be fooled by surface water. A thin rich vein of worthless pyrites might prove an exciting discovery to a geophysicist, while the mine manager would view it with cold disdain.

The fact, however, remains beyond a doubt that good conductors can be located underground by several different electrical and electromagnetic methods, while they could not be detected by magnetic methods, and that suitable schemes may, and probably will, prove to be of great service alike to mining men and to geologists.

In games like golf and billiards, and in the more serious hazards of war by land or by sea, as much, or truly much more, depends on the man than upon the clubs, cue, weapons and ships—on the material things which he has at his disposal. For the expert and skilful man will insist on using to the utmost the very best, and on its maintenance at the very best. So too in geophysics a torsion balance, or a magnetometer, does not make a survey. These things are subsidiary to the skill and intelligence of the man who uses them, who understands their possibilities and limitations, who interprets their readings wisely. Since these things are true, the greatest country or state will always be that which develops most properly the real intelligence of its children and youth—always the sole greatest asset of any people.

To return to our main subject—geophysical prospectors sometimes claim too much, mine managers often expect too much. Disappointment leads them to join the scoffers. That is not the road to progress! There must be mutual confidence and cooperation between managers, engineers, geologists and physicists. So far none of them has proved infallible; all have to play the game of "blindman's buff" or "hoodman blind." All have to search with all the scientific aids possible. Diamond drills can not be used over the whole face of the earth. The day may come when the geologist will go before, and the geophysicists will follow after, next come the engineers with diamond drill, and behind them all the other men who, with joy and singing, will gather up most of the dollars.

A. S. EVE

McGILL UNIVERSITY

THE MULTIPLE ORIGIN OF TUMORS

PARTLY from clinical observation, partly from the intensive experimental work on neoplastic disease which has been carried on during the last quarter century, we now know of a considerable number of means by which tumors, particularly malignant tumors, may be artificially induced. These come under several distinct categories.

First, certain chemical irritants may induce them with some regularity. Coal tar applied to the skin over long periods of time, or injected into the tissues; indol; various arsenic compounds; and—a matter here of clinical observation—various aniline products —all have the effect of stimulating tissues to malignant hyperplasia, in some cases at least preceded by a period of benign overgrowth.

Second, physical irritation, best manifested by long applications of the X-ray, has the same effect. Third, embryonal tissues introduced into the adult animal may in certain circumstances develop into malignant tumor; best, perhaps, when to the effect of transplantation is added the element of chemical irritation, as by coal tar or indol. Fourth, malignant tumors may develop, as shown by Maud Slye, purely on the basis of hereditary factors. We may have malignant tumors induced by certain nematode parasites, acting in a manner as yet not fully determined. And finally, rather recently it has been shown by Blumenthal and his coworkers that in a certain proportion of human cancers *B. tumefaciens* may be isolated from the outskirts of the tumor. This, grown in pure culture and inoculated into plants and certain animals, may cause what to all appearances are tumors in them. In the case of the animal inoculations, sections show a fairly definite picture of malignant neoplastic growth, and on transfer to other animals of the same species the new growths behave like typical inoculable tumors. Of some significance in this connection is the fact that the organisms disappear in the later stages of the tumor, and in those resulting from transfer.

Instead of being in ignorance of the causative factor of neoplastic growth, we are really in a position of embarrassment at having too many possible causes, and the real problem in connection with the etiology of tumors would appear to lie in the reconciling of these to a single common factor. That there is such a common factor can not be questioned; the entire picture of neoplastic disease, both benign and malignant, is too definite to permit doubt on that score. As a matter of fact, the nature of that factor is shown in the histology of all tumors—it lies in their common possession of the property of more or less unrestricted growth—absolutely unrestricted in the case of the more malignant ones.

Viewed in this light, neoplastic proliferation must then be considered as a common type of reaction to a variety of causes—a reaction characterized by the more or less complete suppression of the usual normal balanced cellular activities with a corresponding accentuation of the single activity of cellular multiplication. In the sense of being a reaction to injury, tumor development would then be simply a special type of inflammatory phenomenon—one which is shown originally by the single cell or group of cells, as a result of which it loses its normal environmental

inhibitions and becomes capable of free and unlimited growth.

If this view is right, then neoplasms, and especially malignant neoplasms, must be regarded as tissues which in response to a number of different irritants react by showing a release from the normal growth gradients which ordinarily regulate body structure. The real problem of cancer then becomes a study of these gradients, and from the therapeutic standpoint their reestablishment when once lost or the prevention of their loss. The present line of study, largely devoted to the determination of the character of these irritants, will of course always be a matter of importance, but of secondary rather than primary grade.

Some of our therapeutic measures in the control of cancer are already directed to the former end. The use of radium and the X-ray, for instance, is essentially an effort to accomplish two things which tend toward a reestablishment of lost gradient—the inhibition through destruction—at best usually partial—of the unrestricted cell division, plus the stimulation of connective tissue growth to the point where this more nearly equals the proliferation rate of the tumor cells.

It would seem no longer correct to speculate as to the "cause of cancer." We would seem to have reached the point where it is necessary to recognize that there are a number of distinct causes, related only in the sense that they produce a common effect. Neoplasms then constitute an entity in the same sense that acute inflammation is an entity—a single type of reaction brought about by a variety of causes—and like that, an inflammatory process in that it is a reaction to injury.

To prove this experimentally aside from the finding of a still greater number of causative factors will be a difficult matter. One possible means would lie in the establishment of immunity, as to *B. tumefaciens,* and the demonstration that this immunity did not protect against other causes of cancer, as for instance, coal tar.

H. E. EGGERS

UNIVERSITY OF NEBRASKA
COLLEGE OF MEDICINE

THE CUTICULA OF NEMATODES

IN an abstract published in the December, 1927, number of *The Journal of Parasitology,* on "The Cuticula of the Neamathelminthes," Justus F. Mueller states that he has found the cuticula of Gordius and Macracanthoyhynchus to be chemically homogeneous and that of Ascaris to be separated into two chemically distinct substances. All four substances are proteins of albuminoid character, none related to chitin. The three substances found in Ascaris and Gordius are fairly similar, while that of the acanthocephala is different. He states that he does not agree with me (*Camallanus americanus,* nov. spec. *Trans. Amer. Microsc. Soc.,* 1919, 38: 49–170) in calling the substance in Ascaris cornein, and states I was in error because of incorrectly translating Reichard.

Since the abstract does not call attention to the point I was attempting to make in my study, it would leave in the minds of those not familiar with the facts an incorrect idea. In the first place my error, for which I apologize, was due to my misconstruing the force of the subtitles of Reichard's paper; I thought at that time the heading "Cornein" was intended to refer to the cuticula of worms and my error was not one of translation. The whole purpose of my contribution was to show that the cuticula of nematodes was not chitin but was a protein of albuminoid nature. In this I am glad to see that Mueller agrees. Cornein is also an albuminoid. I had been taught and had read in many commonly used texts, in the article written on nematoda for the Encyclopedia Britannica by Shipley and Beddard and in articles on nematodes by such men as Ransom, Hall and Ward that the cuticula of nematodes was chitinous. Indeed some zoologists, as proof of the supposed relation of worms and arthropods, stated that both had chitinous covering. As pointed out in my paper, Leuckart was undoubtedly responsible for the misconception, although he knew that the two coverings were fundamentally different. In spite of the fact that men as far back as Lassaigne (1843) pointed out the difference in the cuticula of Ascaris and the chitin described by Odier, authors still referred to the covering of nematodes as chitinous. My study was undertaken to show conclusively the differences in the structures. I concluded that the cuticula of nematodes was composed of a protein of the albuminoid type, closely related to connective and supportive tissue and unrelated to chitin. In this I carried on further the work of Reichard and agreed with him in his work, but incorrectly stated that he called the substance cornein.

Mueller states that he has for the first time correctly analyzed this material, basing his statement on the fact that he analyzed the two parts separately, unless he means to imply that the actual analysis of all other authors was erroneous. The former statement depends on the point of view. By his own statement the two parts form the cuticula; therefore to analyze them together would certainly constitute a true analysis of the cuticula, just as, in analyzing the liver or spleen, one does not separate them into their many components. Reichard called attention to certain physical and chemical differences in two layers of Ascaris and I described four layers for Camallanus. Others have subdivided the cuticula of nematodes into