L. E. DICKSON: 'On the quaternary linear homogeneous groups modulo p of order a multiple of p.'

L. E. DICKSON: 'On finite algebras.'

VIRGIL SNYDER: 'On a type of rational twisted curves.'

E. J. TOWNSEND: 'Arzelà's condition for the continuity of a function defined by a series of continuous functions.'

H. S. WHITE: 'Rational plane curves as related to Riemann transformations.'

F. R. MOULTON: 'A class of periodic solutions of the problem of three bodies.'

C. N. HASKINS: 'Note on the differential invariants of a surface and of space.'

E. V. HUNTINGTON: 'The continuum as a type or order: an exposition of the modern theory.'

The next meeting of the society will be held at Columbia University, on Saturday, October 29. The San Francisco section meets at the University of California, on September 30. The annual meeting of the society for the election of officers will be held on Thursday and Friday, December 28–29.

F. N. COLE,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE PROBABLE ORIGIN OF CERTAIN BIRDS.

IN a recent article in SCIENCE,[1] Mr. W. E. D. Scott attempts to apply the 'mutation' theory of de Vries to the origin of certain puzzling forms of North American birds, his conclusion being:

> In the light of the evidence set forth [in the preceding pages of his article] only one answer can be made to the question as to the part the process defined by de Vries as 'mutation' is playing among higher animals to-day. Beyond doubt we have witnessed the birth of new species of birds during the past seventy years. Moreover, some of these new species have flourished so as to have become a salient part of the bird fauna in the region where they occur and where they were unknown to skilled ornithologists, who carefully studied these regions in the early part of the last century.

The birds here considered by Mr. Scott are nine in number, all from the 'Hypothetical List' of the American Ornithologists' Union Check-List of North American Birds, and, in the order of discovery, are as follows: Small-headed warbler (*Muscicapa minuta* Wilson, 1812), Blue Mountain warbler (*Sylvia montana* Wilson, 1812), carbonated warbler (*Sylvia carbonata* Audubon, 1831), Cuvier's kinglet (*Regulus cuvierii* Audubon, 1832), Townsend's bunting (*Emberiza townsendii* Audubon, 1834), Cooper's sandpiper (*Tringa cooperi* Baird, 1858), Brewster's linnet (*Acanthis brewsterii* Ridgway, 1872), Lawrence's warbler (*Helminthophaga lawrencei* Herrick, 1874), Brewster's warbler (*Helminthophaga leucobronchialis* Brewster, 1876). The first four of these birds are known only from the descriptions and figures given of them by Wilson and Audubon; of each of the next three, the original and still unique type specimen is preserved. The remaining two, both forms of *Helminthophila,* are known from numerous examples, they being of more or less frequent occurrence (if we reckon the variants of each) over a limited area in southern New England (mainly the lower Connecticut Valley), the lower Hudson Valley and northern New Jersey.

Mr. Scott comments on the first seven very briefly, but states, in concluding the enumeration, that he is compelled 'to consider these forms as mutations (which were not perpetuated) from species still existing.' About seven pages are then devoted to the remaining two forms, *Helminthophila leucobronchialis* and *H. lawrencei,* in which he gives a partial list of the known captures of each, mostly in footnotes in small type, with more or less extended extracts from the records relating to them, and often a summary of the opinions that have been expressed regarding the status and relationships of the two forms. The number of specimens of *H. leucobronchialis* at present extant is estimated to be 'at least 150,' and of *H. lawrencei* 'between 20 and 25.'

These two forms are discussed separately, at some length. Under *H. leucobronchialis* (*l. c.,* p. 278), he expresses his conclusions respecting them as follows:

[1] 'On the Probable Origin of Certain Birds,' by William E. D. Scott, SCIENCE, N. S., Vol. XXII., No. 557, Sept. 1, 1905, pp. 271–282.

In view of the foregoing facts, I am of the opinion that in *H. lecubronchialis* and in *H. lawrencei*, . . . we have examples of two separate and distinct 'mutations' from a common parent stock or species. That is, I believe that *H. pinus*, early in the last century became unstable as a species and began to throw off what must be considered as 'mutants,' taking de Vries's definition of the word. In other words, *H. pinus* is alone responsible and is the direct ancestor of both *H. leucobronchialis* and *H. lawrencei;* that these 'mutants' have up to the present time generally bred back into the parent stock, and that in so doing the instability of *H. pinus* has increased geometrically with the constant result of the increasing number of both kinds of 'mutants.'

While the 'mutation' theory may be a good hypothesis to consider in respect to these peculiarly unstable groups of birds, it must be noted that the method of their origin and the results, as now known, are very unlike the methods and results of mutation in plants, as made known by de Vries. The facts and conditions are not to any great extent parallel. Instead of the resulting 'mutants' remaining constant and breeding true, as in the case of primroses, they are in this case unstable and are believed[2] to interbreed freely with each other and the parent stock. Besides, in building up his theory of 'mutants' in the case of these warblers, we think Mr. Scott has belittled the evidence of hybridity and laid too much stress upon the (assumed) completeness of knowledge 'in the early part of the last century' of the ornithology of the area now inhabited by these birds. While it is true that most of these puzzling birds have been taken within the last twenty or twenty-five years, it does not follow that, as Mr. Scott says:

> It is not likely that a form or kind of bird so common as *H. leucobronchialis* is at the present time, and ranging over as large an area as from Pennsylvania to Massachusetts and from Virginia to Michigan, should remain unknown to the earlier ornithologists, such keen field naturalists as Audubon and Wilson, Baird, Lawrence, Coues and Prentiss. Nuttall made careful and prolonged study of birds in the region where Mr. Brewster collected the type. Yet none of these close observers and good collectors either recorded or secured an individual of this kind. Clearly then, the presumption is that this bird could not have been so common early in the last century as it is now, if indeed it existed at all at that time.

[2] By those who are most familiar with the facts. Interbreeding is known to occur between the two stock species, and also between their offspring and both of the stock species, and it has been repeatedly assumed by the best authorities that the hybrids are fertile *inter se*. This feature of the case is of course impossible of demonstration, owing to the nature of the conditions—the impossibility of continued observation of the same individuals for a series of years.

First as to the range of these two forms, with reference to that given in the above quotation. *H. lawrencei* has been found only in the northern part of New Jersey, the lower Hudson Valley, and the lower Connecticut Valley. *H. leucobronchialis* has but five records (all of migrants) south of northern New Jersey, two of which are for southeastern Pennsylvania, two for the immediate vicinity of Washington, where collectors abound, and the other (not mentioned by Scott) for Louisiana. The bird has been reported as observed in northern Ohio, but the only record of a captured specimen for the region west of New Jersey and eastern New York is a single bird taken in southern Michigan. There are also only two records for the region north of Connecticut, which include the original type specimen (Newtonville, Mass., 1870) and one other (Hudson, Mass., 1858). Thus the known distribution of these forms, at least for the breeding season, is narrowed down to practically northern New Jersey, the southeast corner of New York (extreme lower Hudson Valley) and Connecticut. This is quite different from the distribution conditions that might be implied from the sweeping statement above quoted from Mr. Scott.

Now as to the work of the earlier naturalists. Both Wilson and Audubon explored the region around Philadelphia, where, notwithstanding all the careful field work of many expert collectors during recent years, there are only two records for *leucobronchialis* and none for *lawrencei*. These naturalists also each made journeys to New England, but their visits were brief and for the most part with other interests than field work, and it is well-

known that neither was accustomed to preserve, or even to collect, many specimens. Lawrence lived in New York City, and doubtless made frequent excursions into the adjoining country, but business exactions permitted little real field work comparable to that of present-day collectors and observers. In fact, his collection shows that he collected very few birds himself, but acquired them by purchase. Nuttall was a botanist and, although intensely interested in birds, if he ever collected many birds in the vicinity of his Cambridge home, the fact remains unrecorded. That his field work there in ornithology is to be compared with that of any one of the many enthusiastic collectors that have gleaned the region year after year for the last three decades is not to be even suggested. In any case, he worked, as already shown, practically outside of the range of these forms, since only two specimens have yet been obtained from eastern Massachusetts. Coues and Prentiss did their field work hundreds of miles distant from the principal range of these forms, and their collecting was casual and intermittent, in comparison with that of the numerous recent collectors in the Washington vicinage. Baird's field work, restricted to his early days, was also outside of the region here in question. Finally, the rapid increase in the number of these curious birds taken or observed during the last ten or fifteen years certainly has not more than kept pace with the greatly increased number of collectors of an expert class unknown 'in the early part of the last century.' There are now, within the area favored by these interesting birds, hundreds of private collections, each numbering more specimens of birds, nests and eggs, than all that had been collected in New England, New York and New Jersey prior to the middle of the last century. While there are now hundreds of persistent collectors within this prescribed area, one could probably count on the fingers of the two hands all those who have taken or observed in life any representatives of the two birds here in question. If Mr. Scott, who has done an exceptionally large amount of collecting in New Jersey and New England, has ever taken a specimen of either of these forms he seems to have neglected to record the fact of such an interesting capture. Evidently, then, the facts in the case fail to support the supposed rapid increase in the numbers of the birds in question alleged by our author to be so evident.

The ornithologists who are most familiar with these birds, through the examination of specimens and in life, have proposed or supported the theory of hybridity between *H. chrysoptera* and *H. pinus* as accounting in a fairly satisfactory manner for the birds, with their endless variants, known as *H. leucobronchialis* and *H. lawrencei*. But this does not seem to satisfy Mr. Scott, who says: "Nor does it seem that the theory of hybridity is supported when we consider the vast number of known specimens already in collections and the fact that it is possible to observe living specimens . . . each year." He further says: ". . . for, though hybrids do occur among wild birds, they can be considered at best as only casual, and the infertility of hybrids, especially among the higher animals, is too well known to need further comment here"! The case of *Colaptes cafer* and *C. auratus* must have, at this moment, escaped Mr. Scott's recollection, between which two species, for a thousand miles, north and south, along the line where their ranges meet, hybrids of all degrees, with every possible combination of the characters of these two strikingly different looking species are found almost to the exclusion of birds of pure blood of either species. The area of hybridity in this case occupies a belt hundreds of miles in width, the prevalence of birds presenting more or less traces of mixed blood gradually fading out both to the eastward and to the westward.

Mr. Scott makes only passing allusion to Dr. Bishop's important paper on this subject in a recent number of *The Auk* (XXII., January, 1905, pp. 21–24), and none to his conclusions, which are that *H. leucobronchialis* 'is merely a leuchroic phase of *H. pinus*, which, from its appearing frequently only within a very limited area, may in time become a species; and that *H. lawrencei* is a hybrid between *H. chrysoptera* and *H. pinus*.'

Near the end of Mr. Scott's paper, he quotes at considerable length from a paper recently

published in *The Ibis* (1903, pp. 11–18, pl. I.), by Professor H. H. Giglioli, entitled 'The Strange Case of *Athene chiaradiæ*,' a curious variant of *A. noctua*, having black instead of yellow irides, and some variations in the markings of the plumage from the normal form. The facts, and the speculations thereon by Professor Giglioli, are of much interest, and Mr. Scott thinks they help to confirm his view of the case of the two forms of *Helminthophila*. But the facts are not at all parallel, the nine specimens of the abnormal owl being traced back to, presumably, a single pair. This case has the essential features of a 'mutant,' as these peculiar owls were not the product of the union of two species, and hence not 'hybrids.' In other words, it is what Giglioli appropriately terms 'a case of *neogenesis*,' which might, should the progeny survive, constitute a new species. A further history of this case will naturally be awaited with great interest.

As already shown, I fail to see any good basis for Mr. Scott's attempt to employ the 'mutation' theory in explanation of the case of *H. lawrencei* and *H. leucobronchialis*, and believe still that these unstable and ever-varying forms are primarily the result of hybridity between *H. chrysoptera* and *H. pinus*, with which belief the known facts in the case are wholly consistent. Dichromatism may play a part, as several previous writers have suggested. The two forms are known to interbreed with each other and also with the parent stock, producing fertile offspring. They thus far, also, have been found (with the exception of a few migrating birds) only in the area where the breeding ranges of *H. chrysoptera* and *H. pinus* overlap. That they have not been found throughout this overlapping area is more than likely due to the absence from it of a sufficient number of expert observers. No section of the country within this range has a tithe of the expert field observers and collectors, proportionately to the area, that have been working for years throughout the limited district which has thus far almost exclusively produced the known examples of these birds. There seems to be no obvious reason why they should not occur sparingly westward over a narrow belt south of the Great Lakes to Wisconsin, where thus far they seem to have been almost wholly overlooked.

In taking up this subject, Mr. Scott appears to have proceeded without a very clear conception of either the essential facts of the warbler case or of the phenomena of 'mutants.' His assumption of the recent rapid increase of these forms rests on statements that are both misleading and irrelevant. The region of their occurrence is wholly outside of the fields of research of the ornithologists he mentions as evidence of the thorough knowledge of the ornithology of this region he assumes to have existed 'in the early part of the last century,' while, as regards numbers and methods, these early workers are not for a moment to be compared with those of the last few decades. Besides, it is only a few experts, who have made these birds a specialty, and know their haunts and notes, who have any success in their discovery. The facts, as already said, of the known relationships and the instability of these forms, harmonize poorly with the phenomena of mutations, shown by de Vries in relation to plants, in which the new forms arise with definite and stable characters, which they can transmit without modification to an apparently endless succession of generations. J. A. Allen.

SPECIAL ARTICLES.

BATTERY RESISTANCE BY MANCE'S METHOD.

Among the many methods for measuring battery resistance, one of the oldest, and apparently least understood, is that known as 'Mance's method.' As usually discussed in text-books this method is described as being a modification of Wheatstone's bridge, in which the cell to be measured takes the place of the unknown arm and the usual battery is replaced by a simple key. When opening or closing this key produces no change in the steady deflection of the galvanometer the bridge is balanced and, 'therefore, the usual relation of Wheatstone's bridge is satisfied.' It is the object of this paper to show wherein many writers have erred in this explanation,