

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 R s 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).

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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 $\text{Cd}_{10}\text{Ga}_6\text{Se}_{14}$; however, that was a short passing mention. The complete work appears in *Acta Crystallographica* [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.

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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 super-heavy 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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