The weaknesses of Lysenko's *New Genetics* and the inadequacy of the scanty experimental results on which it is based cannot be disputed. It is understandable, however, that the prestige of such a man, whose earlier work was so brilliant and achieved substantial practical success, should be great, and that he should have a considerable following among lesser biologists and agriculturists. We have had a somewhat parallel case in this country in MacBride, who held equally heterodox genetical views based on equally unsound experimental data, but who retained his chair and published books on the subject until his death. The non-scientific taunts that have been hurled by each side can also be paralleled in previous scientific controversies in other countries. Until Darlington produces his "sufficient documentary authority," or further evidence is forthcoming from other sources regarding the death of Vavilov, the only scientific attitude one can adopt is an open mind on the matter.—I am, etc.,

Orpington, Kent.

F. H. BRIGHTMAN.

SIR,—There are a number of controversial statements in the leading article (Oct. 18, p. 616) on the above subject. I would like to refer to one general question, but I hope that the other points will be taken up by the experts in the different fields concerned. The statement which I would question is the one in which you attribute a comparatively recent growth to an alleged state of philosophic calm on the part of scientific investigators. Surely it was only in times before the introduction of the scientific method that the investigator was able to isolate himself in an atmosphere of "philosophic calm." Such a calm could only be derived from seclusion from the outside world and could only result in the elaboration of an idealist philosophy and dogmatic teaching. As a result of their energetic turning back to reality and their iconoclastic attitude to dogma, revolutionaries such as Newton and Darwin were able to dispel the philosophic calm of the armchair "investigator." From then on, to borrow your own phrase, idealism could only be preserved for scientific workers by an "alogical" process—a divorce between religious views on the one hand and the practical results of their own investigations on the other.

It was by an application of the scientific method to political economy, similar to that which Newton and Darwin had achieved in physics and biology, that Marx and Engels were able to dispel the philosophic calm of the pre-Marxist economists. The essence of their teaching was a refusal to recognize the immutability of the present structure of human society, just as Darwin refused to recognize that the world had been created precisely as he found it in the 19th century. Dialectical materialism has often been dismissed by those who do not understand it, or who find it inconvenient, as a "new religion." There is no doubt that the introduction of the use of meteorology in the countries of the Middle East and India to regulate crop sowings in lieu of reference to the priest is regarded by some as the substitution of one religion for another, since to the uninitiated it is just another mystery. I do not think, however, that this should be regarded as a valid reason for classifying meteorology with alchemy and Zoroastrianism rather than with geology and botany.

You state that doubtless a Communist (among others) tacitly accepts some form of realist or material philosophy. Surely, however, it is evident that, whereas this acceptance may be tacit in the case of a professed idealist—e.g., Oliver Lodge—in the case of a Communist scientist—e.g., J. B. S. Haldane or Joliot Curie—this acceptance is by no means tacit. On the contrary, it is clearly and explicitly expressed by the mere fact that the scientist in question professes himself a Communist and thus declares publicly that he has accepted a materialist outlook.

The recent discoveries in atomic physics and spheres of dynamic biology such as genetics have made the mechanical materialism of the scientists of the last century appear inadequate. This has resulted in a turning back towards idealism, as evidenced by the popularity of neo-mystical writers such as Jung. For those, however, who wish to continue to rely on the experimental and scientific method rather than to seek to explain the natural phenomena of psychology and sociology by some hypothetical "*Einsicht*," dialectical materialism offers a satisfactory method of arranging the facts resulting from

experimental work, a method which, by seeking to expose the contradictory forces operating in a natural process, reveals its dynamic nature. Thus the attention of the dialectical materialist is focused, not upon the ideological conflict between the protagonists of "nature" and "nurture," but upon the organism itself as the seat of the interaction of these material forces.

It is one of the essentials of dialectical materialism as stated by Marx, Lenin, or any other of its main exponents that no theory can be wholly adequate to its purpose and that it will need revision or perhaps complete replacement as new facts are discovered. This applies equally to the theories which Lysenko uses to explain his results and for which no permanent validity can be claimed. The best method of assessing the work of such scientists is, surely, not to accuse them without adducing any material evidence of liquidating their opponents, but rather to point to the successes which have been achieved by the use of their methods in acclimatizing useful plants to the wastes of the Arctic or of the Kara Kum and in evolving new strains of domestic plants which will help to banish for ever the spectre of famine which has haunted mankind for so long.—I am, etc.,

St. Mary Cray, Kent.

BRIAN H. KIRMAN.

The Extent of Neurosis

SIR,—Was there really so little neurosis in the Victorian age as Dr. H. Crichton-Miller (Oct. 25, p. 669) tells us? And has fear, economic and social, really been gradually "eliminated"? and also the response of effort to the fear motive? If developments, as visualized by Dr. Crichton-Miller, should have reduced the extent of neurosis, why have they not?

The replacement of individual anxiety by passive solidarity with the group, and of individual effort by "talk of strikes," could not have led to increased anxiety and conflict unless the individual would subconsciously recognize that his striving for collective security through constitutional channels is either wicked or unwise. Do the working people really suffer from this conflict?—I am, etc.,

London, N.W.11.

B. SAALER.

Treatment of Varicose Veins

SIR,—I have read with much interest the article by Prof. A. M. Boyd and Mr. D. J. Robertson (Sept. 20, p. 452) on the passage of sclerosing fluid through communicating veins of the thigh and leg when injections are made into varicose veins. But I do not think that sclerosis of a deep vein ever occurs in consequence, and it has not occurred in over 20,000 injections that I myself have given. Oedema of the lower leg may occur, but this is due to lymphatic block caused by periphlebitis. Even this will not occur if the leg is firmly strapped up with "flexoplast," which has been my invariable practice for many years now after injecting all but the very smallest leg veins. In fact I consider that it is an essential part of the treatment.

The two main communicating veins are at the junction of the middle and lower thirds of the thigh and at the junction of the middle and lower thirds of the leg for the internal saphenous vein. Of these, the latter is the more important and the more difficult to sclerose. It is usually below this that ulcers and other complications usually occur. If I find it patent after the injection of the main trunk, I inject it directly with 2 ml. quinine and urethane with the leg in a horizontal position, and after pressing on the vein for some minutes I apply strapping to the whole leg. This is usually sufficient to sclerose the communicating vein and the trunk below it, but on occasions I have had to resort to a "twin" injection of "lithocaine," 5 ml., and quinine and urethane, 3 ml., and even this massive and powerful injection has failed to produce any sclerosis in the deep veins, though the local result has been excellent. A running stream will not freeze as readily as the stagnant ditch near it, and it is probable that the increased flow in the deep vein quickly dilutes the sclerosing fluid and renders it innocuous.

I agree with much that Mr. Harold Dodd (Nov. 1, p. 708) writes, especially when he stresses the importance of varicose veins. But I fail to understand his objection to clinics. We who work in them probably see ten times as many cases of varicose veins and their complications as come to a hospital surgeon's out-patient clinic. I once had to deal with 104 in one afternoon, and I rarely see fewer than 40. A considerable amount of experience is thus built up.

I find that injection treatment *properly performed* will give as good results as ligation with injection and better than ligation alone. Many of the cases that come to me have scars of operations—in

one case five—and are in a worse condition than they were before their operations. Moreover, operation is not without its danger to life and limb, and many fatal results have been reported. In seventeen years that I have been doing injections I have had no fatality nor lost a limb. Injection therapy is so simple and safe that scarifying the intima, as described by Mr. Riddock, seems to be unnecessary and even barbarous, though it may give excellent results when performed by his skilled hands. Nor is it necessary to pass a long catheter down a vein to enable a sclerosing fluid to reach its furthest point. If the injection is made into the highest part of the vein, simple massage will drive a wave of fluid down the main trunk, its branches, and also into the communicating veins. In this way the complete internal saphenous system can be soundly and lastingly sclerosed from the groin to the ankle.

But I will admit that in a small minority of cases injections seem to have little or no effect. These unfortunate people must submit to operation or wear stockings for the rest of their lives.—I am, etc.,

London, S.W.7.

R. SIMPSON HARVEY.

Balantidiasis

SIR,—I have read with considerable interest Dr. M. Shun-Shin's article on "Balantidial Dysentery in Rodriguez and its Treatment with Mercury Biniode" (Sept. 13, p. 417). I should like to associate myself with the suggestion of Dr. Shun-Shin, and the following case report would not be out of place.

A Hindu male aged 40 consulted me on Oct. 9, 1947, for an attack of intractable diarrhoea of three months' duration, the number of motions being 20–30 per day. Stool examination revealed *Balantidium coli* and giardia cysts. He was put on emetine injections 1 gr. (65 mg.) daily, methylene blue 2 gr. (0.13 g.) in capsules twice a day, and mepacrine tablets, one tablet thrice daily for five days. With this treatment, though the giardia cysts disappeared, the balantidia continued to be present in highly motile forms. Then on the suggestion of Dr. Shun-Shin I gave an intramuscular injection of 1/6 gr. (11 mg.) of biniode of mercury, and stool examination four days later revealed complete absence of the ciliates. In spite of negative stool findings another injection was repeated, and two subsequent stool examinations on different dates failed to show the balantidia. The patient at the time of reporting had been passing 2–3 normal motions per day and was feeling very well.

The application of hydrarg. biniode in the treatment of balantidiasis has opened up new prospects of cure and eradication of an otherwise very intractable and persistent infection, and further reports in support from members of the profession would be welcome.—I am, etc.,

Santipur, West Bengal.

S. PRAMANIK.

Anticoagulants in Coronary Thrombosis

SIR,—The annotation on dicoumarol (Oct. 25, p. 662) and Dr. John T. MacLachlan's letter on heparin for coronary thrombosis (Nov. 1, p. 709) will, I fear, raise hopes which are doomed to bitter disillusionment. I have nothing but praise for the action of dicoumarol in peripheral phlebotrombosis and thrombophlebitis. Not one of my cases has developed an embolism, and I have a strong impression that the local manifestations subside more rapidly. Although there has been an alarming drop in the prothrombin index in one or two cases, there have been no untoward results.

With this experience of dicoumarol it was with high hopes that I commenced to use it in cases of coronary thrombosis a few months ago. The first two cases in which I used it had each had more than one lung infarct before treatment was started, and each had phlebotrombosis in one leg. Despite a satisfactory drop in the prothrombin index both patients developed further lung infarcts. One occurred when the index was 54%, the second while the index was rising over a period of three days from 43% to 74%. In each case the illness ran a particularly stormy course, and occasioned greater anxiety than the average patient with coronary thrombosis and lung infarction. In addition to these two cases two patients with hypertensive heart failure and lung infarct were given dicoumarol. One of them had two further infarcts during the treatment, the first on the day when his prothrombin index stood at 76%, the other with a prothrombin index of 43%. The second patient had an extension of thrombophlebitis during an intermission in her treatment, and had another lung infarct after the treatment was resumed.

The fifth patient to receive this drug completed my discomfiture and disillusionment. A woman aged 57 who suffered from hypertension with angina of effort was admitted to the ward for thiocyanate treatment. On the fourth day, when her blood pressure had fallen

from 220/110 to 180/94, she developed a severe attack of coronary thrombosis. Within half an hour of the onset of her pain she had profuse haemoptysis, attributed at the time to pulmonary infarction, though necropsy subsequently showed that it was due to pulmonary congestion. Thiocyanate was immediately discontinued and dicoumarol was started two days later. On the eighth day after her attack (sixth day of dicoumarol treatment) she seemed to be progressing satisfactorily, and her prothrombin index stood at 38%, when she suddenly had another attack of coronary thrombosis and died after a few hours. Necropsy showed the original thrombus in a coronary artery with widespread recent extension, which, according to the pathologist, had taken place a few hours before death.

This last case makes it clear that the blood coagulability is not the only factor which influences extension of a coronary thrombosis, perhaps not even the most important factor. Figures which I hope to publish in due course, from a series of 242 cases, indicate that the mortality from all causes within the first three months is closely related to the degree of shock accompanying the attack itself. In 154 cases there was no shock, though pain in many was severe; the mortality in the first month was nil and in the first three months was only 2.4%. In 88 cases with shock the mortality was 27% in the first month (29% at ages below 50, 19% at age 50 to 59, and 48% at ages over 60).

It would seem, therefore, that the most hopeful line of attack lies in the early and effective treatment of the initial shock. I am experimenting along these lines, making use of a slow intravenous glucose-saline drip to which an ampoule of "cardophyllin" (0.48 g.) is added by injection into the rubber tubing. Treatment has so far been confined to seriously shocked patients, and they have been given morphine when required for pain. While it is too early as yet to make any claims, the results so far are highly encouraging. There have been two dramatic recoveries in patients whom I regarded as moribund, and a third striking recovery in one whom I thought seriously ill though not hopeless. One of these three patients later developed an embolism at bifurcation of abdominal aorta.—I am, etc.,

Glasgow.

ALBERT A. FITZGERALD PEEL.

Knee-jerk Examination

SIR,—I have been using a simple method of eliciting the knee-jerk during the last few years. Recently other colleagues to whom I have demonstrated it have shown interest. Their interest has prompted me to describe it. In this method no percussion hammer is required.

The middle finger of the left hand is placed carefully over the patella tendon, and then the middle finger of the opposite hand is brought sharply down on to the dorsum of the left middle finger as in percussion elsewhere. A positive response is felt by a tightening of the tendon under the examining finger.—I am, etc.,

London, W.5.

J. J. SHIPMAN.

Yeast Extracts and Fat Absorption in Sprue

SIR,—I was interested to see the conclusions of Drs. D. A. K. Black and L. P. R. Fourman (Oct. 25, p. 645) regarding the effect of yeast extracts on fat absorption in sprue. Cases such as they referred to were in the Sprue Research Unit at Poona, where their fat balances and treatment were carried out under my direction. The biochemical estimations were performed by Drs. Black and Fourman as described by them. In a group of cases treated by yeast extract, examined statistically by Mr. Dilwater, the period of treatment was shown to coincide with improvement in fat absorption. These cases had been previously treated by parenteral liver extract in large doses, parenteral nicotinic acid, and riboflavin shortly before yeast extract was used. In my view this finding showed that the whole pattern of therapy may have decreased fat excretion, not the yeast extract alone, as has been concluded. For example, the possible necessity for previous or coincidental liver therapy for the reduction of fat excretion on yeast extract is not considered by them in their article.

The issue could be easily settled by the use of yeast extract alone. I would like to ask if Drs. Black and Fourman have investigated cases other than those on the Sprue Research Unit, as yeast extract alone was never used on these cases. I think it