do not: a result which strongly suggests the diverse nature of the cancer-producing processes.

Those who read the abstracts of this Symposium can hardly doubt that hereditary tendencies play a part in the causation of cancer. However, the considerations just outlined suggest that while it is possible frequently to generalise from the genetic situation found in one pedigree to that expected in another, caution should be exercised in doing so. Nevertheless, the knowledge that such hereditary tendencies exist is likely to be of use to practitioners. Advice in regard to marriage can clearly be given to members of those families in which pre-cancerous states are inherited on a simple basis (such as *xeroderma pigmentosum*, a recessive with slight heterozygous manifestation, or pre-cancerous rectal polypi when due to a gene effective in the heterozygote). Such conditions are rare, but the facts of genetics suggest a caution of wider application: that the possibility of cancer should particularly be kept in mind when treating members of a family in which several cases of the disease have occurred, such as those illustrated in the pedigrees communicated to the Symposium by Smithers and Tage Kemp. Within the last five years I have myself encountered two relevant instances of this kind. In both of them the patient experienced for some time vague abdominal discomfort with occasional pain for which he had received palliative treatment, and an eventual exploratory operation demonstrated a cancer which by then had become inoperable. These two men each had a family history of the disease, which had occurred in the mother and brother of one and in a brother, a sister and an aunt of the other. Had this fact been taken into account, it is possible that suspicion might have been aroused earlier, perhaps in time for the removal of the growth to be attempted with some hope of success.

All will agree that early diagnosis is the pre-eminent requisite in the treatment of cancer, moreover the first step towards securing it is almost entirely in the hands of the patient. If it were generally appreciated that the probability of cancer is rather higher in some families than in others, those with relations who have developed the disease might pay attention earlier to suspicious symptoms when arising in middle or later life.

There are certain to be some who feel that the public ought not to be told that any such hereditary susceptibility exists, holding that much unnecessary alarm would be given to the relatives of cancer patients. I do not share that view, believing rather that what is true had better be known. The essential co-operation of patients may best be secured by making clear the relevant facts relating to cancer, and its genetic aspect could play some part in securing its early recognition, upon which all hope of cure depends.

E. B. Ford.

THE MOSCOW CONFERENCE ON GENETICS

1. SOVIET BIOLOGY. By T. D. Lysenko. A report to the Lenin Academy of Agricultural Sciences. Moscow 1948. Translated for the Science Section of the Society for Cultural Relations with the U.S.S.R. Pp. 51. London: Birch Books. 2s. 6d.

This address by Academician T. D. Lysenko to the Lenin Academy of Agricultural Sciences reports results so remarkable that they raise questions beyond the scope of an ordinary review. Most writers on biology, especially when dealing with their own work, freely and clearly give all relevant details of their experiments so that the reader can decide whether or not the results and conclusions the author arrives at are justified. It is regrettable that so often in this book and in earlier publications by the author and his associates, such details are not adequately given. We have, however, heard so much about the work of Lysenko and Michurin that all biologists—and especially those engaged in growing, grafting and breeding plants—will, I feel, be impelled to read this latest account of biology in the Soviet Union. There are five points which will doubtless attract attention and at the same time puzzle the critical reader :—

(1) breeding grafted plants ;
(2) vegetative hybridisation ;
(3) improving plants by grafting—Michurin's so-called " Mentors " ;
(4) the inheritance of acquired characters ;
(5) the rapid conversion of " hard " wheat *Triticum durum* into winter wheat *T. vulgare*.

These five points, and especially the first three, I shall attempt to analyse in detail.

(1) The first experiment involved the grafting of a tomato shoot with " pinnate leaves " and yellow fruits on to a plant with potato-like non-pinnate leaves and red fruits. Seeds were sown from the red fruits of the latter part and we are told that most of the resulting plants " did not differ from the initial strain." " Six plants, however, had pinnate leaves, and some had yellow fruits." Lysenko concludes that this result is due to both the leaves and the fruits having changed under the influence of the yellow-fruited, pinnate-leaved shoot grafted upon it.

In this experiment, and indeed in all the experiments referred to in his address, Lysenko and his associates seem to have had no scientific method. It would have been of value if we were told if all the six pinnate-leaved plants had yellow fruits, and if not how many were yellow, and what was the colour of the remainder. Also, if control plants were grown and whether the two plants used to make the composite grafted plant were homozygous or not.

The inheritance of leaf-shape and fruit colour in tomatoes had been known by several workers before 1910. The pinnate cut-leaf character *C* is dominant to the non-pinnate potato-leaf *c*. Two major genes *R* and *Y* are concerned with fruit colour ; *RY* is bright red as in the common tomato of commerce, *Ry* is dull red, *rY* deep yellow and *ry* pale yellow. Since no mention is made of control plants the first question which arises is : were the plants, and especially the red-fruited plant, used in the grafting, homozygous or heterozygous ? That is to say, was it of the constitution *RRYY*, *RrYY* or *RrYy* ? For, although the fruits of all three would be red and indistinguishable, seeds from the last two would give a proportion of plants with yellow fruits, but we should expect them to have the recessive potato-leaf character. If, however a few grains of pollen from the pinnate-leaved upper part of the composite grafted plant came in contact with the female organs of the potato-leaved lower part, then plants with pinnate leaves and yellow fruits would arise. Admittedly natural cross-pollination in tomatoes is rare between separate undisturbed plants, but it is not unknown, and where, as in this grafted plant, two forms are growing together and are probably being interfered with by the experimenter the chance for cross-pollination is greater than between separate plants.

There is another possibility which we may consider. As shown by Jørgensen and Crane (1927), plants grafted together and especially solanaceous plants, frequently develop tissues in which the two components are intimately combined. The most common development is the so-called mericlinal chimæra where even only a small area of one component may be over the other. If such an area is two layers thick, resulting seeds and offspring will be like the component of which these layers are composed. On the other hand, if the outer component is only one layer thick—then the offspring would be like the inner component. In this way seeds from a single fruit could give both pinnate and yellow and non-pinnate and red-fruited plants.

Later, Lysenko refers to a tomato plant which had one yellow and one red fruit. When we consider the numerous recorded and well-authenticated examples of spontaneous somatic variations in plants (see, for example, Darwin, 1868 ; Crane and Lawrence, 1947), involving all kinds of characters including the colour of flowers and fruits, this tomato plant does not stand out as a very wonderful happening. In my own work with tomatoes I have had a fruit partly red and partly yellow and this was even more remarkable than finding wholly red and wholly yellow fruits on one plant.

In Lysenko's *Heredity and Its Variability* translated by Dobzhansky (1946) it appears that the yellow tomato used in the grafting experiments was the double recessive *rryy*. Plants are also mentioned where the fruits were pale yellow or slightly reddish. In *The New Genetics in the Soviet Union* by Hudson and Richens (1946) tomato fruits pale yellow with pink stripes are described. Since such fruits may appear new and unusual I will point out that these descriptions appear to correspond to the fruits of a variety named "Blood Orange" which has been known in this country for ten years or more. In this variety individual fruits vary. At one extreme the fruits appear almost wholly yellow and at the other they are appreciably reddish. Between these extremes fruits with intermediate amounts of red occur, *i.e.* yellow with irregular blotches or stripes of red.

(2) The so-called "Mentors" elaborated by Michurin receive much attention. Lysenko tells us that I. V. Michurin not only recognised the possibility of obtaining vegetative hybrids, but he elaborated the mentor method. This method consists in the following: by grafting scions (twigs) of old strains of fruit trees on the branches of a young strain, the young strain acquires properties which it lacks, these properties being transmitted to it through the grafted twigs of the old strain. That is why I. V. Michurin called this method "Mentor." We are also told:—

> "When grafted, organisms which have not yet reached the stage of full development, *i.e.* have not completed their cycle of development, will always change their development as compared with the plants which have their own roots. In the union of plants by means of grafting the product is a single organism with varying strains, that of the stock and that of the scion. By planting the seeds from the stock or the scion it is possible to obtain offspring, individual representatives of which will possess characteristics not only of the strain from which the seed has been taken, but also of the other with which it has been united by grafting."

I have been profoundly interested in the growing, breeding and grafting of plants and trees for nearly fifty years, and have raised thousands of fruit trees from seed. I have grown many both on their own roots and on the roots of others. I have grafted twigs of an old variety on to a young seedling on its own roots. I have also grafted twigs of young seedlings on to old varieties. I have raised peach seedlings from peaches growing on plum roots, plum seedlings from *Prunus domestica* growing on *P. cerasifera* roots; pear seedlings from pears growing on quince, on pear-stocks, and also on their own roots. In the same way I have used as parents apples growing on widely different root-stocks. In all these there has not been the slightest indication of the different roots having had any influence on the seedlings. In my experience no vegetative hybridisation has occurred.

I have also compared numerous seedlings on their own roots with the *same individuals* grafted, at an early stage, on other roots. Those which were good on the grafted trees were also good on their own roots and those which were inferior on the one were also inferior on the other.

(3) I am often asked the question, what are these "Mentors" and how do they work? We have just read one account of them in *Soviet Biology*, but Lysenko says:—

> "The best way for scientific workers in various departments of biology to master the theoretical depths of Michurin teaching is to study Michurin's work, to read them over again and again, and to analyse some of them with a view to solving problems of practical importance."

We will take Lysenko's advice and refer to "The use of 'Mentors' in raising hybrid seedlings and examples of definite changes induced in fruit-tree varieties by various external factors." This was written by Michurin in 1916 and published in 1939. Here Michurin says the " Mentor " method works as follows :—

> " Supposing we have a well-developed six- or seven-year-old hybrid seedling, which has not started fruiting, we know that it will not start fruiting before some ten years have elapsed since in some cases the parent varieties do not normally start to fruit until their *twentieth year.* Yet by grafting close to the base of the lower branches of the crown several scions taken from the fruit-bearing tree which is known to be of a high yielding variety the seedling can be induced to bear fruit *within two years* " (italics mine).

Further we are told :—

> " In three further instances the method was used for the improvement of the quality of hybrid fruits, namely, for development of certain storage characters, for improvement in colour of the fruits, for increase in sugar content in fruit flesh. In these instances the ' Mentors ' were employed after the hybrids had already fruited once ; "

and again :—

> " It is quite apparent that the method can be used to effect various other changes in the properties and characters of hybrid varieties, such as the increase of fertility, attainment of larger size fruits, etc."

Now I have had many families of cherries in which the earliest seedlings fruited in their fourth year and the latest in their eighth year. These were grown on their own roots without any interference apart from ordinary common-sense cultivation. Amongst tree fruits pears are most delayed. A few fruit in their eighth year, about 50 per cent. in their ninth year and then there is often a small proportion which have not fruited until their fifteenth year. The point is that as far as I can see Michurin used no controls ; indeed the only way to get real control would be to multiply an individual seedling vegetatively and use some as control and some for experiment. Otherwise if observations are confined to a single or very few trees one might " Mentor " trees which will normally fruit early and compare them with others which normally fruit late.

In this country we are of course, well aware of certain root-stock effects, but these important as they are to commercial growers, are indeed trifling when compared with the claims of Michurian Mentors.

We are also aware of the so-called juvenile period of pears and other fruits and of the practice of bark-ringing to bring fruit trees into flower, but neither these nor the root-stock effects are quite as mysterious as " Mentors." It is just conceivable that several grafts at the base of branches might have a similar effect to bark-ringing, but Michurin does not make any such suggestion.

The claims of " Mentors " for improving size, colour, sweetness, fertility, etc., are no more convincing than the others. They are, however, very remarkable, the inferior seedlings allegedly developing fruit with qualities akin to those of the highly desirable Mentors grafted upon them.

There is much more in this paper of Michurin's written in the same loose and, I fear, lightly judged way, which to some extent has flowed over into Lysenko's *Soviet Biology*. Thus Michurin writes : " As to the famous pea laws of Mendel only very ignorant people may think that they may prove useful to the breeder of new hybrid varieties of perennial fruits. Mendel's law is not applicable to perennial fruit trees, nor does it apply to annual hybrids, or if you wish, to kitchen garden crops themselves." This is, of course, rather an ironical statement. I have always considered the garden pea a kitchen-garden crop, and we know many annuals whose characters behave the same as those in peas. As to *only very ignorant people*, it would indeed be a very ignorant person who would expect vegetatively-propagated perennials, such as fruit trees, which are commonly self-incompatible, and hence highly heterozygous, to behave in precisely the same way as peas which are self-pollinating, reproduced sexually annually and in consequence in the main homozygous. Nevertheless my work and that of my colleagues and others has shown that there are many characters in perennial fruits, such as raspberries, peaches, pears, etc., which behave in inheritance the same as those in peas.

This same paper by Michurin also describes experiments in breeding pears. One of the most successful varieties used as a parent appears to be the variety Beurre Diel. Thus we are told : " This combination was a cross between Beurre Diel and a young seedling of the wild usuri pear flowering for the first time. Of the hybrids raised, two-thirds bore fruits maturing in summer or autumn, and one-third were hybrids producing fruits that ripened in winter ; " I read this to mean that 100 per cent. of the family were fertile, and if so this again conflicts with my experience. Beurre Diel is a triploid variety and hence with me it has not been a desirable parent ; on the contrary, and as one would expect it has proved a bad parent with largely sterile progeny.

Following Lysenko's advice I have read all that has come my way on the teachings of Michurin including *Voks. Bulletin* (1945). Here a certain Professor Yakovlev, a Stalin Prize Winner and Manager of the important Michurin Nurseries, writes : " In his science of vegetative hybridisation which is now being developed and expanded by his talented follower G. (*sic*) Lysenko—Michurin dealt a decisive blow to the metaphysical views of the geneticists Mendel and Morgan."

Professor Yakovlev then goes on to what he calls intergenal hybridisation and writes : " for the first time in world practice such fruit-bearing hybrids have been produced in Michurinsk as hybrids of apples and pear trees (by T. R. Gorshkova), plum and peach (by V. N. Yakovlev), cherry and plum, red and black currants." Now plum-peach, gooseberry-currant, pear-quince, peach-almond and other hybrids have long been known. The plum-peach hybrid was raised by Messrs Laxton of Bedford, and was described by them in the Report of the Third International Conference on Genetics (1906). It was raised from *Prunus triflora* × *Amygdalus persica*. I grew it for over thirty years, and it was quite sterile. I have also grown the peach-almond for over thirty years ; it crops well in favourable seasons and produces good seeds. The pear-quince was raised in this country in 1895 ; in Algeria it produces fruits abundantly but they are entirely seedless. I have made cross-pollinations between apple and pear, but without success.

I wonder why in this article, we are, as is usual in recent Soviet writings, left so much in the dark and not told the things we are eager to know. You will note Professor Yakovlev says his hybrids are fruit-bearing. An account of the parent varieties and details of the flowers and fruits of his apple-pear hybrids would, I am sure, be of intense interest to horticulturists and biologists throughout the world. The same applies to his plum-peach hybrid. Soviet biologists should realise that if they would only take the trouble to give us such details we would then be able to appreciate their work much better, and many misunderstandings might be swept away.

On page 28 of his *Soviet Biology* Lysenko says :—

> " altered sections of the body of the parent organism always (*sic*) possess an altered heredity. Horticulturists have long known these facts. An altered twig or bud of a fruit tree or the eye (bud) of a potato tuber cannot as a rule influence the heredity of the offspring of the given tree or tuber which are not directly generated from the altered sections of the parent organisms. If, however, the altered section is cut away and grows separately as an independent plant, the latter, as a rule, will possess a changed heredity, the one that characterised the altered section of the parent plant."

This, like so much in the book is very far from the truth, for we know beyond any disputation that an alteration of the body cells of an organism does not *always* result in an altered heredity.

One of the most brilliant and informative investigations on this subject was carried out by Lysenko's countrywoman T. Asseyeva, and if he refers to her publication Asseyeva (1928) or, better still, discusses the problem with her, he will find that far from such alterations *always* having a changed heredity, most often and, as a rule, it remains unaltered.

Asseyeva investigated many such body alterations, *i.e.* somatic mutant alterations, in potatoes, and in all cases she says " the characters of the mutant are not transmitted through seed, and its offspring are exactly similar to the progeny of the original variety." The reason why there was no changed heredity, although the altered potatoes had for long been grown as independent plants and were so distinct that they had different varietal names, is simple and clear. The alterations did not penetrate as far as the germ-tract, and hence could not change heredity. With body alterations it does not matter whether or not they are removed from the parent organism ; if the alterations go as deeply as the germ-tract, they will be inherited, otherwise they will not. This of course applies to twigs and buds of fruit trees, etc., as well as to potatoes.

(4) Lysenko brings in Michurinism in connection with the inheritance of acquired characters and he states, " the well-known Lamarckian propositions, which recognise the active role of external conditions in the formation of the living body and the heredity of acquired characters, unlike the metaphysics of Neo-Darwinism (or Weismannism) are by no means faulty. On the contrary, they are quite true and scientific." I cannot find anything in the book which proves that the inheritance of acquired characters is true.

(5) The hard and soft wheats provide my last point. Agriculturists, plant breeders and cytologists alike will, I feel, ponder long over the rapid conversion of *Triticum durum* into *T. vulgare*. Lysenko writes :—

> " Michurinists have mastered a good method of converting spring into winter wheat. . . . When experiments were started to convert hard wheat into winter wheat it was found that after two or three or four years of autumn planting (required to turn a spring into a winter crop) *durum* becomes *vulgare*, that is to say, one species is converted into another. *Durum*, *i.e.* a hard 28-chromosome wheat, is converted into several varieties of soft 42-chromosome wheat ; nor do we, in this case find any transitional forms between the *durum* and *vulgare* species. *The conversion of one species into another takes place by a leap.*"

Biologists are familiar with new species arising by a leap, *Primula kewensis* being a notable example, but the conversion of a tetraploid wheat into a hexaploid species is indeed remarkable and I have no explanatory comments. Perhaps, however, I may be pardoned if I say this brings to mind some mishaps I have experienced in a long association with seeds and plants. In our pre-soil-sterilisation days, I have seen a proportion of red currants *Ribes rubrum* among a sowing of black currants *R. nigrum*. I have even see nelderberry plants germinate and grow where only gooseberry seeds were sown.

Throughout this small book much space is devoted to various philosophical and political themes and materialistic arguments. I have not attempted to discuss them ; as they have not, or should not have, anything to do with biology. I have, however, taken space in this review to give my experiences in the growing, grafting, and breeding of plants and trees to show how they have so often differed from those of the Lysenko-Michurin school.

I have also given some account of the Soviet " Mentors " and other things, having done this I will leave it to the reader himself to decide what " Mentors," vegetative-hybridisation and the like are, and how they work.