DISCUSSION

SCIENCE, METAPHYSICS AND BLOOD

HAVING been assigned by the editor the pleasant duty of reviewing for SCIENCE Professor Lawrence J. Henderson's new book, embodying his Silliman Lectures on "Blood. A Study in General Physiology,"[1] and being just on the point of starting to write the review when SCIENCE of January 11, 1929, reached my desk, I read with great interest Professor Yandell Henderson's article in that issue entitled "Is this Science or Metaphysics?" At the bottom of page 39 I concluded that my job had been done for me, and that for once Providence, this time oddly disguised as a Yale professor, had subtracted from the obligations of an already overburdened life, rather than, as usual, adding to them. As I read on through pages 40 and 41, however, it seemed apparent that this was a too hasty conclusion. Something now needed to be said about Yandell[2] as well as about Lawrence.[2] Life almost always turns out that way.

Let me begin by stating that what follows is in no sense to be regarded as a defense of the book under discussion against the attack which has been made upon it. Many years ago there was revealed to me the simple but profound truth that the final and conclusive evaluation of all scientific work is determined by the intrinsic merits or demerits of the work itself, and not at all by what *anybody* says about it, either for or against. There appears no present reason to suppose that this law of human biology will not operate in this instance.

The purposes of this note are, first, to discuss some aspects of the book which, as it seems to me, were overlooked or neglected in the article referred to; and, second, to present another point of view regarding some general questions of scientific methodology which were raised in that article. "Blood" seems to me to be a more important book than it does to Professor Yandell Henderson, and this for three reasons. The first is that the opening chapter of the book impresses me as, on the whole, the most sound, penetrating and illuminating statement that has yet been made of the present status and the ideational development of biology as a science, on the one hand, and of the essential problem of that science—its basic *Fragestellung*—on the other hand. In the passage of time this chapter will come to be regarded a classic of biological literature, unless my judgment is greatly at fault, quite apart from any consideration as to what the ultimate evaluation of the rest of the book may prove to be.

[1] Yale University Press, 1928.

[2] Henderson.

The second reason why the book seems to me important is because of the methodology of its major portion, which deals with the experimental details regarding the physiology of the blood. The essential point of this methodology is what may be characterized as the *multiple free variable* experimental technique. It has long been a working canon of investigation in biology that what the experimenter should endeavor to do is to keep all possible *other* variables, internal and external, constant while he observed what happened relative to just *one*. In actual fact, owing primarily to the enormous intercorrelated complexity of the living organism, as well as to the extraordinary practical difficulty of keeping constant even the most simple and basic of the elements in the purely physical environment, this ideal is practically impossible of achievement experimentally, if one is thinking or working in terms of quantitative precision of anything like the same order as those in which the physicist or chemist works. Furthermore, experience in the other sciences, particularly physics, has demonstrated that it is a relatively sterile and unfruitful methodological ideal at best. Nature in general and organisms in particular, are *organized*. Event A is one thing when B_1, C_1, D_1 . . . N_1 are happening, and quite another when B_2, C_2, D_2 . . . N_2 are happening, B, C, D, etc., being events which vary as indicated by the subscripts. As Whitehead[3] says, even "an electron within a living body is different from an electron outside it, by reason of the plan of the body." Now the traditional methodological canon says that the best thing to do in making experiments is first to keep B_1, C_1, D_1 . . . N_1 constant and see what A looks like; then to hold C_1, D_1, . . . N_1 constant, and let B change from B_1 to B_2, and see what happens to A. But above all, keep everything else possible in the system except A and B under constraint while the investigation of their interrelationship is in hand. What this methodological scheme deliberately neglects (though of course no intelligent investigator overlooks it) is that when C, D, . . . N are put under constraint *A is also and by virtue of that fact put under constraint.* And it is the behavior of A over its whole range of possible behavior that, by hypothesis, we want to find out about.

There is perhaps no better example to be found of the relative sterility of this methodology in biology as compared with the fruitfulness of the multiple free variable technique than is afforded by the history of Mendelism. Before Bateson and Punnett in England and Morgan in this country got started upon the investigation of dihybrid and trihybrid ratios, out of which came the whole present-day conception of the relations of genes and the mechanism of heredity,

[3] A. N. Whitehead, "Science and the Modern World," p. 111. 1925.

Mendelism was already being said to have exhausted itself. All that was exhausted was a single constrained variable methodology.

The essential point in the methodology of Lawrence Henderson's experimental study of the physiology of the blood is that he has, in effect, observed and quantitatively measured *simultaneously,* at suitably separated intervals of time and space, the state at that instant of a whole series of *freely varying* physical, chemical, physicochemical and physiological elementary variables connected with the blood. The result of this technique has been to reach a wholly different order of understanding of the physiology of the regulation of the internal environment of the organism (to use Claude Bernard's phrase) than we have ever had before. It is not intended to imply that Lawrence Henderson is the first or the only investigator[4] who has applied this methodological technique in biology. What he has done, however, is to use it more consistently, intelligently and effectively for the solution of a definite, particular problem than has any one hitherto, so far as I am aware.

The third respect in which the book seems to me to be important is in that it achieves, within modest but by comparison considerable limits, a real *synthesis* of a previously scattered and only very partially integrated set of biological observations. Books which even attempt true synthesis are extremely rare in biology; those that achieve it in any degree are still rarer.

So much for my opinions about the book; now just a few words about Professor Yandell Henderson's. He appears to object to it on three main grounds: he doesn't like the style in which it is written; he thinks it is metaphysics and not science, and finally he thinks that its author has not been sufficiently polite to other workers on the physiology of the blood. Regarding the first of these points there seems little chance of doing anything. It is probable almost to certainty that Professor Lawrence Henderson will not change his manner of expression. In fact it is inconceivable. Nor, on the other hand, can any of Professor Yandell Henderson's friends suppose that he is going to alter his literary tastes. In emotional matters he is known not to be an altering kind of person. Plainly there is no hope on this first point. There is, however, one specific matter here to which exception must be taken. A few sentences are quoted and it is said that this statement might have emanated from Paracelsus. But has Professor Yandell Henderson ever read Paracelsus, or has he just read about him? For surely two men were never further apart in literary style than these. That the readers of SCIENCE may judge the point for themselves let me quote a few sentences from the introduction to an English translation of Paracelsus' "Nine Books on the Nature of Things," published in 1650, which happens to be on my table as I write. He is speaking of sophisters and scoffers

> who contemn all things, which are not agreeable to them, and indeed detract from them: These are pleased onely with what is their own, as indeed all fooles are wont to be, whom their owne toyes onely please, not anything which is anothers, hating all kinds of wisdome. Wherefore they account wisdom as folly: because nothing doth them any good they know the use of nothing. As one workman cannot use the instruments of another, so a foole can use no weapons better than his owne sticke, or boughes; and there is no sound pleasanter to him than the ringing of his own bell.

Now would any one maintain that Lawrence J. Henderson ever did, or could, or would, write like that?

Regarding the metaphysical disability I confess myself to be in doubt, because nowhere does Professor Yandell Henderson define precisely what he understands by metaphysics. He just calls it "that most insidious disease of scientific thought." But, as he complains about the book, this is not informative. From the context of the article as a whole, one reader, at least, gets the impression that he regards everything in science which is not the purest naive empiricism as metaphysics. But Professor E. A. Burtt,[5] who certainly has a clear notion of what metaphysics is, says (p. 137): "Of course Newton's conscious reaction to metaphysics was one of vigorous opposition, as to a collection of quite unverifiable 'hypotheses,' but since no one can avoid ultimate assumptions of some sort he was, like most scientists, a metaphysician against his will." Perhaps, in this respect, Professor Yandell Henderson is like Newton. But again perhaps he isn't. There seems to be no way to resolve this second difficulty until he defines his conception of metaphysics more precisely than as an "insidious disease."

Regarding the third point of objection to the book, my statistical instincts come to the fore and suggest an objective inquiry. With the help of the excellent index, plus a patient reading of pages where the lazy *"ff"* makes it necessary, the following table has been constructed, showing the number of different pages on which the leading investigators of the physiology of

[4] In a very modest way the present writer has endeavored to use this methodology in his investigations on duration of life described in "The Rate of Living." New York (Knopf). 1928.

[5] E. A. Burtt, "The Contemporary Significance of Newton's Metaphysics," in "Isaac Newton, 1642–1727. A Memorial Volume." London (G. Bell and Sons, Ltd.). 1927.

the blood (other than Professor Lawrence Henderson's own students and associates) are specifically mentioned or their work is discussed or both. The names are arranged in descending order of frequency of mention.

Name	Number of different pages on which investigator and his work are named or discussed
Bernard	8
Haldane	8
Krogh	5
Barcroft	4
Hasselbalch	4
Bohr	3
Yandell Henderson	2

Surely the facts disclosed by this table give no ground for the grievance that predecessors and contemporaries do not receive adequate recognition. Or do they?

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THE 1928 SILLIMAN LECTURES

THE last Silliman Lectures at Yale were delivered by Professor L. J. Henderson on a field of physiology to which he has devoted himself during the past twenty years, *viz.*, the relations between the different electrolytes, gases and proteins in the blood, and the alterations in those relations that occur during normal and pathological metabolism. The publication of these lectures in book form has drawn from Professor Yandell Henderson the savage criticism which appeared in SCIENCE of January 11. Independent opinions concerning the relative value of the studies presented and of the criticism against them can be formed only by the few who are themselves engaged in the intricate field of research covered. Hence it appears that, in fairness to those readers of SCIENCE who lack the concrete knowledge, Yandell Henderson's remarks should be reviewed by another student in the field who has formed quite a different opinion.

Essentially Yandell Henderson's criticisms may be condensed to two: (1) that Lawrence Henderson has failed to give due credit to Haldane's magnificent work, and (2) that the lectures are metaphysical.

The first criticism can be met by any one who refers to the several places where Haldane's work is mentioned in the lectures. In the writer's opinion there is no basis for complaint. The lectures are in their nature a review of Lawrence Henderson's personal work, and where it is based upon Haldane's previous discoveries that fact is acknowledged. Yandell Henderson, as an example of insufficient appreciation, quotes a paragraph from the lectures which ends with the statement, "This conclusion escaped us all, and it remained for Christiansen, Douglas and Haldane to discover by experiment that the carbon dioxide dissociation curves of oxygenatized and reduced blood are different." This statement is, it appears to the writer, a sportsmanlike acknowledgement of a debt due Haldane and his collaborators for solution of a problem which, despite its outstanding importance, had eluded other investigators.

The charge of being metaphysical appears absurd against a work which contains 225 diagrams and 86 tables, presenting chiefly quantitative experimental results obtained in Lawrence Henderson's laboratory, together with an appendix on laboratory technique. The lectures, aside from their value in affording mathematical approaches to hitherto insoluble relationships, constitute a most useful compendium of concrete facts and figures to any worker in the field: so much so that the copy in our laboratory is seldom in its place on the shelf. In the introduction, it is true, Lawrence Henderson presents a view-point concerning the historical development of general biology and concerning modes of attack on its problems; and the concluding chapter is of a broadly reflective nature: both, to the writer, afford stimulating and profitable reading. In between are eleven chapters packed with concrete quantitative observations and calculations based upon them.

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THE APPORTIONMENT SITUATION IN CONGRESS

THE apportionment problem will probably be considered again by the House of Representatives during the present session of Congress. Because of that fact and because my attitude towards it is not adequately stated in Professor Huntington's article in SCIENCE for December 14 (pages 579–582), I am glad to outline briefly the situation as I see it.

Neither the bill defeated last May nor the similar bill introduced at this session is a real apportionment bill. It is a bill authorizing a future apportionment by the secretary of commerce after the results of the census of 1930 or of any subsequent census have been announced and Congress has failed to pass an apportionment bill in the following session. Thus, if the field work on the next census should start in November, 1929, the population of the several states would doubtless be announced before Congress assembled in December, 1930. If no bill on apportionment should