braided Mr. A. G. Baker, publisher of Webster's New International, for the way his dictionary pronounced Joule, he defended himself by saying he had written to one of our leading American physicists, a man whom I deeply respect, and had been assured that the English physicists generally pronounced the name as though it were spelled Jowl. They doubtless do; they also says "figgers." But is not Joule himself the supreme authority as to the pronunciation of his own name?

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SCIENTIFIC BOOKS

The Mechanism of Creative Evolution. By C. C. HURST. New York, The Macmillan Company; Cambridge, England, The University Press. Pp. 365. 1932.

BOOKS on genetics are so numerous that any addition might well seem superfluous. The repetition of the same facts, arguments and diagrams has become rather tiresome, and in pessimistic moments we sometimes wonder whether we are not witnessing the growth of a dogma. If we are thus somewhat inclined to be discouraged or bewildered, Dr. Hurst's book comes like a breath of fresh air. The problems of genetics and evolution are restated in an original and extraordinarily lucid way, with full attention to the latest work of the experimenters, and the last findings of the cytologists. The illustrations are nearly all taken from other works, but they are extremely good and well chosen. The print is large and distinct so that the book is easy to read. No attempt is made to shirk difficult problems, as the book is not written as a text for elementary classes. Hurst himself has been actively engaged in experimental work for over thirty years, covering the whole period of modern Mendelism.

The term creative evolution is adopted from Bergson, but interpreted in a biological rather than philosophical sense. It is pointed out that in biology A + B are not equivalent to the sum of A and B, but rather to X, an unknown or unpredictable quality. This may at first seem contrary to the Mendelian theory, which has enabled us in so many instances to predict the results of matings. But just as in chemistry sodium and chlorine unite to produce a substance differing from both in properties and appearance, so also the interactions of living units are continually giving results which could not have been foreseen in advance. A mind contemplating the nonliving universe could not be expected to predict life, the study of the lowest organisms would not lead one to expect the higher complex types, and least of all could the conscious and reflective mind of man have been surmised from a study of the beginnings of life.

We are charmed with the idea expressed by Tennyson in his "Flower in the Crannied Wall," and vaguely feel that somehow it contains the germ of all other life, but practically the conception is not valid. It is too much like saying that Tennyson's little poem is implied in the words it contains, which indeed would produce it sooner or later if arranged at random in all ways possible.

From such considerations Hurst develops a biological philosophy in contrast with the gloomy and deterministic prognostications of the physicist. Appealing to the past, with its story of increasing complexity and development, he imagines a future still more remarkable, but necessarily unpredictable by the human mind. Thus, like Wallace, he ends with metaphysical speculations which take us beyond the realms of experimental science, and will attract or repel according to the temperament and traditions of the reader.

All this, however, may be regarded as secondary to the main character and purpose of the book. Most of the chapters are devoted to a recital of the exact scientific facts in a manner wholly satisfactory to the mechanistically minded. There are said to be four great vital processes acting as random variables. These are mutation, transmutation, sex and natural selection. Especially valuable are the chapters dealing with transmutation and sex.

It has not been appreciated until recently how many "accidents" may happen to chromosomes, and how often those chance occurrences may give rise to new types. Closely related to those matters are the facts concerning polyploids, forms having various multiples of the basic number of chromosomes. The account of polyploidy is particularly clear and interesting, and from it we can see how new types, virtually new species, may arise independently at various times and places and yet be exactly alike. The same thing may happen after crossing, the hybrid progeny occasionally giving rise to new stable types which are fertile and remain constant. In other cases, as with cultivated fruit trees, perennial sunflowers, certain cacti, etc., a cross may give rise to new heterozygous strains which are then propagated vegetatively and so remain constant.

It is pointed out that the latest work of the experimenters is extremely hopeful for results of practical value. It has been shown that through the use of x-rays and otherwise the mutation rate can in certain organisms be enormously increased. While most of these mutations are detrimental, and very many lethal, among so many some will be advantageous under certain circumstances.

It is like a lottery, in which the chances for a prize are increased by the number of tickets taken, only in this case the cost of the tickets is very small, having no relation to the possible value of the prize. Add to this the discovery that virtually new species, and it is said even genera, may be produced through hybridization and we have a possibility of future results which we contemplate with amazement.

Even if we consider Hurst much too optimistic, the actual and unquestioned progress of recent years has reached a velocity which will surely take it somewhere. There is no indication whatever that we have reached the end of our rope.

Nevertheless, as Jacques Loeb used to say, the constancy of the characters of living things is amazing. Hurst puts it thus: "By far the greatest mystery in evolution is the continued existence of numerous species and even phyla whose genes have not mutated and whose chromosomes have not transmutated and whose characters remain the same to-day as they were thousands of millions of years ago." Actually, this is overstated. The ancient fossils, which look so much like living species, are not precisely the same. But the undeniable facts are sufficiently striking. The Foraminifera, the oysters, the oaks and the figs, have gone on under the same generic groupings for many millions of years, producing many specific types, but practically all these suggestive rather of a shuffling of the cards than the evolution of new genes. The experimentalist has accordingly proved too much. He has given us a picture of continual change, compared with which the actual stability of organisms in a state of nature contrasts very strongly. But here Hurst is able to introduce the ever-present principle of natural selection, which hews to the line and under most circumstances makes for stability rather than for change. Morgan's many Drosophila mutants rarely occur in the wild, because nature has no use for them.

But once in a very long while a new "star" is accepted by the manager of the play. I do not understand, in the light of all this, why Hurst, in his introduction, gives his blessing to the age and area theory of Willis. In part it is, of course, a truism, but as an actual picture of the distribution of life it is extremely fanciful. Other things being equal, of course the oldest type would be likely to have spread over the most country; but other things never are equal, and the actual distribution of any species is limited by many and complex factors.

There is this much to be said, the extent of distribution may often be taken as a measure of the stability of an organism. Thus the Painted Lady butterfly (*Pyrameis cardui*), which I saw in the midst of Africa, was quite the same insect which I could observe any day in summer at Boulder, Colorado. It must be an extremely constant type, but added to this is the fact that it is well known for its migratory habits. Very surprising are the results recently obtained by some botanists, who have discovered that certain representative species, in Asia and America, have similar chromosomes in spite of their long separation. These plants can be crossed and will produce fertile offspring.

From such facts and others Hurst undertakes to develop a precise criterion of species, to be determined by cytological and breeding evidence. Such "species" are apt to be more extensive than those commonly recognized, often including strikingly diverse plants which remain entirely separate in nature. It is not likely that botanists will follow this doctrine to its logical conclusion, which would, for example, compel us to unite the blue (*Aquilegia caerulea*) and yellow (*A. chrysantha*) columbines under one specific name. Probably we may use the term superspecies for these aggregates of cytologically similar types.

Another very interesting problem concerns the actual unit of life, which Hurst considers to be the gene. The genes multiply, keep their characters, unless they mutate, and are independent units. It is true that they cooperate to produce results in the cell or body, but this does not invalidate their essential integrity, which in very many cases has remained unchanged for ages.

The smallest units of life, concerning which we are just beginning to have some definite knowledge, may be essentially free genes. The diversity of the genes in higher forms of life could only exist where they were bound together in a cooperative system. Thus we may imagine the primitive genes, in the early days of the world, possibly mutating as often as genes do to-day, but the results were usually fatal. Then when they got into the same boat, as it were, and one could steer and another row, they could afford to be more diverse. Finally, in such a creature as man, they can take on all sorts of aspects, producing results which could never have been imagined in the beginning of things. It is more or less so with all of us. What might we not have become, could we have secured that job exactly suited to our potential talents!

After the book was in type, Hurst constructed a genetical formula for the inheritance of general intelligence in man, to which he attaches great importance. It is briefly explained in a long footnote (pp. 234–235). He recognizes ten grades of intelligence, following Woods, and, assuming that these represent different genetic formulae, gets statistical results closely agreeing with the theory. It seems improbable that ten such grades, independent of environmental influences, can be objectively recognized, and the least one can say is that the burden of proof is likely to be a very heavy one. Nevertheless, these matters can be scientifically investigated, and perhaps the expression of unconvincing opinions may lead to fresh light and ultimately to some basis of general agreement.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

SOY-BEAN PASTE AS AN EMULSIFYING AGENT[1]

IN the United States the soy-bean (*Glycine hispida*) has been used chiefly for animal feeding and as a source of industrial products, but in the Orient it has long provided valuable staple foods for man. One property of importance in the preparation of foods and also for certain industrial processes is the stabilizing effect of soy-bean on oil-water emulsions. By substituting a cooked paste of soy-bean for eggs a salad dressing equal in quality to mayonnaise can be made.

Since lecithin occurs in soy-beans[2] we attempted to determine whether it contributed to their emulsifying action. Known additions of lecithin were made to other pastes of rather low original emulsifying power. The pastes were made by cooking corn starch, wheat starch or wheat flour with distilled water. Three samples of lecithin were used. Lecithins I and II were freshly prepared by extraction from egg yolk and soy-bean flour, respectively, while lecithin III was a commercial preparation of unknown age. The test consisted of preparing an emulsion under standardized conditions with each paste and noting the volume of oil which had been added when the emulsion began to "break." The experiment was then repeated with a duplicate sample of paste, into which lecithin had been dispersed before adding the oil. In a few cases the lecithin was dissolved in the oil itself. The amounts used, expressed as percentages of the weight of paste, were as follows: Lecithin I, 0.14 per cent. and 0.35 per cent.; lecithin II, 0.35 per cent.; lecithin III, 1.25 per cent., 2.5 per cent. and 5.0 per cent. In no case was there evidence that the lecithin greatly increased the emulsifying power of the paste; therefore the proteins of the soy-bean appear to be the chief stabilizing factor.

In preparing the mayonnaise-like salad dressing a paste was first made from sifted, finely ground flour, freshly milled from entire soy-beans of the Mammoth Yellow variety. The flour was mixed smoothly with five parts by weight of distilled water, boiled with stirring for two minutes over direct heat, cooked for fifteen minutes in a covered double boiler and cooled.

[1] From the Laboratory of Home Economics, University of California at Los Angeles.

[2] Hugh MacLean, "Lecithin and Allied Substances," "The Lipins," new ed., Longmans, Green and Co., 1927.

Weighed portions of this paste were beaten in the usual way with a hand or electric rotary beater, while a moderate stream of oil was added. Water was added as needed for thinning, and lastly the requisite amounts of acid, dry seasonings and coloring were incorporated. Two parts of paste to twelve or fourteen of oil and three of additional liquid will yield a product of satisfactory flavor and texture, but this does not represent the maximum capacity for oil.

Soy-bean paste as emulsifying agent in salad dressing has several merits. Among these are: (1) low cost, (2) ease of shipping and storing the beans, (3) heat sterilization of paste immediately before use, (4) the incorporation of rather a large volume of liquid for a given viscosity. Emulsions made with soy-bean appear to be less sensitive to low temperature storage than those stabilized by egg, but to be more sensitive to excessive amounts of seasonings, particularly salt. Further work is required on both of these points, however, as our observations were not conclusive.

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CELLOPHANE FOR LANTERN SLIDES

REFERRING to the article, "A New Use for Cellophane," in the December 16 number of SCIENCE, page 573, I would like to add one suggestion regarding the making of charts and tables on cellophane for lantern slides. The carbon paper should be cut twice the width of the lantern slide and folded so that the cellophane can be placed between the two carbon surfaces. With hard finish typewriter carbon paper the results are much more satisfactory with the carbon deposit on both sides of the cellophane.

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THE RADIO-MAT

ALL who are interested in the convenient device described by Dr. Warren in SCIENCE, December 16, 1932, p. 573, may be concerned to know that a device of this kind ready prepared for making slides is marketed under the name of RadiO-Mat, manufactured by the RadiO-Mat Slide Company of New York, and is obtainable from photographic dealers generally. In this laboratory we have made a large number of slides