I have stated "over and over" is that the framework in which I work bases its description of content development on a combination of the researcher's understanding of the mathematics topics involved, the general theoretical perspective being developed in this framework, and the close analysis of data which consists largely of transcripts of in-depth interviews of students trying to understand these topics (Asiala et al (1996), Dubinsky (1992).)

This combination of theoretical and empirical analysis of data leads to a description of the development of mathematical concepts that sometimes agrees with and sometimes is at variance with the sequence that comes from logical and/or historical analysis. The most famous of the examples in which a different sequence is obtained are due to Piaget. I will mention two of the most important.

One is the reversal of the logical and historical development of spacial concepts. In a logical/historical presentation one usually begins with metric ideas (e.g., distance), moves to projective invariants (e.g., straightness), and finally arrives at topological properties (e.g., connectivity). Piaget showed that in children, the sequence is reversed. Topological invariants are perceived very early, then, after some time projective differences, and finally, much later, metric properties are understood, Piaget & Inhelder (1956).

A second example is Piaget's analysis of the development of the idea of the natural numbers. He goes to great lengths in arguing that his experiments lead to the conclusion that the development of the positive integers is a result of coordinating concepts of set inclusion and seriation, as opposed to Peano postulates that are the logical source of this object in mathematical analysis, Piaget (1941).

In my own work, let me note here only our observation that the concept of limit, generally considered to be a single process by mathematicians, can be considered as a coordination of two processes in cognitive development, Cottrill et al (in press).

Examples of this sort are really quite numerous and, I think, they clearly refute the claim that the work relies entirely on some necessary ordering dictated by logical analysis. There are, of course, many examples in which cognitive development is close in sequence to logical and historical development as well. In Piaget & Garcia (1989) there is a discussion of the general situation which goes much farther. Piaget & Garcia are interested not so much in the order of ideas, but in the *mechanisms* by which they develop. They show that in the case of mechanisms as opposed to topics, there is a very close agreement among cognitive, historical, and logical development. Piaget is even able to relate this to evolutionary development in biology, Piaget (1974).

The other things we rely on — analysis of data. In the framework under which I conduct research and curriculum development, data are gathered from students experiencing a particular instructional treatment based on whatever is known about how the concepts being studied might be learned. Although a fair amount of quantitative data is gathered, the greatest emphasis is on data obtained from interviews (which can last from 20 minutes to two hours) in which students are asked to work on various mathematical tasks. In addition to recording verbal and written responses, the interviewer will often ask the student to explain why he or she is doing something, or might ask related questions in order to probe more deeply into what might be going on in the students' mind.

The analysis of the transcripts of the interviews focuses on the differences between specific successes and failures of students with all aspects of the tasks they were asked to perform. If it is possible to isolate a particular step or idea with which the students had varying levels of success, then an attempt is made to explain the differences in terms of the constructions offered by the theory. That is, we ask if it appears that a student succeeded because he or she interiorized a certain action to a process or encapsulated a process to an object or de-encapsulated an object back to its process. If it seems that there were specific mental constructions which the successful students could make, but the unsuccessful ones could not, then these constructions become part of our description of the cognitive development of the concepts in questions.

This analysis of data is then fed back into the theoretical analysis and, after appropriate adjustments, the instructional treatment is revised and the entire process is repeated. So what happens typically is that the researcher's knowledge of the mathematics is very important in the initial stages but plays an increasingly less influential role as data from students is gathered and analyzed.

I don't want to go into specific details in this essay, but the interested reader can see many examples of this analysis of data in the several papers in which it is reported. See, for example, Breidenbach et al (1992), Dubinsky (1989), and Dubinsky et al (1994).

Immutability of sequential development. The question of sequential development brings us again to Piaget's work. Confrey and Costa comment that the people they are talking about accept "both his (Piaget's) description of the construction of schemes (micro-level) and his stage theory (macro-level) which assumes that development universally follows successive hierarchical stages from sensori-motor to abstraction." (p. 161.) Confrey and Costa discard this position by announcing that it is "limiting." Piaget himself questions his stage theory (see his comment in Piaget (1975) that "the chief revisionist of Piaget's Theory is Piaget") but does, in the end, conclude that certain sequences are universal and unchanging. As I hope to show in my discussion of applications to pedagogy, it is hard to see any limitations in practice. I also question this rather off-hand dismissal of a set of ideas that derived from decades of empirical and theoretical work. It is perfectly reasonable to reject conclusions deriving from this work, but this does leave us a little bit at sea as to what to do with all of the data and its analysis. Did Piaget make errors that should cause us to reject his conclusions? Is there data that leads to different conclusions? If Confrey and Costa think Piaget's conclusions are limiting, they have a right to that opinion. But they refer to it as their "position" and I think this obligates them to give us some hint as to what they are doing about the body of evidence and analysis which supports a position they are rejecting.

I do not wish to suggest here that Piaget cannot be criticized — he certainly made a lot of mistakes, especially when he tried to discuss specific content in mathematics. I only urge that this be done with more evidence and argument in less of a dismissive mode. In my own work, I have chosen to make use of Piaget's ideas and results and this has led to curriculum development that is far from limited to a "strictly hierarchical" ordering or the "typical sequence of content". In support of this I might refer the reader to comments of reviewers who take it as a failing of our textbooks that they continually violate the standard logical development of mathematics. I will mention just a few examples in which the treatment does not follow the standard sequence..

In calculus we use the concept of function (in more than trivial ways) extensively throughout a hundred or so pages of text before considering a formal definition of the concept, Dubinsky et al (1995). Also in calculus it seems strange to some mathematicians that we try to get students to construct the chain rule based on their understanding of composition of functions and not as a formal rule extracted from "lots of examples." In mathematical induction, the logical analysis is almost completely ignored in our pedagogy in favor of helping students construct proposition-valued functions and converting each such function to a particular implication-valued function, Dubinsky (1989). These are cognitive steps to construct mental objects that are not only absent in the mathematical development but are even hard to express using only standard mathematical ideas. In abstract algebra, our study of student understanding of Lagrange's theorem and quotient groups (Dubinsky & Leron (1994)) has pointed out the importance of constructing cosets as mental objects, which is a step taken for granted in standard mathematical treatments. Our work with coset product de-emphasizes the product by representatives in favor of set product which violates some mathematicians' notion of logical development, Dubinsky & Leron (1994).

Maybe it's not a sequence at all. Contrary to what Confrey and Costa suggest, our pedagogical program, which is part of the total body of work being critiqued, is not only open to alternatives to the "...typical sequence of content development and...its necessary ordering", we even question whether any sequence at all should be the basic structure for instruction in post-secondary mathematics. In Dubinsky & Leron (1994) we discuss two alternatives to a sequence: the holistic spray and successive refinement. The former is particularly emphasized in the courses we have developed. The term holistic spray refers to the technique of creating situations for students in which as much as possible of the total set of material to be learned is present in one form or another. Our expectation is that in dealing with a given situation, some students will learn some parts of the material. Both the students and the parts vary and so the spray is repeated throughout a particular course. Although the course tries to help, the student is free to learn what he or she is able to learn and to organize what is learned in whatever way is most natural for her or him.

This presents a number of challenges to the developer of a course, especially when one is trying to do this in the context of an existing educational system. What are the implications for assessment? How can a text reflect such a non-sequential structure? How can the organization of the class overcome the sequential nature of the calendar and daily class schedules?

All of these issues are confronted in the curriculum development project which is part of the overall framework in which I work. I will not go into very much detail here, but I would like to mention just a few features of our implementations so that the reader can consider a picture that is somewhat different from that reported by Confrey and Costa.

One overall feature of our courses is the use of computers. On page 164, there is an implication that there is a relationship between the work they are critiquing and the use of the "computer to teach mathematics as if it were a rigidly ordered set of constructs". I will try to make it clear how the nature of our use of computers differs very much from what is suggested by this comment. Readers interested in a more complete description will find one in Dubinsky (1995).

For example, the computer allows the student to work with ideas that have not been fully discussed yet. One important consequence of this is that in every textbook I have been involved in writing (see, for example, Dubinsky (1995), Dubinsky & Leron (1994)) almost the entire course is already contained in the first chapter (which usually takes no more than 2-3 weeks of the course). The computer allows this because one can use computer constructs to substitute for knowledge the student has not yet constructed. This presentation is repeated throughout the course with increasingly less reliance on the computer and increasingly more reliance on the ideas the student is constructing.

This approach fits in well with one potentially negative aspect of using computers and that is the time required to learn to use the technology. In our courses, all of the work students do to learn to program the computer contains a fair amount of mathematics that students may learn, *en passant* as it were.

The actual organization of one of our courses is according to what we call the ACE Teaching Cycle, Asiala et al (1996). It is a cycle whose period is about a week. It begins with students working (in cooperative groups — see Reynolds et al (1995)) in a computer lab on computer activities designed to get them to make mental constructions related to certain mathematical problem situations. Then the students meet in class without computers to reflect on the constructions they have made and relate them to the mathematical ideas of their colleagues (individual students and groups of students) and the community of mathematicians. Finally, the students work on exercises designed to reinforce the knowledge they have constructed and prepare them for future work.

I must repeat that, in this pedagogical approach, we use the activity of students constructing mathematical concepts on the computer, cooperative learning, and alternatives to lecturing to provide experiences for students that implement the ideas I have been talking about. It is in this practice that one can most readily see that attributions such as being restricted to a fixed logical sequence determined by mathematical analysis are not only wrong, they are irrelevant.

1.2.3 The sources of our ideas

Let me focus on just three examples of how Confrey and Costa describe what the people they are talking about use for their sources of ideas about how mathematical knowledge develops and how pedagogy can help.

First, on page 141, they refer to this work as "…minimizing the role of human interaction in mathematical practice…". This is clearly inaccurate with regard to my own work. Cooperative learning is a key component of our pedagogy and both its practice and its relation to our theoretical perspective are discussed at length in research reports and a book on the subject aimed at college math teachers, Reynolds et al (1995). Another key component is the role played by classroom discussion often between groups with the teacher on the sidelines. Moreover, our research proceeds largely through in-depth interviews of students trying to learn the concepts we are studying and the detailed analysis of those interviews. I don't think this can be described as minimizing human interaction.

Second, Confrey and Costa argue on page 140 that "…the over-reliance on a single intellectual mechanism…leads to a neglect of the development and use of mathematical tools and…the critical role tools have played in the development of mathematical thought." I take this to refer to some very interesting work that Confrey and her colleagues have done in historical studies of this role. It is certainly true that the work they are reviewing does not consider their particular historical tools or their way of studying them. But does that justify such a generalization? The work they are talking about makes use of *some* tools. Much of it pays very serious attention to the use of computers (and Confrey and Costa mention this use, although they give a very narrow view of it), both in the development of modern mathematics and to help students learn mathematics. Are computers not tools? Most of the tools Confrey and Costa mention are very old. Computer tools are relatively new. How important is this difference? I would hope Confrey and Costa would reconsider their point and perhaps replace the accusation that we "ignore" tools with an analysis of different ways of thinking about tools.

Finally, Confrey and Costa refer to me specifically and say on page 148 that I talk "very little about the situation, context or perturbation that spawns the construction...". This may be true about the one single paper of mine that they refer to. I was talking about other things there.

But if you look at some of my other papers and the book on cooperative learning, Reynolds et al (1995), you might come to a different conclusion. In those works I describe how the most important moment of learning occurs when existing structures don't work and then there is a need for and opportunity to reconstruct existing structures. I also talk, based on the ideas of Piaget, about disequilibration and the need to reequilibrate as the central motivation for cognitive growth. In connection with this I and others (e.g., Vidakovic (1993)) have written about the emotional and social issues that are involved in this view of cognitive development.

On the other hand, I do agree with Confrey and Costa that, "By listening to children, one can gain insight into fresh perspectives on mathematics." I could hardly think otherwise given the extent to which I am influenced by Piaget who, if he did not invent the very notion of listening to children in order to understand the development of understanding, was certainly its foremost proponent and practitioner. I have tried in numerous publications, none of which are referenced here, to explain in painstaking detail how I and my colleagues try to listen to what undergraduates are saying about their struggles to understand mathematical concepts and how our ideas are influenced by what we think we hear.

1.2.4 Pedagogy

I could not agree more with Confrey and Costa on the importance of relating theoretical analyses to pedagogical strategies. This is not only my stated position but it is reflected in the efforts that I have made in collaboration with others to develop courses in discrete mathematics, precalculus, calculus and abstract algebra. In every case, we have tried to base our curriculum development on our theoretical perspective and, conversely, we have gathered data in connection with the implementation of these courses that has helped us develop our theory. This has led to a number of research papers, textbooks, and workshops for faculty.

Without referring to any of this material or to the substantial amount of reported work by others in the group of people they are talking about, Confrey and Costa make a number of statements about the relation of this work to teaching practice and experimentation. They also offer, by way of comparison, some comments on their own teaching experiments.

Confrey and Costa begin their discussion of the pedagogical implications of the work they are critiquing with the prediction (on p. 143) that "... it is precisely this point of view that will continue to allow the disenfranchisement of the majority of the population from mathematics". Their arguments in support of this statement are complex, but their essence is perhaps captured sufficiently briefly by the following statement (they are speaking here on p. 154 specifically about

Sfard but they do claim that the issues they raise "apply more broadly"):

> "Because in her model, educational practice derives from mathematical practice, the only obligatory educational implication is that teachers should be aware of the object-based thinking mechanism in their teaching of math."

I think it can be argued that neither Sfard nor the other people being discussed derive their views on educational practice from mathematical practice. Indeed, my discussion in Section 1.2.3 of the varied sources, other than mathematical practice, for our ideas would be included in such an argument.

The argument would be based on a body of work which presents a set of educational implications that go far beyond awareness of objects — although that is, naturally, included. Also included is having students construct mathematical concepts on the computer so as to construct cognitive actions, processes, objects and schemas in their minds; having them perform similar kinds of tasks without the presence of computers; having students reflect on both computer and non-computer activities; having them do exercises to reinforce the constructions they have made; and having them do all of this working in cooperative groups. Indeed, if I were to start such a sentence as "The only obligatory educational implication is...", I would finish it, in reference to the work in which I am involved, with something like "...that the students are placed in situations that will lead them to making mental constructions that are effective in dealing with mathematical problem situations". This is not so different from Glasersfeld's position to the effect that what the child constructs must "fit" with what the adult has constructed, Glasersfeld (1987). I think that all of the people that Confrey and Costa are criticizing are likely to make similar statements about their work.

This body of work relates to Confrey and Costa's prediction of "disenfranchisement". The published results, in fact, suggest somewhat the opposite. They suggest that it could be that the educational practice derived from the theoretical framework that I make use of might lead to *more* mathematical ideas being *more* accessible to *more* students. In other words, the opposite of disenfranchisement.

I do not claim these results are decisive. But they do present Confrey and Costa with a contradiction that I think they are obliged to address. They predict dire consequences of the pedagogical application of the ideas they are talking about. The literature provides data suggesting that what they predict does not happen. I think Confrey and Costa must explain what is wrong with these research reports or at least why they do not contradict the prediction.

Confrey and Costa make another prediction in a discussion beginning on page 161. They describe there a three year teaching experiment on learning multiplication and division based on their point of view. They report that the results were very good and this is indeed support for their approach. But it in no way supports their criticism of the work they are talking about. They predict that the approach they took would not be considered advisable by the people they are talking about. Personally, I do not see why, and the objections they are responding to have not, as far as I can tell, actually been raised by anyone. Why can't the approach by Confrey's research group be successful and also various approaches by the people they are talking about be successful as well? It seems to me that this is exactly what is happening, although Confrey and Costa appear to not report on the teaching experiments of the people they are talking about.

So it seems that we have a case of two theoretical approaches both leading to successful teaching practice. I see no problem with this as I do not think there is only one path to the nirvana of effective teaching. If Confrey and Costa insist that the two theoretical approaches could not both be viable, then it is incumbent on them to try to reconcile this view with the reported successes.

To pursue this point a little, Confrey and Costa assert, on page 162, that the people they are talking about would appeal to "logical analysis" to object to their teaching program. Not only has no one done this, but it is the opposite of what the people they are talking about would do. It is not logical analysis, but data, both qualitative and quantitative that is the basis for these ideas. Specifically, they report that Confrey conjectured that "repeated addition was an insufficient basis for multiplication and that multiplication/division must be developed earlier in the curriculum and with stronger ties to two-dimensional graphing, geometry, and the use of tables." Aside from the curiosity of a phrase such as "must be developed" in a conjecture by those who argue for giving a variety of ideas their own voices, the teaching experiment referred to is consistent with Confrey's conjecture. But Piaget has asserted that repeated addition is the essence of multiplication, Piaget (1985) and also has data (together with pedagogical work by people such as C. Kamii) that supports his assertion. Again I insist that what is needed here is not argument but an analysis that tries to explain this seeming contradiction. At the very least, if Confrey and Costa are going to appeal to their own data, they must also consider the data brought forth by the people whose work they criticize.

Considering the issue in the opposite direction, we note that on page 162, Confrey and Costa assert that the ideas they are critiquing are "...simply not a rich enough framework for interpretations of such events and interactions." This may be so, but has it been tried? My experience suggests that the kind of action, process, object, schema analysis I try to make can usually be done and I think that there is a good chance it could be applied to the events and interactions Confrey and Costa refer to. I may be wrong, but one should try to do it, or at least explain why doing it could not possibly work, before asserting that it can't be done.

2 Epistemological issues

I would like to turn away now from responding to the critique and consider some epistemological issues which are raised in this paper. Their comments relate to the aprioristic, Platonist versus constructivist view of the nature of mathematics, how a constructivist might understand mathematical abstraction and the existence of objects, and the issue of epistemological obstacles as opposed to alternative conceptualizations. Of course, each of these questions could warrant much more than a single paper of its own — indeed, a life's work would not be inappropriate. Therefore, I can only hope that the very brief comments on them that I can make in the space that I have here will amount to a positive contribution to what must be an ongoing discussion.

2.1 The nature of mathematics

On page 160, Confrey and Costa assert that the people they are talking about "often take uncritically as their starting point" what Otte refers to as "...the seemingly a prioristic necessity of mathematics." I don't think this is an accurate characterization of the work being critiqued, but I think I have already made that case in enough instances. I would like, rather, to consider this "a prioristic necessity" and explain that, although I do not accept it, I can see why many mathematicians are convinced of it.

The reason comes from what happens when you do mathematical research. It is my experience, and I don't think I am alone, that when you are investigating a complex, unfamiliar situation, raising questions and struggling to find even partial answers, it sometimes happens that you come to realize something quite new and unexpected and this creates certain impressions. Although I am convinced that the researcher has actually constructed this new fact, or connection, or insight, it is not necessarily the case that he or she is consciously aware of having done so. It is undeniable that new ideas often come "in a flash" and it is impossible to escape the feeling, however inaccurate, that you have picked up a stone and found a nugget of gold that was sitting there underneath it all the time — or indeed, that some disembodied, but very real, muse has whispered it in your ear, or shouted it into your mind. I do not for a moment think that this is what has happened, but I can tell you that this is how it feels.

Given our view of the importance of the human, social aspect of mathematics, I think it is incumbent on the epistemologies that we develop to account for the difference between a constructivist view and the way it feels when you are actually constructing mathematics. As a starting point, I think it might make sense to say that it is most useful to be a constructivist when trying to understand the nature of mathematical knowledge and how one might help others develop it, but almost necessary to be a Platonist when trying to solve problems or create (discover?) new mathematics. At the very least, I do not think that either Confrey and Costa or any of the people whose work they are reviewing will reject this idea solely because of an unwillingness to accept alternative conceptualizations of the nature of mathematical knowledge, even existing in the same individual, albeit not in the same context.

2.2 Abstraction, everyday examples, construction of objects, and naming

There is a curious statement on page 163. In advocating a "tool-based" approach Confrey and Costa acknowledge that it would share a central feature with the ideas they are criticizing, to wit, "asserting the importance of structure". But then they say, "However, (in a tool-based approach) rather than demanding departure from activity, the act of seeing similarities in structure across different contexts would be the basis for abstraction." Again, I have said enough about inaccuracies such as the assertion that the people Confrey and Costa are talking about advocate a "departure from activity". What I am concerned about here is the basis for abstraction that Confrey and Costa advocate.

I think that focusing on structure is very different from seeing similarities across contexts. Indeed, I can find no other interpretation of the latter phrase than that there are some ideas which have an independent existence, and that they are sufficiently invariant that one comes to know them by the fact that they can be seen to repeat themselves in different contexts.

For me this is not consistent with the constructivist view that mathematical knowledge has its source in the structures an individual, or a society, or any combination thereof constructs in making sense out of phenomena that are accessed through the senses, or are contained in previously constructed structures. This similarity across contexts is an empiricist view that in my opinion has been discredited theoretically by the writings of Piaget and others. Moreover, I believe that the pedagogy that is derived from it — "looking at lots of examples" has also been discredited by the lack of success of decades of teaching activity based on such a barren notion of abstraction.

Considering now the use of examples, we note that Confrey and Costa, like so many others, turn to everyday life as (on p. 150) in looking for a fountain pen, when they are discussing mathematical epistemology. This may work for more elementary mathematics where the ideas are closely connected to processes and objects to which we have access through our senses, like fountain pens. But it is much harder to deal with the fact that an idea like the structure of a coset of a subgroup or a quotient group does not relate so closely to our sensual perception. I don't deny that we can find "everyday" situations which mathematicians realize are quotients — such as odd and even in the group of integers (Burn (in press), Dubinsky et al (in press)) — but I have never seen any evidence that such a connection is helpful in constructing powerful ideas about quotients. I think it is important when talking about understanding advanced mathematical concepts to deal with examples close to that domain and not some distant domain whose relevance may be problematic. This also relates to examples of research such as the experiments reported in this paper. In this case the subjects were 11 year olds and I don't think it is enough to assert that the same ideas will apply to older students and more advanced mathematical topics. This is a research question and it requires serious investigation.

Finally, on page 164, Confrey and Costa begin to talk about educational use of computers based on the ideas they are discussing. They mention briefly uses of computers to display graphics and also the role of naming a procedure in the "process-object" discussion. I think they make an error here in what seems to me a suggestion that naming has an active role in mental constructions of objects. Our work suggests that it is the other way around. That is, only after encapsulating a process to understand it as an object is it possible to name it. Of course one consequence of this is that the need or desire to name can be a stimulus to encapsulation, but there are more powerful stimuli arising from the fact that in some computer systems (ISETL, Lisp and Scheme, for example, but not Logo) procedures are first class objects and it is possible to apply a multitude of actions to them. I would argue, and there are some supporting results in the literature, that making use of such computer capabilities can be a very powerful way of helping students construct mental objects that normally give them great difficulty.

2.3 Alternative conceptualizations

On page 144, Confrey and Costa call for dealing with epistemological obstacles, not by eradication, but through a "...conscious decision to abandon or modify one approach in preference to another." I am not sure what is the difference between eradication and abandonment, but I strongly agree with what may be the main point here: that whatever happens to the conceptualization that led to an obstacle — abandon, modify, or keep — any changes must be made by the student, albeit under the influence of the teacher to achieve, in von Glasersfeld's words, "compatibility with the adult view", Glasersfeld (1987).

This can all be put another way which is due to Piaget. According to him, an important mechanism that drives the development of knowledge in an individual is the desire for equilibration and a need to re-equilibrate in response to a dis-equilibrating experience. One of the troubles with epistemological obstacles is that, in themselves, they are not dis-equilibrating. The student may be perfectly content to hold ideas which may not be useful, or as powerful as some the teacher would like the student to construct. It is the job of teacher to provide situations that create disequilibration. Then the re-equilibration can occur, as Confrey and Costa point out, through a reconceptualization by the teacher, the student, or the researcher.

The question of how it is decided who reconceptualizes is not an easy one. It is authoritarian to say that the teacher always decides and such an atmosphere is not very conducive to cognitive growth. But neither is an atmosphere in which the student chooses to adopt or abandon an approach according only to her or his "preference". I think it is important to remember that not all approaches are equally easy to understand. For example, if a student prefers to think of a function as something that comes from an algebraic expression in which numbers may be substituted for a variable and does not think of a function as an input/output process, is this because of a conscious choice, or because the student is not yet able to conceive of functions as processes? Students in the schools of today are under enormous pressure to perform and it is completely understandable that they might have a tendency to pick what seems to be the easiest thing to do. But such a choice may not be so conducive to learning and it is the role of the teacher to help students consider more difficult, but more rewarding preferences.

3 Additional points in Confrey and Costa's critique

In this section I would like to list briefly some relatively minor points which seem to me to be errors that have been made in reporting on the work being critiqued. Of course, it may be that I am misinterpreting what Confrey & Costa have written or I might simply be wrong. In any case, I will make these points and let the reader decide where the mistakes, if there are any, lie.

1. On page 143, Confrey and Costa write that "The central approach of these theorists seems to be to elucidate what distinguishes higher level mathematics from more elementary mathematics and, in doing so, to find a satisfactory framework for talking about advanced mathematics". I do not think this is at all what the work is about. Certainly the nature of the relation between "higher level" and "more elementary mathematics" is an important matter to study and some of the work, including my own (Dubinsky (1991)), is concerned with this

point. But it is hardly a "central approach" and the distinction is certainly not the source of the framework I work with nor, I would imagine, is it the source for any of the others being considered.

2. On page 155, Confrey and Costa describe very briefly the work of the French school and assert that there are both "similar constructs" and "critical differences". It is hard to see what these differences are. The main point they make is: "The French tradition combines attention to epistemological obstacles with analysis of teaching and learning in institutional settings." A significant portion of the work that Confrey and Costa talk about is doing exactly the same thing except that it is concerned with the entire question of learning, not just epistemological obstacles.

3. I was surprised to read, on page 164, a suggestion that the people Confrey and Costa are talking about are overly concerned with the production of exceptional mathematicians. What could possibly be the source of what is surely a misunderstanding? I do agree with Confrey and Costa that the best way to produce more exceptional mathematicians is to increase the quantity and quality of mathematics in the cultural life of the entire population. I suspect that the other people Confrey and Costa are talking about have similar views but I am not aware of very much in their published writings that deals with this point one way or the other.

4 Tonal notes

Finally, it is with some hesitation that I introduce a consideration having to do with the style as opposed to the content of this critique. I do not think it would be helpful for this discussion to degenerate into an argument with emotions playing an unwarranted role. I do feel that some of the phraseology in this paper tends to carry with it unnecessary emotional baggage. I am not sure if the best way to de-emphasize these connotations is to ignore them or to point them out in complaint as I am doing.

Here are some examples which gave me concern when I read the critique.

> On p. 140, Confrey and Costa write that what they see as the main aspect of the theory "...may be retrogressive for the field of mathematics education".
>
> Then on p. 141, Confrey and Costa say that the work they are talking about "...may serve to reinforce a narrow perspective of the mathematics community rather than engendering reflective or critical discussion."

> Again on p. 141 we read that the people Confrey and Costa are talking about "...run the risk of detracking educational debate more than contributing to it."
>
> Finally, on p. 143, Confrey and Costa predict that "...it is precisely this point of view that will continue to allow the disenfranchisement of the majority of the population from mathematics".

I wonder if it is necessary or helpful for Confrey and Costa to express their disagreements in terms like "retrogressive", "narrow perspective", "detracking", or "disenfranchisement". Of course, if there were justifications for such disparagements, they should not be hidden. But my point is that these value-laden phrases are used inappropriately in terms of the content and evidence presented by Confrey and Costa. I don't think this is a very useful atmosphere. Rather than confrontation, debate and the calling of names, I think greater progress would come from collaborative investigations to increase our understanding of how mathematics can be learned.

I have a more serious complaint on page 149, where Confrey and Costa repeat an analogy between the ideas expressed by people they are criticizing and ideas describing "...how oppression encloses the oppressed". If something is meant here, then Confrey and Costa should state it openly and defend it. If not, they should not attempt in such an oblique manner to suggest a connection between the people they are talking about and "oppressors". There are a number of struggles between oppressed and oppressors that have occurred in the adult lives of the people Confrey and Costa are talking about and some of these individuals have had a direct involvement in some of them. I do not think that such matters have any place at all in discussions such as this and I think we should dispense with any suggestion that there is an issue of oppression, or even an analogy to oppression in these deliberations.

This kind of hinting is repeated in a different context on page 160 where Confrey and Costa interpret certain historical episodes as suggesting that "...the history of mathematics also includes an attempt to suppress its relationships to tools, everyday thought and to controversy." There are no doubt some examples of suppression that have occurred in mathematics, but I think that this term is probably less applicable to mathematics than just about any other field of human endeavor. I know of no other field that openly lists challenges to its practitioners that, however "narrow" one might find them, make the promise that anyone who solves the problems will be fully recognized, no matter what other attributes they have. I think that the record of mathematics in keeping these promises is not too bad.

Finally, I am not sure the call for debate at the very end of the article is the way to make the most progress. If their paper is an example of debate then I am finding it a bit too competitive and abusive. I wonder if we might not make more progress with investigations that are collaborations amongst individuals with different points of view. Piaget had a practice of identifying people who disagreed most strongly with his views and inviting them to collaborate in some specific study as a way of looking for areas of agreements and synthesizing disparate points of view. One example of this is described in Beth & Piaget (1965), p. xi. It is an interesting tradition and one we might consider emulating.

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