

IF CANCER IS CURED SOMEDAY—

It May Be Due To Detective Work Like This

—ATLANTIC CITY.

AT the center of the tangle of mysteries that constitute cancer is imbedded the following conundrum. A cancerous cell is the offspring of a normal cell. Therefore, a cancerous cell is a mutant cell. The evolutionary process of life requires that the body of every person at one time or another if not constantly contain thousands of mutant cells. The great majority of these mutants are somehow disposed of harmlessly. What is nature's disposal system and why doesn't it get rid of all the mutants?

The answer to the riddle must lie in a set of circumstances that favors multiplication of normal cells at the expense of the "sports." In the belief that such circumstances would have to depend on a particular substance or substances which selectively nourish normal cells and poison abnormals, many researchers have hunted individual chemicals capable of recognizing the presence of cancer in the midst of an otherwise healthy body and further able to remove the unwelcome guest without injuring the host.

The search for the wise chemicals has been long, tedious, expensive, and frustrating. It has been conducted with exceedingly small imagination and (I say this hopefully) may be slowed down or stopped altogether as a futile extravagance because of something that happened in this Atlantic seaside resort in mid-April.

The happening was a report of discovery of a set of circumstances in which the growth of cancer cells in laboratory retorts is inhibited in favor of the growth of normal cells.

As might have been predicted by anyone familiar with the history of science, the discovery did not arise from a bottle-by-bottle sampling of chemicals on a shelf; the finding came in the logical course of seeking understanding of a natural event.

What can be told of the yet unfinished story at this point was told to the 49th annual meeting of the Federation of American Societies for Experimental Biology by Dr. Jorgen Fogh, Danish-born chief of the Sloan-Kettering Institute for Cancer's cell research laboratory in Rye, N.Y. Speaking for himself and

a laboratory collaborator, Mr. Bruce Allen, Dr. Fogh traced the following path to what could turn out to be one of the rare moments of decision in the history of healing:

Eight years ago a global search was made for a living tissue that would be a superior substitute for monkey kidney as a culture on which to grow the poliomyelitis virus. In Nobel laureate Wendell Stanley's lab on the Berkeley campus of the University of California, Mrs. Elsa Zitser, a virus lab technician then working with Dr. Fogh and a graduate student, Miss Thelma Dunnebacke, found the ideal stuff. This was the amnion, the inner lining of the human placenta.

The powers that rule the polio world decided against using amnion tissue despite the fact that amnion is the only human tissue that has outlived its natural usefulness once it emerges from the womb with the newborn child. Before this decision became known, however, Dr. Fogh's interest in the amnion deepened for another, more fundamental reason. He had discovered that after two or three months of growing, the normal amnion cells were invaded by small colonies of mutant cells. The number of the intruders was low—perhaps one stranger to every 10,000 normal residents—but the phenomenon was persistent.

Were the strangers cancer cells?

Dr. Fogh took the question and its various implications with him when he left the Stanley lab to move eastward. A year and a half ago he joined the Sloan-Kettering staff and settled down to the task of digging out the answer. In the course of determining that the answer was positive, he ran into far more fascinating questions.

The amnion is only one cell thick. It is stripped from inside the placenta and placed in a solution of trypsin, an enzyme which dissolves the membrane into constituent cells and suspends the cells as individuals in the liquid. It is then possible to remove as many cells as desired and put them into flasks with food on which to grow and multiply. The walls of the flasks quickly become covered with sheets of descendants of the original cells.

Dr. Fogh grew many sheets of normal amnion cells in his lab flasks. He also grew many sheets of amnion malignants, which he christened with the two initials, FL. In their separate habitats, the two types of cells displayed no particularly memorable behavior. But when he placed small colonies of amnion malignants in the same flasks with normal amnion cells, he observed the following series of remarkable occurrences:

(1)—The greater the number of malignant colonies he introduced, the less each such colony would grow.

(2)—The greater the number of normal cells present, the fewer were the malignant colonies and the smaller each colony.

(3)—The malignant colonies would destroy normal cells around them—up to a certain distance. But the area of destruction did not increase in proportion to the number of new recruits Dr. Fogh rushed in for the attack.

All three of these events could logically be explained by a shortage of food. But Dr. Fogh saw to it that there was always plenty of food on hand to go around, with some to spare.

It seemed obvious to him that something was inhibiting the growth of the amnion cancer cells. It seemed equally plain from the sequence of events that the something was produced by the normal cells after they were attacked by the malignant cells.

SOMEWHERE, then, in the liquid surrounding normal and malignant amnion cells living together, there must be a substance or substances possessing the ability to discriminate between normal and cancerous growth and to curb the cancer without affecting the healthy cells.

The identity of the substance or substances has not yet been established.

The exact circumstances which set it or them to work is likewise unknown.

The way the work is done is another mystery.

But man for the first time has found a something with the power to stop one form of cancer while doing no apparent harm whatever to normal growth.

—WILL JONATHAN.

LETTERS TO THE SCIENCE EDITOR

THE ROCK TIDES

WITH REGARD to the interesting article by Woodruff and Goering ["Do the Mountains of Earth Come from the Moon?" *SR*, Mar. 3], and your note on their lunar mountain-building theory, I would like to make a few comments.

Certainly the Woodruff-Goering idea is of interest, and can, at least in part, be verified. The atomic structure of metals (their analogy) is far different from that of crystals, and even more different from that of rocks. Solid-state ionic diffusion in a crystal under a weak stress field will be much less than in a metal because of the greater degree of crystal order. Ions do not "flow" under bending stresses with the same order of magnitude as in metals because there is a strong energy barrier to be overcome. Furthermore, an intermittent stress (lunar) will be far less effective than a continuing stress. Any intra-crystal effects would be surprising.

Other problems arise too. There is no evidence that crystal strain has occurred as a cause of mountain-building, but rather as an after-the-fact *resultant* of tectonic forces. What explanation can lunar tidal forces offer for the distribution and linearly arcuate shapes of orogenic belts, even considering the possibility of continental drift? If the moon force were to build mountain chains on the earth, the much greater earth force should produce huge differences on the moon. The little evidence available suggests that the reverse side of the moon is not vastly different [from the hemisphere that faces earth].

During early stages of earth formation, before great crustal thicknesses existed, the lunar force would have been more effective than now, but perhaps still insignificantly so. Viscous media or glass-like structures would seem more amenable to such an hypothesis.

However, regardless of the actuality, *SR* is to be congratulated for its presentation of stimulating ideas in science. I know of no other popular, literary magazine with this coverage.

DAVIS M. LAPHAM,
Mineralogist,

Pennsylvania Geological Survey.
Harrisburg, Pa.

EDITOR'S NOTE: *The senior author of the mountain-building theory, Dr. Ray Woodruff (he is, incidentally, a professor of chemistry at Montana State College who taught geology at the college for several years before the study of geology was separated from the school's chemistry department) offers the following points in rebuttal to the above letter: (1) the theory depends more on the limited experimental experience with rock-like ceramic and cement than on metals; (2) since the earth is a globe, and the tidal pull of the moon is felt on a global scale, arc-shaped belts of strain and surface buckling along the edges of broad sweeps of ocean would be logical concomitants of the theory; and it is precisely in such locations that we see arcs of volcanoes and earthquake activity; (3)*

the moon is not thought to be plastic, as the earth is; in any case, since the same hemisphere of the moon always faces the earth, whatever effect there may be of the tidal pull of the earth on the moon probably takes the form of a fixed bulge rather than a moving tide.

HUDSON ONLY FOUND IT

IN THE APRIL 7 issue of *SR* you use the name Hudson's Bay.

I have always thought it to be Hudson Bay (but the Hudson's Bay Company).

Will you check this?

JOHN A. LIVINGSTON.
West Haven, Conn.

EDITOR'S NOTE: *Hudson Bay is correct.*

A GOOD NAME

WITH REGARD to the sketch on myself [Personality Portrait LXXIII] I feel that I was given rather more than justice. I note that you refer to me as John Beals and while I am fully aware of the religious, journalistic and political eminence associated with the name John, I am afraid that I cannot lay claim to it. My name is Carlyle Beals and I was named for Thomas Carlyle for whom I have a limited admiration. While this name may not be as good as "John," nevertheless, it is my name and I propose to stick with it.

May I congratulate you on the interesting scientific articles in *Saturday Review* and offer you my best wishes for the future.

C. S. BEALS,
Dominion Astronomer.
Ottawa, Canada

QUIET, PLEASE

I AM OF the opinion that *SR* should not publish scientific articles devoted almost wholly to the speculative views of the authors on a particular subject. Rather I would urge the publication of *Review* articles in which the authors can, in a natural and balanced way, include a discussion of current ideas. Such articles, rather than the headlined revelation of a brand new theory, would be useful in informing us concerning the nature of science in general, and in making us aware of the past successes, present problems, and future plans of scientists working in a particular area.

SAUL T. EPSTEIN
Professor of Physics
University of Nebraska.
Lincoln, Neb.

METEORITE SHOCK WAVE

RE: "When is an Idea Fit to Print?"

It is shocking that the Science Editor suggests, by inference, that the seven gentlemen from La Jolla should keep hidden from the public the fact that they seriously question work done in their field, regardless of the competence and eminence of the author of the work. Is it not the responsibility of a conscientious scientist to strongly protest when he feels a piece of work is erroneous and the public is being misled? Is it not the responsibility of a science editor to report on the "impartial study of verifiable phenomena" in an impartial way? He, too, is being supported by the public. We laymen have the right to

expect him to give us a fair, scrupulous report on both sides of a theory when it becomes so obvious, as in this case, that there are two sides.

MRS. JOHN A. KNAUSS.
Kingston, R.I.

EDITOR'S NOTE: *SR's Science Editor also is shocked by the inference.*

I ENJOYED SOME of your reviews of science and research in previous issues of *Saturday Review*. However, your last effort, "When is a New Idea Fit to Print?" gives one a sad impression of your role as editor. Here you are not the impartial editor. Here you are partial and opinionated. In the last two paragraphs of the article you are vindictive and scurrilous. Are you attempting to use the prestige of the Seven Eminent Scientists to increase the circulation of *SR*?

W. W. DUECKER.
Pelham Manor, N.Y.

AT THE TIME you published Dr. Gilvarry's theory I was working for the Geological Survey in Washington, D.C. After reading the article I trotted my issue down to the office to show to several of my cohorts, all geologists. They read it and we discussed and argued. I don't know that they all agreed with Dr. Gilvarry but I can say that we all found the theory very interesting and exciting.

I have always been grateful to *SR* for publishing the type of material appearing in *SR/Science*. The "seven eminent scientists" from La Jolla should realize that there are many people like myself who are interested in the various aspects of the scientific world but who, due to lack of time and/or a lack of technical background, do not or cannot peruse the many scientific journals.

Congratulations to *SR* and to Mr. Lear.
(MRS.) PHYLLIS S. WILSON.
Grand Junction, Colo.

YOUR DEFENSE of the publication of the Gilvarry theory struck a particularly cogent note when you related it to the right of the layman to share in new scientific concepts at their formative stage, rather than being obliged to wait until the high priests of science decide what is "safe" for the rest of us to know.

It might be worth pointing out to these well-meaning guardians of science that the pioneer in any field of scientific inquiry must rely heavily upon the intuitive processes; and in this initial phase, the intuitively alert layman can occasionally contribute to the specialists, without necessarily being another Benjamin Franklin or a Leonardo da Vinci. In other words, if a new, unestablished theory is exposed to public scrutiny, it is not improbable that this exposure will generate an intuitive breakthrough from an unexpected quarter.

Just to demonstrate my point, the Gilvarry theory on the formation of the oceanbeds suggested to this layman-reader an idea that may or may not have scientific merit. If, as the La Jolla group claims, it seems unlikely that the youthful moon was blanketed with water, thereby upsetting the Gilvarry theory somewhat, is it not still possible that crashing meteorites