DR. FREDERICK C. LEONARD, chairman of the department of astronomy of the University of California at Los Angeles and president of the Society for Research on Meteorites, gave on April 7 an illustrated lecture entitled "Visitors from Cosmic Space," before the University Chapter of the Society of the Sigma Xi.

THE Linacre Lecture of the University of Cambridge was delivered by Dr. A. V. Hill, Foulerton research professor of the Royal Society on May 10. His subject was "The Heat-Production of Muscle and Nerve: A Critical Survey."

IN the account of the presentation of the medals of the National Academy of Sciences printed in the issue of SCIENCE for May 7, the address on presenting the Henry Draper Medal to Dr. C. E. Kenneth Mees is in error attributed to Dr. Frank Schlesinger. The address was read by Dr. Schlesinger, but should have been signed by Dr. V. M. Slipher, director of the Lowell Observatory, who is chairman of the Draper committee.

THE annual meeting of the Royal Institution of Great Britain was held on May 1. The London *Times* states that it was reported at the meeting that the total membership of the institution, which has been increasing for several years, is now 1,055. The library reconstruction was completed in October last, at a cost, as the accounts showed, of upwards of £15,000. The amount the institution has received by the bequest of the late Mr. Harry Brown is now announced as £29,000. The account of the Davy Faraday Research Laboratory, which is attached to the institution, showed a deficit for the year of nearly £2,000; but the chairman was able to announce a promise by Sir Robert Mond, honorary secretary of the laboratory, to meet this deficit. The following were elected for the year 1937–38: *President,* Lord Eustace Percy; *Treasurer,* Sir Robert Robertson; *Secretary,* Major Charles E. S. Phillips.

THE College of Physicians of Philadelphia held on May 14 and 15 a celebration of the hundred and fiftieth anniversary of its founding in 1787, the year of the Constitutional Convention. The speakers were Roland S. Morris, president of the American Philosophical Society; Dr. David Riesman, professor of the history of medicine in the Graduate School of Arts and Sciences of the University of Pennsylvania; Sir Henry Dale, director of the British National Institute for Medical Research, and Dr. Hans Zinsser, professor of bacteriology in the Harvard Medical School.

AN international colloquium on the physiology of the sex hormones, under the presidency of Professor Pol Bouin, of the faculty of medicine of the University of Strasbourg, will be held in Paris this June under the auspices of the Singer-Dolignac Foundation. Twenty investigators particularly qualified by their work in the subject have been invited to attend, all expenses being defrayed by the foundation. Those in the United States who have received invitations are Dr. Frederick L. Hisaw, professor of zoology at Harvard University; Dr. Edgar Allen, professor of anatomy at Yale University; Dr. Philip Smith and Dr. Aura E. Severinghaus, of the College of Physicians and Surgeons, Columbia University, and Dr. Carl G. Hartman, of the department of embryology of the Carnegie Institution of Washington at Baltimore.

CONSTRUCTION work on the extension of the School of Medicine of Columbia University was begun on April 1. The plans, which will necessitate an expenditure of $600,000, provide for the addition of ten stories to the present six-story extension of the west wing of the building at the Columbia Medical Center. The additional space will be used mainly to house the research laboratories of the five graduate departments—anatomy, pathology, biochemistry, physiology and bacteriology—which now are confined to the lower floors of the wing.

DISCUSSION

THE NEEDS OF THE MIMICRY THEORY

IT seems inevitable that any adverse criticism of the theory of mimicry should bring forth more examples and arguments in its support, of the kinds which have long been on record. Professor Carpenter[1] has therefore done the expected in coming to the defense of the theory after the critical discussions in the author's recent book[2] on evolution.

No attempt will be made to refute his arguments or to show *individually* why the cited examples do not prove mimicry to be the thing it has been claimed to be. The examples could not be proved meaningless without a very much closer study than is possible from the few facts known and recorded. It is likewise true, though not so pleasant a thing to say, that of the examples, new and old, few or none could be successfully advanced to prove the advantages and the origin of mimicry without a much closer study of them than has ever been made. If the same rigid requirements were insisted upon for proof as are currently demanded for disproof, the theory would hardly have gained prominence. We are called upon to accept mimicry until it has been clearly disproved, whereas it would be more logical to reject it until clearly estab-

[1] SCIENCE, 85: 356–359, 1937.

[2] A. F. Shull, "Evolution." New York: McGraw-Hill Book Co. 1936.

lished. It is the author's view that the latter has not happened, though he is quite willing that it should.

Unless proponents and opponents agree on standards of evidence it is not likely that either will satisfy the other. There is no common ground for discussion until the fundamental question, what is involved in judging mimicry, is settled. Examples mean nothing unless it is understood what they must show. A critical biologist will hold that these judgments lie mainly in two fields, genetics and animal behavior—the former relating to the origin of mimicry, the latter to its current value to the mimics.

Into the first of these fields Professor Carpenter would prevent us from entering, on the ground that the problems of mimicry are questions, not of origin, but of survival. If students of mimicry are all prepared to subscribe to that view, a long step toward simplification will have been taken. However, there were certainly some supporters of mimicry in the past who held that mimics came to be what they are because of the advantage it brought to them. The mimicking types were supposed, by these naturalists, to arise out of non-mimicking stocks, gradually through the accumulation of modifications leading to greater and greater similarity to some protected form. This involves survival, but it also includes successive mutation.

It makes a great deal of difference whether similarity to a protected and unrelated animal came about by a single change or by many. If students of mimicry are all agreed that mimicking color, form or habit arose by one mutation in a type possessing no approach to the protecting quality of the model, the argument is simplified and a good deal of history of the evolution idea is now ancient history. If there still be some who maintain that mimicry arose gradually, a distinction will have to be made, and two kinds of arguments advanced to oppose the two kinds of mimicry. If Professor Carpenter is correct in saying that only survival, not origin, is involved in mimicry, which implies the one-mutation origin of the imitations, then the advantage of the new character had nothing to do with its nature. A mutation occurred, often involving (we have been told) many details, and it happened to help its possessor. Since the details have been important in creating the illusion in the minds of predators, the single mutation was a rather remarkable occurrence. Yet its nature was wholly accidental. The marvels of mimicry thus become the wonders of the physiology of development. The latter field is one which the geneticists hope to make their own, but it is important that students of mimicry take the lead in this particular phase of the problem.

Addressing now those who perhaps still believe that mimicry arose gradually, that perfection of the mimic involved many steps, one should point out that survival must be aided by successive mutations which must occur in an order that will build up greater and greater resemblance. If this concept of mimicry be adopted, it becomes of the utmost importance to the theory to know by how many genes a mimic differs from its non-mimetic relatives, how many of these genes have anything to do with appearance, and what phenotypes would result from various combinations of smaller numbers of the differentiating genes. Only with such knowledge can we have any reason to adopt any theory of either origin or survival.

Whichever view of the simplicity or complexity of mimicry be adopted, knowledge of origin belongs in fields which the geneticists claim. If the problems of mimicry are not geneticists' problems, whose are they? They *should* be the problems of all who believe that mimicry as an advantage exists. Students of mimicry are appealed to to furnish the necessary analyses. They know the phenomena in nature; they know, or can learn better than any one else, how to rear the mimics and their relatives; and they have a better opportunity to discover which ones, despite their specific distinctions, still can be crossed. If they can be convinced of the need of these genetic analyses, the outlook for progress is by no means dull.

Let us turn now to animal behavior, the field in which Professor Carpenter holds the entire argument to lie. He implies that it is somehow reprehensible to comment "on the danger of drawing conclusions from experiments on animal behavior," and then cite experiments on the color vision of fowls. Professor Carpenter has missed most of the point to this caution. It is of the greatest importance that there be experiments on behavior. But they must be adequate ones. Without them we go on deciding on very meager bases what an animal "likes" by something that it does, what it "perceives" by some small item of its behavior, what is "suggested" to it by an appearance that suggests something to us, what reaction it gives to a "black and stinking" beetle which impresses us as deserving those adjectives, what it "wants," what it "endeavors" to do. It is very difficult to make the study of animal behavior objective. That is why this branch of biology is one of the least developed of the biological disciplines. Why should we pretend it is easy and continue to form judgments from a few observations? They are almost certain to be wrong in part. We do not even know the entire basis of human behavior. There is not one among us who can say exactly what all elements enter into a judgment. Try as we may to be objective, we all occasionally fail; and when we recognize our failing, we often can not tell what emotion or feeling it was that supplanted reason or a fact. How can we judge other animals' behavior when we do not know

the basis of our own? How likely are we to judge it correctly if, under these circumstances, we insist on judging?

As for experiments on behavior relating particularly to mimicry and warning color, there appears to be only one series that was at all adequate. They were the tests made by Reighard[3] on coral reef and other small fishes and their neighboring predators. Professor Carpenter does not mention these experiments. They turned out to be against the warning color theory, and upset a number of preconceived ideas of how these fishes behave and why they do so. Nor does he mention the stomach contents of birds (McAtee[4]), which are one of the consequences of animal behavior. These go to show that many animals held to be protected, whether through imitation or otherwise, are not very immune to capture after all. It will require a good many casual observations and brief tests to overthrow a thorough, analytical and objective group of experiments and a mass of concrete facts on feeding habits. That is why such tests should cease to be casual and brief. Nothing less complete than the Reighard experiments will suffice, and students of mimicry are urged to make their tests of behavior as full and inquisitorial. The only alternative is to refrain from drawing conclusions.

"Few critics seem to be aware of the great extent of the phenomenon." Were its extent twice as great, its problems would not be solved. What mimicry needs is not a broader foundation, but a deeper one. Those who have made known the large number of instances of it are in the best position to furnish this depth. It is to be hoped they will direct their chief energies to that end.

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HETEROTHALLISM IN VENTURIA INAEQUALIS

IN October, 1935, conidia from cultures obtained by isolating each of the eight spores of an ascus of *Venturia inaequalis* were used to inoculate the leaves of potted Fameuse apple trees that had been held in cold storage and recently forced out in the greenhouse. Conidia from each isolate were used alone and mixed in every possible two-isolate combination. Infection resulted from every inoculation, whereas uninoculated trees remained free from it. Leaves from the experimental trees were overwintered and examined microscopically for perithecia of *V. inaequalis*. None of the uninoculated leaves or those inoculated with conidia from any single isolate of the fungus bore perithecia. The results from the two-isolate inoculations showed that the eight isolates fell into two groups of four each. All the 16 possible combinations between these two groups yielded perithecia that bore ascospores. None of the 8 combinations in which conidia from an isolate were mixed with those from another within the same group yielded perithecia that bore ascospores, except in three cases, in each of which the fertile ascocarps were borne in a strictly localized area. These seemingly aberrant cases are thought to have been due to contamination. Cleared-leaf studies showed that perithecial initials were formed abundantly when single isolates or non-fertile mixtures were used, but they usually attained less than one half the diameter of the normal, mature perithecium. The experiment is being repeated with modifications and supplemented by pure-culture studies. The available evidence seems to justify the conclusion that *V. inaequalis* is heterothallic, each isolate being hermaphroditic and self-sterile.

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AN ANALOGUE OF PLATEAU'S SPHERULE

IF a falling stream of water is examined with a stroboscope at the point where it breaks up into drops, a tiny droplet may be seen formed apparently from the "tails" between successive drops. This droplet is generally known as Plateau's spherule, after the inventor of the stroboscope.

While emptying two flasks with special constricted necks, I was very much interested to observe a similar phenomenon which may be seen quite easily without the aid of stroboscopic vision. The necks of the flasks were so narrow (4.0 mm i.d.) that air entered in discrete bubbles. Between successive bubbles, tiny bubblets of air were formed which could be observed rising slowly through the solution.

The phenomenon was first observed while emptying a saturated solution of barium nitrate in 33 per cent. nitric acid from the flasks. When the flasks were filled with pure water for calibration, no spherules of air were seen on emptying. Apparently, the formation of the tiny bubbles from the thread of air, left as a large bubble breaks off, depends upon a suitable relationship between surface tension, mobility and density of the liquid, for a given type of neck. With concentrated 70 per cent. nitric acid the effect is not as good as with 40 per cent. acid although occasional bubblets may be seen. Fifteen per cent. alcohol is as good as the 40 per cent. nitric acid, and the effect may also be seen quite well with 95 per cent. alcohol. With ethyl ether the formation of the spehrules was also observed. In this liquid there were frequently three

[3] J. E. Reighard, Carnegie Inst. Wash. Pub. 103: 257–325, 1908.

[4] W. L. McAtee, Smiths. Inst. Misc. Coll. 85(7), 1932. Also *Quart. Rev. Biol.*, 8: 209–213, 1933.