their opinions and ideas to this discussion.

STEVE GANTHNER
Carolina Power & Light
10/83 *Raleigh, North Carolina*

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The September issue had a good discussion of physics in our public schools. One thing I believe is important to get more students to study physics is to stress the use of physics in everyday life.

In my neighborhood, almost everyone believes that physics is only needed by those going to college. Since this is an agriculture community, our schools stress vocational agriculture. The students never learn how to use physics in agriculture. All boys and most girls are interested in automobiles, but they have no chance to learn how physics is used in automobiles. Most girls take "home ec," but they never learn how physics is used in the modern home.

Some "Future Farmers of America" (vocational agricultural ed students) go on to agriculture college. They may even get a PhD in agriculture and never learn how to brace a fence post.

The interesting work in subatomic particles seems to have caused the authors of physics textbooks to forget that every living person has a need to understand the use of physics in everyday life. Of course, more than one year is needed to teach physics as it should be taught.

Please stress the need to understand physics in everyday life!

JAMES F. JACKSON
10/83 *Carlisle, Indiana*

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Your special issue, "Crisis in High-School Physics Education," dramatizes a long-standing and growing problem. It is little wonder that there is a crisis! A check of the "Positions Open" at the back of the same issue reveals that of the 54 advertisements for college and university positions, only 4 were for people primarily interested in teaching. The others: 27 research appointments and 21 for people to do both research and teaching. In most cases, more importance was placed on research than on teaching, as indicated by comments such as "Ability to attract grants," "Proven research record" or "Extensive publications." Has higher education become more research than education? In the past, college teachers spent their spare time working with students, planning and building new demonstrations and generally finding ways to make physics interesting and exciting to all students, particularly in the general physics classes where the potential high-school teachers are. Today, administrators require that professors spend their time planning and writing grant proposals as even small colleges scramble to imitate the prestige schools in their quest for the research dollar. How can professors with, at best, divided interest in teaching generate interest in physics and in teaching physics among their students?

FRED B. OTTO
9/83 *Orono, Maine*

Breakthrough in phase problem

Your recent article "New method for determining the phases of diffracted x rays" (November 1982, page 83) and the comment "Breakthrough questioned" given by Robert Colella (February, page 112) seem to ignore the fact that the first determination of a difficult crystal structure has been realized. In my paper cited in your article, a concrete method for phase determination was developed, and the success in solving a difficult structure that could not be solved previously with the ordinary direct methods was reported. A series of articles on the application of this method to crystal-structure determination [Acta Cryst. **A38**, 414 (1982)] and on the discussion [Acta Cryst. **A38**, 516 (1982)] of the shortcoming of the previously proposed methods by others (those mentioned in your article) have been published. A detailed account, presented in the 1982 March meeting of the American Crystallographic Association, on the technique of solving difficult crystal structures using my method together with the direct methods, appeared recently in *Acta Crystallographica* [**A39**, 98 (1983)].

The citation of the 1982 Warren award referred mainly to Benjamin Post's 1977 contribution. Post deserves this award for his deep understanding of the mechanism of dynamical n-beam Borrmann (transmission) diffractions in connection with the invariant phases of structure factors, although correct phases cannot be determined using his method because of the influence of the crystal thickness on the transmitted intensity.

The phase determination of germanium and zinc tungstate, mentioned in your article, has nothing to do with the award, since these phases were determined using reflection-type multiple diffraction, which has no connection with Post's 1977 paper. This determination of phases resulted from the consideration of the relative motion of the crystal lattice, which was originally proposed by me during my sabbatical leave at the Max Planck Institute for Solid-State Research in 1981. (My earlier publication, Appl. Phys. **A26**, 221 (1981), concerning this idea was not cited in your article.) Without considering this lattice rotation, correct phase information cannot be attained. Moreover, reflection-type diffractions differ from the transmission ones in the involved scattering matrix. The matrix of the former is Hermitian, while that of the latter is not. Besides, the degeneracy of the dispersion surface in the reflection case is quite different from that in the transmission case. The sentence, "Chang has developed a variation on Post's procedure and has used it to determine phases in an intermediate compound," contained in your article is therefore incorrect.

In the 1974 paper of Colella, the neglect of the effect of the coupling reflection on the diffracted intensities in n-beam cases affects the correctness of this paper. This neglect usually disconnects the phases and the n-beam diffractions, because the invariant phase is the sum of the phases of the primary, the secondary and the coupling reflections. It is meaningless to deal with the phase problem without considering the coupling reflections. In addition, because no concrete method was proposed in this paper, in no case would it be able to provide phases.

The so-called "virtual Bragg scattering," is, according to Colella, due to the failure of the conservation of energy; this statement is of course mistaken. In fact, the energy is conserved at any crystal setting in n-beam diffractions. Simple proof can be obtained by considering the Poynting vectors of the diffracted beams. Furthermore, the effect of the lattice rotation on the diffracted intensities was not considered. Colella's claim of priority in this field is then not supported by his publications.

Both Post's and Colella's methods lead to no correct phase determination. This is why no crystal-structure determination using their methods has been reported.

SHIH-LIN CHANG
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5/83 *Campinas, S.P., Brazil*

AN AUTHOR RESPONDS: A careful reading of Shih-Lin Chang's letter and the publications to which he refers raises a number of disturbing questions regarding the validity of the points which he makes in his letter and about some aspects of his research activities.

In the letter, we are informed that his procedure made possible "success in solving a difficult structure that could not be solved with the ordinary direct methods." That phrase is repeated, almost verbatim, in at least four of Chang's recent publications.[1] Three were published in 1982; one in 1983. Nowhere in his letter, nor in any of the three 1982 publications, does Chang mention the very relevant fact that the "difficult structure" was solved and described in a paper submitted to *Angewandte Chemie* on 12 March

1981(2), by H. J. Deiseroth and Han Fu-Son, several months before any of Chang's series of articles were submitted for publication. It should be noted that Han Fu-Son (or Fu-Son Han, as he has been referred to recently) began a scientific collaboration with Chang early in 1981 and is a coauthor of two of the papers mentioned by Chang.

In the fourth paper of the series, Chang and Han mention the prior determination of the structure briefly. In the published abstract of that paper, however, immediately following a short discussion of their phase activities, they write: "The final E map shows all the atomic positions of Cs, Ga and Se. The structure refinement gives $R = 0.047$, $R = 0.053$." (The Rs are measures of the quality of the structure determination.) The authors neglect to inform the reader that those results were taken, unchanged, from the earlier Deiseroth–Han paper. Without that information, the reader would undoubtedly assume that the quoted data were obtained as a direct result of the application of their phase procedure.

Clearly, Han, and presumably Chang as well, knew the atomic positions and the reflection phases early in 1981, well before "the first determination of a crystal structure had been realized." Prior knowledge of the phases does not, of itself, rule out the possibility of presenting a procedure for the experimental determination of x-ray reflection phases in a convincing and scientifically acceptable manner. That could be done by justifying the phase assignments for all detectable n-beam interactions by explicit references to the original data chart. The phase indications displayed by each interaction should be clear and unambiguous, to the reader as well as to the authors. There is little evidence of any such approach in Chang's presentations. For that reason and because of the circumstances outlined above, it is difficult to take seriously the points he raises in his letter.

References

1. S. L. Chang, Phys. Rev. Lett. **48**, 166 (1982); Han Fu-Son and S. L. Chang, Abstract P4, Meeting of the American Crystallographic Ass'n, March 1982; S. L. Chang, Acta Cryst. **A38**,, 521 (1982); Fu-Son han and S. L. Chang, Acta Cryst. **A39**, 98 (1983).
2. Hans-Jorg Deiseroth, Han Fu-Son, Angew. Chem. **20**, 962 (1981).

BENJAMIN POST
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9/83 *Brooklyn, New York*

AN AUTHOR RESPONDS: In regard to the letter by Shih-Lin Chang I have the following comments:

I have not been able to find, in any of the references mentioned by Chang, a case in which an unknown structure has been solved using his methods. The only reference was in the *Physical Review Letter* mentioned in the November editorial concerning $Cd_{10}Ga_6Se_{14}$; however, that was a short passing mention. The complete work appears in *Acta Crystallograhica* [**A39**, 98 (1983)] by Chang *et al.*, in which it is explicitly stated that the structure had been previously solved by other authors in 1981 using other methods. The point made in my letter, namely, that no unknown structure has been solved so far using multiple diffraction methods, still holds.

The criticisms of my previous paper in *Acta Crystallographica* (1974) are worded in such obscure terms as to defeat any rebuttal. Chang's view is that my 1974 paper is wrong and therefore does not constitute a precedent for establishing priorities. However, his justification for such an assertion is completely gratuitous.

Chang's comments on my work on "virtual Bragg scattering" indicate that he does not know the meaning of "virtual transitions" in physics.

ROBERTO COLELLA
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Teaching physics with history

Much attention has been given recently in your pages to the Bronx High School of Science, its teaching methods, its distinguished alumni and the problem of teaching science today, especially physics, to both high-school and college students.

As a young physicist who was privileged to take graduate courses with three Bronx Science alumni—Sheldon Glashow, Steven Weinberg, and Roy Glauber—while a special graduate student in the physics department at Harvard (1979–1981), I think I can make an observation on the similarity of their viewpoints with regard to both teaching and learning science, especially physics.

All three, it seems to me, place a heavy emphasis on the historical view of science. Glashow introduces "matter" and the organizing principles used to understand its macroscopic structures with original source material, following the scheme of Weeks' *History of the Elements*. Weinberg introduces 20th-century physics to his students with original source material, following the scheme of Whittaker's *History of the Theories of Aether and Electricity*, Part I. And Glauber sent me to the Niels Bohr Library of AIP (when I was a teaching assistant to one of his courses) to examine and obtain reproductions of photographs of all the major figures of the 19th- and early 20th-century physics researchers posing *with their apparatus*.

These three major figures of elementary particle physics research and physics teaching today, Glashow, Weinberg and Glauber, it seems to me, all have what is commonly referred to as "a deep sense of history."

In this conception, history of science is not thought of as an obstruction to progress, or a burden of irrelevant personal anecdotes somehow to be borne, but rather as a psychological vehicle for understanding conceptually how discoveries were made in the past, how current discoveries "fit in," like pieces in the grand puzzle, akin to Kuhn's *paradigm* concept, and how discoveries are likely to be made in the future. History of Science thus understood tells us: "What is a discovery?"

Einstein, it seems, thought of science as answers in search of questions. History of science provides us with these "answers."

Likewise, Glashow can frequently be heard admonishing an experimentalist using SLAC to "do Rutherford"—and can justify the search for new superheavy elements in manganese nodules by pointing out that because Mendeleev thought of the number "7" as a "magic number," he mistakenly refused to accept the possibility of an eighth column in the Periodic Table, with its requisite member elements. And so also elementary particles are organized in the "eight-fold way"; but is there perhaps a ninth way? a tenth? Glashow argues from the history of science that we should learn to "expect the unexpected," in our higher and higher energy search of matter and the Universe.

As these three wise men teaching physics at Harvard know, not only does an orderly presentation of the history of physics form a conceptual framework within which their students can organize their understanding of the abstractions of modern physical mathematical formulae and derivations, from Rutherford scattering (after all, the original Geiger counter was Hans Geiger himself!) to relativity to quantum theory to field theory to electroweak theory to Grand Unified Theory; but also, the historical conception can serve as a means for those students, as it serves their teachers, to provide a justification and lucid rationalization to organize, identify, integrate and develop their own discoveries, which, after all, compose our future.

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