

On Schwinger and QED

We read the Search and Discovery piece "Physics Nobel Prize to Nambu, Kobayashi, and Maskawa for theories of symmetry breaking" (PHYSICS TODAY, December 2008, page 16) with great interest. Since it addresses history, we feel compelled to point out that work at Harvard University anticipated the cited breakthroughs in both quantum electrodynamics and the electroweak synthesis.

Julian Schwinger was undoubtedly the first to solve the problems of renormalization in QED.¹ Richard Feynman's second paper on QED directly precedes Schwinger's second covariant reformulation of his own theory. It is seriously ahistorical to attribute perturbative QED to only Feynman and Sin-itiro Tomonaga. All three shared the Nobel Prize for QED. We think it was the physics community's frequent failure to recognize Schwinger's fundamental contribution to QED that led C. N. Yang to observe, "I believe Schwinger was justifiably unhappy that the younger generation, dazzled by the brilliant performer that Feynman was, have forgotten that it was Schwinger who had first scaled the mighty peak that is known as renormalization."²

In the 1950s Schwinger went on to lay the groundwork for what eventually became the electroweak synthesis.³ Details were wrong because of the experimental confusion at the time, but his work led directly to that of Sheldon Glashow,⁴ six years before that of Steven Weinberg. Glashow, Weinberg,

and Abdus Salam shared the Nobel Prize for the unification of weak interactions with electromagnetism. But as Peter Higgs correctly noted, "That vacuum expectation values of scalar fields, or 'vacuons,' might play such a role in the breaking of symmetries was first noted by Schwinger."⁵

Fame is fleeting, and even Nobel Prizes do not confer lasting memory on their recipients. But let's try to keep the record straight for a new generation of physicists, who may pick up their knowledge of history from PHYSICS TODAY's pages.

References

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2. C. N. Yang, in *Julian Schwinger: The Physicist, the Teacher and the Man*, Y. J. Ng, ed., World Scientific, Singapore (1996), p. 175.
3. J. Schwinger, *Ann. Phys. (N.Y.)* **2**, 407 (1957).
4. S. Glashow, *Nucl. Phys.* **22**, 579 (1961).
5. P. W. Higgs, *Phys. Rev.* **145**, 1156 (1966).

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Fired Tsukuba professor's defense

I have some comments in response to the letter by Hiroshi Mizubayashi (PHYSICS TODAY, February 2009, page 12), in which he defends the University of Tsukuba report that claimed that I falsified data in an article published in 2006 in *Physical Review Letters (PRL)*.¹

No falsification took place. Certain analyzed data files were lost or erased by the two students who lodged a complaint, but we fortunately had equivalent original raw data files. Any errors that may have occurred in the analysis of the raw data were entirely inadvertent. If the procedures were insuffi-

ciently explained or if insufficient caveats were given, that was also unintended. Any such deficiencies in the *PRL* paper were not only inadvertent but also innocuous and left intact our main lines of thinking. A subsequent, more detailed publication,² not limited by the *PRL* page limit and inappropriately ignored by Mizubayashi's committee, remedied any identifiable deficiencies—and notably reached the same conclusions drawn in reference 1.

Nonetheless, I was ordered by the University of Tsukuba to withdraw the *PRL* article. I refused, because a simple erratum would suffice to adequately acknowledge any problems. I have asked the editors of *PRL* to publish the following simple erratum: "Part of the data used in Figs. 1 and 3 is based on different equivalent shots. However, a subsequent published analysis of a single equivalent shot, together with more detailed explanations of the methods of analysis, reached identical conclusions."² I ask my colleagues who have been urged by the university to disavow the article to join me instead in this succinct and sufficient erratum. There is no need whatsoever for disavowal of the article or its conclusions.

The Tsukuba position is incredible, outrageous, heavy-handed, and profoundly damaging. At stake for the field are scientific freedom and process. At stake for me is something far beyond the loss that I have endured of a directorship and a professorship. No one can know the full agony that this defamatory and unfair report has caused me, but anyone who has, like me, fully devoted himself to a life of scientific pursuit can appreciate the special kind of pain that such a report can cause.

What heartens me is that three senior coauthors also refused to withdraw our article. I am heartened as well by the fact that the notorious Tsukuba report has been seriously questioned by 11 prominent scientists in the field (PHYSICS TODAY, December 2008, page 10).

To find a fair forum through which to counter their defamatory report, I have brought civil suit against the University of Tsukuba. Through the legal

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discovery process, Tsukuba has now been forced to reveal facts that undermine any presumption of fairness and honesty that would normally be accorded an academic institution in the preparation of such a report. I and my coauthors have an online response to the Tsukuba report (see <http://www.cho-teruji.org/ScientificExplanationFigss.pdf>).

References

1. T. Cho et al., *Phys. Rev. Lett.* **97**, 055001 (2006).
2. T. Cho et al., *Phys. Plasmas* **15**, 056120 (2008).

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Hiroshi Mizubayashi's letter defending the University of Tsukuba's action against Teruji Cho suggests that we and our nine letter cosigners might not have had a full grasp of the incident and the procedure followed by the university. We did, however, have access to the reports that summarized the university's evidence and found them seriously wanting in reaching the conclusion of any falsification of data. It seems to us that it is the university that lacks access, since its report fails to consider the subsequent clarifying article published by Cho in *Physics of Plasmas*.¹ Mizubayashi observes that following his investigation, 23 coauthors—all at Tsukuba—asked *Physical Review Letters* to withdraw their names from the paper. Yet Vladimir Pastukhov, one of four coauthors dissenting from the university's findings and the only one outside the university's disciplinary influence, stands by the original publication. He believes that it is one of the more significant works of the GAMMA-10 group. In summary, Mizubayashi's letter does not allay our and our cosigners' concerns about whether an accurate, fair, and transparent academic procedure has been followed.

Reference

1. T. Cho et al., *Phys. Plasmas* **15**, 056120 (2008).

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Mizubayashi and Akahira reply: The University of Tsukuba finds no reason to alter its position that Teruji Cho's conduct in the preparation of the *PRL* paper¹ constitutes scientific misconduct (*PHYSICS TODAY*, February 2009, page 12). Cho claims that any error or insufficiency in the *PRL* paper is inadvertent and innocuous. The Investigation Committee, which included three internationally known plasma physics experts from outside the university, did not find them so after a fair and thorough investigation.

Cho also claims that the *Physics of Plasmas* paper explains any deficiencies,² and furthermore reaches the same conclusion as the *PRL* paper. It is our view that the *PoP* paper, which was submitted after the investigation started and without Cho's giving notice to the Investigation Committee, cannot be used to judge whether Cho carried out scientific misconduct in the preparation of the *PRL* paper. Needless to say, the conclusion of a paper reached through misconduct is meaningless.

Cho has brought a civil suit against the University of Tsukuba. We are confident that the court will fully sustain the university's position on this issue.

References

1. T. Cho et al., *Phys. Rev. Lett.* **97**, 055001 (2006).
2. T. Cho et al., *Phys. Plasmas* **15**, 056120 (2008).

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Reviewer dislikes Hoax, perhaps intensely

Every author has to expect that some reviewers will dislike his book, perhaps intensely. That is par for the course. But one might hope that even a scathingly negative review would be accurate in its summary of the book's contents and principal arguments. Alas, Peter Saulson's review (*PHYSICS TODAY*, December 2008, page 56) of my book *Beyond the Hoax: Science, Philosophy and Culture* (Oxford University Press, 2008) fails to meet that minimum standard.

Saulson implies that the whole book is a rehash of the stale science wars debates from the mid-1990s—a characterization that could at best apply to the first third of the book, whose function is simply to set the stage for the rest. Saulson does not even mention the two chapters on the philosophy of science or the long chapter on pseudoscience; and he mentions the chapter on religion only to grossly misrepresent it (see below).