

ZOOLOGICAL NOMENCLATURE.

BY J. A. ALLEN.

THE subject of trinomial nomenclature seems just now to be attracting much attention, not only in this country but abroad, especially in England, where a special meeting was recently held to consider the matter. The meeting was held July 2, in the lecture room of the Zoölogical Department of the British Museum, pursuant to the subjoined call,* which sufficiently explains the occasion of the meeting. From the report of the proceedings in 'The Field' of July 6, and in 'Nature' of July 10 and 17, we learn that among those present were Lord Walsingham, Professor Flower, F. R. S., Dr. Günther, F. R. S., Dr. P. L. Sclater, F. R. S., Dr. H. B. Woodward, F. R. S., Professor Traquair, F. R. S., W. T. Blanford, F. R. S., Henry Seebohm, F. L. S., Howard Saunders, F. L. S., Professor J. Jeffrey Bell, J. E. Harting, F. L. S., G. A. Boulenger, H. T. Wharton, F. L. S., S. O. Ridley, F. L. S., W. F. Kirby, Sect. Ent. Soc., Herbert Druce, F. L. S., W. R. Ogilvie Grant, and R. Bowdler Sharpe, F. L. S. The chair was taken by Professor Flower, who, in opening the proceedings, read a letter from Professor Huxley, P. R. S., expressing his regret at not being able to be present, in

* "ZOOLOGICAL NOMENCLATURE.

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"SIR: Taking advantage of the presence in this country of the distinguished American Zoölogist Dr. Elliott Coues (who represents the advanced opinions of American Naturalists), it is proposed to hold a meeting of British Zoölogists to consider the expediency of adopting certain changes, more especially in the direction of trinomial nomenclature.

"For the purpose of obtaining a discussion of the question a meeting will be held in the Lecture Room of the Natural History Museum on Tuesday, July 1st [2d], at 3 P.M. (Professor Flower, F. R. S., in the chair), when Mr. R. Bowdler Sharpe will read a paper (with illustrations) "On the expediency, or otherwise, of adopting Trinomial Nomenclature in Zoölogy."

"As the question is one of great importance to Zoölogists your attendance at this meeting is *earnestly requested*. Dr. Coues will be present.

I am, sir,

Your obedient servant,

R. BOWDLER SHARPE."

consequence of pressing official business. From the full report of the meeting given in 'Nature' we condense the following abstract of proceedings:—

The Chairman, Professor Flower, in his opening remarks, alluded to the extreme importance and difficulties of the subject, for while the name of any natural object is one of its most trivial and artificial attributes, laxity in the use of names causes endless perplexities and hindrances to the progress of knowledge. He often found little difficulty in making out the characters and structure of an animal, but when called upon to decide by what name to call it he often found himself in a sea of perplexity. He hoped the present discussion would help to clear up our ideas on the subject. Abstaining, with the impartiality due from the chair, he would withhold his opinion upon the merits of the rival schemes to be proposed until after hearing the arguments, and called upon Mr. R. Bowdler Sharpe to read a paper 'On the expediency, or otherwise, of adopting Trinomial Nomenclature.'

Mr. Sharpe said he approached the discussion of the subject without the least prejudice either for or against the adoption of trinomial nomenclature. He alluded to the fact that for some time the system had been recognized and followed by zoölogists on the other side of the Atlantic, and stated that to a certain extent the principle had been admitted by more than one worker in the Old World. The presence in this country, he said, of one of the most able advocates of the system, Dr. Elliott Coues, has recently stimulated the thoughts of many of us as to the wisdom of its adoption for the zoölogy of the Old World, and it had occurred to him that a friendly meeting to discuss the matter with Dr. Coues and some of the leading British zoölogists could certainly do no harm, and might be productive of a considerable amount of good. It seemed to him that there are certain facts in nature which we all recognize, but about the expression of which many of us entertain different views. He proposed merely to bring forward certain difficult aspects of the question as they presented themselves to him, and would be glad to have an expression of opinion upon the facts to which he should call attention. In illustration of the difficulties he laid upon the table a series of specimens illustrating what he considered to be one of the most interesting examples of what he conceived to be a series of subspecies, or representative races, of one dominant form.

The birds in question were the *Astur badius* group of Goshawks.

‘In Southern Africa is a small form called *Astur polyzonoides*, which inhabits the whole of the South African subregion, but does not, so far as my knowledge goes, extend beyond the Zambesi. In Senegambia and Northeast Africa it is replaced by a race called *Astur sphenurus*, in which the color of the under surface is much more delicate than in *Astur polyzonoides*. From Central Russia, throughout Turkey, Asia Minor, Persia, and Syria, a large race called *Astur brevipes* replaces the two foregoing subspecies, and forms a third. From Baluchistan, throughout India, and Ceylon, a somewhat smaller form, *Astur badius*, takes up the running, and throughout the Burmese countries, extending to Formosa and Hainan, we have yet another race, *Astur poliopsis*, which is a purer and more elegant edition of *Astur badius*. This little group of Goshawks has been well worked out, and we may fairly presume that we have the facts before us. Now I should like to know if this is a case where we might adopt the trinomial system, and call these birds

Astur badius,
Astur badius poliopsis,
Astur badius brevipes,
Astur badius sphenurus.
Astur badius polyzonoides.

“At present, were I writing about the South African bird or the Abyssinian bird, I should never speak of them as *Astur badius*, which is the name belonging to the Indian bird exclusively, and I am not quite sure that we gain in this case anything whatever by adopting trinomial nomenclature. The same parallel may be drawn with some of the species of *Scops* among the Owls, as may be seen by the series now exhibited, and here trinomial nomenclature might perhaps be employed. Thus the representative races of *Scops giu* would be *S. giu capensis* in Africa, *S. giu pennatus* from the Himalayas, *S. giu minutus* from Ceylon, *S. giu stictonotus* from China, *S. giu japonicus* from Japan, *S. giu malayanus* from Malacca, *S. giu rufipennis* from Madras, and *S. giu brucei* from North-Western India.”

In further illustration he adduced a group of Asiatic Crows, where he believed trinomial nomenclature could be employed to advantage. A case of a different kind was presented by several species of *Chibia* from the Malay Archipelago, where the

Drongos from different islands or groups of islands were representative insular forms. The use here of trinomial designations he believed conveyed an exact impression of the value of these forms, which are so closely allied as to be almost indistinguishable. A more difficult case is that of the Yellow Wagtails, in treating which Drs. Finsch and Hartlaub, and also Baron von Heuglin have employed, as he believed prematurely, trinomial nomenclature. Mr. Sharpe considered that the intermediate forms which undoubtedly exist are due to another and totally different cause, viz., to hybridization, although the case is not proved.

Mr. Sharpe, in continuing, said: "There is one advantage which we must all admit that the American zoölogists possess over ourselves, and that is, that they have a clear idea of the natural geographical divisions of their continent, and their zoölogy has been studied from many distinct points of view, such as the presence or absence of rainfall, etc., and it only requires a glance at Mr. Hume's essay on the distribution of Indian birds with respect to the distribution of rainfall throughout the Indian peninsula to see how very important is this aspect of the subject. Even in the British Islands there are variations in the size and coloration of some of our resident birds, as any one may learn from Mr. F. Bond, who has devoted sixty years of his life to the study of British ornithology, and who now has one of the most interesting collections in this country. But when we come to study the birds of Europe and the Palæarctic region generally, how small is our real knowledge, and what vast areas are there concerning the ornithology of which we know next to nothing! Great praise is, therefore, due to men like Dr. Menzbier, who has just written the first part of an elaborate treatise on the geographical distribution of birds in Russia; but it will be a long time before we can have in any museum such a series of birds as is possessed by the Smithsonian Institution for any one wishing to study the geographical distribution of the birds of North America." He added that the British Museum was fully alive to the importance of the question, but he found that nothing was more difficult than to procure from his colleagues in other countries of Europe representative sets of the common resident birds of their respective countries.

In regard to the Goshawks, the Scops Owls, and the Crows, he was not yet certain whether treating them as subspecies, as

he had done in his 'Catalogue,' was not as advantageous as the employment of trinomial nomenclature. In regard to the Long-tailed Titmice (*Acredula caudata* group), where several forms are connected by intermediate gradations, he believed the adoption of the trinomial system would be a positive advantage.

In concluding he stated that the great difficulty he perceived in the way of the adoption of trinomial nomenclature was encountered in the fact that it would open the door to a multiplication of species, or races, founded on insufficient material by authors lacking in experience of the difficulties of the subject; "but," he added, "I cannot conceal from myself that the code of nomenclature proposed by the British Association and followed by most of us, scarcely accounts for the treatment of facts as they have been developed in zoölogical science since the promulgation of that code, and that before long it will be the duty of British zoölogists to attempt its modification."

Mr. Seebohm followed with a paper in continuation of the subject, in which he showed an exceptionally clear conception of the conditions of the problem to be met, and proposed a "modification of the American system of nomenclature." He said: "The question of a binomial or trinomial nomenclature is not a very simple one. So long as ornithologists were under the delusion that all species were separated from each other by a hard and fast line, the binomial system of nomenclature was sufficient. Now that we know that many forms which have been regarded as species are connected by intermediate links with each other, and that many species present important local variations which cannot be ignored, we are obliged to admit the existence of subspecies as well as species. There can be no doubt that the too tardy recognition by European ornithologists of what might not unreasonably be regarded as the most important fact in ornithology discovered during the present century has been very largely due to a pedantic adherence to a binomial system of nomenclature. Now that we have emancipated ourselves from the fetters with which our predecessors, with the best intentions in the world, cramped our ideas, the question arises, how shall we recognize in our nomenclature the existence of sub-specific forms; by a word, or by a sentence? The ornithologists of America think that a system of trinomial nomenclature will answer the purpose. They have come to the conclusion that the insertion of a third link in the

chain which binds us will give our ideas scope enough. Their theory is that the judicious ornithologist will be able to select from the infinite number of steps which form the series of intermediate races which lie between two intergrading species, one, two, three, or even in some cases more local or climatic races which are worthy of being dignified by a name. This theory is on the face of it somewhat illogical. It credits ornithologists with an amount of discretion which their past history does not justify, and totally ignores the inordinate desire to introduce new names which is unfortunately too conspicuous in most if not all ornithological writers, culminating in the absurdities of a Brehm. That ornithology should be preserved from being Brehmised must be the devout prayer of every well-wisher of the science. On the other hand, the recognition of subspecies by a sentence would be to revert to the customs of the præ-Linnæan dark ages of nomenclature, a retrograde step from which all zoölogists would instinctively shrink. Members of the British Ornithologists' Union are probably all prepared to admit that a medium course is safest at least for an Ibis (*medio tutissimus ibis*), and, with a very slight modification I, for one, am prepared to adopt the American system in spite of its dangers. If no paths are to be trodden in which the indiscreet may err, there is an end at once to all progress.

“To point out the modifications which I propose to introduce into the American system of nomenclature to change it from an empirical system to a logical or scientific system, I will take as an example the Common Nuthatch (*Sitta europæa*), and show how the nomenclature of its various races may be made exhaustive, so that the temptation to introduce new names, which appears to be irresistible to the indiscreet ornithologist, may be minimised.

“*Sitta uralensis*, with white under parts, is found in Siberia; *Sitta cæsia*, with chestnut under parts, is found in England; intermediate forms connecting these species together are found in the Baltic provinces. What can be more simple than to call the intermediate forms by both names, *Sitta cæsia-uralensis*? But there is a third species which turns up in China, *Sitta sinensis*, and which is also connected with *Sitta uralensis* by intermediate forms. Never mind; they too can be called by both names, and our series of Nuthatches runs geographically in an unbroken series:—

Sitta cæsia,
Sitta cæsia-uralensis,
Sitta uralensis,
Sitta uralensis-sinensis,
Sitta sinensis.

“So far so good; but, unfortunately, two more complications arise. Besides the series running southwest into *S. cæsia*, and that running southeast into *S. sinensis*, two other series run from the central form *S. uralensis*, one running due west and then round by the Baltic into the Scandinavian *S. europæa* (a larger bird, and somewhat darker on the under parts), and a second running due east and then round the Sea of Okotsk into the Kamchatkan *S. albifrons* (a bird much paler on the head, which shades into white on the forehead), so that it is necessary to add four more names to the list, which will stand as under:—

“*Sitta cæsia* is found in Britain, South-West and South Europe, and Asia Minor. It is medium in size, but extreme in the darkness of the chestnut of the under parts.

“*Sitta cæsia-uralensis* (with a hyphen between the two specific names) represents all the forms intermediate between South European and Siberian examples, which occur in Denmark, Pomerania, the Baltic provinces of Russia, Poland, and the Crimea.

“*Sitta europæa* is the Scandinavian form, and represents the extreme of size, whilst in color it is intermediate between the forms found in the Baltic provinces of Russia and Central Siberia.

“*Sitta europæa-uralensis* comprises all the intermediate forms in Russia which connect the Scandinavian with the Central Siberian forms.

“*Sitta uralensis* is found in the valleys of the Ob, the Yenesei, and Lena, and combines the small size characteristic of the various Asiatic subspecies of Nuthatch with the dark upper parts of the sub-tropical forms, whilst the under parts are nearly as white as in the Kamchatkan form.

“*Sitta uralensis-albifrons* may be applied to all those intermediate forms found in East Siberia and the north islands of Japan which are not quite so pale on the upper parts as the Kamchatkan form.

“*Sitta albifrons* is found in Kamchatka, and represents the

extreme form so far as whiteness of the forehead and under parts is concerned.

“*Sitta uralensis-sinensis* may be applied to the series of forms found in the valley of the Amoor, the island of Askold, and the main island of Japan. They are intermediate in color between the Central Siberian and Chinese forms, and are scarcely to be distinguished from the Baltic province forms.

“*Sitta sinensis* is found in China, and only differs from the British form in being slightly smaller and in not having quite so much dark chestnut on the flanks.

“I have purposely chosen a complicated case in order to show the capabilities of the system, which, if the specific name of *europæa* is always repeated after the generic name of *Sitta*, becomes a compromise between that adopted by the Americans and that which I imperfectly carried out in the fifth volume of the ‘Catalogue of Birds in the British Museum,’ and which was originally suggested to me by a conversation with Mr. Salvin. It has at least the merit of being exhaustive, and differs so slightly from that in common use in America that its adoption does not involve a change in, but only an addition to, the system which in some form or other is destined to supercede the binominal system now rendered inadequate by the acceptance of the theory of evolution.

“As an example of the compromise I propose, I add a list of the local races of the Dipper, with their geographical ranges:—

“*Cinclus aquaticus melanogaster* (Scandinavia).

“*Cinclus aquaticus melanogaster-albicollis* sive *Cinclus aquaticus* (West-Europe, as far north as the Carpathian and as far south as the Pyrenees).

“*Cinclus aquaticus albicollis* (South Spain, Algiers, Italy, Greece).

“*Cinclus aquaticus albicollis-cashmiriensis* (Asia Minor, Caucasus, Persia).

“*Cinclus aquaticus leucogaster* (East Siberia).

“*Cinclus aquaticus leucogaster-cashmiriensis* (Central Siberia).

“*Cinclus aquaticus cashmiriensis* (Cashmere, South Siberia, and Mongolia).

“*Cinclus aquaticus cashmiriensis-sordidus* (Altai Mountains).

“*Cinclus aquaticus sordidus* (Thibet).

“In this system it must be observed that wherever there is a fourth name it is always connected by a hyphen to the third name, and comprises all the intermediate forms between the two. It is somewhat cumbersome, but it provides for the contingency of any intermediate links that may occur. To express it algebraically, it provides not only for AB and BC, but also for AC. It is perhaps the only system which is theoretically perfect, but the question whether its voluminousness renders it impracticable or undesirable is one requiring careful consideration.”

Dr. Coues, following Mr. Seebohm, said that he was much gratified at the interest shown in the subject of zoölogical 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 them. He referred to the revolution in opinion that has taken place since the time of Linnæus in respect to what constitutes a species; a revolution brought about by the acceptance of the theory of evolution. It was now idle to ask “What is a species?” no such thing existing any more than a genus. So intimately related are all forms of animal and vegetable life, if they were all before us (including the extinct as well as the living), 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?” He then briefly stated his views on the points at issue, citing in illustration of the subject our well-known case of the Hairy Woodpecker (*Picus villosus*). Dr. Coues’s views are too well known, however, on this side of the Atlantic to render it necessary to give his remarks at length.

Dr. Günther said that he looked with favor on the method proposed by Dr. Coues and his compatriots, and stated that 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. If Dr. Coues and those who

were with him would follow the system of adopting trinomial nomenclature for all forms he for one would gladly employ it in all those cases in which the geographical range of certain forms is clearly ascertained.

Dr. Sclater would remind Dr. Coues that this mode of designating the forms of life was by no means new, as might be seen by reference to Schlegel's 'Revue Critique,' published in 1844. His own chief objection to the system of trinomial nomenclature was its liability to abuse. The time had now come when it would be advisable to a certain extent to use trinomials. It is only in cases where faunæ have been fully worked out that trinomial names should come into use, and for such forms he was quite prepared to adopt the system.

Mr. Blanford advanced some objections to the proposed system. It involved more terms, any one of which was liable to be changed to suit personal views, and therefore rendered fixity in nomenclature more remote than before. He thought it also less suited to some other classes of animals than to birds, and alluded to the fact that the system was almost universally rejected by a recent meeting of geologists.* He did not consider that the time had come for any innovation.

Professor Bell agreed with Mr. Blanford that the method would not be universally applicable.

Mr. W. F. Kirby said that it was necessary to distinguish subspecies and varieties at times; but he feared that the system of naming varieties was open to great abuse, especially in entomology, where the number of species is so great. He urged, very properly, that whenever a named form previously regarded as a variety was raised to specific rank, the varietal name, wherever practicable, should be retained for the species, instead of a new one being imposed as is sometimes done.

Lord Walsingham cited a number of cases of geographical variation among insects and inquired how the system would apply in the particular cases instanced.

Dr. Sharp, a well-known entomologist, thought a system of names for forms lower than species would lead to complete chaos,

* It should be said, however, that there was no one present to properly explain its scope and aims, or who understood its purpose well enough to speak intelligently in its defence. A glance at the report of the discussion is sufficient to show that it failed partly through prejudice against innovation, but mainly through ignorance as to what the system really is.

as no line could be drawn until we gave a separate name for each individual which passed through the hands of zoölogists.

Dr. Woodward, speaking from the point of conchology, could mention cases in which perhaps the system would be convenient. But the additional third term would impose additional labor upon the student, as was the case whenever a group was broken into genera, subgenera, species, and subspecies.

Mr. H. T. Wharton admitted the value of the trinomial system when well-marked intermediate forms had to be dealt with, but he would prefer to see no other names introduced unless they were absolutely necessary. He called attention to the fact that the method was not new, for trinomial names are to be found in botanical catalogues.

Mr. H. Saunders said that he would like to direct attention to a practical point in this question. "Most of those present were aware that there was an unpretending annual called the 'Zoölogical Record,' which consisted now of about 800 pages, and that if trinomialism were adopted, it would make the volume of two great a size."

Dr. Traquair felt convinced that were any such system to receive the authoritative sanction of naturalists, its proper limits would not be observed by the ordinary crowd of name-manufacturers. In fossil ichthyology he had been brought face to face with the question of the definition and naming of species. Here he conceived that the 'species' must include all those forms which can indubitably be shown to graduate into each other. For these the only practicable way seemed to be to have one generic and one specific name — a binomial system — and he would leave each author free to treat 'subspecies' and varieties as he pleased, but without permitting him to apply any authoritative name to such. If the present binomial system is abused by people who name 'species' which have no existence except in their imaginations, what might we not expect such writers to do if the adoption of a trinomial system afforded them further scope for their faculties!

Mr. J. E. Harting strongly opposed the system from the opportunity it afforded indiscreet specialists for naming mere individual variations as species, which was already so great an evil. He would agree to the recognition of climatic variations in any given species when they were found to be constant and well-marked, but he could not agree that the only way of recognising such variations was by adding a third name to the generic and specific

names. He would prefer to regard such forms as allied species and retain a binomial nomenclature. Nomenclature was not science, and he did not see how science could be advanced by the most perfect system of nomenclature that could be devised (!). It is true we could not get on without nomenclature, but the simpler it is the better; and the less time we spend 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 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 zoölogy 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 the 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's own country, where the infinite gradations defy the discrimination either of a binomial or a trinomial system. Zoölogists 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 organization of the meeting is entirely due."

It appears from the report of the meeting that the chief objection, and almost the only one advanced by the ornithologists present, to the system of trinomial nomenclature, was its liability to abuse on the part of indiscreet writers. This objection we incline to think is overrated, and is applicable with greater or less force to any system. The other objections have really little weight, and were raised mainly by those who, as their remarks clearly show, had not a proper conception of the workings of the proposed system.

Mr. Seebohm's proposed compromise is certainly worthy of serious consideration, respecting which we beg to submit in this connection a few comments. In short, Mr. Seebohm would adopt trinomials pure and simple for subspecies, or for well-marked intergrading geographical forms, and to this extent is in full accord with the 'American school,' but would engraft thereon a means of designating the connecting links between such forms, through use of a polynomial designation. There is certainly a real gain in this, offset to some degree by the objection of cumbrousness. While still trinomial in principle and spirit, it practically adds a fourth term. The idea, as now fully unfolded by Mr. Seebohm, is not new to us on this side of the water, and though it has not been publicly brought forward, it has been to some extent considered privately and rejected—perhaps too hastily—as likely to add, as least seemingly, complexity and an undue burden to the system. Some years since, while engaged on a monograph of the American Squirrels, I employed a modification of Mr. Seebohm's method in labelling specimens, and have used it, and know of its being used by others to a small extent on labels in private cabinets, to express the relationships of connecting links between recognized subspecies. Without some such compromise such intergrading specimens cannot be satisfactorily designated, there being many such—all inhabiting certain intermediate geographical areas—that cannot be referred with propriety to one form rather than to another, they being so exactly intermediate between them; and yet to give them still another name, thus raising them to the rank of an additional subspecies, seems an unwarranted or at least injudicious piece of refinement. But for the proper designation of such connecting links Mr. Seebohm's compromise seems to go but half the way. For

instance, to illustrate, taking (hypothetically) Mr. Sechohm's case of the Nuthatches: For the Nuthatches the full form of designation requires the repetition of the specific name (*europæa*) after the generic name (*Sitta*) in each case. So we have *Sitta europæa cæsia*, *Sitta europæa cæsia-uralensis*, *Sitta europæa uralensis*, and so on. Mr. Seebohm asks, "What can be more simple than to call the intermediate forms by both names, *Sitta* [*europæa*] *cæsia-uralensis*?" Certainly, nothing could be simpler. But the intermediate forms—the connecting links— are obviously not of uniform character; in the nature of the case they cannot be. As we proceed eastward from the habitat of the typical or most differentiated phase of *cæsia* toward the region of the most extreme phase of *uralensis* we meet first with intermediates which are more closely allied to *cæsia* than they are to *uralensis*; then with phases as nearly allied to the one as to the other; and finally, in our eastward journey, with those more like *uralensis* than like *cæsia*. But all these intermediates that depart appreciably from either type Mr. Seebohm would call *cæsia-uralensis*, thereby ignoring the fact that a large part of the intermediates are allied more closely to *cæsia* than they are to *uralensis*, and another large part more closely to *uralensis* than to *cæsia*. If, however, we employ for the first element of the fourth name the name of the form to which these intermediates are most closely allied we are able in every case to exactly express their status and affinities. Thus, on the one hand, we would use the combination *cæsia-uralensis* for those intermediates which are more nearly allied to *cæsia* than to *uralensis*, and, on the other, *uralensis-cæsia* for those that more nearly resemble *uralensis* than *cæsia*. This would be equivalent to saying, *Sitta europæa cæsia*, varying toward *uralensis*, and similarly in other cases. Theoretically there should be a distinctive designation for those which are exactly intermediate — as well referable to the one form as to the other; but such intermediates being few in comparison with the number that lean appreciably to the one side or the other, they may be practically ignored without great loss in exactness of expression; unless we further compromise by agreeing to designate them by writing the two names as one word, without the hyphen, thus, *cæsiauralensis*, the first term, *i.e.*, whether *cæsia* or *uralensis*, being determined by the rule of priority, the older name being allowed in all cases to stand first. It might seem preferable to place first the

name of what may be supposed to be the stock form, or that from which the others have been differentiated; but the objection to this would be the liability to disagreement among zoölogists as to what was the stock form, and thus open the way to diversity of ruling, which adherence to the rule of priority prevents.

In this way we have provision for designating all possible degrees and qualities of relationship in the connecting links between subspecies. This, added to the trinomial system, allows for a degree of refinement in the expression of relationship sufficient to meet every possible contingency. It furnishes a system at once complete and exhaustive, and involves the use of no more terms than Mr. Seebohm's compromise contemplates. We simply ring the changes on the two hyphenized words making up Mr. Seebohm's third term. It likewise should prove a check upon the tendency on the part of indiscreet authors to invent new terms in their struggle to give 'handles to facts' in geographical variation among animals. I do not see why the system may not apply equally well to other classes of animals, and indeed in palæontology, where we have intermediate phases due to gradual differentiation in time, as well as under the geographical condition of space, the principle involved being the same.

But what does all this give us as a system of nomenclature? Not a *trinomial* one certainly, but rather a *polynomial* or, as Dr. Coues would say (see *anted* p. 321), a *polyonymal*, one; and yet one not in any way comparable with the *polyonymal* system of præ-Linnæan writers, but one based on a definite principle, and contrived with reference to the expression of ascertained facts in the evolution of life.

The only objection to the system is its cumbrousness, and this, at first sight, seems a grave one when compared with the *binomial* (or *dionymal*) system, but when weighed in view of the great degree of precision and refinement of expression attainable, the question as to its utility is certainly an open one. Were there not evidently a feeling on the part of at least a few leading zoölogists that even a *trinomial* (or *trionymal*) system, while a step in the right direction, fails to meet the requirements of the case, as so forcibly stated by Professor Flower in his closing remarks already given in this paper, I should not have ventured upon the suggestions above made. These, as above shown,

propose merely a modification, to suit different emergencies, in the composition of Mr. Seebohm's complex third term. I fail to see any objection to this proposed modification, while, on the other hand, it seems to offer special advantages.

Finally, a word on the composition of these polyonymal names. Obviously the specific name of a group of subspecies should be the earliest name applied to any member of the group; this of course should invariably form the second term in the designations of the several subspecies. Then follows the name of the different subspecies as the third term, when relating to their ordinary phases. When the third term becomes complex, through an effort to designate intermediate forms between two formally recognized subspecies, the first element of the complex term should be that of the subspecies to which the intermediates are most nearly allied; and so on, as already explained.

Doubtless for all ordinary occasions the simple trionymal form will be sufficient, but when greater exactitude may be required or seem desirable, as not infrequently happens, I certainly can see no shorter or more explicit way of designating the facts in the case than resort to the complex third term, with the above designated changes of position, etc., of its component elements.

COLLECTING IN THE COLORADO DESERT— *LECONTE'S THRASHER.*

BY F. STEPHENS.

DURING the last week of March, 1884, I spent four days in the extreme western end of the Colorado Desert, during which time I picked up several items of interest to ornithologists. As some reader of 'The Auk' may desire to try collecting on this desert, I will give a few hints, especially as they may help others to a better understanding of the 'lay of the country.'

The Southern Pacific Railroad enters the desert from the west through the San Gorgonio Pass, between the San Bernardino Mountains on the north, and the San Jacinto Mountains on the south. These ranges, or spurs from them, diverge toward