SCIENCE IN THE USSR TO-DAY

Transcript of the lecture given by PROFESSOR J. D. BERNAL, F.R.S., on Saturday, October 15th, 1949, at the Beaver Hall, London, Mr. J. G. Crowther in the Chair, for the S.C.R. Science Section.

PROFESSOR BERNAL : Ladies and Gentlemen—I am not going to give you —I could not possibly give you—an account of science in the Soviet Union. That would require the residence of a very large number of scientists for a very long period in the Soviet Union. All I can do is to give you certain impressions—certain samples of scientific work in the Soviet Union which I was able to witness myself in the very short time that I was there. I can do that with some confidence, though with the full realisation that I am only giving you samples. To get an over-all view, to get the proper weights distributed in the proper fields of activity, to find out which are the priorities and which are the general trends, would go far beyond the opportunities which I had in the Soviet Union. Nevertheless I think these samples are at least illustrative, and can cover some of the questions and still some of the doubts that have been raised about Soviet science in countries outside the Soviet Union.

I had, of course, one minor advantage in this field, in that it was not my first visit to the Soviet Union. My previous visits were a long time ago—in 1931, 1932, and 1934. But science has great continuity, and I was very glad to be able to greet many of the scientists that I knew in those days and to see the continuation of actual pieces of research which were going on at that time. Most of what I saw, however, was entirely new.

I WILL begin more or less chronologically with the examination of my samples. The first and most natural field for me to occupy myself with, is my own specific field of crystallography and crystal structure. I visited the laboratory of Professor Schubnikof. He was already in Moscow when I was previously there, and now he is the head of an independent institute of the Academy—the Institute of Crystallography—which is carrying on fundamental work in crystallography over a very wide field of studies. I should say that I am perhaps starting at the most difficult end, because crystallography is a small subject which is unfamiliar to most people. In fact, most people do not even know what it is, because I get letters about crystal gazing and so forth! We do not look at crystals in that way. The object of crystallography is to find out about the crystal, and not about anything else. What we are really doing is to unravel the patterns which the atoms and molecules make inside the crystals. We interpret the term crystal fairly widely to cover quite irregular substances that we do not normally think of as crystals, such as hair and muscle—in fact, anything that is more or less solid.

Crystals can be investigated by a large number of different methods. The method which I have been concerned with is the X-ray method. But there are a large number of other methods, and I found them all being employed in these laboratories. A great interest has recently arisen in one of the earliest questions of crystallography—the question of how crystals grow. The growing of crystals has now become a matter of scientific and technical importance, because more and more in other fields of science—particularly physics—crystals are required. They are urgently needed in radio; quartz crystals are required for oscillators and quartz and tourmaline crystals for piezo-electric receivers, rochelle salt crystals and other ferroelectrics are required for various types of oscillators and filters. There are not enough natural crystals so artificial ones are used instead, and we must turn to the crystallographers to find out how to grow them. This is where fundamental research comes in. You cannot really learn to grow any particular thing until you know more about the general mechanism of growing: how, in detail, each molecule settles down on a surface; how this crystal grows by adding molecules along certain lines and edges. In fact, the building of a crystal is quite as complicated as the building of a house, only it is done by means of a balance of natural processes which have to be understood before they can be controlled. I saw some very very beautiful work on the fundamental principles of the growing of crystals, and new and simple methods for examining the fine details of their faces.

I also saw a great deal, of course, about my own type of work as well—the analysis of crystal structure by X-rays. That, as Mr. Crowther said, has been a peculiarly British branch of science, and I was glad to see it was starting up in a big way in the Soviet Union. From the examples I could see, it was mostly on mineral structures.

What interested me particularly was the apparatus. Various kinds of cameras—Weissenberg and rotation—were in function, and they were all Soviet made. As I know this kind of apparatus well, I was able to form a fairly shrewd idea of how well made it was. I noticed particularly that the X-ray tubes for this purpose were Soviet made, and this leads me to a curious commentary on the present state of the world. We do not make any such tubes in this country. It is considered that the subject is not important enough to justify the expense. We import them from America, where they are made—we must admit rather badly. However, we could not send any from America or this country to the Soviet Union if we had them, because that is prohibited on the ground that this is electronic apparatus with which, if they had them, the Russians might make an atomic bomb. The question arises whether we should be at liberty to import from the Soviet Union the material we are not allowed to export to it. However, that is just an example of what I noticed in all the laboratories and industrial exhibitions which I saw in the Soviet Union. They have now not only a very fine machine-building industry, but also a very fine instrument-building industry as well, including optical and mechanical instruments and electrical instruments of all kinds; and they are made on a large scale by semi- or complete mass-production methods, and are consequently, I imagine, though I was not able to get any prices, a good deal cheaper than many of ours.

Other work which I saw at the crystal laboratory was new to me—the use of electron diffraction for complete analysis of crystal structure. The full development of this method—and they seem to have made a very good start at it—would have an enormous advantage if it could be done, because with electrons you could examine crystals very much smaller than those which can be examined by X-rays, since electrons have a greater scattering power. In many cases it is very useful to work with a small crystal—you cannot always get big ones. It is one of the things I would like to take up now, as a result of my visit to the Soviet Union. That is enough, I think, for crystallography. I could go on, but I do not want to bore you with technical details on that subject.

THE next laboratories I studied were also laboratories in which I was interested as supplying the material with which I work. As Mr. Crowther said, I have been working recently on proteins. I was very interested to know that work was being done on proteins in the Soviet Union. Here I met another old friend of mine, Professor Talmud, and also Dr. Bressler from Leningrad, and they showed me the most amazing work on proteins that I must say has completely altered my views on a large number of protein problems.

If I can explain it fairly simply, the kinds of protein I deal with are the globular proteins which furnish the mobile parts of cells of animals and plants. The commonest—the first of the proteins, so to speak—is egg white, albumen; but we have the globulins of the blood; we have the myosin of muscles; we have the enzymes and such important substances as insulin. Up to now, proteins have been the most difficult of all chemical substances to study. They are so complicated that most chemical methods break down before they get anywhere near the answer. We have been attacking them in my own laboratory with X-rays and we are still a very long way from the answer. The Soviet scientists' attack on the proteins was on rather different lines. They attacked them as if they were dealing with a complex mechanism held together by various different kinds of links. Some of those links are electric charges, others are electric di-poles like little magnets, which we call hydroxy bonds. Others are like the links between ordinary fatty substances. Now, those different links are susceptible to different kinds of chemicals, and by treating the proteins very delicately, so as not to break them up, by such things as urea and benzene Bressler and Talmud were taking one link at a time. I am quite sure that this physico-chemical method, combined with X-ray and with other analytical methods, will enormously further our study of the proteins. I want to go straight back myself to some of the proteins I have been studying, and with these different reagents see what difference it makes to the X-ray pictures.

More exciting than this analytical approach is their synthetic work. I had heard something of it before I went to the USSR in a vague kind of way. This time I was able to see the work itself, and I think it is one of the major discoveries of this century. Ordinarily, when we eat some meat, let us say, the pepsin in our stomach breaks down the protein into something completely soluble. We cannot digest protein as such. The protein that we eat does not become the protein of our tissues. It is broken down into the smallest pieces capable of going through the walls of the intestines and then of being resynthesised, reassembled into our own private proteins in the cells of our own bodies. We have known for a long time that the breaking-up of the proteins is a biochemical process carried out by special proteins. You have simply to extract the pepsin, called enzymes, from the stomach, and put it into the protein solution. It will break up the glutinous material into a clear solution with smaller units, which will go through the membrane of the digestive system in a way the original protein will not do. Bressler and Talmud have reversed that process. They have taken a protein and broken it down completely, leaving the activity in it—that is important. They have then compressed it to about 10,000 atmospheres, and have got the protein back again. Until they did it the failure to synthesise proteins remained almost the last refuge of the pure ideas of vitalism—that a vital force, or something mysterious, was necessary to put proteins together. It is like all the previous steps in this region of the unknown : simply that people have not tried hard enough, or tried the wrong way. I would not say the protein they have synthesised is identical in all respects with the protein they start off with. It is clearly not. But its mean molecular weight is the same and it has some of the right characteristics and properties, such as the immunological property of producing reactions in animals sensitised to the original protein. I feel that Bressler and Talmud have made a very important step forward in the structure of protein, both on the side of analysis and, as I think far more important, on the side of synthesis. That is just one piece of work out of many that are going on at this biochemical institute.

I had a long talk with Professor Oparin, the head of a growing school of biochemistry, about the origin of life, on which he has written a book, and also with Professor Engelhart, who was responsible for another very big discovery in the protein field—the discovery that the muscle was the substance that activated the adenosine triphosphate-diphosphate reaction which is the main channel of energy transfer in living systems.

Unfortunately, I was unable to see Professor Frumkin, of the Physico-Chemical Institute, though I saw one of his colleagues, Professor Frost. In this field too there have been enormous advances linked up with the general development of the chemical industry in the Soviet Union. I saw some of the results of the work on the chemical industry in the Polytechnical Museum, and some very interesting developments are coming out of it. First of all, there is the idea of the full rational use of chemical resources. In the past people have been content to make use of natural resources which have only to be slightly processed—purified and combined with other things to make something useful, like turning latex into vulcanised rubber. Now, the tendency in chemistry is to work in a more drastic way—to break down the materials practically to the atoms and then to put them together in a controlled way. That has been followed very largely in the Soviet Union, but without any prejudice as to the use of purely artificial synthetic methods. Use is also made of zymogenetic or microbiological methods, such as the use of yeast.

One of the things that interested me very much was the use being made of peat. The peat resources of the Soviet Union are extremely large. They are, I think, some thousands of times larger than the oil resources, which are quite large in themselves. The full utilisation of peat is one of the things which will, I think, make an enormous difference to the general economy of the Union. It has been developed very thoroughly, and they have what are virtually peat combines working through the peat bog and handing out the peat in a suitable dried form for further processing. It is then vacuum-distilled, and some extremely useful waxes are extracted. Some of the residues are used for growing yeast, which can be used for food, and the remainder can be turned by water gas, or a similar process, into a fuel and even synthesised into an oil or petrol. In other words, there is now a totally integrated chemical industry based on peat. The same is done with a large number of other materials.

FROM those two fields of scientific research I would like to go on to spend more time on another, because of the great interest it has aroused inside and outside the Soviet Union. One of the things our delegation particularly wanted to see before leaving the Soviet Union was Lysenko's establishment. The general impression one gets of the Soviet Union is quite different when one is there, even to a person like myself, who has spent a great deal of time in reading about it and studying it, and who has earlier memories of it. The impressions we formed were so different from what we expected, and I think that applied most of all in the case of Lysenko. Quite honestly, I do not think anyone who has not at least had the time we had with Lysenko can make any judgment, not so much on whether he is right or wrong, but—a far more important point—on what it is all about. It is so different from what we are accustomed to in biological science, both intrinsically and in its relationship to practical work, that you really have to see what is going on, to talk to Lysenko, to get the hang of it.

I was lucky, before I met Lysenko, to get hold of a book which you can now buy here, *The Selected Papers of Michurin*. Until I read that book, my knowledge of Michurin was derived from popular journalistic accounts, and from an extremely beautiful film shown by the Soviet Embassy on Michurin's work. From all of these one got the impression that Michurin was a man of great practical genius in the handling of plants and the growing of new varieties, particularly of fruit. He has often been described as a Russian Luther Burbank. Burbank was the great hybridizer and nurseryman who raised many new kinds of fruits in the United States at the end of the nineteenth century.

If you read Michurin's works for yourselves—and perhaps not only those of you who are scientists, but any who happen to be interested in gardens and fruit trees—you will find something quite different. Here is a person who for a great number of years, from 1885 to 1935 (and time is important because fruit trees do not grow quickly—you have to follow them for years to see what they are capable of), carried out his plant hybridisation and grafting. He was a real scientist, that is, in the sense that he studied his material with a view to understanding how it worked in order to make it work in the way he wanted. There is a complete cycle from the material to the understanding and back to the material again. The really effective test of a scientist is whether, as a result of his laws and theories, he can predict and control the nature he is handling.

From his practical experience as well as from planned experiments, Michurin did arrive at a large number of general principles which lie outside the main focus cf interest in the biology of our time. If you study the history of science, you will see that nature, being extremely wide, is nicely parcelled out in the text books and in University courses into different subjects—botany, biology, plant physiology, genetics, and so on. But if you look into it carefully, you will find that only a very small part of a broad field is covered. The rest remains in a kind of stagnant backwater until someone breaks into it and cleans it up. To go back to the physical field, you probably remember at school learning about the laws of friction—that the force needed to move a thing depends not on the load or on the area, but only on something called the co-efficient of friction. That was discovered in 1665 by Amonton, and no new work was done on it until 1927, when Bowden found out what happened when you rubbed one thing against another. Similar things have happened in the biological field. It is fairly clear that Michurin's work was an untilled section, one very largely concerned with plant physiology, including the effects of the different conditions on plant growth—the light, the soil, the effect of one plant on another, and last but not least its hereditary conditions He came upon many general principles, but those general principles were not of a kind that interested the great majority of other biologists. For they were concerned, at least from 1900 onwards, with other very exciting and interesting problems concerned with the mechanism of inheritance and the selection of pure and cross-bred lines.

I bring Michurin in because Lysenko is a Michurinist, though not in the sense I had originally thought. Lysenko first heard of Michurin in 1930, during a discussion—the kind of discussion, I imagine, that goes on all the time in biological circles in most parts of the world, when people are saying, "What absolute nonsense this is! It is completely against all the basic theories of science". While Lysenko listened to this, he found himself sympathising with Michurin and against his colleagues, for though he had been brought up, like everyone else, on the accepted theories, he sensed more contact with nature, as he knew it, in Michurin's views. Then he thought he had better find out something about it; he read Michurin's works and began to apply them to his own field—that of field crops. The point I want to make is that Michurinism is not something that needs personal transmission. It is not a kind of mystery. It is a general approach—an idea of biology which is quite easy to grasp if you have any practical experience of biology—and that, of course, is what Lysenko had. He is the son of a peasant, now a collective farmer, who had an agricultural training as an agronomist—a horticulturist—who has spent his whole life with plants, and who appreciated the intrinsic understanding of plants that was in Michurin's work. I think any of you who are biologists or gardeners would see it at once on reading his papers, and would appreciate it still more in repeating and extending his actual experiments on fruit trees.

I read Michurin's works while eating some of the Michurin apples, and they were very good apples indeed. But it was not so much that they were good apples: we have good apples in this country and in many others. Michurin points out exactly why. The occurrence of good brands of apples is largely accidental; in time, if there are enough people to grow apples, sooner or later a very good apple will be found on a tree self-sown or planted in some wood or orchard, and this will be selected and propagated by gardeners or nurserymen.

Michurin apples are constructed—they are not accidental. He coined the phrase: "We cannot wait for favours from nature—we must snatch them from her." They are produced by a deliberate planned attack on the plant to get definite results, and this is a very definite step in advance in horticultural production. I am not competent—and I do not propose, even in questions—to argue about the purity of the stocks, the existence of viruses, and so forth. All I can say is that as a scientist I was very impressed by the intrinsic quality of Michurin's writings.

NOW I come back to Lysenko himself. Lysenko received the whole deputation. Four of us had some scientific experience, Mr. Crowther, the Dean of Canterbury, and Ivor Montagu, who started off in his extremely versatile career as a scientist, has even a degree in zoology, and I think is the greatest expert on the minor rodents of Eastern Europe. Apart from that we had no particular biological experience, but we were able, for the period of about six-and-a-half hours, to keep at it, question and answer, and demonstration—which is quite a long stretch, especially as poor Lysenko had a bad throat and found it obviously painful to talk. I think, with a knowledge of the genetics controversy beforehand, and especially of particularly disputed questions, we were able to get a fairly accurate general picture of Lysenko's work and of his attitude to orthodox genetics.

We first saw him at the Agricultural Institute, one of the oldest and most charming houses in Moscow. His study, where he received us, looked very much like a country seedsman's back shop. The room was full of seeds, scions of various kinds for grafting, shoots from different kinds of plants, fruit, and all kinds of things. Lysenko is definitely, I think, one of those people who, if he were not in the Soviet Union, would be the darling of the old-fashioned scientists in this country. He is one of the string-and-sealing-wax (or rather grafting-wax) type of scientist. In that sense, I could not help thinking he must have worked very much in the way Darwin worked in his time. There were none of those beautiful streamlined fitments associated with modern scientific offices, calculating machines, card index systems, and all the rest. It was extremely *ad hoc*. Nevertheless, he moved round in this mass of vegetation with complete mastery. He knew exactly what he wanted. He could say, "Fetch that thing out from behind so-and-so", and the man would bring the specimen just as it came up in the argument to illustrate the particular point Lysenko wanted to make.

We had the same impression when we got to his farm. We went out to the farm, which is at Lenskygorod, about fifteen miles away from Moscow, in the depths of the country. It is an old agricultural station—quite a small one. Curiously enough I had been not there, but next door, on a previous visit in 1934. There is nothing massive about it. The large-scale work of Lysenko is done in his field laboratory, which is the thousands of square miles of the Soviet Union. If you want to know something about how this vast laboratory works, I can recommend to you a little booklet which has just come out, called *The People's Academy*. It took me about two hours to read it, and I could not put it down. It is the story of one of those efforts that have been made in recent years in the Soviet to increase agricultural productivity. It is the story of how the yield of a particular grain (millet) was raised from two to eighty—I think it was pounds—per hectare in the course of about five years, by the Lysenko methods. These were developed and applied—and this is the important point—by thousands and tens of thousands of collective farm workers up and down the country, working with the scientists. That is a different kind of scientific tie-up from what we have in this country, and we must expect it to be different in other respects also.

The greenhouses, where we saw the plants, were again quite small, but I must say they made up in quality what they lacked in quantity. We went into one of the strangest greenhouses anyone has ever been in, because there was hardly an ordinary plant in it. Most of the plants were growing quite different things in different parts. For example, when he was discussing the question of graft hybridisation with us in his study we were shown the celebrated tomatoes. These tomatoes were, as the critics have said, wax tomatoes—and, of course, you can prove anything with a wax tomato. But when we went to the greenhouse we saw the actual tomatoes growing on the plants, and they were exactly the same as the wax ones. The demonstration of graft hybridisation was very simple. Two kinds of tomato plants were used—small red, and large yellow. When a young shoot of the yellow was grafted on the red, its fruits were pink, while those of the stock below grew larger and the seeds from these tomatoes gave rise to new plants, which we saw, with a variety of fruits of mixed character.

I do not claim to know what the mechanism is, but I am prepared to say that I have seen the actual plants and other things which were even more startling, and which fit in with this general theory. There is a cabbage there—a very peculiar looking cabbage. The ordinary cabbage has a head, and if you leave it, it will push out a long spike of flowers, and go to seed in the next year. This cabbage plant had a large cabbage head on one side and a spike of flowers growing out of the other side. It was doing two years in one, and it illustrates one of the major principles of the Lysenko-Michurin theory which I did not find so peculiarly unscientific. It seems to me to fit in very closely with the work done in embryology in the animal field.

If you take any organism in an unstable state—which may mean taking it very young—or when any particular part of it, like a bud or a shoot, is growing very rapidly, it is much more susceptible to changes in the environment than it normally is. Normally a plant is pretty stable to its environment. Otherwise we would not have the things that breed true. But according to Lysenko you can, by working on unstable states, not only modify the organism itself, but also definitely affect the seeds of the organisms if the original part worked on contributes to their formation. This is his form of the theory of the inheritance of acquired characters, which was supposed to be disproved once and for all by the old experiments by Weissman. But cutting off the tails of rats generation after generation is not, of course, a proof that you cannot transmit characters. It is simply a proof that that is not the way to do it. By acting on organisms in their unbalanced state, you can get results. Take the case of these cabbages, for instance. They were produced by grafting one-year cabbages on to two-year cabbages. If the graft is young enough it becomes a two-year cabbage straight away and never goes through the first-year stage at all. If, however, it is a little older, it remains a one-year cabbage growing on a two-year stock. Similarly, there are other queer grafts of that sort, such as carrots on parsley (I collected some of these seeds and hope to sow them, though I don't know quite what is going to come out of them). Lysenko showed us himself how these grafts were done. All he needed was a penknife, string, and grafting wax. It was really so simple. Anyone could do it. and there is an enormous amount of fun and games to be got out of it. If you take shoots young enough, you can apparently graft practically anything on to anything else. Michurin shows a picture of a lemon grafted on to a pear tree, for instance. This graft took, but it did not produce a fruit half-way between a lemon and a pear—all it did was to make a pear tree evergreen.

ANOTHER very important side of the work that Lysenko showed us was that on vernalisation, an agrobiological technique that he started himself. I had no idea until I saw some of this, what a precise thing vernalisation was. He showed us diagrams of it in his study, and then the actual plants in the greenhouse. He takes a winter wheat, or any other cereal, and treats the seeds for a certain number of days at low temperatures. If the number of days is less than a certain amount, they will grow into low plants like grass, and will not form ears. For rye, which was the plant he showed us, the vernalisation period was 32 days. With 30 days' treatment there is absolutely nothing: at 35 days, every one of them springs up and forms a true ear. There is a sharp distinction. The important thing is that between 30 and 35 days the plant is in a state of instability, or is "suffering", to use Lysenko's language. It is metaphorical but, I think, quite accurate and descriptive language. The plant cannot quite make up its mind. If it was an animal, we should say it was having a neurosis. It does not know whether it is going to send up a shoot or not, and in these circumstances it is extremely susceptible to external changes. Take it at 32 days, and that material is the right kind of material on which to try particular modifying tests.

Lysenko told us something of the story of what was one of his major achievements—how he made the winter wheat for Northern Siberia. In Northern Siberia the summers are quite hot, but very short—for spring wheat you have to sow at the end of June or July and harvest early in September at the latest. There is a very short growing season and a very poor yield, so it would be ideal to have a winter wheat. Lysenko tried all kinds of special frost-resistant winter wheats, and realised they were no good, so he took the spring wheat and turned it into a winter wheat by sowing it in the autumn—not ploughing the ground, but sowing on the bare stubble. Most of it died, but some grew; by doing that three years running, he was able to produce from the spring wheat a winter wheat that was suitable to the climate, and would give very good yields if sown in ploughed land. The process of open sowing was only necessary for protection from frost. The ground is permanently frozen in these parts, and the real danger was frost coming up from below rather than air frost, from which the seeds are protected by the snow. All that involves a study not just of the genetics of an organism but of the whole complex—agricultural practice and everything else.

That is the real genius of Lysenko's work and the source of the mystery of Lysenko. He is not just an intelligent peasant, or a monk, or anything of that sort—he does not hypnotise the Supreme Soviet into putting him in power. He gets results, and he gets results in a way other people cannot get them because he works quickly. He works on the whole set-up, seed and plant, land and weather, man and machine. For instance, take this millet story. I cannot tell you the whole of it, but the essential point is that if he had waited until he had selectively bred a high-yielding millet he would have had to wait four or five years. But the Soviet Union could not wait, for they needed to increase the yield of millet five times in one year. The first step was simply a matter of finding how to sow the millet. Millet is, or was, a troublesome crop because of the enormous amount of weeding it required. He sowed it very much later, when the ground was warm, and in that way got it up ahead of the weeds. Sowing the millet far apart also helped; he could plough in the weeds between the rows and give the millet a chance. You can actually get a better yield by ploughing in four-fifths of the crop itself than if you let the whole field grow. It is largely by such agrotechnical methods that Lysenko has made his name and reputation.

The reason why all this interests me is partly because I was brought up on a farm and partly because of my war experience, particularly in operational research. The harvest in the Soviet Union is a real operation, on which the lives of more people depend than on the outcome of most battles. Consequently, you have to take not an academic but an operational view of it. You have to consider not whether you can raise the yield by a few per cent; nothing less than 200 per cent is worth thinking about. That is the kind of attitude which can be carried through in agriculture only in the Soviet Union and in the countries which are following her lead.

Among the remaining things I saw which were of great interest were the cattle, particularly the new Kostroma breed, an all-purpose breed for milk and meat. I was brought up on a dairy farm, and have a very shrewd idea that these cattle are extremely good, but Lysenko does not claim that they are better, for instance, than some British breeds of cattle, and he stressed that Professor Hammond's work on breeding, for instance, is exactly on the lines he would recommend in the Soviet Union. He has the greatest admiration for the practical breeders of Britain and for the long tradition of animal breeding from the eighteenth century onwards, because these people do not work with genes and chromosomes but with two things—the general character of the beasts they are trying to get and the kind of feeding and treatment to give the animal in order to get the high milking or high beef yield of the stock.

We asked him what he thought about chromosomes and their functions. "Well," he said, "I do not know everything. I really do not know what the functions of chromosomes are. I suppose we shall find out." But he does not believe the chromosome is some kind of definite permanent pattern which imposes itself on the organism, nor does he believe that when you get a fine breed of cattle, or a fine crop showing characters which never appeared in their wild ancestry, that these characters always did exist. He rejects altogether the picture that God from the beginning of all time had laid down all the characters of animals and plants, or even that they arise by chance mutations, which we cannot control and must select. He does not deny that both these processes do occur, but he considered them relatively unimportant in natural evolution and of secondary importance in agriculture, where he claims there are much more direct ways than relying on selection from chance variations to produce specific improvements.

I THINK I have said enough about Lysenko, and I will come on to my final topic, which is the more general one. One of the most useful interviews we had was with Vavilov, the President of the Academy of Sciences, and about eight other leading Academicians, where we discussed the general situation in Soviet science, the changes that had been carried out as a result of Lysenko's work in the direction of the teaching of biology and genetics in the Soviet Union, and the relations between science in the Soviet Union and this country.

I think I can say this : it is quite clear from everything one sees in the Soviet Union that the scientists there have a feeling that they are a part of a general enterprise. It is extremely difficult for us who live in a society without any common purpose, to realise what it feels like to live and work in a society that has a purpose, and how differently scientists and workers think about such questions as freedom and responsibility. I learned something of it through meeting the same people after fifteen years. When I was in the Soviet Union fifteen years ago, they asked me in Moscow what I thought about science there, and I said I thought science in Moscow was rather like science in Cambridge, and so it was. What surprised me at that time was that they took it as a compliment. Their ideal of science was to have it like it was in Cambridge. Science in the Soviet Union to-day is not like science in Cambridge, and if you think that science like it is in Cambridge is necessarily the only kind or the best kind of science in the world, you will completely fail to understand science in the Soviet Union. It reminds me very much of a discussion I attended in the war at a very high strategical level, when someone asked a very important general whether there were any lessons to be learned from what was happening on the Eastern Front, and he said : "Certainly not! That is a second-class war."

I think that represents pretty fairly the attitude of many of our scientists in this country towards Soviet scientists. I found old colleagues of mine who never grew anything at all, or nothing much more than a tadpole, let us say, pronouncing on the scientific level and barbarian nature of Soviet scientists who have produced vegetation over areas that never had any before, who have doubled and trebled and quadrupled yields, who have transformed old crops into entirely new crops. It may not be science, but we had better find out what it is.

The impression the scientists there gave me was that they knew perfectly well what they were about. They were getting an enormous advantage out of the feeling that their work was, as it were, flowing into the field all the time, that they were getting something out of the field and putting something back. I have been talking about biology and agriculture, but that goes just as much for the physical sciences. What impressed me was the way in which the work of the scientists and engineers, which here gets across in practice slowly or not at all, was done there by scientists and manual workers together, and did get across extremely quickly. And these technical achievements, particularly in the mechanical and electrical engineering of the Soviet Union, are of such an impressive nature and are being so rapidly added to that it is easy to see why the scientists there take a very different view of our criticism from that we should have thought they would take. They do not feel themselves to be slaves of a higher power, to be going round under the dictates of the Kremlin. You cannot run a country by orders, but only by having people who do things very largely on their own initiative, and get permission to do them afterwards. I found that in the war, and I am sure that is exactly what happens in the Soviet Union. These people know where they are going, and they each and all have an interest in getting there fast. They do not need orders, but only the most general directive. It is extremely simple, though I do not suppose it is put in quite these terms: "You can do what you like, but you get hell if you do wrong." That atmosphere has a very different effect on different temperaments. On the positive it produces a most terrific spirit of enterprise; on the cautious and lazy it produces complete paralysis: and that acts as a selective process. The opinion I got of the Soviet scientists—the ones I met—is that they were of the positive kind. Certainly Vavilov is.

On the question of plans, I think Lysenko put it in the shortest form: "You must not think that science or thoughts are planned in this country. The tasks are planned. The thoughts are free. You have certain jobs to do. You can think out how these jobs should be done." That is what the Soviet scientists are doing and, of course, in order to get these problems solved, they are obliged to go into fundamental science. But they are also very determined that their science is going to stand on its own feet, and there you get one of the characteristics of what I might call the modern or post-war Soviet science—the tying-in of this formula with the past of Soviet science.

We are apt, because of our total ignorance, to think that there never was any science in the Soviet Union, except for isolated individuals, before the Revolution—I have even written so in my own books. Well, even the scientists in the Soviet Union in the early years did not know how much there was. They have been finding it out ever since. The work, for instance, of Popov in connection with the development of wireless is, I think, both scientifically and practically better than that of Marconi and was certainly earlier, but it is completely unknown outside. The greatest figure in the eighteenth century in physical science, with one possible exception, was Lomonosov, who was certainly a much more thorough and all-round scientist than even such geniuses as Franklin, and who compares well with the late eighteenth century scientists such as Lavoisier; yet outside the Soviet Union we do not know anything about him. I have read some of his works in preparation for an SCR lecture, but I do not think you will find any of them in the English language at all.

They are learning about their cultural past in the Soviet Union, and they are using it to inspire them to add worthily to its achievement. They have a science which is self-generating, which is self-supporting, which has a steady flow of people coming in from the schools and universities, which can create these new ideas. They are well aware of all the work that is being done outside. Where we go wrong is in not being aware of the work being done there. This is a fact we discuss and we deplore, though not very seriously. One of the reasons for this is obvious. You notice we do not acknowledge a great deal of Soviet work. We do not know anything about Soviet science because we can never read it. It is in Russian, and we cannot read Russian. Vavilov said to me in this connection: "Russian is now one of the major scientific languages in the world, and perhaps it might be a good idea to learn it." I think that is the answer. On the whole, we are going to be the losers, and not they, in the present situation. The output of Soviet scientific work is enormous, and is very rapidly increasing. The Physical Science Section of the Academy is now producing a large volume every ten days, full of meaty papers, and the Americans at least have gone to the trouble of having the whole thing bulk-translated and issued as a periodical for the benefit of their own industry. Sooner or later we will wake up to the fact that Soviet science has arrived. The Soviet Union is a country with a science of its own, and it is not going to bear any dictation from outside; all we do by our denunciations of Soviet works is to work ourselves up into tempers: we shall not have any effect there one way or the other.

FINALLY, the whole of science, as I have said, is tied up with the general development of the country. Science is part of this great movement of new construction. It is no longer reconstruction. Science is something that passionately interests people; it is a thing which is the topic of everyday talk; it is visible in the building, in the new facilities for people's participation, in the great publicity for science in the papers. You get the impression that science is one of the things that people realy mind and care about. This is the major lesson I learnt from my visit to the Soviet Union—a lesson I might not have learnt, I think, without it: that this devotion to construction, this devotion to raising standards of living and raising and creating a new culture, is something which we ought to welcome rather than criticise, because it is the greatest security for our own future. This is the guarantee of the peacefulness of Russia, and if we can persuade the rest of the world to be as peaceful towards Russia we may be able to go forward with them in a common enterprise.

QUESTION TIME

Question.—Could Professor Bernal give any indication of team work existing in the Soviet Union among the scientists?

Answer.—All the work, really, is done by team work, but not of a formal kind. It is very characteristic, I think, of the general Russian way of doing things that there is no formal organisation of more than a very few people. For instance, Lysenko's own research group consists of only five people. There are a number of studies in industry that require work in a large number of different places, and comparison of results between groups and a very large number of discussions take place. But my impression is that formal team work other than semi-voluntary team work is much rarer there than I would have expected.

Question.—What proportion of women are doing scientific work in the Soviet Union?

Answer.—I should say, as in everything else in the Soviet Union, a very large proportion. I cannot give you statistics, but I know in the two or three lectures I gave, about half the audience were women. They were all workers in the field. At the Institutes you may just as well have a woman as a man at the head. They really have abolished the distinctions of the sexes as far as active work is concerned, though in domestic life I think the whole set-up is extremely like it is in this country.

Question.—How do advances in science and progress in other ways reach the schools?

Answer.—I am glad you asked that. I meant to say something about the schools, but I did not have time. I had one opportunity of going to a school and trying to find out directly what was being done there. I went particularly to the science classes, and I found that they were right up-to-date in their science. I have some of the recent science text books, which I took as they were being handed out. It was the first day of term. I noted particularly that they had allusions to and pictures of all the latest physical devices—the electron microscope, for instance, which is a fairly recondite thing. I have two pamphlets on it—one at 90 kopecks and the other at 40 kopecks. I am afraid I cannot tell you what that is worth, but it would be less than 1s. or 6d. You could pick them up anywhere. I kept seeing them in every bookshop and stall, and they are very thorough accounts of their subjects. I noticed in the technical museums a very large number of school children obviously passionately interested, because when we had the guide explaining to us there was a crowd of children listening to what was being said.

The school curriculum gives a high place to science, but not a disproportionate one. I was very interested in the balance between literature, history, and science, which is about equal—one-third of each. There is no doubt that the school system is going to have a most terrific effect, especially now the ten-year school period up to seventeen is universal in the towns and higher education is on such a large scale. At the particular school I saw, I asked how many of the children went on to higher education; I was told it was about 80 per cent., but in the last two years they had all gone on. It will be bound to have an effect on the town population if over 60 per cent. receive higher education up to the age of 21 or 22 —not always full time, but at any rate some kind of higher education. It means an enormously wider popular scientific appreciation.

Question.—Could you throw some light on the study of the history of science in the Soviet Union, and say whether there has been collaboration between the natural scientists and the historians about the study of scientific research, and what is known as general history?

Answer.—We had a talk with Professor Koshtoyantz, the Head of the History of Science Department of the Academy, and discussed this very question. There is an extremely lively interest in the history of science, and science is always taught with reference to its history. We have come away with a large number of books on the history of science, but they are all in Russian, and we still have to extract what is in them. If you are interested, I am sure you could be put in touch with historians of science.

Question.—What happened to those of Lysenko's colleagues who ventured to disagree with him?

Answer.—I can tell you something about the ones referred to in the Decree of the Agricultural Academy, because we asked particularly about them in our interview with Vavilov. They are all working in scientific institutes, most of them in the same subjects as they were working in before. Those who are so definitely opposed to Lysenko that they would not in any circumstances work under a Michurinist general direction, are working in different fields. For instance, Dubinin, who is a kind of leader of the Mendelian scientists, is working on the control of insect pests in connection with the new afforestation plan, but he is working on his own and not under the direction of Lysenko. Most of the others are working in their own fields, many of them as part of the Academy; for instance, Orbeli. I mention Orbeli because he was specially referred to by Sir Henry Dale in his letter of resignation from the Academy. Orbeli is a distinguished physiologist, and is one of those people who happen to have a very large number of jobs. He was head of the Biology Section of the Academy, head of the Physiological Society, head of the Military Medical Academy, and six or seven other things. Now he has lost the first of these jobs, but he continues in his other functions, and has taken a very large part in the recent Pavlov celebrations.

Mr. Crowther.—I think the position is, roughly, that whereas before he had twelve important jobs, he now has eleven.

Question.—How does Lysenko's work at the Agricultural Station compare with the Rothamsted experimental work here? Secondly, is all Russian scientific work fully published, and is it accessible to the whole world of science?

Answer.—I am not really competent to answer your question about Rothamsted. Some of the work, such as that on protection against drought and soil science, is very closely related to the Rothamsted work, but I cannot make any very useful comparison between the two. As to the second point, everything that is published is available, but it is unfortunately available only in Russian. For instance, I can give you a very good example. One of the most disputed points in the Lysenko controversy was the turning of 28-chromosomed wheat (durum) into 42-chromosome wheat (vulgare), and this has been attacked here on the ground that Lysenko must have had some of the vulgare wheat mixed up with his other wheat, and when he sowed it one died and the other lived, and that was how the transformation took place. He gave us a detailed account showing how he had done it. It was not one of his experiments, but was done by one of his workers. The sowing had been done grain by grain, and each individual ear was found to contain some seeds, perhaps only two or three, which were different from the other seeds, and these were the vulgare seeds. I asked why he had not published this, and he said he had published it. It is in a number of his journal *Yarovizatsia*. So far as I know there are no numbers of this journal, in Russian or a translation, covering the period in question, available over here. It is not that the information is not available, but the business of getting the journals here and translating them has not been adequately tackled.

Question.—What chances are there of Lysenko's work being made available here?

Answer.—I think more and more will be translated into English in the Soviet Union, but a proper search would be desirable, and in a more reasonable way. I do not know whether it would be possible to get a jury of impartial scientists: there are probably none. But it might be possible to get a fifty-fifty pro- and anti-group to go through the work. Up to the present the reading of Lysenko's works has been done for the most part by people who are violently anti-Lysenko. I notice in the case of other scientists as well as Lysenko that if you try to put down baldly what a person says without any background, the statements appear quite meaningless. But the man who made them had some purpose in making them, and you have to find out what he meant. That requires more than translation. It is quite a difficult job. The ideal would be to have British biologists working with Lysenko for a year or two, and then coming back to write it all up for us in England, because it is not only a case of English and Russian. The scientific terminology is different, and that is one of the reasons why this enormous amount of misunderstanding has arisen. We just do not know what he has done or what his ideas really are. Although I have read a good many accounts, I did not know half of what the Lysenko case was from anything that has appeared in this country.

Question.—Do you suppose Lysenko knows himself how he gets his results? Both what his opponents have said and what you have said give the same impression to me: he is a marvellous type of person, with an enormous uncultivated field to work in.

Answer.—I am sorry to have given that impression. It is that, I think, but it is more than that. He is constructing—working out—theories of his own. I have mentioned two or three of them. There is this idea of acting on an organism in a particularly unstable state, and he has very shrewd ideas, for instance, as to the effect of a higher or lower temperature. He does not know the detailed mechanism, and one could not know it without doing another kind of research altogether. On plant physiology this is a goldmine, because every one of the effects I saw lends itself to physiological and biochemical research. These effects are obviously produced by certain chemicals moving from one part of an organism to another. By various experiments you could find out what they are, break them down, analyse them, synthesise them, try them out, and so on. There are several hundred man-years of work in that. He is not doing it himself, but other people are doing it in the biochemical laboratories of the Soviet Union. But he is concerned with what you might call naturalistic laws of the kind that are adaptable to the living material. It is a mistake we are apt to make, I think—especially people like myself who are physical scientists—to think that we can take nature and immediately reduce it to simple basic laws dealing with atoms. It is very nice when you can do it, but in the first stages of growth nature is a bit more complicated, and you have to use rather rougher laws, which are not expressible immediately in molecular terms. That, I think, is the work Lysenko is doing.

Question.—With regard to the 28 and 42-chromosome wheat, did the transformed grains breed true?

Answer.—Yes, the transformed wheat bred true, and that was the interesting point. It seems to be in every ordinary way a complete vulgare wheat. I am only just repeating to you what I was told. I do not understand at all how a thing like that would be explained in the ordinary way. All I was saying was that it is not a mixture of seeds. What the mechanism of transformation is I do not know. The experiment was done to make durum wheat into a winter wheat, and it completely failed. They could never make it into a winter wheat. Every effort resulted in producing a vulgare wheat.

Question.—How far is research applied in industry, and how does it compare with our methods of standardisation here?

Answer.—The application of research to industry is extremely intense, and science arises very closely out of industry and ideas coming up from industry. Standardisation has been carried out to an enormous extent: in the building industry there is complete standardisation of all major components over the whole Union. All joists, door frames, and so on are standardised and completely interchangeable, independent of the material. There is a very interesting journal I have come across here on mechanisation, which shows the extent to which they are developing a really scientific approach to industrial design problems. I think myself that in a few years' time they will be well ahead of the Americans in both mechanisation and in the chemical industry.

Question.—Persons hostile to the Soviet Union frequently say that no scientist is allowed to follow a line of research on theories which are—or may be—hostile to communist political theory. It is said, for example, that no research could be done on anti-Lysenko lines in the biological field and no psychological research on Freudian lines. Is this so? One has great difficulty in arguing with people on these matters because of the lack of real knowledge. If it is so, why is it?

Answer.—I think there is no doubt about the two cases you have mentioned as far as State-subsidised research is concerned. The reason is, of course, that they consider scientific *theory* has a very much larger part to play in science than we are apt to think here. This again is where the history of science comes in. They consider the basic ideas underlying a particular kind of science may determine its actual scientific content, and therefore if from that point of view the basic ideas are wrong, they suspect the whole edifice built on them. That is undoubtedly true of Freudian psychology, which they consider arose out of the bourgeois idea, held at the end of the nineteenth century, that life is essentially a matter of individuals living in a competitive society. They feel that has no proper application to conditions in a socialist society, and so they have an entirely different basic psychological theory. That basic theory is the one which is taught, and on which research is based. I do not want to go over the Lysenko matter again, but it is the same in that field. In physics, where all kinds of basic theories enter in, there is no fixity at all at present. Considerable debate is going on. The essential factor, in their view, is whether a thing works or not, and they will naturally favour a theory which fits in with their view of socialist development. But it must work in the practical material world as well, and they will only accept an approach that satisfies both. They will criticise the theoretical grounds and see how far the thing can be reformulated. That is the case in physics; I am afraid I cannot say more: in general there is a definite concentration on all lines of research which are in general conformable with dialectical materialism.

Question.—Arising out of the last question, I should like to ask whether the difference between the biologists of the West and Lysenko and the Michurinists, is regarded as absolute by Lysenko, or whether it has become exaggerated on account of political differences between the two countries which lead the Soviet Union to give their political support to the Lysenko biologists with corresponding abuse of, or at any rate antagonism to, Western genetics. Is it the same on this side, and if the biologists got together without the politicians, would they find a great deal of agreement between themselves?

Answer.—That question really requires quite a long answer, but I think I can say this: in actual detailed accounts of what happens in the breeding of plants and animals, the two views are not as different as they might seem. That was put forward in the Soviet Union by Zavadovsky, who is still continuing his work, by the way, and in this country by Professor Haldane. The view of Lysenko, and the officials' view in the Soviet Union, is that the *approach* is quite different. You might argue, as it was argued at the time by such cautious people as Tycho Brahe, that there was no real difference between the ancient Aristotelian and Ptolemaic systems supported by the Church, and the new views of Copernicus. To Brahe they were merely different ways of describing the same phenomena. But most people at the time felt very violently about one or the other, not so much because they were different in their immediate adequacy, but because they had different starting points and tended in different directions. Over here, if you want to explain ordinary breeding practice, you start with Mendelian laws, and where they don't fit the facts, you add a few such ideas as the inter-connection of genes, plasmogenes, and polygenes, and modifying factors. If you add enough of these operations, almost any facts can be made to fit in with the gene scheme. That is what biologists do here. They start with Mendel, but by the time they are where they are now, his original views seem not very important and rather crude. Naturally they object to the Russians for attacking Mendelism, pointing out that they are attacking the old crude Mendelism, and that now they have got beyond all that, and that by the various improvements they have smoothed all the difficulties out. The Soviet view is that you ought to start with the organism and work inwards, instead of from hypothetical genes outwards, and then work out the contradictions within. They may have reached the same place, but the *starting point* and the *directions of advance* are the real differences.

As to the political side, it is rather the other way round. The Soviet biologists did not take this up to spite the Westerners. They took it up for purely internal reasons. The major question is the practical one of whether they can wait until they get new breeds from pure stocks and selection. They believe, on the basis of practical experience, that Lysenko's methods can improve agriculture far more quickly. That was the internal quarrel, and it had nothing whatever to do with what was going on outside. Now as a result of the announcement of the internal changes, it has been the basis for a big ideological and political attack outside, and that hardens the situation inside and sharpens the whole controversy. But originally it was a strictly internal business of the Soviet Union's. I do not think any compromise in the narrow sense could be achieved, but a wider theory which will include the valuable results of both approaches will undoubtedly be built up. I do not know how long it will take, but I think it would be a good deal quicker if we did not have political differences. I do not think scientists in the Soviet Union will agree to any middle course at the present time.

Question.—Could you tell us something about the way in which Russia has managed to spread the knowledge of science, and also to spread an interest in science? Generally in this country it is assumed that only a very small proportion of the people can and will be interested. It is obvious that a very much larger proportion are interested in Russia. Have they any devices that might profitably be used in this country?

Answer.—I think the main device they use, a very simple one, is the device of socialism. I say that quite seriously, because to the greater number of people in a country like this there is no particular advantage in science and a lot of disadvantage: science and the applications of science in industry mean, on the whole, harder work and unemployment, and you cannot arouse very much enthusiasm for that. On the other hand, on the scientific side there is the feeling that science is for the elite and is vulgarised if more people know about it; that it is very much better to be able to talk in your own symbols, which your own pals know, and not to have to try to explain it in a way that will certainly spoil some of the finer points. That is an attitude characteristic of any closed society. As for the purely technical problem, they have used three or four different methods. They do it through the schools; there are a great many popular exhibitions; and there is the enormous educative activity of the scientists themselves. They work with the people in the factories and fields. The scientist's time is very largely spent in going round helping along with things. He is himself now a man of the people, under a school system that does not draw the scientist from scientific or "educated" families, and he goes back to the people, whom he finds very much easier to talk to than our scientists do.