ever sums are contributed from other sources up to the amount of the pledge.

DR. TORALD SOLLMANN, professor of pharmacology at Western Reserve University, has been appointed dean of the school of medicine. He succeeds Dr. Carl A. Hamann, whose request that he be permitted to resign was regretfully acceded to by the trustees.

THORNE FITZ RANDOLPH has resigned his position as assistant principal and instructor in chemistry in the Franklin High School to accept a position as professor of chemistry in charge of the chemistry department of the Indianapolis College of Pharmacy, Indianapolis.

DR. HAROLD B. FRIEDMAN, instructor at the University of Maine during the past year, has been appointed research assistant in chemistry at Columbia University for the coming year.

C. A. HOPPERT, research chemist for the Soft Wheat Millers Association, has become associate professor of biological chemistry at the Michigan State College.

ERNEST VICTOR JONES, formerly science adviser and professor of inorganic and physical chemistry at the University of Nanking, China, after spending a sabbatical year at the University of California, has accepted the position of head of the department of chemistry at Birmingham-Southern College, Birmingham, Alabama.

DISCUSSION AND CORRESPONDENCE

PHYSIOLOGY AND MEDICINE

THE address of Professor C. A. Lovatt Evans on the "Relation of Physiology to other Sciences,"[1] delivered before the section on physiology of the British Association for the Advancement of Science this summer at Glasgow, presents questions of very great interest, especially to me, from the point of view of the relation of physiology to medicine, that is to say, to the study of disease. Professor Evans has in general adopted very liberal and on the whole just views of the relations of physiology and medicine. His address was concerned with this particular relation as one only of the many intellectual contacts of physiology with the world of science. But it is a relation which has occasioned much discussion in times past, and still continues to do so; one furthermore about which there is yet no unanimity of opinion, either in the United States or elsewhere. In England, especially, traditional views have regarded the position of medicine in a scheme of knowledge to be somewhat different from the one which is now held by the Medical Units in London and elsewhere in the United Kingdom at least in part, and certainly by three university clinics in the United States, at Harvard, Chicago and Cornell, and at the Rockefeller Institute in New York. If I single out this subject for discussion from among the many interesting ones about which Professor Evans spoke, I do so because I believe this issue is still unsettled and important and because I think something is to be gained in the interests of the general understanding of the problem by directing attention to it again.

The prevalent view in the United States is that one of the great functions of the university clinic is the effort to get on with enlarging and deepening knowledge of disease. We believe that the person most likely to do this is the person who has elected this to be the great interest of his life and work. The meaning of what has been so badly labeled the full-time position exists, in the view at least of three of the institutions just mentioned, for the purpose of affording opportunity to devote themselves to this end on the part of those professors of medicine who accept their posts with the awareness that this is at least one of the great purposes of their calling. Those professors who adopt this interpretation of their posts must necessarily adopt also, perhaps not always consciously, a definition of medicine, meaning by medicine in this particular instance the study of disease.[2] They wish to be so trained and so to train those students who elect to follow in their scientific footsteps as to master whatever technique, whether physical, chemical, physiological or immunological, is necessary in order to advance their pursuit. They are in no other situation in this regard than professors of biology or physiology or of any other scientific discipline. Nor are they under any illusion concerning the difficulty of their undertaking.

Their attitude toward the study of disease is, it seems, different from that which Professor Evans assumes or at all events discusses. I believe that in his remarks there is failure to distinguish between persons and disciplines. A discipline, conceptually, is a unit conditioned by its subject-matter; its unity does not depend on what the training and antecedent interest are of the person who cultivates it. It may very well be that an anatomist or a physiologist may work at disease or that for reasons of his own a physician may study anatomy or physiology. That is his own affair and does not concern the logical structure of the sciences. Some of us in America have appre-

[1] SCIENCE, 1928, 68: 259–264 and 284–291.

[2] This definition is discussed at length in my paper, "Medicine and Science," *Journal of Philosophy*, 1928, XXV: 403, and need not be elaborated here.

ciated the fact, or think we have, that there is advantage in facing this issue. We have accordingly provided the means for making it possible in each discipline, including medicine as the study of disease, or pathology, to use the English designation, to bring discipline and person together. He who is interested in anatomy may, and usually does, profess anatomy; he who is interested in physiology may, and usually does, profess physiology; he who is interested in disease may and, if we are to get on with knowledge of it, should profess medicine.

Professor Evans is well aware that medicine is the mother of the sciences. He knows how chemistry, anatomy and physiology all had their inception in the world's general interest in disease. So they began. They grew and soon conquered provinces of their own. That is the meaning of the separate institutes these disciplines now so often possess. But now, although physiology has made itself independent, Professor Evans still harbors fears. He fears to cut the guiding strings of the alma mater, lest physiology lack nourishment. And like many, especially modern, children he fears lest the ancient mother be too feeble intellectually and too powerless, having reared and weaned her children, to be able to continue to order and to develop her own house. But the situation is just this: having learned as it were and indicated to her many offspring how they might best set up houses of their own, medicine is at length free to cultivate her own garden. In America in a tentative and prayerful way, despite many hardships and much misgiving, we in medicine are at work upon our proper domain—proud meanwhile of the children of medicine and when we require it, as naturally we often do, eager for their support. But we want to be so equipped ourselves as to be able to cultivate our own garden. What we mean by this is that the idea is dawning that the study of disease is, or may be, something not necessarily coextensive with practice; that it may be pursued as a phase of disinterested learning. There is in short a difference between the practice of medicine and the academic study of disease just as there is a difference between academic physics and practical engineering. Both interests are essential; both have legitimate human value, though they engage the attention of individuals differently equipped.

Professor Evans believes that "the physician's duty with regard to it [disease] is a threefold one: he must diagnose, prognose and treat." And concerning the "two important principles" of treatment he has this to say concerning the support medicine receives from physiological knowledge:

> One is that the consequential alterations which take place in the course of the disease are of the nature of adaptations which tend to restore the function to normal; these adaptations take the form of increase or diminution of some particular factor, of hypertrophy or atrophy often of some definite organ, always of some function—it is, in fact, the *Vis medicatrix* of the older physicians, the underlying principle of expectant treatment. The other principle is that nearly all positive measures of treatment, including drugs, produce their effects by augmenting or restricting some function or other.

This undoubtedly is one way of regarding the happenings in a diseased body. But may not another one also be urged, one which has indeed been urged by me in the paper to which I have referred. The view which is there taken is that a disease, instead of being merely a quantitative deviation from health, is a collection of new phenomena, a new complex, and is sufficiently different to be regarded as a qualitative change. Whitehead has illuminated this point of difference between quantity and quality when he says that "In the past human life was lived in a bullock cart; in the future it will be lived in an aeroplane; and the change of speed amounts to difference in quality." So it is in disease. And if this is so, whose business is its study so much as it is that of the physician devoted to this pursuit, in our case the university professor of medicine, whose it has always been since there have been university professors, and who brings to its contemplation his undivided interest?

ALFRED E. COHN

HOSPITAL OF THE
ROCKEFELLER INSTITUTE,
NEW YORK CITY

PRELIMINARY NOTE ON THE LIFE HISTORY OF HYMENOLEPIS CARIOCA

EXPERIMENTS on the life history of *Hymenolepis carioca* have established in a preliminary way that one of the dung beetles, *Aphodius granarius,* serves as an intermediate host. Beetles fed with eggs of this tapeworm developed cysticercoids in the body cavity and tissues, and when such infected beetles were fed to chicks some of these birds showed the presence of *H. carioca* ante mortem and post mortem; control birds under the same conditions but not fed infected beetles remained free from all helminths.

Previous studies in the published literature report the development of this tapeworm in chicks fed wild stable flies presumably naturally infected, but in these experiments larval stages were not found and the evidence that the stable fly is a host is incomplete. The results reported here are important in view of the work based on results from feeding wild insects to chicks kept in fly-proof cages, or attempts to raise chickens free from tapeworms by using screened enclosures, since such small beetles as