Experiments Trump Precise Definitions for Teaching Science to Middle-School Students

Perhaps the academic community demands too much in the way of perfection, or at least our perception of perfection, in middle-school texts. A case in point is John Hubisz's criticism of a text for defining "acceleration" as a "change in velocity" rather than as the *rate* of change in velocity (see PHYSICS TODAY, May 2003, page 50). I think we are splitting hairs when we criticize so trivial a thing. In a college text, more careful attention to meaning may be needed, but in a ninth-grade physical sciences book, it is perfectly appropriate to define "acceleration" in the terms that the seemingly offending textbook did. The change in velocity is understood to occur over time, as dictated by logic. If one applies a rate of change in velocity as the definition to be learned by the middle-school or beginning high-school student, the concept then becomes too abstract for the typical 13- or 14-year-old.

We must acknowledge the tremendous differences in cognitive maturity that exist between middle-school and beginning high-school students and their older counterparts. What may seem oversimplified for the more mature student is often very appropriate for the younger one. Let's keep that in mind and realize that writers have to consider the level of understanding of the reader who uses the text.

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Hubisz replies: "Acceleration" is a very difficult concept. It is a second-order effect: a rate of change with respect to time of a rate of change with respect to time. It may well be that it is too much for middle-school students, but, regardless, it is mentioned in most physical sciences texts; the definition should be correct, even if the only objective is to have the students memorize something.

Galileo reviewed previous attempts at describing accelerated motion, rejected them, and considered both the rate of change of speed (not velocity) with respect to distance and the rate of change of speed with respect to time.[1] He rejected the former, for reasons that were not too clear, but accepted the latter on the guess that it fit his work on freely falling bodies. This leap of Galileo's is very sophisticated and certainly not "dictated by logic." Logic can only assure us that the process is correct; it says nothing about the physics that Galileo insisted his definition satisfy.

What can we do? Follow Galileo. Forget putting the definition in the textbooks. Have the students carry out experiments. Encourage them to investigate the phenomenon first before we throw definitions at them. They will learn that velocity is not always constant, that the problem is not trivial, and that they have a lot more to learn, perhaps in high school, maybe in college.

Reference

1. Galileo, *Dialogues Concerning Two New Sciences*, H. Crew and A. de Salvio, trans., Northwestern U. Press, Evanston, Ill., and McGraw-Hill, New York (1963), Third Day.

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A Reader Inquires: Origin of 'Critical Mass'

Essayist William Safire, in a recent magazine column,[1] attributed the coining of the term "critical mass" to British science historian Margaret Gowing in 1940. However, that seems unlikely: In 1940, Gowing was an undergraduate studying economic history at the London School of Economics.[2] The *Oxford English Dictionary* (2nd edition, Clarendon Press, 1989) gives several examples of the use of the term, the earliest tracing through Gowing to the famous Frisch–Peierls memorandum of March 1940.[3] Otto Frisch and Rudolf Peierls used the terms critical size, critical amount, and critical radius but not critical mass.

It may be impossible to determine the term's origin in the rush of events surrounding the discovery of fission. Determining when it first appeared in the scientific literature would be an easier task. Three papers from May and June of 1939 explored the conditions necessary to sustain a chain reaction.[4] The first, by Francis Perrin, was published in the 1 May edition of *Comptes Rendus B*. Near the bottom of the second page of that paper, in the context of achieving a moderated-neutron chain reaction in U_3O_8, he wrote, "d'ou une mass critique M_C de 40 tonnes." The only slightly later papers by Siegfried Flügge and Peierls did not mention critical mass, but Peierls did use the term critical thickness.

Do any readers know of a precedent for "critical mass" earlier than Perrin's paper?

References

1. W. Safire, *New York Times Magazine*, 27 July 2003, p. 15.
2. For a brief biography of Gowing, see http://www.keele.ac.uk/depts/ir/gowing/career.htm.
3. R. Serber, *The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb*, U. of California Press, Berkeley (1992).
4. F. Perrin, *Comptes Rendus B* **208**, 1394 (1939); S. Flügge, *Naturwissenschaften* **27**, 402 (1939); R. Peierls, *Proc. Cambridge Philos. Soc.* **35**, 610 (1939).

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Author, Reviewer Play Ball

It may not be good form for authors to respond to reviews of their books—but forbearance has its limits. In his review of my book *The Science of Soccer* (PHYSICS TODAY, June 2003, page 64), John D. McCullen says that I give an "extraordinary incorrect" account of the bounce of the ball. In fact, my description is perfectly good physics. In a nutshell, here it is.