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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