been the case had he drunk water, because of the dehydration involved, and death was hastened.

In any case, it seems of interest that the lack of an adequate amount of an essential food like protein is likely to be accompanied by more pain than starvation without any food or water.

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Training of College Teachers

I note in your editorial "Real professionalism" [*Science* **132**, 439 (19 Aug. 1960)] that the National Education Association is now attempting to impose state licensure and a requirement "to study the theory and practice of education in the course of their professional education" on college and postgraduate educators.

I have long been disenchanted with the training too many of the school educators receive at the "teachers colleges," whose curriculum is so heavily laced with courses on how to teach but seems so weak in the subjects to be taught. Are we now to inflict the same "uniformity" (or is it mediocrity) on the collegiate instructors?

Perhaps I am too naive in my belief that the two essential characteristics of a good teacher are (i) enthusiasm and (ii) thorough knowledge of and interest in his subject.

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Brain Dysfunction

A report by Wells and Wolff [*Science* **131**, 1671 (1960)] offers a slim but adequate excuse for me to make some remarks which I have been saving up for some time.

The authors cite a fascinating experi-

ment in conditioning of cerebral responses, finding significant differences between "normals" and persons evidencing severe anxiety. This observation in itself would be of sufficient interest, but the authors have gone on to use the data in a rather backhanded fashion to imply that the differences must be due to brain damage of some sort, hinting that with sufficiently sensitive techniques, they might "demonstrate other evidences of impairment" which are the real, actual, true cause of functional behavior disorders and not some magical kind of "neurosis" or "psychosis" as some people think. Perhaps I am unfair to Wells and Wolff, but I have heard essentially this view expressed by enough members of the working and teaching medical profession to convince me that it is widespread.

In support of this view that brain damage lies behind every psychological malfunction is an impressive array of clinical information demonstrating that psychological malfunctions can be and are caused by certain kinds of brain damage (although brain damage sometimes shows a disconcerting failure to affect behavior at all). There is no evidence at all to show that every functional disorder is the result of structural damage to the brain. The science least likely to resolve this implied problem is the science of electroencephalography.

The electroencephalograph measures the gross electrical activity of the brain, which results from the fields and currents generated in unknown patterns by an unknown set of neurons somewhere near the surface of the brain. The electroencephalograph can thus detect changes in activity of large assemblies of neurons, and because of this and because of the complexity of the signal, can differentiate between grossly different patterns of brain activity. In the same manner, a high-impedance probe inserted into the main frame of a computer could be used to detect (empirically) changes in the routing and number and time patterns of electrical signals; a patient observer might correlate these changes with certain gross malfunctions in the computer and use them for diagnosis, as the electroencephalogram is often used.

Suppose, however, that one day the computer technician attached his probes and recorded the patterns of signals and found some extreme changes which failed to resemble any of the "normal" traces. Should he decree an emergency operation? Perhaps, or perhaps not; he will have to find out first (i) whether there is a mistake in the program so that the computer has started dividing by zero; (ii) whether a new type of program is being used so that the patterns of activity are not typical of the usual sort of program; (iii) whether the machine has been told to do something it cannot do; and (iv) whether any of the other things which can have the observed effect without failure of any part of the machine (and there would be many of them) have happened. True, certain typical disorders are likely to have typical effects on the "electrohardwareogram," so that after one has observed the coincidence enough times he feels confident in making certain diagnoses, but it is entirely possible, if not probable, that merely by programming the machine *just so* one could simulate a tumor in a transistor or a lesion in a loop.

A medical student once protested to me, after listening to some of my propositions concerning a model for behavioral organization, "But you can't know anything about function without knowing structure!" In the brain, I suspect that the opposite is true: you can't know anything about structure (that is, organization) until you first know function (what the structure does). I suspect that the medical student sometimes uses machines of which he understands the function considerably better than the structure, and I know for fact that even some of the most sophisticated of com-

puter users may well consider a study of flip-flops, diode storage times, and bit density on tape a waste of their time, leaving that to those who want to build, not use, computers. The best way to learn how computers work is certainly *not* to start by studying the metallurgy of copper wire, vacuum tube or transistor theory, and the quantum mechanics of conduction processes. One could know all that there is to know about these "fundamental" aspects of a computer and still be completely, or at least largely, ignorant of the various functions which a computer can carry out so usefully. One could know all there is to know about synaptic processes, cell metabolism, and the routing of impulses through the brain and still not be able to make heads or tails of what the blamed thing is *for*. Only through a thorough analysis of the things which people can do, and orderly construction of a model representing these functions and their interrelationship, can one understand "brain activity." Only then can one group the various structures he has found and has yet to find into meaningful assemblies which tell him anything, which bear any comprehensible relationships to each other. I stood by once and heard a neurosurgeon comment to a colleague, during a cleaning-up process where a brain tumor had been, "Look there—that's all good brain." This might be called the simplest concept of function; as long as there's enough good brain left to secrete behavior, you're OK.

I don't claim that psychologists or cyberneticists have yet published anything that is very helpful in helping us understand brain structure, but I will assert that no neurologist has ever published anything (limited sample admitted) that helps us understand why our neighbors' children are so inferior to ours.

Behavioral science and neurology are still a long way apart, and the attitude of many neurologists toward the psychological approach is no help in getting them together. The childlike faith that sufficiently fine measurement and sufficiently thorough chemical analysis will explain behavior may sustain one through periods when nothing seems to explain anything, but it *is* a faith, not a fact, and ought not be referred to as if it were proven and self-evident. The essence of science is measurement, and measurements yield numbers; the numbers remain numbers until some human being fits them into his concept of a system which performs certain functions; only then do the numbers mean anything at all. And if the human being had not started with some guess about function, he never would have made a meaningful measurement.

I argue, therefore, that not only is

the functional approach essential and prerequisite to understanding of any structure but that any scientist who does more than fill catalogs full of numbers uses the functional approach all the time, willy-nilly. He does so because that is one of the functions which human beings are built to carry out.

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William Powers states a viewpoint which we support and have done much to document. Our only regret is that he should have misinterpreted the statements made in our original communication and have attributed to us a position which we deprecate. Far from implying that the differences in "normal" and "anxious" subjects "must be due to brain damage of some sort," as Powers deduced, we stressed that "studies of the microscopic structure of the nervous system have revealed no significant changes in the brains of persons suffering from the common neuroses and psychoses." The remainder of our communication presented evidence that one might be able to measure electrographically dysfunction in this group of chronically anxious subjects who have no known demonstrable damage to structure. Although Powers purports to find us hinting that structural damage is the cause of neurosis or psychoses, there is no such statement or suggestion anywhere mentioned or implied; nor indeed do we subscribe to such a view.

Our brief report concerned one electrographic method which might be used to evaluate brain function or dysfunction, regardless of the basis of the impairment. We would reaffirm our original statement that "perhaps more sensitive methods of measuring responsiveness in the electroencephalogram may demonstrate other evidence of impairment [of function] in the 'functional' disorders of the brain."

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Conversions

Pembroke J. Hart in his letter on conversions [*Science* **132**, 256 (22 July 1960)] uses a conversion factor of 1.1516 statute miles per nautical mile. This is the factor given in most current reference works, yet since 1 July 1954 the Department of Commerce and the Department of Defense have been using the international nautical mile, defined as exactly 1852 meters, for which the conversion factor is 1.1508 statute miles per nautical mile.

Prior to 1 July 1954 the United States used a nautical mile of 6080.20 feet (1853.248 meters). The international nautical mile at the time of its adoption by the United States was equivalent to 6076.1033 U.S. feet, but effective 1 July 1959 the United States adopted the international yard, equivalent to 0.9144 meter. Therefore the international nautical mile is now equivalent to 6076.11549 international feet.

It is apparent that the term *nautical mile* is ambiguous and, when encountered in a scientific paper, is difficult to interpret. As Hart points out, conversions in the metric system are much simpler.

I suggest that the use of *nautical mile* be restricted to air and surface navigation, where it has real value, and that metric distance units be used in space flight and rocketry.

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Pembroke J. Hart, in his letter on conversions, evidently used for his conversion the "old" nautical mile, which for the United States was 6080.20 feet and for the British, 6080.0 feet. The former would give his ratio of 1.1516 (1.15155).

The "new" nautical mile or international nautical mile, as defined by the International Hydrographic Bureau, was adopted by the United States on 1 July 1954; this length is 6076.1033 feet (*1*), and the ratio is 1.1507575.

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P. J. Hart's letter complaining about two instances of imprecise conversions from metric to English units is meant to point up one of the advantages of converting to the metric (decimal) system universally. I wish to use it for a different lesson. We should rid ourselves of the pedants who translate a news item about a 4540-kilogram spaceship into 10,009 pounds, as well as the squares who round off the conversion factor and come out with 9988 pounds. I don't have access to the original report but suppose that it came out of the U.S.S.R. as a news item, not as a scientific datum. The aim was to command admiration, not to provide a basis for computing the burning time of the rocket motor. Rendered into English (U.S.), the weight of the satellite, as a news item, is 5 tons.

Now for the generalizations: (i) "If you wish to have what you say remembered, put it in the words that your