counties in Kentucky by Professor W. S. Webb and W. D. Funkhouser, of the University of Kentucky, who have spent the last twenty years in excavations of this character. Of 1,255 sites 667 represent earth mounds, 21 shell mounds, 39 earthworks and fortifications, 162 camp and village sites, 170 cemeteries, 108 rock shelters, 57 inhabited caves and 33 such things as springs and licks, workshops, quarry sites, caches, pictographs and fish-traps.

The Rockefeller Foundation and Vanderbilt University are cooperating in a survey on hookworm disease in several counties in Mississippi in a special program promoted by the state board of health. A preliminary survey was conducted in Waynesboro and Wayne County.

The governor of Illinois recently appointed a commission of representatives of the State Department of Health to study the current outbreak of encephalitis in St. Louis. The four physicians are Drs. Hubert S. Houston, Springfield; Sandor Horwitz, Peoria; Henry Reis, Belleville, and William F. Grayson, Granite City.

According to the Canadian Press, Montreal University will close its doors on December 1 unless the Provincial Government makes necessary provisions for an annual $200,000 grant. A delegation which visited Premier L. A. Taschereau stated that the university was in grave difficulties in the financing of current expenses. This year's anticipated deficit is to be about $193,000. Back salaries are due professors for several months. The Prime Minister promised to place the request before the Cabinet Council. He pointed out, however, that an additional subsidy could only be granted at the next session of the legislature.

An Associated Press dispatch reports that an offer of a complete biological laboratory has been made to Montreal University by Copley Amory, of Boston, who established the laboratory several years ago. Whether university officials will accept the offer will depend on the report of Dr. P. Prefontaine, professor of the faculty of sciences, who is visiting the laboratory, which is at Matamek, Quebec, 300 miles east of Quebec City, on the north shores of the St. Lawrence. Conditions at the university do not warrant immediate acceptance of the laboratory, but an agreement may be reached by which it may be accepted when better times return.

Museum News reports that the New England Conference of the American Association of Museums will be held at Worcester, Massachusetts, October 20 and 21, with headquarters at the Bancroft Hotel. The first day's sessions will be held at the Worcester Art Museum, Lawrence Vail Coleman presiding, and at the Worcester Historical Society. In the evening there will be a subscription dinner, with an address by Paul J. Sachs, president of the association. On the second day the sessions will be at the John Woodman Higgins Armory, for discussion of art and industry, and at the Natural Science Museum. The late afternoon will be devoted to inspection of the library of the American Antiquarian Society. Delegates will be entertained at luncheon by the Worcester Art Museum and by John W. Higgins and at tea by the Worcester Historical Society. The committee in charge of the meeting consists of U. Waldo Cutler, executive director of the Worcester Historical Society, *chairman;* Francis Henry Taylor, director of the Worcester Art Museum, *secretary;* Harry C. Parker, director of the Worcester Natural History Society, and John W. Higgins, president of the John Woodman Higgins Armory.

The National Association of Audubon Societies has announced its acceptance of a suggestion made by John J. O'Rourke, Richmond Park commissioner, that about fifty acres of woods, fields and marshes in the southeastern part of New Springville Park, Staten Island, be set aside as the Staten Island Bird Sanctuary and managed by the association.

DISCUSSION

ARE GENES THE PRODUCT OF CROSSING-OVER

Although we are quite ignorant of the precise nature of genes, there is a pretty general consensus of opinion among geneticists that genes are very small, discrete bodies having a definite serial arrangement within or at least along the chromosomes and having the property, like so many little organisms, of producing other genes similar to themselves. It is now possible to make rough estimates of the dimensions of these minute bodies. We can not state with assurance that a gene consists of only a single molecule, although, according to some estimates, genes are not far from the size of some of the large protein molecules. We have commonly regarded genes as dividing by fission much as this process occurs in a whole chromosome, a plastid or any other small unit of living structure, and we have not troubled ourselves with the chemical changes which may be involved in this process. The multiplication of genes is thus put in the same category of biological phenomena as multiplication by fission in general. If we regard the gene as a multi-molecular structure, we may let the matter rest, at least for the present, with this disposition of the case. If, however, we regard the gene as a single molecule, we are brought face to face with the problem of how a molecule may be conceived to reproduce itself. If we assume that the molecule divides by fission, each moiety will be different from the other, and each must be imagined somehow to regenerate, or form anew, the missing part. Here we encounter formidable difficulties. How the two parts of a complexly organized molecule can each reconstitute the missing part is not easy to imagine. We may escape this difficulty by supposing that genes do not really divide, but that they multiply by building copies of themselves out of the surrounding medium. If we suppose our molecule to have a configuration in three dimensions, we still have trouble in imagining how it can build up a similar molecule by its side Haldane has presented what, so far as I am aware, is the only intelligible picture of how a complex molecule can generate another, by supposing that it is organized as a sort of flat plate so that each atom or radicle might be supposed to attract to it corresponding atoms or radicles, and thus form a new molecule of a similar composition. A chain arrangement might of course duplicate itself in a similar manner. Thus a gene could produce other genes by a process which might fittingly be described as ectogenesis, although one hesitates to employ this term in a sense different from that in which it is used by the author of "Daedalus."

This view of the structure and propagation of genes naturally leads to the supposition, which has been made a number of times, that free genes constituted the most primitive forms of life, if indeed we are justified in applying the term living to self-perpetuating molecules of this kind. But whatever may have been the origin of genes, the hypothesis that genes represent single molecules receives a certain measure of support from the fact that gene mutations may be induced through occasional hits by electrons arising from exposure to x-rays and that the proportion of such mutations varies roughly with the time and the intensity of the exposure. One might contend, however, that similar results could be produced if genes consist of a relatively few molecules, and that gene mutations represent quantitative changes whose differential effects are caused by their varied rates of activity.

But leaving aside these very uncertain and possibly futile speculations as to the nature of genes, we may suggest an alternative supposition that genes, as we know them, are not primitive organic entities, but the product of a long series of evolutionary changes. We may suppose that in primitive organisms which have developed the essential mechanisms of sexual reproduction, including the conjugation of homologous chromosomes in synapsis, the chromosomes or the parts of them especially concerned in heredity, consisted of the same kind of substance throughout their length. A chromosome would then be more or less analogous to plastids and chondriosomes, which, at times at least, are known to multiply by a process of fission. The stages in the evolution of chromosomes among the primitive Protista are largely an unexplored field of cytology, but the supposition that chromosomes were homogeneous structures like other cell organs before they came to include diverse materials in their different parts is one that naturally commends itself on evolutionary grounds. In different strains of unicellular organisms it might be assumed that chromosomes would come to vary slightly in their chemical composition, as is probably the case with other cell organs. The development of crossing-over from the conjugation of homologous chromosomes is a very simple transition, but it is a step which greatly enhances whatever advantages may have been derived from amphimixis. As a result of one cross-over a chromosome may come to be composed partly of one substance and partly of a slightly different substance. By further cross-overs with other chromosomes of similar diverse composition we may conceive that the number of segments of slightly different composition would be continually increased. After crossing-over had been repeated for some millions of generations these segments would have long since become reduced to the smallest dimensions compatible with their continued perpetuation as individual units. In other words, they would finally become what we now talk about as genes. From this point of view, genes would represent the product of crossing-over, and the great diversity of genes in the individual organism a consequence of amphimixis.

The diversification of genes made possible through sexual reproduction has played an enormously important rôle in the evolution of life. Without sex, evolution has never proceeded beyond the production of relatively very simple organisms. Sexual reproduction not only enhances hereditary variability, but it makes possible the combination in one strain of the favorable variations which originated in a number of separate lines instead of merely one single line. Not only this, but it makes possible the accumulation of new characters arising from the interaction of different genes. Crossing-over affords the advantages of a sort of permanent heterozygosity by making possible the combination in a single chromosome of favorable groupings of different hereditary factors. Hence after the crossing-over mechanism was once evolved, nature would ever tend to diversify the genome.

Genes, as thus interpreted, represent the limiting stage in the diversification of the hereditary material.

Even if genes consist of no more than a single molecule they might still be considered to have originated in the manner indicated, because they would naturally tend to approach a certain minimal size, and this, so far as we know, may be molecular. If genes evolved in the way suggested according to this very simple hypothesis, we have a plausible explanation of why they came to have a serial arrangement in the chromosomes. On any other view this peculiar arrangement of genes would require subsidiary hypotheses for its explanation.

In accordance with this method of origin it might be expected that the process of quantitative reduction had not in all cases reached its final stage. One might suppose, as Goldschmidt has done, that the various multiple allelomorphs, such as those occurring in the locus for scute or for white eye in Drosophila, represent purely quantitative changes in the gene due to the loss (or gain) of one or more molecules. Genes vary greatly in the number of different forms they assume, and the readiness with which they undergo change. There are doubtless numerous genes which have never been observed to mutate, even in Drosophila, while there are a few which appear to mutate with riotous unrestraint. Evidently there are genes and genes. It might be that some genes have been reduced to molecular dimensions, whereas others are still multi-molecular and subject to a considerable degree of quantitative variability, albeit by discrete steps. For some reason the various phenotypic changes resulting from mutations occurring in a given locus are, to a considerable degree, interpretable as due to different rates of gene action. The doctrine of mere quantitative variations in genes has been almost entirely banished from genetics, after a due amount of controversy, and I would not venture to defend it in the form in which it was formerly advocated. If we adopt a quantitative theory of gene changes, it must be in some form which harmonizes with the discreteness of mutations.

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A NEW CORDAITES FROM MISSOURI

THE occurrence of fossilized tree trunks in the Burlington limestone series of the Ozarks is quite rare.

When my attention was called to a fossil tree found last year by a farmer on his land near Springfield, Missouri, I made a preliminary study on March 30, and found a trunk, thirteen feet long and eight to ten inches in diameter, imbedded in a ledge of chert which is part of the Upper Burlington limestone series. The chert bed contains brachiopods, crinoids, bryozoans and other marine invertebrate forms typical of that time. The segment of tree evidently was imbedded after having been displaced from its normal position, and was weathered a great deal before induration of silica took place. All outer cortical tissues, leaves, roots and branches, such as might have been present, were lost before fossilization occurred.

Sections prepared on a petrotome to show transverse, radial and tangential aspects include to date only secondary wood. The stem was found to be considerably compressed, crushing much of the woody tissue, but leaving some areas unaffected. The following features are typical.

There is no evidence of annular rings or differentiation due to seasonal growth, a feature of Paleozoic secondary growth generally. The radiating wood rays are numerous and typically uniseriate, but occasionally they become biseriate. Tracheae are absent, so that the wood is made up entirely of tracheids. These in transverse aspect reveal a greater radial than tangential dimension, are quite square in outline and possess bordered pits on the radial walls only.

In the radial view the rays are seen to vary from one cell to twelve cells high. The border and central cells show no essential distinctive differences. Bordered pits connect the ray laterally with the tracheids. Bordering membranes are apparently limited to the tracheal side of the wall. These pits are ofttimes larger than adjoining tracheal pits. There are no pits joining the cells within the ray itself. The ray cells are smooth-walled and are devoid of the sculpturing commonly present in the Lepidodendrids of that period. The end walls of tracheids are long and tapering, with the end walls being radially disposed in such manner as to present the broad face in the radial view. The sloping end wall appears in the tangential view, where it is seen to bear pits of the same type as occur on the radial walls of the tracheid.

In the tangential view the linear series of bordered pits show on the radial and end walls, cut transversely. The tangential walls are devoid of pits. The occasional biseriate condition of the wood rays are evident in this view.

No distinctly scalariform or reticulate elements have been found, although the elongated bordered pits approach the scalariform marking and show all transitional forms between such condition and rounded borders. In all pits, however round the borders may be, the pit itself is elongated. The tracheids with rounded pit membranes are more or less irregularly distributed among those which bear elongated pits, the latter being much more numerous. The axis of the pits varies from horizontal to approximately vertical.

There is no observable thickening or torus on the membrane opposite the pits. Bars of Sanio, which are present in the Ginkgo and conifer forms, are conspicuously absent.