missioned by Harry Burgess, governor of the Canal Zone, to undertake the restoration of his paintings. The treatment finally used was devised by Albert B. Newman, director of the department of chemical engineering at Cooper Union, with the cooperation of Charles Thom, chief mycologist of the Bureau of Chemistry and Soils of the Department of Agriculture; Alexander Scott, of the British Museum, and other mold experts. Mr. Van Ingen said that the thin film of paraffin covering his pictures would remain unchanged indefinitely, and would prove an absolute protection against the effects of exposure to the tropical climate as long as it existed. The use of paraffin was suggested by the experience of New York City engineers, who made an extensive examination of the Obelisk in Central Park to determine how it could be protected from the city climate. Among the mold experts who assisted Mr. Van Ingen and Professor Newman, in addition to Drs. Thom and Scott, were: Lewis T. Bates, chief of the laboratories of the Health Department of Panama; Bernard O. Dodge, plant pathologist of the New York Botanical Garden, where important research in this field is in progress; Charles F. McCoombs, of the New York Public Library; T. R. Beaufort, professional picture restorer; Leslie Ewart Morris, of the British Cotton Research Association; George Smith, of England, and Hugh L. Robinson, editor of the *Journal of the Textile Institute of Manchester*.

PLANS are now well advanced, under the direction of Sir Richard Allison, chief architect to the Office of Works, London, for the construction of the building which is to house the Museum of Practical Geology. The museum is at present in Jermyn Street, and the new site is in South Kensington, adjoining the Science Museum in Exhibition Road, just beyond the northeast corner of the Natural History Museum, and the proposal is to connect the new structure by a bridge with the Science Museum, from which it will be only a few yards distant. Although the plans for the new building are not yet complete, they are far enough advanced for a beginning to be made very shortly in constructing the foundations. The estimated cost of the new building is £220,000. The general scheme is that it shall be on four floors, with a top-lighted roof and central well. The problem of finance is simplified by the fact that the site in Jermyn Street, which belongs to the Crown, is of great value. The museum was founded in 1852. Its library is essentially a library of the geological surveys and maps of the world, together with the economic geology of Britain, the British Dominions and the world generally. Huxley and Tyndall were among those who at one time lectured in the museum. In 1900, during reconstruction, the lecture hall was thrown into the museum library. It is hoped to resume the lectures in the new building or in the lecture theaters of adjacent museums in South Kensington.

The British Medical Journal reports that the act to incorporate a Royal College of Physicians and Surgeons of Canada received last June the royal assent, and in accordance therewith an inaugural meeting, convened by the general secretary of the Canadian Medical Association, was held at Ottawa on November 20. This meeting was attended by sixty of the leaders of the medical profession in Canada, designated charter fellows, and it marked the culmination of a movement which began in 1920. The act provides that the council may organize the college into medical and surgical divisions, those admitted into them being designated fellows of the Royal College of Physicians of Canada or fellows of the Royal College of Surgeons of Canada (or their equivalents in the French language). At the inaugural meeting, Toronto was chosen as the headquarters of the college; Professor Jonathan C. Meakins, of McGill University, Montreal, was elected president; Professor Duncan Graham, of Toronto, vice-president in charge of the medical division, and Dr. F. N. G. Starr, also of Toronto, vice-president in charge of the surgical division. Dr. T. C. Routley, general secretary of the Canadian Medical Association, was elected registrar-secretary.

DISCUSSION

SOME LIMITATIONS OF THE EXPERIMENTAL METHOD IN BIOLOGY

AT the present time much is being written as to the value of the experimental method of attack in biological problems. There can be little doubt of the great utility of the experimental method of approach, but in itself this method may lead to extremely fallacious conclusions. A notable example, apparently, of this deplorable result is presented by current investigations on the experimental production of new species.

Obviously, in pursuing the study of experimental evolution we must keep our eye closely focused on the conditions in nature because it is under the conditions prevailing in nature that new species appear from time to time. The normal course of events in the case of the coming into existence of members of a species has been contemptuously designated by one of our prominent experimental biologists as "the passing show." If we were to continue the figure of the show, we would on the same basis naturally characterize a great deal of the experimental work on species, at the present time, as a "side show."

One must, indeed, apparently regard the prevailing use of radiations on the chromosomes of plants and animals in this light. It has been shown beyond any question that it is possible to modify the chromosomal structure in both plants and animals by means of radiations of various sorts. It has not been shown that such treatment can produce permanent and normal species. Further, it is far from clear that radium emanations or X-rays can ever have played any important rôle in affecting permanently the chromosomal organization of the germ-cells in either plants or animals. Consequently, from the practical standpoint of the experimental evolutionist, actinic experimentation is not likely to lead to any result of lasting importance. In making this statement it will naturally be admitted that such experimental work is of great interest and, from the standpoint of the pathologist and the surgeon, may be ultimately of great importance. From the standpoint of the investigator of the origin of species, it is ruled out of court at the outset by reason of the non-viability of its product.

We are becoming more and more aware, particularly on the plant side, that species cross in nature to a hitherto unexpected extent, and that when two species are crossed almost anything may happen, from the production of an almost infinitely variable progeny to the origin at once of a new, fixed species. It is apparent as a result of correlated cytological and experimental work that many of our large genera of plants are composed of species which are more or less contaminated by hybridism. This, for example, is notably the case with the roses of Europe, the hawthorns of America and the blackberries of both continents. Nor is the northern hemisphere alone in this hybrid multiplication of species. The hundreds of species of Acacias, or "wattles," in Australia afford a similar case of multiplication and variability of species due to hybridization. The same is true of the Australasian genus Eucalyptus of which there are so many and so variable species that their study has given rise to specialists in Eucalyptology.

In his "Origin of Species," Darwin, with that sanity of judgment which makes him so outstanding a figure in the biological sciences, pointed out that the most important element in the origin of species was the innate tendency of species to vary. In his time no explanation was forthcoming of this tendency to variability, but Darwin, with characteristic keenness of insight, pointed out that the greatest variability was found in the species of large genera. This statement, in view of the investigations of the last two decades, was nothing less than prophetic. As a consequence of our present correlation of experimental and morphological study of natural and experimental hybrids, we have come to realize that many species of plants in nature are the result of previous hybridization. There can be little doubt that the same condition prevails on the zoological side and under the same conditions. It is becoming, for example, apparent that many organisms representing large genera of animals are extremely variable and in many instances present the cytological characteristics of hybrids.

One of the most fruitful fields of investigation in connection with Darwin's great work, "Origin of Species," was the comparison between domesticated animals and cultivated plants and plants and animals in nature. In our time, hybridism plays an enormous rôle among the plants of our gardens. Many of the most desirable of these are known to be of hybrid origin. Further, many of our useful plants whose origin is buried in antiquity, by their variability and cytological peculiarities, seem clearly to represent old-time hybrids. This is notably the case, for example, in such common plants as the potato, Indian corn, etc.

Not only are many cultivated plants of known or suspected hybrid origin, but more and more examples of hybridism are being recognized in nature. For example, Trelease has called attention to the fact that there are practically as many hybrids of our American oaks as there are recognized species. A similar situation has been emphasized by Kerner in the case of European willows. Brainerd has described a large number of hybrid violets, and has resynthesized them by crossing again their supposed parents. Almost numberless examples have been supplied in recent years of natural hybrids of plants in all parts of the world and under all conditions. In animals the situation is as yet not so clear, but hybridism is the most natural explanation of the great variability and numerous species of air-breathing snails, of Orthoptera, Lepidoptera, etc., etc.

A particular case of variability is presented by so-called mutations. Plants and animals presenting this sort of variation which leads to the immediate formation of relatively fixed types (elementary species), present striking features of resemblance to known hybrids, particularly in the meiotic or reduction division. Further, in Leptinotarsa (the potato-beetle), among animals, and in Rubus and in the Brassica-Raphanus cross among plants, known hybridization has been followed by clear mutation.

The unwillingness of certain geneticists to admit the hybrid origin of mutating types is one of the outstanding features of present activity in evolutionary investigation. It is probable that this unwillingness arises out of the fear that the Mendelian hypothesis or the chromosomal hypothesis of heredity would be more or less compromised by such an admission. Certainly hybridism furnishes a satisfactory and sufficient explanation of mutation.

As regards the experimental production of species at the present time, we must choose between the practicality of the experimental production of new species by the action of radiations and by that of crossing. It seems scarcely possible to question the choice which should be made. A very weak point in connection with the production of species by radiations is the fact that there is no good reason to suppose that radiations of sufficient power have been active in this manner to any important extent in nature. Secondly, if such radiations were active, there is very grave doubt, on the basis of existing experimental work, whether they could produce viable species. If we take the other alternative, that of hybridization, we find an overwhelming amount of evidence for its occurrence in nature in all parts of the world and under all climatic conditions. Common sense would seem to indicate that the study of hybrids is much more likely to lead to permanently valuable results in experimental evolution than that of the effect of radiations.

The former generation attributed marvelous powers to electricity, which was then coming into the foreground in practical relation to human affairs. Almost every household attempted to cure itself of the various ills of humanity by the use of electrical appliances, from batteries to electrical belts. At the present time radiotherapy is as prevalent and as popular in the public mind as electrotherapy was a generation or two ago. An outstanding feature of our time is ray-mindedness. The popular exponents of biology have merely found this ray-mindedness an easy line of exploitation. There is no reason to believe, however, that radiations will, in the long run, occupy a higher position in biology, or even the popular understanding of that subject, than does electricity. We must apparently come back, as a consequence, to the sound Darwinian conclusion that environment may select variations but it can not in any important respect give rise to them. This dictum is apparently quite as true for special chemical and physical conditions as it is for the general non-living environment.

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ADSORPTION AND EMULSION FORMATION

IF oil and water be shaken vigorously together in a bottle or test-tube, an emulsion is formed which soon separates completely on standing. But if oil and mud be shaken together, the emulsion formed is a thick paste that not only does not disintegrate on standing but resists heating and chemical reagents to a remarkable degree. The oil used may be crude or refined, heavy oil, gasoline or even ether. The mud used may likewise be almost anything: sand, clay, road dirt, metal powder or fibrous dust mixed with water. The mud may be acid or alkaline. Gasoline and clay mud make a convenient pair to experiment with.

We get a clearer picture of what the soil, water and solid particles are doing if we take merely muddy water and shake it up with gasoline. The oil and water now separate clear quite quickly, leaving the solid particles in the form of a thin skin at the interface and on the glass wall. It is quite startling to see a clay suspension, that would not settle clear in months, changed to clear water in a few minutes by shaking with gasoline. The skin formed by the fine particles is quite tough and may be lifted out on a wire like a piece of wet rag. On drying it returns to a powder, indicating that no permanent chemical change has taken place. The solid particles can not spread (like a liquid) at an interface, and being themselves insoluble, can only adsorb the two liquids and mesh together.

Reinders[1] in 1913 made many observations with powdered metals and insoluble salts in water shaken with ether, kerosene, the alcohols, etc., and stated the theory very clearly—the interfacial tension between the two liquids must exceed the sum of the other two. Hofmann[2] about the same time made an extended series of similar observations. Wheeler P. Davey[3] in 1926 read a paper before the Fourth Colloid Symposium giving the theory of the formation of cup grease, which is a carefully prepared emulsion of water, soap and heavy oil. Davey's idea is that the long soap molecules (sodium stearate) in such an emulsion all have their small OH ends turned inward toward the water droplets and their larger CH_3 ends turned outward toward the oil. The meshing together of these bristling units (like hair brushes or chestnut burs) makes a stiff paste of the cup grease. Heating to 250° C. completes the dispersion and removes excess moisture.

The theory of adsorbed polar molecules is hardly sufficient to account for the formation of a tough layer of solid particles at an interface between two liquids. Each particle may be an aggregate of tens of thousands of molecules each but weakly polar in itself. Very clean dry sand easily floats on water, the grains gathering in patches. Here the lighter upper fluid (air) is adsorbed on all sides of the sand grains, causing them to float, and the air layers on adjacent grains tend to coalesce, bonding the grains together. However, in the clay-water-gasoline emulsion, the fine particles of silicates constituting the clay are them-

1 Reinders, *Kolloid Zeitschrift*, 13: 235, 1913.
2 Hofmann, *Zeit. Physik. Chemie*, 83: 385, 1913.
3 Davey, Fourth Colloid Symposium Monograph, p. 38.