Math and Science Education

I would like to expand a little on Lauren B. Resnick's article "Mathematics and science learning: A new conception" (29 Apr., p. 477) on the basis of my own experience teaching geophysics at the university level. Geophysics is a field in which the qualitative aspects of science that she discusses are perhaps more obvious and accessible than in older, more precise disciplines, such as physics. My 8 years of teaching have taught me that my two greatest challenges are students' inexperience with problem-solving and with verbal expression of a scientific problem. I see these as closely related and as a symptom of the broader deficiencies in literacy and numeracy in students today.

The first deficiency, inexperience with problem-solving, is manifest in the persistent tendency to grab the nearest formula and start substituting numbers without first considering its relevance to the problem at hand. At a more advanced level, it is manifest in a reluctance to do first some "back-of-the-envelope" estimates before launching into a calculation that may be more elaborate than the problem demands. The engineering and physics majors in my classes, evidently having been drilled in applied mathematics methods, are more prone to this. It is also a widespread tendency in the research literature (and is probably abetted, in this context, by the desire to impress the audience with mathematical machismo). This much accords closely with Resnick's comments; in more old-fashioned terms, we might say that these students have not been forced to *think* enough about their scientific problems.

The second deficiency, in verbal expression, goes further. In order that my students appreciate the observational and logical basis of scientific inferences, I have had them write brief essays defending some hypothesis (for example, that the earth has a liquid metallic core). While most students have learned to do these reasonably well, it has usually been their first experience of such writing, and the shortcomings of a few have been illuminating. Some do not have a clear understanding of the difference between theory and evidence: cause and effect are confused. Many have difficulty organizing the material, and often the writing is wordy. The worst cases (these are college juniors and seniors) are essentially illiterate: their writing is ungrammatical and totally disorganized, although many of the relevant words and phrases might be present. I have concluded that these students have not been forced to think much about *anything*, be it science, history, or poetry. (Nevertheless, they often go on to graduate.)

I strongly agree with Resnick's suggestion that "teaching has to focus [more] on the qualitative aspects of scientific and mathematical problem situations" (my insertion). It is tempting in science classes to try to race through all of the topics that might be included in a given subject, but it is much better instead to be selective and to go carefully over the relevant observations and the comparison of these with deductions from various hypotheses. Quantitative problems may give practice only in the deductive phase of science; writing assignments expose students to the inductive phase and to the winnowing of rival hypotheses.

We might even find that, with more emphasis on qualitative aspects of science, the gulf between the "two cultures" will disappear. Scientists and engineers might become more literate, while nonscientists might realize that science is more than the dry recitation of "facts" and "laws" and begin actively to appreciate it as an integral part of our culture. After all, as Resnick's comments suggest, we will get through life on the basis of a complex of "naïve" theories about how the world works.

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Resnick provides an excellent brief account of current work in cognitive psychology and its important implications for math and science education. As she indicates, most cognitive psychologists view knowledge as consisting of highly organized schemata into which new experiences are assimilated and view the learner as actively constructing new knowledge. This view is consistent with the ideas that Piagetian theorists and educators have been propounding for many years, although Resnick's discussion is rooted in the more detailed analysis of specific knowledge and learning in specific content areas that typifies the information-processing paradigm of modern cognitive science.

Unfortunately, although Resnick may not have intended this, her article can be read as suggesting that the self-constructed theories children bring to their science classes are, on the whole, naïve and inappropriate views that must be replaced by more adequate scientific conceptions and that may hinder students in learning the latter. Although children undoubtedly do bring some incorrect preconceptions to their science classes, it should be emphasized that they also bring a wealth of crucial mathematical and scientific intuitions (for example, basic conceptions of speed, causality, transitive relations, and so forth) that they have constructed over the years and without which meaningful assimilation of the content of those classes would be impossible. Thus, the fact that classroom experiences are naturally assimilated into children's prior understandings is not so much a hindrance to learning accurate science as a basic phenomenon of cognition that makes learning possible at all and that educators should use to maximum advantage.

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It is a pleasure to reply to letters such as Davies' and Moshman's because their comments provide some of the elaboration and argument that were not possible in my brief essay. I am especially intrigued by Davies' suggestion that the processes of reasoning in the sciences and in more humanistic disciplines may turn out to be more similar than is often supposed. Cognitive research in language understanding and production is indeed suggesting that processes that have much in common with qualitative analysis in the sciences play a role in comprehending and writing complex texts of various kinds. Nevertheless, there is also evidence that the specific kinds of knowledge that people have affects the form of their reasoning. This means that, if reasoning can be taught, it can probably only be done in the context of specific domains of knowledge. Whether such domain-specific learning will in turn produce improved reasoning and expression in other domains remains to be seen, but I agree with Davies that there is room for cautious optimism.

Moshman's suggestion that children's intuitions and invented theories may be the very stuff out of which scientific competence can be built raises a central question for a cognitive theory of learning. At present, we do not know exactly what role invented theories play in learning. We know only that such inventions are virtually unavoidable and that invented theories are sometimes in fundamental conflict with scientific ones. We do not yet know much about the cognitive processes involved in modifying one's theories or in building new ones. Nor do we know whether typical invented theories are necessary steps on the way to scientific ones or just the result of gaps in experience and knowledge. We cannot say, therefore, exactly how invented theories should best be treated in the classroom.

These are the kinds of questions that can be answered only by the kind of continuing research in mathematical and scientific cognition that was advocated in my article.

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Soviet–U.S. Exchanges

With reference to John Walsh's article "Soviet–U.S. exchanges under scrutiny" (News and Comment, 22 July, p. 346), perhaps the members of the National Academy of Sciences will recall the comments of the late Philip Handler. Fittingly enough, the following words were delivered at the Helsinki Accords meeting in Madrid in 1980:

> We perceive no essential distinctions between pursuit of truth about the nature of man or of the physical universe and pursuit of truth about the human condition in the societies in which we live. We will continue to speak out for those whose rights have been denied, for the cost of silence is the abandonment of human rights, and that is a price we will not pay.

Later, at the Sakharov 60th birthday celebration held on 2 May 1981 at the Rockefeller University in New York City, Handler said:

> [W]e were criticized by some . . . for suspending our small program of Soviet bilateral symposia (not our individual exchanges) on the ground that we were deliberately reducing the very type of exchange we consider most essential to scientific progress; on the ground that we were punishing the Soviet scientific community which, itself, has no control over what happens to such people as Yuri Orlov, Sergei Kovalev, Anatoliy Shcharanskiy, or Andrei Sakharov; and, not quite consistently with the latter argument, on the ground that the cause of peace is deflected or damaged when scientists are prohibited from meeting; and finally, on the ground that we were not being even-handed, not also cutting exchanges with Argentina or Uruguay or Korea for their violations of human rights.
>
> Our response has been that the suspension of bilateral symposia rests on the fact that the Soviets have for years insisted on bilateralism to the exclusion of virtually all other modes of interchange. We are, in effect, forced to meet on their terms and conditions, therefore, in order to send a message that is loud and clear, we must do so in their chosen environment. And we lack similar opportunity in those countries with which we have no such agreements.

Of course we have no desire to punish the Soviet scientific community and we certainly agree to the presumption that the individual members of that community are innocent of the acts of indecency committed by an arm of their government with which they have no contact and on which they have no influence. But if one accepts that premise, one has a hard time in also accepting the notion that two astrophysicists, for example, talking their brand of science at a quiet meeting, somewhere, have any more influence in the Kremlin or in the White House concerning the outcome of SALT II, or the invasion of Afghanistan, or the state of play in Poland, or Cuba, than they have on the Soviet Government's respect for human rights. If they were unable to converse, the loss would be to science and, quite probably, to the morale of the already somewhat isolated Soviet scientific community, but not to these other, larger causes for which we had hoped so much. It has taken me all these years to acknowledge to myself that the loss to the cause of peace from loss of these innocent meetings would be very, very small indeed. . . .

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Japanese Computer Project

At the Sixth International Conference on Software Engineering in Tokyo last September, several senior officials of the Japanese Institute for New Generation Computer Technology (ICOT) presented a description of their Fifth Generation Computer (5G) project at a special session for the benefit of foreigners attending the conference. The reaction of many of the guests was incredulity; a few were outright hostile.

After recovering from my initial shock at the behavior of some of my fellow countrymen (who were, after all, guests at a presentation arranged especially for them), I tried to figure out what was evoking such a strong reaction. In discussing it later with my colleagues, we concluded that it was probably the vagueness of the ICOT plans for achieving what everyone admitted were very ambitious research goals.

The reaction of American researchers to the Japanese project plans says something important about the research funding climate in the United States. If a researcher in this country were to present such an ambitious and expensive research proposal with such vague plans, not only would he not receive funding for the project but most likely he would also suffer such a "loss of face" that he would not receive funding for any future research proposal.

The Japanese, on the other hand, say that "vagueness" is necessary and unavoidable—perhaps even desirable—in long-term projects of "basic" research. They very openly admit that they have never before attempted such a large basic research undertaking as the 5G project and that they are very much concerned about how to manage such a "high risk" endeavor. To be sure, they went through an exhaustive 3-year study to decide what the goals of the project should be but, having done that, they did not insist upon detailed plans spelling out just how the goals would be achieved. Rather, they sought to put together highly qualified research teams, provide them with carefully selected leaders and advisers, assure them of ample funding for at least the first 3 years, and leave them alone to get on with the job. If the Japanese must prove to the world that they are capable of "creativity" in doing basic scientific research, I cannot think of a better environment in which to do it—except, perhaps, one with less publicity and fewer interruptions by a constant stream of visitors.

Thus, the reaction of the audience in Tokyo last fall may not really have been directed at the 5G project itself nor even at the challenge to American supremacy in computer science that it represents. Instead, it may have been prompted by the conditions under which American scientists must try to respond. Certainly, we have the best of intentions in trying to get as much as we can out of every last research and development (R & D) dollar, but perhaps we are overmanaging R & D in the United States. "Golden Fleece Awards" notwithstanding, we cannot expect to know in advance (i) how long each project will take, (ii) what the results will be, and (iii) how much it will cost. If we knew all of that, it simply would not be R & D. Moreover, increasing concerns over "technology transfer" may be leading us to poison our own research environment still further by imposing Department of Defense censorship.

Perhaps, as with previous Japanese efforts, the primary contribution of the Japanese 5G project will be managerial, not technological. In that case, the form of American responses to the project may be more important than their content.

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Erratum: In the report "Spectral consequences of photoreceptor sampling in the rhesus retina" by J. I. Yellott, Jr. (22 July 1983, p. 382), the first sentence of the legend to figure 1 (p. 383) should read: "(Column 1) Photomicrographs (× 900) of 60- by 72-μm sections of the rhesus retina (7)."