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A DIFFERENTIATION BETWEEN "BIG SCIENCE" VS. "LITTLE SCIENCE:" LAWRENCE AND TUVE, FIRST EXPERIMENTS WITH DEUTONS

My talk will deal with the interactions between instruments and research. At the beginning of the thirties a number of experimental groups undertook advanced research in nuclear physics in the US. They were composed of young physicists with new ideas and methods and styles of research. In this respect, the thirties were a period of change, in which one may try to recognize the early signs of future choices.

Team research started in this period. Mainly in experimental physics the new techniques or machines required the development of large laboratories. The average number of authors of a single paper increased during the thirties. Management became a part of scientific activity and provided increasing funds for large scale research and facilities. My talk deals with a specific part of nuclear physics: the early investigations using the first particle accelerators in the United States (1).

Three groups developed these activities:

1) that of Lawrence in Berkeley;

2) Tuve at the Department of Terrestrial Magnetism (I will call it DTM) of the Carnegie Institute of Washington;

3) the group of Crane and Lauritsen in Pasadena.

I will be concerned mainly with Lawrence and Tuve.

Other authors (Seidel, Davis, and others) (2, 3, 4) have already analized some striking aspects of Lawrence's character and attitude: his competitive and managerial leadership, his constant trend towards larger machines and higher energies, his ability in collecting everywhere financial support, and in this connection his concern in showing the practical usefulness of his products.

Tuve, on the contrary, had a very different, and in many respects opposite, attitude. In fact it is striking that he was one of the scientists who gave major fundamental contributions to the progress of nuclear physics during the thirties (and of other disciplines after the war) but his name and achievements are almost unknown to the great majority of today physicists, who on the contrary generally well know Lawrence's name and role. Lawrence, moreover, was awarded the Nobel Prize in 1939, while Tuve always missed it, even if it is conceivable that two or three of his achievements could have merited it.

I suggest here that these facts greatly derived from the peculiar Tuve's character and attitude, which led him to dislike the mechanisms and spirit that were increasingly pervading a research activity of ever growing dimensions (laboratories, research groups, and so on). In this sense Tuve at the end came out defeated by the incoming changes. I will deal mainly with a case study reconstructed on the basis of primary sources. I recall first that Lawrence and Tuve were born in the same town, were school-friends and constantly linked by a deep friendship all along their lives. The bitter remarks Tuve had to pronounce about Lawrence's researches are then even more significant.

Lawrence's group published the first results of experiments in nuclear physics with charged accelerated particles well before Tuve's group (5): unfortunately the lack of rigour in these experiments became evident in short time and was recognized and constantly remarked on by Tuve himself. On the other hand it is well known that Lawrence, working at the cyclotron and disposing of it, really missed some of the main discoveries, namely artificial disintegration of the nucleus and artificial radioactivity.

The opposite attitude of Tuve's group is striking: a great accuracy in designing the machines and the experimental techniques, in testing the apparatuses, before really entering nuclear physics research.

On March 18th 1933, for instance, Lawrence wrote Tuve (6):

> Just a note to tell you that we have recently found that we were wrong in assuming that the radiations we observed from aluminium were alpha particles as stated in our recent letter to the *Physical Review* (7). We have lately shown definitely that most of the radiations observed are made up of X-rays.

In the letter received by Tuve, some hand-written significant notes appear dated March 24th 1933 where Lawrence cites Tuve as the first to have produced artificial neutrons, the latter added: "We have never reported or written to anyone that we had accomplished this..." (6).

The first experimental results published by the DTM group (10) were in fact in clear disagreement with Lawrence's previous results, but Lawrence replied insisting on his own results, even if "there is always, of course, the possibility that these alpha particles are due to impurities" (11) (and Tuve added a note to the letter: "Impurities ?!"). In the same letter Lawrence reported the first results on the scattering by accelerated deutons, obtained in collaboration with the chemist Lewis. It is interesting to remark that, in spite of the growing divergencies, the Lawrence—Tuve friendship was so deep that the first provided the latter with the heavy water necessary to perform the experiments with accelerated deuton beams (12).

On the other hand these experiments became the major point of disagreement.

In fact, Lawrence was elaborating at that time the famous "deuton disintegration hypothesis" (13), that he reported at the 7th Solvay Conference raising the criticism of the European physicists (4, 5).

Tuve was already very sceptic on this hypothesis; he had warned Lawrence: "I am not able to follow your suggestion" (14). Lawrence had already replied that, if the initial evidence was effectively scarce, "I think we have now pretty conclusive evidence on that point" (15).

After the Solvay Conference, Lawrence had to perform more accurate tests in order to exclude that his results derived from systematic contaminations (17), as he wrote Tuve on December 21, 1933 (16).

Tuve, significantly conscious of the relevance and the delicacy of the problem, had answered Lawrence's letter on January 6th 1934, specifying that he had no new result since the whole period was spent on a very rigorous test of the experimental techniques (18).

But when careful experiments were performed by Tuve in the following weeks, the disagreement exploded. The "preliminary runs" already showed "a great deal of difficulty in correlating our observations with those you have published" (19)—with the whole set of observation, not only with the deuton results!—and suggested:

> [...] that you check over your apparatus very carefully, since at present [...] there appear to be the basis for suspicion that at least part of your observations are due to some factor common to all your target, which may be contamination, slit edges, target mountings or some other factor (19).

At that point Lawrence's reply (20) was lengthy but appeared very embarassed, and outlined the first autocritical considerations, since in the meantime his deuton results had been contradicted also by the Pasadena group (21) and at the Cavendish Laboratory (22):

> You are quite right in surmising that in our preliminary measurements there have been some errors [...]. Rather than continuing experiments we have decided to embark on a program of careful observations of things already brought to light and it is our intention to get as accurate measurements as we can.

Lawrence finally admitted his mistake in the deuton disintegration hypothesis (23). But Tuve criticism, as we have remarked, was much deeper and concerned not a single result, but the whole set up and method of the experiments performed in Berkeley and the hurry and lack of caution with which they had been published. It is important to remark that on the contrary Tuve, up to the moment, had avoided to make public the controversy, although he was already sure of his own results. At that moment, he sent on April 14, 1934 a letter to *The Physical Review* (24) contradicting practically all the results published from Berkeley and he sent a copy to Lawrence with some bitter notes:

> I wrote you at the end of February warning of the direction which our results were undoubtedly taking. After working up all of our results, we reached the astounding conclusion that we were unable

to check a single one of the observations which you have reported so far [...]. I must say that we were certainly not enjoyed the position in which we have been placed. Once in a lifetime is once too often (25).

We may remark that in the action that Tuve now developed one may recognize a mixture of real embarassment and professional ethics, of a kind that probably has progressively disappeared in subsequent years.

In this sense, on one side, evidently pressed by a growing debate on the issue, he personally pointed to Lauritsen that

[...] the question for many people as to whether we check Lawrence's work or not have become so insistent that there is no way of avoiding the issue and we decided that a bald statement was far preferable to any evasion of the question on our part. We have been very circumspect in what we have said even to close friends visiting the laboratory until the abstracts had to be written (26).

On the other side, however, a harsh press release was emitted by the Carnegie Institute of Washington after the Meeting of the A.P.S. of April 26, with the ironic title "Atom-Smashers Reveal Atomic Masquerade," containing such statements as the following:

Speaking before the American Physical Society meeting here today (April 26), Drs. Tuve and Hafstad of the DTM, Carnegie Institute of Washington, dramatically announced that they had succeeded in unmasking the outlaw atoms which have played havoc with the results of atom-splitting investigations currently in progress in various laboratories. The renegade atoms which gave rise to pseudo-transmutations of carbon, oxygen, and other targets when bombarded by high-speed atoms of heavy hydrogen, are the atoms of heavy hydrogen itself, sticking in the pores of the solid target after being driven there by the high-speed beam (27).

On August 4th, 1934 Tuve himself sent *Science*—through Fleming—an official rectication (28) since the *Journal* had reported in "erroneous and misleading" terms the results obtained at the DMT, had not explicitly referred to the "contamination effects" and had expressed the opinion that the experimental results from various laboratories were not in contradiction.

The whole story inspired in Tuve a sense of deep regret that be expressed to Lauritsen bitterly remarking that such an accident "must occur rarely, if at all" and, having Lauritsen replied that "that sort of things should never appear in print," firmly adding that rather "the sort of things that should never appear in print were what led to the necessity for such a statement by me" (29).

This course of events reveals, in my opinion, not only different Tuve's and Lawrence's personal characters, but really the early emergence of different styles in performing research activity. In the following years Lawrence concentrated on cyclotron building and insisted mainly on its use in medicine, while Tuve obtained from his rigorous and careful practice some of the most significant results in nuclear physics (5), namely in 1935, with his beautiful experiments on proton-proton scattering, charge independence of nuclear forces, the first widths of nuclear resonances. This is only an example. But these events were typical of Tuve's attitude towards a changing style and the growing dimension of research activity as is confirmed by the uncommon Tuve's choices in the war period, while the "Big Science" mechanisms really developed.

It is important to remark that Tuve had given important contributions in more than one field and there were in principle many possible fields in which he could have given relevant contributions to war research. When he and G. Breit had tried already in 1925 to determine the ionosphere height observing the echoes of short radio pulses, "they were troubled by echoes coming from airplanes, which interfered with their measurements" (30); "this was the first recorded instance of distance measurements made by the pulse-radar method" (31).

Tuve gave moreover leading contributions to the study of nuclear fission. With Roberts, Mayer and Hafstad he showed the first fission process at the DTM accelerator (32), discovered the emission of the "delayed neutrons" (33) and subsequently they contributed to show the possibility of a chain of reaction (35):

> We have been hard pressed to get some data on uranium fission, largely because Fermi, Rabi Sziland etc. have been afraid of chain of reaction possibilities. Regular "war secr" with secret meetings etc.! Pres. Bush is anxious to see it settled. All indications now are that no chain can occur but it is pretty close (36).

A confidential memorandum of June 1st, 1939 to the Director of the DTM by Gunn, Technical Adviser of the Naval Research Laboratory at Anacosta, explicitly mentions in this respect Tuve's availability "to carry on the final tests at his laboratory" (37) ; on May 23, 1940 the Carnegie Institute of Washington appropriated $ 20.000 "for study on uranium fission" (38).

Tuve was a member of the Uranium Committee called by Roosevelt after Einstein's letter, but his attitude changed at the beginnings of 1940. "It all started in February 1940 [...]. At that time, Roberts, Hafstad, Heidenberg and I simply decided that we would do no more physics research if the likes of Hitler were to inherit our efforts. We undertook to find a way that we could contribute to the technology of modern war" (39). While "by May 1940, in talks with officers in the R and D division of BUORD, US Navy, I had learned about the ridiculously low effectiveness of antiaircraft fire, I heard the term 'influence fuze' (later 'proximity fuze'), as a wistful hope" (40).

The history of the "proximity fuze" has in part been written (41). We are here interested in one specific aspect. In organizing and directing first the "Section-T" and then the Applied Physics Laboratory (APL), Tuve followed an attitude opposite to that then prevailing of early Big Science, that practically was born in the other projects. He started with the "four Indians" and followed the concept of "a local and flexible group to test the feasibility of various ideas submitted to it" (42). In Tuve's words :

> [...] one of the greatest 'new developments' of the war [...] was the rediscovery [...] of the efficiency of the democratic principle of directing the effort of organized group of people [...]. A boss using the democratic principle does not depend on just giving order from above [...]. Asking people to help with the whole job was what I used in running the proximity fuze development [...]. The democratic system

is more effective, dollar for dollar and hour for hour, than the autocratic system [...]. The key to the effectiveness of the democratic system is simply that criticism flows both ways ; criticism and ideas come up from workers as well as down the bosses (43).

Nevertheless, in spite of his wishes, the APL itself, under the pressure of the events, became a model of advanced big laboratory. One way conclude that the force of things was stronger than subjective intentions : Big Science was a necessary product of the path followed by science !

At the end of the war, Tuve left the APL and came back to the small and quiet DTM, assuming its direction, but abandoned also nuclear physics : "I left nuclear physics when it changed from a sport into business" (44). He repeatedly made very strong statements against Big Science. In 1959 he published in the *Saturday Review* a long paper by the title : "Is Science too Big for the Scientist ?" (45). He repeated this concept in a meeting in which President Eisenhower announced the appropriation of 100 million dollars for the future Stanford linear accelerator (46) ; Tuve used such a bald statement that his collegues publicly reacted against Tuve's statement, claiming that "this was neither the time nor the place" for it (47).

REFERENCES

1. E. M. Mc Millan, "Early History of Particle Accelerators", in : Roger H. Stuewer (ed.), *Nuclear Physics in Retrospect*, University of Minnesota Press, 1979.
2. N. P. Davies, *Lawrence and Oppenheimer*, New York : Simon and Schuster, 1968.
3. R. W. Seidel, Ph. D. Thesis, Berkeley, 1978.
4. J. L. Heilbron, R. W. Seidel, B. H. Wheaton, *Lawrence and His Laboratory Nuclear Science at Berkeley 1931—61*, Office for History of Science and Technology, University of California, Berkeley, 1981.
5. A. Baracca, R. Livi, M. Pettini, E. Piancastelli, S. Ruffo, "Il decollo della fisica nucleare negli USA (1930—36): le premesse della Big Science," *Proceedings of the IIIrd Italian Conference on the History of Physics*, p. 546 ; A. Baracca, R. Livi, E. Piancastelli, S. Ruffo, *Proceedings of the International Conference on "The Recasting of Science between the Two World Wars"*, Firenze-Roma, 20 Giugno—4 Luglio 1980, vol II, Roma : La Goliardica, 1985.
6. E. O. Lawrence, M. Tuve, March 18, 1933, Tuve Papers, Manuscript Library, Library of Congress (Box 12, Special Letters 1933).
7. E. O. Lawrence, M. S. Livingston, Phys. Rev. 43, 369 (1933) ; February 11, 1933.
8. M. A. Tuve, C. C. Lauritsen, March 19, 1933, Tuve Papers, *loc. cit.*
9. A. Fleming to E. O. Lawrence, March 30, 1933, ivi.
10. M. A. Tuve, L. R. Hafstad and O. Dahl, Phys. Rev. 43, 942 (1933) ; May 1933.
11. E. O. Lawrence to M. A. Tuve, May 3, 1933, Tuve Papers, *loc. cit.*
12. A. Fleming to G. N. Lewis, May 9, 1933, Tuve Papers, *loc. cit.*
13. E. O. Lawrence, M. S. Livingston, G. N. Lewis, Phys. Rev. 44, 56 (1933) ; June 10, 1933.
14. M. A. Tuve ato E. O. Lawrence, October.2, 1933, Tuve Papers, *loc. cit.*
15. E. O. Lawrence to M. A. Tuve, October 9, 1933, Lawrence Collection, Bancroft Library, Berkeley.
16. E. O. Lawrence to M. A. Tuve, December 21, 1933, Lawrence Collection, cit. ; v. pure lettera del January 12, 1934, ivi.

17. G. N. Lewis, M. S. Livingston, M. G. Henderson, E. O. Lawrence, Phys. Rev. 45, 242 (1934).
18. M. A. Tuve to E. O. Lawrence, January 6, 1934, Lawrence Collection, *loc. cit.*
19. M. A. Tuve to E. O. Lawrence, February 28, 1934, Lawrence Collection, *loc. cit.*
20. E. O. Lawrence to M. A. Tuve, March 14, 1934, Lawrence Collection, *loc. cit.*
21. C. C. Lauritsen, H. R. Crane, Phys. Rev. 45, 345 (1934) ; *Science* 79, 234 (1934).
22. J. D. Cockroft, E. T. S. Walton, Proc. Roy. Soc. A144, 704 (1934) ; M. L. E. Oliphant, P. Harteck, Lord Rutherford, Proc. Roy. Soc. A144, 692 (1934).
23. G. N. Lewis, M. S. Livingston, M. C. Henderson, E. O. Lawrence, Phys. Rev. 45, 497 (1934).
24. M. A. Tuve, L. R. Hafstad, Phys. Rev. 45, 651 (1934).
25. M. A. Tuve to L. O. Lawrence, April 18, 1934, Lawrence Collection, *loc. cit.*
26. M. A. Tuve to C. C. Lauritsen, April 18, 1934, Tuve Papers, *loc. cit.* Box 16, Letters—Special 1934—5—6.
27. CIW archives, folder "DTM—Miscellaneous 1934—35".
28. A. Fleming, J. Mckeen Cattel, August 4, 1934, Nuclear Physics Symposium : A Correction, CIW archives, *loc. cit.*
29. M. A. Tuve to C. C. Lauritsen, September 26, 1934, Tuve Papers, *loc. cit.* Box 16, Letters—Special 1934—6—6.
30. Report of the President, 1952, Carnegie Institute of Washington.
31. Biografia di M. A. Tuve (anonima), p. 4, CIW archives, Folder Tuve 1.
32. M. A. Tuve, Report to the Director of DTM for January 1939, 7.2.1939, Library of Congress, Manuscript Library, Tuve Papers, Box 15, "Monthly Reports ;" see letter to The Phys. Rev. 55, 416 (1939). See also Roger H. Stuewer, "Bringing the News of Fission to America," *Physics Today*, in press.
33. M. A. Tuve, Report for February 1939, 9.3.1939, *loc. cit.* ; see letter to The Phys. Rev. 55, 510 (1939).
34. Louis Brown, private communication.
35. CIW, *Year Book 1939* (July 1939—June 1940), p. 87.
36. Tuve to G. Breit, 2.8.1939, DTM Office archive file "Archive-Uranium."
37. R. Gunn, Memorandum for the Director, 1.6.1939, DTM archive.
38. Minutes of the Executive Committee, Meeting of May 23, 1940, CIW archives.
39. M. A. Tuve in APL News, Feb. 1982, p. 8.
40. *Ibid.*
41. R. Baldwin, *The Secret Weapon of WW2*, San Raphael, Ca. : Presidio Press, 1980, pp. XIII—XV.
42. F. R. Roberts, "Development of the Proximity Fuze," manuscript required quickly by Abelson on Oct. 20, 1977, CIW archives, Folder DTM Misc., p. 6
43. F. R. Roberts, *op. cit.*, p. 5.
44. L. T. Aldrich, L. Brown *et al.* obituary of M. A. Tuve, p. 3, 28.5.1982, CIW archives, Folder Tuve 1.
45. M. A. Tuve, *Saturday Review*, 6 6 1959, p. 49.
46. V. J. Lear, *New Scientist*, 21 5 1959.
47. *New Scientist*, 25 5 1959.