Comments and Communications

The Synthesis of Vitamin B_{12} in the Digestive System of the Sheep

Several months ago it occurred to us that a relationship might exist between cobalt deficiency disease in sheep and the fact that vitamin B_{12} contains cobalt (Smith, E. L. *Nature*, Lond., 1948, **162**, 144). Accordingly we fed one sheep 0.4 milligram of cobalt containing radioactive Co^{56}, and a second sheep 1 mg of cobalt containing traced cobalt. On subsequent examination of the feces it was found that more than half of the traced cobalt had been incorporated into an organically bound form. On treatment of the feces with 0.5 *N* HCl almost all the active cobalt could be extracted. On extraction with butanol most of the activity went into the organic solvent in a manner similar to the behavior of B_{12} obtained from liver extracts (*ibid.*, **161**, 638). Tests with inorganic cobalt show that a negligible amount passes into butanol from water solution under these conditions. Biological assay, using both *Lactobacillus lactis* Dorner (Shorb, M. S. *Science*, 1948, **107**, 397), and *Lactobacillus leichmannii*, indicated the presence of large amounts of vitamin B_{12}. Thus, sheep feces appear to be an important source of B_{12}.

We have seen the paper of L. S. Gall *et al.* (*Science*, 1949, **109**, 468), showing that the growth of certain bacteria in rumen of the sheep is stimulated by the administration of cobalt. It is tempting to assume that these rumen bacteria synthesize the B_{12}.

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Scientific Research vs. the Theory of Probabilities

There is an increasing tendency to force use of the theory of probabilities upon those engaged in scientific research. To me, scientific research is the attempt to discover and establish principles for accurate prediction of what will happen. Can we use measurements and the absolute truths of the mathematicians for accurate prediction in human or biological affairs?

To whom should one go for accurate prediction as to how long one will live? Mathematicians deal with this subject by means of the theory of probabilities, and the actuaries they train make the necessary calculations for life insurance companies. For the purposes of these companies one goes to a medical examiner to be classified as to length of life. However satisfactory for the companies these calculations and classifications may be, for the individual case the prediction may seem no better than that made by an astrologer. I was refused life insurance over twenty years ago, and the other day a neighbor was accepted for life insurance in the morning and died going upstairs in the afternoon!

The prestige of mathematics is so great that many persons forget that even in mathematical hands, *probability*, *chance*, and *random* mean ignorance. They come to think that, in the alembic of mathematics, chance in some way becomes certainty. They take great care to select random samples without realizing that, insofar as a sample has been random, they don't know how it was selected.

The biologist's greatest gift from mathematics might well be, not a theory that may delude him into belief that he is wise when he is ignorant, but rather the ideal of clear definition and precise use of his terms and symbols, not excepting *science* and *research*. When we are faced with discrepant results in our handling of facts, four courses are open to us. First, we may gloss over our failure in prediction by saying that the exception proves the rule. Second, we may abandon our principle of prediction and fold our hands. Third, we may hold onto that principle and by piling up results and treating them mathematically try to show accurately, for intellectual satisfaction or for practical action, just how much or how little the principle determines what happens. Scientists who content themselves with testing theories or supposed principles can well use the theory of probabilities and may call this scientific research. To go no further is to abandon the search for new principles that may permit accurate prediction in the individual case.

Finally, we may be stimulated by the discrepancy between our results and our expectations to discover unknown principles; this will be true scientific research. It is to be contrasted with, not assisted by, use of the theory of probabilities. The latter is a most valuable tool for practical action on the basis of current knowledge and current ignorance.

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Name of the Soybean

J. Paclt (*Science*, 1949, **109**, 339) has proposed that the name *Glycine Max* (L.), as used for the soybean, be rejected as having been based on a *nomen confusum* (*Phaseolus Max* L.) and that the name *Glycine Soja* (L.) Sieb. et Zucc. be taken up in its place. Perhaps no plant has been subjected to more nomenclatorial buffeting and name-changing than has the soybean—a situation that always is unfortunate, and the more so for a plant of economic importance. In a more recent extensive accounting for the correct name of this plant I have presented detailed analyses to support the contention that the legitimate name of the soybean is *Glycine* Max (L.) Merrill (Lawrence, G. H. M. *Gentes herbarum*, 1949, 8 fasc. 1.)

The name of the soybean dates from 1753 when, in his

Species plantarum, Linnæus designated it as *Phaseolus Max.* The description he gave is of itself inadequate. Paclt alluded to the presence in this description of "some specific characters derived from another element, namely *Phaseolus Mungo* L." In the absence of specific details in support of his claim, it is indeed hazardous to accept his contentions and, contrary to his statement, I know of no contemporary botanists who treat the mung bean as conspecific with the soybean. Offsetting this deficiency in his description of 1753, the earlier references cited by Linnæus and the available type specimen of the plant make clear the identity of the soybean. Careful study of them fails to indicate the basonym of *Phaseolus Max* L. to be a *nomen confusum.* The specimen of *Phaseolus Max,* on which Linnæus based his name, was provided him by George Clifford, and is currently reported to be in the Linnæan herbarium. The more ample description by Linnæus in *Hortus Cliffortianus* (1738) is presumed to have been based on the same Clifford specimen, and this earlier account may serve to supplement the inadequate diagnosis in *Species plantarum.*

It is the opinion of Paclt (*loc. cit.*) and, for wholly different reasons, of Hill (*Bot. Mus. Leaflets Harvard Univ.,* 1939, **7**, 107) that the name of the soybean is *Glycine Soja* (L.) Sieb. et Zucc. The name as used contemporarily, and not originally by Siebold and Zuccarini, was based on *Dolichos Soja* L. As was true of *Phaseolus Max,* Linnæus provided only a fragmentary description of *Dolichos Soja* in his *Species plantarum,* but cited his earlier and identical description as given in the *Flora Zeylanica* (1747). This earlier description was based on a specimen collected from cultivation in Ceylon by Paul Herman prior to 1677. After Linnæus' time the wild indigenous prototype or counterpart of the soybean became known to science. Moench (1794) considered it distinct from the cultigen and named it *Soja hispida.* In 1845 Siebold and Zuccarini treated the same plant under the new name of *Glycine Soja.* This is a case involving two different types of specimens collected from two divergent geographic regions: *Dolichos Soja* L. from cultivation and *Glycine Soja* Sieb. et Zucc., an indigen. Other early botanists considered the two plants to be different entities; later botanists have treated them as conspecific. However, by Article 18 of the Rules of Botanical Nomenclature, we are not allowed to take up a name based on a different type from that accepted by the author of the name. Siebold and Zuccarini clearly excluded Linnæus' *Dolichos Soja* from their concept of *Glycine Soja.* It is most unfortunate that they chose the name *Soja* for their plant. Because of these circumstances it is incorrect to cite Linnæus as a parenthetical author of their binomial.

I have attempted to refute Paclt's contention, unsupported by requisite data, that *Glycine Max* (L.) is based on a *nomen confusum* and to show that in no case is the name *Glycine Soja* Sieb. et Zucc. available as a legitimate name for the soybean. It seems clear to me, until such time as the case may be reviewed and an opinion given by more competent authority, that we should continue to designate the soybean as *Glycine Max* (L.) Merrill.

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A Six-Segment Head Regenerate in a Supposedly Refractory Earthworm Species, *Lumbricus castaneus* Savigny 1826

It has been shown (Carpenter, E. *Science,* 1948, **108**, 625), that, contrary to general belief, a head of six segments may be regenerated in the manure worm, *Eisenia foetida* (Savigny) 1826. This species, in proper laboratory conditions, regenerates readily and rapidly. *Lumbricus castaneus,* however, has been thought to have little or no regenerative capacity, presumably because of Hescheler's failure to secure regeneration (*Z. Nat.,* Jena, 1896, **30**, 177).

Material was secured from a pile of old leaves behind a Harvard building. Experimental conditions were the same as for *E. foetida* (Gates, G. E. *Biol. Bull.,* 1949, **96**, 129), except that in this case all regeneration was terminated at 30 days. The species has been found only twice in the U. S., and inability to secure further material ended the experiments.

All posterior substrates with transections at levels from 4/5 to 7/8 inclusive survived and regenerated (no operations behind 7/8). Regenerates at 4/5 or 5/6 had little or no metameric differentiation. Regenerates at the next two levels were normally cephalic, of three (1 specimen) and four segments (1) at 6/7, and at 7/8 of six (1) and 5½ (1) segments. In the latter case the half-segment was wedge-shaped and on the left side. The prostomium of each regenerate, apparently completely differentiated, was epilobic, rather than tanylobic as supposedly characteristic of the genus *Lumbricus.*

Regeneration of a normal head of six segments at 7/8 enables prediction of a species capacity to regenerate equimeric heads at 6/7 and all levels anteriorly.

A six-segment-head regenerate from such a limited number of operations, on a supposedly refractory species, seems to warrant another prediction, namely, that further investigation will show that the capacity for head regeneration, throughout the family Lumbricidae, has been underestimated.

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Determination of Condition of Oysters

It is difficult to devise a method of evaluating the condition of an organism by making analyses of only a few of the factors concerned. A recent publication by Robert M. Ingle (*Science,* 1949, **109**, 593) illustrates the nature of this problem in the extensive researches now being made on oysters.

Ingle mentioned that "later workers have adopted the measurement of glycogen content as a supplementary method of evaluation," meaning supplementary to the "index" method, as explained herein, which was developed by the writer and published in brief form in 1938 (in Higgins, E. *Rep. Commis. of Fish,* 1937). The glycogen method is the traditional one and has been employed by various investigators—P. H. Mitchell (*Bull. U. S. Bur. Fisheries,* 1917, **35**, 151), P. S. Galtsoff *et al.* (*Bull. U. S. Bur. Fisheries,* 1935, No. 18), and others.