Letters

Tumor Development

With reference to the report by H. K. Mitchell which appeared in *Science* [**133**, 876 (1961)], we should like to make the following comment.

That our preliminary investigations of tumor-inducing factor (TIF) have proved successful is shown by the fact that they have stimulated further related investigations in the field of mammalian carcinogenesis. These initially crude methods have led to the development of different bioassay techniques and refined purification procedures which are now being successfully applied to a broad study of tumor development in our laboratories as well as elsewhere. In addition, it has led to the study of tumor breakdown in mammalian hosts.

It would serve no useful purpose to engage in a vituperative exchange over techniques that have already served to stimulate a new approach to the cancer problem. We regret that Mitchell did not choose a more pleasant method of approaching the problem, but we will comply with his request and make no further reference to his participation in the early, historical aspects of this research.

LAWRENCE BURTON
FRANK FRIEDMAN
Hodgkins Disease Research Laboratories, St. Vincent's Hospital, New York, New York

Satellite Communication

I think that it is unfortunate that the primary issues of satellite communication were so lost and beclouded in the news note "Space communications" [*Science* **133**, 1812 (1961)].

In principle, communication satellites could afford valuable international communication, first by linking the highly developed but inadequately interconnected common-carrier communication networks of Europe and North America, and later by improving communication with other parts of the world. Such a peaceful use of space would certainly be to our credit, and the more so the sooner it was attained.

I have noted in "Hazards of satellite communication" [*Bull. Atomic Scientists* (May 1961)] that this will not be easy technically, and that nontechnical obstacles could delay it indefinitely.

Thus, I think it is very misleading to say, with John Finney, that the direct issue is, "who shall sow and who shall reap the first big financial dividends of the space age." Much more direct issues are: Can satellite communication useful to the people of the world be brought into being quickly, and if so, how can this be done? When these questions are answered, we may then, if we wish, seek to meet reasonable standards of regulation and ownership. But, to give primarily political questions precedence over the realities of technology and the realities of international communication could delay satellite communication indefinitely.

One reality of international communication is that you can pick up your telephone and call a person in any one of over 160 different political areas in all parts of the world. Through the International Telecommunications Union, which is almost a hundred years old and which is now a part of the United Nations, and through its organs, international agreements on the use of frequencies and on standards and operating procedures have been worked out. Agreements for communication and agreements involving the shared ownership of international cables have been brought into effect. In the face of this existing international situation, it seems chauvinistic and arrogant to the point of madness to suggest that the United States Government or any purely American company or group of companies *could*, by itself, own an international satellite communication system.

The research and development necessary to make a satellite communication system possible is an entirely different matter. We cannot afford to have this wait on the formation of a new company. NASA's Project Relay is a natural step. So is the Bell Laboratories' work, which A.T.&T. is financing. Why should obstacles be put in the way of any research and development at this time?

Like ships, boosters are now among us, a resource of man. Must only the government take advantage of this resource? The electronic and communication arts, which provided the transistor and the solar cell, are largely the products of work done by private enterprise without government support. Should the government be forbidden to use transistors and solar cells in space? Who is giving away what, and to whom?

The question, "Does private industry have the capital, and so on" is completely unrealistic. A proper question is, Do the common-carrier communication systems of the world have the capital? (Most of these aren't private-enterprise systems.) The answer is yes. This is demonstrated by the continued growth in international submarine telephone cables, an art comparable to satellite communication in difficulty and cost.

Must the very first satellite communication system connect us with all the underdeveloped countries, where internal communication itself is poor? How long should we wait to make sure that these will be included? Until the Russians have satellite communication first?

The chorus of "You shan't have satellite communication unless" is already too loud. I hate to see *Science* embroiled in this windy, empty, but terribly dangerous debate.

J. R. PIERCE
275 McMane Avenue, Berkeley Heights, New Jersey

Communication Channels

I would like to comment briefly on certain statements made by H. H. Goldstine in the article on information theory which appeared in a recent issue of *Science* [**133**, 1395 (1961)].

After discussing the concept of channel capacity for discrete noiseless channels, defined by Shannon in his classical paper [C. E. Shannon, *Bell System Tech. J.* **27**, 379, 623 (1948)], as the limit, as $T \to \infty$, of $[\log_2 N(T)]/T$, where $N(T)$ is the number of distinct messages of duration T, Goldstine proceeds to discuss the noisy communication channel and makes the curious statement that the coding theorem for such channels, ". . .

roughly speaking, guarantees that the channel capacity as just defined is precisely the maximum rate at which it is possible to receive information with arbitrarily high reliability."

Are we to conclude, then, that by the use of an appropriate encoding procedure the information rate through a noisy channel may be brought arbitrarily close to the channel capacity of the corresponding noiseless channel? Apparently so, for the article continues, "A priori, one might imagine that as the noise in a channel increases, the rate at which one can transmit information . . . down the channel would necessarily decrease to zero." The clear implication that such a decrease does not occur leaves us with the impression that there have been revolutionary (and unpublished) developments in communication theory such that the degradation of communication channels by noise now arises only because of the absence of suitable encoders and decoders. Of course, Shannon's coding theorem for a noisy channel is actually a proof that it is possible, by suitable coding to communicate information even in the presence of noise at a definite rate and with an arbitrarily small frequency of errors, although this rate is naturally lower than that for the corresponding noise-free channel. The theorem is based on a definition of channel capacity

$$\max\ [H(x) - H_y(x)]$$

where $H(x)$ is the source entropy, $H_y(x)$ is the equivocation, and the maximization is carried out over all possible information sources used as input to the channel. It is in the definition of equivocation that the channel noise enters the theory and reduces the channel capacity from the value for the corresponding noiseless channel.

ALAN B. LEES
Systems Laboratories Division,
Electronic Speciality Company,
Los Angeles, California

Lees has been good as to call my attention to an ambiguously worded sentence in my article. This sentence which appears on page 2, column 1, is quite unambiguous if it is changed to read as follows: "This theorem, roughly speaking, guarantees the existence of a nonzero rate at which it is possible to receive information with arbitrarily high reliability."

H. H. GOLDSTINE
Thomas J. Watson Research Center,
International Business Machines Corp.,
Yorktown Heights, New York

Identification of Aldehyde in Mars Vegetation Regions

In an article in *Science* (*1*), W. M. Sinton reported three infrared bands, at 3.67, 3.56, and 3.43 μ, respectively, which he found in the vegetation regions of Mars but not in the desert regions. These bands he identified as being due to the presence of carbon-hydrogen linkages, and therefore to organic material in these areas. He also mentioned that terrestrial plants have bands in the region 3.56 to 3.43 μ but that the strong band at 3.67 μ is not found in terrestrial plants.

While there are many organic materials with bands in the region 3.56 to 3.30 μ, there are very few materials of any type with a prominent band at 3.67 μ, so a possibility of specific identification presented itself.

Most organic aldehydes (but not formaldehyde) have bands at 3.65 to 3.70 μ and 3.53 to 3.56 μ, due to the aldehyde carbon-hydrogen linkage, and they are among the few materials with a strong band near 3.67 μ. As is indicated in Fig. 1, the band at 3.67 μ is probably due to the aldehyde group, which also contributes to the band at 3.56 μ. The band at 3.43 μ is probably due, as Sinton states, to the carbon-hydrogen linkages in carbohydrates and protein organic matter in plants which resembled terrestrial plants, although an exact identification is difficult, due to the extremely large number of materials with carbon-hydrogen bands which absorb in this region (*2*). The most likely aldehyde is acetaldehyde, since most other aliphatic aldehydes have more nonaldehydic carbon-hydrogen than is indicated in Sinton's data, but some other aldehyde is a possibility. The presence of the terrestrially reactive aldehyde is perhaps a reflection of the near absence of oxygen on Mars and the consequent lack of oxidation.

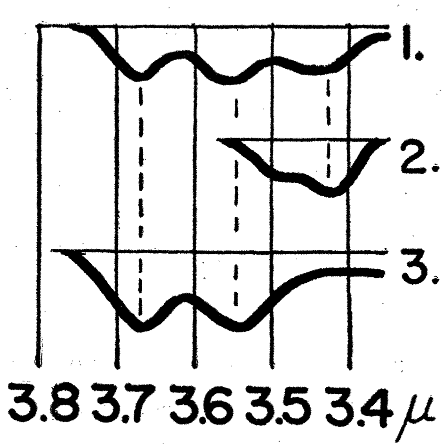

Fig. 1. (*1*) Infrared bands found in the Martian vegetation regions but not in the Martian deserts [adapted from *Science* **130**, 1234 (1959)]; (*2*) infrared bands in flour (terrestrial organic material); (*3*) infrared bands in acetaldehyde.

If I may be permitted to speculate a bit, acetaldehyde may be an end product of certain anaerobic metabolic processes. A familiar one is the metabolic fermentation of carbohydrates to acetaldehyde and then to alcohol (*3*). This process yields much less energy for the organism than conventional oxidation (carbohydrate + oxygen → CO_2 + H_2O + energy), but certain organisms on earth use fermentation as their source of energy when oxygen is not available, and perhaps this happens on Mars.

N. B. COLTHUP
Stamford Research Laboratories,
American Cyanamid Company,
Stamford, Connecticut

References

1. W. M. Sinton, *Science* **130**, 1234 (1959).
2. L. J. Bellamy, *The Infrared Spectra of Complex Molecules* (Methuen, London).
3. J. H. Rush, *The Dawn of Life* (Hanover House, Garden City, N.Y.).

I am pleased to see Colthup's interpretation of the spectra of Mars.

It has been brought to my attention that in my article the wavelengths one would conclude from Fig. 2 (namely, 3.45, 3.58, and 3.69 μ) do not agree with those that are stated. The precise wavelengths are difficult to determine. The instrumental width was 0.06 μ in the best spectra. For calibration I relied largely on atmospheric absorptions. A polystyrene film and a mercury arc were used in addition, but there were some erratic shifts in calibration, and it was felt that more reliable calibration was obtained by measuring atmospheric bands in the same spectra that were used for measuring the bands.

From the spectra obtained in 1958, the wavelengths from Fig. 2 of my article seem more reliable than the ones stated in my article. However, more spectra, which were of inferior quality, were obtained in 1960. From these new spectra I obtained values which are in agreement with those stated in the article. My conclusion is that the most reliable wavelengths are 3.45, 3.58, and 3.69 μ, with a possible error of 0.02 μ. These revised wavelengths are still in agreement with Colthup's interpretation.

WILLIAM M. SINTON
Lowell Observatory, Flagstaff, Arizona