standing cognitive functioning is not so simple. If one took the trouble to extend Weir's curves for the two individuals through their adulthood, he would discover that eventually the slopes would be the same. The individual with an I.Q. of 50 would level off at MA 8 at the age of approximately 16, and therefore his MA would best be represented by a straight line. This is also true of the other individual, except that his MA curve would level off at MA 16. If it is the slope that allows us to make predictions concerning the quality of cognitive functioning, can we then argue that in adulthood the cognitive performances of normal and retarded individuals will be the same?

The major point is that one makes a number of theoretical assumptions when he asserts that, since the I.Q. is a measure of one kind of rate, then it must also be a measure of another kind of rate, namely a measure of the rate of learning or information processing on individual tasks. One can, of course, assert that both MA (level) and I.Q. (an hypothesized determinant of rate of cognitive functioning) influence cognitive tasks. But this is exactly the argument examined in my article. The person who holds that the I.Q., independent of level or MA, determines rate of cognitive functioning on short-term learning tasks is a difference or defect theorist. Which general position is correct is open to investigation, but there is no doubt that the two major approaches examined in my article generate quite different predictions.

I am in sympathy with Weir's argument that the MA obtained on standard intelligence tests is a far from perfect indicator of the nature of cognitive functioning (*2*). Indeed, if there were a consensus that the MA was a perfectly adequate measure of the formal features of cognition (for example, rate of information processing), there would be no argument between developmental and defect theorists, since by definition individuals of the same MA level would have identical cognitive structures. However, in his efforts to champion the predictive efficacy on cognitive tasks of the I.Q. over the MA, Weir appears to go too far. To argue that the MA is not an important determinant in the quality (including rate) of the child's learning of new and unfamiliar cognitive tasks is an error. Evidence on this point is clear, and I doubt whether anyone working in the area of cognition would take exception to it. In spite of its shortcomings, the single MA measure and its factorial components have more cognitive correlates, including performance on purer Piaget-like cognitive tasks, than any other measure in psychology.

With respect to Weir's task argument, he and I probably could agree that an investigator should use a task sensitive to the particular factor that the investigator would like to demonstrate as being operative. Thus one interested in demonstrating the effect of motivational factors employs experimental tasks sensitive to these factors. There is no argument, therefore, that if one wishes to test the hypothesis that I.Q. is a measure of rate of information processing he should use a task that makes this type of cognitive demand on his subjects. My criticism of the various difference or defect positions was not based solely on findings obtained with motivational tasks, but rested also on the fact that the findings obtained by the supporters of these positions on tasks of their own choosing frequently have been equivocal. Furthermore, to imply that the holders of the developmental position have been reluctant to adequately test their views by using cognitively demanding tasks is to do them an injustice. They have frequently employed the same tasks used by the expounders of the various defect positions. These tasks include not only the concept-switching tasks referred to by Weir but a variety of discrimination learning, reversal learning, transposition, and learning of set tasks. Indeed, workers sympathetic to the developmental position have employed the probability-learning task used by Weir in his laboratory. Although Weir does not state the criteria by which we might know if a task were truly cognitive in nature, I find it difficult to believe that none of these tasks involves information processing and that they are therefore inadequate tests of the hypothesis of "equal MA–equal cognitive" functioning.

Weir attempts to close the gap between the developmental theory of familial mental retardation and the various "defect" positions by noting that certain "defect" theorists argue that retardates have less of something that normals of the same MA have, rather than having something that the normals do not have. This is true; however, other defect theorists have argued that retardates are qualitatively different from normals. It is for this reason that throughout my article I referred to the general approach as a defect or difference orientation. It is the difference between familial retardates and normals of the same MA that is the point of contention between the developmental theorist and the difference theorist, whatever the hypothesized deficit underlying this difference may be. The gap between the developmental theorist and all the defect or difference theorists remains a wide one since the developmental position generates the hypothesis that there are no differences in formal cognitive functioning between familial regardates and normals matched on general level of cognition (typically measured by MA). What should be emphasized is that the developmental position at this point in time represents a tenable hypothesis. As long as the hypothesis clearly generates behavioral predictions, I would certainly entertain the possibility that it is wrong. Clearly, as my article pointed out, most theoretical workers in the area are entertaining this possibility. The argument presented in Weir's letter indicates that he shares their views. Fortunately, resolution of this can be achieved through thoughtful experimentation.

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1 May 1967

Feedback of Speech Muscle Activity during Silent Reading: Two Comments

Hardyck, Petrinovich, and Ellsworth report that the presentation of auditory feedback from the speech muscles produces a "long-lasting cessation of the subvocalization" that occurs during silent reading (*1*). The auditory cue is effective, they conclude, because it allows subjects to make fine motor adjustments in their speech musculature. While this conclusion is consonant with other findings (*2*), the lack of systematic control data weakens the strength of the inference that auditory feedback is the critical variable. It is possible that the complex of giving the subjects a set to the effect that their subvocalization would be reduced, the placing of electrodes only on the throat, and a variety of other extraneous variables, all of which focused attention on the speech mechanism of individuals who clearly subvocalized, played a role in the reduction of the response amplitude.

We conducted some exploratory research in which the subject, an 11-year-old girl who manifested heightened covert oral behavior during silent reading, read in the laboratory for two sessions (of approximately 45 minutes each) on successive days (Fig. 1). The procedures previously reported (*3*) were used, with the extension that the amplified signal from the chin electrodes entered a meter relay that was activated at 15 and 25 (arbitrary units). These levels were selected in accordance with the amplitude of the chin response during silent reading such that when the amplitude exceeded 25 a noxious buzzer (a potential punisher) automatically started in the room. When the response amplitude fell below 15, the buzzer ceased (a potential negative reinforcer). The length of time that the buzzer was on was automatically recorded, as was the time that it was off. Each buzzer change (on or off) was defined as a trial. The subject decreased the amount of time that the buzzer was on (reduced her response amplitude) as trials progressed. The response stabilized at a low amplitude level for the last 10 minutes of reading until the session was terminated, even though she was later unable to verbalize the fact that the sounding of the buzzer was contingent on her own behavior. The feedback from the covert oral response may have caused the reduction of response amplitude.

The total amount of time that the buzzer was off was determined for each of the two reading sessions and a percentage, relative to total time that the buzzer was on, was calculated. This summary of the data presented in Fig. 1 is shown for the subject in Fig. 2 where the sharp increase in the percentage of time that the buzzer remained off can be noted.

Two additional subjects who subvocalized during silent reading were run under control conditions: the second subject, a 10-year-old boy, read for two 45-minute sessions on 2 successive days, while the third subject, an 11-year-old girl, read for four sessions on 4 successive days. These two subjects were treated the same as was the first sub-

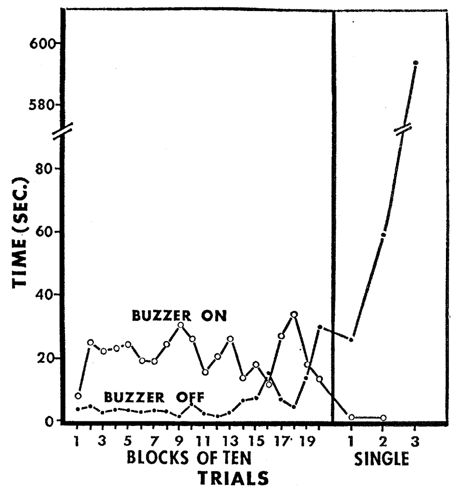

Fig. 1. Amount of time that chin EMG was of a sufficiently great amplitude to produce the noxious stimulus (buzzer on) or of a sufficiently low amplitude to remove the noxious stimulus (buzzer off) as a function of trials for the first subject.

ject except that the buzzer was never presented. The amount of time that the buzzer would have been on (and off) had it been presented was automatically recorded, just as for the first subject. The percentage time that the buzzer would have been off during each reading session (vertical axis) is plotted for the two control subjects in Fig. 2. The curves for all three subjects are remarkably similar for the first two sessions—they all show sharp rises that indicate relatively rapid reduction in the amplitude of the chin electromyogram.

Signals from the chin, arm, eye, and pneumograph transducers were recorded for the third subject on magnetic tape. The analog signal from the former three entered a root-mean-square voltmeter which yielded a d-c signal that could vary between 0 and −1 volt maximum. This value, which is proportional to the true root-mean-square value of the input signal, entered a voltage-to-frequency converter which could vary between 0 and 10,000 count/sec, and thence to an electronic counter which counted the frequency for each 10-second period. The resulting frequency was converted to a binary-coded decimal signal that was printed out on a digital recorder; this value is a mean integrated voltage for each 10-second period. A mean voltage was computed for the rest period before the reading period, and for the reading period. The former was subtracted from the latter for each session, and the results for chin EMG are plotted in Fig. 2. The curve for this measure is an approximate mirror image of that for the first measure for the third subject. A direct recording of covert oral behavior also shows that the amplitude of the response is sharply reduced. The results for the arm, eye, and respiration measures are not relevant here but transducers were placed on various regions of the body.

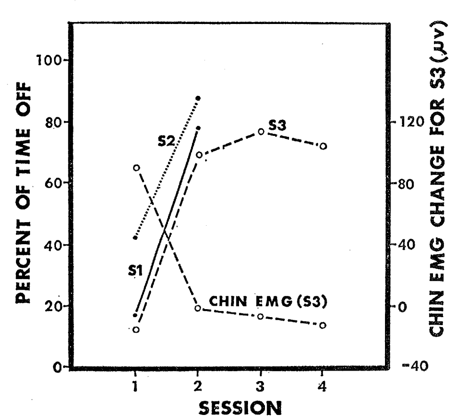

Fig. 2. Percentage of time that the buzzer was off as a function of number of sessions for the first subject. For the second and third subjects the left vertical axis is a measure of percentage of time that the buzzer would have been off. The right vertical axis is change of amplitude of chin EMG as the third subject went from resting to silent reading for each of four sessions.

These data indicate that subjects rather rapidly decrease their amplitude of covert oral behavior in the absence of feedback arranged by the experimenter. Furthermore, in contrast to the procedures of Hardyck *et al.*, this effect occurred without calling the purpose of the study to the subjects' attention, and without them being able to verbalize the response-contingency relationship (*4*). While the effect that Hardyck *et al.* report may be real, the assertion that it is must await the results of research that rests on a sounder methodological basis.

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References and Notes

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2. J. V. Basmajian, *ibid.* **141**, 440 (1963).
3. F. J. McGuigan, B. Keller, E. Stanton, *J. Educ. Psychol.* **55**, 339 (1964).
4. The modification of behavior occurred for all subjects in spite of the fact that none were aware of this relationship, including a college student who read for nine sessions while we were merely observing how the procedures worked.

5. Research performed pursuant to a contract with the United States Office of Education under the provisions of the Cooperative Research Program.

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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.