letters

help from Roger Colton (who succeeded Blair as director at SCL), to abandon the use of microwaves in 1936 and go to the pulse-echo method at 110 MHz where more power and effective receivers could be developed.[1] I described a complete pulse-echo system in a memorandum dated 21 September 1936, based in part on an earlier memorandum of November 1933 in which I suggested the use of the pulse method. This method was not viewed with favor at that time, and work was limited largely to Blair's simpler ideas from 1933 to 1936.

Alvarez also cites as the official history of the MIT Radiation Laboratory a book with the title "Five Years (The MIT Rad Lab Memory Book)." I believe that a more balanced and objective work is that of Henry Guerlac, who wrote his history to accompany the 26-volume Rad Lab Series. Guerlac identifies himself on the title page as "Historian, Radiation Laboratory, MIT." In his history, Guerlac mentions the first airborne radar equipment developed at RCA and flight tested in 1937 and 1938. The first development of this kind of gear is sometimes erroneously attributed to the British. I was hired at RCA primarily to work on the development of this equipment; it operated at first at 500 MHz with 1-microsecond pulses. It embodied both an altimeter and an obstacle detector that displayed echoes from the Blue Mountain in Pennsylvania out to a range of 5 miles.

Reference

1. McKinney, "Radar: A Reluctant Miracle," Signal Journal of the Armed Forces Communications and Electronics Assn., November 1968, **21**, page 34.

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THE AUTHOR COMMENTS: In answer to W. D. Hershberger's criticism of my letter, I'll quote a sentence from that letter. "If I had been describing the history of the US radar" (instead of recounting the life story of Alfred L. Loomis), "I would not have mentioned the Loomis or Hansen devices, or that of RCA, because in my opinion, *none of the three had any appreciable effect on the development of microwave radar, as it was practiced by the Allies in WWII.*" (Emphasis added.) A careful reading of Hershberger's letter shows nothing that is in disagreement with that statement. For example, Hershberger tells of the potential usefulness of the RCA magnetron in cw and Doppler radars, techniques that weren't *practiced* to any appreciable extent by the Allies in WWII; cw radar is now useful in apprehending speeders, and Doppler radar was used extensively after the war to assist commercial airliners in navigating their way across the oceans. The microwave radar that was used almost exclusively in WWII was high peak powered pulsed radar, and not the nearly continuous wave microwave sets that were later used in automobile traffic control and air navigation, and to which RCA-developed techniques did contribute substantially.

The final paragraph in Hershberger's letter refers to a 500-MHz pulsed airborne radar set tested by RCA in 1937 and 1938. I believe his account, but I don't see what it has to do with a *militarily useful microwave* radar set. I am sure it was useful as an altimeter and as an obstacle detector, but 500 MHz is by no stretch of the imagination in the microwave range, and the ground—or a mountain—has a cross section enormously larger than that of an airplane. So I also recommend the radar equation to the reader. How far could such a set see an airplane, if it could see a mountain "out to a range of 5 miles"?

I'm sorry that my "insistence on historical accuracy" makes me sound negative about some really admirable achievements by RCA and other organizations engaged in the development of radar in the 1930s. But the facts in the case are quite clear: Microwave radar as practiced by the Allies in WWII sprang almost full-blown from the invention of the pulsed cavity magnetron, by John Randall and Henry Boot, in Birmingham, England, in early 1940.

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Poetic desecration

Lovers of poetry cannot fail to have noticed that the Elizabethan drama that unaccountably serves as your June Guest Comment (page 9) fails regrettably to scan in the final two lines of A's first speech to F about B ("Which I here have called Lorentz's Rules/and number one through five.")

Such readers should know that this jarring breach of form is an interpolation in the original text, contrived without the poet's knowledge or consent in the editorial offices of PHYSICS TODAY, presumably to replace a technical footnote in the original text (of flawless iambic pentameter) which would have made little sense in the new setting.

I urge those planning amateur performances for graduations, weddings, Christmas parties or other festive occasions to delete the above-mentioned spurious lines. The purpose for which they seem to have been designed can be

served without violence to the muse by everywhere replacing the phrase "Rules One through Five" by the metrically equivalent "Lorentz's Rules."

N. David Mermin
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7/83

Negative impact of computers

Your May issue was dedicated to the role of computers in physics. It was a largely uncritical analysis of the great benefits brought to physics by the development of computers. I suppose that, by almost any standard, it was on the mark, although I wonder if with all the good there still isn't some bad. In 1960, when I was a student at the University of Notre Dame, a professor told me that he worried about the possible negative impact of computers on the development of physics. He feared that the computational power provided by computers would remove the impetus to develop better and more efficient theories. The editorial, "Needed: more computers in universities," by Kenneth Wilson stated, "First, basic theoretical science is moving away from analytically soluble model systems, such as the hydrogen atom, to problems of real-life complexity, such as the chemical bond, turbulence. ... Computer simulation is needed in all these subjects. ..." It is possible, after all, that "real-life complexity" only appears to be as such because no one has yet looked at it in the proper manner. Furthermore, because of today's computer-oriented environment, we may never discover that "simple way" of looking at "real life."

Carl G. Adler
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6/83

THE AUTHOR REPLIES: Carl Adler has expressed the widespread fear that computer simulation will reduce the stimulus for analytic thought. To find out how computers will affect theoretical science, I suggest the study of a precedent. The development of experimental science, 400 years ago, has diverted numerous potential theorists into experiment, where they tinker with electronics or build equipment rather than concentrate on beautiful ideas about nature. However, experimental science has vastly increased the range and challenge of the armchair science, which in turn has attracted many fresh students to theory. I believe the increasing use of computer simulation in science will have exactly the same effect. The range of problems that are open to practical scientific inquiry will increase, the overall challenge of both theory and experiment will increase and, as a result, new recruits to theoretical and experimen-