and auricles but a much shorter head than that occurring in the normal forms. This might seem to indicate that, simultaneous with the non-specific effect of x-rays upon head development, other effects, as upon cell division and growth, are occurring. As regards the non-specific nature of the more immediate effects of x-rays, it is significant that for any one dose of 4, 8 or 12 skin units the range of distribution of types regenerated increases as crowding increases.

No explanation is offered regarding the significance of the disappearance of tissue differentiated during the first two weeks. It seems to be an effect of x-rays on planarian tissue which gains expression at a period after regeneration has proceeded to its limits and which is first apparent in that region of tissue having the highest rate of metabolism.

In all x-rayed forms receiving 4, 8 and 12 skin units, the ultimate effect of x-rays is complete cytolysis. Cytolytic effects first become prominent on the thirty-fourth day after exposure. The rate of cytolysis is not greatly affected by crowding. The results of these experiments regarding the effects of x-rays are in accord with those of Bardeen and Baetjer, who conclude that x-rays affect cell division and cell differentiation and that the effects are probably confined to these two. They cite evidence from which they conclude that cell differentiation is not as much affected as cell division and that the effect upon cell division is not direct.

While both the more immediate effects and the delayed effects of x-rays may be specific upon the protoplasm, it does not necessarily follow that, because head frequency is affected by x-rays, the factors which control head frequency are specific and directly related to the activity of special formative cells. The formative cell theory of Curtis does not recognize the fact that the variation in head forms regenerated are the same type as those produced by other physical and chemical agents. It is no more necessary to assume the selective action of x-rays on formative cells than it is necessary to assume selective action of other physical and chemical agents which alter head frequency. The first apparent effects of x-rays, like various other agents, seems to be not on special formative cells but upon non-specific protoplasmic factors upon which head development depends.

Crowding varies only the rates at which effects of x-rays gain expression, allowing, in some instances, an increase in head frequency and a delay in cytolysis. As for the ways in which crowding alters these effects of x-rays on head frequency and length of life, the following possibilities may be considered: (1) Mechanical stimulation of the group upon each member; (2) a lowering of metabolic rate favoring a delay of cytolysis; (3) whatever these factors favoring group survival are, they are more effective as the dose of x-rays is increased.

The above conclusions were reached after studying the effects on over 800 Planaria.

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THE METHOD OF PROBITS—A CORRECTION

SINCE submitting the paper which appeared under the above title,[1] my attention has been called to recent papers by Hemmingsen[2] and by Gaddum,[3] in which substantially the same method has been proposed for toxicity tests with mice. Their "normal equivalent deviations" are measured from zero at 50 per cent. kill, taking the standard deviation as the unit, so that the elimination of a change in sign at 50 per cent. kill, as provided by the "probits," seems justified. However, the constant multiplier of 1.344447, used to equilibrate 0 and 10 on the probit scale with 0.01 and 99.99 on the percentage scale, interferes with the conversion from one system to the other. It seems desirable, therefore, to redefine the probit unit as equal to 5 plus (algebraically) the deviate of the normal curve expressed in terms of its standard deviation. As convenient sources of this deviate, either the Sheppard-Galton Table I[4] or the column of x corresponding to p and q in the Kelley-Wood Table[5] may be suggested. At 50 per cent. kill, the probit will be 5.00 as before; below 50 per cent. kill it will

TABLE I

Per cent. kill	Probits	Per cent. kill	Probits	Per cent. kill	Probits	Per cent. kill	Probits
1.0	2.674	50.0	5.000	80.0	5.842	95.0	6.645
5.0	3.355	52.0	5.050	81.0	5.878	96.0	6.751
10.0	3.718	54.0	5.100	82.0	5.915	97.0	6.881
15.0	3.964	56.0	5.151	83.0	5.954	98.0	7.054
20.0	4.158	58.0	5.202	84.0	5.994	98.5	7.170
25.0	4.326	60.0	5.253	85.0	6.036	99.0	7.326
30.0	4.476	62.0	5.306	86.0	6.080	99.1	7.366
34.0	4.588	64.0	5.358	87.0	6.126	99.2	7.409
36.0	4.642	66.0	5.412	88.0	6.175	99.3	7.457
38.0	4.694	68.0	5.468	89.0	6.226	99.4	7.512
40.0	4.747	70.0	5.524	90.0	6.282	99.5	7.576
42.0	4.798	72.0	5.583	91.0	6.341	99.6	7.652
44.0	4.849	74.0	5.643	92.0	6.405	99.7	7.748
46.0	4.900	76.0	5.706	93.0	6.476	99.8	7.878
48.0	4.950	78.0	5.772	94.0	6.555	99.9	8.090

[1] SCIENCE, 79: 38, January 12, 1934.

[2] A. M. Hemmingsen, *Quart. Jour. Pharmacy and Pharmacol.*, 6: 39 and 187, 1933.

[3] J. H. Gaddum, Med. Res. Counc. Spec. Rept. 183, His Majesty's Sta. Of., 1933.

[4] K. Pearson, "Tables for Statiticians and Biometricians. Part I," Cambridge.

[5] T. L. Kelley, "Statistical Method," Macmillan, 1923.

equal 5 minus the deviate read from one of these tables; and above 50 per cent. kill 5 plus the corresponding deviate. For convenience, these corrected probits are shown in Table I for the same percentage kills as before.

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WHO'S WHO IN THE BERI-BERI VITAMIN FIELD

I HAVE been surprised and somewhat overwhelmed by the amount of publicity which our recent work on the antineuritic vitamin has had. The press has perhaps naturally ignored a great deal of other work of equal or greater importance. In the April 6 issue of the *World-Telegram* there was an editorial, the overemphasis of which upon my own achievement I have attempted to correct by writing the editor of that newspaper along the following lines:

Your editorial of April 6th on the beri-beri vitamin is one of a gratifying series of newspaper recognitions of the work of our group, Mr. R. E. Waterman, Mr. John C. Keresztesy, Miss Marion Ammerman and myself. As is probably inevitable, popular taste for a hero being what it is, the press articles have generally accorded me an over-generous share of the credit, to the detriment of this group of loyal collaborators. I wish here publicly to record their substantal part in the undertaking and our debt to Dr. W. H. Eddy of Teachers College whose interest and influence has been indispensable to success.

But I am especially concerned about your comment in that editorial on the work of Eijkman. His was an achievement of first rank. Without the experimental production of the disease in animals progress would have been impossible. While it may seem obvious now that the disease can be produced by feeding animals on polished rice, it was not obvious then. Indeed the disease he produced was not generally accepted as beri-beri for fifteen years after Eijkman's first paper. During this time his conclusions had to be reinforced by supplementary work of Pol, Grijns, Fraser, Strong, Vedder, Andrews and a score or more of others.

Notable names in the subsequent developments include Casimer Funk, a Pole, who while working in London first correctly guessed the general nature of the curative substance, Seidell of Washington, D. C., who invented the use of fullers' earth for adsorbing the vitamin, and Jansen and Donath who working in Eijkman's former laboratory in Java, first isolated small amounts of the substance and described it. Peters of Oxford, England, Ohdake of Japan, Windaus of Germany have also made important advances. I could, however, fill a column of your paper with the names of those who in various ways and in many lands have added their bits to the beri-beri vitamin problem.

Science is international. Science at its best is also a fraternity. As in other fields of endeavor, we must recognize that in reaching for our objectives we stand on the shoulders of our predecessors and companions.

R. R. WILLIAMS

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"WHEN THE SKY RAINS STONE"

IN the issue of *The Literary Digest* for March 17 there appears an article under the caption, "When the Sky Rains Stone." The article is presented under the name of the present writer. The facts are that the article was written by a professional magazine writer after an interview and was not seen by the present writer until its appearance on March 17.

Unfortunately the article does not in every instance present the views of the man whose name it bears. He wishes to use this opportunity for disclaiming its authorship.

H. H. NININGER

DENVER, COLORADO
MARCH 28, 1934

WHO PAYS REPARATIONS?

From time to time, in the columns of this and other journals, gentle voices of protest have been raised against the prices for scientific books charged by the German publishers. I would like to draw particular attention to the latest flagrant example of "gouging the public."

We are informed that the most recent "supplement" volume to "Beilstein" can be supplied to us at the modest price of $60.55! Since, unfortunately, "Beilstein" still remains the bible of the organic chemist, since these "supplement" volumes come out ever so often, and since one must, after all, keep up-to-date, why not charge any fancy price that you want to charge? Apparently, so argue the Germans.

We in the department of chemistry at the college have decided not to get any further volumes until the Germans cut down these "reparation payments."

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REPORTS

GRAVITY STATIONS ON THE NILE DELTA

IN a report on the geodetic work accomplished in Egypt for the years 1930 to 1933, which was presented at the meeting of the International Geodetic Association held in Lisbon, Portugal, in September, 1933, is a brief account of the gravity survey that was made over the Nile Delta.

Many geologists have felt that the earth's crust is