“Some Early Synchrotron Radiation History”

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A month or so ago when I was asked if I would give a talk to you of the early days of your field, I demurred; I thought I had given the talk some years ago at the Instrumentation pow-wow held here in Ithaca. But I was assured this would be a different crowd and that I really had no excuse. So I agreed; after giving that talk back then I learned a few things I had not then known, which might make this one at least a bit different.

In all papers covering the history of synchrotron radiation, the work of Lienard in 1898, who first calculated the classical radiation from a high speed accelerated charge is cited. Then comes the work of Schott who won the Adams' prize for his essay "The Radiation from Electric Systems or Ions in Accelerated Motion and the Mechanical Reactions of Their Motion Which Arise From It". He was attempting to provide a basis for an electron theory of matter and calculated the radiation from various atomic models and groupings of electrons, both slow and relativistic, in orbits. In 1912 he included the work as Chapters 6 and 7 in his Electromagnetic Radiation---a book not widely available; it is not in the Cornell Library.

And that's about where synchrotron radiation sat for some thirty years until high energy accelerators were coming to the fore.

In 1922 Slepian at Westinghouse took out a patent on an accelerator based on the transformer: instead of a wire surrounding a changing magnetic field, he would have a vacuum vessel replace it and insert some electrons. The changing magnetic field within the circle would induce an electric field to accelerate the electrons which would be confined to the vessel and orbit by the magnetic field existing there. Others proposed similar gadgets. But it wasn't until the early forties that Kerst and Serber at Illinois reported a successful embodiment of the idea, in an eight inch, 2.3 Mev accelerator dubbed the betatron. The average magnetic field within the electron orbit had to be twice that existing at the orbit and as important, for focusing, the field at the orbit had to fall off more slowly than 1/r to overcome the centrifugal force which goes as 1/r. In their development they considered radiation, deciding incorrectly that it was unimportant; it was not mentioned in their paper, which Kerst later allowed was a mistake. Serber had even apparently assigned it as a homework problem in his E and M course.

With that success Kerst, then with G. E., developed a 20 Mev betatron, which encouraged going ahead at G. E. with a 100 Mev development. The G. E. interest in high energy x-rays is easy to understand. Radiography was important in factory and heavy industrial operations and they were also into medical x-rays. Besides Coolidge was director of the outfit and he had changed x-ray technology with his highly evacuated x-ray tube incorporating as electron source filament of a tungsten material which he had learned how to draw into wire. So he pushed the betatron. In the meantime, in a letter to the Physical Review, Ivanenko and Pomeranchuk, pointed out a limitation to betatron energies inherent to the device brought about by the radiation the fast moving radially accelerated charges gave off; when this loss per turn came to equal the energy gained in the changing field, no further increase in energy would result. The limit was something like 500 Mev.

Today, construction of the 100 Mev doughnut seems bizarre. The vacuum chamber consisted of 16 sectors---22.5^o each---placed end to end to complete a circle. They were carefully ground and clamped in position in a way not entirely clear to me. Each sector was internally silvered of not too low resistivity film to avoid surface charging effects (presumably connected and grounded) and each joint covered over with red glyptal. You people probably don't even recognize the name; it was a red paint which was very effective in covering leaks. It was a rare system back then that did not use quantities of glyptal to cover defects in workmanship. Anyway, between the opaque glyptal and the opaque metalized coating on the inside one saw nothing of the inside of the vacuum chamber. Which was unfortunate---otherwise we would know of betatron radiation. John Blewett, then at G. E. and now retired from Brookhaven, had seen the Russian letter and thought the radiation should be looked for. He looked assiduously for it from 50 to 1000 megacycles and found nothing. He did, however, observe the orbit contraction at the peak of the magnetic cycle when no more energy was being fed into the beam, was of the right order of magnitude of that which Ivanenko and Pomeranchuk had calculated. Blewett deserves a lot of credit for persevering in his research against the poh-pooers around him who "knew" a DC current in a loop doesn't radiate. It was later when a communication from Schwinger (published in full three years still later in the Physical Review) that it was realized that the harmonics of the fundamental frequency were not of low order but extended up to 10^7 that of the fundamental; the radiation might be in the infra-red or even the visible.

In 1945 McMillan at Berkeley proposed a new accelerator to get around the energy limitation of the betatron. It might be mentioned that even the cyclotron, then fairly well developed, had energy limitation pointed out here at Cornell by Bethe and Rose: because of the increase in ion mass with energy, accelerated ions fall behind and get out of resonance with the rf voltage driving them. Berkeley was not very happy with this news. Only after the war did Thomas propose a way out of the obstacle---strong focusing. McMillan proposed a ring of magnets creating a field at an electron orbit which fell off slower than 1/r again for focusing. Having a field over the entire area within the orbit is not necessary. Electrons would cross the accelerating gap, not at the peak of the voltage but at the time when the voltage is near zero, changing from accelerating to decelerating. In the equilibrium orbit, if an electron was going too fast it would arrive at the gap to find a decelerating field to slow it down, and vice versa. To increase the energy now either the frequency or the magnetic field must increase; the electron phase slips to accept energy from the gap but the phase stability still obtains. The phase might slip by as much as ten or twenty degrees. It's like the synchronous motor in that respect, which is why McMillan called his idea the synchrotron, Vecksler in Russia proposed the same idea at the same time and they shared the big prize for their concept.

G. E. proposed to test the idea; they had a betatron magnet sitting around which could be utilized. Betatron action would always be present in any such accelerator but that should not defeat the general concept. In a visit of Lawrence (the cyclotron man) to G. E. during which the concept was discussed, Pollock at G. E. suggested the betatron principle actually be used to inject the electrons into the synchrotron at perhaps a couple Mev getting them started at about .98c in phase stability in the increasing synchrotron magnetic field. The doughnut was this time transparent---of glass---but that was incidental. Why it was decided wall charging was not then important I don't know; the first Cornell machine had conductive coating on the inside of its glass sections. In any event that was a fortunate accident. In the fall of 1946 they had a 70 Mev synchrotron operating and could let McMillan know that his idea was sound. And then a coil shorted out and operations came to a halt while a new coil was built and various improvements in the electron gun and pulse transformer drive were implemented. By the following April---seems reminiscent of some down periods we experience here these days---they were ready to go again. The machine was behind a shielding wall for obvious reasons and operation was watched via metering various currents including x-ray ion production. In tuning up the new assembly some sparking was manifest presumably at the electron gun. A technician, Floyd Haber, was asked to peer with a mirror around the shielding wall to see where the sparking was. Doing so, he spotted a bright arc and called to turn her off. There had been no indication at the controls that a spark had occurred. Furthermore, the arc did not die immediately, pronto, on killing the voltage. The vacuum was fine. Others took a look, It wasn't an arc, and it wasn't Cerenkov radiation, or gas excitation. Robert Langmuir (no relation, surprisingly, to G. E.'s great Langmuir) and Pollock realized it must be Blewett's radiation. It moved around the doughnut depending on the site of observation; you had to be in the orbital plane; it was polarized as would be expected; they were "seeing" the electron beam in essence. It created quite a stir in the laboratory. It was shown (via mirrors) to many notables visiting the laboratory; laboratory directorship of course. One such visitor, not so notable at the time became so later. Mr. Ronald Reagan, publicity man for the Company at the time, visited around and was shown the bright spot. I'm not sure how this reflects today on his science policies. Klaus Fiech was another.

Having the radiation, the G. E. people investigated it over the visible range, which was as far as their glass doughnut permitted. Over the limited region, Schwinger appeared to be correct. And there it sat---an interesting scientific curiosity.

During this time, McMillan and Lawrence at Berkeley were building a 300 Mev synchrotron, which was being paralleled by similar construction at Cornell under Bob Wilson. Both groups experienced difficulty with injection but Cornell managed to get around it and achieve high energy acceleration of electrons. McMillan visited here and learned of the solution. Berkeley followed shortly, leaning on what the Cornell group had done to alleviate the difficulties. High energy photon physics could go ahead. The machines were not easy to use, space was crowded, vacuum was a problem; one didn't just come in on an evening after supper and turn it on like a light bulb. The radiation of course was there to be seen, bluish and bright. Corson did a nice experiment on the radiation here at Cornell, measuring the rate of loss. He imaged the beam on the face of a photo-multiplier tube ruled with opaque stripes. When the rf was cut off the beam would contract under its loss of energy and the rate of contraction could be accurately measured by the rate of movement of the beam image across the photo tube, as recorded on an oscilloscope trace. The rate of loss was shown accurately to be proportional to the fourth power of the beam energy. A nice result; further confirmation of the theory.

At the time, and it's still largely true, the Cornell physics department had two main lines of endeavor: solid state (condensed matter physics) and high energy nuclear physics. In the solid state research, radiation was much involved, be it hard x-rays, soft x-rays, ultra-violet, visible, or infra-red. Like Oliver

Twist we always wanted more. I was working in the vacuum ultra-violet on spectra of alkali halides, using a hydrogen discharge source and wanting something better. I went one day down to see x-ray physicist Leonard Jossem about the possibility of using bremsstrahlung of a low voltage, high current x-ray tube. He was not very encouraging but suggested instead that the synchrotron be used, employing Schwinger's radiation. As a source for my research, the prospect was not the most appealing; the high energy people had their own program, it was a complicated source, and to have two differently oriented laboratories in the same space seemed pretty unrealistic. But the prospect of doing something in my energy range, yet of real interest to the high energy people seemed exciting and useful. I left Jossem and went down the hall to see D. H. Tomboulian. He worked in the soft x-ray region, we had had a small collaboration, and he would obviously be interested. He was, and perhaps more than I even now know. He would very much like to explore the radiation. I talked to Bob Wilson about the possibility of getting some machine time and he was more than agreeable that we do it, so we made plans.

At the exit port of the machine we replaced a lucite cover with quartz to enable our looking at radiation down to the quartz transmission limit. I recall the lucite cover showing a brown trace across it produced by long exposure to the narrow beam of radiation: the synchrotron was a different source than any we were used to all right. Aimed through the window at a tangent to the beam we located a small quartz spectrograph, in front of which was a rotating disc with a slot cut in it, turned by a motor synchronous with the magnetic field excitation of the synchrotron, the motor itself rotatable so as to provide a phased shutter in front of the spectrograph. In that manner we could look at the radiation from electrons in a given phase of the cycle. Having secured exposed plates, we then had to go through the deconvolution of plate sensitivity, dispersion of the instrument, transmission of the window and instrument, and so on to arrive at intensity distribution with wavelength of the radiation. The work somewhat extended that of the G. E. people and really only served again to indicate Schwinger's theory to be correct.

In the meantime we proceeded with the construction of the world's first synchrotron radiation beam line, and making calculations on what might be expected on the basis of Schwinger's theory. It was in the days before pocket calculators and the calculations were sort of messy. I recall running into those fractional order Bessel functions for the first and last time; I'm not sure I ever did understand their origin. Tomboulian did much of the analysis and I did much of the mechanical preparation. The "beam line" bore little resemblance to what you people know as a beam line. We exited the machine through a flexible Wilson seal with a brass pipe extending through one of the C-shaped magnets---one with extended length to allow radiation through. The pipe coupled to a simple manually operated vacuum valve incorporating a window to make alignment with the beam possible with the valve in the closed position. On the other side of the valve a brass pipe connected to one of Tomboulian’s spectrographs, a rather compact grazing incidence grating instrument, easy enough to get from his laboratory in Rockefeller Hall over to Newman Laboratory and the synchrotron. This could be evacuated separately before opening the valve to put it on the synchrotron vacuum system. And that was it.

It was an uncomfortable moment when, with the spectrograph loaded with a plate ready to expose, we opened the valve and saw the vacuum going to pot. The sliding seal was again a Wilson seal---we didn’t have O-rings back then---but a liberal application of pump oil around the seal edge took care of the leak and all went well thereafter. After a short exposure to the beam we closed the spectrograph off and carefully, we thought, retrieved the plate. The place was dark in the late night but not exactly a photographic dark room. Upon development over in Tomboulian's real dark room the plate showed a disconcerting streak, non-uniform top to bottom, as indeed of a light struck plate. But Tomboulian had means to check it. We put one of his thin beryllium films back on the entrance slit of the spectrograph and repeated the operation, this time no leaks, either vacuum or stray light, for sure. That plate was beautiful; the now more or less uniform dark streak was interrupted twice by a sharp change in density---the K edge of beryllium in both the first and second order. The radiation did extend down into the 100Å range. I'd like to have that plate today. I don't know where it went. A second try with aluminum showed the $L_{2,3}$ edge, also in two orders. And an unfiltered exposure to the radiation, with the electron energy brought only to 230 Mev instead of the full 320Mev, made possible the comparison of the wave length distribution at two disparate electron energies. That was a very satisfying night's work, for us, if not for the high energy crew running the machine, to whom we were indebted.

Then followed months of plate reduction. Recall that we had superposed the radiation of electrons of all energies from the minimum right up to the maximum; no shutter had been involved in the beam line, largely from vacuum considerations. And we had the problem of the overlap of different orders from the grating. Tomboulian did the lion's share of the reduction. It was somewhat unexpected, but the non-uniformity in density from top to bottom of the streak gave us information about the angular distribution of the radiation. All in all it was very successful and we had a nice long paper in the Physical Review in 1956. But it had been a long time from the first experiment we did.

Not much happened after that for quite a lengthy period. It was clear that the radiation was there, it was intense and could be useful. But it was also true that we were all going to be pirates, and the high energy people were not building their machine with us in mind. In 1961, Peter Joos, did another radiation experiment---on the next Cornell machine at 1.1 Gev. He checked for the first time the polarization of the radiation in and out of the orbit plane finding polarization parallel to the orbit plane to be maximum in that plane and the perpendicular component with maxima on either side of the orbital plane. This too followed from Schwinger; some people are just very smart.

The opening up of the use of the radiation in new physical investigations came in 1964 in the work at the NBS 180 Mev machine by Madden and Coddling in looking at spectra of rare gas atoms. And things have gone on from there at an even faster clip, in more and more laboratories. And of course we have now the storage ring which makes life considerably easier.

This recital is presumably of the early history of the radiation, so I won't go into wigglers. Undulators are another matter. They are ancient history. In 1951, Motz at Stanford proposed using high energy electron beams running through a periodic straight line magnetic structure as a means for generating high intensity microwaves. This was even before Tomboulian and I got into our work. In his paper (Jour. of Appl. Phys.) he refers to Schwinger and some more compact and elegant but less complete treatments of Schiff. In the Michelson-Morely 100th year memorial issue of Physics Today, Jackson (the E and M Jackson) tells how it is. In the rest frame of the electrons the magnet spacing is much reduced by the Lorentz-Fitzgerald contraction; the electron sees the rushing magnetic field as an alternating electric field and so it oscillates and radiates. Back in the laboratory frame the radiation is relativistically doppler shifted and so one gets radiation of energy much reduced in wavelength. Motz even calls his idea the undulator. It is interesting that it has come into its own rather in the synchrotron radiation game.

After I told much of this story at the Instrumentation meeting here six years ago, Madden cornered me and indicated that some of the G. E. tale I had related might not be quite correct. I had relied for that on a letter to Physics Today berating the authors (Perlman et al) of a paper in a previous issue of Physics Today on synchrotron radiation for their neglect of G. E. in its discovery of the radiation. Baldwin at G. E., who had worked on the 100 Mev betatron, told of the discovery much as I have given it. Madden said I should write Blewett at Brookhaven and get the true story. Which I did and learned some interesting details. Blewett indicated that safety had been compromised by having observers within the shielding perimeter and that the arcing was at a deflection electrode, neither of which was correct, the safety matter decidedly not, according to Pollock who was leader of the synchrotron development. More interestingly, Blewett enclosed the copy of an affidavit filed by one Gerald Knowlton in Cook County, Illinois to the effect that he had been the discoverer of the radiation and not Floyd Haber. It was a long document, ten or so pages of close spaced typing on legal size paper, full of whereas and wherefores, some single sentences covering more than a full page. In the slight accorded him in crediting the discovery to Haber, his reputation had been damaged; perhaps he had even been physically harmed in the work. He had been a faithful worker, done things above the call of duty. He cited one suggested company improvement he made for which colleagues had told him he should submit for reward. He earned $5 for the suggestion. He had showed many people around and the radiation in particular, citing various names: Dushman, Hull, the great Langmuir, and others of G. E., and outsiders like Alvarez, A. H. Compton, Bethe, Dr. Enrico J. Fermi, L. Ron Hubbard of all people (the dianetics scientology promoter), and G. P. Thompson. He voluntarily left G. E. "---that such termination was in part prompted by a conversation had by the affiant with said (Dr.) Enrico Fermi in the Spring of 1949, begun on an elevator in the lobby of said Building #37 and continued into the 4th floor of said Building #5, and wherein he (affiant) found his (own) services might as well or better be applied at the University of Chicago, etc., etc., "all one continuing long sentence, which goes on to cite the many short articles he has written as consultant to the Britannica, covering such things as black hole, space-time, LaPlace transform, triangular numbers etc. in short articles, certainly to be found in current editions of the reference work but uninitialed. But they turned down his article on synchrotron radiation, on and on. It's a very strange business; he actually submitted two affidavits. In the first 1974 he claimed discovery in July of 1948, a date quite after it had been announced in the Physical Review. A following affidavit changed the date to April 1947, the correct date. He worried the company some; they did not know what he was up to and they feared a possible suit, although there was no evidence of a physical disability (except possibly mental) as a result of his G. E. employment. Whitney, a director of the laboratory, always insisted on laboratory employees keeping a daily record in their notebooks. In Knowlton's case it was perhaps a mistake for him. A copy of a page from his notebook of the time includes this entry dated May 9, 1947: "Much excitement around on account of "visible" radiation losses. This effect is experimental confirmation of Schwinger's theoretical prediction that much of the radiation loss could be expected in the higher harmonics of the RF excitation. See papers by Schott and Blewett. Spectrograph shows energy to the concentrated in blue end of spectrum". And squeezed in between that entry and the next dated May 13, is the note: “First observed April 24, 1947 by Floyd Haber.” Too bad for Knowlton, although it was acknowledged he may have seen a flickering light previously to that date without making any mention of it. He said Haber had gone from the lab for ten or so minutes during the tune up and he was the one who spotted the spark and hollered to Langmuir at the controls what to do to make it brighter, using it for the tune. Very odd. Knowlton submitted his long briefs to the A.I.P. history archives and to rectify that, Pollock, after hearing at Stanford of my talk six or so years ago here on the discovery wrote the definitive story and filed it also with the A.I.P.

Pollock’s notebook has an entry on the discovery. After the technician (Haber) spotted the presumed spark, and noting the excellent vacuum, Langmuir and Pollock peered around the protecting wall to observe: Quote “At first we thought it might be due to Cerenkov radiation but it soon became clear that we were seeing Ivanenko and Pomeranchuk’s radiation. The intensity remained high when we decelerated the electron beam from 70 Mev to 10 Mev without bringing the beam to the target or gun. We observed the bright spot with mirrors, looking tangent to the orbit at two or three points in the room. The intensity decreased as the peak energy was reduced. When the energy was of the order of 20 Mev it was no longer visible. We showed the effect to Dr. Charleton, Dr. Kingdon, and various others. The beam appeared stable and of small cross section (perhaps 1 mm square).”

Langmuir, gone to Cal Tech recalled his remembrance of the discovery: “I have very definite and clear remembrances about the discovery of synchrotron radiation. I don’t recall the date but in the afternoon one of the technicians reported to me that there seemed to be sparking in the synchrotron tube. He observed this by looking in the large (about 6’ high by 3’ wide) mirror that permitted us to observe the machine without getting too much radiation. You (Pollock) were at the controls of the machine. Upon seeing the light, I asked you to ruin the timing, which you did and the light went away. It returned when you returned the injection pulse to the proper time. I immediately said “that must be Schwinger radiation”. The whole incident took about thirty seconds. We then changed the energy of the beam and noticed that the blue-white color at 70 Mev became yellow at about 40 Mev. I don’t remember whether we had good shades on the windows at that time, but then or later we could see the beam become red (and quite weak) at about 30 Mev. In view of the above, the light was first seen by the technician---I don’t remember his name. He thought it was sparking. I, and almost immediately thereafter you (Pollock) recognized it to be what I called at the time “Schwinger radiation”. Just who gets the credit for discovering the light is not clear. What is clear is that you and I knew what it was and the technician didn’t. I see no reason why the technician should get any scientific credit---just credit for keeping his eyes open.”

Finally, Ed McMillan, who had read in Russia Pollack’s letter in 1970 to Ivanenko and Pomeranchuk recounting how it came about wrote Pollock:

“Your account of the discovery of synchrotron radiation is fascinating, it should go into the history books. I understand now why it is called “synchrotron radiation”. I saw it first on a visit to Schenectady when you showed it to me, coming from the 50 Mev betatron, and I assumed you had seen it first in that machine. I am, pleased that it was first seen in the synchrotron, with the result that a word I introduced into physics now occurs in astronomy and cosmology.” He refers of course to super-nova remnants such as most notably, the Crab nebula, the presence of which from 1054 AD certainly antedates any of the preceding chronicle.

One matter which I have long wondered about is why Tomboulian had not gone for the radiation himself before we did. I can't believe he did not know of it before I ever did; it would have been much more needed in his work than my own and he followed the literature closely. I can't recall his reaction when I went to him with Jossem's suggestion; I only remember his enthusiastic agreement that we look at it, no memory of his acknowledging knowing about the G. E. work and Schwinger's theory. But six years ago when I got in touch with Blewett and then Pollock, Pollock sent me a copy of his brief paper on the Discovery of Synchrotron Radiation. After telling of the event, he ends with this paragraph: "Subsequently the G. E. synchrotron group published both an account of the first observation and a more detailed paper on the characteristics of the radiation. The radiation immediately proved valuable. It was not just a laboratory curiosity as Perlman et al have suggested, for it permitted an optimization of synchrotron design and performance. D. Tomboulian, who visited us from Cornell, later undertook thorough studies of the radiation from the Cornell synchrotron and extended its usefulness as a scientific tool. There will be no attempt here to document the subsequent extensive literature on "Synchrotron Radiation". And then he quotes the above note from McMillan. But Pollock could not tell me when Tommy visited. Was it before or during our work? Had I butted in on a project on which he was going to work? He was a rather tough minded, difficult man, and even though we were friends, he played his cards fairly close to his chest and one was not always sure what he was thinking. All I know is that after our meeting following my talk with Jossem, things went ahead. I rather think Tomboulian who had been in the Department for some years before and during the war looked with jaundiced eye on the young group joining the Department and the grandiose plans they had for high energy physics. He may have been reluctant to get involved with them and may explain why at his untimely death he was involved with the Cambridge (Harvard University) accelerator group in planning a radiation facility over one section of the machine being planned. I don't know. I've always been grateful to have been in on it with him; perhaps not as exciting as the discovery at G. E., but useful and not too bad. And that's about it.