COMMENTS

by Reader's

Pennsylvania State College is to be congratulated for establishing a Laboratory of Applied Geophysics and Geochemistry (**Science,** November 1, 1946, 422). This action is forward looking and indicates an awareness of present and future problems in the discovery and wise exploitation of supplies of the chemical elements both common and rare. Other educational institutions are organizing groups of teachers and research workers with similar objectives.

It is, indeed, true that geochemistry is becoming recognized in American academic institutions as a borderline field worthy of intensive study and research.

It is also true that geochemistry, under that name, has not, in the past, been widely recognized by American scientists. However, the neglect has not been so marked as to justify the statement, "For the first time in the history of American academic institutions a course in geochemistry will be given at Penn State."

Research in geochemistry, particularly as it applies to the less familiar elements, has been conducted at Purdue University under the direction of D. W. Pearce since 1935. (H. B. HASS, *head, Department of Chemistry, Purdue University, Lafayette, Indiana.*)

There are certain fundamental objectives to an organized program of cancer research. As Dr. Kosolapoff points out (*Science*, November 22, p. 491), our military use of nuclear explosives was due to an accidental discovery that a particular nucleus, under a particular treatment, would result in a fission of special properties, especially needed at the time. As Dr. Kosolapoff says, "On the basis of knowledge in the 1930's the finding of such a nucleus may well be called fortuitous."

He says that in the case of penicillin chance determined the first observation of antibiotic effects, but once the goal was perceived, mass methods in taking advantage of the fortuitously acquired knowledge was a foregone conclusion.

He points out that cancer research is not analogous to the programs of research and development in the fields of atomic fission and penicillin because in these latter projects the basic discoveries had already been made and only required "development," which could be accomplished fairly rapidly by mass effort.

This statement carries a great deal of truth, but as far as penicillin is concerned, it is not the whole truth. It is indeed true that Fleming's original observation of a *penicillium* contaminant on an agar plate inoculated with mold-sensitive staphylococci was the result of chance. Yet 11 years elapsed before Chain, Florey, *et al.* at Oxford initiated the further research which was ultimately to demonstrate the clinical possibilities of penicillin and to stimulate American workers to solve difficult problems of production.

Why did a delay occur? Because the basic observation was not enough to do more than suggest the possibilities ahead!

Further research was needed before the goal could even be seen very clearly. In the meantime, 11 years were lost because no one happened to have the right combination of motivation and equipment to follow an interesting lead.

Yet, if there had happened to be in progress an all-out search for some therapeutic agent for combating staphylococcus septicemia, this lead would certainly have been explored much earlier.

Dr. Kosolapoff next points out that there is no clearly defined line of approach to a solution of the cancer problem. This also is doubtless true, but there are certainly some promising observations that justify further exploration as much as did Fleming's original observations on penicillin.

To mention only two, we may note the selective effect of impure penicillin on sarcoma cells in tissue culture and the reported inhibiting effect of certain estrogenic substances on carcinoma of the prostate in males.

There are others of equal or greater significance.

But even if there were none, it is hard to see how that fact could constitute an argument against a well-organized National Cancer Research Program.

There are numerous opportunities for fundamental research. Two promising fields for investigation are: the use of the radioactive tracer technique in a study of the metabolism of normal and malignant cells; and an investigation of the blocking of certain enzyme systems of mammalian cells in tissue culture by structural analogues.

This is not to say that competent investigators are not already working along these lines, as well as others, equally promising. But the problem is too big, and the need too great, to leave it all to uncoordinated individual groups. There is no reason why the necessary *fundamental research,* cannot be expedited by well-directed teamwork as well as the so-called *development* phases.

Dr. Kosolapoff suggests that because we do not *yet* see an obvious approach that is bound to lead to a practical solution, there is nothing to be gained by a mass effort.

It is my contention that there are worth-while leads crying to be followed and that there is fundamental work which, when completed, cannot fail to suggest other lines of approach. If all of this work is left to be accomplished by a leisurely peacetime research program, it may take decades. There is a serious shortage of well-trained investigators in this field which will still further hamper this work. What is to stimulate increased interest and provide for the training of the number of new workers needed if not a national program of cancer research? It is well recognized that fundamental research provides the new observations and discoveries which serve as a basis for the development of new applications!

Fundamental research in the case of cancer does not have to proceed slowly because of a lack of new ideas. There are plenty of them. It is slow because of the shortage of personnel, facilities, and funds. Given these, the now difficult experimental techniques will become simple procedures. (K. S. PILCHER, *associate director of research, Cutter Laboratories, Berkeley, California.*)