cannot be explained by some mathematical structure, algebraic, topologic or otherwise.

References

1. E. P. Wigner, Commun. Pure Appl. Math. **13** (1), 1 (1960).
2. A. Einstein, *On the Method of Theoretical Physics*, Clarendon Press, Oxford, UK (1933).
3. Quoted in Fritz Rohrlich's obituary of Jauch, PHYSICS TODAY, December 1974, p. 70.

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A Spacetime Note, with Strings Attached

In his excellent review article, "Reflections on the Fate of Spacetime" (April, page 24), Edward Witten paints an intuitive picture of where string theory may be headed. I would like to add a footnote on the implications for cosmology.

Assume a stringy cosmos ($\alpha' \neq 0$) that is gravitationally closed. Then, as the cosmos begins to contract, there is a finite limit, $\sqrt{\alpha'}$, to how small its radius can become. After this limit is reached, momentum and wrapping states are exchanged and the cosmos again expands in a different "direction." In other words, just as replacing worldlines with worldtubes eliminates the singularities from Feynman diagrams, $\alpha' \neq 0$ eliminates the singularity of the Big Bang's initial state. Of course, so does $\hbar \neq 0$. However, $\alpha' \neq 0$ also explains why a closed cosmos will oscillate. Indeed, because string theory predicts gravity, in the sense noted by Witten, a stringy cosmos requires only sufficient mass to create the Big Bang.

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Clarification on De Vaucouleurs Obit

In our obituary of Gérard de Vaucouleurs (April, page 76) the late Antoinette de Vaucouleurs should have been identified as his first wife. De Vaucouleurs' second wife, Elysabeth, survives him.

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Author Disputes Review of Richard Feynman Biography

I take strong exception to Silvan S. Schweber's review (October 1995, page 64) of my book *The Beat of a Different Drum: The Life and Science of Richard Feynman*. I find it to be intemperate and patronizing.

Much of his tract is devoted to faulting me for having failed to make use of or acknowledge the efforts of other scholars. He begins by declaring that it is "somewhat difficult to comprehend" how one can write about Feynman without mentioning James Gleick's *Genius: The Life and Science of Richard Feynman* (Pantheon, 1992). I seriously disagree. (I also wonder how Schweber could praise Gleick's book as a "superb prior biography" without mentioning the fact that one of the two people acknowledged by Gleick for their "patient guidance and sharp insights in matters of physics" is Schweber himself.)

My biography and Gleick's were written independently during the same period, 1988–92. Upon Feynman's death on 15 February 1988, Gleick wrote an obituary published in the *New York Times*, and on 24 February I gave an invited memorial talk at Cornell University on "My Last Encounter with Richard Feynman." On that occasion, I also interviewed Hans Bethe about Feynman. A few weeks later, Bethe notified me that Gleick intended to write a biography of Feynman and that, if I heard from Gleick, I should give him whatever assistance I could. I sent Gleick a copy of my Cornell talk on Feynman and telephoned him to discuss our respective projects. Although most of my scientific/historical writing to date had been quite technical, I had intended to write a more popular scientific book for the general reader. However, upon learning about Gleick's plan to write a popular biography, I decided to write about Feynman's work in rigorous scientific terms, albeit in a deeply human context, primarily for the scientific community.

Because of protracted illness, I did not submit the manuscript to my publisher until the end of September 1992, several weeks before Gleick's biography was published. My publisher decided to let Gleick's book have its play before issuing mine, and to proceed with the production phase of my book at a leisurely pace. I took advantage of the delay to send my manuscript to Bethe and Willis E. Lamb Jr, among others, for their

review; their comments proved to be immensely helpful.

I also read Gleick's book at that time. Although I found it interesting and also informative on certain issues—such as Feynman's private life, especially his sexual escapades—I also found that I disagreed with Gleick on certain aspects of Feynman's scientific achievement. For example, in assessing *The Feynman Lectures on Physics,* Gleick declared that "no scientist since Newton had so ambitiously and unconventionally set down the full measure of his knowledge of the world—his own knowledge and his community's" (page 363). Feynman was a great physicist, but comparing him to Newton was journalistic hyperbole, I thought. In discussing with me his discovery of the V – A law of weak interactions, "the only law of nature to which I could lay a claim," Feynman had been realistic enough to acknowledge that "it was nothing like the discoveries of Maxwell or Dirac," not to mention Newton.

Schweber alleges that my book gives only "reticent acknowledgment" to Charles Weiner's 1966 interview with Feynman. What exactly is "reticent" about my having mentioned the interview in my acknowledgments and cited it 16 times in the text? On 6 January 1988, shortly before I was to visit Feynman for an extended series of taped discussions, Feynman called me to suggest that I read his 1966 interview, for he was doing the same to refresh his memory. Unfortunately, I was not able to obtain a copy until after my visit, and when I did study it, I found it essentially a most interesting monologue by Feynman, with no attempt on the interviewer's part to explore critical questions.

Damning me with faint praise, Schweber asserts that my book holds interest because I deal with Feynman's thoughts and opinions (Pray, what else should a book concerning Feynman's life and work deal with?) and because I have made use of the "best available secondary sources." He implies that I have failed to make use of primary materials. I infer that he means such sources as Weiner's 1966 interview with Feynman. I also infer that he does not mean any of my own formal interviews with Feynman, the first having been in May 1962 and the last early in 1988 (on that last occasion, Feynman and I talked every day for over two weeks). I am baffled as to why Schweber scorns me for citing my own interviews with Feynman, in addition to the references I give to other inter-

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views with Feynman. Why else would I have conducted interviews with Feynman, or others, if not to use them?

Nor is Schweber willing to give me credit for my analysis of Feynman's original scientific contributions in their historical context. Although he does admit that I have gathered "new information" from Feynman's boyhood friends and acquaintances, teachers in high school, college and university, professional colleagues, friends and students, as well as scientific experts (in fact, I interviewed some 80 people), he seems to believe that they are all merely secondary sources.

Schweber quibbles about the space I have devoted to various aspects of Feynman's work. Feynman's work on the use of the principles of least action and path integrals in quantum mechanics and his contribution to quantum electrodynamics constituted the greatest achievement of his life—the centerpiece of his scientific oeuvre—and they naturally occupy pride of place in my book. Even so, I believe I have properly covered all his other contributions as well.

Schweber wonders why I give the polaron problem such detailed treatment. If he had read the relevant chapter carefully, he would have learned that Herbert Fröhlich's writings had led Feynman to believe that the solution of the intermediate coupling problem in the theory of the polaron would be the key to the solution of the problem of superconductivity—a solution to which Feynman was deeply attached emotionally and psychologically. When he did solve the intermediate coupling problem in the theory of the polaron, he asked Fröhlich, "Now that we have solved the polaron problem, what about superconductivity?" As I point out in my discussion of Feynman's work on the polaron (pages 428–30), J. Robert Schrieffer was led to the famous BCS *Ansatz* via an adaptation of the intermediate coupling polaron wave function developed in 1953 by Tsung-Dao Lee, Francis Low and David Pines (*Physical Review*, volume 90, page 297). That insight did not occur to Feynman, who was content to solve the polaron problem not necessarily because of its intrinsic interest but because he found in it a fundamental application of his path-integral method to derive his variational principle. However, the chief goal had been to solve the problem of superconductivity by first solving—as Fröhlich had predicted—the polaron problem.

This goal escaped Feynman, and that made him very unhappy.

Schweber finds my book to be a "pastiche" and thinks I have done the "easy" work of writing a biography. My book is no more a pastiche and no less easy work than Schweber's own *QED and the Men Who Made It* (Princeton U. P., 1994). I have told the story of the life and achievements of one of the great physicists who made QED and did much else. I have done it with accuracy, dedication and much hard work.

Schweber concludes his review by declaring that Feynman "deserves better" than my book provides. I think my book deserves better than Schweber provides in his review, which impugns my scholarship and integrity without, in my view, offering either justification or evidence.

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SCHWEBER REPLIES: I stand by my original review.

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US A-Bomb Success Was Very Graphitic

In his letter about the World War II atomic bomb projects (January, page 83), Mark Singer states that the Germans used scarce heavy water as a moderator while we used relatively abundant and inexpensive graphite. The fact is that our graphite was an extremely refined material, made before the war only in small amounts in St. Louis for electrodes for arc lamps used in spectroscopic analysis.

Constructing the facilities to manufacture a roughly 40-foot cube of the material for the first Hanford reactor (where I was working at the time), and doing so in little more than a year and with no manufacturing pilot plant, was in fact a great coup for American industry. As noted by Singer, the Germans experimented with graphite as a possible structural material, but with discouraging results.

As is well-known, the German supply of heavy water could have been much more abundant if the Norwegian underground had not been able to sabotage its transport. The Allies were able to manufacture quite large quantities of heavy water by electrolysis of ordinary water in British Columbia, with a relatively easy separation.

After the war, heavy water was