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a theory that Schwinger in fact de-
tested. Schwinger likewise had little
interest in such subjects as construc-
tive field theory and asymptotic free-
dom. 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 pro-
posed 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 nei-
ther "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 pri-
vacy, 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 moti-
vations 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 insensitiv-
ity 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 histori-
cal details mentioned in my review
"insubstantial failings." The exception,
they say, "is important," and they dis-
agree with me on that one: In the re-
view, I wrote, "To say that in 1964 'the
approximation symmetry group was
not yet established' directly contra-
dicts 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 state-
ment by claiming that "the flavor
SU(3) symmetry . . . was not generally
accepted until . . . 1964." Their rebut-
tal, 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 fla-
vor symmetry. That is why I men-
tioned the soft-mass that was de-
signed by Glashow² 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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