

interesting ones of course are relatively short-lived, so that you have to make them on the premises. Nowadays they are shipped by air and distributed so effectively that the necessity for manufacture at home has become much less of a problem, and the relatively low level of energy needed to manufacture some of the very common ones has now become so universally available around any reasonable institution that this problem has disappeared.

Some of these experimental problems that were problems of equipment, that we were concerned with in 1933, 1934, 1935, 1936, 1937, 1938, have in a certain sense disappeared by now. But this of course is not defeat, this is victory. This is just exactly what should have happened, and this money was not only not wasted, this was money which made the victorious advance to a much more effective present situation possible.

Q: Have we gone as far as 1938 yet?

Weaver: We've just about come to 1938. And that was the year, wasn't it, of the first grant from the RF to Ernest O. Lawrence. This grant, which was \$30,000, followed by \$50,000 the next year, made it possible for Ernest Lawrence to build what seemed at that time a large cyclotron. He had built his earliest cyclotron with assistance from the Research

Corporation, and they should receive the greatest credit for having recognized the importance of giving him assistance at a moment which was critical in his development and in the development of this great instrument. We came in a little after that, when the financial necessity had moved at least a decimal point, possibly even two decimal points, off of the books of the Research Corporation.

But this cyclotron, with a 60" poleface, was only under construction before Earnest, with his clear vision, saw the necessity for a much larger one. He began to talk to me about his plans for a 184 inch cyclotron which, instead of 16 million volt deuterons, would be in the 100-300 million volt range.

The grant for the "giant" cyclotron (for we thought of it, in those days, as being a giant) was \$1,150,000; and it was made in 1940.

I think that this was one of the first important large projects that I had to present to the Board of Trustees, and I remember working so hard on the problem of exposition of this instrument to the lay board. I remember being so deeply convinced that I had to make that lay board get some sense of the excitement that was inherent in the project. And on the other hand, I was equally deeply convinced that I was not going to hold out any cheap promise of practical applications in this or that direction. This was put up to the board as a pure intellectual adventure. Like most pure intellectual

adventures, it turns out eventually to be something more than that, but it was put up to them in those terms.

I was also convinced that I had to say all this to them in a way that would make them really understand, and I remember working very hard on that presentation, and I think that it must have been at least partially successful, because within the last year or two, I have run into more than one trustee who said, "By the way, I still remember your presentation of Lawrence's cyclotron."

We had no sooner made the grant -- or almost no sooner made the grant -- than it was clear that we had a bear by the tail. You see, when the Rockefeller Foundation (and the critical decision on that was of course before my time) decided to finance the great 200 inch telescope on Mt. Palomar, the assumption was almost explicitly that this was the great telescope, and the THE could be put in capital letters: it was extremely unlikely that you'd have to reset the type to lower case at any later time.

It then seemed extremely unlikely that anybody would ever build a larger telescope -- mind you, I do not say a greater telescope -- a larger telescope than the 200 inch. And rather curiously, I think this is now much more certain than it was then. It seems to me absolutely inconceivable now that anybody would ever build a telescope in which there was a light-gathering mirror of diameter more than 200 inches.

We have, in fact, developments in modern photo-electronics for the multiplication of energy which are so beautifully powerful that the way to get a better image out of the sky is not to put a bigger and bigger mirror, but is to put more and more effective electronics onto the signal that you've got. So I think it is without any question true that the 200 inch telescope will remain upper case THE.

The joke of it is that when we financed the 184 inch cyclotron, I think that we had -- or in any event, I think that I had -- the notion that that was probably the biggest cyclotron that was ever going to be built. And nothing could turn out to be more humorously inaccurate than that has turned out to be. I realized that perfectly well.

Shortly after we made the grant on the 60 inch, I went out to see Ernest Lawrence; and he told me about how it was coming along. But that evening, before we had dinner, he asked me if I wouldn't like to ride up with him, higher on the hill above his home, to watch the lights as they began to prick out in that beautiful Bay region. And as we sat there in his car, I was at first surprised and almost officially horrified to realize that Ernest didn't want to talk at all about the 60 inch cyclotron that they were then engaged in building. He wanted to talk about the next one, which they hadn't of course started to build. His mind had leaped completely away from what was going on in the shop,

going on on the drawing board, and had advanced to the next one. And that was the first conversation I held with him about the 184 inch.

The 184" was just well under way when it became clear that the world was moving under our feet very rapidly indeed. And presently I had a telephone call from California, from Ernest, and he asked me if I could possibly come to Washington and meet him there. He said, "I cannot tell you over the telephone the topic which I wish to discuss with you, but I have received authorization from the highest levels to talk to you about a certain topic. I have gotten the necessary clearance for you, and I want to meet you in Washington. I would like to meet you in some very neutral place," and he suggested -- and this is actually the fact, although it sounds a little like E. Phillips Oppenheim-- the hall, not an office, the hall in the Carnegie Institution in Washington.

I did go to Washington, and I did meet Ernest in the hall. But before he could open the topic with me, I told him that I didn't want him to tell me. I did not want the clearance which he had obtained, and I did not want him to discuss the subject with me. There were two reasons why I didn't want this to happen. One was, I didn't want clearance at that moment because clearance carried with it

a very heavy responsibility for silence, and as long as I didn't have the clearance, I was under no restrictions as to silence, and I wanted to maintain that position as long as I could. Secondly, I told Ernest that he didn't have to tell me what the topic was because I knew perfectly well what it had to be.

He had indicated that they were working on a 12 hour day building the great magnet for this cyclotron, and he had indicated the practical problem was to get an additional \$60,000 so that they could work on a 24 hour day, and finish that big magnet just as rapidly as it could possibly be finished. I said to Ernest that he didn't have to tell me why it was necessary to have that big magnet just as fast as it could possibly be produced, because although that magnet was primarily designed to furnish the field which would cause the particles to go in a nearly circular merry-go-round as they were speeded up within the cyclotron, it was also perfectly clear that that magnet could be used as the magnet for a mass spectrograph -- that is to say, it could perfectly well be used to separate out isotopes of different materials. And anybody who knew anything about atomic physics, to say nothing of a little smattering of nuclear physics, knew perfectly well by that moment that if anybody could produce anywhere the merest pinch of U-235, that this was terribly

important. There were strong rumors that the diffusion method which Dr. Urey and some of his colleagues had suggested was going to be used to produce some U-235. But it was also obvious that the first critically important little pinch of it could be produced sooner by a large mass spectroscopy, if you only had a large enough magnet.

So I said: "Please don't tell me why you want it, because I know why you want it, and if you don't tell me, then I will be in possession of no secret information and I will be under no handicaps. I think it's quite clear that you've got to get the \$60,000."

It is a rather poetic fact that the critical dimension of the then biggest cyclotron and the critical dimension of the biggest telescope were not too far different -- the 200 inch telescope and the 184 inch cyclotron.

The interesting sequel of this Washington meeting with Lawrence is that I came back to New York and went to see Ray Fosdick, ~~then~~our president, and told him what was afoot. You see, I could tell him what was afoot because nobody had told me in any official or confidential or secret terms what was afoot. So I told him that Ernest Lawrence wanted this emergency \$60,000, and I told Mr. Fosdick that I thought that some way or other we just had to give it to him. We were, however, up against the extremely curious circumstance that we couldn't possibly explain why.

Mr. Fosdick agreed with me, (a) that we had to do it and (b) that we couldn't tell anybody why, not even including our own trustees. So the fact is that at the next meeting of the executive committee, Mr. Fosdick stated to the executive board of trustees that upon my recommendation he was asking them to approve a grant of \$60,000 to the University of California for a purpose which unfortunately he could not reveal; that the moment would arrive in the future when he would be glad to explain to them why he was asking this confidence of them; that it was impossible to explain what it was needed for, and he was simply making the flat statement that I had recommended it, that he approved of the recommendation, and that he asked the trustees to vote. The trustees shrugged their shoulders a little bit and said, "Well, this is not only unusual, this is unique. It never happened before. But of course we trust you." And they voted \$60,000 to the University of California.

The files of the Rockefeller Foundation contain a letter and my personal files contain a personal letter from Ernest Lawrence, written after the war, in which he says in effect: "If the trustees of the Rockefeller Foundation were never informed what was done with that \$60,000 and why, let me tell you -- let me tell you how many days this gained, let me tell you how critical it was in getting enough U-235 for our first experiments. Let me tell you how absolutely

critical this was in the development of the atomic bomb."

Now, from there on I suppose this story goes down one of two roads. One can look back and say, "Well, was this a good idea? Was it a good idea that the atomic bomb was developed?"

I don't think that is the question. I don't think that is one of the choices between two existing alternatives. I don't think that the alternative was to develop the atomic bomb or not to develop the atomic bomb. If that had ever faced the world as an alternative, I would myself choose the alternative of not to do it; but I'm perfectly certain that that never was the alternative. The alternative was simply shall we do it now, or shall somebody else do it later? Or, indeed, shall we do it now, as rapidly as we can, or shall we run the risk that somebody else is not only doing it now but is already ahead of us?

The answer to this, I think, was absolutely inevitable. It seems to me a complete contradiction of the whole intellectual history of the race to suppose this thing would not happen. It had to happen. So it was only a question of, under what auspices should it happen, and what should the timing be?

If it was a question of auspices and timing, then I think the only decision that could be made was the decision that was made. So I remain glad that we helped.

This has been debated, as a matter of fact, not around the conference table, but, let me say, over cocktails, by the trustees of the Rockefeller Foundation on numerous occasions. Well, we helped; but are we glad we helped? Again, I have always tried to introduce into the conversation the caution, that the question is not really are we glad we helped; the question is, what were the accessible alternatives, confronting us? And did we, among those accessible alternatives, choose the right one?

Personally I think this is the only kind of moral question that can arise, since I do not believe in any absolute code of morals, any more than I believe in any absolute way of measuring velocity. I think that the only proper moral question at any moment is: among the accessible alternatives, which is the better or, if there are more than two, the best?

I do think that one has to use a very great deal of concern that he is being honest about exposing all of the alternatives. I think one has to be very careful that he isn't choosing the easier of two alternatives simply because he hasn't had the courage to face a third one that he prefers not to put up on the table where it can be seen. I think one has to face all the alternatives. I think one has to use all of the skill and judgment and perception and conscience that he has, in locating alternatives and in judging them. But having them up, I think it means nothing to say, "Is

this the best of all possible things to do?"

The best of all possible things to do is, as far as I can discover, not often accessible on this planet. Maybe on other planets, or in other reincarnations, but I have seldom been faced by this opportunity myself. And I think that of the accessible alternatives, we did the right thing.

In any event, that is the story of the Rockefeller Foundation's association with the development of the atomic bomb.