Letters

Research and Education: Restoring the Balance

In his editorial "One-sided criticism of university research" (28 May, p. 1177), Abelson discusses public opinion concerning the effects on teaching of heavy federal support of research. These effects are certainly being felt, and I agree that a cut in federal grants is not a realistic solution. The solutions Abelson offers, however, seem weak to me. He says: "Scientists must cheerfully meet their responsibilities as teachers. University administrators must make it clear that their institutions value good instruction. Federal agencies must align their policies so that support of research in universities contributes to, and does not compete with, the educational function."

The first two statements are wishful thinking. As long as faculty members derive their operating funds directly from the federal government, they are, in effect, employees of the federal government and not of the university. Moreover, as long as they can continue to attract large sums of money to their institutions, administrators will continue to encourage them to do so. Any change toward restoring the professor to his proper role in his university must be initiated by the government. To that end, the granting agencies should make grants to departments instead of to individuals. Those at the local scene who are better informed about local situations would then make the necessary evaluations of faculty that are now being made by the granting agencies.

One result of the present system is that the lure of federal funds often distracts the younger faculty member from the classroom and laboratory into weeks of proposal writing. What is even worse, if his work is not along popular lines, he may be led into areas away from his main interest.

Another frequent result of grants to individuals is the large research group. It is not uncommon to find 20 graduate students under the supervision of one professor. The proper training of such a number is virtually impossible, not to mention undergraduate teaching and other professional duties. If the practice of granting funds to individuals were to a great extent replaced by grants to departments, the faculty member could again be a professor for the university and not an administrator for the granting agency.

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. . . There is no question that research and teaching can—nay, must—coexist in the modern university and that one-sided criticism of the two endeavors is, as Abelson warned, potentially destructive. It is my fervent hope that all administrators of academic research and university teaching will heed his admonition that "research in universities [must] contribute to, and [must] not compete with, the primary educational function." Only the restoration of true balance can stave off increased criticism and legislative repercussions.

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Mathematics vs. Numerical Analysis

In his article "Numerical analysis vs. mathematics" (23 Apr., p. 473), R. W. Hamming makes an attack upon "mathematical numerical analysis," by which he presumably means the attempt by mathematical techniques to solve general problems in numerical analysis. The reductionist line of argument which he puts forward demands that the concepts and techniques of theoretical science should be tied hand and foot to the technological practice of the moment, conceived in the narrowest possible sense. If this argument were to be taken seriously, it would have a devastating effect upon the development and fruitfulness of all theoretical work in science.

What concerns us even more is that Hamming bases his argument upon a critique of mathematics as a whole, which he expresses in the following form:

> Generally speaking, in the early history of mathematics long experience in the real world preceded both the abstraction of the postulates and the formulation of the definitions of geometry, and subsequent experience has validated their general usefulness. Thus early mathematics tended to follow the classical test of science, the regular (though not exclusive) appeal to observations in the real world. But it is difficult to imagine how by appeal to observations many of the postulates of current 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.

It is our view that Hamming seriously misconceives the nature of mathematics and its role in the scientific enterprise. Mathematics is the science of structure. Where intuition and unanalyzed experience indicate the existence of common structural features in a number of varying contexts, it is the task of mathematics to formulate these basic structural features in a precise and objective form. The mathematician *abstracts* from other variant and irrelevant features of these contexts in order to focus on these basic relations, and then must ask (and find out) what consequences follow from the basic relations alone. In its baldest form, this is the so-called axiomatic method, and one must always remark that the justification of a system of axioms lies in what can be proved from them, in what insights of a significant kind they furnish about the context from which the axioms sprang. These contexts need not be, and most often are not, systems of material objects, passively observed, but rather acts and processes: the act of counting for arithmetic, the acts of measuring and drawing figures for geometry, the act of finding roots of polynomials for algebra, and so on. Once crystallized in a definite form and proved fruitful, the acts and processes and objective difficulties of a mathematical theory may provide the context for the creation of a new mathematical theory on a higher level by a new act of mathematical abstraction. Yet this new abstraction is justified not by any passion for abstraction for its own sake but by the urgent pressures of mathematical discovery, whose process is not an idle weaving of fantasies without limitation but rather a confrontation with the implacable enemy of the unknown, unstructured, and inaccessible.

It is a paradox which lies at the heart of what Wigner [*Commun. Pure Appl. Math.* **13**, 1 (1960)] has called "the unreasonable effectiveness of mathematics in the natural sciences" that it is these successive acts of mathematical abstraction piled upon abstraction, urged on by the force of the autonomous development of the mathematical structure, that have made mathematics a significant tool and a dynamic force in the development of the physical sciences. And in turn, the confrontation at crucial moments of this development of mathematics with its physical applications has led to new mathematical abstractions and to the creation of new mathematical theories. We must emphasize, however, that as a general historical fact the creation of the groundwork of major mathematical theories preceded their significant physical applications, and usually was independent of them. The geometry of the Greeks, founded by the Pythagoreans and made a rational scientific discipline in the Platonic academy, found its physical fruits in the astronomy and mechanics of the succeeding Hellenistic age. The algebra of the Arabs, of Renaissance Italy, and of France, culminating in Fermat and Descartes, created the groundwork for the differential and integral calculus (which was almost explicit in Barrow, for example) before it was created by Newton and Leibnitz and before Newton applied it to the creation of his theories of mechanics and gravitation. One could multiply examples from mathematical fields like differential geometry, group theory and group representations, the spectral theory of operators, and, to take a very recent example, the discovery by theoretical physicists of the practical importance to them of such relatively abstract portions of the present-day body of mathematics as the representation theory of Lie algebras, the theory of distributions, algebraic geometry, and algebraic topology.

From the point of view of the physical sciences, the role of mathematics has been historically that of creating the concepts and techniques needed to formulate physical theories and to derive their consequences. From the point of view of mathematics itself, the role of mathematics is to carry forward the process of the autonomous development of its own inner logic and to confront and solve the problems resulting from that development. It is a fascinating and highly significant fact of scientific history, valid to the present day (and one which refutes those who would put more rigid forms on the mutual relations of mathematics and the physical sciences), that these two independent purposes have been in essential harmony. Mathematics has generated the concepts and the theories, both of which have found significant physical applications. This is not to denigrate more explicit forms of relationship between physical problems and mathematical discoveries, but rather to point up the crucial fact that the latter sort of relation is only one aspect of a deeper interconnection. The embodiment of mathematical concepts and techniques in the structure of physical discovery has long been and will undoubtedly continue to be one of the crucial features of the development of mathematics, but one whose most significant appearances are often in an oblique and subtle form.

Mathematical research, in its own right as well as in its role in creating the concepts and methodology of theoretical science, is a vital and dynamic element in man's efforts to understand the universe. It will continue to be so as long as science is a living force.

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. . . Hamming ignores what mainly makes mathematics relevant to science and to knowledge in general. Even if no numerical problems were ever to be solved by mathematics, it still would occupy a central position among the disciplines of the intellect. It supplies an ever-increasing body of knowledge with a conceptual framework without which understanding becomes impossible beyond a certain degree of complexity, and it makes logical inference possible to an extent otherwise unattainable. For example, the notion of differential equation is important quite aside from the knowledge of techniques of solution, simply because, among other things, it permits a clear and precise formulation of many physical laws and facilitates the manipulation of them.

Hamming complains that pure mathematics does not have ready answers or the best answers for many problems of numerical analysis. He is not alone in his distress, for the same situation prevails in other areas of pure as well as applied mathematics. Topology does not have ready answers for all the topological questions that analysts encounter, and conversely. And I venture to say that in this respect no scientific discipline is different from mathematics.

One should not forget that knowledge of even small and seemingly simple parts of the universe is not and can never be exhaustive. The frontiers of science are pushed back with various degrees of energy depending on the presumed promise of the territory beyond. With changing tools and needs, numerical analysts may find today that there are important rewards to be reaped from mathematical land abandoned in the past in the search for greener pastures. I have no quarrel with Hamming in this respect. But what I find remarkable is that he seems to be proposing a methodology for numerical analysis different from that of pure mathematics. He is vague about what he has in mind here. Does he suggest that there are problems in numerical analysis which are beyond the reach of mathematical methods? Or that there are more effective approaches to deal with them? Perhaps he wants to make it an empirical discipline. Empirical evidence of mathematical truths should not be dismissed too lightly, but it should never be given the status of what it is not, namely, knowledge inspiring the same degree of certainty as a mathematical proof; nor should one forget its relative scarcity and its general irrelevance to the central role of mathematics as a universal, conceptual methodology. For this reason, and unless we want to indulge in semantic distortions, this type of evidence should better be considered part of the disciplines where the problems to which it pertains arise, and it should not be permitted to influence our judgment as to what is worthwhile 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 psychome-