the CAT *and the* CREAM

The following is the text of an invited address presented at the banquet of a conference on superconductivity which was organized by IBM and held last June at the Thomas J. Watson Research Center, IBM's new computer laboratory in Yorktown, N. Y.

By ***A. B. Pippard***

THE thoughts that I am going to lay before you this evening are, I am afraid, serious thoughts, but they are not in any sense the distilled wisdom of much anxious pondering. They are more like random jottings all related to one central theme, and I hope you will take them in this spirit and forgive me if some of them seem wrong-headed and none of them profound. I believe that, hidden in what I have to say, there is a message, but what this message is I am not very sure. Perhaps you can see it more clearly. It has something to do with the idea of Progress.

Right in the van of progress, as everyone knows, or ought to know, comes the Cavendish Laboratory. After all, didn't Rutherford first split the atom there? The answer to that is No; he did it in Manchester shortly before he was appointed Cavendish Professor. But this doesn't alter the fact that in the eyes of the older English physicists and schoolmasters Cambridge is the home of nuclear physics. And it isn't only in England that this view prevails; as recently as 1955 I was informed over here that, since Rutherford, the Cavendish has become of little significance. Apparently it counts for nothing that after the war it was impossible for any successor either to match Rutherford's imperious command of his department or to build a team of nuclear physicists who could maintain the prestige of Cambridge in the world, and that what was achieved was a less costly development in radio astronomy, metal physics, molecular biology, which, if they haven't the popular appeal of expensive fundamental research, do in their modest way allow scope for originality. But I'm not setting out to defend the Cavendish Professors since Rutherford—they can look after themselves; I only want to point out what you must know already, that great men are the very devil. If there is one man who is more likely than any other to wreck English physics today, that man is Rutherford, a quarter-century dead.

Probably not now, in the simple overt way I have mentioned, by encouraging an overestimate of nuclear physics (with luck we've got past that), but in another, more subtle way, and one which doesn't affect only English physics. For I think we can see in Rutherford a symbol of the golden age of physics to which we of the silver age are tempted to look back with nostalgia and which we take as an ideal in our secret thoughts. It must indeed have been exciting to work in Cambridge or Copenhagen in those days when, one feels now, every experiment might reveal a new phenomenon, every calculation dispel an old mystery. Exciting, that is, if one was good enough to do it, and not worried by where the bread was to come from if one failed to obtain a University appointment. For, make no bones about it, plenty of us, if we had thought of becoming physicists before the war, would be happy to have finished up teaching at a good high school (I'm talking about England now—I don't know how it was over here, probably even worse). I had the fortune myself to be taught science from the age of 15 by a group of

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Photo by O. R. Frisch

teachers you simply would not find in a school nowadays—they would all have their PhD's and be doing solid-state research in industrial laboratories.

Let us recognize that those great days were the days of pure research in physics, when physics was an academic pursuit like zoology or history, only a good deal more interesting because a major breakthrough had occurred. And let us acknowledge that the position has changed enormously for two reasons: first, of course, because of the public demand for physicists, and second, because the era of the great breakthrough is over. These two are not disconnected, but before I come to their connection I want to defend my second statement. I have found that when I suggest to senior physicists that the end of physics as we know it is in sight, they tell me, "That's just what everybody was saying in 1900". Now this may be a justification for optimism, but let's first ask whether the historical parallel is sound. I think in many ways it is.

In 1900 one could point to a vast territory of classical science in which no major development could be expected, only consolidation, and that with great pains. One has only to look at Rayleigh's *Sound* to see that many branches of physics were virtually complete from a conceptual point of view. So nowadays we can see how the applications of quantum mechanics have been taken to the point where further spectacular progress demands something nearer genius than talent. But just as in 1900 there must have been a majority who realized that there were whole tracts of physics hardly explored and certainly unexplained, so now it doesn't take much imagination to see that we still preserve a considerable ignorance on many topics, notably in the field of fundamental particles.

All this is a commonplace, but I want to think a little further along these lines to the point where, as I see it, the parallel either fails or by its success gives us warning of what may be in store. I wouldn't for a moment suggest that research in physics is a dying craft. For all I know there may be discoveries being made now or new ideas adumbrated that will prove to be the starting-point for a new breakthrough in fundamental theory. If this is not so, I think physics research will indeed grind to a halt within a few years, say fifteen or twenty, just as I believe classical physics would have come to an end now if the atom had remained intractable, although not through lack of problems. There are still men—rather few—who can exercise great skill and imagination on the classical problems. The end would have come because there would have been no incentive for the merely intelligent to enter the field. Genius can do what it likes, but the average first-class man isn't going to waste his powers on tidying up a little corner of a science whose basic principles are well established. How many first-class men go in for heavy electrical engineering nowadays? Yet in the days of Kelvin there was scope for talent and rewards both intellectual and financial.

Still, let us grant that new principles will continue to be discovered and the parallel with 1900 is sound in the sense that there is no reason to suppose we have discovered virtually all that we are capable of discovering.

GRANTED that, where does it leave *us?* I don't mean physicists in general. I mean you and me, who are not mixed up in the fundamental-particle business. Because, you see, we are the classical physicists of our day; it is our physics which now presents an orderly structure, with a great deal of detail to be filled in, but with no reasonable chance of being overthrown by any later discovery. If you don't believe me, ask yourselves this question: Apart from the field of fundamental particles, what is the most recent discovery in physics that still remains in essence a mystery?

I'm not going to tell you the answer, though I think I might remark that in low-temperature physics the disappearance of liquid helium, superconductivity, and magneto-resistance from the list of major unsolved problems has left this branch of research looking pretty sick from the point of view of any young innocent who thinks he's going to break new ground. I hope you'll see from this that I don't mean everything is worked out—this conference is enough to show that it isn't—but with the new IBM Laboratory, and all those other labs that we represent, plugging along assiduously doing research, ten years is going to see the end of our games as pure physicists, though not as technologists. And of course, it isn't just the low-temperature field that is like this. If you divide all physics into what is potentially of commercial importance and what isn't, there isn't much you'll find in the second category. And as for the first category: what problem is going to stand the hard pounding that industrial research organizations are prepared to give to anything that looks like yielding cash or credit? The last generation of settlers in the new land of Physics found it green and fertile; *we* shall leave it a dustbowl.

Is this too harsh? I don't think so. Just look at some typical patterns of experimental discovery today: for example, the discovery made by the theoretician (the transistor, the maser), or the genuine happy chance (the Mössbauer effect, magneto-acoustic oscillations), which are fitted into their appropriate theoretical niche almost as soon as they are made. I don't need to stress again the lack of mystery that now surrounds discovery, but these examples reveal another phenomenon: the hordes of eager workers who rush in to tear the guts out of a problem. It occurs not only in this country; every year sees new countries gaining independence, political, economic, and intellectual (by intellectual independence I mean the right to show one's modernity by establishing research centers in pure physics). Of course I'm exaggerating, but the rise of Japan as a major scientific power is enough to show us the magnitude of the task which has fallen unsolicited to us, to mankind as a whole, and to which I think there is no historical parallel—the task of feeding the hungry minds.

Now don't imagine that I am complaining. I don't want you to think that I am criticizing the great research laboratories for not leaving the academics alone to plod on at their own happy pace. No, indeed. I view the situation as a simple fact of experience, an evolutionary process that it would be as futile to criticize as it would be to speak ill of a dinosaur. As an ivory-tower academic I ought, if I adopted any moral attitude at all, to welcome the enormous increase of knowledge which I have seen come to light even in my short life of active research. But as an academic I am not permitted, like many of you, to spend my whole time in research. I have to teach, and it is as a teacher that I speak of the troubles ahead. I don't mean the problem of keeping abreast of the present research position; that's bad enough, but, if it can't be solved, at any rate one can come to terms with it after the manner of the bishop and his theological doubts, "I look them firmly in the face and pass on."

The real problem lies in the attitude which has grown up, particularly in this country, that a PhD degree is an essential step on the road to success in scientific work. If, as a University teacher, I am responsible for turning out from my modest factory a steady stream of young men who have been trained in the methods of research by what is probably the only sound way, doing a research problem themselves, then I must provide a corresponding stream of research problems. It is not enough that a research student should learn to operate an existing apparatus and then use it to amass a sufficient bulk of experimental data of questionable value to get his PhD. That is good enough for the lower-class intelligence, but a first-rate man deserves to get intellectual satisfaction and to be taught how to deal with genuinely novel problems; if he cannot get this satisfaction from me, he will encourage his younger friends to seek their fortunes elsewhere. If, on the other hand, I am able to suggest a good topic to him and he makes something of it and joins a research laboratory, there is quite a high probability that he will want to extend his work, rather than branch out into a wholly fresh field. After all, who can blame him when there are more experienced workers around him racking their brains to think up something original and worthwhile? But this makes my task more difficult, for I cannot now put a new research student on to tidying up the loose ends of the problem. There is something of a vicious circle here. In order to get a good start in a research laboratory a man must have successfully solved an academic problem in more or less pure physics. He is encouraged by this to believe, in his heart, that romantic notion which is a legacy of the golden age, that pure research is more noble than applied research. Moreover, his research laboratory, if it is liberal-minded, will encourage that belief and allow him to do just what he fancies, knowing that this policy will pay dividends in the long run; and his University professor will suggest to him that he try to get a job with A, who have a splendidly equipped new lab where he can get on uninterruptedly with *real research*, rather than with B, who expect quick returns for their capital outlay and will make him solve ad hoc problems which have arisen in the course of their commercial activities. Well, of course he goes to A, where his work is so highly regarded that he becomes an asset to the firm, and he and his colleagues not only encourage the view that the men who are really needed are the bright new PhD's, but they are able by their reputations to attract such men. All right, so long as it continues, but it can't go on for ever! There is a limited amount of cake and it's getting eaten up good and fast.

WELL, what will happen when the cake is gone? Here I begin to wander even further from firm ground, but my guess is this: Rather suddenly there will be a reaction among the most intelligent of the young away from our sort of physics as a career. They will still go for those branches which offer an intellectual challenge of the sort they feel is worth taking up—fundamental particles, plasmas, biophysics, cosmology—your list is as good as mine. But we in our universities and research organizations will see a drying-up of the stream; second-class men will be appointed to positions that need a first-class mind. And, as we get older and less able to inspire new approaches, we shall look in vain for younger men to take on the task. I'm not now talking just about academic physics; that will suffer first, perhaps is suffering already, but it will not be long before senility spreads to the applied sciences that are dependent on our efforts. If you have watched a research laboratory going senile you will appreciate that it is as difficult a process to retard in a laboratory as in a man. This is a fascinating pathological study in itself, but I cannot enter into it now.

Let me rather turn back to 1900 to gather up a few loose ends. What is the historical parallel between then and now, between them and us? I suggest that there are two. The first is a beacon of hope for the mind—there is no reason to believe that the questing spirit has reached its goal. The direction of the pursuit may change but the chase goes on. The second is more disquieting. We are, as I said before, the classical physicists of our age. And if 1900 was no deathbed of science, it may be considered to have been near mortal to classical physics, and that means to engineering. The abandonment of classical physics by all but a few of the most inspired has left us with a technology that is dominated by the rule of thumb, by the grotesquely exaggerated factor of safety. There have, to be sure, been great engineers in the last 60 years but not enough to maintain the momentum generated in the nineteenth century. The vigor that drove the railways across our countries has probably never been equalled since, even in the most spectacular of the more recent technologies like aeronautics. And, in some other less dramatic branches of the engineer's art, it is only the ability to repeat the triumphs of two generations back that reveals the presence of life at all.

What is it that impels a first-class intellect into technology? This is a big question on which I can offer only rather general and naïve views. One powerful motive is the desire for the spectacular or dramatic, as exemplified by aeronautics, large civil constructions, or the practice of law; even more compelling is the moral impulse that leads to medicine or priesthood. These can dominate a man's outlook, but if a technology possesses neither then it must either have an intellectual content of its own or perish. As far as we here are concerned, I think we can forget the moral impulse, and frankly I don't think we can offer much in the way of spectacle or drama, nothing to compete with space research. It is the intellectual challenge of the new solid-state technology that is going to keep it lively, if at all. At present I have no doubts about the scope for development of transistors, masers, solid-state memory devices and a host of other things, and what I hope is that my Cassandra-like warnings will stimulate some of you to think about how to conserve the vitality of this still young organization, so that in twenty years' time when you are all research directors you will be able to relax comfortably like research directors do today and know that the younger ones will be able to get on with the job.

I haven't much constructive advice to offer. I think I know what is needed, and that is a swing of emphasis now away from pure research to applications—fewer PhD's, in fact. If more first-class men left the university after completing their course work and were taught the methods of research in industrial laboratories (and I mean properly taught, not just left to find out for themselves), if they learnt their lessons in these surroundings, with the goal of their research not knowledge but useful devices, they would in general be better educated and happier people. And at the same time there would be many university teachers who would thankfully halve the number of their research students and give more time to genuine teaching and less to artificial research. But then we've got to educate the university authorities into judging a teacher on something more than the mass of his published papers. Oh, it's not an easy task because we all know that pure research at its best is one of the loftiest of human activities and "we needs must love the highest when we see it"; but how can one stop the cat from drowning itself in cream? I know one answer to that question but I dare not tell you. Can *you* find an answer that is consistent with your cherished traditions?

NOTE: The balance of responsibility in a university between teaching and research is a very subtle matter, and I should not wish anyone to infer from my last paragraph that I have a simple formula for resolving the tensions that arise from this problem. Certainly I have no patience with the view that there is only one primary duty of a university and that is to teach; if there is any primary duty it is to conduct all its activities, including teaching and research, at the highest attainable level. If I knew how to establish university departments of applied physics which could give as good a training in technological research as could be achieved by the leading industrial laboratories, I should do my utmost to see such departments established. But this would mean that they would have to be in the forefront of technological advance, and at present I cannot see how it is to be done; I hope there are others who can.—A. B. P.