Library of the British Museum in the Years 1880-85," the latest of the series of indexes published under the auspices of our great national library. Having recently had occasion to consult the same with reference to certain biological works, I experienced so much difficulty in finding what I wanted, that I thought it worth while to inquire more fully into the trustworthiness of the volume as an aid to the working naturalist. I append my notes, in the interest of workers in a field the literature of which is already overburdened.

The book is printed in unnecessarily large type, whence there results a ponderous volume of 1044 pages quarto. Upon the immensity of the task before the bibliographer set to compile such a work I need not comment. If the duty be well performed, there can only result a product useful to all, and helpful to the specialist; if otherwise, confusion becomes confounded, and there ensues to the worker loss of time, if not actual disaster.

Printers' errors and minor inaccuracies are to be expected in a work of the kind, and any such compilation must of necessity be faulty. It is not surprising, therefore, to find "travel" for "translation" in recording the title of Scott Russell's work, on p. 1016. Other similar defects could be quoted, but why are the press-marks occasionally omitted? surely they are next in importance to the titles themselves? The compositor cannot be at fault here.

The compiler sets forth his scheme in a short preface, by no means a bad one if followed. One of the chief provisions reads thus: "Headings and sub-headings are in strictly alphabetical order; precedence being given under each to the larger and more important works." It is in the execution of this self-imposed dictum that the compiler is seen at his best. Who is to be the judge of the relative merits of the collected works, and what is to be the standard of comparison? Presumably, the compiler performs the difficult task in person, but it is indeed well that his method is not revealed, for Huxley's "Crayfish" heads the list of general works upon Mollusca (p. 653), and Miss Buckley's "Life and her Children" that upon Micro-organisms. The thing must be a joke! Nothing of the kind!! "Coralline Algen" brings up the rear of Corals (p. 212), while "Les Batrachospermes" are exalted to an equality with the Batrachians—a truly novel means this of demonstrating the unity of the biological sciences. The aforementioned inaccuracies amount almost to incredibilities, and it is difficult to realise that they are perpetrated in earnest. It is inconceivable that they can have resulted from mere carelessness; but, even were that so, the fact that they exist at all would be sufficient in itself to disqualify the compiler. We can only regard them as the natural outcome of giving to one man the work of half a dozen; they disfigure the work, and shake our confidence in much that remains. A little less pardonable, perhaps, is the relegation of "Der Tasman-Gletscher" to the Column Tasmania.

The choice of headings is often ill-advised. Four works are recorded upon the Polyzoa; these are equally divided between Polyzoa and Bryozoa, to neither of which headings is there a cross-reference. Writings upon organisms in air generally are distributed between sections Air and Atmosphere, Micro-organisms, and Bacilli and Bacteria, with no cross-references. Papers dealing with the broadest generalisations and the subtlest of details are muddled together regardless of system; while under Micro-organisms there is a cross-reference to Infusoria, sole possessor of which department is Kent's well-known Manual. Section Palæontology is especially introspective. It is divided into the sub-heads General, Bibliographical, Collections of Fossils &c., Fossil Fauna, and Flora—a sufficiently inconsistent arrangement in itself. Conspicuous among the general works is Brongniart's "Recherches sur les graines fossiles silicifiées." A knowledge of the publications of his own department ought certainly to be expected of the compiler, through whose hands the covers of the said works must surely pass. But no; for while there has been recorded under the last-named heading Rupert Jones's "Catalogue of Fossil Foraminifera," we look in vain for the companion volumes of Hinde and Lydekker, both of which were published within the period embraced. There is here something akin to contempt for authority! Marsh's "Odontornithes," divorced from Palæontology, finds a home among the Birds.

Having found that the leading tenet of the scheme was violated—that relating to the precedence of important works—I turned next to the second one. It reads: "All books are, so far as possible, grouped together according to their language, in

the following order—English, French, German, Italian. . . . There are, however, some deviations from this system—for example, in the history of each country the titles follow each other in chronological sequence, irrespective of language." How the two methods of arrangement thus vaguely formulated are to be simultaneously adopted, we are not informed; and in this ingenious attempt to combine two schemes into one neither has been adhered to—both have suffered. A cursory glance at some of the more important sections, e.g. Birds, shows that the conditions have not been in the least fulfilled, so far as pertains to precedence of language; while, to go afield, we find under Clergy, Priesthood, &c., a unique arrangement in which the languages run thus—Spanish, English, German, French, Italian, English, *ad finem*. Where, here, is the professed patriotism? It deserts the author under the inspiration of Clergy, Priesthood, &c., and leaves him reckless. What now of the chronological sequence? One turns instinctively to various historic headings. In none of those examined did I find the exception to be the rule.

The major scheme nullified, the preface becomes a blank; but the details of the superstructure are no more to be relied upon than is the foundation itself. Under Insects, to which the reader is referred from Entomology, there are titles in abundance in which the two catchwords occur; but he looks in vain for references to the fact that the majority of entomological papers are scattered among Coleoptera, Diptera, Hymenoptera, Lepidoptera, Ants, Wasps, Bees and Bee-keeping, and other headings. The Colorado Beetle figures under that heading and under Potato, but not under Coleoptera; while, to complicate matters, a German and an English work upon the subject appear under Potato, the place of the former being, under Colorado Beetle, usurped by a Spanish rival. Fish and Fisheries embraces all branches down to Oyster-Culture, but why not Salmon Fisheries? It appears to suffice that they should take shelter under Salmon, with no cross-references. A special heading is set up for Angling, but several definite works thereon are to be found only under Fish and Fisheries, no references being given under Angling.

Titles of works are not unfrequently repeated, and that unnecessarily. For example: Morris's "Letters about Birds" occurs under Birds (sub-headings—General, and Great Britain and Ireland); Dixon's, Swainsland's, and Watkins's books are not under their appropriate headings; while Patterson's "Birds, Fishes, and Cetacea frequenting Belfast Lough," which should be under Fauna, Local, is misplaced. Under the last-named heading Canon Tristram's "Fauna of Palestine" appears beneath sub-section Italy—the compiler seems doomed to disaster in his relations with the clergy. In one or two cases where comprehensive titles occur, a process of dismemberment has crept in. For example: Romanes's "Jelly-Fish, Star-Fish, and Sea-Urchins" is to be found under Star-Fish and Sea-Urchins, with the title mutilated beyond recognition, but not at all under either Jelly-Fish, Medusa, or Echinoderm; Scott Russell's "Wave of Translation in the Oceans of Water, Air, and Ether," to which allusion has already been made, occurs, also with a misquoted title, under Water, but under neither Ether nor Air and Atmosphere.

Such are the results of an attempt to gauge the working capacity of the "Index" mainly from a single stand-point. I leave other specialists to inquire for themselves. It has been said that this "work proves the ability and industry of its compiler, and that the Trustees are anxious to make the treasures of the great library a success." Granted the good intentions of the governing body and the industry of the compiler; of the rest the reader must judge for himself—I refrain from further comment thereon. It cannot, however, be admitted that the compiler has done his best; errors cannot be avoided where non-technical hands are applied to technical work, but slovenliness is ever intolerable. I submit that a distribution of labour is demanded for the success of the next essay of the kind.

JUSTITIA

The Origin of Species

IN a recent issue of NATURE (Nov. 25, p. 77) Mr. Catchpool writes:—"Is it, or is it not, the fact that allied species, which are confined each to a particular island, prove, when brought together, far less frequently infertile than species, equally dissimilar, which had lived in the same district, might be expected to prove? On the answer to this question depends, as far as I

can see, the fate of the theory of physiological selection. Can no one answer it?"

This is a question which has arisen out of the theory of physiological selection itself, and, as Mr. Catchpool was the first to propound that theory, I am afraid he will find that no one as yet is able to answer his question. But it may interest some of your readers to know that I have collected a considerable body of facts tending to show that there is a correlation between fertility of allied species and the fact of their living on isolated areas, as well as another correlation between sterility of allied species and the fact of their living on continuous areas. But it seemed to me undesirable to publish these facts until very largely augmented; and, finding that no observations had been expressly made upon the subject, I read my paper before the Linnean Society for the purpose of inducing naturalists in different parts of the world to try experiments on the fertility of nearly allied species inhabiting isolated areas, as well as to test for degrees of sterility between natural varieties and parent forms on continuous areas. Meanwhile I am myself trying experiments on the mutual fertility of isolated species, as well as upon the sterility of natural varieties with parent forms. Where birds and mammals are concerned, I am using the numberless islands of all sizes on the west coast of Scotland, which are admirably suited to the purpose. Any of your readers in any parts of the world who are acquainted with well-marked natural varieties of birds or mammals which (together with their parent forms) would be likely to thrive on these islands, would greatly oblige me by communicating suggestions.

I may take this opportunity of also requesting any of your readers who may have further remarks or criticisms to make on the theory of physiological selection not to delay their publication. For it is surely desirable that discussion of the subject "on the high *priori* road" should come to an end. Until a large number of experiments shall have been made, any definite judgment upon the theory must be either biased or premature; and therefore the only influence that can now be exercised by adverse criticism is that of discouraging the work of verification. On this account, and on this account alone, is it worth one's while to answer such criticism. Hitherto I have waited till it should come to an end, and withdrawn the answer previously referred to in these columns as having been in type for the *Fortnightly Review*. When it has come to an end, I will furnish a general reply in the *Nineteenth Century*, and shall then hope to show that whatever "fate" may be in store for the theory at the hands of Nature, at all events it is certain that it has been in no way affected by the assault of naturalists.

GEORGE J. ROMANES

Heredity in Abnormal-Toed Cats

THE calculation made by Mr. J. H. Hodd in last week's NATURE (p. 53) of the numerical proportion of the sexes of Mr. Poulton's race of extra-toed cats, in relation to the recurrence of the abnormality, is very interesting as an inferrable deduction from the premises of Mr. Poulton's elaborate tabulation of his observations; but it is, I think, doubtful whether it is not misleading as a generalisation from the facts collected by him. A conclusion arrived at by means of a mathematical method is too absolute an inference, and not necessarily reliable, when applied to purely biological cases, on account of the numerous intervening factors (perhaps mostly unknown, but yet of importance) which are incapable of being appreciated within such an estimate, and which may, in consequence, invalidate the main point under consideration. This appears to be the case on the present occasion; for Mr. Poulton mentions, in describing the kittens of VIII⁵ (p. 39), that by far the most highly specialised development of the abnormality he has observed, throughout the entire race, was attained in a male—VIII⁵ iv. Considering that the large majority of individuals possessing the character are of the ♀ sex, as pointed out by Mr. Hodd, this fact is really very striking. But, at the same time, if the point alluded to by Mr. Hodd were considered as amounting to a principle, we should naturally be led to infer that the abnormality might become equivalent to a secondary sexual character, which is so improbable that it scarcely bears suggestion. Besides, it is necessary to bear in mind that all the observations carried on as yet by Mr. Poulton have dealt with the ♀ influence alone; and it may be accounted for by the very reason that all the ♂s have been carefully eliminated from exercising any influence on the race, that the predominant effect, numerically, is on the ♀ side; while it is most probable

that if an abnormal ♂ were selected (say, the individual just referred to) as a starting-point for experiment with normal ♀s, the result would exhibit a general tendency towards superior persistence of the character operating with greater potency amongst the males than the females. It would be very interesting to experiment in this manner under the isolation happily promised in the Madeiran Islands.

Mr. Hodd's statistical statement is, doubtless, correct in the main, under the restricted provision just suggested, only one must not attach too great importance to it. The preponderance in number of the ♀s over ♂s is, indeed, still more noticeable when the ratio of each sex to the total number of the thirty-six cats is considered. Thus it appears that of twelve ♂s 13 $\frac{3}{4}$ per cent. are normal, and only 19 $\frac{1}{4}$ per cent. abnormal; whilst of the twenty-four ♀s 19 $\frac{1}{4}$ per cent. are normal, as many as 47 $\frac{3}{4}$ per cent. are abnormal. It is perhaps a little premature to place entire reliance upon so small a number, but it will prove interesting to compare the statistics brought out in Mr. Hodd's letter with a larger number in time to come.

In considering such a close and carefully detailed analysis as that presented by Mr. Poulton, it is of no little interest to find, on referring to that wealth of facts and principles, "The Descent of Man," that these results are in complete accord with, and confirm the laws of inheritance formulated by Darwin, if the strictures imposed by him, as if in anticipation of future observations, are duly regarded; and as his remarks apply to the present subject very directly,—although bearing more especially upon sexual selection in general,—I cannot do better than quote some of the more pithy:—"Why certain characters should be inherited by both sexes, and other characters by one sex alone, namely by that sex in which the character first appeared, is in most cases quite unknown. We cannot even conjecture why with certain sub-breeds of the pigeon, black striae, though transmitted through the female, should be developed in the male alone, whilst every other character is equally transferred to both sexes; why, again, with cats, the tortoiseshell colour should, with rare exceptions, be developed in the female alone" ("Descent," 2nd ed., p. 232). It is curious that the cats under observation happen to be tabby-tortoiseshell, and the remark made by Darwin (p. 230), that "as a rule it is the females alone in cats which are tortoiseshell, the corresponding colour in the males being rusty red," fully obtains in the present case, since the ♂s are, unless tabby, sandy-coloured, but never tortoiseshell. It is furthermore important to notice that the sandy individuals have the supernumerary digits as fully developed as any of the ♀s in the same litter.

Darwin's explanation of the persistence of abnormal characters is on the lines of the theory of pangenesis, as follows:—

"It is in itself probable that any character appearing at an early age would tend to be inherited equally by both sexes, for the sexes do not differ much in constitution before the power of reproduction is gained. On the other hand, after this power has been gained, and the sexes have come to differ in constitution, the gemmules (if I may again use the language of pangenesis), which are cast off from each varying part of the one sex, would be much more likely to possess the proper affinities for uniting with the tissues of the same sex, and thus becoming developed, than in the other sex" (p. 232). On p. 237 we find the further remark:—"The presence of supernumerary digits, and the absence of certain phalanges, must be determined at an early embryonic period, . . . yet these peculiarities, and other similar ones, are often limited in their transmission to one sex: so that the rule that characters developed at an early period tend to be transmitted to both sexes here wholly fails."¹

And, in conclusion:—"Characters of the parents often, or even generally, tend to become developed in the offspring of the same sex, at the same age, . . . in which they first appeared in the parents. But these rules, owing to unknown causes, are far from being fixed. Hence, during the modification of a species, the successive changes may readily be transmitted in different ways; some to one sex, and some to both; some to the offspring at one age, and some to the offspring at all ages. Not only are the laws of inheritance extremely complex, but so are the causes which induce and govern variability" (p. 240).

It is quite remarkable, though it is not at all surprising, how closely Mr. Poulton's facts fit into these deductions of Darwin's, drawn, as they were, from such few instances that they seem to be little less than preconceived ideas.

But, as it may be due to a one-sided influence in special

¹ This is a point which perhaps requires further evidence.