## UNIVERSITY AND EDUCATIONAL NOTES

THE will of Dr. Charles W. Eliot, late president emeritus of Harvard University, leaves the bulk of his estate to his heirs and directs that the income from the Charles William Eliot Fund, raised for him by alumni, be given to the president and fellows of Harvard. The income from the fund, accrued and accruing, is to be applied at the discretion of the president and fellows of Harvard to the support of instruction or research in landscape architecture or preventive medicine. The amount is estimated at $200,000. The will further provides in connection with this bequest that in case any descendant of his by direct line should ever need aid in obtaining a thorough education, aid shall be provided from the income of the Eliot Fund.

DR. H. L. SHANTZ, botanist, senior pathologist in charge of plant geography and physiology in the Bureau of Plant Industry, U. S. Department of Agriculture, has been appointed professor of botany at the University of Illinois.

DR. RALPH V. CHAMBERLIN has resigned his position as curator of arachnids, myriopods and worms at the Museum of Comparative Zoology of Harvard University to become head of the department of zoology at the University of Utah.

DR. H. A. SIMMONS, of the University of Pittsburgh, has been appointed to an assistant professorship of mathematics at Northwestern University.

DR. RAY H. DOTTERER, of the Pennsylvania State College, has been appointed professor of psychology at Franklin and Marshall College.

PROFESSOR B. BROUWER, director of the neurological institute and laboratory of the University of Amsterdam, has been called to the Johns Hopkins University.

A. E. INGHAM, fellow of Trinity College, Cambridge, has been appointed reader in mathematical analysis in the University of Leeds, in succession to Dr. W. E. H. Berwick. Mr. Leonard R. Johnson has been appointed assistant lecturer in agricultural zoology.

## DISCUSSION

### THE CALORIMETRIC METHOD OF DETERMINING BLOOD FLOW IN THE EXTREMITIES

MY attention has been called to àn article in SCIENCE (July 2, 1926) by Dr. Sheard in which he says; "I do not believe that the Stewart equation or any similar equation can be applied correctly to the determination of blood flow. Calorimetric data *per se* can be used only for the determination of heat elimi-

nation." I confess I am at a loss to make out what it is that Dr. Sheard thinks he has discovered, nor do the elementary formulae on heat conduction which he reproduces help at all. That the heat given off can be stated as the heat given off is, I believe, incontrovertible. That it may be useful for some medical or physiological purposes to so state it without endeavoring to procure the conditions necessary for translating it into terms of blood flow, may also be true. But that it is impossible to obtain the calorimetric data under standard conditions, permitting the blood flow to be calculated from them, is demonstrably an inaccurate statement. The necessary condition is that the temperature slope in the part from the deeper layers to the surface should be stabilized by a sufficiently long immersion in the bath before the readings from which the blood flow is deduced are taken. In other words, the preliminary immersion must establish a definite distribution of temperature in the part, or a definite average temperature of the part, which henceforth remains approximately unchanged throughout the period of an ordinary experiment. The heat then given off minute by minute to the calorimeter must represent the heat lost by the blood. Dr. Sheard must know, if he has read my papers, that it is not "calorimetric data *per se*," obtained anyhow, which are applied to determination of the flow, but data obtained under essential conditions. Yet no reader would gather from his article that the establishment of these conditions is part of the technique on which the validity of the equation depends.

I do not know whether the critic really understands what is implied in the position he apparently takes up, but what is implied is a denial of the possibility of producing the steady distribution of temperature mentioned.

Now this is sheer absurdity and demonstrated to be so, for instance, by measurements of deep and surface temperature, from the earliest observations to the most recent, such as those of Taylor and of Benedict. If a steady slope of temperature, in the sense in which the term is employed by us, can not be established in a part in a given environment, and supplied by a given stream of blood, the temperatures measured by these investigators would not remain constant long enough for observations to be made. The matter is not one which needs to be settled by discussion. Simple experiments, such as comparison of the temperature of different layers of an animal's leg (not too thick) with that of the water in which it is immersed, are recommended as instructive upon this point. Sheard's remarks on the disturbing effects of opening and closing of capillaries, according to Krogh's greatly overworked conclusions, and other factors are not entirely intelligible to me, and have, I think, little bearing on the point in question.

The fundamental condition, notwithstanding what Dr. Sheard may "believe," can be realized, and therefore blood flows can be deduced from the calorimetric data. The realization is naturally easier in thin than in thick parts, in parts whose chief blood flow is superficial than in parts where an important part of it is deeper. I have, as a matter of fact, always considered the hand as approaching most nearly to the ideal requirements because its dimensions and the distribution of the principal blood flow were the most favorable, and have never used the foot, in preference to the hand, except when necessary. This is not the place in which to recapitulate the rules which must be observed if the estimations are to be properly made. I have more than once been called upon for assistance by zealous but (in this matter) ignorant persons, who were violating the most obvious rules and were disappointed with the results.

Direct comparison of the results of the calorimetric method with that of other methods have given a satisfactory agreement. For instance, Utheim in Erlanger's laboratory compared the flow in the legs of dogs, measured by the stromuhr, with that deduced for the calorimetric observations. The agreement was good. Will Dr. Sheard inform us how this could be so if the calorimetric method did not give the blood flow?

I have myself laid great stress on securing data in human cases which serve as a test of the accuracy of the method. The most valuable of these data are in cases where a known mechanical interference with the circulation was produced. For instance, the innominate artery and right carotid were ligated for aneurism. The operation caused a great reduction in the right-hand flow. But there was a corresponding increase in the left-hand flow, so that the combined flows in the two hands remained unchanged (18.9 grams per 100 cc per minute before ligation and 18.8 grams after ligation). The cutting off of the path through the inominate obviously permitted more blood to enter the alternative route of left subclavian and left carotid. That the flow in the left carotid was increased after the operation was indicated by the increased throbbing and filling of the left temporal. The case was followed, as the collateral circulation opened up, for thirty-one days after the operation. The combined flow was twenty grams per 100 cc per minute seventeen days after the ligation. The ratio of right-hand flow to left-hand flow went on increasing steadily as the collateral circulation developed (1:4.5, 1:3.2, 1:1.8, 1:1.6 in successive observations). Only a very ignorant physiologist will believe that these numbers were deduced from data from which it was impossible to estimate the blood flow.

In another case (rheumatic endocarditis complicated by multiple emboli and thrombosis), studied along with Dr. R. W. Scott, the flow in the right foot became smaller (one fifth of that in left foot at the last examination), and before death the leg became much discolored. When the flow in the right foot diminished there was a corresponding increase in that in the left foot, and the ratio of combined foot flows to combine hand flows in the different examinations did not vary over four months. It was suggested (in a paper in the *Journal of Experimental Medicine*) from the reciprocal relation of the flow in the two feet that the obstruction was in the right common iliac and not, for instance, in the femoral. This was confirmed at autopsy about two months later. It would be inappropriate to claim that there might not have been an element of luck in the prediction. But it was made on the basis of blood flow measurements, and could only have been made on that basis. Numerous other instances could be given where any physiologist would at once recognize from the order of magnitude of the numbers and their relations to each other and to the anatomical or physiological conditions, that they must represent blood flows.

G. N. STEWART

WESTERN RESERVE UNIVERSITY

## THE SHAPE OF CELLS IN MASSES

IN the issue of SCIENCE for June 18 is printed a communication from Frederic T. Lewis under the title "An Objective Demonstration of the Shape of Cells in Masses." This contains the following statement, page 608: "Meanwhile Lord Kelvin had found that a fourteen sided figure—a cube truncated by an octahedron—having six quadrilateral and eight hexagonal surfaces, solves the problem of dividing space without interstices into uniform bodies of minimal surface."

The statement is a correct expression of Lord Kelvin's claim as set forth in the somewhat famous Baltimore lectures of several years ago. But the claim is entirely wrong. Having been myself much perplexed by so direct and confident an announcement from such an eminent source, let me ask that a definite correction of a serious error be now made in order that others may be saved from being misled and perhaps from consequent mistakes.

The volume described, called the tetrakaideca-hedron, does not possess the properties as stated. Equal volumes of this pattern will not fit together without voids, as a brief consideration of the diedral angles or angles between faces and the relations of these faces or a practical trial with models would at once have shown. Lord Kelvin's subsequent discussion of similar volumes with warped or distorted faces, which is referred to in Mr. Lewis's original papers, indicates that the error was later recognized