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 become a law before the end of that session and if in the interim the bill defeated last May or some similar bill should have been passed, it would authorize and instruct the secretary of commerce to apportion the present number of representatives among the several states by the method used in the most recent apportionment, that of 1911, and report the results to Congress. These results would go into effect unless or until Congress changed them.

The advantage of the plan is that it would automatically readjust the existing number of representatives to each decennial shift of population, in case, and only in case, Congress failed to agree upon some other plan.

Upon the moot question of method, the main problems before the census committee which has reported the bill seem to have been these two:

(1) What method is likely to give the bill the best chance of passing Congress? The committee had to choose between a method which had been used in a previous apportionment and a novel, untried method. They selected the method of major fractions which had been used in 1911, believing, I suppose, that this choice would improve the bill's chance of passage. They are in a better position than an outsider to decide what method would be preferred by Congress, and the fact that, after considerable discussion, they introduced into this winter's bill the same method as that specified in the bill of last spring shows that in their judgment this method was not "a distinct hindrance to the passage of the bill."

(2) What method is most likely to satisfy Congress when its results are brought home to the members by a specific apportionment? At this point the method of major fractions has a marked advantage. If the secretary of commerce is called upon, for example, in 1930 to apportion 435 members by the method of major fractions, he would probably send to Congress not merely a list showing the number of representatives allotted to each state, but with it a table showing the population of each state by the latest census divided by a constant divisor and one representative allotted for each unit and each fraction larger than one half in the series of quotients. The whole series would sum to 435. But if the secretary of commerce had to perform the same apportionment by the method of equal proportions, he would probably report merely the number of representatives assigned to each state. If Congress then asked how his results were reached, he would reply either that the prescribed method did not lead up to a table such as Congress has had before it on every previous occasion, or by submitting a table with a constant divisor and a series of quotients, wherein a state with a quotient, for example, of above 1.414 and below 1.500, as well as a state with a quotient of above 1.500 and below 2.449, would receive two representatives, because 1.414 is the geometric mean between 1 and 2, and 2.449 the geometric mean between 2 and 3. In my judgment, Congress would not be satisfied with either form of reply. For this reason I believe that if the method of equal proportions should be substituted in the pending bill for the method of major fractions, the resulting law would be more likely to go the way of the law of 1850, which prescribed a method since recognized both by special students and by Congress as unsatisfactory. Partly for that reason the law quickly became a dead letter. I am anxious to avoid a similar failure of the present effort.

The choice of a method seems to me of little importance compared with the need of securing congressional compliance with the constitution. I would gladly abandon my preference for the method of major fractions if I thought another method had a better chance of acceptance by Congress and the country. As many persons interested in the practical problem seem to be baffled by the mathematics of apportionment, let me state again, as simply as possible and without argument, the essential differences between the different methods, taking for my example the population of the states in 1920, but neglecting, for the sake of simplicity, the constitutional guarantee to each state of at least one representative. The procedure may be described as follows:

(1) Arrange the states in the order of decreasing population from New York to Nevada.

(2) Divide the population of each state by a figure slightly above the population of New York. The result would be a series of decimals ranging from .999 in the case of New York to .007 in that of Nevada.

(3) Divide the population of each state by a figure slightly above that of Pennsylvania. The result would be a series of quotients ranging from 1.198 in the case of New York and .999 in that of Pennsylvania down to .009 in that of Nevada, the quotient for each state being larger than in the preceding division but by an amount which diminished with the total population of the state involved.

(4) Continue this process, steadily decreasing the divisor and thus enlarging each quotient, until the resulting quotients give a House of Representatives as large as desired.

The differences between the various methods of apportionment hinge upon the differences in weight given to the decimal fractions in these computations.

(1) At one extreme is the method of rejected fractions used in all apportionments before 1840. By that method every decimal fraction, no matter how large,

is disregarded. Thus, the division described in (2) would give no representatives; that in (3) would give one to New York, etc.

(2) At the other extreme is the method of the harmonic mean, or as I have preferred to call it in arguing before the census committee, the method of minimum range. By it every decimal fraction, no matter how small, entitles the state to a representative. Thus, the division described in (2) would give forty-eight representatives; that in (3) would give forty-nine, etc.

(3) Next to the method of rejected fractions is the method of major fractions. By it every fraction larger than one half entitles the state to an additional representative. Thus, the division described in (2) would give four representatives to the four most populous states; that described in (3) would give five, etc.

(4) Between this and the method of the harmonic mean is the method of equal proportions. By it every quotient above the geometric mean between the two numbers of representatives under consideration entitles the state to the larger number. Thus, the division described in (2) would give forty-eight representatives; that described in (3) would also give forty-eight.

During the many years that I have worked upon the problem of federal apportionment, my main object has been to improve upon the method apparently preferred by Congress. Many scholars at various times have suggested methods which they thought better; Congress has rejected them all. The only revolutionary change of method ever made resulted from the constitutional argument of Daniel Webster when chairman of a Senate committee on apportionment. The report of his committee argued that every remainder above one half entitled a state to an additional member. The Vinton method adopted in 1850 was supposed at that time to be merely a variant of Webster's method. My contribution has made Webster's method more workable.

From the point of view of Congress and the average citizen I would arrange the methods in the order of decreasing persuasiveness, as follows:

Method of major fractions
Method of minimum range
Method of rejected fractions
Method of equal proportions

On scientific grounds I would place them in the same order, if we take as a criterion, as I think we should, the degree to which the several methods satisfy the legitimate purposes of the constitution and of Congress.

The main object which Congress and the country desire to realize by an apportionment is in my opinion either one of these two:

(1) To give the residents of the United States as nearly as may be equal representation in the House of Representatives, irrespective of the state of residence; or

(2) To give the members of the House of Representatives as nearly as may be equal numbers of constituents.

It might seem as if these two objects were one and the same, although viewed from different sides. But in fact they lead to different methods of apportionment. If the first is the controlling object, the method of major fractions is the one to be used. If the second is the controlling object, the method of minimum range is the one to be used. If the two are to be given equal weight, or an average is to be struck between them, the method of equal proportions is the one to be used.

The preceding statement probably reveals my reasons for thinking it undesirable "to request a report on the mathematical facts from the National Academy of Sciences." The fundamental problems are political. What is the main object of apportionment? What method of apportionment is best calculated to satisfy Congress and the country? On problems of this sort the judgment of the average representative or congressional committee is of far more importance than that of any group of scholars.

CORNELL UNIVERSITY,
DECEMBER 24, 1928
WALTER F. WILLCOX

"UNPROFITABLE METEORS" PAY LARGE DIVIDENDS

IN the December 14, 1928, issue of SCIENCE, pages 590–1, there appears an article by my good friend Dr. Heber D. Curtis, director of the Allegheny Observatory, entitled "Unprofitable Meteors." Apparently its publication was caused by annoyance and loss of time sustained by him due to people desiring further information about the Perseid and Leonid meteor showers of this year. As a result he is rather hard on the newspaper reporters for sensational articles on the subject, and indirectly even harder on professional astronomers who were obviously the sources of their information.

Nearly thirty years' acquaintance with Dr. Curtis, and a year or more of work as his assistant at Lick Observatory, have given me the highest opinion of him both as a man and a scientist. Paradoxically, it is for this very reason that I feel compelled to point out the true state of the case, in the same journal in which his note appeared, for otherwise I fear his remarks will do real harm to amateur astronomy.