common thing for them to do so here, and they are frequently drowned in making attempts beyond their strength.

Some years ago I was rowing on Lake George in this State, when I observed one of these little animals in an open place, where from the course he was pursuing he must have swum nearly half a mile. He seemed almost exhausted, and when I held my oar towards him he readily accepted the invitation to come on board, ran up the oar, and then to my surprise ran up my arm and ascended to my shoulder! I do not know whether he simply followed his climbing instincts, or whether he sought an elevated point to get an observation. However this may have been, after a short pause he descended and took his station in the bow of the boat, from which in a few minutes he plunged into the lake and struck out for land. He evidently miscalculated his remaining powers, for he was unequal to the effort, and soon gladly availed himself of a second opportunity of gaining a place of refuge. He now sat quietly while I rowed him towards the land, evidently satisfied that he was in friendly hands, and that his wisest plan was to remain as a passenger. When close to the shore he made a flying-leap and scampered for the trees, doubtless grateful in his little heart for the kindness that had helped him over the critical part of his voyage.

This was near the narrows of the lake, where it is about one mile in width, with groups of islands which shorten the traverses to less than a quarter of a mile. My little friend however had not availed himself of the easier and more circuitous route, but had boldly undertaken a directer course and a longer swim, which, but for the timely rescue, would very likely have been his last aquatic attempt. FREDERICK HUBBARD

New York, March 10

IN connection with a recent letter in NATURE on the squirrel taking to water, the following facts may be of interest:—While camping for two summers recently in the wilderness of northern New York, I was much surprised at frequently seeing squirrels crossing the ponds and lakes of the region. We would sometimes find several of these strange navigators in the course of an afternoon's row. They were seen most abundantly during the early part of July; indeed, later in the season, they were but rarely found. During many summers of camping elsewhere I have never seen them take to the water. It has occurred to me that the explanation of this peculiarity (if it be such) of the squirrels of this locality may be found in the nature of the region visited; for we find there a most intricate water-system, the whole region being dotted with ponds and lakes connected by small streams. The necessity of taking to the water at times has perhaps enabled the squirrels to overcome their aversion to this element, and they have thus become semi-aquatic in their habits. The squirrel to which reference is made is the common "red squirrel," *Sciurus Hendsonius*. C.

Worcester, Mass., March 8

IN the autumn of 1878 I was salmon fishing in the River Spey, a few miles from its mouth, where the stream was broad, strong, and deep—when just beyond the end of my line I perceived a squirrel being carried down, but swimming higher out of the water than is usual wi.h most animals. Its death by drowning seemed inevitable, as the opposite bank was a high, perpendicular cliff of Old Red Sandstone, where even a squirrel could hardly land. However it swam gallantly on, heading straight across the stream, and finally, after being swept down a long distance, emerged on the other side, where a burn intersected the rock, and fir-trees grew down to the water's edge. The left bank, where the squirrel must have entered the river, was low and shelving, and it selected a spot, accidentally or otherwise, whence the current carried it opposite to an easy landing-place on the right bank. CECIL DUNCOMBE

March 18

THE LATE MR. E. R. ALSTON

THE death of Edward Richard Alston, which took place at his rooms in Maddox Street on the 7th inst., leaves a vacancy in the thin ranks of the working naturalists of this country that will not be easily filled up. At the time of his death Mr. Alston was secretary to the Linnean Society, a member of the Council of the Zoological Society, and treasurer to the Zoological Club, and up to within a few days of his decease was engaged in active zoological work. Mr. Alston, who died of phthisis at the early age of thirty-five, although somewhat retiring in disposition, was of a particularly kind and amiable nature, always most friendly with those with whom he was brought into contact, and ready to help them by advice or assistance. Mr. Alston was of Scotch parentage, and a native of Ayrshire. Being from infancy of delicate constitution he was educated chiefly under private tuition, and did not go to school or college. Notwithstanding these disadvantages he was a good scholar and a neat and concise writer, and had an excellent acquaintance with comparative anatomy. Taking early to the pursuit of natural history he became a contributor to the *Zoologist* and other popular journals, principally upon mammals and birds. Mr. Alston's first important paper was an account (published in the *Ibis*) of his journey to Archangel, made in 1872, in company with his friend Mr. J. Harvie Brown, in which excellent observations are given on the summer migrants and other feathered inhabitants of that previously little explored district. Shortly afterwards Mr. Alston moved his head-quarters to London during the first part of the year, and undertook the compilation of the portion of the *Zoological Record* relating to mammals, which he carried on in a very painstaking and methodical way for six years (1873-78). A new edition of Bell's British Mammals, which had long been called for, appeared in 1874. Mr. Alston, although he is only credited with having "assisted" in this work, was, we believe, its virtual compiler. From that date also he became a frequent reader of papers at the meetings of the Zoological Society and author of several excellent memoirs in the *Proceedings*. Amongst these we may call special attention to his revision of the genera of Rodentia, published in 1876, as a most successful exposition of the many difficult points connected with the arrangement of this group of mammals, and to his memoirs on the Mammals of Asia Minor, collected by Mr. C. G. Danford (1877 and 1880). Mr. Alston's last and most important work, which he had fortunately just brought to an end before his untimely death, was the "Mammals" of Salvin and Godman's "Biologia Centrali-Americana"—a great work on the fauna and flora of Mexico and Central America. The first part of this was published in 1879, the eighth number containing the completion of the Mammals in December last. The death of this promising naturalist, when in the full tide of work, must be a subject of universal regret among all lovers of science.

RECENT MATHEMATICO-LOGICAL MEMOIRS

THE Boolian reform of logical science is at las beginning to manifest itself and to bear the first-fruits of controversy. Thirty years ago Boole's remarkable memoirs were treated as striking but almost incomprehensible enigmas. Even De Morgan did not know exactly how to regard them, and in his "Syllabus of a Proposed System of Logic" (p. 72) thus allows their mysterious truth:—"In these works the author has made it manifest that the symbolic language of algebra, framed wholly on notions of number and quantity, is adequate, by what is certainly not an accident, to the representation of all the laws of thought." But time and the efforts of several investigators have cleared up much of the mystery in which Boole wrapped his logical discoveries. The controversies now going on touch rather the precise form to be given to the calculus of logic, than the former question of the new logic against the old orthodox Aristotelian doctrine.

The most elaborate recent contributions to mathematico-logical science, at least in the English language, are the memoirs of Prof. C. S. Peirce, the distinguished mathematician, now of the Johns Hopkins University, Baltimore. Not to speak of his discussions of logical questions in the *Proceedings* of the American Academy of Arts and Sciences (vol. vii. pp. 250-298, 402-412, 416-432), we have from him the wonderful investigation contained in his "Description of a Notation for the Logic of Relatives, resulting from an Amplification of the Conceptions of Boole's Calculus of Logic" (*Memoirs* of the American Academy, vol. ix. Cambridge, U.S., 1870, 4to). The contents of this remarkable treatise, which fills sixty-two quarto pages, demand the most careful study, but it would be quite impossible in this article to enter upon such study. Prof. Peirce has however quite recently interpreted his own views in a new memoir "On the Algebra of Logic," of which the first part, completed by the author in April last, was printed in the *American Journal of Mathematics*, vol. iii., and issued in September (4to, 57 pp.). After noticing the beautiful typography in which the *American Journal* rejoices, we find in this memoir a very careful inquiry as to what is really the form and nature of logical inference.

Prof. Peirce treats in succession of the Derivation of Logic, of Syllogism and Dialogism (a new name for a form of argument), of Forms of Propositions, the Algebra of the Copula, the Internal Multiplication and the Addition of Logic, the Resolution of Problems in Non-relative Logic, with a further chapter on the Logic of Relatives. The fundamental point, however, which is under discussion in the first two chapters touches the nature of the copula. There is abundance of evidence to show that given a few elementary forms, it is possible to spin out logical or mathematical formulæ simply without limit. But the superstructure rests entirely upon the basis of elementary truth contained in the first axioms. In logical science it is emphatically true that "C'est le premier pas qui coûte." There is a momentous choice to be made at the outset, and if we then take a wrong view of the nature of the logical copula, we can never come right again by any amount of development or formulisation.

Prof. Peirce after mentioning that four different algebraic methods of solving problems in the logic of non-relative terms have been proposed by recent English and German logicians, adopts a fifth, which he thinks is perhaps simpler and certainly more natural than any of the others. Peirce commences by expressing all the premises by means of the copulas $-\!\!<$ and $\overline{-\!\!<}$, "remembering that A = B is the same as A $-\!\!<$ B and B $-\!\!<$ A" (p. 37). These new symbols are to be interpreted so that A $-\!\!<$ B means (A implies B), in the way that water implies liquidity, or all water is liquid. The symbol $\overline{-\!\!<}$ is the negative of the above, so that C $\overline{-\!\!<}$ D means that C does not imply D. He then lays down five other processes which give the elementary theorems of the calculus, showing how to develop, simplify, transpose, and infer equivalency by these symbols. As however these processes occupy two quarto pages in their first statement, it is evident that they cannot be reproduced here. The question which really emerges is not as to the power and originality shown by Prof. Peirce, about which no reader of his memoirs can entertain the slightest doubt, but as to the wisdom of the first step, the selection of the relation expressed by the symbol $-\!\!<$ instead of that expressed by the familiar sign of equality =. Prof. Peirce begins by remarking that A = B is the same as A $-\!\!<$ B with B $-\!\!<$ A. For instance, all equilateral triangles are equiangular, and all equiangular triangles are equilateral. But though these two assertions are equivalent to "equilateral triangle = equiangular triangle," Prof. Peirce elects to treat the two parts of the apparently compound proposition separately, his reasons being given partially on p. 21. This is not the first time that the same choice has been made; for, not to speak of Aristotle and the Aristotelians generally, De Morgan elected to base his systems of logic upon inclusion and exclusion, instead of upon equality. In his symbols X || Y is compounded of X)) Y and X ((Y (Syllabus, p. 24), that is to say all Xs are all Ys is made of all Xs are Ys and all Ys are Xs. Now without going far afield, I believe that a sufficient reason may be given for holding that both De Morgan and Peirce have chosen wrongly. A class is made up of individuals, and the very conception of a class thus implies the relation of identity expressed in A = B. If I say the colour of glacier ice is identical with the colour of pure rain water, it is impossible to break this assertion up into "The colours of glacier ice are among those of pure rain water," and "The colours of pure rain water are, &c." The colour is one indivisible and identical. Now if there is at the basis of all reasoning an elementary assertion of the form A = B, which is incapable of resolution into anything simpler, this sufficiently proves that Peirce's A $-\!\!<$ B, or De Morgan's A)) B cannot be the original elementary form of assertion. Moreover, when we say that all equiangular triangles = all equilateral triangles, the real basis of assertion is that each possible equiangular triangle is identical with one possible equilateral triangle. The plural is made up of the singular, and the singular is incapable of logical decomposition. You may decompose A = B into As are Bs, and Bs are As, but ultimate decomposition gives us $A' = B'$, $A'' = B''$, $A''' = B'''$, A', A'', &c., being individuals.

It is highly curious, however, that this very question arises again with reference to the so-called Calculus of Equivalent Statements recently published by Mr. Hugh MacColl, B.A., in the *Proceedings* of the London Mathematical Society (First paper, November 1877, vol. ix. pp. 9-20; Second paper, June 13, 1878, vol. ix. pp. 177-186; Third paper, vol. x. pp. 16-28; Fourth paper, vol. xi.; see also *Mind*, January 1880, pp. 45-60, and the *Philosophical Magazine* for September 1880).

There can be no doubt that Mr. MacColl has shown much skill in devising neat symbolic forms, and much power in using them. Comparing his processes with those of De Morgan, for instance, it is impossible not to admire their symmetry and lucidity. But when we touch the real point, the nature of assertion and inference, I am obliged to hold that Mr. MacColl has, like De Morgan and Peirce, elected wrongly. What De Morgan expressed by X)) Y, and Peirce by X $-\!\!<$ Y, MacColl puts in the form $x : y$, calling the assertion an *implication*. Curiously enough, he professes never to treat of things, but only of assertions, so that with him $x : y$ means that the assertion x implies the assertion y, or whenever x is true, y is true. Having carefully considered Mr. MacColl's proposals, I felt obliged to write of them in a recent publication as follows:—"It is difficult to believe that there is any advantage in these innovations; certainly, in preferring implications to equations, Mr. MacColl ignores the necessity of the equation for the application of the Principle of Substitution. His proposals seem to me to tend towards throwing Formal Logic back into its Ante-Boolian confusion."

In a paper printed in the *Philosophical Magazine* for January 1881, Mr. MacColl takes me to task and invites me to make good the charge about Ante-Boolian confusion, by entering into a friendly contest in the problem columns of the *Educational Times*. Having just recently spent the better part of fifteen months in solving other people's problems, and in inventing some two or three hundred new ones, published in "Studies in Deductive Logic," I certainly do not feel bound to sacrifice my peace of mind for the next few years by engaging to solve any problems which the ingenuity and leisure of Mr. MacColl or his friends may enable them to devise. I therefore decline his proposal with thanks. But I can easily explain what I mean by ante-Boolian, or what comes to much the same thing, anti-Boolian confusion. The great reform effected by Boole was that of making the equation the corner-stone of logic, as it had always been that of mathematical science. Not only did this yield true and simple results within the sphere of logic, but it disclosed wonderful analogy between logical and mathematical forms, to which De Morgan adverts in the passage quoted above. All true progress in the philosophy of those fundamental sciences depends upon ever keeping in view the fundamental identity of the reasoning processes, as depending on the process of substitution, practised explicitly by algebraists for some two or three centuries past, and implied in the geometrical reasoning of Euclid.

But Mr. MacColl takes a backward step; he says he can make a simpler notation by taking $a:\beta$ instead of my $a = a\beta$. In regard to form there is absolutely no novelty in the implication, for it is simply De Morgan's X)) Y, or the ancient Aristotelian proposition A is B. It is true that Mr. MacColl makes his terms consist of assertions, so that all his assertions would appear to be assertions about assertions—a needless complexity, landing us in the absurdity that a calculus of equivalent statements has no means of exhibiting the statements themselves. Mr. MacColl claims indeed considerable advantage for his notation on the ground that in the syllogism $(a:\beta)(\beta:\gamma):(a:\gamma)$, the very same relation which connects a with β, and β with γ, connects also the combined premises $(a:\beta)(\beta:\gamma)$ with the conclusion $a:\gamma$. He thinks that my notation is very clumsy and roundabout, because, as my propositions treat of things or qualities, I should have to use words to express the inference of one proposition from others. In that case Mr. MacColl must bring the like charge of clumsiness against the whole body of mathematicians, because their equations are between things or their magnitudes, and they still use language "hence," "therefore," &c., to express the fact that certain equations lead to other equations. If there is any mathematical sign to denote inference, it is rarely used, unless it be the familiar ∵ and ∴, which are merely shorthand signs.

Mr. MacColl however, while pointing out the excellence of his implications, objects to my statement that he rejects equations in favour of implications on the ground that his method admits of both forms: "As a matter of fact," he says, "I employ both, sometimes even in the same problem. In my first paper . . . I adopt the equational form throughout; in my second and third papers, which relate entirely to questions of pure logic, I generally adopt the implicational form, as the simplest and most effective; while in my fourth paper, which treats of probability, I mainly adopt the equational form." There is nothing which I can see in this to contradict my objection that Mr. MacColl rejects equations *in favour of* implications. Mr. MacColl uses implications as "the simplest and most effective," but he adopts the equational form, I suppose, when he finds it indispensable; if not, why does he not hold to his simple and effective implication? If he finds one form best in logic and the other in mathematics, then he is ante-Boolian, because it was the whole point of Boole's labours to establish identity of method in logic and mathematics. I have really no wish to condemn Mr. MacColl's calculus or to enter into controversy with him, but in the interests of truth and sound science I must assert my belief that his implication $a:\beta$ is at the best but a shorthand rendering of $a = a\beta$, which is Boole's form adopted by me. I have not said, and do not undertake to say, that Mr. MacColl's formulæ are not concise and neat. But a shorthand notation is bad if it obscures the real nature of the reasoning operation, and the fact that Mr. MacColl always keeps the equation in the background as a reserve method to call into operation when needed, shows to my mind that his methods are mistaken in a philosophical point of view. The very name of his method is "The Calculus of Equivalent Statements," and the word equivalent sufficiently implies that the equation is at the bottom of the matter. The end of it all then is that $a:\beta$ has one letter less in it than $a = a\beta$, and to save the trouble of writing this one little letter Mr. MacColl would have us obscure all the grand and fertile analogies which Boole disclosed to the astonishment of mathematicians in 1847 and 1854. Mr. MacColl says: "The question whether the implication $a:\beta$ or its equivalent the equation $a = a\beta$ should be preferred in a symbolical system of logic, must be decided on the broad grounds of practical convenience." It is not however a question of practical convenience, but of philosophical truth which is at issue, and in thus playing fast and loose with the equation, Mr. MacColl shows his entire want of comprehension of what is involved in the Boolian reform of logic. It may be added that were Mr. MacColl to discard implications and use only the equations which he admits are equivalent to them, there would be no formal difference between his calculus and that modified form of Boole's calculus which I proposed in 1864, and have been ever since engaged in developing, excepting indeed Mr. MacColl's unaccountable adoption of assertions as terms.

Perhaps it ought to be added that Boole, both in his "Mathematical Analysis of Logic," and in his great "Laws of Thought," introduces chapters on what he calls "Secondary Propositions" or Hypotheticals, which deal, like Mr. MacColl's assertions, with the truth of other assertions; but nothing emerges from Boole's discussion of secondary propositions except that they obey exactly the same formal laws as primary propositions, and are of course expressed equationally.

W. STANLEY JEVONS

ILLUSTRATIONS OF NEW OR RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION[1]

III.

THE animals we now speak of are again inhabitants of North-Eastern Asia—a country which, as before remarked, has of late years produced a considerable number of accessions to the list of Mammals. Both of them also belong to the great group of Ruminants—which is of special interest, as embracing all the animals upon the flesh of which civilised man principally subsists.

6. The Japanese Goat-Antelope (*Capricornis crispa*). For many years Siebold's "Fauna Japonica" was almost our only authority on Japanese zoology. The Dutch, having long had a monopoly of Japan, were enabled to stock their great National Museum at Leyden with a host of objects unknown to the other cabinets of Europe, but of which their travellers and residents managed to obtain specimens from various parts of the land where they only were permitted to penetrate. The "Fauna Japonica," although Japan is now open to all the world, still remains the best work of reference on the mammals of Japan. In it will be found the first description of the singular goat-like antelope of which the Zoological Society have recently obtained their first living example, drawn up by the celebrated naturalist Temminck, formerly director of the Leyden Museum. Temminck named the animal *Antelope crispa*, from the rough coat of hair which covers it, and tells us that it inhabits the higher alps of the Japanese Islands Nippon and Sikok, and is known to the Japanese as the "Nik." But a more complete account of its habits has lately been published by Capt. H. C. St. John in his recently-issued "Notes and Sketches from the Wild Coasts of Nipon." Capt. St. John tell us that the Japanese chamois, as he calls it, "is a very difficult animal to find, and to bag when found; they keep to the highest mountains, and to the highest and must rugged peaks of these ranges. I have hunted them with the natives, and with their dogs, and this often; and yet only once, although often close to the creatures, have I had a

[1] Continued from p. 417.