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More on polaron theory history

A recent Readers' Forum section (PHYSICS TODAY, April 2015, page 10) contains a discussion by Mark Dykman and Emmanuel Rashba about my review (October 2014, page 54) of *Polarons* by David Emin and about some aspects of the early days of polaron theory.

The term "polaron" was indeed coined by Solomon Pekar.[1,2] I agree that Pekar's coinage is sometimes overlooked in the literature.

The development of the polaron concept was a gradual process initiated by Lev Landau. Pekar recognizes in his monograph[3] that "in 1933, L. D. Landau proposed an important idea on the auto-localization of an electron in an ideal crystal as a result of a lattice deformation by the field induced by the electron. These local states were assumed to be immobile, and Landau tried to associate them with *F*-centers in colored alkali halide crystals." I think it is fair to state that all successive steps on polaron physics emerged from Landau's first step.

For my book review, I found it unnecessary to write a more detailed introduction, and I limited the historical remarks on the polaron concept to the initial contributions of Landau. He introduced the nascent concept to Herbert Fröhlich,[4] who, in turn, introduced the commonly used basic polaron Hamiltonian for the continuum approximation to Jiro Yamashita, Theodore Holstein, and others, who laid the foundation of small-polaron theory.

Given the context of a short book review, I think not mentioning Pekar's coinage of "polaron" and his important contributions does not constitute a major omission, as Dykman and Rashba suggest. In a different context—in a book or a review article—Pekar's work would be amply cited. For example, reference 5 contains a section devoted to Pekar's polaron.

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Author clarifies credit for H-bomb calculations

I am pleased by Cameron Reed's excellent and insightful review of my book *Building the H Bomb: A Personal History* (PHYSICS TODAY, July 2015, page 46). Let me add a couple of minor clarifications.

Reed reports that we used "card-fed and plug-board computers" for the thermonuclear calculations. Indeed, we used such computers in 1950 and 1951, but the final calculations that led to the 7-MT predicted yield of the "Mike" device were carried out on the SEAC (Standards Eastern Automatic Computer) at the National Bureau of Standards in Washington, DC. SEAC was a stored-program computer that, in 1952, was probably the best computer in the world, with its 3 kB of memory and its 1-MHz clock speed.

Reed refers, overgenerously, to "Ford's calculations." They were mine only in the sense that I was the person who shepherded the calculations night after night for several months on the graveyard shift. The coupled differential equations that we were solving numerically were devised principally by John Wheeler, with my assistance and that of John Toll and other young theorists at Princeton University's Project Matterhorn and in the theoretical division at Los Alamos. I wrote the code, with Toll's help.

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