

Chinese; by M. Ujfalvy, on the Aryans north and south of the Hindoo Koosh; and by Baron Vaux, on the Kanakas of New Caledonia.

THE additions to the Zoological Society's Gardens during the past week include two Quebec Marmots (*Arctomys monax* ♂ ♀) from North America, presented by Mr. N. Stainfield; a Prairie Wolf (*Canis latrans* ♂) from North America, presented by Mr. R. Payze; three Suricates (*Suricata tetradactyla*) from South Africa, presented by Mr. W. R. Dobbin; two Red-beaked Weaver Birds (*Quelea sanguinrostris* ♂ ♀) from West Africa, presented by Mrs. Nettleship; two Swift Parrakeets (*Lathamus discolor* ♂ ♀) from Tasmania, presented by Mr. J. Abrahams; four Common Vipers (*Vipera berus*), British, presented by Mr. Walter E. Blaker; two Smooth Snakes (*Coronella lewis*), British, presented by the Rev. Charles Harris; two Red Kangaroos (*Macropus rufus* ♂ ♀), a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited; four Beautiful Finches (*Estrelda bella* ♂ ♀ ♀) from Australia, a Lanner Falcon (*Falco lanarius*), captured at sea, purchased; a Hybrid Luddorf's Deer (between *Cervus luddorfi* ♂ and *C. canadensis* ♀), a Hybrid Mesopotamian Deer (between *Dama mesopotamica* ♂ and *D. vulgaris* ♀), a Red Deer (*Cervus elaphus* ♀), four Australian Wild Ducks (*Anas superciliosa*), a Mandarin Duck (*Aix galericulata*), bred in the Gardens.

ZOOLOGICAL NOMENCLATURE¹

II.

DR. COUES said that he was much gratified at the interest shown in the subject of zoological nomenclature, and indorsed the words of the Chairman that names were of the greatest possible consequence. Nomenclature was a necessary evil, and the point was always to employ that method of naming objects which should most clearly reflect not only the characters of the objects themselves, but our ideas respecting those characters and the view we take of them. As to what constitutes a species, there had been an absolute revolution in the definition of a species since the time of Linnæus, the opinion having been long held that every species was a distinct and individual creation. But that idea had passed out of existence in the minds of most natural historians of the present day, who accepted a general theory of the evolution of species by a gradual modification. That being the case, it was idle to inquire "What is a species?" no such thing existing any more than a genus; and so intimately related were all forms of animal and vegetable life that, if they were all before us, no naming would be possible, for each would be found to be connected completely with another; therefore the possibility of naming any species was, as it were, the gauge and test of our ignorance. Having thus touched very briefly upon the subject of missing links, which alone enable us to name objects which still exist, Dr. Coues proceeded to inquire, "What of so-called species the connecting links between which are still before our eyes?" In illustration of this he would cite some instances of connecting links which exist between certain forms. He then referred to the case of one of the best-known Woodpeckers in North America (*Picus villosus*), and discussed its climatal and geographical variation. He was of opinion that all these geographical races were indistinctly separable forms, and he would indicate them by trinomial names, proceeding upon the definite principle of geographical variation according to conditions of environment, meaning by this all the external influences which modify the plastic organism. Moisture, the humidity of the atmosphere, appeared to have the greatest effect, particularly in regard to colour. Latitude, with its varying degrees of heat, determined size more than any other influence. As a matter of fact this condition of things was found to occur, and the question was, How should we recognise it in our language? Specification had ceased to be of use, and the question was whether the system in favour in America was sufficient or insufficient to meet the case. On these points he would be glad to hear opinions; and in concluding he would read a paragraph from the new edition of the "Key to North American Birds," giving formally the rule for the employment of trinomials as now

¹ Continued from p. 259.

in use by American ornithologists and many other zoologists of the United States. This rule is as follows:—

"No infallible rule can be laid down for determining what shall be held to be a species, what a con-species, sub-species, or variety. It is a matter of tact and experience, like the appreciation of the value of any other group in zoology. There is, however, a convention upon the subject, which the present workers in ornithology in this country (America) find available—at any rate, we have no better rule to go by. We treat as 'specific' any form, however little different from the next, that we do not know or believe to intergrade with that next one;—between which and the next one no intermediate equivocal specimens are forthcoming, and none, consequently, are supposed to exist. This is to imply that the differentiation is accomplished, the links are lost, and the characters actually become 'specific.' We treat as varietal of each other any forms, however different in their extreme manifestation, which we know to intergrade, having the intermediate specimens before us, or which we believe with any good reason do intergrade. If the links still exist, the differentiation is still incomplete, and the characters are not specific, but only varietal, in the literal sense of these terms. In the latter case, the oldest name is retained as the specific one, and to it is appended the varietal designation."¹

Dr. Günther, F.R.S., said that during the whole of this discussion it appeared to him that this new movement was in fact a reaction. It had always appeared to him that ornithologists went too far in attaching to the slightest modification of colour the rank of a species; and when he looked over the list of species of a genus well known to him, he found there a number of different forms distinguished for very different reasons, and could not help being struck by the great diversity of value which was attached to the distinctive characters of these various forms. There was nothing to show that there was any system in distinguishing and naming the species of birds. He looked with favour on the method proposed by Dr. Coues and his compatriots. It was a system he had himself employed occasionally in his systematic writings since 1866, and Dr. Coues would find that in some cases he had adopted it pure and simple. He (Dr. Günther) had been disappointed in looking over the new edition of Dr. Coues' "Key to North American Birds," for he found there that Dr. Coues adopted trinomials in some cases and binomials in others. He maintained that logically one ought to adopt the trinomial nomenclature for all other forms, and keep the binomial only for that category in which these varieties may be contained. If Dr. Coues and those who were with him adopted that system, he for one would gladly employ it in all those cases in which the geographical range of certain forms is clearly ascertained.

Dr. Sclater, F.R.S., would remind Dr. Coues that this mode of designating the forms of animal life was by no means a new one, as might be seen on reference to Schlegel's "Revue Critique," published in 1844. He thought the only difficulty lay in the extent to which it was likely to be carried out. Dr. Coues, in his preface to the new "Key to the Birds of America" had hinted at this difficulty. If too much stress were laid upon the value of trinomialism we should open the flood-gates to an avalanche of new names by naturalists who have not taken enough trouble to investigate the matter under consideration. The time had now come when it would be advisable to a certain extent to use trinomials. He could not at all agree with Dr. Coues when he said that no such thing as species exists, for forms were known which had all the characters of well-marked species. It was only in cases where faunæ had been fully worked out that trinomial names would come into use, and for such forms he was quite prepared to adopt the system.

Mr. Blanford, F.R.S., said that he would add one word to the discussion, as nobody else had taken up the one or two points which might be advanced in opposition to the proposed system. He thought the movement an unfortunate one, for the reason that it would certainly have the effect of rendering nomenclature in general less certain than it was before. An equation containing three variables was much more complicated than one in which there were only two, and when one had three names any one of which was liable to be changed to suit personal views the fixture of nomenclature would be even further off than it is now. Then the case of ornithology was not nearly, in point of fact, so complicated as some other classes, as, for instance, in the Mollusca. Trinomial nomenclature had been proposed to,

¹ A more formal and elaborate presentation of Dr. Coues' views may be found in the *Zoologist* for July, 1884, p. 247, being the verbatim report of the address delivered before the National Academy of Sciences at Washington U.S.A., in April last.—ED.

but almost universally rejected by, a meeting of geologists. He did not consider that the time had arrived for any innovation, and thought it desirable first to agree upon strict rules of zoological nomenclature.

Prof. Bell agreed with Mr. Blanford that the method would not be universally applicable. How could it be applied for instance to cases where varieties were found living with one another, as was often the case with littoral forms with free-swimming embryos?

Mr. W. F. Kirby said that it was necessary to distinguish sub-species and varieties at times, and there seemed to be only two courses open to us, either to retain the binomial nomenclature, and to treat sub-species, so far as nomenclature was concerned, as equivalent to species, or to retain varietal names. Still it was difficult to lay down hard and fast rules, applicable to all cases; and he feared that the system of naming varieties was liable to great abuse, especially in entomology, where the number of species is already so great. Thus we have 100,000 species of *Coleoptera* on our lists; and one of the most variable families is that of the *Coccinellidae*, in which some entomologists have lately begun to name mere colour varieties of single species by twenty and thirty at a time. Mr. Kirby thought, too, that, whenever a named form previously regarded as a variety was held by a later author to be worthy of specific rank, the varietal name should, wherever practicable, always be retained for the species, instead of a new one being imposed. He knew that this was not always adhered to, but in his own work he made it an invariable rule.

Lord Walsingham said that, as Dr. Coues suggested that the trinomial system should be used only in distinguishing gradations, he would instance two species very common in England, small species of the genus *Teras* (*Teras hastiana* and *Teras cristata*). These exhibited a very extensive series of individual variations, but some varieties, although perhaps reared from the same brood of larvae, showed marked differences not distinctly connected by intermediate gradations with other forms of colouring. He asked how the method was to be applied to these if indeed it was intended to be applied at all to such cases. He himself knew several cases in North America in which variation, according to latitude, is very marked. You get a form in Vancouver Island gradually merging into the form in California, and in Mexico something apparently distinct if taken by itself, and probably only an extreme variety in colour and markings, but you have no form for the South of California. Would it be proposed to treat this Mexican form as a proper subject for the trinomial system or to give it only two names as at present? The principle appears to be right provided it facilitates the recognition of the forms we are naming. He hoped there was no danger of drifting into the inconvenient multiplication of names too commonly known in the catalogues of professional horticulturists.

Dr. Sharp said that whatever names we gave to morphological forms of less than specific value, whether we called them varieties, or sub-species, or morphological forms, we could not define or limit them; and if we attempted to name them, as no line could be drawn, we must go on till we gave a separate name to every individual that had passed through the hands of zoologists. He considered Dr. Coues' system of a third name unnecessary, because all the purposes it sought to attain could be accomplished without it by the old-fashioned system of "var. *a*," "var. *b*," and so on. The adoption of a system of names for forms lower than species he thought would lead to complete chaos.

Dr. Woodward, F.R.S., said he might mention two cases which occurred to him in which perhaps the system would be convenient. It was considered desirable by many palæontologists that the group of Ammonites should be broken up into a number of genera, and he thought the present plan of erecting specific names into generic ones was inconvenient. The student was already hampered with too many names, and we ought to remember that students were harder worked now than they were twenty-five years ago. The system of cramming he considered deteriorating to the stamina of the future naturalist. Every time a group was broken up into genera, sub-genera, species, and sub-species, the labour of the student was increased. Therefore it appeared to him that the use of a third term following the generic and specific one, as proposed, was very convenient if not insisted on as a matter of instruction.

Mr. H. T. Wharton would prefer not to see other names introduced unless they were absolutely necessary. But when well-marked intermediate forms had to be dealt with he ad-

mitted the value of the trinomial system, but of course Dr. Coues knew that the method advocated by him was not new to naturalists, for trinomial names were to be found in botanical catalogues. He should be glad to know how it was proposed to deal with such a form as *Corvus cornix*, for example, which in the new edition of Yarrell's "British Birds" had been united with *Corvus corone*.

Mr. H. Saunders said he would like to direct attention to a practical point of the question. Most of those present were aware that there was an unpretending annual called the *Zoological Record*, which consisted now of about 800 pages, and that if trinomialism were adopted, it would make the volume of too great a size. He would also remind those present that Mr. Sharpe was the recorder of Aves, and he did not know how that gentleman would relish the additional labour which would be thrown upon him if this system were generally adopted.

Dr. Traquair said:—I think I quite understand the scope and limits of the system so ably advocated by Dr. Coues, but I feel convinced that were any such system to receive the authoritative sanction of naturalists, these limits would not be observed by the ordinary crowd of name manufacturers. My own studies in recent zoology have been more especially of an anatomical and morphological character, but in the subject of fossil ichthyology I have been brought face to face with the question of the definition and naming of species. Here I conceive that the "species" must include all those forms which can be indubitably shown to graduate into each other. For such species, the only idea of a species which seems to me practicable, one generic and one specific name are quite enough, and I would leave each author to deal with "sub-species" and varieties as he pleased, but without permitting him to apply any *authoritative* name to such. So great, in many cases, is the amount of variation observed in fossil fishes and fish remains, and so difficult is it also to arrive at safe conclusions as to specific identity or distinction with the material before us, that, were the proposed system of trinomial nomenclature to receive the authoritative sanction of naturalists, I am convinced that in this department the flood-gates would simply be opened for a deluge of new names, from people whose sole function in life seems to be to invent such on the most trifling pretext. If the binomial system is at present often abused by such people for the creation of "species" which have no existence, save in their own imaginations, what might we not expect them to do if the adoption of a trinomial system afforded them further scope for their faculties!

Mr. J. E. Harting said if he could be satisfied that the introduction of a system of trinomial nomenclature, as proposed, would be of any real benefit to science, he should have no hesitation in adopting it. But, so far from any advantage resulting from it, he feared that a positive disadvantage would accrue from its adoption in a way which had not been sufficiently considered. The tendency to describe as new species mere individual variations had already (with certain specialists at least) become very prevalent, and had led to an expression of regret and dissatisfaction amongst those who were content to take a broader view of things, and who regarded such a process of refining as tending to perplex, while in no way advancing science. All workers in zoology found themselves sooner or later in one of two classes, which had been named, expressively, if not elegantly, "lumpers" and "splitters." Now, if the proposed system of trinomial nomenclature were to be adopted, the former class would have either to surrender at discretion to the latter, or a wider gap than ever would be created between them, a result which would surely lead to great inconvenience; while the latter, who had already gone to great lengths in what he had termed the process of refining, would receive fresh encouragement to go to still greater lengths in that direction, to the disadvantage, as he conceived, of those who were to come after them. We had been told by the advocates of the trinomial system that it was impossible not to recognise climatic variations in any given species when they were found to be constant and well marked. In this he agreed: he only dissented from them in regard to the mode by which such recognition was to be effected. To say that the only mode of recognising such variations was to add a third name to the generic and specific names was begging the question. If any such variation as that alluded to was sufficiently well marked to distinguish it at once from the species of which it was said to be a variety, he would prefer to regard it as an allied species, and bestow on it a specific name, retaining a binomial nomenclature. The binomial system had been found to work well enough in practice, from its simplicity; and it was

surely simpler to write *Turdus propinquus* than *Turdus migratorius propinquus*. After all, nomenclature was not science, and even if we had the most perfect system of nomenclature which could be devised, he did not see how science would be thereby advanced. It is true we could not get on without nomenclature, but the simpler it was the better; and the less time we spent in discussing it the more we should have to devote to real study.

Dr. Coues, replying to previous speakers, said that the system of trinomial nomenclature had nothing whatever to do with individual variations of specimens from one locality. It was not a question of naming varieties or hybrids, but there was a definite principle to proceed upon, namely, that of geographical and climatal variation. He was well aware that the use of three names to designate objects in zoology was no new thing; but he believed that the restricted application of trinomialism to the particular class of cases he had discussed was virtually novel, and that his system would prove to be one of great practical utility. He thought that the application of the principle was a question which, after this discussion, and after further private discussions, might well be left to the discretion of authors.

The Chairman concluded the meeting by saying:—I hope that Dr. Elliott Coues is satisfied with the manner with which his views have been received. Although there are some uncompromising binomialists present, many have pronounced themselves as what may be termed limited trinomialists, and some appear to go as far as Dr. Coues himself. Distinctly defined species undoubtedly exist in great numbers, owing to extinction of intermediate forms; for these the binomial system offers all that is needed in defining them. But on the other hand there are numbers of cases in the actual state of the earth, and far more are being constantly revealed by the discoveries of palæontology, and nowhere so rapidly as in Dr. Coues' own country, where the infinite gradations defy the discrimination either of a binomial or a trinomial system. Zoologists engaged in the question of nomenclature are being gradually brought face to face with an enormous difficulty in consequence of the discovery of these intermediate forms, and some far more radical change than that now proposed will have to be considered. In conclusion I must express the thanks of the meeting to Dr. Coues for having brought his views and those of his countrymen, of whom he is such a worthy representative, before us, and also to Mr. Bowdler Sharpe, to whose zeal and energy the organisation of the meeting is entirely due.

A unanimous vote of thanks was given to Prof. Flower for presiding.

KRAKATOA

AT the meeting of the Meteorological Society of Mauritius on May 22 some interesting communications were made relating to the Krakatoa eruption. Among others was a letter from M. Lecomte regarding detonations heard at Diego Garcia on August 27. In his letter, which was written at Diego Garcia on April 24, M. Lecomte says:—"Le lundi 27 août entre 10 et 11 heures du matin, pendant le déjeuner, nous avons entendu des détonations sourdes mais violentes. Nous avons cru tellement à l'appel d'un navire en détresse que nous avons couru et que j'ai envoyé plusieurs hommes vers le rivage extérieur de l'île sur plusieurs points différents, en observation. Le Capitaine Florentin de l'*Éva Joshua* et son second, M. Daniel Sauvage, venaient de quitter Pointe de l'Est pour aller mouiller à Pointe Marianne, lorsqu'ils ont entendu les mêmes détonations. Ils ont aussitôt envoyé des hommes en observation à l'extrémité des mâts. Mais comme les miens ils n'ont rien vu.

"Ce jour là et les jours suivants le soleil était comme obscurci, probablement par la formidable quantité de vapeurs et de cendres qui ont du s'élever dans l'atmosphère."

The information obligingly furnished by M. Lecomte was valuable, inasmuch as, taken in conjunction with the reports which had been received from Rodrigues, it confirmed verbal information which had been previously obtained. There could now be no doubt that the explosions which took place at Krakatoa were distinctly heard both at Diego Garcia and Rodrigues, and there was probably no other recorded instance of sound having travelled over so great a distance. The fact, also, that at Diego Garcia the sun was partially obscured on August 27 and on several subsequent days, as well as at the Seychelles and Rodrigues, was an additional proof of the great quantity of

matter which must have been ejected from Krakatoa, and of the rapidity with which it was conveyed from its source. There could be no reasonable doubt that the presence of that matter in the atmosphere was the cause of at least the lurid sunsets and sunrises which were observed over the Indian Ocean on the last days of August and in the first week of September.

The Secretary, Dr. Meldrum, stated that the Royal Society of London had appointed a Committee to collect information regarding the phenomena which had been observed during and after the volcanic eruptions that took place at Krakatoa in August, and requests had been received from that and other quarters for information from Mauritius. To these requests the Secretary had replied that he was preparing for his Excellency the Governor a detailed account of what had been observed at Mauritius and several of its dependencies, but that owing to the almost daily reception of additional details his report was not yet ready. All he did, therefore, was to give the general results as far as they had been determined.

Several remarkable phenomena had to be described. In the first place, there were disturbances of the sea water, or, as they had been called by some, tidal disturbances, and these had been observed all over the Indian Ocean.

There were also barometric disturbances, to which attention had first of all been called in Mauritius early in September, and which at the time were ascribed to the explosions at Krakatoa. Some time afterwards it was ascertained in England that these disturbances had extended over the whole globe and that they were recorded by all self-registering barometers in both hemispheres. At Mauritius there were at least seven well-marked disturbances of which the epochs of *maximum intensity* were as follows:—

		h. m.	
(1)	August 27,	0.6	p.m. local time.
(2)	" 28,	2.20	p.m. "
(3)	" 28,	10.40	p.m. "
(4)	" 30,	1.35	a.m. "
(5)	" 30,	9.17	a.m. "
(6)	" 31,	1.48	p.m. "
(7)	" 31,	8.00	p.m. "

At first these disturbances were supposed in Mauritius to have been due to successive eruptions, but General Strachey, who examined a number of barographs received from different parts of the world, had recently adduced evidence to show that they were produced by an air-wave proceeding outwards from Krakatoa in all directions round the earth, expanding till it was half round, then contracting till it reached the antipodes of its origin, and afterwards returning, the wave thus travelling round the globe two or three times. Assuming that view, the first disturbance at Mauritius (which was at its maximum at oh. 6m. p.m. on the 27th) would be caused by the passage over the Observatory of the wave travelling from east to west; and the third, fifth, and seventh disturbances would be returns of the wave to Mauritius after having gone round the earth. Similarly the second disturbance would be the first passage of the wave travelling from Krakatoa eastward, and the fourth and sixth would be its returns to Mauritius. Now, the mean interval in time between the returns of the wave to Mauritius, in its passage from east to west, was 24h. 38m. and in its passage from west to east 35h. 44m. It would thus appear that the rate of progression had been greater from east to west than from west to east, which may have been partly or wholly due to the great circle passing through Krakatoa and Mauritius being within the tropics, where the prevailing wind was from the eastward. The rate of progression from east to west was very nearly 709 miles an hour, and from west to east 697 miles. By taking as nearly as possible the times half way between the commencements and endings of the disturbances similar results were obtained. There was also an *eighth* (but small) disturbance between 7 and 9 a.m. on September 2, which may have been the fourth return of the wave from east to west, the interval in time between that disturbance and the seventh having been nearly thirty-six hours. The sixth disturbance was the last indication of the wave in its passage from west to east.

Another effect of the Krakatoa eruptions was the spread of ashes and pumice over considerable portions of the Indian Ocean, and a good deal of information on that point also had been collected in Mauritius. The first intimation of the probability of volcanic action in the direction of the Straits of Sunda was contained in a letter published by Capt. Walker, of the *Actea* in the *Mercantile Record* of June 16, 1883. At noon on