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implication that such a decrease does not occur leaves us with the impression that there have been revolutionary (and unpublished) developments in communication theory such that the degradation of communication channels by noise now arises only because of the absence of suitable encoders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.

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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."

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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.

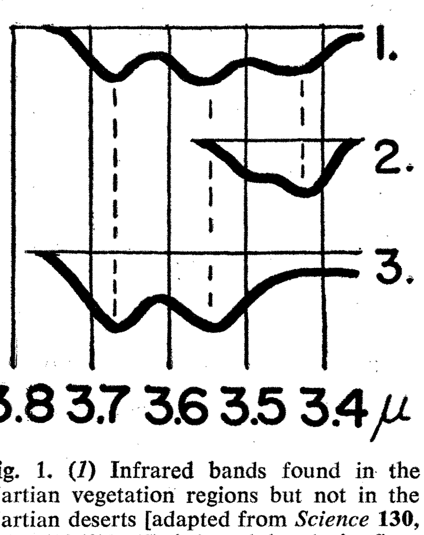

Fig. 1. (*1*) Infrared bands found in the Martian vegetation regions but not in the Martian deserts [adapted from *Science* **130**, 1234 (1959)]; (*2*) infrared bands in flour (terrestrial organic material); (*3*) infrared bands in acetaldehyde.

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.

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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.

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