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HOW LITTLE SCIENCE BECAME BIG SCIENCE IN THE U.S.A.

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How did little science become big science in the U.S.A. during the past half century? That question is really only one of a perplexing set of related questions which are of concern to most of us. Others are: Can science, as we have known it, survive the evolution from "little" to "big"? Has the character of science been changed? If it has, has it changed for the better or for the worse? And what has happened and what will happen to the creative scientist in the world of large and expensive facilities, equipment, research, and research groups? These are all questions with which many of us have been concerned. This conference is concerned primarily with the past, not the future. It may be important to understand the past in order to cope with the future.

The year 1930 is a particularly significant one in the world of nuclear and particle physics research. Stanley Livingston went to the University of California, Berkeley to work under Ernest Lawrence for his PhD degree. It was in May, 1930 that Lawrence assigned Livingston the project of constructing a small cyclotron. Work was finished on that project ten months later, in March, 1931, and it worked, confirming the cyclotron principle.

So the year 1930 is a good one to mark the beginning of the era of the particle accelerators which became the microscopes through which particle physics has advanced so remarkably in the past 50 years. During that same period, particle physics led the way with regard to the evolution of little science into big science. Perhaps the most important facet of that evolution was development of a new modus operandi for particle physicists from that of a single individual, self sufficient, working alone, - to a large group of specialists, each an expert in one or another area of equipment or physics, but few having the breadth of view and of knowledge necessary to encompass an entire experiment, its motivation, its methods, and its conclusions.

To carry the Berkeley story a little further, after completion of the first cyclotron in March, 1931, Lawrence raised the magnificent sum of $500 from the Research Foundation for the purpose of building a larger cyclotron. Work on that second accelerator started in April, 1931 with Livingston assigned the principal responsibility. The instrument had an 11" magnet and produced 1 MeV protons. Livingston worked fulltime on the project and the physics department shop provided the main support services.

With that large investment of resources the second cyclotron was successfully completed in January, 1932. That accelerator was passed along to Milton White to use for his thesis, and Livingston moved on to the construction of a 27½" cyclotron which produced 3 MeV protons and 5 MeV deuterons. The latter had just recently been discovered to exist. That same accelerator was later expanded to a 37" machine and to an 8 MeV energy in 1936. In 1939 the 60" medical cyclotron was built at Berkeley, producing 16 MeV protons, 20 MeV deuterons, and 40 MeV helium nucleii. In 1939 the Nobel Prize went to Ernest Lawrence for his invention of the cyclotron.

The first four figures show the evolution that has just been described.

Fig. 1 : Ernest Lawrence holding the accelerator portion of the first cyclotron (1930)

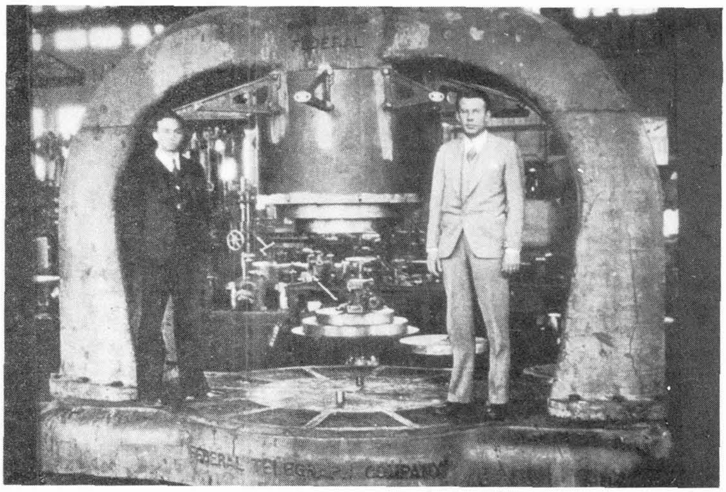

Fig. 2 : Ernest Lawrence and Stanley Livingston standing in the magnet yoke of their 10 inch (1 MeV) cyclotron ($\sim$ 1931)

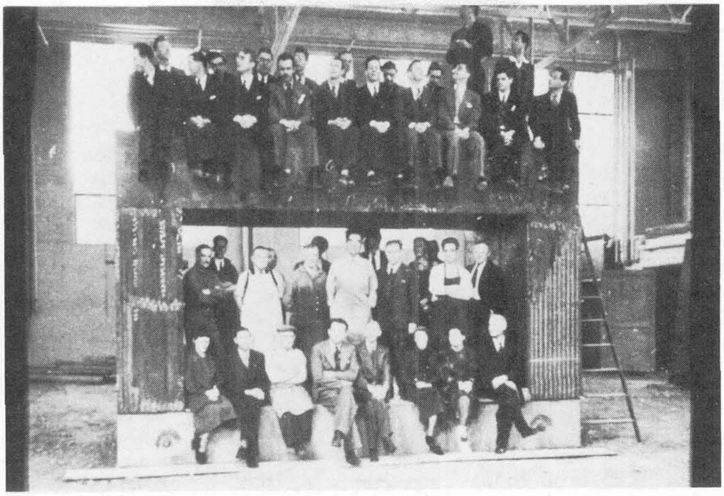

Fig. 3 : Entire technical staff of the Radiation Laboratory with the magnet yoke of the 60 inch cyclotron (∿ 1939)

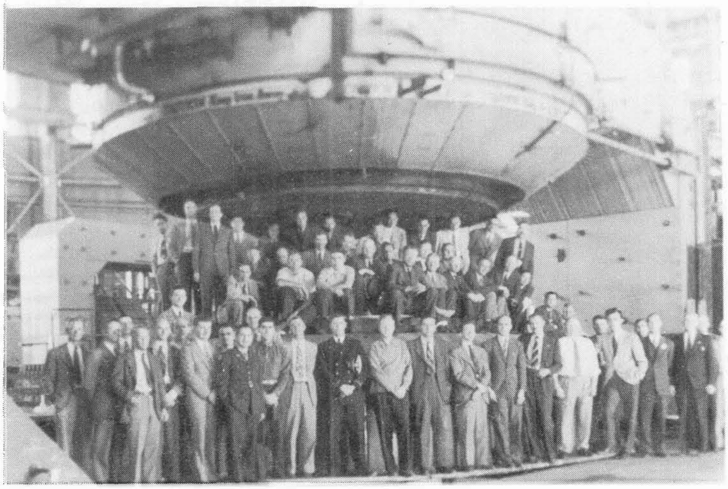

Fig. 4 : Radiation Laboratory staff in the pole space of the 184 inch cyclotron (∿ 1947)

I have hastily cited the above history, because it has within it the information which calls attention to the fact that particle physics was still small science at the beginning of the accelerator period. One man built a cyclotron in one year, and the associated cost was about $1,000. That was a style of work which differed little from what was common in physics in those days.

During the interruption of World War II, an important new development occurred in physics in the United States. In its effort to win the race to a nuclear weapon, the Manhatten Project was organized and the Los Alamos Laboratory was established. First a handful of scientists was assembled, but that number was rapidly increased to meet the varied demand of the crash program. Scientists were thus initiated into an experience of working in an environment that was rich in the presence of the very best theorists and experimentalists who could be assembled. Most of the scientists who participated in that adventure still remember it as a uniquely stimulating experience.

The success of the project was an important ingredient, not only in the future thinking of scientists, but also in the future thinking of the federal government. The notion that a large amount of money invested in a large number of the best qualified scientists could bring about the solution to an extremely complicated scientific-technical problem was important for two, separate reasons. From the point of view of the scientists, it demonstrated the possibility of working cooperatively on research which traditionally would have been conducted by only a very small number of scientists and therefore which would have been vastly stretched out in time and limited in scope.

After the war, some of the scientists who had felt the exhilaration of working on a tough problem with a large group of their peers tended to look for a post-war situation in which a powerful group would be formed at a university for the purpose of pressing further the frontiers of basic research on the structure of matter. Many of the Los Alamos scientists were attracted to Berkeley where Lawrence's initiative had already established a start. The Radiation Laboratory which had been formed in 1931 eventually became the Lawrence Berkeley Laboratory.

From the point of view of the government, the notion took root that an investment of federal dollars in a group of scientists could bring about the solution of any problem, whatsoever. The "atom bomb" appeared to be as impossible a problem as one could pose. Yet, simply by investing a large number of dollars and rallying a large number of scientists a solution had been found.

This illusion tended to propagate itself after the war with effects that were both good and bad. On the one side, having become convinced that it was in the national interest to be in the forefront of research, the federal government invested liberally in research after the war. On the other side some entertained the notion that the government, itself, could choose the direction in which it wished research to progress and that by investing funds in that direction, progress would, perforce, follow.

The fact is that no one is as well fitted to choose the direction of research as the scientists who are involved in the research. The most important skill of a good scientist is the ability to choose the right problem at the right time. It is the scientist who knows when a research idea is ready to bear fruit, when work should be postponed, when the direction should be changed or when it should be given up entirely. On occasion, since the war, the government has attempted to take more than its appropriate share of initiative, with the result that time and money and people may have been wasted pursuing an unprofitable direction of exploration. Fortunately those cases have been rather few. For the most part the support of science has been generous and wise. It has leaned upon peer review for its evaluations and judgments.

After the war the most powerful single group of nuclear/particle physicists was assembled at the University of California in Berkeley. However, at the same time, cyclotrons began to blossom at universities and institutes all across the country. The cost of such a machine, given the new government interest in sponsoring research, was within the means of many institutions. The manpower required to build a machine was within the means of a single institution. The potential research output of one of those machines was well-matched to the group of scientists which one might find at most first-class universities. Among the institutions in the U.S. at which cyclotrons blossomed both before and immediately after the War were: the Bartoll Institute, Carnegie Institute of Washington, Carnegie Tech, Columbia University, Cornell University, Harvard University, Massachusetts Institute of Technology, Princeton University, Purdue University, Rochester University, University of Chicago, University of Illinois, University of Indiana, University of Michigan, and Washington University.

During this period developments were also being made on linear accelerators. In 1948 Luis Alvarez completed a 32 MeV proton linac at the Radiation Laboratory. In 1955 construction was started on a large electron linac at Stanford (SLAC).

During the same period the principle of phase stability was developed by MacMillan at Berkeley and Veksler in the USSR. The first synchrotron was built, and the betatron was developed by Kerst at Illinois.

This period was marked by a proliferation of facilities and a burgeoning of the population of physicists who were interested in building and using the facilities. As the projects became physically larger and as the costs became significantly higher the size of the group of involved scientists also tended to become larger. Specialization became common, if not a necessity. Theorists and experimentalists had long ago formed fairly distinct, specialized groups. Now, however, in most places, accelerator builders established an identity quite distinct from that of the scientists who wanted to use the accelerators. At the same time the size of experimental equipment which was needed in order to detect particles and to do an interesting experiment grew even faster than the accelerators.

The proton-synchrotron concept made it possible to build a ring of magnets to contain an accelerating beam instead of building a solid, continuous poleface. And so the GeV generation of accelerators was initiated. The cost was one or two million dollars per GeV. (Taking inflation into account, a lower unit cost than that which applied for the first cyclotron, $1,000 for 1 MeV). The Cosmotron at Brookhaven was finished in 1952. The Bevatron at Berkeley soon followed, and the 1959 Nobel Prize went to Segre and Chamberlain for the discovery of the antiproton at the Bevatron.

Figures 5 and 6 show the scale of the multi-GeV generation of accelerators as compared with the machines shown in the first four figures.

Fig. 5 : Lawrence, Fidler, Brobeck and Cooksey in the aperture of one of the many Bevatron magnets ($\sim$ 1953)

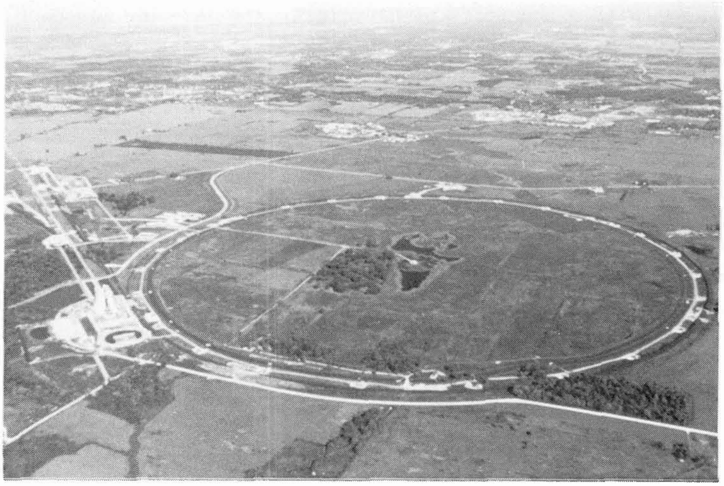

Fig. 6 : View from the air of the Fermilab site showing the 1 kilometer radius circle below which the accelerator is housed (~ 1977)

The newer, larger machines were not necessarily associated with a single university either in name or in fact. However the U.S. Atomic Energy Commission made a conscious effort to maintain the style of university-based research so, along with the AGS at Brookhaven and the ZGS at Argonne came the Cambridge Electron Accelerator at Harvard-M.I.T. and a proton synchrotron at Princeton. Cornell, with NSF support, maintained its special, R.R. Wilson tradition of a series of university-based accelerators. However it became clear that it was not a sound investment of research funds to support any facility which served primarily to extend a way of doing physics and which did not actually represent an advancement of some frontier of particle physics.

"National Laboratories" became the new fashion. The philosophy was quite simple. When the physical size of a facility, its cost, and its scientific output become too great to be supported by a single university, a national laboratory should be used to house the facility and to operate it for the use of scientists at many universities. That new style of doing science made it possible to start the move toward a reduction in the number of facilities. The trend became one of fewer facilities, on the one hand, while each facility served many more experimenters, on the other. But along with that administrative invention came a set of new problems. Scientists at universities became concerned that the national laboratories would gain research identities of their own and, since a national laboratory staff would consist of scientists who would be full-time residents, that they would capture the predominant use of the facility, leaving little or no research time for outsiders.

The solution to that problem involved a major policy decision. In the United States, the style of doing science has been one in which research has been wedded to an academic setting. In general, we have found that, for us, research flourishes best in that setting, because the university environment assures a perpetual flow of new, bright young scientists, always ready to challenge the comfortable beliefs of the past and free of the prejudices and commitments which can keep established, permanent, staff scientists in pursuit of narrow, unchanging goals, even when interest in those goals may have dwindled considerably.

Given that philosophy, it became a matter of great importance to set up the management of national laboratories in such a manner that they would be responsive to the needs of the university scientists whom they were intended primarily to serve. The idea of a university consortium was born. Brookhaven National Laboratory became one of the first examples of a successful management consortium. A group of universities in the eastern United States joined together to form Associated Universities Incorporated (AUI) to contract with the Atomic Energy Commission for the construction and management of Brookhaven. Associated Midwest Universities was formed to play a role in policy formulation for the Argonne National Laboratory. That consortium was later followed by Argonne Universities Association which, with the University of Chicago acting as manager, participated in the formulation of policy at Argonne for a number of years.

The largest of such consortia was formed in 1966 to build and manage the largest of the accelerators. Fermilab was built and is run by Universities Research Association, a consortium of more than 50 universities throughout the United States and Canada.

But even with the invention of these consortia, scientists at universities were still concerned that accelerators might be designed and facilities built in such a way that some of the more interesting experiments might not be able to be accommodated. Therefore the idea of a "Users Group" was invented. An early group of that kind was started at Brookhaven National Laboratory, but that group was headed by and essentially run by the Laboratory management. An Argonne Users Group was formed in 1958 in response to persistent demands by midwest physicists that their needs and plans be heard as the ZGS was designed, built, and put into operation. This particular case commanded senatorial and even presidential attention.

In a letter of that time Senator Humphrey wrote that he "placed great emphasis .... on the necessity of taking all practical steps to get the Midwest universities behind the Argonne National Laboratory and to find ways to give them a greater voice in the program for management at the Argonne National Laboratory." And a letter dated January 16, 1964, from President Johnson to Senator Humphrey, stated (in those days presidents were really worried about high energy physics): "I would hope and expect that the fine staff of MURA would be able to continue to serve the Midwest through the universities and at Argonne, and I have asked Glen Seaborg to use his good offices in that direction. I have also asked him to take all possible steps to make possible an increase in the participation of the academic institutions of the Midwest in the work of the Argonne Laboratory. He has outlined for me a concrete proposal to accomplish this. I share fully your strong desire to support the development of centers of scientific strength in the Midwest, and I feel certain that with the right cooperation between government and the universities we can do a great deal to build at Argonne the nucleus of one of the finest research centers in the world."

With the formation of the Argonne Users Group as a quasi-independent entity came the idea that major facilities which were to become an intrinsic part of a laboratory's program could be constructed <u>at</u> universities and <u>by</u> university groups for use at the national laboratory. A 30" bubble chamber was built at Wisconsin and used at Argonne by physicists from many universities.

While there was a general trend toward National Laboratories, the Stanford Linear Accelerator Center and the Radiation Laboratory at the University of California at Berkeley remained major laboratories in the single university management format. In the Stanford case, safeguards were built into the AEC contract in an attempt to assure user access. But neither Stanford nor Berkeley had as much user participation as was characteristic of the consortium-operated laboratories. An associated lack of confidence of the user community was a significant factor in the siting of the National Accelerator Laboratory (now Fermilab) outside of California.

Although the Radiation Laboratory at Berkeley remained under single university management, with the advent of the 72" bubble chamber it became clear that more data could be produced than could possibly be handled, even by the large group at Berkeley. Accordingly a few experimental proposals were accepted from physicists from universities, and bubble chamber runs were made which delivered film to those user groups.

As user groups were formed, new liaisons began to be made between physicists at different universities. The complexity of apparatus in experiments became such that larger groups were necessary in order to cover all of the various techniques which were involved in the design and operation of an experiment. A typical experiment, in addition to requiring a very complex accelerator facility, also required a complicated line of beam transport to bring particles from the accelerator to the experimental equipment.

In addition to beam transport there were often complicated cryogenic systems, very large and complicated detectors, complicated computer systems, as well as conceptually complicated physics. It became impossible for most participating scientists to be familiar with the whole thing. Specialization became essential.

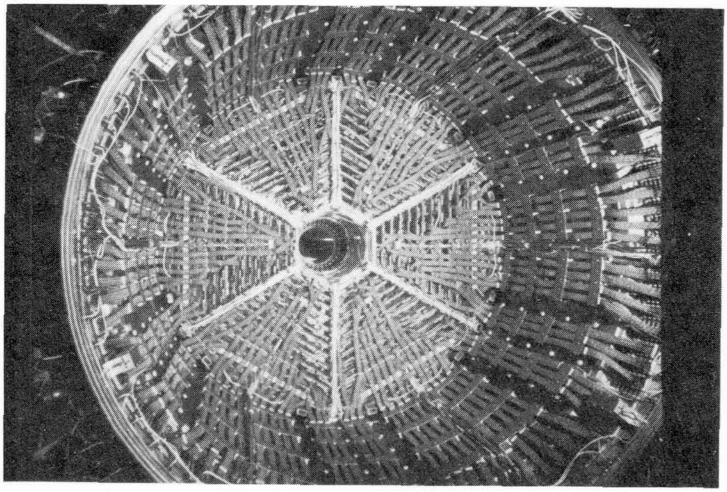

Fig. 7 : Inside of one piece of the Time Projection Chamber at SLAC (1982)

Figure 7 shows the inside of one piece of a new detector, the Time Projection Chamber, just going into operation at SLAC. The main detector performs a pulse height analysis of signals collected in 16,000 different data channels each of which is subdivided into two hundred time bins covering a period of ten microseconds. It is essential that the information that is provided by the elements of the total detector be processed immediately in order to screen events and then store them for later more complete analysis. That involves a prodigious computer programming job.

By 1960 a number of such specialized experts in the various techniques were required by a given experiment. So a whole new sociology of research developed. Scientific papers which used to be signed by one or two people frequently carried the names of dozens of authors. Today, 100 or even 200 authors loom as a common occurrence. No one believes any longer that each such author has a fundamental responsibility for the whole experiment. Names are included in authorship as a means of providing recognition and reward to all people who contribute in an important way to an experiment.

Complicated problems about publication of results frequently arise. Particularly as collaborations have begun to encompass scientists from different institutions and from different nations, questions have arisen concerning the readiness of one subgroup to publish a certain result while another subgroup may not believe that the result is well enough established for publication.

As little science became big science, competition frequently became exceedingly sharp. The CERN PS and the Brookhaven AGS had almost identical capabilities. The same has been true of the Fermilab accelerator and the CERN SPS. It is also the case for PETRA at DESY and PEP at SLAC. With these redundant facilities came certain advantages. They simultaneously accommodated explorations of a given physics problem through the use of entirely different approaches and techniques. Thus truly independent checks and measurements were provided, and this frequently led to clarification of some issue or to the avoidance of what otherwise could have been misleading confusion.

Only recently have we reached the point where identical facilities are not likely to be built at two different laboratories - even in two different nations. No one proposes to build an analogue to CERN's LEP anywhere else, and no one is designing an Energy Doubler similar to the one under construction at Fermilab.

In the era of redundant facilities, along with real advantages came an increase in pace. Competition became severe, and the rush toward publication became intense. The well-established scientific method of publishing complete articles describing equipment and method as well as results gave way to shorter articles, "letters to the editor" which in their turn gave way to pre-prints as a principal method of communication and of staking out claims. And finally preprints even gave way to talks at conferences as a principal means of initial "publication" of results.

That reminds me of a practice that dates back as far as Galileo. He is reported to have published one result of his work in code in order to establish primacy in addressing that problem while still keeping secret a result about which he was not yet certain. Sometimes some of the articles that I try to read seem also to be in code, but, in fact, I don't think we are yet commonly using that technique.

One problem that has developed in the U.S. with the advent of user-oriented national laboratories may be very serious in the future. Whereas the user group concept has kept experimental high energy physics in the universities, accelerator physics has largely left the universities. With the decrease in the number of accelerators and with their absence from university campuses, students have generally not been attracted to the problems and techniques of accelerator physics. This has left a hole in ranks of U.S. high energy physics.

One may ask whether big science still leaves it possible for a young student to get a "proper" education as a scientist. Is he not likely simply to become a technician with one or another speciality? An article which recently appeared in Science Magazine raised this question in connection with the LEP accelerator being built at CERN. "How well will the huge experimental collaboration work? .... One can conclude that each LEP detector will weigh 2,500 tons or more, cost $30 million, and be built by a group of 200 or more physicists. .... And there are many questions. How do you train students to be physicists in such a large group where specialization reaches an extreme? During the years-long construction period physicists will have few or no publications on the subject on which their careers depend. Finally, an old question, but one exacerbated by the complexity of the new detectors is, who is to run and maintain the instrument once it is built? The natural tendency, already in evidence, is for collaboration members to retreat to their home laboratories for more or less independent data analysis. .... One possibility is that elementary particle accelerators have reached their natural limit and that the era of ever larger machines is drawing to a close."

Indeed, one may quite properly be concerned about all of these questions.

However, it is my belief that what we are seeing is simply an evolution, not a revolution. Particle physics is no longer done in the same way as it was done 50 years ago. The problems are different, the technologies are more sophisticated, and the means for meeting those problems and for providing those technologies have had to be discovered and improvised. Graduate students still gravitate toward one or another activity, depending upon their own interests and abilities. Certainly there are some graduate students who get swallowed up in the computer intricacies of an experiment and never really become exposed to other facets of the enterprise. But the world of high energy physics research has become a world in which such people are needed just as much as senior members of any group. Thus the graduate training that such a student receives will serve well for the new kind of career which has been created for the new way of doing research.

Finally I should like to call attention to one tough problem which is just now becoming sharply apparent. Not only have high energy physics facilities become so large that they are many fewer in number, but also the individual experimental detectors are so large and expensive that a much smaller number can be mounted at any given facility. Furthermore the large fixed-target accelerators of the past provided numerous external beams and even more numerous stations at which there could be independent experimental activity. As colliding beams become the experimental technique of the future the number of target locations will become severely limited. It is true that the new, mammoth detectors that are being planned and built for the sharply limited number of intersecting beam regions have a complexity which requires more participating scientists than ever before. Yet that number should be established by need and not by sociology or the physics will suffer. It will be interesting to see how the world of high energy physics adjusts to this new phase of its evolution.

COMMENTS AFTER THE ROUND TABLE

F. CERULUS.- How did N. Bohr, in a small country without special tradition in physics, manage to surround himself with a large group of collaborators ? How many research positions did he have and who funded them ? How was all the travelling among the different institutes financed ?

V.F. WEISSKOPF.- That is a very interesting question. First of all, let me answer the last question, namely the travel expenses. The idea that you get your travel paid is a very new idea. At that time, I remember myself that I had to go to my father to get some money to go from Zürich to Copenhagen. That's number one.

Number two, it is not quite true that Denmark did not have a scientific tradition. Indeed think of Oersted, which was quite a long time ago ; but there was a scientific tradition. It did not come out of nothing. However, the tremendous strength of the personality of Niels Bohr cannot be over-estimated ; he not only had the strength of the personality, but he was also a very good money getter, two things which not always go together. For example, he got a lot of money from the Rockfeller fondation in the early twenties, and that money helped him to build the famous Institute of Theoretical Physics, the Copenhagen Institute, and also to pay for visitors and for permanent and non permanent jobs. Later on of course, he got support from the Danish government, when the Danish government saw that this was an asset. Let's not forget the important support of Copenhagen physics, an unusual source, the Carlsberg beer bewery. This again speaks for Niels Bohr tremendous talent to convince people to spend their money. Indeed, the money from Carlsberg beer was used for a very important purpose at that time, namely to support many refugee physicists from Hitler's Germany and Austria. I was one of them. Bohr used his connections in many countries, and went several times to America in order to provide jobs for those refugees at universities in America and in England.

He did this most successfully ; most of these people got jobs through Bohr somewhere in western universities ; I, myself, is an example. Bohr's success as an administrator and manager was due to his tremendous enthousiasm. He was a personality who could impress people.

G. von DARDEL.- I feel that a mention of the East-West collaboration in the first place between CERN and DUBNA, is very appropriate at this session since it was started by Professor Weisskopf and pursued very vigorously by the late Bernard Gregory.

V.F. WEISSKOPF.- You mean the collaboration with the soviets. This is a great problem to which probably Goldwasser and other people have something to say too. All I can say is this. When I was Director of CERN from 1960 to 1965, we tried to extend the collaboration at CERN, beyond the 12 member states. Among other things, we introduced observer states like Poland and Turkey. In particular the collaboration with Poland was extremely useful and lead to the discovery of the double-hypernuclei by Danysz and Pniewsky. Also at that time we started to have a contact with the soviet government to have some collaboration with Dubna and later with Serpukhov. At the beginning everything went quite well, but as you know, negociations with the soviets take time and effort and have some difficulties. At the beginning, especially in the sixties, it was not too bad, and it really began to be effective under Gregory. At that time about 12 to 20 russian physicists were working in different groups at CERN which contributed apparatus to Serpukhov. For example, an RF separator was constructed at CERN and used at Serpukhov. Later on things turned out to become difficult, it was hard to get people for a longer time and often the persons we asked to come were not the ones who were then sent. So we had our ups and downs. Obviously, to day the situation is somewhat critical because of the deterioration of the East-West relations.