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Coming Up for Air: But Is It Oxygen or Phlogiston?

A Response to Taber's Review of

Constructivist Instruction: Success Or Failure?

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Keith Taber's lucid review (Taber, 2010) of the Tobias & Duffy (2009) book presents an articulate contrast to the typically contentious and unjoined debates about pro- and anti-constructivist approaches to education. Indeed the clarity of Taber's summary of those approaches highlights some specific issues about which I offer a few points of disagreement in what I hope is the same spirit of constructive (but not constructivist!) criticism.

When Theories Compete, There Is Ultimately a Winner

The first such point is Taber's analogy between the incommensurability of the two different "paradigms" represented by the "instructivist" and the "constructivist" camps and earlier scientific controversies. He cites the 17th century debates between phlogiston believers vs. oxygen believers as one example, and the earlier debates between heliocentric believers vs. geocentric believers as another. But in each case, there is more than a world view involved: one theory really does describe

the natural world substantially better than the other. No scientist today believes in either phlogiston or a geocentric universe because the empirical evidence overwhelmingly refutes those theories in favor of their alternatives: the oxygen theory and the heliocentric theory. Thus, if Taber's analogy to pro- and anti-constructivism is appropriate, at some point one of these views will simply disappear from serious scientific discourse (and it is clear where my bets lie). However my analogy is incomplete, because a major difference between the examples from chemistry and physics and the current debate between instructional-isms is the dearth of operational definitions.

Operational Definitions Are Crucial

And that is my second point: the importance – but disconcerting absence from these debates -- of clear descriptions of what is actually going on in the instructional situations under investigation. As I have argued elsewhere (Klahr & Li, 2005) it is surprising and distressing that when education researchers and science educators join in heated debates about 'discovery learning', 'direct instruction', 'inquiry', 'hands-on', or 'minds-on', they often abandon one of the foundations of modern scientific discourse - the operational definition. However, no science can advance without unambiguous, operationally defined, and replicable procedures and measurements.

Take, for example, Taber's discussion of the various nuances of bullying. While in everyday usage “bullying” is an extremely vague and subjective term, a researcher can describe a clear set of indices for it. These can range

from observations of behavior, to questionnaires, to teacher ratings, to descriptions of contextual features, to physiological and psychological measures of its extent and effects. Researchers will certainly have different opinions about which measures "really" define bullying, but if operational definitions are used, there will be little ambiguity in any particular study about what is being characterized as bullying behavior and its effects. Consequently, over time, evidence will accumulate that is consistent, or inconsistent with one theory or another.

Although Taber acknowledges the importance of operationalization as "obvious" ("At one level, this is simply stating the obvious: we need to operationalize our research question, rather than just ask which arrangement is best."), he repeatedly lapses into a perspective that seems to argue that it is impossible to operationalize many questions and processes that are of importance to the constructivist "educational research paradigm". But as Mark Anthony put it in his soliloquy on Caesar's death, “If it were so, it were a grievous fault”, for it removes assessments of constructivism from the realm of scientific research. In the absence of clear operational definitions, replication, re-analysis, and application from research to practice are all severely limited, if not impossible, because without them, we quite literally don't know what we are talking about.

And Taber does seem to believe in the impossibility of any empirical assessments of different approaches to instruction. For

example, he favorably cites Jonassen's lament that

“ .. high-quality research studies comparing the effectiveness of inquiry methods and direct instruction ... probably do not exist and cannot exist. Researchers examining the effectiveness of direct instruction begin with fundamentally different assumptions, evoke significantly different theory biases, and use different research methods than researchers examining informal or inquiry learning. Therefore the questions they ask, the learning outcomes they seek and the research tools and methods they use are also quite different.” (Jonassen, 2009, p. 29)

This attitude toward our science is disastrous. The same lament about 18th century chemistry would, as I suggested earlier, leave us with a vibrant Phlogiston community in the 21st century!

Multi-attribute Comparisons Are Possible

Taber (p. 31) seems sympathetic to the constructivist complaint that comparisons between "instructionist" and "constructionist" approaches invariably and necessarily use a severely impoverished form of constructionist instruction because the very nature of empirical experimental comparisons necessitates varying only one potentially causal feature at a time. But this is simply not true. In the comparisons with which I am familiar the contrasting instructional

approaches are combinations of features – vectors that include several contrasts that are characteristic of each approach (cf., Klahr & Nigam, 2004; Lorch, *et al*, 2010; Minner, Levy, & Century, 2010). And only the entire package—the vector of instructional components—can be assessed, not its individual features.

At the fine-grained level, of course, such comparisons are confounded experiments, but at the aggregate level they are perfectly acceptable. They are analogous to comparisons between other types of complex multi-attribute artifacts, such as comparisons between two types of aircraft, or two types of web browsers, or two types of reading programs. It is certainly possible to compare two multi-attribute systems by using a similarly complex evaluation function. For example, fixed wing aircraft have a vector of performance measures that are consistently different than helicopters, and assessments such as these are done in a wide variety of domains. Constructivists cannot use complexity of treatments or assessments as an excuse to avoid rigorous evaluations of the effectiveness of an instructional process.

Taber argues that

If we accept that {personal constructivist perspective} ..., then a number of things follow. One ... is that it is essential that teachers plan teaching based on accurate intelligence about students' current knowledge states – and so (on-going) diagnostic assessment becomes a very important part of teaching.

However, the instructional approaches that provide the most careful, validated, individualized, and precisely targeted and direct instruction – cognitive tutors (Koedinger & Anderson, 1998; Koedinger, Anderson, Hadley, & Mark, 1997) – exemplify exactly the kind of instruction that “personal constructivists” should value, even though they are clearly by-products of the “instructivist” camp.

Who *Really* Cares About Transfer?

My final point of disagreement with Taber is with respect to his apparent endorsement of the claim by Wise and O’Neil that, when compared to “instructionists,” constructivists have “greater ambitions where transfer of learning is concerned.” This is patently false, as just about every “instructionist” study that I’ve ever read (or written), puts a very heavy emphasis on both the theoretical and the empirical aspects of transfer. To give two specific examples from my own research, in Klahr & Nigam (2004), we studied the effects of highly focused training on simple experimental design on children’s ability to evaluate the science fair posters of other children – a task quite “distant” from the initial instruction on several dimensions, and in Strand-Cary and Klahr (2008) we measured transfer of simple experimental design skills three years after training. (For a more general treatment of transfer, see Barnett & Ceci, 2002; Chen & Klahr, 2008.) Where is the evidence to support the claim of “greater ambitions” for transfer from constructivists?

I believe that the burden of proof is on constructivists to define a set of instructional goals, an unambiguous description of instructional processes, a clear way to ensure implementation fidelity, and then to perform a rigorous assessment of effects: near transfer, intermediate transfer, far transfer, delayed transfer, robust transfer, “preparation for future learning” (Schwartz, & Martin, 2004), or whatever. But they really need to “do the right thing” rather than wish away the need for principled and replicable empirical evaluations in a barrage of post-modern obfuscation.

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