Conclusions in the report by C. Hardyck *et al.* imply what the report does not in fact substantiate, namely, that the "treatment resulted in immediate and long lasting cessation of subvocalization"—that is, if they mean that after the subvocalization has ceased, reading is still going on.

In Hardyck's study there is no indication that his subjects were comprehending what they were reading. It is inferred that they were tested for subvocalization by reading for 30 minutes. But there apparently was no test for comprehension after the reading. When the individual is no longer subvocalizing is he still reading? Some tests of comprehension would have to follow before conclusions could be drawn.

This factor might be responsible for the extinction occurring so "quickly and easily." The subjects might have concentrated on the reduction of feedback (control of the motor aspect) to the exclusion of actual reading (comprehension). But we can never be certain of this unless some comprehension references are established initially and are subsequently used as frames of reference for proof that reading was still taking place.

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The purpose of our report was to communicate a striking learning phenomenon which seems to be related to reading speed and comprehension. The report, based on 17 subjects, was not intended to be a controlled study, and was not presented as such. We have recently completed some experiments in which subjects who do not subvocalize under normal reading conditions read material scaled on conceptual difficulty. Two experimental groups are relevant here. The first group read the material while surface electromyograms (EMG) were recorded from the larynx, chin-lip, and trapezius. A second group read the same selections under our feedback procedure. (Any increase in laryngeal activity over resting level triggers a 500-cycle/sec tone. The subjects were instructed to keep the tone off.) Results show that without feedback, EMG activity in the larynx increases as the difficulty of the material increases. Corresponding increases in activity are not found for EMG's recorded from chin-lip and trapezius. Comprehension tests administered immediately after reading show high comprehension for the first group over all selections while comprehension for the feedback group (which does not have the increased laryngeal activity) diminishes as difficulty increases. However, comprehension is still fairly high even for the feedback group reading the most difficult material. We have also measured reading comprehension systematically in a number of feedback treatment cases and have found that comprehension of light fictional material does not suffer if the laryngeal activity is eliminated in these chronic subvocalizing subjects. Therefore, it seems safe to conclude that reading is taking place.

The data reported by McGuigan do not seem germane to our findings since we record from electrodes placed over the thyroid cartilage. We compared surface and needle electrode recordings and found that this placement detects primarily laryngeal muscle activity. McGuigan, however, records from electrodes placed above and below the point of the chin which detects muscle potentials from the depressor labii inferioris, genio-glossus, the digastric muscles, and the platysma (*1*), none of which are directly connected with laryngeal activity.

In our feedback treatment studies, we routinely use a multiple screening procedure and record EMG's from several sites. Subjects are screened two to four times before feedback is used. All feedback treatment is done on subjects who reliably show a large increase in laryngeal activity during reading as compared to relaxation level.

We have investigated the effects of informing the subject that he is subvocalizing and of instructing him to eliminate the activity. This procedure results in no drop in laryngeal activity. Our regular control subjects are given the same instructions as our feedback subjects, but do not receive feedback and the activity does not disappear spontaneously.

McGuigan's use of the phrase "covert oral responding" to refer to both his electrode placement and ours is misleading, since we have found a similar chin placement to be of no value. For our subjects, chin-lip and laryngeal EMG's show no relationship. Chin-lip activity does seem to relate to trapezius activity which we consider to be a measure of general tension level independent of our measure of vocal activity. Subjects may be able to decrease their chin-lip activity in the absence of experimenter-arranged feedback, but this does not warrant either equating chin-lip and laryngeal activity under the general heading of "covert oral responses" or generalizing to adults from data obtained on children.

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Carbon Replicas of Siliceous Sponge Spicules

To study the surface ultrastructure of siliceous sponge spicules and other siliceous structures of biological origin (*1*), the following technique was developed. Specimen-covered grids are placed on a rotatable stage in a vacuum evaporator. During evaporation, the stage is carefully rotated at least twice through 360°; about 400 Å of carbon is deposited on each specimen (*2*). The grids are then carefully immersed (without agitation) in 5 percent hydrogen fluoride for 10 minutes, removed, gently dipped in and out of deionized water several times, and allowed to dry. Replicas may be lost or damaged if not handled gently during this treatment.

When compared with the uniformly dense images of intact spicules (Fig. 1), replicas of spicules clearly show surface patterns and microspine structure (Fig. 2). Intact spicules absorb the electron beam, become hot, and move, whereas the replicas are very stable and can be stored for at least 1 year.

The combination of water-cast formvar and rotation during carbon deposition seems to be responsible for the three-dimensional aspect of the carbon replicas prepared by this technique; other procedures produce replicas which usually collapse (*1*). With the rotation technique, shadowing of the specimens with heavy metals does not critically enhance the electron-microscopic image and is unnecessary. Low-magnification examination (× 1000 to 2000) is sufficient for the study of spicules; this can be achieved with almost all operating electron microscopes, new and old.

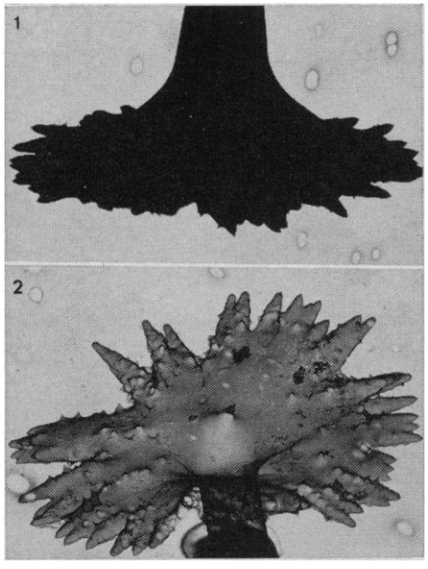

Fig. 1. Rotulate end of a birotulate microsclere from a gemmule of the freshwater sponge *Heteromyenia* sp. (× 3000). Fig. 2. Carbon replica of a rotulate end similar to that in Fig. 1. The surface appears smooth, and the microspines are clearly seen (× 3000).

Much of sponge taxonomy relies on spicule structure for identification of different taxa. Because many spicules are composed of amorphous silica, they are difficult to examine rigorously in the light microscope due to inherent chromatic and spherical aberrations. The microspines in Fig. 2 are not clearly seen with even oil-immersion phase contrast. Because of the importance of spicule structure in sponge taxonomy, precise images of spicules are essential for definitive classification and identification. This technique offers a convenient means of obtaining accurate pictures of spicules. It has also been used with great success to study frustule structure, shell structure in foraminifera and radiolarians, and opaline phytoliths in higher plants. It can probably be used to study any siliceous particles of biological origin and perhaps siliceous minerals as well.

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References and Notes

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3. I thank Dr. W. D. Hartman, Yale University, and Dr. T. L. Simpson, Tufts University, for valuable discussion regarding this work.

5 June 1967

Irradiated Parabiont Animals

The report by Carroll and Kimeldorf (*1*) interested me because they are apparently surprised by finding "a remarkable degree of survival for irradiated members of parabiont rat pairs if the partner were shielded." While the details of their experiments differ somewhat from those obtained in our laboratory, we have been working with such parabiont rats and taking advantage of this protective effect for some 17 years.

Our first mention of protection of irradiated parabiont rats by their shielded partner was made in the "Semi-Annual Progress Report of the New England Deaconess Hospital to the United States Atomic Energy Commission November 30, 1950." Edwards and Sommers reported from this laboratory (*2*) that in irradiated rats protected by parabiosis there was increased resistance of parabionts to radiation when one of the pair was shielded.

Warren, Chute, and Farrington (*3*) demonstrated protection of the hematopoietic system in irradiated rats by parabiosis and demonstrated that leukocytes were supplied temporarily by the shielded partner.

It is of interest that Carroll and Kimeldorf have used a pathogen-free strain of rats and that they have used a skin-to-skin anastomosis rather than the coelomic one which we employ.

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6 June 1967

We were aware of the two published papers cited by Warren but not of the semiannual progress report from the New England Deaconess Hospital to the AEC. The two published papers are relevant from the point of view that the investigators used parabiosis to analyze for the existence of a toxic factor affecting radiation-induced pathology.

The work of Finerty and his colleagues (see *1*, for example) could have been cited as well. Indeed, Barnes and Furth (*2*) used parabiosis in mice to study radiation injury as early as 1943. Some degree of protection may be inferred from all of the studies cited, although none have described the remarkable survival rates, noted by us in our report, over the much higher dose range of 1200 to 2400 roentgens. The major point of the results and discussion in our report was that parabiosis could be used to prolong survival and even override the gastrointestinal syndrome that leads to death within 120 hours after irradiation.

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14 June 1967

Contamination of Planets

The concern expressed by Horowitz *et al.* (*1*) is sufficiently important to the scientific community and the taxpaying public to warrant further examination. We were primarily concerned with the probability of growth of a terrestrial microorganism on Mars (P_m, Sagan-Coleman model).

While it is true that Horowitz *et al.* used the latest information regarding the physical environment of Mars, their data regarding the growth of organisms in a simulated Martian environment are limited. The question of whether a terrestrial microorganism will grow in the environment is the cardinal point regarding the possibility of contamination; it's importance demands more comprehensive treatment than theirs (*1*).

One must recognize that all interpretations of P_m are based on a limited number of organisms; it is possible that the sampling has omitted the organism that will tolerate the environment. Several observations not cited (*1*) must be taken into account before the probability of growth is established at a lower level than was originally suggested.

Studies (*2*) have demonstrated that *Bacillus cereus* survives but does not grow in a simulated Martian environment under the following conditions: 100-percent CO_2 at 10 mb; diurnal temperature cycle, with 20 hours at