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"Big Science" vs. "Little Science" : Laboratories and Leading Ideas in Conflict : Nuclear Physics in the Thirties and Forties in USA, Europe and Japan

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“BIG SCIENCE” VS. “LITTLE SCIENCE”: LABORATORIES AND LEADING IDEAS IN CONFLICT; NUCLEAR PHYSICS IN THE THIRTIES AND FORTIES IN USA, EUROPE AND JAPAN

1. – Today we are so accustomed to large-scale research, that we tend to consider it as an almost natural way of organizing and performing this activity. From an historical point of view, however, we cannot avoid questions such as: what was the genesis of Big Science? What were the causes and the conditions of its birth and development? Which were the steps that prepared its advent?

In fact “Big Science” did not suddenly grow out of war-time emergence and of such enterprises as the “Manhattan Project”. It was instead prepared and partly anticipated by a series of previous choices and changes that took place in the leading fields of scientific research. Such innovations developed in connection with the evolution of the role, social position, stimuli and cultural horizon of the scientific community and of the role of science and technology and their mutual relationships.

In order to get a better understanding of these transformations and to place them in a historical perspective, I have chosen to investigate the contrasting attitudes that developed (explicitly or implicitly) against the early trends towards large-scale research and the alternatives that were proposed to them. Such an investigation should not give the impression of a nostalgic point of view, since our purpose is to contribute to the understanding of the objective historical trends. This approach does show in fact that the road to large-scale research was not a compulsory choice from a point of view of scientific investigation in itself: extremely valuable experimental and theoretical physics was being done by those scientists who did not accept this road; they sometimes got even more accurate or better results. But Big Science turned out to be the winning choice because it corresponded to the stream of historical and social development.

With this purpose in mind, I have studied the growth of nuclear physics with accelerated particles in the thirties, I have followed the war and post-war choices in research activity made by some of the leading scientists in this field and I have compared the developments in different countries, in order to distinguish and characterize conflicting or divergent roads or styles of research.

2. – Let me start with the U.S. The outburst in this field of research took place here at the very beginning of the thirties and one is struck by its coincidence with the worst period of the economic recession: the growing difficulties in the funding and development of scientific research in general, strongly contrast with the relative easiness with which atom-smashers found financial support and started large-scale research. Behind this one recognizes the precocious interests of the leading industrial sectors toward the emergent fields and the new role that scientific and technological innovation had to play in the *New Deal*. New features appeared in scientific activity in such fields as particle accelerators and nuclear physics in the U.S. (in contrast – as we will see – with other countries): growing costs and dimensions of machines and labs, team research, competition and rush for the results, growing mean number of authors for each paper, management as part of scientific activity raising increasing funds.

Three groups developed early particles accelerators in the United States¹:

- 1) that of Lawrence in Berkeley
- 2) Tuve at the Department of Terrestrial Magnetism (I will call it DTM) of the Carnegie Institution of Washington
- 3) the group of Crane and Lauritsen in Pasadena.

Ernest O. Lawrence was probably the most significant representative of these new trends, while his friend Merle A. Tuve – another protagonist and leading scientist – expressed perhaps the strongest and most explicit opposition toward large-scale research trends.

Some striking features of Lawrence's character have already been analyzed^{2, 3, 4}: his competitive and managerial leadership, his constant trend towards larger machines and higher energies, his ability in collecting financial support everywhere, and in this connection his concern with 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 made 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's physicists. Lawrence moreover was awarded the Nobel Prize in 1939, while Tuve missed out, even if he probably would have deserved it more than once. These facts greatly derived from Tuve's particular character and attitude, which led him to dislike the mechanisms and spirit that were increasingly

pervading a research activity of ever growing dimensions. In this sense Tuve at the end ended up defeated by the changes taking place.

Remember that Lawrence and Tuve were born in the same town, were school-friends and constantly linked by deep friendship all through their lives. The bitter remarks Tuve had to make about Lawrence's research are even more significant.

Lawrence's group published the first results of experiments in nuclear physics with charged accelerated particles well before Tuve's group⁵: unfortunately the lack of rigour in these experiments became evident in short time and was recognized and constantly remarked 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.

The first experimental results published by the DIM group⁶ 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"⁷ (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 divergences, the Lawrence-Tuve friendship was so deep that the first provided the latter with the heavy water necessary to perform the experiments with accelerated deuteron beams⁸.

On the other hand these experiments became the major point of disagreement. In fact, Lawrence, proposed at that time the famous "deuteron disintegration hypothesis"⁹, that he reported at 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"¹⁰. Lawrence had already replied that, if the initial evidence was effectively scarce, "I think we have now pretty conclusive evidence on that point"¹¹.

After the Solvay Conference, Lawrence had to perform more accurate tests in order to exclude that his results derived from systematic contaminations¹², as he wrote Tuve on December 21, 1933¹³.

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 in a very rigorous test of the experimental techniques¹⁴.

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”¹⁵ – *with the whole set of observation, not only with the deuteron 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”¹⁵.

At that point Lawrence’s reply¹⁶ was lengthy but appeared very embarrassed, and outlined the first autocritical considerations, since in the meantime his deuteron results had been contradicted also by the Pasadena group¹⁷ and at the Cavendish Laboratory¹⁸:

”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 measurement as we can”¹⁶.

Lawrence finally admitted his mistake in the deuteron disintegration hypothesis¹⁹. 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 interesting 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*²⁰ 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”²¹.

In Tuve’s action one may recognize a mixture of real embarrassment 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”²².

On the other side, however, a harsh press release was emitted by the Carnegie Institution of Washington after the Meeting of the A.P.S. of April 26, with the hironic 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 Institution 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"²³.

On August 4th 1934 Tuve Himself sent *Science* – through Fleming – an official rectification²⁴ since the Journal had reported in "erroneous and misleading" terms the results obtained at the DTM, had not explicitly referred of the "contamination effects" and had expressed the opinion that the experimental results from various laboratories were not in contradiction.

The whole story inspired Tuve with a sense of deep regret that he expressed to Lauritsen bitterly remarking that such an accident "must occur rarely, if at all" and, since Lauritsen replied that "that sort of things should never appear in print", he firmly added that rather "the sort of things that should never appear in print were what led to the necessity for such a statement by me"²⁵.

This course of events reveals, in my opinion, not only 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 physics⁵, namely in 1935, the first widths of nuclear resonances and, with his beautiful experiments on proton-proton scattering, charge independence of nuclear forces.

I could note that the cyclotron was perhaps mainly the father of the post-war new generation of accelerators, while Tuve's "Atomic Observatory", built up at the Department of Terrestrial Magnetism, perfected electrostatic machines, but preserved the familiar atmosphere still existing today in this institution.

Lawrence's choices appear instead dictated more by the goal of rising funds for big enterprises, by a need of guiding or following the stream of advanced research, than by true scientific motivations. For instance, in 1935 he wrote Bohr: "In addition to the nuclear investigations, we are carrying on investigations on the biological effects of the neutrons and various radioactive substances and are finding interesting things in this direction. I must confess that one reason we have undertaken this biological work is that we thereby have been able to get financial support for all of the work in the laboratory. As you well know, it is so much easier to get funds for medical research"²⁶.

A different spirit was really born, anticipating the mechanism of Big Science.

3. A stronger confirmation of the new features that are appearing may be obtained following more thoroughly Tuve's uncommon choices during and after the war.

It is important to remark that Tuve had made 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 as early as 1925 to determine the ionosphere height observing the echoes of short radio pulses, "they were troubled by echoes coming from airplanes, which interfered with the measurements"²⁷; "this was the first recorded instance of distance measurements made by the pulse-radar method"²⁸.

Tuve made moreover leading contributions to the study of nuclear fission. With Roberts, Mayer and Hafstad he showed the first fission process at the DTM accelerator²⁹, discovered the emission of the "delayed neutron"³⁰ and subsequently they contributed to show the possibility of a chain reaction³¹: "We have been hard pressed to get some data on uranium fission, largely because Fermi, Rabi, Szilard, etc. have been afraid of chain 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"³². A confidential memorandum of June 1, 1939 to the Director of the DTM by Gunn, Technical Adviser of the Naval Research Laboratory at Anacosta, explicitly mentions in this respects Tuve's availability "to carry on the final tests at his laboratory"³³; on may 23, 1940 the Carnegie Institution of Washington appropriated \$ 20.000 "for study on uranium fission"³⁴.

Tuve was a member of the Uranium Committee called by Roosevelt after Einstein's letter, but his attitude changed at the beginning of 1940. "It all started in February 1940... At that time, Roberts, Hafstad, Heudenburg 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"³⁵. While "by May 1940, in talks with officers in the R and D division of BUORD, U.S. Navy, I had learned about the ridiculously low effectiveness of antiaircraft fire. I heard the term «influence fuze» (later «proximity fuze»), as wistful hope"³⁶.

The history of the "proximity fuze" has in part been written³⁷. 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 and growing in the other projects, of early Big Science. 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 him"³⁸. In Tuve's words: "one of the greatest «new develop-

ments» 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 ad 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”³⁹.

But, in spite of Tuve’s subjective wishes and intentions, the Applied Physics Laboratory evolved into a model of advanced large-scale research. This happened, in my opinion, not only under the pressure of emergence in the war-period, but mainly because the force of things – in this case of the Big Science mechanism – was stronger than subjective intentions.

Tuve’s post-war choices were an attempt to react concretely against Big Science and to follow a different path. In a research program he proposed in the spring of 1946⁴⁰ a preliminary choice was discussed in the initial “General comments”: “It is pertinent to question whether the Institution should have any postwar program at all in nuclear physics, with large-scale government support assured in many countries and with this field of scientific effort sure to be tied up with political power-struggles, certainly for many years to come. The conclusion was reached, however, that work in this field should be continued at the Department.”⁴⁰

The end of war-time emergency thus no longer justified “large-scale government support”. As a matter of fact, Tuve – coherently with his positions – had come back to the DTM (his pupil Hafstad had succeeded him as Director of the Applied Physics Laboratory and fully entered the Big Science mechanism). When Jewett submitted to Lawrence himself and other members of the Committee on Terrestrial Sciences of the Carnegie Institution on March 18, 1946 Bush’s suggestion that Tuve be appointed to the Directorship of the DTM, he underlined Tuve’s qualities, but raised doubts because “he has at times in the past shown a tendency to rub men the wrong way” (even adding that he “has matured very considerably in the last few years”) and concluded that “both Bush and I are agreed that Tuve will be either a great success or a very great failure as Director”⁴¹.

Tuve, on his part, presented the already mentioned suggestions⁴⁰, and a subsequent more official statement⁴² concerning the future research program of the DTM. In the official report the premise on “General objectives an emphasis specifies the connection between the choice of continuing the research activity in a Department of limited possibilities and the kind of research that can be performed: “Bearing in mind the special character of the opportunity presented by the Carnegie Institution of Washington, with its unusually great freedom of objectives, since there are no external groups

whose interests limit the program, and viewing the corresponding obligations which go along with this freedom, it is agreed that we must make every possible effort to emphasize creative work, work with new potentialities, and work which lies on the front lines of knowledge. There are serious restrictions as to possible size of staff and annual expenditures, and accordingly our program must be chosen with regard to its effectiveness as a stimulus or catalyst to the work of all other groups concerned with a given field. These considerations lead naturally to a major emphasis on cooperative endeavors, in which the Institution and the Department can be of great influence and value if we are capable of vigorous leadership in fresh and significant directions"⁴². In this connection, Tuve proposed that work in nuclear physics should be continued anyway by a "recognized and well qualified group quietly working on private funds at an agency of high standing and very wide connections, such as the Carnegie Institution"⁴⁰. More precisely "True research – creative research – is always done in very small groups, rarely exceeding five or seven individuals, and hence this separation of the Department's staff into very small discreet groups, with reasonable fluidity for shifts between groups, is regarded as both realistic and healthy"⁴².

"...*creative research* is never carried on by groups larger than seven members – usually four is a better size. Larger groups invariably concern themselves with engineering or development, not with the painful carving out of really new ideas or directions of progress. Several groups of three to seven members, each with one or two strong men (age difference is valuable), can be loosely *associated* but creative research is *not* carried out by large teams who are coordinated (that is, ordered) or closely directed by a single head man. A *leader* can stimulate several groups to productive activity, but real creative research is not carried out toward goals which are defined in advance too specifically or in too limited a way. At best, its limitations can only amount to a positive encouragement or emphasis in a selected broad area of interest, and valuable offshoots are sure to occur in other related but rather unexpected directions. A single over-all leader, stimulating and guiding toward *general* goals, is, however, most valuable and even necessary, to insure cooperation and integration in place of fragmentation into separate compartments and unrelated interests"⁴⁰.

And again: "It is our conviction that investigators can be stimulated and led to creative contributions, but they cannot be driven; hence we must evolve leaders in our small groups, but we cannot use authoritarian procedures. Individual professional responsibility, however, also means that individuals should be judged by their creative research contributions; steady or devoted work is almost irrelevant as a criterion of accomplishment or virtue. Since individuals differ in their capacity to contribute creatively, however, they will be expected to recognize this and to invest their energies

willingly in directions which are pointed out by other members of the group working in their field of interest, after group consideration indicates that these suggested directions for effort give promise of creative fruitfulness.

One picture should always be kept in mind by the professional research staff: it must surely be evident to everyone that the Founder of the Institution had no thought whatever that his great free endowment should be used to keep 150 people simply busy six hours per day! In fact, he must have intended just the opposite; his endowment was intended to free a certain creative group of men from the necessity of having to be busy, and their success in measuring up to their opportunity can only be measured in term of their creative output"⁴².

What kind of research did Tuve suggest in this context? "The chief aim of the suggested program as for any research program, appropriate to the Institution, may be stated as an effort to underwrite and support the vigorous personal activities of modest number of competent research men, associated in a congenial and cooperative group with a variety of different and related interests, who are pushing forward the front-line boundaries of knowledge. To be appropriate, their objectives should be to establish basic principles, or the materials on which such generalizations may be expected to be formulated; the work should be directed toward major unknowns or big unanswered questions, and it should lie in areas of learning in which such new knowledge, if attained, would have importance, in the sense that it could be expected to have considerable significance to many human beings, other than the specialists directly concerned. The specialized laboratory work in nuclear physics at the Department before the war – resulting, for example, in the demonstration and measurement of the proton-proton and proton-neutron interactions – and the biophysical work with radioactive tracers during the war – concerned with fundamental physical processes in physiology – has met these criteria. Much more work of this fundamental kind remains invitingly open to immediate postwar attack. This is one appropriate goal for the laboratory program"⁴⁰.

But the research work "in government and private research institutes, contrasted with those of similar groups in various universities, also public and private" poses, in Tuve's opinion, a fundamental problem. "The impact of young minds has long been recognized as a major factor in keeping university staff members productive and creative in fresh directions... In the course of ten years a (lively) professor will give half a dozen different courses, each of which requires him to work over a different area of his broad professional field. He will also be obliged many times to take charge of research students who select problems which lie more or less outside of his own special field of current interest and work; this, too, requires him to study, think, discuss, and even create new ideas in various different areas of his broad professional field. ... Contrast this with staff members of special-

ized research institutes; in the same ten years, working *all* of his time in a narrow field, the specialist dries up many of the channels by which he should receive nutrition from his own broad professional field⁴⁰. It follows that “the prewar program should go forward, but it should be modified to become something other than just a specialist group-activity in nuclear physics or biophysics. The dangers of over-specialisation in these fields may be a great as in many others”⁴⁰. Instead, “a research specialist should actually *work* at least a fifth of his time *outside of his speciality* and in some other area of his broad professional field”⁴⁰. More precisely, “it seems reasonable that an investigator might be required to «*work*» one-fifth of his time on problems which lie outside of his speciality, and that an actual output in this other area should be expected (that is, some arrangement is needed which requires him to face critical judgments of others) and furthermore that, although he may be a lifelong specialist in some one field, this second or minor area of his work should not remain the same subject for a number of years (this would just make him a bifurcated specialist)”⁴⁰.

In the same context, “as before the war, the laboratory program in nuclear physics should again be concerned with «philosophical» problems relating to the primary particles of matter and the laws governing their interactions with each other and with radiation (...) (The Manhattan Project work was not directed toward these problems of nuclear physics; they were really concerned with nuclear «chemistry»)”⁴⁰.

In the following years Tuve’s positions explicitly clashed with many choices of scientific community. Allan Needell of the Smithsonian Institution has thoroughly reconstructed Tuve’s struggle against Lloyd Berkner concerning the establishment and operation of a national radio astronomy facility in Green Bank⁴³. Tuve in fact had left nuclear physics since “it changed from a sport into a business”. In the struggle with Berkner he expressed the conviction that the new, expensive tools of research were “subsidiary and peripheral” when compared with the support of individual researchers. He insisted that those tools, in his words, “...did not serve appreciably to produce or develop creative thinkers and productive investigators. ...At best they serve them, often in a brief and incidental way, and at worse they devour them”.

He repeatedly expressed himself against Big Science. In 1959 he published on the *Saturday Review* a long paper with the title: “Is Science too Big for the Scientist?”⁴⁴. He repeated this concept in a meeting in which President Eisenhower announced the appropriation of \$ 100 million for the future Stanford linear accelerator⁴⁵: Tuve made such a bald statement that his colleagues publicly reprimanded him that “this was neither the time nor the place” for it⁴⁶.

Since I started my analysis with a comparison between Tuve and Lawrence in their early research activities, I may just recall here the very different road followed by the latter, which remained most representative of

the choices made by the scientific community and of the radiation of Big Science. Lawrence collaborated with the National Defense Research Committee on microwave research and submarine detection, took part in the Manhattan Project, actively gave advice on the construction and the use of the bomb. After the war the Radiation Laboratory was financed with funds from the Manhattan District. In 1952, on request of the AEC, Lawrence founded a new laboratory at Livermore for military research, a prototype of large-scale specialized structure.

4. – I have dwelt on Tuve's personality in order to single out, in contrast, the changes in American nuclear physics in the thirties that anticipated and led to Big Science.

But, instead of looking at specific personalities, one may study and compare the developments of nuclear physics in the same period in different national contexts. Such comparison shows the peculiarity of the conditions that led the U.S. to play an original role of absolute leadership in introducing and guiding the transformation of science and research.

It is not the task of this paper to perform a thorough analysis, but I would like to try to give some ideas.

The French, British and Italian physicists brought major contributions to nuclear physics in the thirties. Trends towards large scale research may undoubtedly be individuated also in these countries, but in my opinion a careful analysis, which does not stop at superficial events, shows that these remained isolated examples and did not turn into a general and deep transformation of science involving its methods, structure, role and connection with technological change and with society in general.

In 1937 Hafstad, Tuve's most strict collaborator, visited Joliot's laboratory in Paris, where work was being done on a program of cyclotrons, high-voltage and electrostatic accelerators. Hafstad noted that "no apparatus was in condition for the making of observations", "in the U.S. this state of development was passed about three years ago" and "it was evident that Paris was far behind the U.S."⁴⁷. A final judgement included also the Italian group in Rome: "Nearly all European laboratories are at present engaged in a building program. This perhaps accounts for a rather surprising exchange of positions between American and European laboratories. A few years ago it was being said that, whereas much work on apparatus was being done in the U.S., practically all scientific results had been obtained in Europe using radium technique. The situation is reversed as scientific results are being obtained from the perfected apparatus in the U.S., whereas the possibilities of the old radium technique in Europe are now practically exhausted. It is of the utmost significance that, for perhaps the first time, Europe is definitely behind the U.S. in experimental physics and that they now find it necessary to send men to this country to acquire techniques which can be carried back to Europe"⁴⁷.

It seems evident that large-scale apparatuses and new techniques in American nuclear physics were not *in themselves* a step towards Big Science; they were only the exterior events, induced by much deeper processes. The better confirmation is perhaps given by a comparison with the British situation, where accelerating machines had been built and used for the first time.

In 1930 British nuclear science had already a sound tradition. It however identified itself with Rutherford's personality, which had a very strong ascendancy on his pupils. The prevailing spirit was extremely different from that of the Americans. It was marked by the ethics of pure science as a disinterested academic activity. There was no interest in the possible technological value of the investigations (Cockroft was in some sense an exception and a special figure: he was an electrical engineer; in 1935 he abandoned active research for some years and, after Rutherford's retirement, started the building of new machines). The figure of the British scientist seemed more eighteenth century-fashioned than similar to the American one. He had faith in the cognitive value of the experimental result in itself. The experimental groups hardly ever exceeded the number of a couple of scientists and had substantially distinct fields of interest, avoiding consequently competition. Direct interaction between experimenters and theoreticians was rare.

After 1935 there was a sensible decline in British nuclear physics, deeply contrasting with the growth of Americans physics. Chadwick had found in Rutherford opposition in following an advanced research program. It was not chance that, after Rutherford's retirement and death in 1937, only Chadwick and Cockroft undertook a program of building new machines and they were among the British scientists most directly involved in war-time collaboration on the main projects with the Americans (Cockroft on radar and Chadwick as the leader of the British team in the Manhattan Project).

5. – There is however another national situation whose careful analysis would be extremely interesting and meaningful I refer to nuclear physics in Japan. There is, in fact, a very interesting, peculiar feature of this situation: the Japanese nuclear physicists did build up and use with a very short delay the new machines and instruments introduced by the Americans the other western scientist but followed an original line of thought, linked to the Japanese philosophical tradition, that led to physical ideas different from and incompatible with the framework emerging from nuclear investigations of the western physicists.

In spite of the choice of machines and instruments and of their use in the laboratories, no large-scale style of research at all was induced in pre-war Japan, and the previous philosophical tradition had a much stronger influence on the programs and the results than the above mentioned material choices and the experimental results and programs.

A thorough analysis of this case-study would then, in my opinion, throw light on the complex of factors that created the conditions for the birth of large-scale research and the premises of Big Science.

I will not actually develop in detail this suggestion and I refer to important contributions by Takabayashi⁴⁸ Takeda and Yamagouchi⁴⁹, Brown Konuma and Maki⁵⁰, Hayakawa⁵¹. I will limit myself to adding some brief comments.

Japanese physicists acquired the new quantum concepts between the end of the twenties and the beginning of the thirties. Some of them came back after stays in Western countries: Nishina in particular visited Bohr and Rutherford and played a very important role in orienting the activities in nuclear physics and cosmic-ray physics.

These activities grew rapidly: the first cloud chamber was built in 1933 and coincidence methods and automatic operation were realized soon after Blackett and Occhialini and quite independently from them; in 1934 three Cockcroft-Walton accelerators started working (one of 200 KeV, and successively another of 600 KeV at Riken in Tokio; another of 600 KeV at Osaka); after 1935 Watase and Itoh started building a cyclotron. But the experimental activity, although intense, did not play a leading role, since the Japanese physicists were not so much interested in applied or technical aspects, as rather in elaborating a unifying scientific conception, having its roots in the Japanese philosophical tradition. Thus it was that they strictly linked together the problems of the nucleus and of cosmic rays and fundamental particles, that on the contrary kept for a long period distinct characters in Western physics.

They managed to build for this whole field a comprehensive, unifying conception very different from the set of theories and models that were elaborated by Western scientists.

In short, let's refer to Yukawa's meson theory. The meson was not only the agent of nuclear forces – as it was accepted in Western physics – but was a central element of a much more general and complex conception, that never was fully perceived in Western countries.

Apart from the easiness with which Japanese physicists introduced new particles (as contrasted to the early hesitations of Western physicists, for instance of Pauli for the neutrino hypothesis), Yukawa's meson was supposed to decay into an electron and to be consequently responsible for β -decay as for nuclear forces: contrary to Fermi's theory of β -decay, deriving from an interaction different from the nuclear interaction – the weak interaction – the conception proposed by the Japanese scientists had a unifying character. (We may recall that previously, in 1933, under the influence of Heisenberg's model of nuclear structure, and before Fermi's paper, Yukawa had proposed to attribute β -decay to a transmutation of the proton: at that time he considered the electron as a field mediating the nuclear force. In that

clear force. In that occasion Nishina had suggested that the exchange of a boson between two nucleons would have preserved spin and statistic.)

Starting from the previous comments, it could be very interesting to follow the further developments of the views of the Japanese scientists in the following years, in a condition of substantial isolation and independence from the evolution of the lines of thought of Western particle physics. It will suffice here to mention, apart from important contributions by Tomonaga and Yukawa himself, the evolution of meson theory with contributions of Taketani and Sakata. Their motivations were again not primarily experimental, but mainly ideological. The two scientists were working in the framework of marxist philosophy.

A further development, stemming from the problems posed by the mean life of the meson, was the "two-meson theory". Only later this theory proved to be wrong when compared with the experimental data that were accumulating.

In 1952, finally, Sakata proposed a theory with tree fermions as the fundamental constituents of matter ("sakatons") linked together by an unknown "B-matter". Sakata's theory anticipated in some sense the unitary approach, but was in fact quite independent from it and had moreover completely different origin and motivations. One may also perceive an analogy with actual gauge theories in terms of quarks and gluons, and probably such an analysis has become sounder, having a unifying proposal at its basis.

I hope to have given, from the perspective I have chosen, a modest contribution toward the individuation of the specific factors that created the conditions for the birth of large-scale research and of the features that really characterize a turning point in the development of science and research activity.

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