while mathematical research or what kind of mathematics should be taught.

Finally, I should like to comment on Hamming's remarks on mathematical elegance. Tastes, including mathematical tastes, have never been legislated. Nevertheless, it is fair to say that most mathematicians associate elegance with simplicity and economy of means rather than trickery. Besides, the notion of what is tricky and what is not is highly subjective and changes with time. Very frequently what seems to be tricky at first turns out later to be in the nature of things. Our natural wisdom does not always lead us to expect things to be the way they are.

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These two letters are difficult to reply to because the authors and I are talking about different things.

The letter by Albert *et al.* is an exposition of the modern pure mathematician's point of view. It accuses me of attacking mathematics, yet four times I explicitly said (in varying words) that I am *not* attacking mathematics or questioning the activities of pure mathematicians. "The purpose of this paper," I said in the opening sentence, "is to illustrate by means of examples some differences between numerical analysis and mathematics." I did not question the mathematical truth of the examples, and Albert *et al.* do not question my observation that the mathematical results were inappropriate for many situations in numerical analysis, so I suppose that we would be in actual agreement if we were writing about the same things.

With regard to the paragraph they quote in part, let us not quibble over the extent of experience behind the formal postulational stage of geometry, algebra, and the calculus, and instead concentrate on the crucial sentence:

> But it is difficult to imagine how by appeal to observations many of the postulates of modern mathematics could either be verified or shown to be unsuitable, and one can only conclude that much of modern mathematics is not related to science but rather appears to be more closely related to the famous scholastic arguing of the Middle Ages.

(The phrase "is not related to" may have caused unnecessary confusion. It would have been more accurate to say "does not share some essential properties of." Certainly, as should have been clear from the context, I did not mean that mathematics is not useful to science.) As the overthrow of the parity principle showed, scientists usually accept the primacy of experimental results and ultimately abandon even very elegant theories which do not seem to agree with experimental facts. Since Albert *et al.* do not deny that "it is difficult to imagine how by appeal to observations many postulates of current mathematics could either be verified or shown to be unsuitable," I can only assume that they would agree that mathematics does not accept the basic test of observational verification. Thus, while we may be quibbling over a definition, I do not think that mathematics is a science. This is clearly not the same as saying that mathematics should be a science, which is apparently what they are accusing me of saying. What I did say was that numerical analysis should try to follow the path of science, and nothing they said seems relevant to this point.

Calderón says, among other things, that I am vague about how I propose numerical analysis should differ from pure mathematics. I thought that I was quite definite in asking that the models used in numerical analysis be occasionally checked against actual experience on computing machines. I was questioning not the "certainty" of a mathematical proof but the appropriateness of many currently used mathematical models to particular situations in numerical analysis. And finally, I was surely not "legislating" taste but was merely observing that taste might be different in different fields.

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Objective Tests and the Highly Able

Henry Chauncey and Thomas L. Hilton are to be congratulated on the frankness with which they discuss certain limitations of the statistical evidence presented in their article "Are aptitude tests valid for the highly able?" (4 June, p. 1297). By it they may well have paved the way for a serious confrontation by psychometrists and others of one of the crucial aspects of multiple-choice testing.

In their first paragraph they mention three criticisms that have been made of objective tests, the third being that these tests "not only fail to distinguish but actually discriminate against the most able students, by penalizing them for their ability to see imperfections in keyed answers which average students accept without qualms," and they cite pages 99–101 of my book *The Tyranny of Testing* (Crowell-Collier, New York, 1962; Collier Books, New York, 1964) by way of reference. At the end of their article, when discussing conclusions to be drawn from the statistical evidence presented (p. 1303), they say frankly:

> Whether there is evidence [by which they mean statistical evidence] in regard to the criticism that objective tests discriminate *against* highly able students is not answered. If there is such discrimination and it is extreme, then the studies that have been examined are irrelevant: the very students who would have provided pertinent data would have been excluded from consideration. . . . If the discrimination is not so extreme (which seems likely), there is still the possibility that only a small group of exceedingly able students is discriminated against and that the lack of validity for these is not detected when large samples are observed. [Compare *The Tyranny of Testing*, pp. 141–3.] In none of the studies were perfect correlations reported. The possibility that some of the departures from prediction resulted from the alleged discrimination cannot be completely discounted.

It might be well to point out the significance of the quoted passage by providing a context. I am, of course, not alone in criticizing current test procedures. Among the criticisms of tests that I have been making over the years is that even the best multiple-choice tests penalize depth, subtlety, creativity, intellectual honesty, and superior knowledge. I have explained how they do this, and have shown that arguments used by important testers in rebuttal have in fact been tantamount to admissions that the charge is valid.

If the charge is valid, multiple-choice tests have a defect of major proportions, and their widespread use has grave educational and national consequences. This is surely something that we dare not ignore or even treat lightly. Yet there has hitherto seemed to be considerable reluctance in many psychometric circles to face this and related issues squarely. Indeed, when my various criticisms of multiple-choice tests appeared in print they evoked an understandable but nonetheless unfortunate defensive reaction from a number of psychometrists (though there were notable exceptions). If the charge were false, the obvious strategy for the psychometrists would have been to seek to demonstrate the fact by the statistical methods that they find convincing. Yet it is almost a decade now since I brought the charge to the attention of Educational Testing Service, and in all that time they have produced no evidence, statistical or otherwise, that refutes it despite their unrivaled opportunity to make experiments using their own multiple-choice tests, which are certainly among the very best.

The important fact, then, is not just that there does not happen to be statistical or other evidence to refute the charge but that, had the charge been refutable, there *ought* to have been such evidence by now. Because there is not, the charge that multiple-choice tests penalize depth, subtlety, creativity, intellectual honesty, and superior knowledge must be held to prevail not only on its own merits but also by default. And this leaves a crucial question that we must all face: What is to be done about the matter?

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Toward Restoration of the Whole

Cyril Stanley Smith's article on "Materials and the development of civilization and science" (14 May, p. 908) reaches a conclusion that is particularly interesting from the point of view of the history of science. It is clear that science is now reaching ultimate limits in its efforts to decompose all phenomena into isolated entities that can be studied without interactions with the complex surroundings in which they normally occur. It is too often forgotten that this type of systematic investigation, although indispensable as an initial step, is essentially artificial. A truer understanding of nature can result only from a knowledge of the highly differentiated and interdependent entities that characterize any natural or artificial system. This is Smith's concept of scientific abstraction at a higher level, which should lead to the understanding of complex interrelations. At the same time it is in essence the Greek view of science; the Greek philosophers understood phenomena mainly as entities integrated with their environment, surroundings, and previous evolution, which forced the investigation of everything under innumerable sets of special circumstances. Such an approach was evidently doomed to fail at the time of Aristotle, since little valid knowledge can be gained without an analytical study of systems existing under idealized conditions. It is nevertheless interesting to see that science is presently evolving into the integrating approach that the classical Greeks already assumed to be the "true" form of knowledge.

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Proposed Regional Medical Centers

Elinor Langer (News and Comment, 14 May, p. 932) says that the report of the President's Commission on Heart Disease, Cancer, and Stroke, and the legislation subsequently introduced in Congress to implement some of its recommendations, "have received endorsements from groups such as the American Heart Association. . . ." In the immediate context in which she places this statement—following reference to opposition which has been developing among state health officers and basic scientists—the inference is easily drawn that both the report and the bill are completely satisfactory to our organization. This is not so, and I hope you will allow me to set the record straight.

When I appeared on 9 February to testify for the American Heart Association at the public hearings on S. 596, I made it clear that our support of the administration's health aims did not preclude criticism of the legislation as drawn. I underscored the need for clinical training programs to help provide the large numbers of highly trained physicians and paramedical personnel who would be needed to staff the projected medical complexes. Specifically, I said:

> To establish such a system without first seeing to the expansion of clinical training facilities might, in effect, do more harm than good. It would dilute our existing supply of trained clinical personnel and might well lower, instead of elevating, existing standards of diagnosis and treatment.

For this reason, the Heart Association strongly urged that the most immediate effect of the bill, if enacted, should be to enable existing training centers to expand and upgrade their clinical and paramedical training programs. We also recommended that, instead of attempting in one stroke to blanket the nation with regional medical complexes, attention be given to a series of planning grants and, possibly, to a few pilot projects.

In other recommendations we were concerned with the administration of the regional complexes (we favor administration by the National Institutes of Health), with the proposed basis for use of matching funds, and with the structure of an advisory council for the regional complexes, and we suggested rewording of the bill at several points to eliminate any unintentional denigration of the service now being rendered the American people by the medical profession.

I believe the foregoing will make it clear that we in the American Heart Association are engaging in quite the same sort of thoughtful analysis of the commission report and the legislative proposals that Langer indicates is in process among other sections of the medical and scientific community. We favor the commission's aims, but we have not bought the legislative package uncritically, and we see flaws that must be corrected if the worthy objectives are to be achieved.

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Chamberlin's Method: A Proposed Application

Chamberlin's 1890 paper "The method of multiple working hypotheses" (7 May, p. 754) can be used today, without changing a word, as a manual for practitioners of an applied science that was still but a gleam in the eye of its creators when the article was first published. I refer to psychodiagnosis and psychotherapy; to theories and practices which have developed over the past seventy years largely out of the work of Freud, his followers, his competitors, his refuters, and his detractors.

Today one still finds many "ruling theories," each with its adherents and disciples, attempting to explain the etiology of deviant and abnormal behavior. These theories range from the narrowly biological (for example, "never a crooked thought without a crooked molecule," various neurological theories) through the narrowly psy-