letters

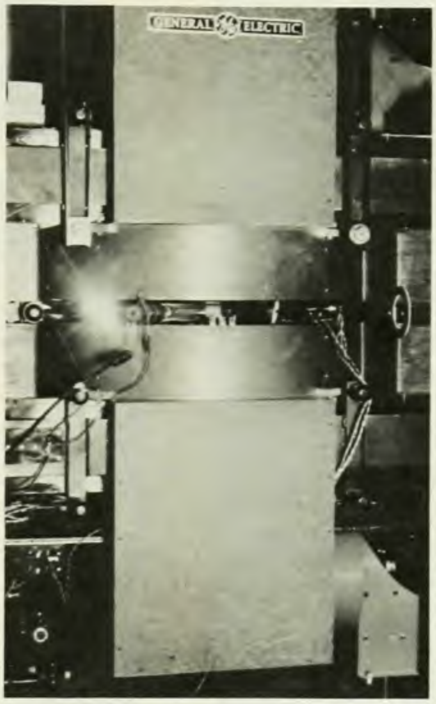
Origin of synchrotron radiation

It is indeed unfortunate that the informative article on synchrotron radiation in the July issue (pages 30–37) contains no reference to the original discovery of the effect, although, less relevantly and somewhat accurately, it does mention the "invention" of the betatron. Those who predicted the existence of this remarkable radiation and those who discovered it deserve recognition along with those who have since found interesting uses for it, so well described in your article.

The discovery of synchrotron radiation is an interesting case history in the nature of scientific discovery: It was made possible by a trivial design change and by a conscious disregard for the rules of radiation safety. I was a particularly close witness to much of the history; in fact, had I only been more open-minded three years earlier, I might have been a co-discoverer of what would instead now be called "betatron radiation."

Early attempts to use the induction acceleration principle first described in 1922[1] were unsuccessful. D. W. Kerst and R. Serber, therefore, made a thorough theoretical study of the electron orbits,[2] leading to the former's invention of a pulsed injector and, with that improvement, to his construction of the first betatron to accelerate an appreciable current, a 2.3-MeV unit. Among the subjects that had been considered by Kerst and Serber was energy loss by radiation (it was an assigned problem in the latter's course in electrodynamics). Apparently having concluded that radiation loss was not a significant factor in the design of induction accelerators, however, they did not mention this possibility in their published paper,[2] nor anticipate those properties of the radiation that are so useful today. And, since neither the original 2.3-MeV Illinois machine nor the 20-MeV betatron built by Kerst and W. F. Westendorp in Schenectady produced appreciable "synchrotron radiation," the subject was forgotten.

Following summer participation at General Electric and graduate work at Illinois in the 20-MeV development, I joined GE in 1944, when initial tests were underway of the 100-MeV unit designed by Westendorp after Kerst's return to Illinois. An interested observer of our early experiments, John Blewett,



Synchrotron radiation from 70-MeV machine at General Electric Research Laboratory where it was first discovered in 1947.

who has since made many contributions to the accelerator art, called our attention to a short paper by D. Ivanenko and I. Pomeranchuk, which predicted significant radiation losses in circular accelerators,[3] and he suggested a search for radiation. Westendorp and I were critical of this paper, pointing out that it was based upon a single-electron model, and that, when a large number of electrons are distributed around the orbit, their individual fields tend to interfere destructively. As Westy put it, "Direct current does not radiate!" It did not occur to us (nor, apparently to Ivanenko and Pomeranchuk) to consider the fluctuations and relativistic effects that account for its remarkable spectral properties, and which lead to an energy loss exactly equal to that given by the single-electron model.

Blewett nevertheless insisted upon a test, so, with a sensitive radio receiver we looked for radiation at the 57-MHz orbital circulation frequency and at several harmonics thereof—first, using antennas external to the tube, and finally, within the silver-coated doughnut—all with negative results. That convinced us, but not Blewett, who suggested that we look for other evidence, in particular, energy losses. We knew, in fact, that the orbit radius began to shrink when the electron energy neared 90 MeV, so that the beam would contract to target at about 106 MeV. We estimated the orbit radius at each intermediate energy by measuring the deflection current required to bring the beam to target, and found that it did indeed behave in accordance with Blewett's calculations[4] based upon the Ivanenko–Pomeranchuk hypothesis.[3] Because an alternative hypothesis (phase difference between center flux and guide field) also accounted satisfactorily for the data, no radiation had been detected, and our consultant, Marcel Schein, agreed with our objections to the Ivanenko–Pomeranchuk paper, we remained unconvinced.

Blewett correctly surmised that the radiation would be found to be distributed over a great many harmonics[4] and found support for this belief from J. Schwinger. But, with an opaque doughnut coating, complete concrete radiation shield, and closed minds, the rest of us did not see the light, literally or figuratively. We also were very busy with other matters. It was the nuclear age! Sources of longer wavelengths were commonplace. The betatron had been built to produce high-energy x rays; its uniqueness was attested by a constant stream of visitors to the Research Laboratory, each suggesting new experiments, but none that we look for synchrotron radiation—except Blewett, who kept faith. His paper, which should have been cited in the PHYSICS TODAY article, was both accurate and prophetic.

Three years later, in the same laboratory, a 70-MeV synchrotron was completed under the direction of Herbert Pollock. At its initial location, it was not completely enclosed by a radiation shield; even more significantly, the doughnut coating was transparent, facilitating placement of electrodes for beam extraction.

Knowing that Pollock was concerned about breakdown, Floyd Haber, his assistant, ventured a "very quick glimpse" (with the aid of a mirror) around the corner of the radiation shield to be sure there was no spark-

over, and on 24 April 1947 he became the first to observe man-made synchrotron radiation.[5] Haber reported an intense "arc," which, to everyone's amazement, persisted after beam deflection voltage was turned off!

Word passed rapidly throughout the laboratory, and it became quickly apparent to the throng who gathered that they were actually *seeing* the electron beam. But by what mechanism? Bob Langmuir was the first to suggest that this must be "Blewett radiation," although the majority favored a gas discharge or Cerenkov origin, despite the low tube pressure. I suggested that, if the light were indeed fundamentally related to electron circulation, it should be polarized. Polaroid quickly settled the question.

Pollock's group subsequently undertook a detailed spectroscopic study[6] in collaboration with analysis by Schwinger.[7] This paper should also have been cited in the PHYSICS TODAY article. But John Blewett deserves full recognition as the true discoverer of the phenomenon, having correctly interpreted experimental observations that demonstrated not only its existence but its spectral characteristics, three years in advance of its direct observation by Pollock's group. Ivanenko and Pomeranchuk deserve credit for stimulating his thinking on the subject, but they did not anticipate the spectral properties, its most distinctive characteristic.

There seems to be an unfortunate tendency among non-industrial members of the scientific community to overlook or denigrate the many significant contributions, not only to accelerator development and applications, but also to basic science, by the General Electric Research Laboratory in those early post-war years. I trust that his footnote to your article will furnish an improved perspective.

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KERST COMMENTS: There are some interesting historical matters that indicate much more awareness of the centrifugal-acceleration radiation problem than George Baldwin has indicated; for example, it is remarkable that in 1912 a complete book was written on it with relativistic electrons grouped in many ways in orbits. It is the Adams Prize essay of Cambridge University written by G. A. Schott, and this was one of the three treatments of the problem that was examined at the University of Illinois before it was considered reasonable to try to make the betatron. The other two treatments were one by Harold Mott-Smith and a treatment by Robert Serber demonstrating that randomly spaced electrons in a continuous ring would radiate individually just as much energy as an isolated electron would lose. Fortunately this loss was small enough so that there was no obstacle to the first betatron but that fact should have been published in our first papers. It was however seriously taken into account when 100 MeV was chosen by Maurice Goldhaber and me as an energy of possible interest for meson production in the early days, and because the radiation loss was tolerable, the design was worked up for the 100-MeV betatron.

Also there was communication with the MIT radiation laboratory during the war about the possibility of purposely bunching the betatron beam to make microwaves, but the power was too small.

Leonard Schiff, at Los Alamos during the war, demonstrated that an evenly spaced ring of electrons radiated just as much per electron as a single electron but that it radiated especially strongly in particular harmonics, and of course Schwinger did the complete job for the whole spectrum a little later. At that time there had also been attempts at Los Alamos to detect the loss of energy of the beam by clamping a 22-MeV beam and letting it coast to a small radius. It didn't work. The beam radius grew and hit an exterior target in one-tenth of a second. It remained for the quantitative determination of the amount of radiation to be made later.

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