Cognitive Science and Physics Education Research: What We've Got Here Is Failure to Communicate *† ◊

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In 1999 cognitive scientist (CS) Allan Collins wrote: "Recently researchers have begun to study teaching and learning in the context of real-world learning environments," evidently unaware that Physics Education Researchers (PER's) had been doing classroom research for about three decades. Then in 2004: (a) David Olsen maintained that the search for "what works" in education is folly, contradicting PER results; and (b) CS's David Klahr & Milena Nigam purported to show the superiority of what they called "direct instruction" (DI), defining DI - in a sense unknown to PER's - as a limiting form of what PER's call "Interactive Engagement" (IE). But CS's Kirschner, Sweller, & Clark (2006) outdid the non-recognition of PER by Collins, Olsen, and Klahr & Nigam by not only defining DI as a form of IE, but also proclaiming the "failure of constructivist, discovery, problem-based, experiential, and inquiry-based teaching," despite PER evidence for the relative effectiveness of all but unguided "discovery." Will articles such as this in the multidisciplinary *Journal of the Learning Sciences* assuage, to any extent, past failures of CS's to communicate with PER's and vice versa?

1. Physics Education Research: Three Decades Of Classroom Research – Evidently Unknown To Some Cognitive Science (CS)

It appears that the work of physics education researchers (PER's) is virtually unknown to some cognitive scientists CS's. Several years ago I stumbled upon Allan Collins' (1999) valuable article "The Changing Infrastructure of Education Research." Collins wrote: "Recently researchers have begun to study teaching and learning in the context of real-world learning environments," citing Brown (1992) and Collins (1992). This puzzled me since PER's have been studying "teaching and learning in the context of real-world learning environments" for about three decades. In Section IV, "Empirical Studies," of McDermott & Redish's (1999) "Resource letter on physics education research," I count over 80 articles, dating from McKinnon (1971), that feature classroom research. However, it must be admitted that relatively few of those articles meet the criteria for "design based research" (DBR) suggested by Brown (1992), Collins (1992), and Kelly (2003).

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† "What We've Got Here Is Failure to Communicate," is a famous line from 1967 film *Cool Hand Luke* <http://en.wikipedia.org/wiki/What_we've_got_here_is_(a)_failure_to_communicate>.

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As far as I know, one of the earliest examples of DBR by physicists is the effort involved in the development of the Science Curriculum Improvement Study (SCIS), now available through Delta Education <http://www.delta-education.com/>. In "One Physicist Experiments with Science Education," Robert Karplus (1964) wrote:

> The experimentation with science teaching that I have described is being carried out by the Science Curriculum Improvement Study at the University of California in Berkeley. The parts of the science program which have been constructed by SCIS staff members over the past three years are now ready for classroom trial. The kindergarten and first grade teachers in several schools are working with a unit called *Material Objects*, while the second and third grade teachers are working with a unit called *Interaction and Systems* with their classes. . . . . . . Staff members and consultants are available to evaluate the effectiveness of the teaching program and to help participating teachers in using the materials. Reactions and suggestions from the teachers and the results of observations of the pupils' behavior will help determine what revisions in the teaching plans are necessary.

After puzzling over Collins's apparent implication that classroom research was initiated by cognitive scientists in the 1990's, I picked up the *Educational Researcher* of January/February 2004 to find cognitive scientist David Olson (2004) essentially maintaining that the search for "what works" in education is folly, contradicting PER results. In the same issue, psychologist Robert Slavin (2004) wrote:

> One key assumption in Olson's response is that there have been thousands of experiments evaluating educational practices and that they haven't amounted to much. There may be thousands of brief laboratory studies, but experimental. . . . .[i.e., Randomized Control Trial (RCT)]. . . .studies of replicable treatments that take place over a semester or more are extremely rare. . . . .[as judged by Slavin's survey of the *American Educational Research Journal* <http://www.aera.net/publications/?id=315> over the period 2000–2003]. . . .

Slavin may be unaware of the many replicable, non-laboratory, multi-semester, experimental PER studies, if the word "experiment" is taken in mean the same as in traditional hard-core physics research – "an experiment can be thought of as an act of observation designed to yield a particular type of empirical knowledge" [Ziman (2000, p. 93)], or to mean the same as the word "quasi-experiment." The latter term was used by Cook & Campbell (1979) to designate a study that does *not* involve RCT's. That RCT's are the "gold standard" of educational research, as Slavin appears to believe, is a matter of current debate [see e.g., Shavelson & Towne (2002), Cook (2006), Scriven (2006)].

Olsen's claim that the search for what works is folly, is in direct contradiction to the fact that PER's have been able to shown what works and what does not work for many areas of physics instruction, most notably Newtonian mechanics.

For Newtonian mechanics, PER's have demonstrated that "interactive engagement" (IE) methods can show about a two-standard deviation superiority in average normalized learning gains <g> over traditional passive-student lecture methods, as recently reviewed in "Design-Based Research in Physics Education Research: A Review" [Hake (in press)]. Some definitions are in order:

(a) IE methods are those "designed at least in part to promote conceptual understanding through interactive engagement of students in heads-on (always) and hands-on (usually) activities which yield immediate feedback through discussion with peers and/or instructors"; and

(b) the average normalized gain <g> is the average *actual* gain [<%post> - <%pre>], divided by the *maximum* possible average actual gain [100% - <%pre>], where the angle brackets <. . . > signify class averages.

The most popular IE methods surveyed in Hake (1998a,b) were *Collaborative Peer Instruction*, *Microcomputer-Based Laboratories*, *Concept Tests*, *Modeling*, *Active Learning Problem Sets*, *Overview Case Studies*, and *Socratic Dialogue Inducing Laboratories* – for the references see Hake (1998a). As emphasized by Kenneth Heller (1999), all of these methods are in associated loosely with learning theories from cognitive science – for the references see Hake (2002):

(a) "developmental theory" originating with Piaget [Inhelder & Piaget (1958); Gardner (1985); Inhelder, deCaprona, & Cornu–Wells (1987); Phillips & Soltis (1998)];

(b) "cognitive apprenticeship" [Collins, Brown, & Newman (1989); Brown, Collins, & Duguid, (1989)].

In addition, all the methods recognize the important role of social interactions in learning [Vygotsky (1978); Lave & Wenger (1991); Dewey (1938/1997); Phillips & Soltis (1998)].

2. CS's "Direct Instruction" Is a Limiting Form of PER's "Interactive Engagement"
Of course, IE methods bear near zero resemblance to the extreme "discovery learning," researched by Klahr & Nigam (KN) (2004) and widely interpreted by direct-instruction zealots such the virulently anti-reform Mathematically Correct [MCSC (2007)] as demonstrating the superiority of "direct instruction." Mathematically Correct advertises:

> Carnegie Mellon Researchers Say Direct Instruction, Rather Than
> "Discovery Learning" Is Best Way To Teach Process Skills In Science.

Recently Kirschner et al. (2006) have added to the chorus proclaiming KN's endorsement of "direct instruction."

BUT WAIT! What do Klahr & Nigam and Kirschner et al. *mean* by "discovery learning," and what do they *mean* by "direct instruction"? In discussion list posts Hake (2004a,b) criticizing the California Curriculum Commission's (CCC's) anti-hands-on "Criteria For Evaluating K-8 Science Instructional Materials In Preparation for the 2006 Adoption," I opined that popular pedagogic terms such as "discovery learning," "direct instruction," "hands-on activities," "active learning," "cooperative learning," "inquiry," and "interactive engagement," should be *operationally defined* [see, e.g. Holton & Brush (2001), Phillips (2000)] even despite the "antipositivist vigilanates" [Phillips (2000), Phillips & Burbules (2000)]. More generally, rigorous operations should be defined for distinguishing pedagogic method X from other methods Y, Z, A, B, C, . . .

Although operational definitions are uncommon in the educational literature, in Hake (2004b,c) I indicated my own guesses as to what various authors have meant by "direct instruction":

(a) <u>*Mathematically Correct*</u> <http://tinyurl.com/3e49wc>: "drill and practice," "non-hands-on," "teach 'em the facts" [Metzenberg (1998)], and "non-discovery-learning," where "discovery learning" means setting students adrift either in aimless play or ostensibly to discover on their own, say, Archimedes' principle or Newton's Second Law.

(b) <u>*Physics Education Researchers*</u> (PER's): traditional *passive student* lectures, recipe labs, and algorithmic problem sets.

(c) <u>*Klahr & Nigam*</u> (2004): . . .instruction in which "the goals, the materials, the examples, the explanations, and the pace or instruction are all teacher controlled," but in which hands-on activities *are* featured. At least this is Klahr & Nigam's (KN's) definition of what they call "*extreme* direct instruction" (extreme DI), possibly having in mind the reasonable idea of a continuum of methods from extreme DI to extreme "discovery learning" (DL). In extreme DL, according to KN, there is "no teacher intervention beyond the suggestion of a learning objective: no guiding questions, and no feedback about the quality of the child's selection of materials, explorations, or self assessments." I suspect that KN might classify "interactive engagement" methods (Hake (1998a,b; 2002) and inquiry methods [NRC (1997, 1999, 2000)]; Bransford et al. (2000); Donovan et al. (1999)] as somewhere along a continuum ranging from extreme DI to extreme DL, since "interactive engagement" and "inquiry" methods both involve various degrees of judicious teacher intervention so as to guide students' conceptual understanding, problem solving abilities, and process skills towards those of professionals in the field.

(d) <u>*Association of Direct Instruction*</u> [ADI (2004)]:

(1) teaching by telling (as contrasted by teaching by implying), or

(2) instructional techniques based on choral responses, homogeneous grouping, signals, and other proven instructional techniques, or

(3) specific programs designed by Siegfried Engelmann and his staff.

Direct Instruction programs incorporate the above "2" coupled with carefully designed sequences, lesson scripting, as well as responses to anticipated children's questions as expounded in Englemann & Carnine (1982).

Thus the *interpretation* of Klahr and Nigam (2004) that “direct instruction” (as defined by KN) is superior to “discovery learning” (as defined by KN), while consistent with KN’s research, appears to be a *misinterpretation* to physics education researchers (PER’s) if they use the PER definition of “direct instruction,” and are unaware of the KN definitions of “direct instruction” and “discovery learning.” Thus there appears to be a *communication failure* involving different meanings for these terms.

Consistent with the above regarding the importance of operational definitions of educational terms, Klahr & Li (2005), disturbed by the misinterpretations of Klahr and Nigam (2004) in the media, wrote [my insert at “. . . . .[insert]. . . . “; my *italics*]:

> Only when we tuned in to the recent political debate in California about the permissible amounts of “hands-on science” vs. “direct instruction” . . . . . . [Hake (2004a,b,c; 2005a), Strauss (2004); Woolf (2005)]. . . . . . did we become fully aware of how easy it is for someone to pick up a terminology, and imbue it with whatever meaning suits the purpose of an argument. . . . . . . One thing is clear from all of this: it is essential for the field of education to make much more precise use of terminology before moving on to public debates and policy decisions. Indeed, *it is surprising that when education researchers and science educators join in heated debates about discovery learning, direct instruction, inquiry, hands-on, or minds-on, they usually abandon one of the foundations of science—the operational definition. The field of science cannot advance without clear, unambiguous, operationally defined, and replicable procedures* [my *italics*.]. Education science is no exception.

3. Failure Of Constructivist, Discovery, Problem-Based, Experiential, And Inquiry-Based Teaching?

Another example of what I would regard as a communication failure is provided by the previously mentioned paper of Kirschner, Sweller, & Clark (2006) with its seemingly non-sequitur title "Why Minimal Guidance During Instruction Does Not Work: An Analysis of the Failure of Constructivist, Discovery, Problem-Based, Experiential, and Inquiry-Based Teaching," even despite PER evidence reviewed by Hake (2002; 2005b; 2007a,b; in press) for the effectiveness of all but extreme "discovery teaching." Kirschner et al. wrote:

> Klahr and Nigam (2004) in a very important study, not only tested whether science learners learned more via a discovery versus direct instruction route but also, once learning had occurred, whether the quality of learning differed. Specifically, they tested whether those who had learned through discovery were better able to transfer their learning to new contexts. The findings were unambiguous. Direct instruction involving considerable guidance, including examples, resulted in vastly more learning than discovery. Those relatively few students who learned via discovery showed no signs of superior quality of learning.

But here again, as with Klahr and Nigam (2004), "direct instruction" appears to mean to Kirschner et al. (2006) pedagogy rather similar in some respects to the "interactive engagement" methods shown to be relatively effective by physics education researchers, as can be seen from the Kirschner et al. abstract [my insert at ". . . .[insert]. . . ."]:

> Evidence for the superiority of guided instruction. . . [read "interactive engagement"?]. . . is explained in the context of our knowledge of human cognitive architecture, expert–novice differences, and cognitive load. Although unguided or minimally guided instructional approaches are very popular and intuitively appealing, the point is made that these approaches ignore both the structures that constitute human cognitive architecture and evidence from empirical studies over the past half-century that consistently indicate that minimally guided instruction is less effective and less efficient than instructional approaches that place a strong emphasis on guidance of the student learning process. The advantage of guidance begins to recede only when learners have sufficiently high prior knowledge to provide "internal" guidance. Recent developments in instructional research and instructional design models that support guidance during instruction are briefly described.

In my opinion the failure of communication between CS's and PER's is related to the insularity of both camps. Among the evidence of PER's provincialism is:

1. Some PER's do not appear to recognize that most "interactive engagement" methods are associated with learning theories from cognitive science, as discussed above.

2. Some PER's have no interest in in design based research, as judged by the rejection of "Design-Based Research: A Primer for Physics Education Researchers" [Hake (2004d)] by the editors of the *American Journal of Physics*, without even bothering to send the manuscript to reviewers!

3. The rejection of an earlier version of this paper by the editors of the *Physics Education Research Conference 2007*, not because the paper was incorrect but because the editors thought it would not be understandable to the PER audience because of its unfamiliarity with the cognitive science literature. In the words of a PERC 2007 editor:

> As you well know, many PERs are relatively unfamiliar with authors and journals in the larger field of cognitive science and educational research. . . . . The fact that the authors and the publications you cite are well-known and well-respected in the cognitive science field is not in dispute. . . . However, the point is that a great many PER's, perhaps most, do not know this. . . . . As erroneous and uninformed as [the referee] objections may be, they represent the state of the knowledge of the audience of the PERC proceedings and the papers in the PERC Proceedings must be written to address that audience, just as physics instruction should be aimed at the knowledge state of the students.

In my opinion, it will be a sad day for science if only articles that are understandable to a majority of the readership are published, regardless of the opinion of experts. That opinion *should be* (but in the above case was not) reflected in peer review.

In any case, the dreadful abyss of non-communication that appears to separate some cognitive scientists from some physics education researchers is, I think, well illustrated by the above discussion. Will articles such as this in the multidisciplinary *Journal of the Learning Sciences* assuage, to any extent, past failures of CS's to communicate with PER's and vice versa?

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