SCIENCE

VOL. 96 — FRIDAY, SEPTEMBER 25, 1942 — No. 2491

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS

Lancaster, Pennsylvania

Annual Subscription, $6.00 — Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary in the Smithsonian Institution Building, Washington, D. C.

AGRICULTURAL SCIENCE AND THE PEOPLE'S WELFARE[1]

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I AM going to talk to-day not as a citizen of one country addressing the citizens of other countries, but rather as one scientist talking to other scientists. The concepts and methods of science, including our own agricultural sciences, are still international; they cut deep under the conflict that has now spread over the world. And the faith of science is international; it is that the truth, which science is forever seeking, must ultimately prevail. If it does not prevail now, it is partly because we do not yet know enough of the truth and not enough people are convinced, or even aware, of that which we do know. Meanwhile the scattered fragments of truth that science has discovered can be perverted, and are perverted, to cruel and brutal ends—as when modern technology, a clean-cut product of science, is used to bring whole peoples under the domination of a small group of power-hungry men and to destroy millions of human beings.

But I think there is not a scientist in this audience who would not agree that this *is* a perversion, and who does not feel horror and shame, deep down in his being, that science is so perverted. It is not what we, as agricultural scientists, want. We want to do everything we can to prevent this kind of perversion in the future. We want science to be used to serve the welfare of the people of the world. We know that it is a

[1] Address given at the Second Inter-American Conference of Agriculture, Mexico City, July 6, 1942.

powerful tool to advance human welfare. It can help us to feed, clothe and house ourselves better than the human race as a whole has ever done in the past—and to be healthier and stronger and saner—and to adjust ourselves better to our environment, and to one another as individuals in a society. Science can do these things by helping us to know the truth about the world in which we live—and about ourselves as human beings. "Ye shall know the truth, and the truth shall make you free." The fundamental faith of scientists has never been better expressed than in those simple words, spoken two thousand years ago.

In spite of all the shortcomings of our knowledge, we know enough to-day so that we should be far freer than we are of want and misery, suffering and fear. Our duty as agricultural scientists, then, is threefold: First, to keep on everlastingly seeking for more of the truth, each of us in that field in which our particular work lies; second, to see to it that whatever truths we discover become as widely known as possible; and third, to advance the use of these truths for human well-being.

Sometimes I think we are too modest about that second duty; we hesitate to talk about our discoveries lest it sound like boasting, with the result that they are sometimes neglected or forgotten and, like Mendelism, have to be rediscovered. And we are often too timid about the third duty. We feel that our job is to discover the facts and that the job ends there. Some scientists feel that they are not concerned, as scientists, with how, when or where the facts are applied. The result is that they may be misapplied, even completely distorted—and science gets the blame. I do not say that we can entirely prevent this; I know we can not, because all the scientists in the world are relatively a small handful of people. But we can *help* to prevent it by taking a much stronger, more positive interest in the use made of science by society.

Who should take this interest more than the scientists? After all, it is our work that is used; it plays an enormous part in forming the physical environment and the mental atmosphere of our time. We have the strongest motives for seeing that it is used as universally as possible to create better conditions for human beings. And what is even more important, to make all of us better human beings—wiser, more understanding and more tolerant because we see more of the truth.

This need has now become critically urgent because we are at a turning point in human history. We must build a post-war world that will not be torn apart every few decades by its own inner strains and stresses. We must do this or face a future more destructive than even we of this generation can conceive. The time has come when no scientist can sit any longer in a nice ivory tower, believing that the truths of science by their very nature are something apart from the practical affairs of men. There will be no one even to discover what these truths are if all the ivory towers are blown up by bombs and their occupants are shot by firing squads. That is what we do not intend to permit.

Such thoughts as these are in all our minds; they are a sort of undercurrent in our work in this year 1942. But our immediate concern, of course, is with the everyday problems that face us as agricultural scientists, and my object is to discuss some of the broader aspects of these problems.

Agricultural Science Now Faces a Huge Task

Agricultural science is bound to find itself in a strengthened position as a result of recent events. A short while ago some people felt that it had done its work only too well; it had enabled farmers to increase production to the point where they could no longer dispose of all they produced at a profit. Actually, of course, this was simply another proof of the great power of modern technology. Give it a job to do, like making two blades of wheat grow where none ever grew before or enabling one man to do the labor of five, and it will do the job. Meanwhile, society, including our system of distribution, has to be so ordered that hungry people are able to get the bread—or any other product—that modern technology can produce in abundance. We can not create a powerful tool like technology and then try to throttle it with old-fashioned forms and ideas. We have to use science to advance along the whole front—or else go backward. And once we start going backward, we run a grave risk of returning to something like barbarism.

To-day there is no doubt in any one's mind about the need for science in agriculture. Vast armies have to be fed, clothed, housed and kept healthy. Our allies need enormous quantities of agricultural products. Our industrial workers must be well nourished to produce efficiently. The purchasing power of consumers has increased. Imports of critical commodities have been cut off; we must produce them in the Americas or create substitutes. And meanwhile there is a shortage of agricultural labor in the United States which must in part be made up by technical proficiency. None of these problems can be solved without making full use of all we know about scientific methods—and finding out as much more as we can as quickly as possible.

Nor will the present emphasis on agricultural science end with the end of the war. We shall still have a very big job to do. Much of Europe is starving and in ruins. Asia, Africa, Australia have all suffered in greater or less degree from the blight of war. Immense areas and millions of people will need to be rehabilitated. To rebuild an exhausted world will necessitate making full use of agricultural science.

At no time have we been faced by so great a challenge. To meet the situation adequately will require the best we have to give in the way of energy and brains. The wisdom and foresight we apply to our work during the war and post-war period of world emergency will be influential not only in having the value of agricultural science fully recognized for a long time to come but in actually shaping the world.

What are some of the things we need to do in conducting our research to meet present needs and lay the foundation for a better future?

THE NEED FOR MAXIMUM EFFICIENCY

For some time my own task and that of several of my associates has consisted mainly in following through on a few urgent problems connected with the immediate needs of the war. Getting started on the production of Hevea rubber in South and Central America and of guayule rubber in the Americas and exploring the possibilities of other sources of natural rubber that could be grown in the Western Hemisphere is one of these problems. Working out processes for making synthetic rubber substitutes from agricultural products to help meet the present emergency is another. Still another is the speeding up of new ways of processing and packing foods to save shipping space and still supply the demands of overseas shipment and our armed forces, and to make up for the shortage of tin formerly used in cans and now diverted to military needs. Another has been to develop Western Hemisphere sources of important drugs, fibers and oils which we formerly received from the Orient.

These are jobs that have to be done as quickly as possible. They have meant working under pressure, concentrating scattered research facilities and personnel on a single problem and reducing what we call red tape—the ponderous procedures of government agencies—as much as we can in order to get quick results.

This suggests the first point I want to bring out in connection with agricultural research as a whole. We need to make a critical study of our research procedure to see where we can do more of this kind of thing—cut corners wherever possible, eliminate slow and cumbersome procedures that have grown up as agricultural science became divided into more and more special compartments, and bring people together for quick, common action. We scientists have had our own ways of doing things, very thorough but sometimes rather slow ways, and we were not going to let any one who did not know the necessities of science interfere with them. There are, of course, many things that can not be speeded up in scientific research; but I am convinced that there are also places where we can rid ourselves of excess baggage and strip down to a more clean-cut basis that will enable us to proceed faster and more efficiently. This is the time to do it. We can not afford to be slow in meeting the problems of human welfare that face us all with increasing urgency.

WE SHOULD LEAD AS WELL AS FOLLOW

There is a second point of quite an opposite kind, however, in connection with this war work. We must beware of the temporary pressure of public opinion—which operates very strongly on a public research agency—to drop certain projects or certain lines of work that seem to us very worth while. I am reminded of this particularly in connection with the work on natural rubber. The Bureau of Plant Industry in our Department of Agriculture has been studying the sources of natural rubber for the Western Hemisphere for many years. As far back as 1921 our scientists foresaw the situation we have to-day. "Suppose," they said in effect, "we were to be cut off from the supplies of rubber in the Far East; we ought to be ready to produce it nearer to our own doorstep. Otherwise we might find ourselves in a very serious situation at a critical time."

This caution undoubtedly seemed far-fetched to a good many people, and there were unquestionably sound economic arguments against developing natural rubber here. Perhaps the scientists seemed like dreamy, impractical fellows. Nevertheless, they kept on studying rubber-bearing plants, doing what work they could, even if it was only a modest amount. It was this far-sightedness that has enabled us to go ahead rapidly with the present Hevea project in South America and the guayule project in the Americas. These "impractical" scientists laid the groundwork for what we are doing to-day.

The point is that the scientist is often the shrewdest judge of what needs to be done in his own field. In this case, even though there was no immediate need to develop sources of natural rubber in our part of the world, he knew how long it takes to breed and evaluate rubber plants, and find out where they will grow best, and work out the best methods of growing them in a new environment. "Let us be prepared," he said, "at least to the extent of knowing how to do the job if we have to." We can see now that he was right.

The lesson, so far as agricultural research in general is concerned, is that the research agency should carry along certain projects when it is convinced, after rigid self-examination, that they have an insurance value for the future, even though they may not have popular appeal at the time. The research worker must do more than follow the trends of current events; he also has a responsibility as a leader who looks ahead. Where scientific work is closely and directly tied up with the public welfare, as ours is, this responsibility is especially great. If we exercise it wisely, we can be sure of public approval in the long run.

THE NEED FOR FREEDOM OF INQUIRY

Closely related to this point—the need to carry along certain projects whether or not they seem to have immediate value—is the question of carrying on basic research.

Many people think of basic research as purely theoretical; they are unable to see, for instance, that it has any practical bearing on farm problems. "Why spend time and money on it," they ask, "especially in a critical period like this?" To the scientist, the answer is simple: The whole progress of science depends on filling in the gaps in our knowledge of the nature of things. To the layman, however, it is not so obvious that much of the structure of modern science, with its vast number of practical applications in our daily lives, is based on a comparatively few fundamental discoveries.

In the case of basic research, we can seldom tell in advance what these practical applications will be; we simply know that we are on the trail of a fact which explains something not previously understood. But once the fact is established, the practical applications—sometimes very surprising ones—are likely to turn up in abundance. Three cases in our own work occur to me because they are very recent.

It has been known for a long time, of course, that plants respond among other things to temperature and also to intensity of light. A few years ago it was discovered that they also respond to length of day. After this fundamental discovery was made, a good deal of experimental work was carried on to find out the nature of the responses, and many important facts were uncovered, the upshot of which is that the growth, flowering and fruiting characteristics of many plants—including many of economic importance in our agriculture—vary with variations in the length of the light period to which they are exposed.

Originally this may have seemed like an interesting botanical fact not of great practical importance. But note some of the developments to which it has led.

Instead of expensive and lengthy field experiments, we can carry out comparatively inexpensive experiments in a greenhouse, under controlled light and other conditions, which show the range of adaptability of various kinds of plants; and on the basis of these experiments we can predict with considerable certainty what plants will do well in a given locality. If certain varieties of plants prove not to be well adapted to the day length and other conditions there, we can breed varieties of high quality and great uniformity that will be adapted.

We used to be almost entirely dependent on Europe for our supply of sugar-beet seed. By determining the rather exacting temperature requirements for seed production by sugar beets, we were able to find localities in our own country where large crops of seed could be produced. In fact, we now save a great deal of time and labor by growing the beets for seed as a winter annual, the cycle being much like that of winter wheat. Not only are we independent of European sources, but the seed we produce is better adapted to our conditions than that from Europe because it has been bred for resistance to the plant diseases prevailing in our country. In the breeding work, light exposure as well as temperature is manipulated in the greenhouse so that we can bring our selections to the seeding stage in the minimum time.

There are many other practical applications of this essentially *botanical* discovery about the nature of plants. Onion varieties can be found or bred that will produce bulbs in a short-day region, others in a long-day region; likewise strawberries that will fruit abundantly in our long-day northern areas, others in the short days of our South. The same thing is true of other agricultural crops. Florists can hasten or delay the flowering of plants simply by controlling the light period in the greenhouse. And so on. All this means a better adaptation of production to the needs of both producers and consumers in a given area.

If I had more time, I would like to go into this matter in greater detail. For example, it was found recently that a few minutes of exposure to light near the middle of the dark period gave results similar to those from a full exposure. Grafting a single leaf of an Agate soybean, which blooms readily under long-day conditions, onto a Biloxi soybean, which requires a short day, enabled the Biloxi also to bloom under long-day conditions. Such facts as these open up fascinating fields for new explorations in plant chemistry and physiology.

The discovery of growth-regulating substances, generally called plant hormones, is another example of basic research that is now giving practical results. We carried this work along quietly for several years, treating various parts of plants—roots, stems, buds and flowers—with many different substances, some of which had marked effects of great theoretical interest; in some cases the natural behavior of the plant was completely altered. In the course of these experiments it was found that apple trees treated with a very dilute hormone solution would hold their fruit without dropping it far beyond the normal period. As a result, hormone spraying of apples is already becoming a standard orchard practice that promises to be of immense value to producers in preventing the dropping of fruit before it has reached good size and color. This single application should save far more than all the money spent on all our hormone research. Yet it was not even dreamed of in the beginning of the research work; in fact, it was simply one of several by-products of the research job. I think we can say confidently that there will be many other practical applications of this work. One of them, already of considerable value, is the use of hormones to stimulate root development in cuttings of plants that ordinarily root with great reluctance. Another, so far tried only experimentally, is the dusting of blossoms to obtain fruit of better quality with such plants as the tomato.

The third example I want to mention briefly is waxy corn. In the course of research on the genetics of corn, a plant was developed with a peculiar waxy quality in the starch of the endosperm. It was of no practical use whatever, but because of its theoretical interest it was not discarded but carried along by the breeders. Now this curious corn promises to be of great value as a substitute for tapioca starch, the supplies of which have been cut off by the war.

The examples I have used have all come from the plant field, but similar ones could be given in practically every other field of agricultural science. Time permits mentioning only a few cases in which basic research has played a part in one way or another, though not always in as clean-cut a fashion as in the illustrations already mentioned.

In the animal-disease field, there is the discovery of phenothiazine as an anthelmintic. One of the remarkable things about phenothiazine is that it is effective for a considerable range of internal parasites in various classes of animals, whereas most anthelmintics are quite specific and limited in their usefulness. In addition, it can be administered more easily and conveniently than most anthelmintics. All in all, phenothiazine promises to be one of the most valuable aids we have in combating worm parasites, which as you know are a major enemy of livestock production. As part of the background of this development there were years of patient work studying the effects of parasites in the animal organism and testing many different kinds of drugs for their anthelmintic value.

Similarly, in our modern insect control work, there is a background of studies in the life histories of insects to determine at what points each one is most vulnerable to attack, as well as detailed studies of the physiology and functioning of the insect organism, especially in relation to the effects of various poisons.

In animal breeding, we are engaged in a major project to increase the production of our dairy herds by the wide-spread use of tested and proved sires. This is an undertaking of very large scope, and it would never have been started if our breeders had not been convinced, on the basis of a long, careful study of the fundamental genetic problems involved, that it could be successfully done—and that it would result in very worth-while economies in milk production.

Fundamental researches in the genesis of soils led to the development of the principle of ionic exchange or "base exchange." The practical results have been numerous and far reaching. We can now apply lime to acid soils accurately according to the needs of different crops and combinations of crops. Even more important, we can control irrigation much better. The formation of puddled alkali soils can be prevented, and alkali soils can be reclaimed through chemical treatments and the control of irrigation water and drainage.

In the field of human nutrition, there have of course been many interesting developments in recent years. One of them of particular importance to all of us in agriculture is based on what was essentially a piece of basic research in the dietary habits of large numbers of individuals, families and economic groups. Out of this has come a method of determining the nutritional status of whole segments of a population. This is immensely valuable not only in production planning but also in what I might call the statesmanship of human well-being. Before we can raise the nutritional level of a population or a group, we must know where they stand and what improvements need to be made—and this information has not hitherto been available.

In all these cases, either basic research precedes the practical applications of science, or a certain amount of this kind of research is found to be necessary somewhere along the line to clear up obscurities that block further progress. As public service agencies, our agricultural research institutions exist primarily to help agriculture solve practical problems. But the point I am making is that in research there is no single road to practical results. If we keep our eyes constantly and exclusively on what seem to be immediate needs, we miss some of the richest fruits of scientific work—the fruits that grow from the discovery of important fundamental facts.

This is not to say that we do not need to use good judgment in the choosing of research projects in a given program. We need to use the best possible judgment. But in the last analysis, it should be the judgment of scientists. This is the essence of scientific freedom or freedom of inquiry.

Perhaps that is what it gets down to—freedom of inquiry. This freedom is denied whenever an effort is made to use science solely for the support and aggrandizement of a special group. In the long run this is a terrible mistake. Freedom to explore all kinds of facts wherever they may lead is absolutely necessary if science is to advance human welfare. It is a freedom worth fighting for.

The emphasis I have given to basic research and freedom of inquiry does not mean that we should pay any less attention than we do to homely experimentation directed toward solving everyday problems. On the contrary, most of our time will necessarily be spent on this kind of work. After the basic facts are discovered, there is always an immense amount of what might be called best-way research to be done. What is the best way to prune fruit trees? What is the best way to meet the mineral requirements of livestock? What is the best way to cook vegetables so they will retain their vitamins? The number of such problems is endless, especially since in every case the best way varies under different conditions of cost, available equipment, environment and final result desired. To determine all these best ways means carefully controlled, systematic experimentation, demanding much patience and frequently a great deal of ingenuity.

Coordination is Needed

The next point I want to emphasize is the need for coordination, briefly touched on earlier in these remarks.

I don't know how it may be here in Mexico City, but in the city of Washington, this period might well be called the Age of Getting Together. I have never seen so many gatherings of different groups to discuss urgent needs and decide on combined action to meet them. They are so numerous, in fact, that I sometimes feel as though I were not doing anything else but attend conferences.

This has its drawbacks, but it is a sign of the times. In a time of common peril, separate groups tend to pull together and act as one group to achieve a single aim.

We in the agricultural sciences are feeling this influence very strongly. It is a good influence. I hope and believe that we are at the beginning of a new time of closer cooperation and better integrated planning. Many of us have given lip service to this principle in the past, but we have carried it out only in a partial, half-hearted way. From now on we will be compelled to carry it out much more vigorously and wholeheartedly if we are to solve the pressing problems of the public welfare.

In the ordinary course of events, an agricultural problem might be tackled something like this: Perhaps the chemists see the problem first and approach it from the chemical standpoint. They work along for a couple of years and then find that there is a bacteriological aspect. The bacteriologists work on this for a few years, and discover that there is an entomological problem mixed up in it. After the entomologists have worked for a while, some one finds that the home economists should be brought in. Meanwhile each group works strictly along its own bureau or departmental lines, jealously guarding its bureaucratic prerogatives and fearful that the others will trespass on its ground. Perhaps at the end of ten years, the problem is finally solved.

Why not get the chemists, the bacteriologists, the entomologists and the home economists all together in the beginning and have them working simultaneously on parallel aspects of the problem, instead of end to end, separately and in sequence? In that way the job might be done in two or three years instead of ten. This is what we are doing now with more and more of our problems. The method is not new, of course, but its use is being accelerated by the conditions we all face to-day. We still have much to learn about this kind of integration, but as it is extended over larger areas of our work, the possibilities seem to me stimulating and inspiring.

I should like to mention briefly a research project that may serve as an example of the sort of coordination I have in mind.

As part of the background of this project, three facts are especially important. (1) Most agricultural research with crops has had as its ultimate object the production of the greatest quantity of a given product of a high market quality from a given acreage. (2) Recent work in the field of nutrition, however, has developed hitherto unknown facts which give us a new understanding of nutritional quality and its immense importance for human well-being. (3) There is ample evidence that food and feed plants grown on soils deficient in certain minerals—iron, copper, manganese, cobalt, phosphorus, for example—fail to supply enough of the deficient minerals to maintain good health. Human beings living on these foods suffer from serious physical disabilities; animals may actually die by the thousands.

These facts raise important questions: To what extent would it be possible for us to improve and insure high nutritional quality in food and feed plants as an objective in agricultural production, in addition to our older standard objectives of quantity and market quality? And how can health and human welfare be improved by adopting such objectives?

Recently, in cooperation with many states, we established a nutrition laboratory on the campus of Cornell University, in the State of New York, to study this question.

As you will readily see, it breaks down into a good many different parts because there are many things that may affect the nutritional quality of both crop and livestock products. For instance:

What does climate have to do with the vitamin or mineral content or other nutritional factors in the case of such farm products as vegetables, fruits and grains? What variations are due to differences in soil? What effects do soil amendments, such as fertilizers, have? What are the effects of other agronomic practices besides the use of fertilizers? Do different varieties of the same plant vary significantly in nutritional quality? If so, to what are the differences due—and would it not be possible to breed plants with superior nutritional value?

We have hardly more than begun this work, but so far I think we can say definitely that it will be possible to breed some plants at least that will have uniformly superior nutritional quality so far as their content of vitamins and minerals is concerned. That is not the point here, however. The point is that this project necessarily involves simultaneous work by scientists in many fields. We have brought into the picture outstanding experts in human and animal nutrition, in soils, in plant breeding, in plant physiology, in home economics and in public health problems. Each has vital contributions to make on different aspects of the whole project. As a result, I believe that we shall avoid the common danger of going too far in one direction while missing something of great importance in some other direction. The whole project, to my mind, is an excellent example of cooperative research focused on a common objective that not only has great scientific interest but should make a real contribution to human welfare through better nutrition.

WE NEED SOCIAL AWARENESS

The final point, which I wish to emphasize again, is the need for research workers to be very much aware of the relation of their work to the problems of human welfare.

We have perhaps taken this too much for granted. Science has accomplished so much in the modern world—its achievements are so evident all around us in the form of new products and new modes of communication, transportation, manufacturing, and so on—that we assume that of course it contributes to human welfare.

Yet we have been faced by the stark facts of poverty and unemployment in the midst of plenty—ignorance in an age of enlightenment—and finally, total war, meaning total destruction, in an age supposed to be dominated by science, which is essentially a builder and not a destroyer.

Can we, as scientists, look at the world to-day and say we have done all we could to make our work contribute to human welfare? Have we not been rather naive in assuming that our job was to do the particular piece of research in front of us, irrespective of how the results were used?

A discussion of these questions would take me very far afield. I am raising them simply to suggest that it is part of our business as agricultural scientists to try to understand and as far as possible anticipate human needs—social and economic needs—in our work. To take an immediately pressing example—what is the world going to be like after this war? How can the enormous productiveness of science be put to work to make it a better world to live in than the one before the war? How can we direct our work as scientists to prevent wars and to create peace, plenty and opportunity for far more of the world's people? Those are the kinds of problems we must face in our thinking and planning. There is no question but that the viewpoint and method of science can be a powerful factor in solving them.

The agricultural research institutions of the Americas constitute, in the aggregate, a very large body of scientific facilities and skilled personnel. Our big job in the years immediately ahead is to use our combined power and vision for the welfare of humanity—and when I say our, I mean those of us who work in the natural sciences as well as the economists. If we make this the unswerving intent and purpose behind our work as agricultural scientists, I am sure we shall find the means to contribute a great deal to a brighter future for mankind.

SCIENTIFIC EVENTS

RECENT DEATHS

DR. HECTOR RUSSELL CARVETH, electrochemist of Niagara Falls, N. Y., died on September 7 of injuries sustained as the result of the explosion of a cement tank. He was sixty-nine years old.

DR. JOHN ARTHUR WILSON, consulting chemist, New York City, president of John Arthur Wilson, Inc., died on September 17, at the age of fifty-two years.

DANIEL A. LEHMAN, professor emeritus of mathematics and astronomy of Goshen College, died on September 8, at the age of eighty-two years. He had been a professor at the college since 1906.

SIR JOHN MACPHERSON, emeritus professor of psychiatry in the University of Sydney, died in England on August 14 in his eighty-fourth year.

Nature reports the death at the age of seventy-three years of Dr. Kurt Brandenburg, professor of special