work has, thus far, been very largely left. With some State recognition very important results would accrue which would prove of value to agriculture and the fisheries.

WALTER E. COLLINGE.

ATTEMPTS TO MANUFACTURE SCIENTIFIC DISCOVERY.

IN an excellent article forming one of his admirable series of essays entitled "Science from an Easy-chair," published in the *Daily Telegraph* of December 15, 1914, Sir Ray Lankester deals particularly with the case of the recent proposal that the Lister Institute should be handed over to the Medical Research Committee of the National Insurance Commission. The proposal was rejected on November 18 by the votes of the members; and Sir Ray Lankester preaches a useful sermon upon this text. He maintains that men of science "have no confidence in vague invocations of 'centralisation' and 'co-ordination'—abstractions with which Lord Moulton endeavoured to allure them. They do not wish to see the Institute placed at the disposal of centralisers and co-ordinators. The men they desire to maintain in undisturbed control of the Lister Institute are of a totally different class, namely, the rare individuals known as 'scientific discoverers'—a variety of humanity impossible to drive or to co-ordinate, inevitably paralysed by official programmes and stultified by ignorant though well-meaning superintendence." He continues:—

There is a widespread but erroneous belief in official circles, and among wealthy philanthropists, to the effect that you can hire a scientific discoverer and then say to him, "Discover me this" or "Discover me that" (naming to him a possible and greatly desired piece of new knowledge), and that he will thereupon proceed right away to make the discovery which you want. . . . But valuable and important scientific discovery cannot be produced directly in response to orders given and money expended. You cannot manufacture scientific discovery like soap. The great difficulty, in the first place, is to catch that rare and evasive creature—a scientific discoverer—and when you have found him you have to humour him and let him do as he fancies. Then he will discover things, but probably not the things which either you or he wanted or expected.

All this is very true, and I for one entirely agree. But I think that we should distinguish between *major discovery* and *minor research*. To be frank, the former has been made almost entirely by amateurs, or at least by men who were amateurs when they started; and such discoveries are somewhat rare and depend upon the production by nature of a peculiar and equally rare type of mind. Thus history shows that there are vast nations, consisting of hundreds of millions of people, who never make a scientific discovery from century to century; while, on the other hand, small peoples who appear to be in some favourable biological condition, turn out major discoveries by the score. To me it has always seemed that major discovery is a kind of efflorescence of the

human race, occurring only for a brief period in the life of a people. But minor discovery, or rather research, is of another order. It is not of the epoch-making type of major discovery, but is still useful and must not be depreciated. In the present day, when science has advanced so much, this second class of research becomes absolutely necessary for the purpose of filling in the innumerable petty details which go to complete a great scientific theorem; and the difference of opinion which certainly exists regarding the best method of encouraging discovery depends chiefly upon a failure to make this distinction. Thus, when some of us talk of research, they refer only to major discovery, while others refer only to the minor work.

My own opinion is that both should be clearly recognised. The best way to encourage major discoveries is to remove difficulties as much as possible from the path of the unique individuals who make them; and that is why I have always advocated a proper State recognition of such work. On the other hand, minor researches do require a certain amount of organisation—though, even here, great care must be taken not to interfere by too much direction from above; and the question is what kind of organisation is the best. At any rate, most men of science will agree with Sir Ray Lankester when he records a note of objection to "the existence of a Board of Trustees with a Managing Committee, or of any committee playing the part of employers and proprietors towards the men of science who are heads of laboratories in an institution designed for scientific discovery."

Personally, I think that our British notion of constructing such committees chiefly out of "superannuated politicians, retired civil servants, lawyers, medical men, peers, and clerical dignitaries, as well as men who have made fortunes and retired from business," is quite foolish, and indeed improper. But we seem to pursue this habit in nearly everything—the argument being apparently that the men who know nothing about a subject are the best to direct efforts in connection with that subject; so that we appoint lawyers to be heads not only of scientific and learned institutions; but even of the War Office and the Exchequer. In my own humble opinion, lawyers would be much better employed in revising or reforming the mass of confusion (as they themselves admit it is) called law. I do not object to lawyers more than to other amateurs being placed in such positions; but the point is whether research institutions should not be put exclusively under the management of men who have proved their capacity for research by success in it—and only under such. The existing British custom has an unpleasant savour of secret wire-pulling and other methods of acquiring "influence." It would be quite useful, were it possible, to analyse the committees of our few research institutions; but the time has come when men of science should begin to look into all these matters a little more closely than they have done in the past.

RONALD ROSS.