excuse is provided for the first time for the public to leave research in the universities to "look out for itself."

I am not interested in discussing here the ethics or morality of the matter. The way it is working out is proving dangerous: it tends to shut off unselfish exchange of ideas and information, it tends to kill a critical and impartial attitude, it tends to introduce quarrels and bitterness and to consume time and funds in lawsuits. It may quite naturally influence the choice of university personnel and the choice of research problems. If, in addition, the policy of taking out patents for revenue be interpreted as a declaration of independence the public may quite cheerfully acquiesce and leave research work to earn its own way. Why should gifts intended for the general welfare play the rôle of capitalizing a business? And what becomes of the peculiar function of university research as contrasted with that of the shrewdly administered business enterprise?

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THE COEFFICIENT OF CORRELATION

IN a recent article describing a least-square curve fitting machine[1] the authors make use of the Pearson product-moment coefficient of correlation to test the agreement of the parameters obtained by use of this device with those obtained by an algebraic method. This use of the correlation coefficient to test the agreement of two methods of measuring the same quantity is frequently seen in the literature, but the soundness of the procedure is open to question.

The idea of correlation as conceived by Galton implies that there is such a measurable quantity as *degree of relationship,* and the coefficient developed by Pearson is taken to give this measure. In keeping with this idea it is argued that if two methods of measurement agree well, their results will be "closely related" and *vice versa;* hence the use of the coefficient. As a matter of fact, the correlation coefficient is more correctly understood, specifically as a particular parameter obtained in a least-square fit of a straight line to the data correlated, and its significance is not general, but varies with the character of the data to which the fit applies.

In the case at hand, it can easily be shown that the coefficient measures, in large part, something quite different from what is intended. The method of procedure when two instruments or methods of measurements are to be compared is as follows: A series of determinations over the entire range of instrument or method x is made, and upon the same data determinations are made with instrument y. The corresponding measurements are then correlated. The coefficient may be given by

$$r=\sqrt{1-\frac{\sigma_{x.y}^2}{\sigma_x^2}}$$

Where r is the coefficient, σ_x is the standard deviation of the x measurements, $\sigma_{x.y}$ is the partial standard deviation of x, for a fixed value of y. Now, in instances of this kind, the distribution of x is not generally normal, but rectangular, *i.e.,* about the same number of measurements is made at each value of x. Under these circumstances σ_x depends on the range of x, being given by $\sigma_x^2=\frac{R_x^2}{12}$ where R_x is the range of x. Consequently, r is greater the greater the range of the measurements made. That the method can easily lead to paradoxical results may be seen as follows. Suppose two instruments to be uniformly comparable over their entire ranges, one with the other. A series of measurements is made over the lower half of the range and correlated, yielding a particular value of r. The same procedure is repeated for the upper half and the same value of r is obtained, appearing to give a good check of the measure of how well the two methods agree. But if now all the data are put together as a single series, the correlation will be greater. For instance, if r was 0.5 in the first case, it would be 0.9 in the second.

This fallaciousness in the use and interpretation of the correlation coefficient creeps in in other instances than where methods of measurement are compared. For instance, it is present when a physical trait like height is correlated with age, and the age range is arbitrary. It also vitiates not a little of the use made of the coefficient by psychologists.

In passing, referring to the main topic of the article first mentioned, it may be noted that the idea of obtaining a least-square fit of a line on the principle of elastic bands stretched from the points to be fitted was mentioned at least as early as 1921 by L. J. Reed.[2]

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ATTEMPT TO CONFIRM THE EXISTENCE OF A FILTRABLE CYCLE OF BACTERIA BY THE USE OF "K" MEDIUM[1]

AN attempt has been made to confirm the results of Kendall reported in the *Northwestern University Bulletin* (xxxii, 5, 1931). Complete failure with 'K' medium made by us according to his directions, was

[1] Gains and Palfrey, "A Least-Square Curve Fitting Machine," SCIENCE, 76: 472, November 18, 1932.

[2] Lowell J. Reed, "Fitting of Straight Lines," *Metron*, Vol. 1, p. 3, 1921.

[1] A detailed account of the work appears in the *Journal of Infectious Diseases*, 52: 20. 1933.

followed by experiments with 'K' powder kindly sent to us by Dr. Kendall. A protocol suggested by him, using the Rawlins strain of *B. typhosus,* was followed exactly. At least five attempts in accordance with this protocol and many more based on the first reports of the medium were made.

In connection with the 'K' medium, many difficulties were encountered; for example, no uniformity of pH from tube to tube could be attained. It also occasionally develops a spontaneous turbidity without inoculation.

In summary we may state that persistent efforts to confirm Dr. Kendall's experiments have been uniformly negative.

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UNIVALENT CHROMOSOMES OF TRADESCANTIA VIRGINIANA

AN esteemed correspondent, in SCIENCE (Vol. 77, pp. 49–50), states that, in *Tradescantia virginiana,* "the apparently serially arranged, miscalled univalent chromosomes represent in reality a segmented pachytene in which the synaptic pairs are lined up, back to back." I have lately been studying the pachytene of this species, and also of *Rhoeo discolor:* and have succeeded in obtaining clear preparations. In Rhoeo, only the sub-terminal parts of the 12 chromosomes synapse; and are afterwards seen at diplotene to separate, leaving terminal junctions. Thus the 12 bodies seen in the rings appear to me to be these univalents attached at the ends. For if they were "pairs . . . back to back," there would be, I think, 6 of them. In *Tradescantia virginiana* (which is, in my opinion, rightly regarded as a tetraploid), 12 of the 24 chromosomes can be seen to have synapsed, at pachytene, with their 12 homologues; while their sub-terminal parts show synapsis between 4 chromosomes. These can be seen to separate at diplotene, except for their terminal junctions. The 24 bodies seen in rings, in chains, or singly, are, I think, the univalents. For if they were "pairs . . . back to back," there would be apparently 12 of them.

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SPECIAL CORRESPONDENCE

UNIVERSITY EXPEDITION TO STUDY THE NATIVES OF CENTRAL AUSTRALIA

IN August, 1932, an expedition to study the natives of Central Australia was organized by the Board for Anthropological Research of the University of Adelaide, in conjunction with the South Australian Museum. The expenses were chiefly met by a grant from the Rockefeller Foundation, administered through the Australian National Research Council. The situation chosen on this occasion was Mount Liebig, situated almost on the Tropic of Capricorn, 200 miles by track west of Alice Springs. The natives belonging to this portion of Central Australia have hitherto escaped contact with Europeans, save to a very superficial extent. By means of a preliminary survey by Mr. Kramer, of the Aborigines Friends Association, in whom these natives had complete confidence, over 130 aborigines, including men, women and children, were gathered together for observation. Their equilibrium had been a little disturbed by a raid from an adjacent tribe, in which 7 men had traveled 100 miles to secure two women, killing their common husbands and one of the women, and abducting the other.

The personnel of the expedition included amongst others Dr. T. D. Campbell, to whom much of the credit of its success is due, Professors T. Harvey Johnston, C. S. Hicks and J. B. Cleland, Dr. H. K. Fry, Mr. H. M. Hale, director of the South Australian Museum, and Messrs. N. B. Tindale and J. H. Gray. As on previous occasions, the natives submitted themselves to many kinds of tests, some of them very trying, without the slightest murmur or objection.

Their good humor and sense of fun were again shown to a striking degree. Not the slightest attempt was made by any native to abstract articles from the camp, even though they could very easily have done so and must have coveted many of the objects seen. Any article accidentally lost by the party was almost invariably found by the natives and returned; one member, whilst watching a corroboree at night, dropped, without knowing it, two postage stamps; these were picked up and returned to camp, evidently being recognized as something unusual.

As the expedition had only a short time at its disposal, the study of the natives had to be concentrated. In physical anthropology this is an advantage; the native readily tires and novelty soon wears off. It would be probably difficult to hold together a body of 100 natives for a period as long as a month; many would desire before then to go for a "walk about."

Professor Hicks with his assistants studied each day the basal metabolism of two fresh natives. This was accompanied by a close study of the pulse and respiration rates, and of the body and skin temperatures throughout the morning as the warmth increased.

[1] Died February 28, 1933.