

of the students as possible are given definite work in the wards as dressers and clerks, under the name of "Co-assistenten." In the larger universities—for example, Berlin and Munich—it is only possible for a comparatively small proportion of the medical students to work in the wards as Coassistenten, but the smaller universities, nineteen in number, which are much more typical of German medical education, give infinitely more opportunity in this respect.

The recent institution of the "Praktisches Jahr," which every doctor is obliged to undergo after his qualifying examination, shows clearly that we in Germany are increasing the practical character of medical education. The author of the note in the *Muenchener med. Wochenschrift* expresses the opinion that the amount of practical work should be increased as much as possible in accordance with the English system, an opinion which I held myself as a result of my more intimate acquaintance with American educational methods.—I am, etc.,

Munich, March 23rd.

FRIEDRICH MÜLLER.

STANDARDIZATION OF PANCREATINS.

SIR,—The two letters under the above title from the respective pens of Mr. F. F. Shelley, F.I.C., and Dr. P. J. Cammidge, in your pages of March 9th and 16th, reveal a very serious state of things which has been known to some for several years past. Dr. Cammidge writes (p. 647), "A few years ago I tested samples of all the commercial preparations of pancreas that I could meet with, and was surprised to find what a large proportion were inert." As injections of pancreatic ferments first appeared in medical practice early in 1906 Dr. Cammidge possibly did not refer to such preparations, but it is an undoubted fact that were one to substitute in his letter for "preparations of pancreas" the words "preparations of pancreatic ferments for injection," his statement that "a large proportion were inert" would also be true. None the less, by all the writers who have used such inert preparations—without even knowing that they were inert—and who have dealt with their supposed actions or lack of actions in their writings, it has been assumed that the true character of a pancreatic preparation can be determined merely by reading the label. But, indeed, something more than "standardization" of pancreatic preparations is called for, since, even when "standardized" and put up in ampoules, the further question arises as to the length of time during which particular preparations retain any of their activities. In recent years the pancreatic ferments have been treated as though they were ordinary "drugs," which as a rule retained their properties unaltered, ignoring the fact, well known to some, that they are extremely delicate bodies, which, as active agents, lose very quickly all their original powers and become quite inert. Therefore it is not necessary to suppose that "preparations of pancreas," such as those mentioned by Dr. Cammidge, or injections of pancreatic ferments, had originally (when made up) no ferment powers at all. Possibly in all cases they had some powers, even on occasion great ones, but all the chemical evidences I have seen would lead me to believe that something or other in the manufacture of such inert preparations had led to their instability. On mentioning some of the facts concerning such inert ferment preparations to a well-known Glasgow surgeon, he said that it was my "duty to see that this was put right." Well, with deference to him and to you, I venture to think that this "duty" falls more appropriately and very much more effectively to the lot of the Editor of the BRITISH MEDICAL JOURNAL.

Not many weeks ago the writer received unexpectedly certain results of assays of various pancreatic preparations manufactured and sold in England, and the sender, who is a specialist in the study of ferments, remarked that probably the figures would be found "astounding." Indeed, an impartial examination and assay of all the pancreatic preparations at present on sale in this country would, without doubt, give results regarding certain of them not only "astounding" in character, but such as would put quite in the shade similar examinations of patent medicines. This question has been allowed to drift for years, and it is only common sense to ask "How long still shall it be possible to sell as active pancreatic prepara-

tions things which often are almost, or even in many cases quite, inert?"

Edinburgh, March 26th.

J. BEARD.

THE NEW CELL PROLIFERANT.

SIR,—Mr. H. C. Ross's two statements (p. 523 and p. 646) with regard to the circumstances connected with my refusing to allow my illustrations to be used for a book he is writing differ from each other as to matters of fact. They cannot both be true. I never for a moment supposed that Mr. Ross had seen the only letters I wrote upon this subject, as they were all marked "private." Mr. John Murray, to whom they were written, has, however, given me a quite unnecessary assurance, unasked for, upon this point, so it is evident that Mr. Ross was not and is not in a position to make the statement he did. Unless it be discourteous to refuse any request made by Mr. Ross personally or by proxy, my letters to Mr. Murray were not discourteous.

As to the rest of Mr. Ross's arguments and pretensions, it is difficult to deal with any one who so continually shifts his ground and disregards pertinent criticisms. On March 2nd he complains that "scientists" obstruct his progress by adverse criticism without repeating his experiments. When I point out that I have repeated them and produced the results he describes, but that these results cannot be made to bear the interpretation he puts upon them, he says that "the technics are very difficult," and as I may not have worked upon the correct lines, he cannot accept my opinion. Mr. Ross took some 280 pages to describe in minute detail a technique which is full of sources of error, but I followed it with sufficient accuracy to produce results which agreed in detail with both his descriptions and his illustrations. The difference, then, between him and those "scientists" of whom he complains lies in the interpretation of the results following upon the application of his "technics." What are these results? That when living cells (in Mr. Ross's experiments almost invariably mammalian leucocytes) are placed in certain abnormal environments each of them breaks up into two or more parts. Mr. Ross's interpretation of this is that he has discovered certain substances which so act upon the cells as to induce in them the phenomenon of mitotic or indirect division, a process of which the details have been the constant study of some of the best known biologists for more than thirty years. The acceptance of Mr. Ross's interpretation involves the abandonment of practically everything that has been unanimously accepted by these men, and the assumption that they have one and all shown themselves absolutely incapable of observing the simplest matters of fact. One example of what has to be swallowed in order to agree with Mr. Ross will be sufficient: "The so-called nuclei of leucocytes ought, we think, in reality, always to be called centrosomes, and the word 'nucleus' deleted from their morphology."¹ One might be inclined to consider such claims more seriously did Mr. Ross in his writings show any acquaintance with the work in this particular line which has been done in the past, some of it before he was born. One example will again be sufficient. We are told that: "There is no doubt that the observation of the living cell is a new study."² Every biologist knows that, as a matter of fact, it is about as old as the cell theory itself.

On the other hand, it is a matter of common knowledge that when a living cell is placed under certain conditions of environment it is broken up into two or more parts. The difference between this phenomenon and that of mitosis is the difference between the verb passive and the verb active. It is the verb passive which Mr. Ross describes and illustrates, and which I have seen when using his technique. His results correspond with the results of osmotic and other disturbances, and could mislead no one who was at all intimately acquainted with cell phenomena; they are by no means surprising and can be produced by many other methods than those he uses; they bear not the slightest resemblance to mitotic divisions. To interpret the fact that because leucocytes are divided as they are in his experiments, they have been induced to divide mitotically, is just as reasonable as to claim that a man

¹ *Induced Cell Reproduction of Cancer*, p. 13.

² *Ibid.*, p. 10.