and states: "Of course there is life on Mars; there is no doubt about it."

Lowell has been fortunate in being able to personally build and maintain an observatory, which has been the means of advancing the science of astronomy in a number of lines. See asks what Blackwelder has done in comparison. This question implies that only those whose personal fortunes have enabled them to do what Lowell has done should criticize his work, since those familiar with the scientific results of both will hardly see cause on such lines for invidious comparison.

Blackwelder casually mentions, to the extent of one sentence, "Lowell's implicit belief in the Laplacian hypothesis which now, to say the least, is on the defensive," a remark which calls forth a column from See embracing such statements as, "If Professor Blackwelder will study my own (See's) paper carefully, and the work now in press (by See) when it appears, he will find that most of the recent speculations on cosmogony are not worth the paper they are written on."

See further states that he has proved in four memoirs "that the oceans are gradually drying up and the land increasing, as Lowell maintains. Therefore Lowell is right and Blackwelder wrong; and that too in a subject which he represents as his own." This statement is highly amusing, to say the least, to those cognizant of recent work on paleogeography, especially if they have also read See's voluminous publications on mountain building and related subjects, and noted that they center about the old hypothesis of a free downward permeation of ocean water. A hypothesis which is not open to direct proof, and though still advocated by certain physicists and geologists is distinctly relegated to a subordinate rôle by many economic geologists and such leaders in the more philosophic side of the earth-science as Suess, Chamberlin and Van Hise; partly because of the theoretical difficulties attending an effective downward diffusion of ocean water through the zone of rock flowage, but much more because of the failure of the hypothesis to account for many of the facts now known to geologists. These point rather to a directly opposite view, which is well expressed by the words of Suess, "volcanoes are not fed by infiltration from the sea, but the waters of the sea are increased by every eruption."

The voluminous nature of See's writings on the subject is due to a dressing out of this old and, to say the least, doubtful hypothesis with many speculative additions, with much repetition of well-known facts and theories, and with specific applications in such frequent obvious discord with modern teaching of the principles of physiography and known details of geologic structure and history, that no geologist has felt called upon to comment. In the words of See, "geologists have discreetly kept silent."

On every topic See cites his own work as the authoritative utterances on the subject, and in the last paragraph denounces, as the worst evil of American science, "this clique and faction business, by which a man who is not in the ring never can get justice or fair consideration." Since no group of geologists or, so far as the writer is aware, no single geologist of recognized standing has followed and promulgated the special views in the teachings of See and Lowell, this clique and faction evidently includes the several hundred working geologists of America. To those who are familiar with the situation, this gives the key to the whole of See's article on "Fair Play and Toleration in Science." It is a vicarious castigation in which Blackwelder stands to receive the blows for a host of unnamed men of science, because they have not accepted See's memoirs at the valuation which he places upon them. Is vicarious atonement "fair play and toleration in science"?

JOSEPH BARRELL

NEW HAVEN, CONN.,
June 15, 1909

DETERMINATION OF THE COEFFICIENT OF CORRELATION

TO THE EDITOR OF SCIENCE: I should like to make a few remarks on Dr. Franz Boas's letter on this subject in your issue of May 21. There is some danger, I think, unless we see

how new values for the correlation coefficient are related to old values, in a multitude of formulæ leading to divergent and possibly inconsistent results.

Dr. Boas's first value for r

$$-r=\frac{[(x-y)^2]-\sigma_x^2-\sigma_y^2}{2\sigma_x\sigma_y}$$

is a very old friend indeed and has been widely used in a multiplicity of practical cases. It is one of a general series of formulæ noted by me[1] in 1896, and used in our work on the personal equation[2] in 1902 and on wasps[3] in 1906. Since 1896 it has been frequently referred to, *e. g.*, in the memoir "On Further Methods of Determining Correlation"[4] and *Biometrika*, VI., p. 438, etc. It is quite reliable and often convenient.

Dr. Boas's second formula

$$r=\frac{[(x_1+x_2+\cdots+x_n)]^2}{n(n-1)\sigma_x^2}-\frac{1}{n-1}$$

suffers from the difficulty that in the form in which he gives it, it involves the number in the fraternity, being taken as *constant*, whereas in practise we may often have five in one fraternity and ten in a second. Its chief value is when n is very large as in long series of homotypic characters, or in series other than man when the number of offspring is very great. In such cases the second term $1/(n-1)$ is usually of the order of our probable error and may be neglected and $n-1$ may be taken $=n$, within the same limits. Thus:

$$r=\frac{(\text{S.D. of means of fraternities})^2}{(\text{S.D. of population})^2}.$$

Under this aspect it is easy to extend the formula to cases in which n is not the same for each fraternity. A like formula was used in 1898 for our studies on the inheritance of fecundity of thoroughbred horses.[5] It has been since employed in various homotypic investigations. It must be very carefully distinguished from that for the correlation rates

$$\eta=\frac{\text{S.D. of means of arrays}}{\text{S.D. of population}}$$

where $\eta=r$ for normal correlation.

The *arrays* in the latter formula contain many *fraternities*, and their means have far less variability than that of those of fraternities.

Lastly I come to Dr. Boas's formula

$$r=\frac{p_{1,2}-p_1p_2}{\sqrt{p_1(1-p_1)p_2(1-p_2)}}.$$

If we have a fourfold table represented by

a	b	$a+b$
c	d	$c+d$
$a+c$	$b+d$	N

I find Dr. Boas's r is our old friend

$$r_{bk}=\frac{ad-bc}{\sqrt{(a+b)(c+d)(a+c)(b+d)}},$$

i. e., is the correlation in the deviation of the mean of one variable from its mean value with the deviation of the mean of the second variable from its mean value. It is not a true correlation of the first variable with the second variable. I have discussed v_{bk} at length in memoir of 1900:[6]

> It has the advantage of a symmetrical form and a concise physical meaning. It does not, however, become unity when either, but not both b and c vanish, nor does it, unless we multiply it by $\pi/2$ and take its sine, equal the coefficient of correlation when $a=d$ and $b=c$.

Thus it differs in the simplest cases from the true coefficient of correlation, and often differs considerably. In the bulk of cases it does not approach r nearly as closely as the "Q_5" coefficient of association, and its use is liable to be misleading, especially if compared with values of the true coefficient found by other processes.

When there is a measurable quantity grouped in arrays under classes of a non-measurable quantity the right method, I venture to think, is to use the correlation ratio η as defined above. This will be equal to r if the correlation be normal, and if not it has a perfectly definite physical meaning of its own.[7]

[1] *Phil. Trans.*, Vol. 187 A, p. 279.
[2] *Phil. Trans.*, Vol. 198 A, p. 243.
[3] *Biometrika*, Vol. V., p. 409.
[4] Dulan & Co., Drapers's Research Memoirs.
[5] *Phil. Trans.*, Vol. 192 A, p. 272.
[6] *Phil. Trans.*, Vol. 195, pp. 12 and 15 bottom.
[7] "On the General Theory of Skew Correlation, etc.," Drapers's Research Memoirs, p. 10.

I am not able to follow Dr. Boas's deduction of a formula for r in this case, and it does not appear to give the true correlation r of the two variables.

KARL PEARSON

BIOMETRIC LABORATORY,
UNIVERSITY COLLEGE, LONDON,
June 6, 1909

THE DARWIN CELEBRATION AT CAMBRIDGE

TO THE EDITOR OF SCIENCE: I shall be obliged if you will allow me to contradict a statement which has been made in an American newspaper in reference to Professor Haeckel and the Darwin Celebration. The article in question was sent to me by a friend as a cutting and I am unable to give the name of the newspaper. The writer of an article entitled "Haeckel, the fighting scientist retires from Jena University," says: "He (Professor Haeckel) would have been glad to accept an invitation to the Cambridge celebration of the Darwin centenary—had he received it. None came, however, although a large number of such invitations have been sent to scientists who, to say the least, are no more distinguished than himself and to hundreds of scientific societies. It is strongly suspected that clerical prejudice has had a large share in this extraordinary omission. It is quite unjustifiable, for, whatever may be thought of Professor Haeckel's philosophic speculations, not even his enemies venture to deny his great service in the development of Darwinism."

The facts are these: A large number of universities, academies and learned societies were invited by the University of Cambridge to appoint delegates to attend the Darwin Celebration in June of this year. In response to this invitation the University of Jena appointed Professor Haeckel as its delegate. At a later date, after replies had been received from universities and other corporate bodies, several invitations were sent to individuals other than those already nominated as delegates. A short time ago Profesor Haeckel wrote to express his regret that ill-health rendered a visit to Cambridge impossible, and his successor in the chair of zoology, Professor Plate, was appointed in his stead. I need hardly add that if Professor Haeckel had not been appointed a delegate he would certainly have been invited as a private guest. I may state that some years since Professor Haeckel received from Cambridge University the honorary degree of doctor of science.

I am, Yours faithfully,

A. C. SEWARD

One of the Honorary Secretaries to the Darwin Celebration Committee; Professor of Botany in the University.

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QUOTATIONS

VIVISECTION

LITERARY reference or allusion makes readable sometimes the barer facts of science. The vogue of Rudyard Kipling will render more popular a scientific cause to which he happens to lend his name. It is for that reason, rather than for the value of his statement, that we quote the poet as follows on a question of the day:

> The doctor is exposed to the criticism of persons who consider their own undisciplined emotions more important than mankind's most bitter agonies; who would cripple and limit research for fear research might be accompanied by a little pain and suffering. But if the doctor has the time to study the history of his own profession he will find that such persons have always been against him—ever since the Egyptians erected statues to cats and dogs on the banks of the Nile.

The opponents of vivisection ought to oppose murder, and therefore to be vegetarians. They should also object to forced labor and therefore never ride behind a horse. They should in sound logic oppose larceny and not drink milk. They should never allow an animal to be punished in process of being trained. In scientific experiment few animals are taken, compared to those killed for food or kept at forced labor all their lives. Most of them are unconscious. The question of when to use anesthetics must be left to science, since in a small but important fraction of the work drugs must be dispensed with; and it would be fatal to have ignorant outsiders concerned in so critical a decision. Such outsiders are cap-