Cinema as Physics Lesson

I enjoyed the article "Teaching Physics with Superheroes" (PHYSICS TODAY, November 2002, page 29) and Jim Kakalios's idea of using a medium familiar to many students (comic books) to teach principles of physics.

At the University of Central Florida, a similar project has been under development for some time by two of our faculty members, Ralph Llewellyn and Costas Efthimiou. They developed a new course, "Physical Science: Physics in Films," so that students would be motivated to learn physical science, would understand the scientific method, and would learn to distinguish between science and pseudoscience. The course focuses on Hollywood films' use of physics concepts, including electricity and magnetism, mechanics, relativity, and time travel.

Each instructor has so far taught the course twice, to a total of more than 750 students. The results are very encouraging. The instructors found that the traditional "fear of physics" common in general education students seems to be greatly reduced. The instructors can cover more difficult topics that are typical of algebra-based college physics courses and students do not object because they are familiar with the medium that the materials arose from. Furthermore, the students' overall performance is greatly enhanced, as measured by test scores from this and the traditional course given by the same instructors.

During the regular end-of-course evaluation, many students have written comments encouraging further development of the new approach. The university's student newspaper has published two articles dealing with the course.

We believe that this course, like those of Kakalios and undoubtedly others, can make a difference in educating non-science majors about scientific principles and public issues that include scientific components.

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Julian Schwinger's Paradoxical Legacy

We were pleased to see that PHYSICS TODAY has published a review (April 2002, page 77) of our book, *Climbing the Mountain: The Scientific Biography of Julian Schwinger* (Oxford U. Press, 2000), but not so pleased with the content of Tian Yu Cao's review nor with the fact that PHYSICS TODAY chose to headline it as "Julian Schwinger, But Not the Definitive Julian Schwinger."

The review seems to us, first, to be unrepresentative. The first two-thirds of the book, which deals with Schwinger's early remarkable career in nuclear physics, his profound work on waveguides and synchrotron radiation during World War II, his conquest of quantum electrodynamics, and his groundwork on what became the standard model, is hardly mentioned. Instead, the reviewer concentrates on Schwinger's disillusionment with operator field theory in the late 1960s, his resulting development of source theory, and his increasing isolation from the mainstream. It is, of course, of great interest to explore that late part of his career. Indeed, the points illuminating Schwinger's rejection of his own creation, renormalized quantum field theory, mentioned by Cao, appear verbatim in our book.

The penultimate paragraph of the review seems particularly unfair. Our book is not intended as a definitive review of quantum chromodynamics,

a theory that Schwinger in fact detested. Schwinger likewise had little interest in such subjects as constructive field theory and asymptotic freedom. As for the establishment of the flavor SU(3) symmetry, we disagree with the reviewer. Although it is true that Murray Gell-Mann and others had proposed the symmetry at least as early as 1961 (as mentioned in our ref. 17, p. 408), it was not generally accepted until the discovery of the predicted Ω^- hyperon in 1964. This history is important to understanding Schwinger's 1964 work on the field theory of matter, in which he proposed an alternative symmetry group $W_3 = U_3 \times U_3$. By the following year, he had adopted unitary symmetry.

We were gratified that Cao found our book "extremely timely" and the anecdotes "entertaining." However, he claims that our biography is neither "definitive [nor] scholarly." We cannot seriously dispute the claim that it is not definitive. How can any account of a multifaceted genius be so? Particularly difficult is a subject like Schwinger, who prized his privacy, carefully erased his tracks in his writings, and was, in many ways, far less accessible than Richard Feynman. But his legacy lives on in his many students and his seminal oeuvre, so that a century from now a clearer view will be possible.

From the present vantage point, we have attempted to construct the most complete biography possible, based on extensive interviews with Schwinger, his wife, and a great many of his students and colleagues, and on close attention to his work. We do hope we have, in the reviewer's words, brought out something "of value to those physicists, historians, and philosophers who are concerned with foundational problems in fundamental physics." To impugn our scholarship on the basis of insubstantial failings seems unfair. Let the reader judge!

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Cao replies: The scholarship of a scientific biography is mainly judged by its analysis and assessment of the subject's scientific legacy. Julian Schwinger's legacy lies primarily in the foundations of fundamental physics: his operator formalism of quantum field theory, combined with a renormalization scheme; and his motivations and justifications in searching for a new scheme—the source theory—that has great impact on the conception of effective field theories. This paradoxical legacy has been extensively explored over the past two decades,[1] but was virtually untouched by Mehra and Milton. That insensitivity explains their failure to properly treat Schwinger's works about scaling, asymptotic freedom, and quantum chromodynamics—works that were dictated by his foundational concerns.

With one exception, Mehra and Milton call the inaccuracies in their conceptual understanding and historical details mentioned in my review "insubstantial failings." The exception, they say, "is important," and they disagree with me on that one: In the review, I wrote, "To say that in 1964 'the approximation symmetry group was not yet established' directly contradicts the historical facts: Since 1959, Sheldon Glashow and Murray Gell-Mann were publishing on the subject in terms of the soft-mass problem." Mehra and Milton rebut that statement by claiming that "the flavor SU(3) symmetry . . . was not generally accepted until . . . 1964." Their rebuttal, ironically, demonstrates that they still do not understand that the two notions are not identical. For Glashow, the approximation symmetry group mainly referred to a softly-broken gauge symmetry rather than the flavor symmetry. That is why I mentioned the soft-mass that was designed by Glashow[2] to break the gauge symmetry softly.

References

1. See, for example, T. Y. Cao, *Conceptual Developments of 20th Century Field Theories*, Cambridge U. Press, New York (1997).
2. S. L. Glashow, *Nucl. Phys.* **10**, 107 (1959).

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