CHARLES DARWIN, EDWARD BLYTH, AND THE THEORY OF NATURAL SELECTION

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CONTENTS

ABBREVIATIONS USED IN FOOTNOTES

A: The Autobiography of Charles Darwin, edited by Nora Barlow, London, Collins, 1958.
FO: Foundations of the Origin of Species, edited by Francis Darwin, Cambridge University Press, 1909.
MLD: More Letters of Charles Darwin, edited by Francis Darwin and A. C. Seward, 2 vols., London, John Murray, 1903.
MNH: The Magazine of Natural History, London.
N: Charles Darwin and the Voyage of the "Beagle," edited by Nora Barlow, New York, Philosophical Library, 1946 (this book contains the rough notebooks kept by Darwin during the voyage).
O: The Origin of Species, by Charles Darwin, 2nd edition, Cambridge University Press, 1935.
PG: Principles of Geology, by Sir Charles Lyell, 4 vols., third edition, London, John Murray, 1834.
LLD: Life and Letters of Charles Darwin, edited by Francis Darwin, 3 vols., London, John Murray, 1888.
VAP: Variation of Animals and Plants Under Domestication, by Charles Darwin, 2 vols., Orange Judd & Co., New York, 1868.

All of the three papers by Edward Blyth utilized in this study will be indicated by date alone after their first listing since they were all published in *The Magazine of Natural History*.

I. INTRODUCTION

On a gloomy day in October, 1836, a barnacle-encrusted ten-gun brigantine sailed into Falmouth Harbor, England. She was back from around the world in what was to become one of the century's most celebrated voyages—the voyage of the *Beagle*. On her deck, impatient for the sight of home was a tall, spare young man with the brooding eyes of a philosopher. He had looked much upon savage men and animals in remote uncharted lands. What he thought upon such matters was destined, in turn, to change the thinking of the world.

Already the lines which were to make that face one of the saddest and most familiar of the Victorian era were beginning to show upon the young man's countenance. Seasickness and insomnia had marked him; in Chile he had suffered from some affliction about which he was never to speak. It was Charles Darwin back from the five-year voyage which had convinced him of the enormous antiquity of the earth and of the changes time had wrought in the life which swarmed upon the planet's surface. With his own hands, Darwin had pried from the earth the bones of vanished creatures related to those of the present. He had seen man, in almost bestial simplicity, wandering on the bleak shores of Patagonia.

In spite of his carefully stored specimens below decks, the young man had returned from his travels puzzled if not completely baffled. He believed in evolution, or was at least impressed by the theory. He had come to suspect the alteration of the forms of life from age to age. The mechanism which molded life to fit innumerable strange environments eluded him, however. He had gone far enough to see that the explanations ventured by the older discredited evolutionists such as the Frenchman Lamarck would not satisfactorily account for all he had observed. The delicate adaptations of organism to purpose, as seen in a hummingbird or a woodpecker, could not be produced by such vague forces as climate or interior willed effort on the part of the animal. There must be something else, some other mechanism lying hidden beneath the surface of life.[1]

[1] "I came to think from geographical distribution, etc.," Darwin once remarked, "that species probably change; but for years I was stopped dead by my utter incapability of seeing how every part of each creature . . . had become adapted to its conditions of life" (*MLD* 1: 208).

Again and again, around and around, Darwin's diaries and notebooks reveal his pursuit of the subject. In his autobiography he says of the early period after his return from the voyage: "Nor did I ever intermit collecting facts bearing on the origin of species; and I could sometimes do this when I could do nothing else from illness."[2] Finally, by Darwin's own account, he chanced to read the political philosopher Thomas Malthus in October of 1838. He claimed to have seen, in the latter's treatment of the struggle for existence among human beings, the key to natural selection in the animal world.

> Being well prepared to appreciate the struggle for existence which everywhere goes on, [he said,] it at once struck me that under these circumstances favorable variations would tend to be preserved and unfavorable ones to be destroyed. The result of this would be the formation of new species. Here then, I had at last got a theory by which to work. . . .[3]

This account of the Darwinian discovery has been hallowed by tradition and reaffirmed by Darwin's descendants. It is true that as time has passed precursors have been located—Darwin himself was led to attach an account of them as a historical introduction to later editions of the *Origin*—but it has been generally assumed that Darwin arrived independently at his final disclosure, natural selection. It was taken for granted that Darwin was unacquainted with the minor anticipations of his work which research has revealed and that, like his great colleague Wallace, his act of synthesis was essentially a stroke of individual genius.

Today, one hundred years after the publication of the *Origin*, books and magazine articles by the score continue to extol this point of view. Because we are only a century away from a great reorientation in human thought it may be that we unconsciously prefer to see the formulator of the evolutionary hypothesis in solitary grandeur and isolation, a modern-day Moses descending with the tablets from an Andean mountain. Darwin, like George Washington, has come in science to bulk larger than human. He fills, and fills admirably, our need for a symbol. He has become one of the immortals. He is inviolate and sanctified. Investigations of the sources of his thought fade before the majesty of his achievement. If we are forced by facts to acknowledge that a few men entertained inspirational flashes of similar thinking before Darwin's time, we do so with discomfort and a feeling of guilt before that awe-inspiring father-image in our minds. We frequently prefer to drop the subject or to repeat the old formula of total independent invention.

In being thus evasive, however, we are falsifying scientific history. We are making the assumption that one of the widest read naturalists of his day was incapable of perceiving in books what he was so remarkably adept in seeing when he looked at tortoises and finches. To examine the sources of Darwin's thought is not to deprecate the magnitude of his accomplishment. It merely places that achievement in proper perspective, so that we can see how easily and imperceptibly the flow of thought passes from age to age, even when superficially there appear to be unrelated leaps or spectacular dissension.

Darwin, with a slight twinkle, has recounted how, after the delivery of his and Wallace's joint papers before the Linnean Society in 1858, Professor Haughton of Dublin had commented "that all that was new in them was false, and what was true was old."[4] Though we need not join Professor Haughton in condemning what was new in the papers, it is still interesting that the learned gentleman seemed to catch echoes of something out of the past. Perhaps, after all, his ear had not played him false. It is this very brief period, between the time when Darwin was rowed ashore from the *Beagle* and the opening of his first notebook on the "species question" nine months later in 1837, that I wish to turn. In those months is contained the real secret of the Darwinian story. What came afterwards was merely the long slow toil of building a great book. For that task Darwin was preeminently fitted. He had the indomitable building energy contained in one of his own reefs of coral. Like the coral he built slowly and soundlessly upon a submerged and hidden base.

Two years ago, in composing a work dealing with the major aspects of evolutionary thought, the present writer remarked that the *Origin* and its author have a history which runs silently and mysteriously through twenty years of ill health, lone effort, and corroding doubt. We know a little of the family influences which affected

[2] Barlow, Nora, *The autobiography of Charles Darwin*, 99, London, Collins, 1958.

[3] *A*, 120.

[4] *A*, 122.

Darwin's childhood; we can trace, in some degree, the impact of his Edinburgh and Cambridge education. We can follow his thoughts from an inspection of his diary and his letters to his family as he begins his long erratic passage around the world. But we know, for the documents lie ready to our hand, that as late as 1836 and for a short time thereafter, Charles Darwin was still struggling with the problem of the rapidity of organic change. "If one species does change into another it must be per saltum," he writes, "or species may perish."[5] No one has indicated what led the young Darwin to think in this way—a way, incidentally, quite foreign to the views he was later to express in the *Origin*.

In his master work slow and imperceptible transformations extending over vast ages of time have replaced these early and immature speculations. The fact is, however, that this important shift in Darwin's thinking remains undocumented. It has not even been discussed. Is this the view that he entertained on the famous voyage and brought home with him? Did he get it in the first months ashore after his return from the voyage and if so where? Is it his own? Is it someone elses? And if so whose? On this the published notes fall silent. They ask only whether a Henrietta St. Bath would probably answer a letter and give information about a tailless breed of cats near Malmesbury Head.

If it were not for a small series of accidental circumstances the events we are now about to recount would never have been transmitted to posterity. As it is, one is forced to call upon all the skills of the literary detective in order to make one's way, albeit dimly, into nine months in the lives of two young men one hundred and twenty-two years ago. One of these accidental circumstances to which we have referred was the discovery of Darwin's trial essays on the road to the *Origin*. Written in 1842 and 1844 they had never been printed. They were found long after Darwin's death in a cupboard in the old house at Down. Darwin's son Francis published them as the *Foundations of the Origin of Species* in 1909. Since they date into the earlier years of Darwin's work on evolution they yield clues to the past which have vanished in the *Origin*. The second accident was the rediscovery of Edward Blyth. These seemingly unrelated incidents have a deep bearing upon the problems we have been discussing but, like a puzzle, they have to be fitted together. In that puzzle even the tailless cats will have their place.

[5] Barlow, Nora (ed.), *Charles Darwin and the voyage of the Beagle*, 263, New York, Philosophical Library, 1946.

II. EDWARD BLYTH

Twice in a hundred years Edward Blyth has been faintly and somewhat timidly mentioned as a Darwinian precursor. Once after his death, though while Darwin was still living, in the annals of an obscure local natural history society;[6] once again in a letter to *Nature* in 1911.[7] The second discoverer was apparently unaware of the first. In neither instance does the announcement seem to have aroused attention. By the time this resurrection occurred the excitement over Darwin's forerunners had abated. Moreover, from the public standpoint Blyth, as we shall see, was not a very good example of a real precursor. In spite of his importance to our study he was not, at this time, an evolutionist. To grasp fully his part in early Darwinian events one must know his relation to Darwin.

It is just this point which Blyth's two discoverers failed to pursue.[7a] This was indeed a strange oversight because Edward Blyth was at least a casual friend of Charles Darwin and Darwin's later volumes are spotted with numerous references to his work. Anything, therefore, which Blyth might have to say upon species is deserving of the most careful scrutiny.

The paper which excited the attention of Geldart and Vickers appeared in *The Magazine of Natural History* in 1835[8] a short while before Darwin returned home. It was followed in 1837 by a lengthy second paper[9] which was apparently ignored by the two discoverers of the first document. Both papers are equally important to our study, and I shall make extended use of

[6] Geldart, H. D., Notes on the life and writings of Edward Blyth, *Trans. Norfolk and Norwich Naturalists Soc.* **3**: 38–46, 1879.

[7] Vickers, H. M., An apparently hitherto unnoticed anticipation of the theory of natural selection, *Nature* **85**: 510–511, 1911.

[7a] Vickers, it is true, conversed with Francis Darwin about the matter but did not pursue it further.

[8] An attempt to classify the "varieties" of animals, with observations on the marked seasonal and other changes which naturally take place in various British species, and which do not constitute varieties, *The Magazine of Natural History* (London) **8**: 40–53, 1835.

[9] On the psychological distinctions between man and all other animals, etc. etc., *The Magazine of Natural History* (London), n.s., **1**: 1–9; 77–85; 131–141, 1837.

FIG. 1. Edward Blyth. Reproduced from the *Journal of the Asiatic Society of Bengal*, August, 1875.

them. Blyth himself, however, deserves a preliminary word. A year younger than Darwin he was born, like Alfred Wallace, into straitened economic circumstances. He received only a trade school education but was an omnivorous reader and observer in natural history. He used a small inheritance to purchase a drug business in the town of Tooting in Surrey, now absorbed into greater London. Blyth, it appears, could not abate his passionate interest in zoology sufficiently to achieve business success. Much of his time was spent reading in the British Museum or making the rounds of kindred metropolitan institutions seeking a post more congenial to his interests. He was a born scholar, but he was trapped in miserable economic circumstances and haunted by ill health.

By 1841 he was advised to quit England for a warmer climate. Having published widely he was offered a small post as Curator of the Museum of the Royal Asiatic Society of Bengal. The next twenty-two years he spent in India, making innumerable specialized contributions to the natural history of southeastern Asia. His health failing, he returned in 1862 to England where he died in 1873. These are the spare, sad outlines of the life of a potentially great scientist frustrated by the time and the circumstances under which he was born. It is not unlikely that he was reading late in London on that day in 1836 when Charles Darwin, just ashore, was posting fast through the echoing countryside toward Shrewsbury. Edward Blyth was one to remember the color and shape of a darting bird or a fox going over a hedge. He saw things hiding, shifting, changing. He had what today we would call a photographic memory. This memory would make, in the end, part of the mystery that lies forever between Charles Darwin and Edward Blyth.

III. CLUES TO DARWIN'S KNOWLEDGE OF BLYTH

The idea of selection, though in its full creative aspect as "natural" selection it contains subordinate propositions, is, superficially at least, a simple concept. Once the scheme is grasped multitudinous examples lie ready at hand for illustrative purposes. There are no intricate propositions, no complicated mathematics which can be traced from one individual to another. In simpler forms the idea of selection was known to breeders, and here and there some dim relationship of selection to the struggle for existence was glimpsed by philosophers in both the eighteenth and early nineteenth centuries. Perhaps it is this fact which has caused scholars to despair of tracing all the peregrinations of the idea through successive hands. By the late eighteenth century it had been touched upon by several writers, including Lamarck, who had, however, skirted the subject without glimpsing its full significance. Its creative aspect beyond the bounds of species had not been grasped—probably both because of religious prejudice and the fact that the length of geological time, along with its successive faunas, remained largely unappreciated.[10]

Because of the enormous prestige of Darwin,

[10] For an account of the history of the concept one should consult Conway Zirkle's Natural selection before the "Origin of Species," *Proc. Amer. Philos. Soc.* **84**: 71–123, 1941. Dr. Zirkle observes very perspicaciously that "the widespread acceptance of teleology made natural selection an unheeded hypothesis" throughout its earlier history.

as well as this very elusive quality of a simple idea, the discovery that Blyth had written upon natural selection in 1835 fell upon deaf ears. No one, it appears, thought of actually examining Darwin's volumes with Blyth in mind. The present writer sought to do so for just one reason: I failed to comprehend how Charles Darwin could have been unacquainted with the periodical in which Blyth published. It was one of the leading zoological journals of the day. Darwin's friends, Henslow, Jenyns, Lyell, had all appeared in its pages. To assume that a man of Darwin's "prodigious memory and power of abstraction" was unacquainted with the thought of a man writing upon the species problem in so prominent an organ as *The Magazine of Natural History* seemed, in the period 1835 to 1837, illogical in the extreme. I set out, therefore, upon a careful and detailed examination of Darwinian materials which might bear upon the problem.

It was obvious that as a first step in my task the *Descent of Man* (1871) and *Variation in Animals and Plants Under Domestication* (1868) were apt to offer more clues than the *Origin* itself. Unlike the *Origin* they are extensively footnoted. Furthermore, since the materials for these books dated very largely into the pre-*Origin* days, it was likely that they might contain useful evidence. After all, both volumes were, in reality, part of the big original *Origin* which Wallace had forestalled by communicating his discovery of natural selection to Darwin in 1858.

My examination of the two volumes led rapidly to some highly interesting discoveries. It became evident that whatever the precise time might be in which Darwin had come upon the series, it was clearly demonstrable that Darwin had held in his hands, and made use of for scholarly purposes, the volumes of *The Magazine of Natural History* containing Edward Blyth's papers upon natural selection. I list all the references to the magazine below and it will be observed that they are numerous.[11] They reveal, not alone that Darwin at some point in his life had handled the volumes in question, but that he had used consistently the entire series. Some of the references are to small items and thus show that the volumes had been scanned with great care. In spite of this, and in spite of the fact that Darwin was fond of quoting Edward Blyth frequently upon a considerable range of topics, there is not a solitary direct reference to the two papers which particularly concern us—those of 1835 and 1837.

Charles Darwin had a high opinion of Blyth.[12] In *The Origin of Species* he spoke of him as one "whose opinion, from his large and varied stores of knowledge, I should value more than that of almost anyone."[13] Nevertheless, the two extensive and interesting papers in which Blyth treated of subjects directly pertaining to Darwin's greatest intellectual effort remain, as I have said, unnoted. Even more strange, there is one reference to a paper immediately adjacent to Blyth's paper of 1837.[14] This seeming failure to examine Blyth's papers of 1835 and 1837 is particularly odd when one comes to realize that Edward Blyth has four and a quarter inches of reference space in the index of the *Variation of Animals and Plants Under Domestication*, a space considerably greater than that allotted any other single individual in the book. One begins to get the feeling that something more than chance is at work in this situation. *The Magazine of Natural History* has been obviously utilized at great length by Darwin. We have his own testimony, corroborated by his son Francis, that he "read and abstracted . . . whole series of Journals and Transactions."[15]

[11] *Descent of man*, 785 n. 31; 788 n. 1; 803 n. 30; 807 n. 47, Random House ed. n.d. All of these references refer to *The Magazine of Natural History* **1**, 1837; n.s., this journal will be hereafter referred to as *MNH*.

Variation of animals and plants under domestication, 1st authorized American edition, New York, Orange Judd & Co., 1868, 2 vols. 1: 55 n. 75 contains a reference to *MNH* **6**, 1833. 1: 82 n. 42 refers to *MNH* **4**, 1840. 1: 115 n. 67 gives a reference to the same journal for **1**, 1829. 1: 335–336 n. 8 indicates a reference to *MNH* **8**, 1835 which is the precise volume of Blyth's first paper on natural selection.

Turning to the second volume of *Variation of animals and plants under domestication*, we find the following citations: p. 30 n. 47, *MNH* **1**, 1837; the same volume of *MNH* is also listed in p. 99 n. 33; on p. 188 n. 30, *MNH* **6**, 1833; on p. 189 n. 39, *MNH* **9**, 1836; p. 192 n. 52, *MNH* n.s. **2**, 1838; p. 193 n. 57, *MNH* **5**, 1832; p. 237 n. 7, *MNH* **8**, 1835; p. 354 n. 4, *MNH* n.s. **1**, 1837; p. 396 n. 21, *MNH* **1**, 1829.

[12] In the *Variation of animals and plants under domestication* Darwin remarks: "Mr. Blyth has freely communicated to me his stores of knowledge on this and all other related subjects" (1: 164 n. 1). The reference is to Oriental pigeons.

[13] *The Origin of Species*, 17, Oxford Univ. Press ed., 1935.

[14] *VAP* **2**: 30–31.

[15] *Life and letters of Charles Darwin* **1**: 83, London, John Murray, 1888. Francis Darwin (p. 153) spoke of his father's literary habits as follows: "When collecting facts on a large scale, in earlier years, he used to read through,

Yet a man whose work he obviously valued, a man whose name Darwin appears to have taken pleasure in promoting before the public, is represented only by his comment upon specific faunal items. Blyth is restricted to the role of taxonomist and field observer.

It may at this point, however, be still contended that Darwin did not know the files of *The Magazine of Natural History* in his earlier years, and that by the time the citations I have given were utilized Darwin was concerned solely with post-*Origin* subject matter. Though in this case it would still appear strange that Darwin made no reference to Blyth in his historical sketch appended to later editions of the *Origin*, for the moment let us allow this objection to stand. Let us go back into the less documented, early phase of Darwin's career in the hope of ascertaining whether any of the surviving papers of that era can throw light upon this curious and absorbing problem. Fragments from the early notebooks of 1836 and 1837, as well as the trial essays, should offer such published clues as are available. We have already seen that although Darwin was cognisant of evolutionary ideas before going to sea with the *Beagle* he did not arrive at a satisfactory solution for organic change until after his return from the five year voyage.

IV. BLYTH AND NATURAL SELECTION

In the notebooks which Darwin kept on the voyage there is contained all manner of stray jottings, ranging from the rise of continental areas to the price of melons. On July 19, 1835, the *Beagle* arrived at Callao in Peru. In Darwin's notebook for that day the following cryptic item is included. "Smelling properties discussed of Carrion Crows, Hawks, Magazine of Natural History."[16] There can be no doubt that this *Magazine of Natural History* is the one which later was to contain Blyth's first article treating of natural selection. It is evident that more than one issue had reached Darwin by mail at Callao. By the good fortune of his bird references we can identify at least two numbers as those of January and May. The volume is that of 1833 (vol. 6) which contains an article by Charles Waterton on the habits of the carrion crow, and another "On the Faculty of Scent in the Vulture." Darwin's friend Jenyns also had a paper on the principles of taxonomy in this volume. It is now unmistakably clear that Darwin as a young naturalist was as well acquainted with this journal as his colleagues and read it just as assiduously.

In the January issue of 1835 Blyth's paper on varieties made its appearance. One year later Darwin was still in Australia and thinking wistfully of home. His final notebook, has, unfortunately, not been fully published. Nora Barlow tantalizingly remarks that "He [Darwin] quotes from recent reading and begins a draft of a geological paper, presumably in the leisure of the slow days sailing home across the Atlantic."[17] One would like to know whether Blyth's paper had reached him, but now the last notebooks grow jumbled in time because Darwin out of long habit continued to carry and use them after having been ashore for two years.[18] Thus, unless additional unpublished material should throw light on the question, we cannot identify the precise month in which Darwin first saw Blyth's papers but that see them he did there can be no doubt. It is significant, I genuinely believe, that Darwin opened his first notebook on the "species question" in 1837. In January of that year Edward Blyth ventured the beginning of a second paper in which there is comment upon the principle of natural selection. In fact, from 1835 to 1837 there is sporadic discussion upon subjects of an evolutionary cast in *The Magazine of Natural History*.[19] We now come, however, to the crux of our discussion: Is it possible to trace in *The Origin of Species* or in the trial essays that preceded it any direct evidence of the influence of Edward Blyth? Our answer will affirm the truth of the connection between Blyth and Darwin but the clues upon which our assertion is based have to be mustered with care. If it had not been for the publication of the

and make abstracts, in this way, of whole series of periodicals." Writing to Huxley in 1859, Darwin commented, "I have picked up most by reading really numberless special treatises and *all* agricultural and horticultural journals; but it is a work of long years" (*LLD* **2**: 281).

[16] *N*, 244.

[17] *Op. cit.*, 260.

[18] *Ibid.*, 257.

[19] See, for example, Fennel, James H., Comments on Mr. Blyth's remarks on specific distinctions, *MNH* **9**: 647–648, 1836. Fennel says, "I do not think that the order of nature has offered any opposition to the structure and habits of any animal becoming . . . gradually altered in a greater or less degree from its primitive parents." Fennel, as is characteristic of the British scientists of this period, finds it necessary to dissociate himself from Lamarck, but his meaning is plain.

Foundations of the Origin of Species in 1909 it is unlikely that the dim outlines of the carefully hidden trail would ever have been perceived. This trail begins to be discernible in the Darwin notebook of 1836 with the curious word, "inosculate."[20] It is a word which never has had a wide circulation, and which is not to be found in Darwin's vocabulary before this time.

Twice in a single paragraph this word which means to adjoin, or pass into, is used in connection with evolutionary jottings and speculations. Moreover, these speculations, as we will see a little later, have a direct bearing upon problems presented in the various Blyth papers. Here I am concerned only to point out that a rare and odd word not hitherto current in Darwin's vocabulary suddenly appears coincidently with its use in the papers of Edward Blyth.[21] In the *Origin* it survives to make one fleeting appearance as "osculant."[22] Taken in conjunction with other evidence the rare and mildly archaic character of this word suggests the fact that Darwin, excited and stimulated by Blyth's discussion of varietal and speciation problems, acquired it from his reading of Blyth.

It is now necessary for us to familiarize ourselves more completely with the nature of Blyth's ideas as expressed in the three papers running from 1835 to 1837 in *The Magazine of Natural History*. At the beginning of this account of Blyth I remarked that he was not an evolutionist. Keeping this in mind one can still see an amazing resemblance between his thought and Darwin's, once one makes allowance for the fact that to Blyth the struggle for existence and natural selection were conservative rather than creative forces. In this restriction Blyth is reflecting the eighteenth-century limitation upon organic divergence which blinded so many early nineteenth-century thinkers.[23] It was Darwin's contribution, of course, that he altered the struggle for existence and made of it a creative mechanism. In doing so, however, he passed by way of the stepping stone of Edward Blyth.

Blyth, in his paper of 1835, composed when he was just twenty-five years old, betrays immediately the taxonomical interests which were to become so evident in his later writings. It is obvious that he is intrigued by organic change but has not succeeded in breaking out of the current thinking of his period except in three very important particulars: (1) Blyth refused to be engulfed by the mysticism in the quinary taxonomical system of MacLeay and Swainson which was so widely popular in the eighteen-thirties.[24] Instead, he sees at the root of any logical system of classification a law of "irregular and indefinite *radiation*." "The modifications of each successive type," he observes, are "always in direct relation to particular localities, or to peculiar modes of procuring sustenance."[25] (2) Blyth described natural and sexual selection in no uncertain terms and recognized their creative role in the emergence of varieties. He was clearly aware of variation and its hereditary character.[26] (3) In spite of denying that indefinite divergence could be produced in this way, he throws out in one amazing and contradictory passage the suggestion that just as man is able to affect the physical constitution and adaptations of domestic animals so wild nature might achieve the same success. He then asks "May not, then, a large proportion of what are considered species have descended from a common parentage?"[27]

It can thus be observed that though Blyth, like Buffon, denied himself, he produced within a short, usable compass an abstract of Darwinian evolution so remarkably complete that its ramifications needed only to be explored and documented. In Blyth's somewhat incoherent and rushing eagerness he threw off, and dashed by, the answers to his own objections. It is an apt illustration of the difficulties involved in escaping from the views of one's own age, particularly when those views are deeply imbedded in one's religious consciousness. Read in the light of Blyth's papers one can see that a large proportion of Darwin's early effort is devoted to finding a way through the species barrier as erected by Lyell and Edward Blyth though the latter is

[20] *N*, 263.

[21] Its use occurs in: Observations on the various seasonal and other external changes which regularly take place in birds, etc. etc., *MNH* **9**: 399, 1836, and in: On the psychological distinctions between man and all other animals, etc., *op. cit.*, 399, 508, 1837.

[22] *O*, Ch. 13, p. 386.

[23] I have discussed this matter at more length in my recent book *Darwin's century*, New York, Doubleday, 1958. See particularly p. 329.

[24] Swainson, William, *A preliminary discourse on the study of natural history*, 91–92, London, 1834; Rylands, Peter, On the quinary, or natural system of M'Leay, Swainson, Vigors, etc., *MNH* **9**: 130–138; 175–182, 1836. See also *MLD* **2**: 305, n. 1.

[25] *Op. cit.*, 406, 1836.

[26] *Op. cit.*, 45–46, 1835.

[27] *Op. cit.*, 135, 1837.

never directly mentioned in his text. To realize the extent of this relationship it is necessary, first of all, to get a clear picture of those aspects of Blyth's thinking which are germane to the evolutionary problem which confronted Darwin when he arrived home in 1836. It will be observed that they have a remarkably Darwinian sound.

In the first place, the leading tenets of Darwin's work—the struggle for existence,[28] variation, natural selection and sexual selection are all fully expressed in Blyth's paper of 1835.

> It is a general law of nature, [Blyth observed,] for all creatures to propagate the like of themselves: and this extends even to the most trivial minutiae, to the slightest peculiarities; and thus, among ourselves, we see a family likeness transmitted from generation to generation. When two animals are matched together, each remarkable for a certain peculiarity, no matter how trivial, there is also a decided tendency in nature for that peculiarity to *increase*; and if the produce of these animals be set apart, and only those in which the same peculiarity is most apparent, be selected to breed from, the next generation will possess it in a still *more* remarkable degree; and so on, till at length the variety I designate a *breed* is formed, which may be very unlike the original type.[29]

Here, of course, we are dealing with artificial selection—with cattle, pigeons, and other domestic forms. After commenting upon sexual selection in herd animals "so that all the young . . . must have had their origin from one which possessed the maximum of power and physical strength; and which, consequently, in the struggle for existence was the best able to maintain his ground," Blyth proceeds beyond purely artificial selection. He applies the principle of selection to wild nature just as Darwin was later to do. The best organized, the most agile will survive and leave the most progeny. He sees selection in nature, however, as a conservative principle "intended by Providence to keep up the typical qualities of a species." The sickly, the ill-adapted must, Blyth contends, soon disappear. The slightest deviation in the coat color of a cryptically adapted form will cause its discovery and destruction.[30]

On this point Blyth stands in about the same position as certain other eighteenth- and early nineteenth-century writers whom I have had occasion to analyze elsewhere except that he is more conscious of ecological selection.[31] He is cognisant that variant traits can be accumulated by selective breeding under domestic conditions. He is also aware of selection under the struggle for existence in wild nature. But Blyth was primarily a student of living forms. He was a careful observer of the delicate adjustment of life to its surrounding environment. In fact he exaggerated the permanence of these exquisite adaptations which he felt disqualified the animals for any other mode of existence.[32] When circumstances changed, he contended, a species must perish with its locality. Like Lyell somewhat earlier, Blyth had glimpsed the negative aspects of the struggle for existence and the way in which species were eliminated. He failed to see, however, that natural selection was a potentially liberalizing rather than conservative factor in life. This is particularly curious because, unlike his contemporary, Swainson, he remained undeceived by the highly artificial numerical taxonomy which was in popular favor at the time. He recognized that species were always "modified in direct relation to particular localities, or to peculiar modes of procuring sustenance."[33] "Just as the surface varies," Blyth observed, "so do its productions and inhabitants." He made out clearly "the reiterate divergence and ramification" of organic relationships.

Blyth's youthful failure, it can now be realized, lay in his provincialism. Perhaps his species would have remained less fixed if he had had Darwin's experience of the new lands. As it was, Edward Blyth tended to see everything except man's domestic productions assigned to its place and kept there by the inexorable force of natural selection. Species boundaries were carefully demarcated. Blyth saw about him, the hedge-constricted, precision-cultivated English landscape. Darwin had brought back with him the memories of foreign weeds invading the new world, of introduced animals overrunning the indigenous products of oceanic islands.

For Darwin, fresh from the wild lands, the boundaries of life seemed less rigid and sharp.

[28] The actual phrase is used. Blyth may have drawn it from Lyell with whose *Principles of geology* he was acquainted. See Blyth, 1835: 48. The phrase occurs in Blyth, *ibid.*, 46.

[29] *Op. cit.*, 45–46, 1835. The italics are those of Blyth.

[30] Similar expressions occur in the paper of 1837, pp. 79–80, 135.

[31] *Darwin's century*, 53–54, 122, 137, 201–202, 329, New York, Doubleday, 1958.

[32] Blyth, 1837: 83.

[33] Blyth, 1836: 406–407.

Nevertheless, he was to spend a great amount of space in the trial essays and the *Origin* answering the arguments of Edward Blyth. He had to seek a way through the mind block of a particular world view; namely, Providence and special creation. Yet, ironically, it was still Blyth who in 1837 suggested the road which passes beyond the purely negative selection to be found in the *Principles* of Sir Charles Lyell. Even in 1835 Blyth had remarked somewhat cryptically that the study of simple variations "properly followed up . . . might lead to some highly interesting and important results."[34] By 1837 he had grown aware that his "localizing principle" was not absolute, that "when a species increases numerically in any habitat beyond what the latter is adequate to sustain . . . either their ranks are mysteriously thinned by what is termed *epizooty*, or an erratic impulse . . . instinctively prompts a portion of them to seek fresh quarters."[35] Blyth noted the dangers which beset those wanderers who pass out of their locality and he noted the fact that they mostly perish without being able to establish themselves in other places. He was, however, beginning uneasily to sense that if his localizing principle, which is really natural selection "breeding in and in" and transmitting selected individual peculiarities in a single stable environment, were to be broken through by removal of forms to other habitats, unlimited organic change might be possible in wild nature. The situation would then be more comparable to the release offered by man in the controlled breeding of domestic forms.

> A variety of important considerations here crowd upon the mind, [confesses Blyth,] foremost of which is the enquiry, that, as man, by removing species from their appropriate haunts, superinduces changes on their physical constitution and adaptations, to what extent may not the same take place in wild nature, so that, in a few generations distinctive characters may be acquired, such as are recognised as indicative of specific diversity. . . . *May not then, a large proportion of what are considered species have descended from a common parentage?*[36]

After toying with this intriguing possibility, Blyth felt forced to reject it because "were this self-adapting system to prevail . . . we should seek in vain for those constant and invariable distinctions which are found to obtain."[37] Living species, he contended, would under such circumstances blend into each other—something which does not occur in nature. In the plant world sea-water prevents the germination of drifting seeds carried far from their own locality; animals have homing instincts. "Such antagonist principles obtain throughout creation, whether or not human observation may have yet detected their existence."[38] Blyth's world, in other words, is still that of the English hedgerows, of a stability which gives way only to extinction, never to really marked organic alteration. This stability he came very close to fastening upon the young Darwin. Many pages of Darwin's preliminary essays and of the *Origin* itself are spent in dealing with Blyth's defense of the species barrier. Where Lamarck had seen most so-called extinct species evolving by alteration into living ones, Blyth saw all past forms as extinct and without representation in the present. The compromise solution would have to be sought by Darwin.

As one leafs through Blyth's small papers, however, one is amazed by the ideas which reappear in the trial essays of 1842 and 1844 and which Darwin never altered throughout his life. Besides taking note of natural selection, sexual selection, and the role of hereditary variation, Blyth expressed the view that macromutations would "very rarely, if ever, be perpetuated in a state of nature."[39] Darwin was apparently convinced by Blyth's argument and clung to it thereafter.[40] Blyth believed in the influence of food in the stimulation of variations in domestic stock and, again, we find environmental ideas of this kind lingering in Darwin.[41] Blyth's emphasis on sexual selection in man[42] anticipates the weight given to it by Darwin in the *Descent of Man*.

These are general comments. They suggest the germs, expressed briefly, of what grew to be chapters, even books, in the hands of Darwin. Since our thesis is somewhat startling; namely, that Darwin made unacknowledged use of

[34] Blyth, 1835: 53.

[35] Blyth, 1837: 134. Compare Blyth's sentence with the following remark from *The Origin of Species*, 64–65, 2nd ed. Oxford Press, 1935: "When a species, owing to highly favorable circumstances increases inordinately in numbers in a small tract, epidemics—at least this seems generally to occur with our game animals—often ensue. . . ."

[36] *Op. cit.*, 135, 1837. Italics mine.

[37] *Ibid.*, 136.

[38] *Ibid.*, 137. Note, incidently, that Darwin devoted considerable attention to the effects of salt water on seeds. See the *Edinburgh Philosophical Journal*, n.s., **4**: 375–376, 1856.

[39] Blyth, 1835: 47.

[40] See *FO*, 247; *O*, 174–175.

[41] *O*, 7–8, 10; *FO*, 1–2, 14–78.

[42] Blyth, 1835: 49.

Blyth's work, the critical historian may still want to ask what interior evidence, what detailed similarities can be used to establish the fact that Blyth is more than a Darwinian precursor, that he is, instead, a direct intellectual forebearer in a phylogenetic line of descent. Edward Blyth, in the writer's estimation, belongs in the royal line. He is not an isolated accident. Instead, he is one of the forgotten parents of a great classic. But Darwin's shadow, grown to almost superhuman proportions, lies massive and dark across the early portion of the century. How can one find, even in this similarity of ideas, more than the accidental repetition of like thoughts by different men?

At this point let us remind ourselves of one thing: We have proved that *The Magazine of Natural History* was abundantly used by Darwin from the very outset of his career. We have shown from his own words that he was an enormous and voracious reader and compiler. From this let us turn to the interior of the papers themselves. It is well, in doing this, to bear in mind that in essence the principle of natural selection is simple. Once the leading idea is grasped illustrations can be altered, new facts can be substituted until an original connection between one man's paper and the work of another is totally lost. This is particularly true when a book has grown from two or three papers and perhaps only from certain paragraphs within those papers. As I intimated in the introductory pages of this study, what exists in the trial essays of Darwin has been almost completely obliterated in the *Origin*. It was the publication of the essays in 1909 which makes it possible to discern the connection which I now hope to demonstrate. We have already dwelt upon the obsolete word "inosculate" which suddenly appears in Darwin's notebook upon his return from the voyage and coincident with its appearance in the papers of young Blyth. There is, it emerges, an even more remarkable use of words in a similar context and comparable order which challenges any attempt to explain the situation through accidental duplication. The statistical probability of the same peculiar animals with the same anatomical oddities being listed together in the same approximate order by pure chance is so remote as to be almost meaningless.

In Blyth's paper of 1835 occurs a statement involving Ancon sheep with an explanatory description of them. A little later in the same paragraph occurs a list of odd mutations including "donkey-footed swine, tailless cats, back-feathered, five-toed, and rumpless fowls, together with many sorts of dogs. . . ."[43]

This odd little concentration of mutative types is duplicated in almost the same order in Darwin's essay of 1844. Like Blyth, Darwin is discussing "sports" or hereditary monsters. Like him, Darwin mentions Ancon sheep, rumpless fowls, and tailless cats.[44] It is true that the solid ungular swine and five-toed fowls have disappeared but they occur in later pages of Darwin's essay. They have merely been dispersed.[45] In the matter of claws, two pages farther on we encounter the phrase, "breeds, characterized by an extra limb or claw as in certain fowls and dogs." In the *Origin* this curious sequence has vanished though the Ancon sheep is still mentioned.

Blyth, in his discussion of food and its effects on animals, comments that "herbivorous quadrupeds which browse the scanty vegetation on mountains are invariably much smaller than their brethren which crop the luxuriant produce of the plains. . . ."[46] Darwin in turn holds that "external conditions will doubtless influence and modify the results of the most careful selection; it has been found impossible to prevent certain breeds of cattle from degenerating on mountain pastures. . . ."[47]

Blyth, in his discussion of hybridity, recognized dominance (i.e. prepotency) and the possibility of the reemergence of suppressed characteristics in the third generation.[48] Here again Darwin, in the essay of 1844, expresses similar views.[49] The use and disuse concept is brought into play by Blyth in his discussion of domestic forms where "an animal . . . supplied regularly with . . . abundance of food, without the trouble and exertion of having to seek for it . . . becomes, in consequence, bulky and lazy . . . while the muscles of . . . locomotion . . . become rigid and comparatively powerless, or are not developed to their full size."[50] Darwin, in his second essay devotes a section to this

[43] Blyth, 1835: 47. It will be remembered that Darwin, in the Notebook of 1836, took a sudden interest in tailless cats.

[44] *FO*, 59–60.

[45] *Ibid.*, 216–217 for swine; 62 for five-toed fowls.

[46] Blyth, 1835: 43.

[47] *FO*, 64.

[48] Blyth, 1835: 48.

[49] *FO*, 107–108.

[50] Blyth, 1835: 44.

subject and comes once more to similar conclusions which are re-expressed in the *Origin*.[51]

Blyth devotes considerable attention to protective coloration and the utilitarian advantages gained by such devices in the struggle for existence.[52] In the course of this discussion Blyth, in his paper of 1835, quoted from Mudie's *Feathered Tribes of the British Islands* the metaphor "grouse are brown heather." In the *Origin* Darwin utilizes the same descriptive device picturing "the red grouse the colour of heather."[53] Further on in the same section, and this time in his own words, Blyth speaks of the ptarmigan as "snow in winter." In the same section in Darwin "the alpine ptarmigan [is] white in winter."[54] The discussion of protective coloration is more extended in Blyth but both men's views are the same. Blyth has a vivid description of the relation of the falcon to its prey. He dwells at length upon the bird's great powers of sight. In the same Darwinian passage hawks are mentioned as "guided by eyesight to their prey."[55] Like Blyth, Darwin then dwells upon the pruning effect exercised by these carnivorous birds in keeping the cryptic coloration of small mammals and ground-dwelling birds, such as grouse, uniform and constant through natural selection. The variant animal is unable to conceal itself successfully and is thus more subject to destruction. Although Darwin's treatment of the idea is not as lengthy as Blyth's, the descriptive material cited above is powerfully suggestive of a direct connection particularly when it is taken in conjunction with the other evidence we have been at some pains to assemble.[56]

If we turn to Blyth's paper of 1837 it will be recalled that in discussing his localizing principle he touched upon the homing instinct in animals. During the course of his discussion Blyth asserted this capacity was "not wholly absent from the human constitution."[57] He referred to the Australian aborigines and other savages as being subject to this "intuitive impulse." Although Darwin almost totally avoided reference to man in the *Origin* it is of interest to note that the homing instinct in man receives attention in both of the trial essays. It is mentioned briefly in the first essay[58] and twice in the second.[59] Here the Australian savage reappears.

The instinctive shamming of death is also mentioned by Blyth[60] as characteristic of certain animals. Once more Darwin treats of it briefly but critically in the essay of 1842 and again in the lengthier essay of 1844.[61] By subjecting Blyth's papers to extended and minute analysis there is no doubt that a few additional items pointing toward a connection between the two men might be established.[62] Even making some allowance for accidental use of the same sources, the effect is cumulative and, in the present writer's view, unexplainable by chance. Furthermore, there is ample evidence that Blyth's restrictions on divarication beyond the species level troubled Darwin and that he was forced to spend considerable time and ingenuity in finding his way around this barrier. To this effort we will soon turn. In the course of our investigation it will be possible to see more clearly than heretofore why Darwin approached the subject of variation in wild nature with such timidity.

V. BLYTH AND THE CONSERVATIVE ASPECT OF NATURAL SELECTION

In a recent study of evolutionary history the writer had occasion to observe that both Darwin and his colleague Alfred Russel Wallace had been profoundly influenced by the work of Sir Charles Lyell. We may now add to the list of potential evolutionists affected by that great book, *The Principles*, the name of Edward Blyth. In comparing the beliefs of Lyell and Blyth in the eighteen-thirties we will be able to see more clearly how evolutionary thought was slowly inching ahead during a time which, to many writers, has seemed totally devoid of evolutionary significance. It is not surprising that Blyth, like Darwin an eager young scholar, had

[51] *FO*, 91; *O*, 10.
[52] 1835: 51–53; 1837: 80.
[53] *O*, 77.
[54] *Ibid.*
[55] *Ibid.*
[56] One could add smaller items such as Blyth's reference to the variation manifested in the beaks of finch-like birds (1836: 400) which may well have intrigued Darwin through the problem presented by the Galapagos finches which he had collected.
[57] 1837: 138.
[58] *FO*, 19.
[59] *FO*, 119 n. 2; 124–125.
[60] 1837: 5.
[61] *FO*, 19.
[62] For example, Darwin follows Blyth quite explicitly in his essay of 1842 when he says that if in any country or district all animals of one species are allowed freely to cross "any small tendency in them to vary will be constantly counteracted." "Such varieties," Darwin says, "will be constantly demolished" (*FO*, 2–3). Compare Blyth, 1835: 46–47.

been impressed by the great learning and elegant sentences of Charles Lyell. What we shall want to see is the way in which the work of Blyth passed, in one particular, beyond the thought of Lyell on species. In this one act we can see the closing of the gap in thought between Darwin's forerunners and the author of the *Origin* himself. Blyth is the missing key that makes the entire transition so smooth as to be almost imperceptible.

First, let us examine the work of Lyell on species. Like others of the late eighteenth and early nineteenth centuries he recognized the importance of the struggle for existence, actually used the phrase, and realized that this aspect of the natural world accounted for much extinction throughout past time. "The most fertile variety," he observed, "would always in the end, prevail over the most sterile."[63] Yet Lyell, after dismissing Lamarck, failed quite to grasp the creative aspect of this selection. Lyell recognized that there was considerable variation in living forms and he commented on the "extraordinary fact, that the newly acquired peculiarities are faithfully transmitted to the offspring."[64] Nevertheless, Lyell did not believe such deviation to be endless. Rather, his interpretation partakes of the theological outlook of the time. He remarks of domestic animals that "attainments foreign to their natural habits and faculties, may, perhaps have been confirmed with a view to their association with man."[65] He contended that the organization of plants and animals was never absolutely constant but that the plasticity of a given species was limited.

> We must suppose [argued Lyell] that, when the Author of Nature creates an animal or plant, all the possible circumstances in which its descendants are destined to live are foreseen, and that an organization is conferred upon it which will enable the species to perpetuate itself and survive under all the varying circumstances to which it must be inevitably exposed.[66]

The range of variation, Lyell thought, would differ somewhat according to whether the animal had been created to occupy a widely varying environment or a narrowly constricted and uniform one. Thus, the great geologist recognized the possibility that the varieties of a species might diverge in distinct environments or under human selection, but he felt that the preponderance of the evidence favored the view that such divergence was limited in scope—a simple adjustment to insure survival. Recognizing the fact that Blyth had read Lyell, let us now see in what manner Blyth approached the same problem.

First of all it may be quickly observed that although Blyth, too, agreed that there was a limit to divergence he handled the whole subject with greater clarity and precision than Lyell. Instead of confining himself to vague references as to the effects of climate, temperature, and other similar factors, in determining organic change, Blyth wrestles directly with the genetics of the problem:

> There would almost seem, in some species, to be a tendency, in every separate family, to some particular kind of deviation; which is only counteracted by the various crossings which, in a state of nature, must take place, and by the . . . law which causes each race to be chiefly propagated by the most typical and perfect individuals.[67]

Blyth, long before Darwin had expressed himself on the same subject, had clearly recognized the analogy between artificial and natural selection. He had seen the latter process, however, as a conservative rather than a liberalizing factor. "The original form of a species is *unquestionably* better adapted to its *natural* habits than any modification of that form,"[68] he asserted, for he saw natural selection as pruning out the least deviation which threatened to unfit the animal for its environment. As a neozoologist Blyth had a sharp eye for the incessant variation in nature, but he was also intensely aware of how frequently the deviant form is destroyed. He saw the plunge of the hawk on the animal whose cryptic coloring was imperfect, he observed what happened to creatures who strayed beyond their normal environment. He saw in his mind's eye a world held in a tight dynamic balance. It was a world that might move but could not:

[63] *PG* 2: 364.
[64] *Ibid.*, 345–346.
[65] *PG* 2: 375.
[66] *Ibid.*, 351.

[67] Blyth, 1835: 46. In Darwin's autobiography he states that his interest in the cross fertilization of flowers by insects, begun in 1838, arose through his belief that "crossing played an important part *in keeping specific forms constant.*" It is worth noting that here he is treating of the conservative aspect of selection just as it was handled by Blyth in the paragraph cited above.
[68] *Ibid.* The italics are Blyth's.

It would be easy to point out additional hindrances to the more extensive spread of species of fixed habit, by treating on the fraction which are allowed to attain maturity, even in their normal habitat, of the multitude of germs which are annually produced; and in what ratio the causes which prevent the numerical increase of a species in its indigenous locality would act where its adaptations are not in strict accordance will sufficiently appear, on considering the exquisite perfection of those of the races with which it would have to contend.[69]

Contained in this remark is a full recognition of the Malthusian struggle of population against natural resources, but Blyth felt the cards to be stacked against extended change. It was not an unreasonable position. Species, Blyth had observed, faced extreme difficulties when they wandered out of their normal range.

Were this self-adapting system [i.e. evolution] to prevail to any extent, we should in vain seek for those constant and invariable distinctions which are found to obtain. Instead of a species becoming gradually less numerous where its haunts grade imperceptibly away, we should discover a corresponding gradation in its adaptations. . . .[70]

Now as a modern student of evolution has recently observed,

by a paradox the process of natural selection has, over short periods, a conserving effect rather than a modifying one. In a constant environment the great majority of the individuals of a species are very precisely fitted to their habitat, and almost any change from the typical will be a disadvantage: consequently many variant forms are eliminated before they can reproduce. . . .[71]

It was this fact which Blyth had sensed so perspicaciously, but at the same time he had failed to see what lay beyond. His failure was partly the result of inexperience, partly the product of the theological atmosphere of the time. This is characteristically revealed in one of his remarks of 1835 about natural selection. "The same law," he writes, "which was intended by Providence to keep up the typical qualities of a species, can be easily converted by man into a means of raising different varieties."[72]

It should now be apparent why the young evolutionist, Charles Darwin, after discovering Blyth upon his return home from the voyage of the *Beagle*, began momentarily to contemplate the possibility that if "distinct species inosculate[73] so we must believe ancient ones [did] ∴ not *gradual* change or degeneration from circumstances, if one species does change into another it must be per saltum—or species may perish."[74] Yet with his South American experience before him we see, on the next page, that he is trying to grope around the difficulty raised by Blyth. "Dogs, Cats, Horses, Cattle, Goats, Asses," he persists, "have all run wild and bred, no doubt with perfect success. Showing how creation does not bear upon solely adaptation of animals."[75] It is evident here that he is considering the possibility that animals can respond to change, that Blyth's "localizing principle" is not absolute. "Nothing quite makes sense," writes Darwin's granddaughter, Nora Barlow, of this point in Darwin's career.

One can observe that the reason for this feeling that Darwin is groping lies in the fact that he is trying to find a way around the obstacle raised in Blyth's first paper—the fact that there are distinct breaks between the modern species rather than indefinite passages between them. Lyell, in addition, had contributed to this difficulty paleontologically by his contention that,

where a capacity is given to individuals to adapt themselves to new circumstances, it does not generally require a very long period for its development;

[69] Blyth, 1837: 137.

[70] 1837: 136.

[71] Barnett S. A. (ed.), *A century of Darwin*, xiii, London, Heinemann, 1958. W. L. Brown, Jr. has treated another aspect of the break between modern species (character displacement) in several papers. For a discussion and bibliography see his: Some zoological concepts applied to problems in evolution of the hominid lineage, *Amer. Scientist* **46**: 151–158, 1958.

[72] Blyth, 1835: 46. See also 1837: 80.

[73] The term appears to be used by Blyth and Darwin in the sense of adjoining, not blending.

[74] *N*, 263.

[75] As early as the first edition of the *Journal of Researches* (1839), we can observe Darwin's thought playing over this problem of the successful intrusion of foreign types of animals. "I will add," he says at the close of Chapter IX, "one other remark. We see that whole series of animals, which have been created with peculiar kinds of organization, are confined to certain areas; and we can hardly suppose these structures are only adaptations to peculiarities of climate or country; for otherwise, animals belonging to a distinct type, and introduced by man, would not succeed so admirably, even to the extermination of the aborigines. On such grounds it does not seem a necessary conclusion, that the extinction of species, more than their creation, should exclusively depend on the nature (altered by physical changes) of their country" (*JR*, 212). Darwin is here already attempting in a subterranean fashion to find a way past the fixity of Blyth's totally adapted world.

if, indeed, such were the case, it is not easy to see how the modification would answer the ends proposed, for all the individuals would die before new qualities, habits or instincts were conferred.[76]

Blyth had presented Darwin with a possible mechanism of potentially indefinite organic change but had at the same time labelled it as a conservative mechanism ordained by Providence to maintain the stability of species. In 1837, however, as we have previously noted, Blyth showed a few signs of doubt, hastily suppressed, upon this notion. It is conceivable that these doubts did not pass unnoticed by the widely traveled Darwin who had seen old world forms, intrusive in the New World, extirpating or successfully competing with forms supposedly created for a specific habitat. Moreover, Blyth's point of view was indirectly challenged in 1838 by W. D. Weissenborn who pointed out that numerous species were flourishing in habitats where they were not originally indigenous.[77] The work of the voyager naturalists was making ever more apparent the facts of animal and plant movement and adaptation. Blyth's hedgerow world was in the process of dissolution.

VI. DARWIN AND MALTHUS RE-EXAMINED

According to Darwin, his recognition of the principle of natural selection came in October of 1838 when he chanced to read Thomas Malthus on population and perceived that the geometric increase of living forms would create a struggle in nature which would in turn promote the survival of advantageous variations. Actually, however, this statement, in the light of the facts we have previously recounted, is open to some doubt even though it may well be that Darwin was additionally stimulated by reading Malthus. In the first place, it was not necessary for Darwin to read Malthus in order to realize the intensity of the struggle for existence. Leaving aside Blyth's contribution, mention of it occurs in the writings of Darwin's grandfather Erasmus, in Paley's *Natural Theology*, and in the *Principles of Geology* by Sir Charles Lyell. Even Lamarck mentions it in the *Philosophie Zoologique*. All of these works were read when Darwin was young and impressionable. His own son, Francis, expressed surprise that he should have had to turn to Malthus for inspiration.[78] Furthermore, Francis pointed out what we know to be true, that in 1837 he had already given vent to the essential aspects of the principle.[79]

There are, it is true, a few references to Malthus in the trial essays before the *Origin* but not to the exclusion of other workers such as de Candolle. Oddly enough, although Darwin confided his great secret to a few of his intimates, he seems to have placed little emphasis upon Malthus. One reads with surprise a letter from Hooker to Darwin written in January of 1863. "Did you ever read that painful book, Malthus on Population?" Hooker writes. "I did the other day and was painfully impressed by it."[80] If one turns back to Darwin's letters of the eighteen-forties, one gets the same impression of neglect. Though Darwin wrote often to Hooker, Gray, and Jenyns about his work, and about the struggle for existence, Malthus remains unmentioned.[81] In fact, the indices to the five volumes of published letters record no reference in the first three, save for the single remark in Darwin's autobiography. The following two volumes contain only three brief references and two of these are directed to Alfred Russel Wallace.

My colleague and former student, Dr. Gerald Henderson of Brooklyn College, has raised an interesting point in this connection. He maintains that *after* Darwin had received Wallace's

[76] *PG* 2: 369.

[77] Weissenborn, W. D., On the influence of man in modifying the zoological features of the globe, etc., etc., *MNH*, n.s., 2: 13–18; 65–70; 122–128; 239–256, 1838.

[78] *FO*, xvi. The remarks quoted by Francis from Charles' notebook take on an added clarity when read in the light of Blyth's work.

[79] A second obstacle to a complete acceptance of Darwin's statement lies in an unnoted half-discrepancy of the *Autobiography* itself. Darwin says he read Malthus in October of 1838 and at this point glimpsed how the principle of artificial selection could be transformed into natural selection in wild nature. Yet farther on in Darwin's own reminiscences (*A*, 127) he tells us that he was studying the cross fertilization of flowers with an eye to the species problem as early as the *summer* of 1838. As we noted on p. 105 this same interest in the conservative aspects of inter-crossing is expressed by Blyth. It seems a little unlikely that if Darwin first grasped the Malthusian principle in the fall of 1838 he would have been so intensively occupied with the conservative aspects of crossing in the summer of the same year unless his thought on natural selection was already well advanced. Rather it suggests, once more, that Darwin was engaged in seeking a way through this obstacle to divergence observed by Blyth and of which Darwin was already aware through a perusal of Blyth's paper.

[80] Huxley, Leonard, *Life and letters of Sir Joseph Dalton Hooker* 2: 43, London, John Murray, 1918.

[81] See, for example, his letters to Jenyns in 1845, *LLD* 2: 31–32, 33–35.

sketch of 1858 and preparations were made for the joint papers to be given before the Linnean Society, the passage from the essay of 1844 which was selected by Hooker, Lyell, and Darwin to be incorporated into Darwin's announcement was the only one in which Malthus was mentioned. Dr. Henderson points out that the famous letter to Gray of 1857 contains no reference to Malthus. Instead, de Candolle, Lyell, and Herbert are extolled as Darwin's authorities. It is Dr. Henderson's considered opinion that the passage referring to Malthus was deliberately chosen for presentation to the Linnean Society "because of its correspondence with the subject matter of Wallace's essay."[82] From the time of Wallace's appearance on the scene, Dr. Henderson contends, the significance of Malthus began to bulk larger in Darwin's public declarations about the origin of his views on species.

Some time ago I pointed out that to have referred to Lyell, for example, as the direct source of one's ideas upon evolutionary struggle in nature would have been to quote a man publicly opposed to evolution in support of that doctrine. Since Malthus was active in a quite different field, and was, in addition, the basic source of much of the thinking on the struggle for existence in early nineteenth-century England, it was convenient to have recourse to him as the "authority."[83] It is possible that Darwin found it easy to fall in with Wallace's use of Malthus partly because a natural rivalry dictated his desire to show he was just as aware of Malthus as was Wallace. Furthermore, this curious chain of events was obscuring ever more deeply the real origin of Darwin's evolutionary system. Some of Darwin's hesitations, long delays over publishing, and almost neurotic anxiety can now perhaps be better understood. He had his secrets, and, as I hope to show a little later, he had his justification for them.

VII. BREAKING THE SPECIES BARRIER

We have previously observed that the main difference between Blyth and Darwin lies in the fact that one was a special creationist and the other an evolutionist. The thing which strikes us as unusual in this connection is that both men were reasoning from the same principle: natural selection. Blyth, however, had discovered its short-time stabilizing effects. He was, as we have seen, still laboring under the theological aspects of the argument from design. His emphasis is largely upon natural selection as a providential localizing principle confining animals and plants to their proper sphere of activity—the place for which they were created. Only man, through the intelligent use of artificial selection, is in a position to multiply varieties and perpetuate peculiar forms.

Although Blyth, like Lyell, appears to have regarded the changing environments of the past as resulting simply in extermination[84] he does hint cryptically in a paper of 1838 that the only evidence for the continued existence of the "creative energy" lies in the "results afforded by the study of fossil remains."[85] Darwin, calling upon his extended South American experience, was not impressed by the hedgerow-everything-ordained-to-its-own-place philosophy of Blyth. Taking such hints as were provided by his own experience and the timid insights which Blyth failed to pursue, Darwin denied that everything in the animal world must perish with its locality. For a time he choked, as we have seen, upon Blyth and Lyell's argument that adjustment would come too slowly to fit an animal to survive in an environment for which it might not, in the beginning, be as well fitted as the indigenous occupants of the ground.[86] For this reason he considered briefly the possibility of macro-mutative leaps as a way through this particular difficulty—adjustment, in other words, of a more instantaneous variety. He soon saw, however, that this was both scientifically dubious and unnecessary. From the time of the first essay he begins to answer the question of how a transitional form can subsist until its

[82] Henderson, Gerald, *Alfred Russel Wallace: his role and influence in nineteenth century evolutionary thought*, Doctoral Dissertation, unpublished, University of Pennsylvania, Philadelphia, 1958, p. 54.

[83] *Darwin's century*, 182 n. 17, New York, Doubleday, 1958.

[84] 1837: 82.

[85] On the doctrine of spontaneous organization, *MNH*, n.s., 2: 508–509, 1838.

[86] Interesting in this connection is a letter from Lyell to A. R. Wallace written in 1867. Lyell says: "When I first wrote, thirty-five years ago, I attached great importance to preoccupancy, and fancied that a body of indigenous plants already fitted for every available station would prevent an invader, especially from a quite foreign province, from having a chance of making good his settlement in a new country. But Darwin and Hooker contend that continental species which have been improved by a keen and wide competition are most frequently victorious over an insular or more limited flora and fauna." Marchant, James, *Alfred Russel Wallace: letters and reminiscences*, 278, New York, Harpers, 1916.

adaptations have been perfected. "I would rather trust . . . pure geological evidence than either zoological or botanical evidence,"[87] Darwin once remarked to Hooker.

It is here that we see him taking boldly to a course at which Blyth only hinted. For Darwin altered Blyth's scheme by openly attaching to it Lyell's infinite time and changing geological conditions. To it he added the view that the species barrier was an illusion created by the imperfections of the geological record and the shortness of human life. Organic change occurred at a rate which was imperceptible. As a matter of fact, men tended to forget even their transitional domestic varieties once a new breed became established. "To be brief," Darwin wrote to Asa Gray in 1856, "I *assume* that species arise like our domestic varieties with much extinction."[88]

Long before this, however, in the first essay of 1842, we can see him marshalling his arguments for the purpose of breaking through Blyth's conservative "localising" system. In summary form his points can be stated as follows:

(1) The conquest of an indigenous fauna by introduced organisms "shows that the indigenes were not perfectly adapted,"[89] that is, there is no such thing as an animal providentially ordained to occupy a given environment. Adaptation is, and will always be, only a relatively successful achievement often broken in upon and destroyed. Thus there is no real barrier to continued change.[90]

(2) Oscillations of sea-level on islands and continents creates shifting conditions—isolation, faunal migrations, etc. This creates opportunities for *new* selection of *new* characters, not just the *conserving* selection of Blyth.[91]

(3) New conditions, Darwin maintained, increase the tendency for mutations to appear. This happens under wild conditions in a form analogous to what Blyth contended was the case among domestic animals.[92] Similarly migrations of faunas and floras into new environments would promote mutation.

(4) Darwin argued cogently that *no* environment is completely static and therefore renewed selection is going on even under superficially uniform conditions.[93]

(5) Darwin invokes a law of succession to explain why the living organisms of a continental area ordinarily show the closest affinities to the extinct life of the same region. This anatomical relationship between the past and the present is only explainable on the basis of evolution.[94]

(6) Finally, in *The Origin of Species*, Darwin devotes an entire section to the formidable point raised by Blyth as to the rarity or absence of transitional varieties between the various modern faunas. It was Blyth's argument, based on modern observation, that transitional varieties could not have subsisted. Referring to his "opponents" with deliberate vagueness in Chapter 6 of the *Origin*, Darwin confesses, just as we had deduced previously from our examination of Blyth and the notebooks of 1836, that "this difficulty for a long time quite confounded me."[95] Nevertheless, he came to believe the difficulty could be explained. We tend, Darwin argued, to think too much in terms of recent climate and geographical gradation and then expect life to grade itself as imperceptibly. Actually life is historical. It may intrude into new areas. Species compete and sharply limit each other's expansion. Intermediate varieties, therefore, tend frequently to contract their range and disappear. Thus Darwin, after long study and analysis, broke through the seemingly durable species-barrier which Blyth had, paradoxically, created upon the basis of natural selection.

All of these points and more were, by degrees, shaped into the final perfected argument of *The Origin of Species*. Natural selection, as we now can see, was used first as a conservative teleological device for maintaining the stability of the natural world. This was its primary function in the hands of Edward Blyth. Charles Darwin, the vast synthesizer, the perceptive coordinator of materials from diverse sciences, remains the massive figure he has always been.

One last question remains to be considered before we turn to the forces which, in some degree, may have motivated Darwin's behavior. We have observed that he chose to ignore publicly the papers of Edward Blyth. Is it possible that the man who once wrote "all my notions about *how* species change are derived from long-continued study of the works of . . . agriculturists and horticulturists,"[96] could have

[87] *LLD* 1: 358.
[88] *LLD* 2: 78.
[89] *FO*, 34 n. 1. Compare with Blyth, 1837: 80.
[90] Later, in a letter to Lyell, Darwin on the eve of the *Origin*, gave forceful expression to this new historical point of view. He says "As each species is improved, and as the number of forms will have increased, if we look to the whole course of time, the organic condition of life for other forms will become more complex, and there will be a necessity for other forms to become improved, or they will be exterminated; and I can see no limit to this process. . . " (*LLD* 2: 177).
[91] *FO*, 30, 35. See also *LLD* 2: 209, 259.
[92] *FO*, 15, 91. Blyth, 1835: 44–45.
[93] *FO*, 90–91.
[94] *FO*, 33.
[95] *O*, 156.
[96] *LLD* 2: 78.

been secretly aware of any other precursors? After the Blyth episode one cannot help but wonder. On page 78 of the 1844 trial essay on the road to the *Origin* occurs the following observation:

> In the case of forest trees raised in nurseries, which vary more than the same trees do in their aboriginal forests, the cause would seem to lie in their not having to struggle against other trees and weeds, which in their natural state doubtless would limit the conditions of their existence.

When one turns to the *Origin* itself one finds that this statement has vanished. In Patrick Matthews' book on *Naval Timber and Arboriculture*, which contains a full anticipation of natural selection, occurs, however, this intriguing remark:

> Man's interference, by preventing this natural process of selection among plants, independent of the wider range of circumstances to which he introduces them, has increased the difference in varieties particularly in the more domesticated kinds.[97]

Darwin's comment, in other words, sounds as though based on Matthew—at least I am unaware of any other convenient source. So strongly did I feel this to be the case that, upon encountering Matthew's statement, I looked into the *Variation of Animals and Plants Under Domestication*, curious as to what one might find there. By 1868 Matthew's book had been publicly called to Darwin's attention. There was thus no reason why he should not refer to *Naval Timber and Arboriculture*. In the second volume of Darwin's work one comes immediately upon the following reference under "Matthew":

> Our common forest-trees are very variable, as may be seen in every extensive nursery-ground; but as they are not valued like fruit-trees, and as they seed late in life, no selection has been applied to them; consequently, as Mr. Patrick Matthews remarks, they have not yielded different races. . . .[98]

This statement appears to be a shortened version of the 1844 comment placed in a new setting with some additional comment from another page of Matthew added.[99]

The reemergence of this discussion of the variability of forest trees in nurseries suggests that Darwin *was* aware of Matthew by 1844. The fact that Darwin, as we have seen, speaks forcefully of his long and persistent searching of horticultural and agricultural sources makes it less easy to accept his ingenuous disclaimer that "one may be excused in not having discovered the fact [i.e. natural selection] in a work on Naval Timber."[100]

Another intriguing, if small point, arises in connection with Matthew. It involves the simple question of where Darwin got the idea for his phrase "natural selection." Though easy to appreciate after its appearance such phrases are deceptive in that they often emerge from at least some similar expression, rather than being totally original. Blyth does not use the expression nor, seemingly, with one exception, does it occur in Darwin's essay of 1842.[101] It does appear in the second essay of 1844. The nearest thing to Darwin's usage occurs in the passage from Matthew which I have already quoted referring to trees. In it Matthew speaks of "*this natural process of selection.*" Darwin uses the expression "natural means of selection,"[102] in the essay of 1842 and utilizes the same phrase in the title of Chapter II in the essay of 1844. Later in the text, however, the term appears as we know it today. The similarity of Matthew's phrase is intriguing, particularly in the light of the tree reference, but cannot, perhaps, be regarded by itself as totally conclusive evidence of connection.[103] Taken in conjunction with our

[97] Matthew, *op. cit.*, 308.

[98] *VAP*, 287.

[99] Matthew, *op. cit.*, 107. This is the page reference given by Darwin which covers part of his comment but not all. To get the part referring to the variability of the trees one must go back to my reference on p. 308 of Matthew.

[100] *LLD* 2: 301.

[101] *The foundations of the origin of species* is deceptive because, as Francis Darwin explains in the preface, he added subheads to the first essay in order to prepare the rough draft for publication. I can find no trace of "natural selection" in the text. It occurs only as a subhead added long afterward by Darwin's son and in one scrap added on the back of a page. This may be later than the text. See *FO*, xxi–xxii and p. 44 n. 4.

[102] Matthew (p. 387) also uses the phrase "selection by the law of nature."

[103] One curious little episode took place as the *Origin* was about to be published. Apparently Murray, Darwin's publisher, had raised some question about the use of the words "natural selection" in the full title. Darwin wrote to Lyell "Why I like the term is that it is constantly used in all works on breeding, and I am surprised that it is not familiar to Murray; but I have so long studied such works that I have ceased to be a competent judge . . ." (*LLD* 2: 153). This odd little statement cannot mean what it appears to mean or Darwin would actually be denying his own originality. The word "selection" was in common use among breeders but, so far as I have been able to ascertain, none before Darwin were using the expression "natural" selection. Darwin himself was to define the phrase in the *Origin* with no such accompanying renuncia-

other material, however, there is no doubt that Matthew deserves more extended attention than he has received.

VIII. DARWIN'S DILEMMA

We have now come, through a very tedious and detailed effort, to a question of considerable magnitude. Why did Darwin, who, after all, is a figure of such stature in science that the sources of his information need not detract from his great synthesizing achievement, feel impelled toward such secretive behavior? Many pages of his biographies are devoted to his magnanimity, his friendliness, his lack of pretense. On the other hand, it is well known that he had his moments of indifference toward his forerunners.[104] He was capable of saying in his autobiography that he had never encountered a single naturalist who entertained doubts on the permanence of species, while in a letter to Hooker in 1847 he had commented jovially, "I see you have introduced several sentences against us transmutationists."[105]

Attempts to explain some of these paradoxes of character have been legion. There have been also the complications introduced through the unconscious process of myth-making, the desire, in other words, to keep this man and his discovery inviolate—a unique act of genius without precedent and without precursive steps. There has thus arisen a tendency to see Darwin's forerunners as having no relationship to his own accomplishment. They are dismissed, as Darwin was inclined to dismiss his own grandfather, as "part of the history of error," as speculative, as lacking in facts. The irony in this situation lies, of course, in the fact that many of the ideas Darwin was later to use came from the researches of these very men. Lamarck, for example, observed the struggle for existence and recognized the significance of vestigial organs before Darwin. Are we to say, because Darwin postdates the earlier workers and had more information available to him, that their observations only became facts in his hands? It has been contended that Darwin sometimes thought in this fashion but he had the grace to confess that he had little feeling for history.[106] There is no reason one hundred years later to embrace the same fallacy.

Charles Darwin, like every other worker in the field of science, used the knowledge and the accumulated stores of information of his predecessors. To their efforts he added his own vast resources and the originality of a powerful, far-reaching mind. R. Taton, in his stimulating book, *Reason and Chance in Scientific Discovery* (1957), points out that new discoveries, particularly where some degree of synthesis is involved, are often made by one who has knowledge derived from more than one field of thought. Certainly this is true in the case of Charles Darwin. He took the providential "localizing principle" of a neozoologist like Blyth and added to it the infinity of geological change. He drew also, from the analogy of domestic breeding, the fact that change was potentially endless and, given time, could carry organisms far beyond the restricted habitats to which Blyth and his fellow naturalists had confined them.

Darwin's solution, in essence, was merely another way of looking at the world from the same set of data, but it was the dispassionate observation of a man on a height to which no

tion of originality as that quoted above. Furthermore, he was later to complain frequently of the public's failure to understand it. See, for example, *LLD* **2**: 317–318; also *MLD* **1**: 160–161. It would appear that in the haste of composition he allowed the pronoun "it" to stand for "natural selection" when what he intended to designate was "selection."

[104] It is interesting to note that Lyell caught Darwin up in the proofs of the first edition of the *Origin* when he was about to ignore Lamarck and St. Hilaire, as evolutionary forerunners (*LLD* **2**: 207). Darwin himself omitted Wallace "by inadvertence" from the final summary of the first edition of the *Origin*. Although this was remedied in later editions Wallace's name was never afterward in any edition indexed for this particular spot. Doubtless these acts were unconscious and not deliberate but they have a certain psychological interest and consistency. See *LLD* **2**: 264. Of Lamarck's book he wrote to Lyell in October 1859: "It appeared to me extremely poor; I got not a single fact or idea from it" (*LLD* **2**: 215).

On another occasion in 1859, shortly after the *Origin* was published, Darwin remarked to Hooker, "I have always had a strong feeling no one had better defend his own priority. I cannot say that I am as indifferent to the subject as I ought to be . . ." (*LLD* **2**: 252).

[105] *LLD* **1**: 355. For autobiography statement see *A*, 124.

[106] He writes, in a letter to John Morley in March of 1871, "I believe your criticism is quite just about my deficient historic spirit, for I am aware of my ignorance in this line "(*MLD* **1**: 326).

Other evidences of Darwin's historical indifference, even to the letters of his most distinguished colleagues, are to be found in his son Francis' account of his filing habits: "when his slender stock of files was exhausted, he would burn the letters of several years. . . . This process . . . destroyed nearly all letters received before 1862" (*LLD* **1**: v). In this manner one of the most important letters Hooker ever wrote to Darwin has perished. See Baker, J. R., A critique of materialism, *The Hibbert Journal* **45**: 32, 1936.

one else had climbed. Nevertheless, Darwin himself once spoke appreciatively of the contributions of the less gifted workers in the field of science. Here again one catches a momentary glimpse of Darwin's ambivalent psychological behavior—his curt dismissal of those who had come close to his pet theory, and yet again his remorseful praise of the "little man" in science.[107]

Charles Darwin, often depicted as a simple, forthright man, was, in reality an enormously complex human being. Consider, for example, the unconscious irony to be found in these remarks to Lyell in 1860: "I have had a letter from poor Blyth of Calcutta, who is much disappointed at hearing Lord Canning will not grant any money. . . . Blyth says (and he is in many respects a very good judge) that his ideas on species are quite revolutionized. . . ."[108] It is within the parenthesis that the irony lies, but it may be suspected that Darwin was only speaking what to him was an experienced truth.

Irrespective, however, of various recent attempts to psychoanalyze Darwin and to consider the psychosomatic aspects of his long illness—his reluctance to meet the public, his disturbing stomach symptoms, his anxieties—there still remain the social imponderables of the time. The French Revolution had had a disastrous effect on English science. One can catch echoes of this conservative reaction in the writers of the period. Alexander Maxwell in his *Plurality of Worlds* (1817) speaks of the "infidel notions and absurdities which abound in Buffon, Hutton, Playfair and many of the French writers upon the same subject."[109] He castigates "astronomical fable" and "visionary speculation." Similarly, the zoologist, William Swainson, speaks critically of what is actually the evolutionary approach. "A cold, ill-concealed spirit of materialism, or an open and daring avowal of wild theories, not more impious than they are absurd, attest . . . the infidelity that attaches to some of the greatest names in modern zoology which France, or indeed any other country has produced."[110]

Evolution was suspect as French atheism. English naturalists discussing the species question disavowed Lamarck with the ritual regularity of communists abjuring deviationist tendencies today. From this standpoint it is likely that the eagerness of Darwin to disclaim any assistance from Lamarck was little more than the common English reaction of the time, however ungracious it may now appear. It would seem that the subject of evolution itself offered difficulties to a man engaged in reopening what was regarded by the majority as a closed episode in biology. Darwin *was* confronted by a genuinely unusual problem. The mechanism, natural selection, by which he hoped to prove the reality of evolution, had been written about most intelligently by a nonevolutionist. Geology, the time world which it was necessary to attach to natural selection in order to produce the mechanism of organic change, had been beautifully written upon by a man who had publicly repudiated the evolutionary position.

Here was an intellectual climate in which men were violently opposed to evolution as godless and immoral. Here was the germ of the idea itself, the struggle for existence, regarded as a mere pruning device for keeping species up to par. "The most important views," Darwin reminisced in his old age, "are often neglected unless they are urged and re-urged."[111]

Sometime very soon after his return from the voyage of the *Beagle* Darwin apparently made a decision: the evolutionary idea would have to be launched again as something totally new and it would have to be launched with a massive accumulation of demonstrable facts to bolster it. In the atmosphere of that time, to have footnoted one's ideas as derived either from French sources or from men who had already taken a different stand on the same evidence would have led to little but embarrassment. Darwin settled down to a long period of fruitful labor and watchful waiting. His reputation as a sound, conservative naturalist was growing. He watched the critical attack on Chamber's *Vestiges of the Natural History of Creation*—an attack made more ferocious by religious bigotry.[112] He watched and was silent while Huxley vio-

[107] Writing to W. Graham in 1881 Darwin said: "I think I could make somewhat of a case against the enormous importance which you attribute to our greatest men; I have been accustomed to think second, third, and fourth rate men of very high importance, at least in the case of Science" (*LLD* 1: 316).

[108] *LLD* 2: 315–316.

[109] P. 178.

[110] *The study of natural history*, 88, London, 1834.

[111] *MLD* 2: 147.

[112] Darwin's concern over the intellectual climate of the times can be glimpsed in a letter to Gray as late as 1857. "You will perhaps think it paltry of me, when I ask you not to mention my doctrine; the reason is, if anyone, like the author of the '*Vestiges*' were to hear of them, he might easily work them in, and then I should have to quote from

lently condemned Chamber's book. Throughout the whole time his own huge treatise was growing. Darwin had an almost pathological patience. In the end it came close to destroying him. Only the fact that Alfred Russel Wallace chanced to send his own discovery to Darwin instead of to a journal prevented his anticipation of *The Origin of Species*.

If one considers this peculiar set of circumstances one can realize, objectively, that these conditions are not those of today and should not, perhaps, be judged by today's standards. Indeed, one observer put the matter quite succinctly in Darwin's final year. Speaking of Darwin's predecessors he wrote,

> To have relied in any way on their authority when Mr. Darwin's book was first published might well have increased the mountain of prejudice against his views without in any way relieving the weight of ridicule that lay upon theirs. When the whole scientific world had been stirred to its foundations and when the whole world almost had been roused into paying attention to science . . . then when it could best be done, Mr. Darwin turned ridicule into renown, and made all who could even remotely claim to have anticipated or shared his views participators of his fame.[113]

This comment by a contemporary who apparently sensed some of Darwin's difficulties is interesting and pertinent but certainly excessive in its praise of Darwin's late atonement to his forerunners. In the first place, Darwin's gesture is meagre and confused, as Professor C. D. Darlington pointed out some years ago when he commented that, "There are many, like Bateson, who complained at least privately, of Darwin's disregard of historical propriety or historical knowledge."[114] There is every indication that Darwin found the historical introduction which was included in the *Origin* a painful task. It would seem that beneath the political necessities which undoubtedly had early contributed to Darwin's reluctance to review his forerunners, there was a genuine and understandable hunger to possess the theory as totally his own.

From a tolerance of what he used to term "speculative men," from a willingness to call himself "a gambler who loved a wild experiment," Darwin, as he became by degrees a British institution, appears to have sobered into what his granddaughter, Nora Barlow, observes as "his repudiation of those who spin their theories without the constant discipline of factual detail."[115] In this attitude, it has been contended, has lain much of his rejection of his forerunners.

It is true that the *Origin* was originally conceived to be an abstract of a larger work and that Darwin wrote to at least one correspondent advising him not to expect footnotes.[116] One can observe, however, that, if there had been any pressing desire on the part of the master to remedy this situation, it could easily have been done in later editions. One can only conclude that Darwin was solitary and elusive beyond even what his family has recorded. "This sense of solitariness," wrote Charles Cox just fifty years ago, "followed him to the end of his life and was, no doubt, an important factor in the formation and preservation of his extraordinary individuality and faith in his own powers."[117]

As one looks back over the curious and intertwined history of Darwin and his associates one is struck more and more by the smooth unbroken evolution of the natural selection concept from the time of the eighteenth-century writers onward. An enormous body of myth has obscured this process. In this hundredth anniversary year since the publication of the *Origin*, the number of eulogies, addresses, and similar encomiums is burying ever deeper the true story of the past. The flash of genius, the master stroke, arouse our pride in human achievement. Few of us want to learn that many less fortunate men toiled to erect the edifice later to be known as Darwinism. Fewer still will accept the fact that Darwin was a human man among men who yearned hungrily for the approbation of the world perhaps, as has been intimated by psychologists, as a compensation for the doubts of a

a work thoroughly despised by naturalists, and this would greatly injure any chance of my views being received by those alone whose opinions I value" (*LLD* **2**: 122).

[113] Stebbing, T. R. R., Letter regarding Butler's contention that Darwin slighted the older evolutionists, *Nature* **23**: 336, 1881.

[114] Purpose and particles in the study of heredity, in *Science, Medicine and History*, ed. by E. A. Underwood, **2**: 74, Oxford University Press, 1953.

Jacques Barzun, in his well-known study *Darwin, Marx, Wagner*, 2nd rev. ed., New York, 1958, expressed similar views (p. 18) and quotes H. F. Osborn (p. 52) as saying that Darwin "owed far more to the past than is generally believed or than he himself was conscious of. . . ."

[115] *A*, 219.

[116] "You must remember that I am now publishing an abstract, and I give no references" (*MLD* **1**: 118). The letter was written to Wallace in April of 1859.

[117] The individuality of Charles Darwin, *Popular Science Monthly* **74**: 345–346, 1909.

domineering father.[118] Yet it is this man, not the bearded idol, who is one of us if we would but see it.

Charles Darwin was an explorer who found no answer for the questions that hounded him throughout the five years of a great voyage.[119] Upon his return to London he combed journals with incredible tenacity until he found what he was seeking. In doing so he broke through the invisible barrier before which other men—even Blyth—had hesitated, the fixity of living forms. Once, in a letter to Hooker, he commented that it was a pity that scientific men read so little. "I have often thought," he insisted, "that science would progress more if there was more reading."[120] This was the man, the plodding man with the enormous unyielding patience and uncanny insight, who piled up folders of evidence until the world capitulated. In Darwin the boy in every one of us is vindicated. Perhaps that has been part of our secret, if confused, adulation. For Darwin, we thought, was the youth who failed at school and ran away to sea only to return in triumph with the secrets of Sinbad's cavern.

This dream need not die because the sailor runaway was helped by others, or because he picked up doubloons of gold secreted in the treasure chests of unread journals. Rather, it should be a lesson to all of us. Sir James Paget once remarked that Darwin's volumes "exemplified in a most remarkable manner" Darwin's power of "utilizing the waste material of other men's laboratories." One might venture the observation, in the light of our present study, that he was equally adept in the utilization of those stray sparks which fell from other men's minds, but which in his own head underwent a marvelous transformation.

The finest vindication of Charles Darwin, man and voyager, lies in the fact that Edward Blyth, who also fled England, and, traveler-naturalist that he was, bargained once for eighteen tigers in the river port of Lucknow, respected Charles. "He is a very clever, odd, wild fellow," Darwin once wrote in turn of Edward Blyth, "who will never do what he could do, from not sticking to any one subject."[121]

They knew each other well, those two. Blyth, so far as the published materials inform us, chose never to remember that he had, in the eighteen-thirties, written of things which, however differently, had swept the world in '59. Being both generous and modest, perhaps he never saw any relation between his youthful cogitations and the great change in human thinking which ensued a quarter of a century later.[121a] Or perhaps he thought the old conception common. One cannot help but wonder. We know, certainly, that Blyth was several times a guest at Down and that many unpublished letters passed between him and Darwin.[122]

It is well to end upon and to respect the two men's mutual silence. But let the world not forget that Edward Blyth, a man of poverty and bad fortune, shaped a key that dropped half-used from his hands when he set forth hastily on his own ill-fated voyage. That key, which was picked up and re-forged by a far greater and more cunning hand, was no less than natural selection. At that moment, probably in 1837, the *Origin* was born.[123] When Blyth died in 1873 there was found among his papers a fragment of a work which he was preparing "On The Origination of Species."[124] It was not, his literary executors opined, worth publishing. It was derivative. In truth it was the dry seed husk fallen from what had grown to be a great tree.

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[118] See Hubble, Douglas, The life of the shawl, *Lancet* **265**: 1351–1354, 1953. Also Good, Rankin, The life of the shawl, *Lancet* **266**: 106–107, 1954.

[119] It is worth noting that the biologist William Ritter was struck, a number of years ago, with the fact that Darwin had become convinced of the fact of evolution before natural selection "had occurred to him." In the light of the present paper Ritter's observation of the dichotomy between belief and discovery takes on renewed interest. Mechanical ideas in the last hundred years of biology, *Amer. Naturalist* **72**: 318–319, 1938.

[120] *MLD* 1: 475.

[121] *MLD* 1: 63.

[121a] Blyth was quick to notice, however, the significance of Wallace's paper of 1855 on the law of succession, and mentioned it to Darwin. This suggests he was following evolutionary discussions in this later period with interest.

[122] Francis Darwin says: "His letters to my father give evidence of having been carefully studied . . ." (*LLD* 2: 316). To my knowledge, these letters have never been published. Nor were Darwin's letters to Blyth forthcoming at the time the published collections were made (*MLD* 1: 62).

[123] In a letter written to Lyell in September of 1838 we find Darwin speaking enthusiastically of the "delightful number of new views which have been coming in thickly and steadily, on the classification and affinities and instincts of animals—bearing on the question of species. Notebook after notebook has been filled . . ." (*LLD* 1: 298). It will be recalled that Blyth's papers dealt with these matters and Darwin had utilized the journals which contained them.

[124] Grote, Arthur, A memoir of the late Edward Blyth, *Jour. Asiatic Society of Bengal*, Part II, n.s., **43**: xiv, 1875.

APPENDIX A

SELECTED ARTICLES WRITTEN BY EDWARD BLYTH, PUBLISHED IN THE MAGAZINE OF NATURAL HISTORY

Vol. 3 (1835), pp. 40–53: ART. IV *An Attempt to classify the "Varieties" of Animals, with Observations on the marked Seasonal and other Changes which naturally take place in various British Species, and which do not constitute Varieties.*

THE appellation "variety" being very commonly misapplied to individuals of a species, which are merely undergoing a regular natural change, either progressing from youth to maturity, or gradually shifting, according to fixed laws, their colours with the seasons, I conceive that it will be useful to some, to point out a few of the less generally known changes which naturally take place in various British animals; some few of which appear to have been hitherto overlooked, and others to have been described incorrectly.

The term "variety" is understood to signify a departure from the acknowledged type of a species, either in structure, in size, or in colour; but is vague in the degree of being alike used to denote the slightest individual variation, and the most dissimilar breeds which have originated from one common stock. The term is, however, quite inapplicable to an animal in any state of periodical change natural to the species to which it belongs.

Varieties require some classification; and though I feel myself hardly adequate to the task, I shall here propose to arrange them under four principal heads; in the hope that this endeavour will induce some naturalists, more competent than myself, to follow out this intricate and complicated subject, into all its details.

I would distinguish, then, what are called varieties, into *simple variations*, *acquired variations*, *breeds*, and *true varieties*. These appear, in general, sufficiently distinct, although the exact limits of each are sometimes very difficult to be assigned. Indeed, in many cases they only differ in degree, and in others they may be all combined in one individual. Moreover, the varieties of either class have a much greater tendency to produce varieties of another class, than the typical animals of a species have to produce any sort of variety.

I. *Simple Variations.*—The first class, which I propose to style *simple or slight individual variations*, differs only in degree from the last, or *true varieties;* and consists of mere differences of colour or of stature, unaccompanied by any remarkable structural deviation; also of slight individual peculiarities of any kind, which are more or less observable in all animals, whether wild or tame, and which, having a tendency to perpetuate themselves by generation, may, under particular circumstances, become the origin of true *breeds* (which constitute my third class of varieties), but which, in a state of nature, are generally lost in the course of two or three generations. Albinoes belong to this first division, and also the other numerous anomalies mentioned in VII. 589—591. 593—598.[125] These *simple variations* occur both in wild and in domestic animals, but are much more frequent in the latter, and are commonly observed in all *breeds* and *true varieties*.

Among the Mammàlia, total or partial absence of colour is always, I believe, continued through life; excepting, of course, the cases of mere seasonal change; and, in this class of animals generally, perfect albinoes are much more numerous than among birds. Perfect albinoes are peculiar to warm-blooded animals, and in them there is a total deficiency of colouring matter in the rete mucosum, and, consequently, in the fur, and even the pigmentum nigrum of the eye is entirely wanting. In birds, these *perfect* albinoes are extremely rare, although several instances have been recorded in VII. 593.—598. There are three sorts, however, of true permanent albinoes, which may be thus designated:—1. *Perfect Albinoes;* which are entirely white, and in which the eyes appear crimson, from the total want of colouring matter, rendering the minute bloodvessels visible: 2. *Semi-Albinoes;* which are either white or of a pale colour all over, and in which the irides are always paler than usual, and not unfrequently blue [I. 66. 178.]: and, 3. *Partial Albinoes;* which are partly of the natural colour, but are more or less mottled *permanently* with white; and in which, if a white patch surrounds the eye, the pigmentum of that organ is commonly wanting. I have thus observed a rabbit, one eye of which was red, and the other dark hazel; but such instances are of very rare occurrence, although (and it is a curious fact) rabbits are

[125] The Roman numeral represents the volume number of *The Magazine of Natural History* for 1834. L. C. E.

often seen wholly white, with the exception of a small patch around each eye; which organ, consequently, is of the usual dark colour. Albinoes, when paired together, as is well known, produce chiefly albino offspring, and a *breed* of them may thus be perpetuated; but, even in a domestic state, they not unfrequently produce young of the usual colour; and, if paired with an ordinary individual, they sometimes produce partial albinoes, or semi-albinoes [I. 178.], and occasionally, if the original colour be brown (as in the case of mice or rabbits*), a black, sandy, or slate-coloured offspring, or an individual with one of these colours more or less varied with white, is produced; but, in the majority of instances, the young wholly resemble one of their parents, and the *preponderance* is decidedly in favour of the natural hue. The coloured offspring of an albino, however, even if matched with another coloured individual, has still a tendency to produce albinoes†, and this fact has been noticed in the human species; but, as Mr. Lawrence observes on the subject (in his *Lectures on the Physiology, Zoology, and the Natural History of Man*), "the disposition to change is 'generally' exhausted in one individual, and the characters of the original stock return, unless the variety is kept up by the precaution above mentioned, of excluding from the breed all which have not the new characters. Thus, when African albinoes intermix with the common race, the offspring generally is black," &c. These observations apply alike to all *simple* or individual *variations*, and to most other varieties, and afford one of many reasons why marked breeds are in a state of nature so rarely perpetuated. There is yet, however, before quitting this subject, another sort of albino to be considered, which, I believe, is peculiar to the feathered race, and which is not, like the others, permanent; these, therefore, I shall denominate *temporary* albinoes. Most of the pale, white, and pied varieties of birds, which are produced in a state of nature, are of this kind. A friend informs me that a perfectly white lark in his possession moulted, and became of the ordinary hue. I lately shot a sparrow which was all over of a very pale brown, or cream colour; it was moulting, and some of the new feathers that were coming were of the usual colour, and others were of a pure white: on the next moult, probably, no more white feathers would have appeared. Of a brood of young robins which frequented my garden, two were white, one partially so, and one of the usual mottled brown; these all moulted into the ordinary colour. I could add other instances to the list, especially amongst domestic poultry. But it does not hence follow that among wild birds there are *no* permanently white or pied varieties; or, in other words, no true partial and semi-albinoes. A blackbird with a white head has now inhabited a garden in this neighbourhood for three successive years; and if the cupidity of collectors did not mark out every white or pied bird for destruction, I doubt not that I should have been able to have furnished some other similar instances of *permanent* variation.

II. *Acquired Variations.*—The second class of varieties which I would designate thus, comprises the various changes which, in a single individual, or in the course of generations, are *gradually* brought about by the operation of known causes: such as the greater or less supply of *nutriment;* the influence of particular *sorts* of food; or, either of these combined with the various privations consequent upon *confinement;* which changes would as gradually and certainly disappear if these causes were removed.

Redundance or deficiency of nutriment affects chiefly the stature of animals. Those herbivorous quadrupeds which browse the scanty vegetation on mountains are invariably much smaller than their brethren which crop the luxuriant produce of the plains; and although the cattle usually kept in these different situations are of diverse breeds; yet either of the breeds gradually removed to the other's pasture, would, in two or three generations, acquire many of the characters of the other, would increase or degenerate in size, according to the supply of nutritious food; though, in either case, they would most probably soon give birth to *true varieties* adapted to the change. In this instance, *temperature* appears only to exert a secondary influence. The Iceland breed of sheep, which feeds on the nutritious lichens of that island, is of large size; and, like the other ruminant animals which subsist on similar food, is remarkable for an extraordinary development of horns. Another example of *acquired variation*, dependent solely on the supply of nutriment, may be observed in the deciduous horns of the deer family, which are well known to be large or small according to the quality

* These observations are chiefly deduced from the results of some experiments with mice and rabbits.

† Of seven young rabbits thus produced, two were albinoes, one black, and the remainder of the usual color.

of their food. That *temperature* also does exert an influence greater or less, according to the species of animal, is very evidently shown in the case of the donkey*, of which there are no breeds, nor true varieties, and but very few simple variations [VII. 590.): this animal is every where found large or small, according to the *climate* it inhabits.

The influence of particular *sorts* of food may be exemplified by the well-known property of madder (*R*ùbia tinctòrum), which colours the secretions, and tinges even the bones of the animals which feed on it of a blood-red colour; and, as another familiar instance, may be cited the fact, equally well known, of bullfinches, and one or two other small birds, becoming wholly black when fed entirely on hempseed. I have known, however, this change to take place in a bird (the small aberdevatt finch, so common in the shops), which had been wholly fed on canary seed; yet this by no means invalidates the fact, so often observed, of its being very frequently brought about by the direct influence of the former diet. In several instances which have fallen under my own observation, feeding only on hempseed has invariably superinduced the change.†

The most remarkable of acquired variations are those brought about in animals in a state of confinement or domestication: in which case an animal is supplied regularly with abundance of very nutritious, though often unnatural, food, without the trouble and exertion of having to seek for it, and it becomes, in consequence, bulky and lazy, and in a few generations often very large; while the muscles of the organs of locomotion, from being but little called into action, become rigid and comparatively powerless, or are not developed to their full size. The common domestic breeds of the rabbit, ferret, guinea-pig, turkey, goose, and duck, are thus probably only acquired variations, which, from the causes above-mentioned, have in the course of generations, become much larger and heavier (excepting, however, in the case of the turkey) than their wild prototypes, and less fitted for locomotion; but which, if turned loose into their natural haunts, would most probably return, in a very few generations, to the form, size, and degree of locomotive ability proper to the species when naturally conditioned.* The crested varieties of domestic geese and ducks, and the hook-billed variety of the latter, are, however, in all probability, *true varieties;* and what are called "lob-eared" rabbits may be either a *true variety*, or a *breed.* The various slight diversities, which I call *simple variations*, are very common in the present class of varieties; and there is also in them a great tendency to produce what I call *true varieties*, as well as those slighter deviations, which, by particular management, may be increased into the sort of variety I denominate *breeds.*

III. *Breeds* are my third class of varieties; and though these may possibly be sometimes formed by accidental isolation in a state of nature,[126] yet they are, for the most part, artificially brought about by the *direct* agency of *man.* It is a general law of nature for all creatures to propagate the like of themselves: and this extends even to the most trivial minutiæ, to the slightest individual peculiarities; and thus, among ourselves, we see a family likeness transmitted from generation to generation. When two animals are matched together, each

* For some curious remarks on this subject, see the excellent article "Ass" in Partington's *Cyclopædia of Natural History.*

† I have not heard, however, that wild bullfinches, hawfinches, and other birds liable to be thus affected, are more commonly found black in localities where hemp is much grown. Amongst others, the skylark and woodlark are very susceptible of the influence of this food.

* [*A Tame Duck which flies with the same Power, and at the same Height, as a Crow.* (H. S., in I. 378.)—Was not this duck a wild one? I am led to ask this question from having myself witnessed a similar instance. I had often seen a duck, which I had taken to be a tame one, flying about, and always returning to the farm to which it belonged. On enquiry, I found that this duck had been taken, when a duckling, from the nest of a wild duck, and began to fly as soon as it was full grown. The case which H. S. mentions might probably be accounted for in the same manner, as it is by no means likely that so unwieldy a bird as the tame duck should think of trying its wings, after its ancestors had for so many successive generations been satisfied with walking and swimming, and fly "with the same power, and at the same height, as a crow."—*W. H. H. Postmark, Burton on Trent, Oct.* 8, 1834.

The late Rev. Lansdown Guilding had remarked as follows on the case stated by H. S.:—

Domestic birds, from flying little, have their muscles relaxed, or, perhaps, they never acquire their natural strength, for want of exercise. I have observed the geese in Worcestershire, in harvest time, to take very long flights; but, though they went on boldly, they never ascended very far into the air.—*Lansdown Guilding. St. Vincent, May* 1, 1830.]

[126] Note that here Blyth has already glimpsed the isolating mechanism which was to become, and remain, an important evolutionary topic. L. C. E.

remarkable for a certain given peculiarity, no matter how trivial, there is also a decided tendency in nature for that peculiarity to *increase;* and if the produce of these animals be set apart, and only those in which the same peculiarity is most apparent, be selected to breed from, the next generation will possess it in a still *more* remarkable degree; and so on, till at length the variety I designate a *breed*, is formed, which may be very unlike the original type.

The examples of this class of varieties must be too obvious to need specification: many of the varieties of cattle, and, in all probability, the greater number of those of domestic pigeons, have been generally brought about in this manner. It is worthy of remark, however, that the original and typical form of an animal is in great measure kept up by the same identical means by which a true *breed* is produced. The original form of a species is *unquestionably* better adapted to its *natural* habits than any modification of that form; and, as the sexual passions excite to rivalry and conflict, and the stronger must always prevail over the weaker, the latter, in a state of nature, is allowed but few opportunities of continuing its race. In a large herd of cattle, the strongest bull drives from him all the younger and weaker individuals of his own sex, and remains sole master of the herd; so that all the young which are produced must have had their origin from one which possessed the maximum of power and physical strength; and which, consequently, in the struggle for existence, was the best able to maintain his ground, and defend himself from every enemy. In like manner, among animals which procure their food by means of their agility, strength, or delicacy of sense, the one best organised must always obtain the greatest quantity; and must, therefore, become physically the strongest, and be thus enabled, by routing its opponents, to transmit its superior qualities to a greater number of offspring. The same law, therefore, which was intended by Providence to keep up the typical qualities of a species, can be easily converted by man into a means of raising different varieties; but it is also clear that, if man did not keep up these breeds by regulating the sexual intercourse, they would all naturally soon revert to the original type. Farther, it is only on this principle that we can satisfactorily account for the degenerating effects said to be produced by the much-censured practice of "breeding in and in."* There would almost seem, in some species, to be a tendency, in every separate family, to some particular kind of deviation; which is only counteracted by the various crossings which, in a state of nature, must take place, and by the above-mentioned law, which causes each race to be chiefly propagated by the most typical and perfect individuals.

V. *True Varieties.*—The last of these divisions to which I more peculiarly restrict the term *variety*, consists of what are, in fact a kind of deformities, or monstrous births, the peculiarities of which, from reasons already mentioned, would very rarely, if ever, be perpetuated in a state of nature; but which, by man's agency, often become the origin of a new race. Such, for example, is the breed of sheep, now common in North America, and known by the name of *ancons**, or *otter* sheep. A ewe produced a male lamb of peculiar form, with a long body, and short and crooked limbs: the offspring of this animal, with ordinary females, was found sometimes to resemble the one parent, and sometimes the other; but did not usually blend the characters of each; and, in the cases of twins, the two lambs were often equally diverse with their parents. This variety was extensively propagated, in consequence of being less able to jump over fences than the ordinary breeds of sheep. The solidungular ["donkey-footed"] variety of swine, tailless cats, back-feathered, five-toed, and rumpless fowls, together with many sorts of dogs, and probably, also the race of fan-tailed pigeons, are other striking examples of *true varieties.*

The deviations of this kind do not appear to have any tendency to revert to the original form: this, most probably, could only be restored, in a direct manner, by the way in which the variety was first produced.

To this class may be also referred, with more than probability, some of the more remarkable varieties of the human species. With regard to colour, we know that temperature exerts no *permanent* gradual influence whatever: white races remain unchanged at slight elevations within the tropics; and the natives of Boothia Felix are very dark; the swarthy inhabitants of

* See, however, a good practical article on this subject, entitled "Breeding," in one of the forthcoming numbers of the now publishing edition of Miller's *Dictionary of Gardening and Rural Economy.*

* *Agkōn*, an elbow, from the crooked form of the forelegs. See Lawrence's *Lectures*, p. 447, 448.

Mauritania are a white race, and their sunburnt hue is merely an *acquired variation*, which is not transmissible by generation, and which does not extend to those parts which are not exposed to the sun. The colouring principle of black races is inherent in them, and is quite independent of external agency; is even darkest in some parts which are the least exposed, and *vice versâ*. The Ethiopian race is nowhere more black than in the vicinity of the Cape of Good Hope, where the crops are sometimes injured by the winter's frost. Strangely enough, this invariableness of colour constitutes about, perhaps, the most fixed character of these races.

There is one fact, however, here to be observed, which is very well worthy of attention; and this is, that coloured varieties appear to have been chiefly produced in hot countries; which seems almost to induce the conclusion that they were originally efforts of nature, to enable the skin to withstand the scorching produced by exposure to the burning rays of a tropical sun.* How far the structural peculiarities of the negro and other races may not, in some cases, be the effects of *breed*, it would be impossible, perhaps, now to ascertain, and would be worse than presumption, in a novice like myself, to try to determine. Wherever a black individual was produced, especially among rude nations, if the breed was continued at all, the natural aversion it would certainly inspire would soon cause it to become isolated, and, before long, would, most probably, compel the race to seek for refuge in emigration. That no example, however, of the first production of a black variety has been recorded, may be ascribed to various causes; it may have only taken place once since the creation of the human race, and that once in a horde of tropical barbarians remote from the then centres of comparative civilisation, where no sort of record would have been preserved. But it is highly probable that analogous-born varieties may have given rise to the Mongolian, Malay, and certain others of the more diverse races of mankind; nay, we may even suppose that, in some cases, the difference, in the first instance, was much *greater*, and was considerably modified by the intermixture which must have taken place in the first generations. The mixed offspring of two different varieties of man thus generally blends the characters of each; though instances are not wanting of its *entirely* resembling (like the mixed produce of an ancon sheep) either one or the other of its parents; but in this case (as in the albino) the perfect characters of the other parent frequently show themselves in the next generation. I am entering, however, into a wide field, already well trodden by many philosophers; and the subject is already probably pretty well understood by the great majority of readers. Those who are not so familiar with it, will find it ably treated in various works; especially in Dr. Pritchard's work on man, and in the published *Lectures on the Natural History of Man*, by Lawrence: some sound and excellent remarks on *varieties* will also be found in the second volume of Lyell's *Principles of Geology*.

Still, however, it may not be impertinent to remark here, that, as in the brute creation, by a wise provision, the typical characters of a species are, in a state of nature, preserved by those individuals chiefly propagating, whose organisation is the most perfect, and which, consequently, by their superior energy and physical powers, are enabled to vanquish and drive away the weak and sickly, so in the human race degeneration is, in great measure, prevented by the innate and natural preference which is always given to the most comely; and this is the principal and main reason why the varieties which are produced in savage tribes, must generally either become extinct in the first generation, or, if propagated, would most likely be left to themselves, and so become the origin of a new race; and in this we see an adequate cause for the obscurity in which the origin of different races is involved. In a civilised state of society there are other inducements, besides personal attractions, and a new variety in this case, unless very *outré* indeed, would be gradually merged, and in a few generations would disappear entirely by intermixture with the common race. The inferior animals appear not to have the slightest predilection for superior personal appearance; the most dissimilar varieties of the same species mix as freely and readily together as the most typical individuals; the most powerful alone becomes the favourite. Instances of this are not rare in the breeds of dogs.

The above is confessedly a hasty and imperfect sketch, a mere approximation towards an apt classification of "varieties;" but if it chance to meet the eye, and be fortunate enough to

* See Dr. Stark "on the influence of colour on heat and odours," in Jameson's *Edinburgh Journal* for July, 1834; also Professor Powell's reply to it, in the number for October, 184.

engage the attention, of any experienced naturalist, who shall think it worth his while to follow up the subject, and produce a better arrangement of these diversities, my object in indicting the present article will be amply recompensed.

Here, however, I may observe, that the classification I have proposed for specific deviations in the animal creation, is equally applicable to those of the vegetable. The "varieties" in both are strictly analogous.

I come now to the second division of my subject, which is to point out

Some Periodical and other Changes of Appearance, which naturally take place in various British Animals, and which do not constitute Varieties.— Among our native Mammàlia, I know of three principal modes whereby a change of colour is brought about; namely, an actual shedding of the coat; a partial shedding of the coat; and an actual change of colour in the coat itself.

I. As an example of change of appearance produced by actual shedding of the coat, may be instanced the fallow deer (*C*èrvus *D*àma), whose white spots disappear with the annual casting of its coat in autumn.

II. Partial shedding of the coat takes place in those animals which acquire in autumn a covering of two different kinds: one long, downy, and warm, which is shed in spring; the other short and glossy, which is retained. This change of appearance is exemplified in the common water shrew (*S*òrex fòdiens), the short summer coat of which is much blacker than the longer downy covering which conceals this in winter. In this little animal the additional winter coat is shed about the latter end of March, or beginning of April; and does not take place uniformly, but progressively, beginning on the head, and ceasing at the hinder extremities; and exhibiting in its progress, throughout, a well-defined line of separation. Animals which (as the British *M*ustelinæ) have two sorts of fur, the *shorter* of which is the more warm and downy, do not undergo this change, but retain both sorts throughout the year. In these the young have only one kind, which is close and woolly; as is well exemplified in the common polecat (Putòrius Fùrc), the young of which are of a very uniform dark brown, and very unlike the old animals.

III. Actual change of colour in the coat itself is exhibited in the appearance of the fallow deer's white spots in spring, and in the case of the mountain hare (*L*èpus variábilis), which is in summer grey, adapted to the hue of the lichens on which it squats; and in winter white, hardly to be discerned upon the snow. The same change also takes place in the stoat or ermine (Putòrius ermíneus), although this is doubted by Mr. Berry (VII. 591.).* In mild winters, such as we have of late experienced in the South of England, but few of the stoats become white, and some of these not until the latter part of the season. The change takes place quickly, but not uniformly, the animal assuming for a short time a pied appearance; but I have not succeeded in ascertaining whether it is accelerated by sudden cold, as the animals are not always to be seen exactly when we want them. One perfectly changed, however, was seen in this neighbourhood soon after the one or two days of very cold weather in the beginning of last October. In reference to Mr. Berry's communication, I may observe, that in many dozens of stoats which I have seen in summer, I have never yet seen a white one; whereas in winter, I have seen in the same neighbourhoods a considerable number of white stoats. Where the climate is excessive, and the transitions of the seasons are more sudden, this change is much more likely to take place generally. In the fur countries, the ermine's change of hue is, I believe, most regular.

There has been, strangely enough, a difference of opinion among naturalists, as to whether these seasonal changes of colour were intended by Providence as an adaptation to change of temperature*, or as a means of preserving the various species from the observation of their foes, by adapting their hues to the colour of the surface; against which latter opinion it has been plausibly enough argued, that "nature provides for the preyer as well as for the prey." The fact is, they answer *both* purposes; and they are among those striking instances of *design*, which so clearly and forcibly attest the existence of an omniscient great First Cause. Experiment demonstrates the soundness of the first opinion;

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* This gentleman should have mentioned, in his account of the white stoats seen in summer, whether the tail was white or black. If the former, they were doubtless albinoes; if the latter, some constitutional debility may have prevented them from resuming their natural hues. I have seen white stoats late in March, but never after this. Both in these and in the white ferret (a domestic albino variation of the polecat) a decided tinge of yellow is always more or less noticeable.

* See Dr. Stark's paper, before cited, in Jameson's *Philosophical Journal* for July, 1834. [See *MNH* 6: 79.]

and sufficient proof can be adduced to show that the other is also sound. Some arctic species are white, which have no enemy to fear, as the polar bear, the gyrfalcon, the arctic eagle-owl, the snowy owl, and even the stoat; and therefore, in these, the whiteness can only be to preserve the temperature of their bodies [VI. 79.]; but when we perceive that the colour of nocturnal animals, and of those defenceless species whose habits lead them to be much exposed, especially to enemies from above, are *invariably* of the same colour with their respective natural haunts, we can only presume that this is because they should not appear too conspicuous to their enemies. Thus, in the eloquent language of Mr. Mudie†, who, however, advocates the first opinion, "the ptarmigan is lichen rock in summer, hoar frost in autumn, and snow in winter. Grouse are brown heather, black game are peat bank and shingle, and partridges are clods and withered stalks, all the year round." So, also, on the Continent, the common red-legged partridge (Erýthropus vulgàris) is of the colour of the gravelly and sandy soils on which it is found. So, also, are the different larks, the common quail, the various snipes, and all the other ground squatters, of the hue of their peculiar localities. So, also, are the numerous small Grallatòres which haunt the margin of the ocean, adapted to the colour of the sand. So, also, are those sylvan birds, which quit the dense umbrage of healthy growing trees, to seek their food and expose themselves on bare trunks and leafless decaying branches, of the hue of their particular haunts. "So exquisitely are they fitted for their office," says Mr. Mudie*, "that the several woodpeckers vary in tint with the general colours of the trees which they select. If it is an alternation of green moss, yellow lichen, and ruby tinted cups, with here and there a spot of black, then the green woodpecker comes in charge; but if it is the black and white lichens of the alpine forest or the harsh-juiced tree, then we may look for the spotted races upon the bark." The wryneck is the colour of the lichened branch; and the night swallow and the owls resemble their peculiar places of concealment. So, also, the gayer colours of nocturnal moths are always on the hinder wings†, and the anterior, which, when they rest, conceal these, are adapted to the hues of the various places where by day they are found: even the bright upper wings of the tiger moths (A′rctia Càj*a*, and A. víllica) are with difficulty recognised upon a lichened bank or paling.‡ It is curious, indeed, the resemblance which subsists between the colours of nocturnal birds and night Lepidóptera; the buff tip moth (Pygæ'ra bucéphala) thus reminds us of the barn owl (*Strí*x vulgàris); and the goat moth (*Có*ssus Lignipérda), and a host of others, are similar in their tints to most of the *Strí*gidæ: in both cases they are doubtless intended for the same purpose, that of concealment. It would indeed be easy to extend this list of examples considerably further; but I shall only now mention the common hare, which, when in form, would hardly ever be seen were it not for its brilliant eye; if its eye were closed, which it probably was before its quick sense of hearing had warned it of our approach, it would almost always, perhaps, wholly escape our observation. This ever continued watchfulness must have given rise to the supposition, that the hare always sleeps with its eyes open.

Seeing, therefore, so many most striking adaptations of colour to haunt, in cases where the concealment thus afforded can be the *only* purpose, I think it is not too much to infer, that the changes of colour in many arctic animals were intended by Providence for the double purpose of preserving their bodily heat, and of enabling them to elude the observation of their enemies. Certain it is, that their *conspicuousness* would otherwise expose them to inevitable destruction. If I had here space, I could satisfactorily prove that the high-flying Falcónidæ can, in most cases, only perceive their prey when it is *moving;* just, as on the sea-shore, *we* can only distinguish sanderlings when they move. Small Mammàlia which frequent open situations are rarely much abroad, except in the twilight; and ground-feeding birds are ever on the watch, and even the smaller kinds (as I have repeatedly observed) can perceive a hovering falcon *long* before it comes within the sphere of human vision; and they instantly flee to shelter, or they crouch, and lying motionless, so exactly resemble a portion of the surface, that even a hawk's eye cannot distinguish them.

† See Mudie's *Feathered Tribes of the British Islands*, i. 50.

* See Mudie's *Feathered Tribes of the British Islands*, i. 190.

† Among day-flying Lepidóptera, the more gaudy colours are usually on the *fore* wings.

‡ Animals of bright and gaudy colours are generally very retiring in their habits: even the common robin mostly turns away his breast as you approach.

Why should the falcon race be endowed with such wonderful powers of enduring hunger and fatigue, if, as is said, at the elevations at which they soar, they can clearly distinguish every living object scattered over the wide expanse beneath them? It is only on such animals as are *off their guard* that they descend; or otherwise, food being so abundant, they would soon multiply to the extirpation of their prey; which, of course, would be very speedily followed by that of the preyer.

How beautifully do we thus perceive, as in a thousand other instances, the balance of nature preserved: and even here we see another reason why sickly or degenerate animals (those, I mean, which are less able to maintain the necessary vigilance) must soon disappear; and why the slightest deviation from the natural hue must generally prove fatal to the animal. How different, thus, are even *simple variations* from the seasonal changes of colour which naturally take place! Properly followed up, this subject might lead to some highly interesting and important results. It certainly points to the conclusion, that every, even the slightest, tint and marking has some decided use, and is intimately connected with the habits and welfare of the animal; and it also furnishes a satisfactory reason, why closely allied animals (or, in other words, animals of very similar form and habits) should so very commonly nearly resemble each other in their colours and in the general character of their markings.

Vol. 9 (1836), pp. 393–409: ART. I. *Observations on the various seasonal and other external Changes which regularly take place in Birds, more particularly in those which occur in Britain; with Remarks on their great Importance in indicating the true Affinities of Species; and upon the Natural System of Arrangement.*[127]

NUMEROUS as are the writers in this department of zoology; assiduously as the study of birds is cultivated in all parts of the civilised world; and talented as are many of the naturalists and close observers who devote their more particular attention to this branch; it still appears to me, that the numerous and very diversified regular changes of plumage and general external appearance, observable in this interesting subclass of animals, have been hitherto very greatly and strangely overlooked, and that, in consequence, the many valuable physiological inferences deducible from their investigation have been quite lost to the purposes of science and of classification.

It is true that many naturalists have in so far attended to the mutations of plumage which some particular species undergo, as that they are able at once to recognise them in every livery they assume; but the exact ages, and seasons, of moulting; the precise nature of the general, or only partial, change that is undergone, and the various accordances and dissimilarities observable between the changes of distinct species; the endless characters of agreement and difference, so important in pointing out affinities, in showing what apparently similar races could never be brought to hybridise together; would seem to have been passed over as unworthy of notice, as undeserving of a particular investigation.

The subject is both extensive and complicated, and involves a number of other recondite enquiries. I could have wished that some naturalist better qualified than myself had taken it in hand. For my own part, I have little time for practical observation; but, having long been in the habit of keeping a number of birds (chiefly the smaller kinds which occur in Britain) in a state of captivity, I have thus enjoyed some very favourable opportunities for making myself fully acquainted with the various changes that a great number of species undergo, both seasonally, and in their progress from youth to maturity and old age; and I have neglected no opportunity of studying those of other races, which circumstances may have variously chanced to place in my way.

It is to be remarked, then, that some species of birds (as, for example, the larks and starlings, the crows, the woodpeckers, and various others) moult the whole of their immature, or nestling, plumage the first year, including the wing and tail primaries; while a very few (as the bearded pinnock, Calamóphilus biármicus, and rose mufflin*, Mecistùra ròsea) shed the primary feathers of the tail the first season, but not those of the wing: numerous other races (as all the modifications of the fringillidous and thrush types) moult their clothing plumage very soon after leaving the nest, and retain the primaries

[127] This two-part article is not so directly pertinent as the papers of 1835 and 1837 to the evolutionary problem but it was thought advisable to reprint it here as bearing in a general way upon Blyth's taxonomical views and powers of observation. L. C. E.

* Pàrus caudàtus *Linnaeus*.

till the second autumn; the Falcónidæ, again, and some others, undergo no change whatever until that period. All those which I have as yet mentioned change their feathers only once in the year, towards the close of summer, immediately on the cessation of the duties towards their progeny: but there are various other tribes (as the wagtails and pipits, *M*otacillînæ, and most of the aquatic races) which regularly undergo another general moulting in the spring; though in no instance, that I am aware of, are the primary wing feathers shed more than once in the year: those of the tail, however, in some rare instances, are; and the different coverts, together with the secondary and tertiary wing feathers, in most, if not all, double-moulting birds, are changed twice. In some migrative species (as the cuckoo, and most of the swallows), the young of the year do not change their plumage until the winter months; whereas the old birds moult in autumn; and in other birds, again (as in various ducks [VIII. 544, 545.]), two general changes of feather take place within the short period of about four months. Very many other similar diversities, of a more or less subordinate character, might be enumerated, if enough have not been already mentioned to show that a wide field for observation is here open to the practical ornithologist.

In like manner may analogous diversities be observed throughout the mammiferous subclass of vertebrate animals; thus, the squirrels and the shrews renew their covering twice in the year, and the rats and rabbits but once. The common squirrel's seasonal changes have never, that I am aware of, been remarked by any naturalist, though it is so common an inhabitant of our island: its summer coat is very different from that of winter, the fur being much coarser, more shining, and of a bright rufous colour; while the ornamental tufts to the ears are wholly wanting: these grow in autumn, while the animal is renovating its coat, and continue usually till about the beginning of July, the time varying somewhat in different individuals. Their winter fur, besides being of a much finer quality and texture, is considerably longer, thicker and more glossy, and quite of a different hue from that of summer, inclining to greyish brown. The first young ones, too, which are produced very early in the season, push forth the winter garb, which, I believe, they then retain throughout the summer; whereas the second race of young ones, which, for the most part, make their appearance about midsummer, are first clad in the summer dress, which is exchanged, before they have become half grown, for that of winter. It is not improbable, also, that diversities of a like kind may obtain in the renewal of the scales of fishes.

What the definite purpose effected by very many of these peculiar and dissimilar changes may be, I confess myself utterly unable to say; nor can I suggest even a plausible hypothesis upon the subject. Why, for example, should the pipits (*A*′nthus) shed their plumage twice in the year, and the larks (*A*laúda) but once? And why, also, should the latter change all their nestling primaries at the first moult, while the former retain theirs until the third (including the vernal) general renovation of plumage? It is easy enough to say, with Mr. Mudie, that, in the wagtails, and certain other species, the colours of the summer and winter dresses are each, in so far as they differ, more peculiarly adapted to the particular season of the year; but this is merely a concomitancy: in other words, this adaptation is not the purpose of the change; for we find that, in certain species which regularly moult twice in the year (as the tree pipit), the summer and winter plumage hardly differ; whilst, on the other hand, as complete an adaptation of colour to season is effected in others (as the stone chat, and most of the *F*ringíllidae), which moult in autumn only, by the wearing off of the extreme tips of the feathers; these in winter having covered and concealed another, and, in many instances, a very diverse, colour beneath. By what reason can we ever hope to account for the curious fact, that the common drake, and also the pintailed and other teals, should moult their whole clothing plumage (including the tail) in summer, and then again in autumn? As Mr. Waterton has well remarked on the subject, "All speculation on the part of the ornithologist is utterly confounded; for there is not the smallest clue afforded him, by which he might be enabled to trace out the cause of the strange phenomenon. To Him alone, who has ordained the ostrich to remain on the earth, and allowed the bat to soar through the etherial vault of heaven, is known why the drake, for a very short period of the year, should be so completely clothed in the raiment of the female, that it requires a very keen and penetrating eye to distinguish them." [VIII. 544.]

In one point of view, however, at least, a knowledge of these changes is of considerable practical use to the naturalist; for they not

unfrequently point out at once, in doubtful cases, the most appropriate situation of a genus in a system, and thus assist him very greatly in his endeavours to fabricate a sound system of classification. Instances of this I shall not here advance, as it is necessary to say something first of what meaning I attach to that most hackneyed of all phrases, "natural system," concerning which it is more than probable that my views may very considerably, and perhaps essentially, differ, from those of many who may perchance honour them with a perusal.

Under this phrase, then, two very distinct kinds of relation are ordinarily blended together and confounded; viz. the adaptive relation of every organised production to the conditions under which it was appointed to exist, and the physiological relation subsisting between different species of more or less similar organisation. These may be aptly designated the *adaptive system*, and the *physiological system;* the system of relative adaptation between the earth, its productions, and its inhabitants, and the system of agreements and differences between the organisation of distinct races.

To illustrate the former of these is, perhaps, superfluous: it is the system by which alone the existence of one species is necessary to that of another, and which binds each race to its locality; where the presence of each is alike necessary to preserve the equilibrium of organic being around; and when circumstances have changed, and the necessity for its agency no longer remains, a whole race perishes, and the fragments of a skeleton in the solid rock perhaps alone proclaim that such had ever existed. It is the grand and beautiful, the sublime and comprehensive, system which pervades the universe, of which the sun and planets are but a portion, and which, to return to ornithology, is so well exemplified in the adaptation of the ptarmigan to the mountain top, and the mountain top to the habits of the ptarmigan; which suits the ostrich to the arid desert, the woodpecker to the forest, and the petrel to "the far sea wave." It is the majestic and admirable system by which all nature works so beautifully together, and to which all that our external senses reveal appertains. It is the system which, exquisite and intensely interesting in all its minutest details, is, if possible, even more so in its complicated relations; by which, by the *unity of design* pervading which, all is demonstrable to be the workmanship of One omnipotent and all-foreseeing Providence, under the beneficent dispensation of whom nought that ever exists or occurs stands isolated and alone, but all conduce and work admirably together for the benefit of the whole; by whose all-wise decree it is ordained, that, while the lofty and sterile mountain peak *attracts* the clouds, which in winter, in consequence, precipitate themselves upon it in the form of snow, it should thus *cause* itself to become clad in the hue of all others the most calculated to prevent its internal temperature from being farther reduced, and itself from thereby becoming an increased source of cold by radiation to all around; while, at the same time, the concretion of snow itself, instead of deluging the country round with superfluous moisture, is thus retained for a time upon the heights, not only to shelter the more tender organised productions of the mountain from severer cold, but also to furnish, by the action of the summer sun, a due supply of water, when needed, to the fountains and rills which irrigate and fertilise the more level country; there having done its part, to flow on to the mighty reservoirs of the ocean, again to arise in clouds, and to fulfil again its appointed rounds, with perpetual never ceasing energy, while the world endures.

"Look round our world; behold the chain of love
Combining all below and all above.
See plastic Nature working to this end;
The single atoms each to other tend,
Attract, attracted to, the next in place
Form'd and impell'd its neighbour to embrace.
See matter next, with various life endued,
Press to one centre still, the general good.
See dying vegetables life sustain,
See life dissolving vegetate again:
All forms that perish other forms supply:
(By turns we catch the vital breath and die,)
Like bubbles on the sea of matter borne,
They rise, they break, and to that sea return.
Nothing is foreign: parts relate to whole;
One all-extending all-preserving Soul
Connects each being, greatest with the least;
Made beast in aid of man, and man of beast*;
All served, all serving; nothing stands alone:
The chain holds on, and, where it ends, unknown."
POPE'S *Essay on Man*, Epistle iii.

* I am unwilling to allow even this harmless line to pass muster without indulging in a few remarks on the distinctness of the human race from all other parts of the animal creation; a distinctness too little borne in mind by many naturalists. Man alone, of all the countless wonders of creation, though clad in a material frame, the functions of which are necessarily identical with those of other animals,

In this sense of the phrase only we trace what may be esteemed a suitable meaning to the term "natural system:" this is the only system by which the wonders of creation are *naturally* arranged; this alone is the system which nature everywhere presents for our contemplation: but, admire it as we may, still this is not the system by which an extensive knowledge of *species* can be acquired, or which can be studied elsewhere than in the wilds.

Every species of organism, as must be obvious to all examining thinking persons, is framed upon a greater or smaller series of successively subordinate typical plans, upon each of which is organised a variety of different species, perfectly unconnected and distinct from each other, however some may resemble, even to minutiæ, and which exhibit each typical or subtypical structure more or less modified, and, in the extremes, generally more or less *approximating* towards the extreme modifications of other plans of organisation, in direct relation to the endless diversifications of the surface of the earth, to variety of climate, or to peculiar modes of procuring sustenance.

Thus far, I believe, all systematists agree.

I must venture, however, to differ from the majority of them, in opposing the prevalent notion, that the extreme modifications of diverse types blend and inosculate[128] by direct *affinity;* contending that, however closely these may apparently resemble, the most similar modifications of diverse types are not, in a physiological sense, more nearly related to each other than are the more characteristic examples of the same.

To this conclusion I was originally led by reflection on various interesting phenomena connected with the changes of plumage which take place in birds; having observed that, however importantly, to suit peculiarity in the mode of life, the general structure of very aberrant forms may be modified, so as to render it even doubtful upon which fundamental type they are organised there are, notwithstanding, certain constant characters, of less importance to the existence and welfare of the species, by which every typical standard may be easily traced to its ultimate ramifications; some of the most valuable of these characters, in the feathered race, being afforded by peculiarities in

is no part of the mere reciprocal system of nature; as they are. He alone is bound to no particular locality, but inhabits alike the mountain and the plain, and *by contrivance* is enabled to endure the fervid heats of tropical climes, and the withering blasts of a polar winter; traverses in all directions the wide extent of the pathless ocean, interchanges purposely the productions of distant lands, and accommodates the respective soils for their reception. He alone degenerates in climates which supply his every natural want; and placed as nature formed him, in the richest soil, is a being out of his element, unable, by the mere unassisted use of his own organs, to maintain his existence as a species. He alone studies the complicated laws of matter, that he may wield them at his will. He alone possesses a power of indefinite self-improvement, and can so communicate his attainments that each generation shall rise in knowledge above the last. He alone has the sense to sow, that he may reap; and, alone, intentionally, and from observation and reflection, opposes obstacles to the course of events in their natural progression; reduces whole countries to an artificial state; and systematically increases vastly their capability of yielding sustenance for him, and for those creatures he has taken under his protection. Other races disappear before him, whose existence is at all opposed to his interest, and those alone remain (but oh! how altered from their former condition!) which minister to his wants and comforts. All other beings are mere creatures of locality, whose agency tends to perpetuate the surrounding system of which they are members; but wherever man appears, with his faculties at all developed, the aspect of the surface becomes changed; forest yield to his persevering labours; the marshes are drained, and converted into fertile lands: the very climate accordingly changes under his influence, and oftentimes to the extinction of some of the indigenous products of the soil. Does not, then, all this intimate that the human race is no part of the mere mundane system, that its agency tends rather to supersede, and is opposed to, that of the rest of organic nature? that a time must come, should nought intervene of what in physics we can take no cognizance, when the human race, having peopled all lands, shall have increased beyond the means of subsistence? But alas! who can dive into futurity? The same awful Being who first awakened man into existence, in common with the meanest atom, who appointed his destiny upon earth to be so diverse from that of his other creatures, who endowed him alone with a capacity to reflect upon his Maker's goodness and power, may (I make no appeal here to revelation, writing only in the spirit of natural theology) close his non-conforming career, as a species, upon earth, in a manner different from the extinction of other species which yields to the progressive changes of the surface. No naturalist can doubt that this beautiful world existed, and was clad in verdure, and inhabited, for countless ages before man became its denizen; and there are no memorials to indicate that an analogous being ever previously existed. Man alone is a creature by himself; the only being whose agency is at all opposed to the mutual and reciprocal system of adaptations prevalent around him. He did not always exist here, and there is no reason to suppose that he always will. All conduces rather to intimate that he is but a sojourner for a short time. In his vanity, he is apt to imagine that all were made for him! and presumptiously enquires *of what use* could have been the creation without him! Yet how ardently does he labour to exterminate every portion of that creation, which he deems to be in the least injurious to his own interests!

[128] See p. 100 of my essay. L. C. E.

the mode of moulting. To illustrate this, I may cursorily adduce the various finch-like *Stúrnidæ* (Aglaíus, Mólothrus, Dolíchonyx, &c.); extreme modifications of the *Córvus* type; as are also, however unlike they unquestionably appear, the genera *Alaúda*, and even Ammódramus. All these, I have ascertained either from direct observation, or from competent sources, shed the nestling primaries the first season, which is not the case with any modification of the fringillidous type, or of the dentirostral. If other characters be wanting, which point alike to the same conclusion, I may mention the constant presence of a craw, or enlargement of the œsophagus, in all the *Fringíllidæ*, and its invariable absence in all, even the most aberrant, modifications of the *Córvus* type; all the latter, too, preserve the ambulatory mode of progression, which, in perfection, is not observable in any *Fringíllidæ*, not even Plectróphanes. Again, other characters of distinction between these two equivalent divisions are sufficiently visible in the general aspect of the bill, even where the extremes approximate: all the *Fringíllidæ*, for instance (to which I would restrict the appellation Coniróstres), possess what may be strictly defined a *bruising*, or *compressing*, instrument; whereas the general character of the same organ in the other division is rather what may be aptly termed a *thrusting* one, intermediate in its structure between those of the *Fringíllidæ* and Dentiróstres; in which last group the bill is modified into either a *snapping*, *holding*, or *tugging* instrument, as the case may be: sometimes all three, as in *Vìreo*.

However, to return to the proposition I was just advancing, that, *physiologically* speaking, there are no combinations of distinct types, no intermediate organisms, save those between a central type and its ultimate ramifications: the general structure may be intermediate, and, consequently, the situation a species holds in the *adaptive system*, the office which it may have to perform in the general economy of the universe; but the latter does not constitute *affinity;* neither, strictly speaking, is it *analogy;* therefore I must distinguish it by another term, *approximation*.

As I shall have occasion to make use of these words frequently, as I proceed, it will be necessary, before advancing further, to define the precise meaning which I attach to them, however much this may appear digressing from the subject more immediately in hand.

First, then, let us consider *affinity*, which, according to the views I hold, is inseparably connected with the doctrine of *types*.

All organised matter is, of course, intrinsically allied in its nature, as contradistinguished from that which is not organised; this, therefore, is the first, or, as some would rather say, the last, the ultimate, the slightest possible, degree of *affinity*. Next, we have a grand primary distribution of all organic matter into the animal and vegetable kingdoms*; a division too obvious to be for a moment called in question, and universally allowed; admitted even, inconsistently enough, by those who hold that every natural assemblage of species, great or small, forms part of some quinary circle. Now, I cannot but observe here, in passing, that, to any unbiassed person, I should think that a due consideration of this first *binary* distribution must at once carry conviction to the mind, must be at once a most unanswerable argument against all *quinary* or similar doctrines; the which, of course, if based upon sound theory, would not only be found to hold good, but would be most obviously indicated by these primary and comprehensive assemblages of every created species. But, to return: here we have the animal type, and the vegetable type, diverse in structure, distinct even in chemical composition, insomuch that the *kingdom* to which any dubious production appertains may be decided by chemical analysis, even in a fossil, should but a very few particles of its primitive substance have been preserved. Say not, that the kingdoms blend at their ultimate extremities; for there are no better grounds for this supposition than those which led many, for a time, to advocate the spontaneous generation of Infusòria; extreme minuteness alone setting the limit to a definite partition. We must therefore admit, that there is a degree of physiological *affinity* between the most dissimilar animals, and also between the most dissimilar plants, which no animal or vegetable can possibly have for each other: species from the two kingdoms, however these may undoubtedly *approximate* at the extreme boundaries, *can* have no higher degree of *affinity* for each other than what they possess in common, as opposed to all unorganised matter;

* The *mineral kingdom* is a superfluous epithet, too vague to have any meaning beyond a negative one. Chemically speaking, it, indeed, comprises both the others. The proper distinction is, of course, between *organized* and *not organized*.

what further relations they may show are, therefore, totally distinct from *affinity*.

Leaving plants, we now enter upon the primary divisions of the animal creation, the separate leading types, the distinct plans, upon one or other of which all animals are organised, and which cannot, any more than the last, be confounded, in any instance, one with another, however in particular cases these too may *approximate;* of which presently. Every vertebrate animal is, therefore, allied to every other vertebrate animal by what, to specify by numbers, may be expressed as three degrees of *affinity;* whereas it is physiologically related to every member of the Annulòsa, and other invertebrate classes, by only two degrees, its affinity with plants being reckoned as one; the proportions of these numbers towards each other pretty accurately denoting the value of these degrees; two being double one, three exceeding by half two, &c. Animals of the same subclass, as different mammifers, or birds, or reptiles, are, of course, related to each other by four degrees of *affinity;* those of the same order by five, and so on; the number of these several degrees increasing in proportion to the number of subordinate successive types upon which different species are alike organised, and of which, successively, they are modifications, not *combinations* of different ones, in the last case any more than in the first. Every modification of every successive type is thus rudimentally different from the most approximate modifications of every other equivalent type, or superior type, to which it does not appertain; and this is the same conclusion to which I have been irresistibly led from consideration of various phenomena connected with the changes of plumage which take place in birds. As every species is perfectly and essentially distinct and separate from every other species, so, except in a retrograde direction, are the successive typical and subtypical plans upon which they are severally organised, however similar the latter may in some instances be, as are also the former. It is unnecessary to enter here upon any remarks on *hybrids*, as further elucidatory of the precise nature of *affinity:* it is well known that these can only be produced within a certain physiological range, and that their degree of fertility (paired with individuals of pure blood) is in proportion to the degree of *affinity* between the parent species.

By the term *approximation*, I must be understood to signify those modifications of particular types, which, adapted to intermediate modes of life, very commonly more or less resemble (in consequence of this adaptation) species which are organised on other and different types. I have already had occasion to mention certain extreme modifications of the corvine or omnivorous type of perching birds, which are close *approximations* towards the fringillidous type (as Aglaíus and other finch-like *S*túrnidæ, Ammódramus, and *A*laúda); the true *affinities*, however, of all which are at once shown by a reference to their moulting. The hag, the lamprey, and the pride, are, thus, extreme *approximations* of the general vertebrate type of organisation towards the class Mollúsca. The Ornithorýnchus, among mammifers, *approximates* very remarkably towards birds; but it exhibits less *analogy* with them, collectively, than many rodent species do. The prong-horned antelope is an *approximation* in its genus towards the *C*érvidæ; but its *affinity* to the latter is not greater than in other antelopes. The frigate bird is an *approximation* towards the eagles; yet no one would consider it as organised upon the falcon type: so the *P*térocles is an *approximation* towards the pigeons, and the Nicobar pigeon towards the *G*allínidæ; each being at once referable to its particular type, though in certain *adaptive* relations they are intermediate. The pipit genus is a most striking *approximation* of a very marked type (subordinate to the dentirostral) towards the larks; but its moultings at once intimate its true position in the system, however its general aspect might, at first sight, render this doubtful. It is by no means nearly allied by *affinity* to *A*laùda; and I will unhesitatingly venture to assert, that by no art could they be induced to unite to the production of a hybrid.

Analogy, in the most definite signification of the term, is well exemplified in the close resemblance between the mouth of the swift, and those of the larger high-flying insectivorous bats (*V*espertílio). It is exhibited wherever species that are modifications of diverse types are organised to perform nearly the same part in the general economy of nature; which latter by no means necessarily implies *approximation;* as may be illustrated by adducing the vultures among birds, and the dog kind among quadrupeds, or certain of the *S*phíngidæ from amongst insects, as compared to the *T*rochílidæ of the feathered race. It is well exemplified by the

deadly spring of the cats, as compared with that of the crushing serpents, and as somewhat contradistinguished from that of the saltatory spiders; all the energy of the body being, in the former cases, remarkably concentrated in a single spring, from which exhaustion follows, while in the latter case it is not. It is curiously shown by a fact related by Sir W. Jardine, of the European howler or eagle-owl (*Bùbo europæùs*), in which the *analogy* of that genus to the cat family is even more strikingly indicated than by the very remarkable general resemblance in their external aspect. "This bird," observes Sir William, "evinces a great antipathy to dogs, and will perceive one at a considerable distance; nor is it possible to distract its attention so long as the animal remains in sight. When first perceived, the feathers are raised," &c., exactly as a cat raises her fur at sight of her natural enemy; though, in either case, it is difficult to say why they should be inimical. No doubt, however, the purpose, the reason for this antipathy, is the same in both instances, and it is for the naturalist to endeavour to find it out. The common pipit, a modification of the dentirostral type; and the Lapland snowfleck, one of the conirostral (as here limited); are in so far related to each other by *analogy*, as that they are both *approximations* towards the lark genus, an extreme modification of the omnivorous or corvine type; they are therefore related to each other by a certain analogy; to *A*laúda, by *approximation;* and to all the members of their respective separate groups, by an additional degree of *affinity* to what subsists between either of them and the others. *Affinity* and *analogy*, of course, coexist, as all organisms are, at least, related by what I have termed the first degree of the former; but the extent of the former does not necessarily affect that of the latter: vultures and dogs, for instance, are allied by three degrees of *affinity;* while the carrion beetles (*C*arábidæ) are related to either by only two degrees: yet the *analogy* is as great in the one instance as in the other. Pure *analogy* may subsist with very trifling *approximation;* as is shown by the already cited case of the cats and serpents, or as may be exemplified by a hundred similar instances of corresponding groups existing in major divisions of diverse structure, in which, however marked the *analogy*, however similar the office they were destined to perform the degree of *approximation* is in many instances quite imperceptible.

Affinity, *approximation*, and *analogy*, may therefore be collectively defined as pertaining to the *physiological* relations subsisting between different species, as opposed to their *adaptive* relations; of which latter they are wholly independent: that is to say, different species, nearly allied by either of these physiological relations, exhibit no mutual, no relative adaptation towards each other's habits and structure; such as we observe in the huge claws of the anteater (Myrmecóphaga), evidently furnished in direct relation to the habits of a particular group of insects, the mounds of which they are obviously intended to scrape open, while the tongue is as expressly modified to collect the aroused inhabitants, upon which alone the creature is fitted to feed, and upon the supply of which, therefore, as an existing species, its being altogether depends. *Adaptive* relations are, in general, even more obvious and striking in groups which are *physiologically* the most widely removed; as may be exemplified by adducing the bill of the crossbill, modified in direct relation to the seminiferous cones of the Coníferæ; or the recurved bills of certain humming birds, to the bent tubes of the corollas of particular Bignon-*iàceæ*, &c. *Physiological* relations are all resolvable into mere *resemblance;* because every species is essentially distinct and separate from every other species; otherwise it would not be a *species*, but a *variety*. The most similar species, therefore, are only *allied* to each other in consequence of the close resemblance of their general organisation; the degree of *affinity* being greater or less, according to the extent of that resemblance (according to the degree of their physiological, not their mere apparent, similitude); in short, according as they are more or less framed upon the same general or typical plan; which plans not only regulate the minutiæ of structure in those species which are organised upon them, but, to a very considerable extent, even their colours and markings.

Of course, the observation here very naturally suggests itself, that, if the colours and markings of species have a definite use (which, in some instances, is sufficiently obvious even to our comprehension), then, we might reasonably expect to find that resemblance which is found to subsist between those of species whose habits are almost the same.

True; but, then, there are many trivialities observable in the marking of allied species, which can only be explained upon the principle

that they are modifications of some particular general or typical plan, of markings, as well as of structure. Such is the pale line along the head of the newly discovered Dalmatian, *Régulus modéstus Gould*, in place of the bright-coloured coronal feathers of its different congeners; which is exactly analogous to the curious fact, that the apparent rudiments of dentition exist in the gums of the fœtal toothless whales; sufficiently intimating that these latter animals are modifications merely of some general typical plan, of which one of the leading characters is to be furnished with teeth. So, also, might be adduced the tiny, soft, deflected spine situate at the bend of the wing of the common gallinule, in like manner indicating that this species, also, is framed upon some particular plan of structure, the more characteristic examples of which have spurred wings, as we find to be the case in the allied genus *Párra*. In all the species organised upon any given type, we may always look for some trivial resemblances of this kind; we may always expect to find some traces of any particular structure or markings, which are observable in those typical forms of which the others are but modifications; the probability of this, of course, increasing with the number of degrees of *affinity;* and it is not unusual, too, to find colours or markings, which, in typical forms are scarcely discernible, developed, as it were, in particular modifications of those forms, to a considerable extent: yet, in the most approximate modifications of diverse subtypes of one general type, we only find such trivial resemblances of this kind as may be directly traced up to the typical standard from which they both diverge; whatever other marks of similitude these may show being obviously analogous adaptations, rather, to similarities of habit, unaccompanied by those trivial resemblances which imply physiological proximity. Thus, however closely, both in form and colouring, our common grey flycatcher (Muscícapa *Grísola*) may *approximate* to some of the smaller Tyránnulæ of North America, the mottled character of its nestling garb at once indicates that it is not framed on exactly the same series of successive types; in a word, that its relation towards these tyrannules must be considered as one of *approximation*, rather than of direct *affinity*. It would be easy, in like manner, to illustrate the preceding several positions; but the limits of the present disquisition will not permit of it.

It only now remains for me to apply the various facts which I have been endeavouring to establish; after which I shall commence a minute detail of observations on the moulting of birds. That our systems of classification should be founded on the true *affinities* of species, rather than upon any arbitrary characters, is now, I believe, admitted on all hands to be the desideratum; and the true *principle* on which alone this can be effected is, as it appears to me, sufficiently obvious; though, from our present very imperfect acquaintance with existing species, it must necessarily be a long while yet before our arrangements can be considered at all final, if, indeed, we can ever hope them to assume that character.

The true physiological system is evidently one of irregular and indefinite *radiation*, and of reiterate divergence and ramification from a varying number of successively subordinate typical plans; often modified in the extremes, till the general aspect has become entirely changed, but still retaining, to the very ultimate limits, certain fixed and constant distinctive characters, by which the true affinities of species may be always known; the modifications of each successive type being always in direct relation to particular localities, or to peculiar modes of procuring sustenance; in short, to the particular circumstances under which a species was appointed to exist in the locality which it indigenously inhabits, where alone its presence forms part of the grand system of the universe, and tends to preserve the balance of organic being, and, removed whence (as is somewhere well remarked by Mudie), a plant or animal is little else than a "disjointed fragment."

Systematists, with few exceptions, err most grossly in imagining that allied species have been created in direct reference to each other (as members of a sort of cabinet system of even proportions) rather than to the localities they indigenously frequent, to the office each was ordained to fulfil in the universal, or *adaptive*, system. One would have supposed that the various facts which geology has brought to light would have sufficed to undeceive them in this particular. It cannot be too often repeated, that, upon whatever plan a species may be organised, its true relation (the reason for its existence at all) is solely connected with its indigenous locality: else, why should so many thousand species have ceased to be, the particular circumstances under which they were ap-

pointed to live no longer requiring their presence? To expect, indeed, for a single moment, that, in any isolated class or division of organisms, a perfect system of another kind could obtain, harmonising in all points, and true in the detail to any *particular number*, appears to me (even supposing that none of the species were now extinct, and that we knew all that are at present existing), *primâ facie*, a manifest illusion. Species are distributed over the earth, wherever the most scanty means of subsistence for them are to be found; and their organisation is always beautifully and wonderfully adapted for obtaining it under whatever circumstances it may exist: just, therefore, as the surface varies, so do its productions and its inhabitants; and there is no locality, or apparently, even vegetable production, so peculiar, but species are found upon it especially organised to find their subsistence chiefly or wholly there. The very underground lake has its own peculiar inhabitants; for the wondrous Pròteus there revels in regions of everlasting night: of course happy in its existence as the bird that cleaves the free air, or as the lion that exults in his conquering prowess. Ponder this well; and it is clear, that upon these grounds alone all *quinary* imaginings must at once fall to the ground.

The more deeply, indeed, I consider the quinary theory (now advocated by so many talented naturalists) in all its bearings, the less consistent does it appear to me with reason and common sense; the more thoroughly am I convinced of its utter fancifulness and misleading tendency. Nothing in this world is without its particular and definite *use*, which observation, in time, generally contrives to discover: but what utility could there be, what *purpose* could be effected, by separate and distinct races of beings, created obviously in direct relation to particular localities, being distributed into even groups of a limited number, like the celebrated groves of Blenheim, "nodding at each other?" If the quinary system be universal, as some would have, pervading all creation, how is it that the stars and planets do not revolve in groups of five? Or why even do not animals mostly produce their young by fives, or multiples of five? The absurdity is, indeed, too great to be dwelt on. If we examine, too, the writings of even the most eminent advocates of this strange theory, we continually meet (as might be expected) with divisions apparently made for mere dividing sake, that the requisite number of groups might be filled up; and, on the other hand, with examples equally glaring of the most dissimilar forms being brought under one general head, that the same particular number should not be exceeded. Thus, in Mr. Selby's in many respects very valuable and useful "*British Ornithology*," while the closely allied linnets and siskins are placed in *separate subfamilies*, between the *types* of which no supergeneric character of the least importance can be descried, we find the buntings actually arranged in a subfamily of which the larks are typical; and, in another division, of like value, among his Sylvìadæ, four genera (*P*àrus, *A*ccéntor, Setóphaga, and Calamóphilus) grouped together, which have hardly a single character in unison that is not common to the whole Dentiróstres, and which, certainly, are but very distantly allied. To adduce additional instances must be superflous: a system which can admit of such very arbitrary arrangements can have but a faint title indeed to be designated the "only natural one."

It is unnecessary now any longer to detain the attention of the reader by further prefatory observations; nor would it be worth while here to offer any remarks on the progress of plumification, the which might be better introduced as occasion may require; but I shall forthwith proceed to point out what I conceive to be of very great importance towards the classification of birds according to their true affinities, the different changes of plumage and appearance to which various groups of them are subject, confining myself, for the most part, to those upon which I can speak quite positively, from having myself had opportunities of witnessing them. On this enquiry there is, indeed, hardly any guide to go by, but direct personal observation; for though, in the books the greater number of these changes of appearance in the feathered race have been often mentioned, it is seldom that the precise manner in which they are brought about is stated; and the term "vernal moult" has been, in general, so very vaguely applied (sometimes indicating an actual shedding and renovation of the feathers themselves, and sometimes merely the seasonal wearing off of their winter edgings), that I have thought it best to decline altogether availing myself of their assistance. I may just premise, however, before commencing, that, independently of moulting, there are two principal modes by which a great alteration in the appearance of the feathers

of birds is, in some cases, gradually brought about; namely, a direct change of colour in the feathers themselves, and the gradual shedding, in spring (as has already been spoken of), of their extreme tips, which are frequently of a different and more dingy colour than that part of the feather which becomes exposed to view when these have disappeared. A familiar and beautiful illustration of both these changes is furnished by the breast plumage of a male of the common, or song, linnet (*Linària cannábina*). The coloured portion of these feathers, in winter, is of a brownish red; and they are tipped with deciduous dusky edgings. In the spring, the latter gradually wear off, and the dark maroon changes to a bright crimson.* The same plumage which the ptarmigan acquires in autumn becomes, in winter, white, and in spring gradually re-assumes somewhat its former colour, but a still deeper one.† Variations in general appearance, however, induced by a change of colour in the feathers themselves, are of comparatively rather unusual occurrence.

Vol. 9 (1836), pp. 505–514: ART. I. *Further Remarks on the Affinities of the feathered Race; and upon the Nature of Specific Distinctions.**

(*Concluded*)

THERE are two modes of estimating the typical standard of a natural group of species. There are two distinct principles upon which, according as we desire to frame a system upon obvious and tangible characters, or upon the physiological relations, that is the true affinities, of species, we may arrive at very different conclusions as to which form is the more worthy to be considered the general type of the whole. I have said (p. 406.), that it is not unusual to find certain characters, which, in typical forms, are comparatively little noticeable, carried out, in particular modifications of those general plans of structure, to a much greater extent; in exemplification of which may be adduced (as a familiar, though not, perhaps, the most striking instance) the great developement of the bill laminæ in the shoveller genus (Spathùlea); also the perfection of the bill, as a groping instrument, and as a sentient organ, in the snipes and woodcocks; in consideration of which, many naturalists, esteeming these to be the most characteristic peculiarities of their respective major groups, have therefore adopted the above-named genera as the types of extensive natural families. Now, this may be very well in a confessedly artificial system; but, where *affinity* is to be considered the basis of classification, these forms will rather have to be arranged as ultimate modifications of their respective types, in a particular direction. They are neither of them *centres of radiation* (at least, to any extent), such as the form of *Â*nas *B*óschas undoubtedly is in the duck family; and such as the godwits (Limòsa) at least approximate to be in the natural family to which the snipes appertain. *C*órvus and *Á*rdea are good examples of thoroughly typical forms, which, modified in every possible way, radiate and ramify in every direction around; and so, also, is *M*érula, and that central division of the finch family to which the term *C*occothraústes has been given. All these graduate, through a series of species, into almost every form referable to their respective groups; and such must necessarily be the case with the more characteristic examples of every general plan of structure, of whatever value. Typical forms, in fact, as a leading rule, are merely those examples of each plan which are the least bound, as a matter of necessity, to particular localities; and we accordingly find them (I mean the *forms*, rather than species) to be of comparatively general distribution; whereas the more one of these plans is modified to suit any particular purpose, the more completely it is adapted to any peculiar sort of locality or mode of life: the *adaptation*, of course, implies a receding from the general, or central, type; and the species may therefore, in technical language, be termed *aberrant*, even though its deviation be a farther developement of characters peculiar to its group.

It is clear that we must either admit this, or allow of a multiplicity of primary types to every natural family, to every group of species framed upon the same general or leading plan: the which must necessarily lead to such gross violations of *affinity* as the adoption of *P*hasiánidæ and

* Curiously enough, however, the song linnet's changes of tint do not, to the slightest extent, ever take place in captivity.

† Inspection of a considerable number of ptarmigans, at different seasons, induces me to dissent from the general opinion, that the time of moulting in these birds is confined to no particular period.

* I wish the reader to excuse, for the present, my not entering into detail on the moultings of birds, as, just now (this being the chief season for moulting), I have some opportunities of considerably extending my information on the subject.

*T*etraónidæ of the Quinarists as separate and independent natural groups, equivalent and equally distinct from each other, as are either of them from the two contiguously ranged families, *C*olúmbidæ and *S*truthiónidæ; and this, too, while the very genera assumed to be typical of them, *T*étrao and *P*hasiànus, are allied so nearly as to hybridise together.

This is so interesting a subject, that a few additional remarks may be well devoted to its elucidation. Assuming a type to be merely the abstract plan upon which a certain number of species are organised, the said plan being variously more or less modified according to the purpose for which a species was designed, it certainly does not necessarily follow that organisms simply illustrative of the mere plan should have been created, seeing that all creatures are obviously framed in direct relation to their indigenous haunts, and not as mere counterparts of one another. At the same time, wherever an extensive array of species are organised upon one general plan of structure, there cannot but appear some tendency to converge to a general centre; a tendency becoming more obvious as we recede from the extremes, whereupon there is usually a marked increase in the number of species exhibiting the same characters, till at length a sort of focus presents itself, as a central genus, the proper limits of which completely baffle the ingenuity of naturalists to define, inasmuch as the various species it comprises blend with, and continuously radiate into, the immediately subordinate divisions.

In illustration, it is sufficient to mention the already cited genera, *C*órvus, *Á*rdea, *M*érula, and what should be *F*ringílla, but which is at present better known as *C*occothraústes.

Take either of these divisions, and observe how difficult it is to define its (artificial) boundaries; how unbroken is the concatenation of species which links them with what are simply aberrant modifications of their structure, but which naturalists have been accustomed to consider as separate and distinct generic divisions. Let us, for a moment, consider *M*érula. Some naturalists try to separate the spotted-breasted thrushes from those in which the markings are less broken; and, unquestionably, taking the extremes, there is much diversity; but there is quite as much between the different spotted-breasted thrushes. In either case, however, where can the dividing line be drawn? The blackbird has, when young, a spotted breast; and, in fact, the characters of its nestling plumage alone forbid its alienation from the spotted thrushes. Where, indeed, can we trace the line of separation between *M*érula and *P*hilomèla even? And does not also the same form, in another of its various gradations, merge imperceptibly into Petrocíncla, and thence into the different saxicoline genera, *E*rýthaca, *P*hœnicùra, and Siàlia? one ousel (Petrocíncla, or, rather, Geocíncla *Gould*) being absolutely a large robin, another a great redstart, while a newly discovered species of Siàlia has the markings, and many of the characters, of a Petrocíncla? But it would be endless to follow *M*érula into all its diversified ramifications. I shall content myself with tracing the series into *P*hilomèla, which is at once conclusive as to the true affinities of the latter.

To be brief, then, we observe in the European song thrush a deviation from the gregarious character of its nearest British congeners, and an approximation to the style of marking in the transatlantic species. *M. m*ustelìna of North America is yet more solitary, and does not even associate to migrate; in this resembling *P*hilomèla, which its habits (as described by all who have observed them) accord with in almost every particular: still it retains a good deal of the true *M*érula; and it builds a plastered nest, like our thrushes. In *M.* solitària the size decreases, the number of breast spots are diminished, the tarse is much lengthened (a character which commences in *M. m*ustelìna), the nest is constructed without plaster, and even the tail is rufous, as in the nightingales. *M.* Wilsòn*ii* has the very form of *P*hilomèla, and is the smallest bird that ranks in *M*érula: its breast-spots are but very few, and these appearing as though more than half obliterated; its habits are exactly those of *P*hilomèla, and so is its nidification; and its bill hardly differs from that of our nightingale. The great nightingale of Eastern Europe has, according to Bechstein, an obscurely spotted breast, also a stronger bill than the common species; it is described to be more omnivorous in its diet, and, consequently, to be more hardy in a state of confinement: even its size implies an approach to the small *M*érulæ. And, lastly, look to the nestling plumage of the song nightingale (*P. L*uscínia), a character of no small importance in indicating the true affinities of birds, and we at once perceive its true station in the system, and how distinct it is from those

forms with which (apparently from its mere size) it has been hitherto associated: it is, in fact, an ultimate modification of the type represented by *Mérula*.

Let us now compare, for a moment, the extremes of the genus *Mérula*; let us bring together the large mottled-backed thrushes of the East, and those diminutive solitary thrushes of the West. Does it seem proper that these should rank in the same minimum division? And yet how are they to be separated? How can the former be divided from those of the missel thrush form*; the last-mentioned from the fieldfare group; the fieldfares from the merles, or from the congeries to which the song thrush belongs, which last we have seen to inosculate with the nightingales? How, in like manner, can we divide the genera *Ardea* or *Córvus*?

It indeed appears that, in these very typical genera, there is a sort of clustering of species (if I may be allowed the phrase) about the centre of radiation. In *Ardea*, and *Córvus* particularly, the central species become extremely difficult of determination; if, indeed, in some instances, the proximity is not even too close for detection. How nearly do some of the typical crows resemble! Upon the most scrupulous and minute comparison, C. L. Bonaparte was unable to discern the least difference between specimens of the European *Córvus Corône* and the common crow of America; and he consequently infers their identity: yet who that attentively peruses the various descriptions of the latter, that considers well its marked gregarious habits, and the diversity of its note from that of our crow, can for a moment coincide with him in opinion? Look again, to the raven, that formerly was considered a bird of universal distribution, as was also the snipe. First, the African species must be detached, as Le Vaillant's description of it should long ago have indicated; for we find that, independently of the differences in the bill, and certain particulars connected with its plumage, the proportionate size of the sexes is the reverse of that of the European species, as Le Vaillant himself ascertained and published. Then the beautifully glossed raven of the Brazils is obviously distinct; also the raven of the South Sea Islands, and, there is reason to believe, that of India. And what if these can be proved to be separate species, by fixed and constant structural distinctions; do they not show how nearly species may resemble, and point to the almost inevitable conclusion, that, in some instances, there may possibly be no means whatever of knowing them apart?

How vastly important is this consideration when we contemplate the natural productions of America! Many years have now elapsed since the genius of Buffon suggested the capital proposition, that there is no absolute specific identity between any organism of the Eastern and Western continents, with the exception of those which inhabit very far to the north. All subsequent investigation has gone to prove the forcè and acumen of this sterling remark; and the number of species (exclusive of evident stragglers) supposed to be common to the two continents has been gradually diminishing, on more careful and exact comparison from that time to the present. In fact, I think we may now fairly venture to assume, putting aside stragglers, that those species alone are satisfactorily identical in the two continents which are distributed over the whole north of Asia, and may be looked for on the north-western coast of America. Very lately, the American scaup (pochard) was found, on comparison, to be distinct from that of Europe, although the difference almost wholly consists in the obliquity of its wing spectrum; a character which, however, proved to be fixed and constant. Had there not been this diversity, the two species would have been, of course, equally distinct: yet how should we have discriminated them apart? The barn owls of the two continents, which are now believed (and on good grounds) to be distinct, are even more similar.*

Equally close resemblances obtain in other departments of the zoology of Europe and North

* On examining a series of specimens of *M. viscívora*, it will be seen that many exhibit conspicuous traces of the mottling on the upper parts, particularly on the rump, and that space covered by the tertiary wing feathers; also on the upper tail-coverts; the latter being broadly edged with a paler tint, which in the former occupies the centre of each feather. Here we have an interesting illustration, in the plumage of birds, of the gradual developement of a particular marking as we recede from the type. There is also a regular increase in the size of the bill, which, in the missel thrush, is rather small. I am unaware that the form of *M. vària* and its immediate congeners is further modified, but suspect them rather to be the extreme ramifications in that direction.

* From subsequent investigations, I am enabled greatly to strengthen the above position. Minute comparison of a considerable number of American specimens with examples of what have been hitherto esteemed the same species in Europe has brought to light distinctions as

America, and particularly in the insect tribes: many butterflies, for instance (as several of the Colìades), from the opposite shores of the Atlantic, being only to be told apart by the slowly acquired practical ken of the entomologist. The natural productions of Japan, again, in many instances, present the most astonishing similitude to those of Europe; yet they exhibit characters which cannot be well reconciled with variation, however unimportant in themselves, because they are distinctions which climate or locality are not in the least likely to bring about. Besides, supposing the latter, we should not only expect to meet with specimens in every degree intermediate, but to find the same species equally flexible to circumstances in other places, which is not the case.

In ornithology, the jay and bullfinch of Japan may be selected from among numerous other instances; the former differing only from the European bird in the greater developement of certain markings about the head, and the latter presenting no other difference than the much paler, or roseate, tint of its abdominal plumage. Taking a series of species, we have every grade of diversity, from the obviously distinct Japanese peafowl (*P*àvo mùticus), to the mealy linnet, which apparently, differs in no respect from that of Europe. In a specimen of a pettychaps from the same locality, the only difference I could perceive from our common Sýlvia *T*róchilus on very minute inspection, consisted in a peculiar slight curve at the extremity of the upper mandible: still we know how nearly two British species of this genus resemble, and yet how very diverse are their notes. Perhaps the song of the Japanese pettychaps is dissimilar from that of either: at any rate, a dry skin is hardly sufficient on which to found a definite opinion.

Of course, all these various facts lead us to the important consideration of, What is a species? What constitutes specific distinction? To which the only rational reply appears to be (and even this is quite incapable of probation), Beings derived from a separate origin. For it appears that hybridism, after all, is but an uncertain guide, however satisfactory in particular cases; there being much reason to conclude, from a general survey of the facts recorded, that, as the degree of fertility in hybrids (paired with individuals of pure blood) varies according to the degree of proximity in the parent species, the possibility of mules being produced at all existing only within the sphere of a certain affinity; so, on the other hand, when the parent species approach so nearly as some that I have had occasion to mention, their mixed offspring would be almost equally prolific, hybrid with hybrid. This is, at least, stated of all the members of the genus *B*ós; and most naturalists concur in the opinion, that our common fowls are derived from the blending of a plurality of species. Certainly, if the analogy of plants can be admitted, the fact is in so far settled; for I know many hydrid plants which of themselves yield fertile seed in abundance: the mixed produce, for example, of the Calceolària purpùrea and C. *p*lantagínea; the former a half-shrubby species, the latter herbaceous.* A variety of additional instances could be enumerated. Hybrid plants, however, are equally sterile with mule animals, if the parent species are not very closely allied.

It is to be hoped that, ere long, the experiments of the Zoological Society will have solved this curious and important problem. Already some highly interesting and complex hybrids have been obtained under their management.†

I have found it to be a very general opinion among naturalists, that specific diversity must of necessity be accompanied by some perceptible difference in the structure. To this I cannot

curious as, in some instances, they were unexpected. Thus, the osprey of North America may be always told, by trivial though constant characters, from that of Europe; and the same obtains with a variety of other species considered identical.

* It is greatly to be wished that horticulturists would not name their hybrid plants in the same manner as genuine species; the confusion thus already induced in many genera being quite inextricable. Surely they could find some other mode of denoting them.

† Since writing this, I have ascertained the fact, that the mule progeny of the *A*'nser *c*ygnöìdes, coupled with the domestic goose, breed freely with one another; and have seen an individual of which both the parents were hybrids. We do not, indeed, know the wild stock of the domestic goose; but, certainly, no one would dream of referring it to *A*. *c*ygnöìdes. As Mr. Jenyns well observes, the common gander, after attaining a certain age, is always white, a character which, it may be remarked, is in accordance with the snow goose (*A*. hyperbòreus) of North America, a species obviously distinct. Let it be, however, borne in mind, that, in every known instance, intermixture of species is solely induced by man's agency; even the mules that have been found wild between *T*étrao *T*ètrix and *P*hasiànus cólchicus: for instance, White of Selborne, who figures one of these, states, in one of his first letters, that black game was formerly abundant in the neighbourhood, but that only one solitary grey-hen had been seen for many years: such an individual might be expected to breed with a cock pheasant.

accede, until I hear of a sufficient reason why it should be the case. We perceive every grade of approximation, till in the shrews, for instance, a slight diversity in the form of one of the back teeth comprehends all the difference. It is therefore presumed that, as so very trivial a deviation cannot be said to affect the animal's habits, for what purpose, then, does it exist, save to intimate the separateness of the species? But, surely, it will not be contended that species were created with a view that man should be able to distinguish them! Surely, differences were not imposed merely to facilitate the progress of human knowledge! Is it not much more rational to conclude, that, as great differences in the structure import corresponding diversities in habit, so, by the same rule, minor differences also imply an equivalent diversity in degree? Let us, again, consider the American and European crows: here it would seem that specific diversity is unaccompanied by any structural deviation.

Of course, it is hardly necessary to hint the importance of these facts to geological enquirers: they intimate the excessive caution requisite ere we can venture to identify the fragments of an organism, when even existing species, in many instances, are not, probably, to be told apart. It must be admitted that they warrant a good deal of scepticism as to many of the identifications that have been assumed.*

But to return now to the four typical genera, which have led to the above lengthy digression. I certainly do not conceive it necessary that there should be, in all instances, an unbroken gradation into the subordinate forms, similar to that from *M*érula into *P*hilomèla; for it is evident that the affinities of *P*hilomèla, and its relations to the thrush genus, would be the same, were there no intervening examples. Still it is reasonable to suppose that, generally speaking, such series would occur; not, however, for the mere abstract purposes of arrangement, but because there are grades in localities and modes of life. That there should be species variously modified upon any particular plan of structure, and that the deviation should be greater in one instance than in another, of course implies radiation from a general centre; and the very circumstance that the same characters are more developed in one species than in another, necessarily also occasions a gradation in the particular direction, which may happen to be more or less regular, according as circumstances (*adaptive* relations) require. That there should be a slight break, for instance, in the series where the fringillidous type is modified into Lóxia, is perfectly consistent with the nature of the deviation; but the true affinities of the crossbills are, nevertheless, equally recognisable, and the same may be said in other cases where the hiatus is much more considerable.

And here it will not be out of place to say a few words upon the terms *perfection*, *degradation*, and the like, as applied to natural productions. Let it be borne in mind, that, although every species is equally and wondrously perfect, even to the most trivial minutiæ, in reference to the office for which it was designed, still, if we desire to cite an instance wherein the adaptation, if not more *perfect*, is, at least, more obviously remarkable and extraordinary than in another, it is to *aberrant* species, rather than to the central or typical exemplifications of a general plan of structure, that we must direct attention; inasmuch as the former exhibit those modifications of that plan, those adaptations to a peculiar mode of life, which are the most calculated to excite our wonder and admiration. Such forms as Lóxia and Recurviróstra are sufficient illustrations of the position. There, perhaps, would be no objection to the word *degradation*, understood strictly in a classical sense; but, when we consider its popular, its *English* meaning, in which alone it will be apprehended by an extensive class of readers, no term should be more carefully avoided: the most *degraded* species absolutely happening to be those which are the most worthy our especial admiration.

The difficulties of classification arise from the necessary fact (obviously necessary when we consider the adaptive relations of species) of there being successive centres of radiation; the different modifications of a leading plan of structure radiating in their turn, and thereby constituting an irregular series of subordinate types, of every degree of value. Thus, the starling type is comprehended in the omnivorous or corvine plan of structure, and, in its turn, comprises others of less importance, upon all of which may be organised an indefinite number of species, diversely modified to suit a variety of localities, and often *approximating* in external appearance to species framed upon other general types of structure, wherever they are alike modified to perform the same office in the

* For some facts bearing upon this subject, see Art. VII.—*Ed.*

adaptive system: such *approximation*, however, by no means inducing an additional degree of physiological *affinity*.

Before concluding this, I must call attention to another point worthy of consideration. To recur again to the four typical genera we have all along been considering, and which, of course, it is most satisfactory to revert to in every instance, it appears that the central species, for the most part, exhibit a marked increase of size, being generally about the largest of those framed on their respective plans of structure. I do not say that this obtains in every instance, but still it is so general as to be quite worthy of attention; and the rationale of it appears simply to be, that, as typical forms are more adapted for general distribution, and better calculated for finding subsistence in a variety of localities, than those modifications of them which are organised expressly for peculiar places only, we must infer that an increase of stature would, as a general rule, be incompatible with the well-doing of aberrant races; or, to put it inversely, that beings of comparatively large size require to be less partial in their adaptations; that (their wants being greater) they should not be too much confined to particular places for the needful supply of food. However, this is a rule so broken into by exceptions, and so entirely dependent on the character of the particular adaptation, that, though obvious enough in the main, it is much more likely to meet with assent than demonstration. Certain it is, that, in very many groups, the largest species are among the most centrally typical. Witness, by way of example, the woodpeckers and the parrots.

In fabricating an arrangement according to the natural method (i.e. based on the true *affinities* of species), we cannot be too much impressed with the consideration that organisms must be ever regarded in their totality; that no one structural character can be expected to hold in all instances, however important in particular cases. We have only to consider the fact, that, in a natural group, it is but the same leading plan of structure which is so variously modified, each organ, in its turn, being adapted differently to diverse circumstances; and we perceive how valueless are the arbitrary characters of those who try to frame artificial systems. Even the dentition of the Mammàlia, so paramount in the majority of cases, becomes quite a secondary means of distinction in the Marsupiàlia; and the structure of the bill in birds, so important and corresponding a character in most instances, yet loses almost all its value in the Certhìadæ. Unquestionably, all the yoke-footed tribes are very nearly related by affinity; yet how discordant are they in the details of their structure! A single, and comparatively trivial, resemblance in the organisation of the foot becomes, in this instance, a character of the very first importance.—*Tooting, Surrey, August* 13. 1836.

Vol. 1 (n.s.) (1837), pp. 1–9: ART. I. *On the Psychological Distinctions between Man and all other Animals; and the consequent Diversity of Human Influence over the inferior Ranks of Creation, from any mutual and reciprocal Influence exercised among the Latter.*

THERE is not, within the wide range of philosophical enquiry, a subject more intensely interesting to all who thirst for knowledge, than the precise nature of that important mental superiority which elevates the human being above the brute, and enables man alone to assume the sway wheresoever he plants his dwelling; and to induce changes in the constitution and adaptions of other species, which have no parallel where his interference is unknown.

I am led to offer a few remarks on this subject, by observing continually that the instinctive actions and resource of animals are attributed, most inconsiderately, to the habitual exercise of their reflective faculties; often where it is utterly and manifestly impossible for them to have observed facts whereon to base those inferences, which alone could have led them, by an inductive process, to adopt the course we find them to pursue. I am perfectly aware that the word "instinct," by not a few, is denounced as a mere cloak for ignorance, as a sort of loophole through which to escape from a rational explanation of phenomena; but, with all deference to those who advocate this over and above refined notion, I venture to maintain that it has a very definite signification, to express which no other term could be substituted: it implies an innate knowledge, which is not, like human wisdom, derived exclusively from observation and reflection, and to assign a secondary cause for which is clearly impossible; wherefore it savours rather, I conclude, of sophistry, to affect to be dissatisfied with any non-misleading expression, which is currently understood to denote it.

Place a juvenile chimpanzee in presence of

one of its natural enemies; a python, or one of the larger Fèles; and it "instinctively" recoils with dread. But does a human infant evince the like recognition? Here, then, is a fundamental distinction at the outset.

Not only, too, do brute animals (as remarked by White of Selborne) attempt, in their own defence, to use their natural weapons before these are developed, but they intuitively understand the mode of warfare resorted to by their brute opponents. They know, also, where the latter are most vulnerable, and likewise where their concealed weapons lie. Observe the deportment of a rat that is turned into a room with a ferret: see how artfully he guards his neck against the wall, instinctively knowing that there only will his enemy fix.* Notice, on the other hand, the wondrous accuracy with which the Mustéladæ constantly wound the jugular vein of any bird or quadruped they attack. Witness a thrush that has captured a wasp, first squeezing out the venom from its abdomen, before it will swallow it. Or see a spider trying to shake off a wasp from its web, and, failing to do so, proceeding to cut it clean away. Can aught analogous be traced in the actions of inexperienced man? Whence, then, the acquired knowledge on which these animals could reason to act thus?

The distinction is, that, whereas the human race is compelled to derive the whole of its information through the medium of the senses, the brute is, on the contrary, supplied with an innate knowledge of whatever properties belong to all the natural objects around, which can in anywise affect its own interests or welfare†; a sort of intimation, by the way, that all the inferior races pertain to some general comprehensive system, all the components of which have a mutual reciprocal bearing, and to which man only does not intuitively conform nor constitute a part of, except in so far as his bodily frame is of necessity subject to the common laws of matter and of organisation.

In every other species, each individual comes into the world replete with "instincts," which require no education for their developement. A kitten reared by hand, or a bird raised from the nest, have the same language*, the same leading habits, as the rest of their species, but little, if at all, modified by change of circumstances. A kitten watches at a mouse-hole, though it has never seen a mouse; the squirrel proceeds by the easiest possible method to get at the kernel of its first nut, by invariably scraping, with its lower incisors, at the softer end, which it instinctively turns in its fore paws to the proper position; and the wasp, crawling forth from its pupa envelope, immediately commences feeding the neighbouring larvæ. The human infant, too, applies instinctively to the breast, like the young of all other mammalians; but, unlike those, it has to attain all its after-knowledge through the medium of its external senses. It looks to its nurses, and those about it, for information; and these are capable of so communicating their attainments, as very materially to assist the infant learner in its acquisition of knowledge. It is preposterous to assert the contrary, as has been done; or to pretend that it rests on the choice of the infant whether or not it will learn.† Practically, it cannot help doing so; and it is equally monstrous to deny that human beings can so communicate the results of their experience, that, with what in addition is ever accumulating, each generation must necessarily rise in knowledge above the last. Unless the faculties were to be much deteriorated, it could not be otherwise. Who can pretend to deny the excessive influence of every generation upon that which immediately succeeds it; the influence both of precept and example? Imagine it possible for those of the present day to refuse to instruct; and what would then be the consequent condition of their

* Even more: he will contrive so to place himself, if practicable, that the ferret's eyes shall be dazzled by the light.

† The indirect effects of human agency on this intuitive knowledge of brutes will be considered presently. In no way is the deterioration more evident, than in domesticated animals poisoning themselves by feeding on that which, in a wild state, they would instinctively reject.

* The reader may probably be disposed to refer this to the structure of the vocal organs. But, admitting to the full extent the reasonableness of this view, it must be borne in mind that the smaller birds have great power of modulation; and it is a certain fact, that, although in most species the song is purely innate, there are many (as the song thrush and nightingale) in which it is, for the most part, acquired; as is proved by the fact of these never warbling their wild notes when reared in confinement, except they have had opportunities of listening to the proper song of their species; which latter, it may be remarked, they imitate much more readily than any other. I do not consider, however, the music of a bird to be so much the language of its species, as those various notes and calls by which different individuals commune together; and these I have never known to vary under any circumstances.

† See *Mag. Nat. Hist.* (old series), vol. ix, p. 612, l. 3., et seq.

offspring? Apply the same test to any other species of animal; and in what measure would the progeny be affected?

I wish not to defend the untenable doctrine, that the higher groups of animals do not individually profit by experience; nor to deny to them the capability of observation and reflection, whereby to modify, to a considerable extent, their instinctive conduct: neither do I assert that the human race is totally devoid of intuition, when I see the infant take naturally to the breast; when I perceive the force of the maternal attachment, and the ardour of the several passions: which latter, however, are, of course, but incentives to conduct common to both man and animals. In only the human species are the actions resulting from them unguided by intuitive knowledge. All I contend for is, that the ruling principle of human actions is essentially distinct from that which mainly actuates the brute creation, whence the general influence of the two is diverse in kind; and I mistake if I cannot establish the position.

The brief period that elapses before most animals are compelled to perform the part allotted to their species, precludes the possibility of their attaining sufficient information from external sources, and renders, therefore, the possession of a substitute for knowledge so obtained absolutely requisite. We have already seen that such a substitute is not wanting; but that all the knowledge necessary to insure their general welfare is intuitively conferred on the brute creation. Their various actions, in wild nature, are consequently based on this innate knowledge; which, being the same in every individual of the same species, in a natural state (that is, as completely uncontrolled by those peculiar changes of condition which man only, the exception of all other animals, can bring about), superinduces a normal uniformity of habit throughout the members of a species, which is rarely modified to any considerable extent by individual experience. Now, this uniformity is at variance with what reasoning from observation could possibly lead to; and, as it extends even to the resource of creatures of the same species, when driven to emergency, we have herein sufficient intimation that their wiles and stratagems, however consonant with what reasoning from observation might suggest, may nevertheless be purely instinctive, perfectly unalloyed with any wisdom resulting from experience.

To ascend from illustrations the least equivocal, let me here cite the nidification of the feathered tribes. Who, that considers the wonderful fact, that not only genera, but even species, of birds are for the most part distinctly indicated by their nests, can fail to recognise in this the operation of a principle essentially distinct from that which we understand by the word *reason?* which latter, in human beings, can of course, be only the result of observation and reflection.* We observe a similar marked uniformity in the fabrics and operations of all animals of identical species (man only exempted), endless examples of which will instantly recur to the reader in the insect tribes; and, if we consider the beaver, and others of the higher grades of animals which join their labours for mutual advantage, or are otherwise remarkable for what has thoughtlessly been deemed *their* ingenuity, the same truth will be found still to hold just as obviously apparent, and forbids us to attribute their proceedings to aught else than the dictates of intuition.

It is most commonly, however, in the resource of brute creatures, the wisdom they display in their expedients, that unreflecting persons fancy they discern the proofs of intellect identical with human; but, even here, this does not necessarily follow; for it is sufficient to refer to the cases which I commenced by detailing, to be assured that Providence has conferred instinctive wiles on animals as a resource against contingencies; the legitimate actions resulting from which according, perhaps, with what reason might dictate in like circumstances, we are therefore apt to conclude must necessarily have been induced by reasoning. To illustrate what I mean, let me adduce the simulation of death practised by so many species, with intent to weaken the instinctive vigilance of their foes or prey. (That another animal, it may be remarked, should suffer itself to be thus duped, is most probably a result of acquired experience.) A cat has been seen to feign death, stretched on a grass-plot, over which swallows were noticed sailing to and fro; and by this ruse to succeed in capturing one which heedlessly approached too near it. The fox has been known to personate

* Brutes appear to reason from innate knowledge, and this in proportion to the developement of the cerebrum; but the extreme promptitude of their expedients (as will be shown), in cases of emergency, often prohibits us from inferring that these can be the result of aught else than intuitive impulse.

a defunct carcass, when surprised in a hen-house; and it has even suffered itself to be carried out by the brush, and thrown on a dung-heap, whereupon it instantly rose and took to its heels, to the astounding dismay of its human dupe. In like manner, this animal has submitted to be carried for more than a mile, swung over the shoulder, with its head hanging; till at length, probably getting a little weary of so uncomfortable a position, or perhaps *reasoning* that its instinctive stratagem had failed in its object, it has very speedily effected its release, by suddenly biting. The same animal has been known, when hunted, to crouch exposed upon a rock of nearly its own colour, in the midst of a river, and so to evade detection by its pursuers; and we perpetually hear such cases brought forward as decisive proofs of its extreme sagacity. However, as regards the latter instance, will not a brood of newly hatched partridges instantly cower and squat motionless at sight of a foe*? and, as concerns the former, do we not find that many beetles, though just emerged from the pupa state, will simulate death every bit as cleverly as a fox or corn-crake? Whence it surely follows that there can be no occasion to attribute the act to a reasoning process in the one animal, any more than in the other.

It would be unnecessary to enter here into any details on the obvious correlativeness of the dominant instincts of animals to the mode of life most congenial to their constitution, to remark on the mutual relations of habit and structure, and the exquisite adaptation of structure to locality. Hence, the natural habits of species of necessity bear reference to their indigenous haunts, as manifestly as their structural conformation. Thus, the elephant, which, like the other great Pachydérmata, affects the vicinity of rivers and marshes, delights to relax its rigid hide in the stream; and afterwards covers it with a thick plastering of mud, probably to retard its too rapid desiccation: the which has been deemed an incontrovertible proof of its reasoning from observation. A young robin, however, the first time that it sees water, will, if it be not too deep, fearlessly plunge in and wash; and a young wren or lark will avail itself of the earliest opportunity to dust its feathers on the ground, the exact purport of which is not yet definitely understood. If, therefore, the latter be thus obviously instinctive, what reason have we to esteem the former otherwise? The uniformity of all these habits and propensities, in creatures of the same species, tends rather to intimate that in neither case are they the result of reasoning.

To infer reflection on the part of brutes, as many have not scrupled to do, as the motive for whatever in human actions could only be the result of reasoning, one would imagine to be too palpable a misapprehension to need serious consideration; yet some writers have gone so far as to attribute forethought to the dormouse, and other species which provide instinctively against the winter season.* Perhaps it might be deemed a sufficient overthrow to this most shallow notion, to call in mind the migrative impulse; to enquire how the untaught cuckoo (raised by permanently resident foster-parents) could reason that in another clime it should escape the rigours of a season that it had never experienced? But herein we have an additional principle involved, which will require a separate consideration. Proceed we, then, to examine into the presumed sagacity of those provident creatures, as the ant and harvest mouse, that habitually lay up a store for future need, and even provide against all possible injury from germination, by carefully nibbling out the corcule from each grain. Can any thing be more truly wonderful as a matter of instinct? All instincts are, indeed, equally wonderful. But it would certainly be even more extraordinary, if every member of these species were to be alike induced to pursue the same course by a process of reasoning. The following anecdotes will suffice to probe the intellect of these animals:—I have a tame squirrel, which, though regularly fed all its life from day to day, nevertheless displays the intuitive habit of its race, in always hoarding the superfluities of its food. Now, in its mode of effecting this, a superficial observer might fancy that he discerned a fair share of intelligence. Carrying a nut, for instance, in its mouth, it scrapes a hole with its fore paws in the litter at the bottom of its cage; and then, after depositing its burthen, scratches together the hay, or whatever it may be, over it, and pats it down with its paws. Moreover, it

* I have noticed a remarkable instance of this, on placing down a stuffed polecat before a yound brood, tended by a bantam hen. A rail or gallinule will also run towards a bank approximating to their own colour; and, if no hiding-place be discoverable, will insert the head into a crevice, and, remaining motionless, suffer themselves to be taken. Of this I have known many instances.

* See *Mag. Nat. Hist.* (old series), vol. ix. p. 611. l. 15.

never fails to remember the spot, and will occasionally, when not wishing for food, examine the place to ascertain whether it be safe. But mark the sequel. I have repeatedly seen the same animal act precisely thus on the bare carpet, and upon a smooth mahogany table; yes, upon a table I have frequently seen it deposit its nut, give it a few quick pats down, and finally thus leave it wholly unconcealed.† The tits (Pàri), also, evince a like propensity of hiding food, one of their many resemblances to the Córvidæ; and a tame marsh tit that I once possessed used habitually to drop the remainder of the almond, or piece of suet, that he had been picking, into the water-glass attached to the cage, although he never could thence reobtain it, and though his water was thus daily rendered turbid. I could narrate analogous instances without number.

Thus it plainly appears, that the instinct of each animal is adapted to its proper sphere; for the mode of life it was destined to pursue, and for that only. With this restriction (if such it can be called), it is in each case perfect. The actions of every creature uncontrolled and uninfluenced by man are invariably such as tend to the general welfare of its species; sometimes collectively, however, rather than individually* (whence we hear of what have been termed "mistakes of instinct"). They evince superhuman wisdom, because it is innate, and, therefore instilled by an all-wise Creator. Indeed, the unpremeditated resource of animals, in cases of emergency, is oftentimes decidedly superior to that of man; and why? Because they need not experience for their guide, but are prompted to act aright by intuition.

In wild nature, this inborn knowledge of brutes thus abundantly sufficing for the attainment of all they require, there is, in consequence, but little to stimulate the exercise of their reflective faculties; and, accordingly, their general agency may be considered as passive, in effect analogous to the operation of the laws of matter. Even the "half-reasoning elephant," in the wild woods, is but a creature of unreflecting impulse, to an extent which wholly dissevers it from all community of mental attribute with the lowest grade of mankind. Witness the subdued tamed animal, which, travelling along its accustomed route, suddenly broke loose from its attendants, affrighted at the near yell of a tiger. At once its former submissiveness was forgotten: it rejoined the wild troops, and was again a free tenant of the jungle. Years rolled on, and it was retaken by the ordinary method. The sight of the stakes never sufficed to awaken its recollection; nor did the mode employed to secure it when entrapped. It was sullen and savage, and acted in nowise differently from its companions. By chance, however, its former keeper was present, who, after a while, recognised the animal. He gave the word of command, and it was instantly submissive; all traces of its wild nature suddenly dissipated; its previous habits were forgotten; it was once more a reclaimed animal, and suffered itself to be led tractably to its place of confinement.* Would a rational being have acted like this elephant?

† It is no new remark, that rodents are much below the Carnívora in the scale of intelligence; a necessary consequence of their inferiorly developed brain. Yet few animals have more instinctive cunning and resource than the common rat: but this is not intellect, of which it displays scarcely any when brought up tame; a condition which, as will be shown, is sure to call forth the non-instinctive intelligence of animals. Judging from my own observation, I should say that the rat was mentally superior to the house mouse, but inferior to the squirrel; which, in its turn, must yield in intellect to the hare; and, I believe the comparative structure of their brains will be found in accordance.

* As in the contests of animals for the other sex; whence it follows that the breed is chiefly transmitted by the most stout and healthy.

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(*Continued*)

MAN only, by the habitual exercise of his reasoning powers, appears to be competent to trace effects to their remote causes; and is thereby enabled to recognise the existence of abstract laws, by assuming the guidance of which he can intentionally modify their operation, or, from observation, convert them to a means of accomplishing his various ends. It is thus he wields the principle of gravitation; and it is thus, from studying the inherent propensities and consequent habits of other animals, that, by judicious management, he contrives to subdue their instincts (as in the case of the elephant just mentioned), or to direct their force towards affecting other purposes than those for which

* I shall have occasion to revert to this presently.

they were more legitimately designed. But a more remarkable sequence of human interference is, that, by removing animals from their proper place in nature, and training them to novel modes of life, wherein the field for the exercise of their original instincts becomes much limited, their faculties of observation and reflection are, in consequence, brought more into play, in proportion as the former are rendered inefficient; till, at length, experience not unfrequently supersedes innate impulse as the main spring of their actions; more especially where they have become attached to a human master, and pass much of their time in his society. Yet even here the difference between man and brute is still manifest, in the transmission of acquired knowledge by generation, in the offspring inheriting as innate instincts the experience of their parents*; so that the tendency of brutes is ever to become slaves to a certain amount of intuition, rather than beings dependent on their own intelligence.

And here we recognise a fundamental principle of domestication, which is only gradually induced to any extent through a series of generations. Thus the elephant, though tamed, is not domesticated, for every individual is separately captured in a wild state; and we have seen that, when one of these returns to its proper haunts, its natural instincts having been only for a time subdued and rendered subservient (not eradicated), these have again become the incentives to its conduct, to the exclusion of those reasoning faculties which had only been excited into action under circumstances adverse to the efficient operation of the former. Far otherwise is what we observe in animals truly domesticated: witness the opposite conduct of even the newly hatched progeny of a wild and domestic duck, though incubated by the same bird. But here a question arises, that, as numerous instincts in domestic animals, which are now hereditary, are known to have been originally habits superinduced by man's agency, to what extent may not all the innate propensities and consequent habits of animals have originated in the acquired experience of their predecessors?

As with all other subjects, we must trace the series upward from its more simple phases. In the insect world, we discern the most complicated instincts; modes of procedure of which the consummate wisdom excites our admiration and amazement, and bearing reference to a future generation, in beings which are but creatures of an hour. Can it be supposed possible that the progenitors of these derived their habits from acquired experience, and transmitted them as innate instincts to their posterity? Here we must ascend to a higher source, which, being admitted, the marked uniformity, also, of the instinctive habits of all wild animals, before commented on, warrant us in concluding that these were from the first imprinted in their constitution, and may, therefore, be legitimately esteemed as forming part of the specific character.*

The tendency of human influence is every where to destroy whatever conduces not to man's enjoyment, as superfluous, and only cumbering the ground; but to secure, by every means the reasoning faculties can suggest, a due continuance and never-failing supply of all that tends to the gratification of our species. Brutes, on the contrary, evince indifference to whatever does not immediately concern them; and although, practically, their influence upon their prey is for the most part decidedly conservative, yet they individually continue to destroy without reflection, and endeavour not, by any forbearance, or plan resulting from reasoning, to insure the perpetuity of their provision. That the squirrel or jay should instinctively plant acorns is, of course, nothing whatever to the purpose: we have already tested the sagacity of the former animal; and we know that the latter, removed from its proper office in wild nature, will bury a bit of glass or clipping of tin as carefully as it does a seed.

It may be worth while to devote a few remarks to the consideration of the unintentional agency of brutes, towards not only preventing

* Propensities are similarly transmitted in the human race, but certainly not the knowledge of how these are to be gratified. It is true, however, that our observation in these matters is too much confined to cultivated, domesticated man, who is, consequently, farthest removed from the brute creation. The Australian savages are known to have a great *penchant* for snails and caterpillars; and I have somewhere read of one of these who had been brought up in a town, and carefully kept away from all communion with others of his race, who, nevertheless, exhibited the same fondness for these dainties, despite the abhorrence with which all his companions regarded them. His *goût* for them must thus unquestionably have been hereditary; though it is probable he may have learned the fact of their being eaten by his race, which, likely enough, induced him to taste and try them.

* The reader will observe that the doctrine here controverted is but an application of the exploded hypothesis of M. Lamarck.

the over-increase of their prey, which would only lead to too much consumption of the food of the latter, and so bring about famine and consequent degeneration from insufficiency of nutriment, but likewise towards preserving the typical character of their prey in a more direct manner, by removing all that deviate from their normal or healthy condition, or which occur away from their proper and suitable locality, rather than those engaged in performing the office for which Providence designed them. In illustration, it will be sufficient to call attention to the principle on which many birds of prey are enabled to discern their quarry. When the tyrant of the air appears on wing, his dreaded form is instantly recognised by all whose ranks are thinned for his subsistence; and instinct prompts them to crouch motionless, like a portion of the surface, the tint of which all animals that inhabit open places ever resemble; so that he passes over, and fails to discriminate them, and seeks perchance in vain for a meal in the very midst of abundance; but, should there happen to be an individual incapacitated by debility or sickness to maintain its wonted vigilance, or should its colours not accord sufficiently with that of the surface, as in the case of a variety, or of an animal pertaining to other and diverse haunts, that creature becomes, in consequence, a marked victim, and is sacrificed to appease the appetite of the destroyer: so profoundly wise are even the minor workings of the grand system; and thus do we perceive one of an endless multiplicity of causes which alike tend to limit the geographical range of species, and to maintain their pristine characters without blemish or decay to their remotest posterity.[129]

Thus it is that, however great may be the tendency of varieties to perpetuate themselves by generation, we do not find that they can maintain themselves in wild nature; nor do the causes which induce variation, beyond the occasional and very rare occurrence of an albino, prevail in those natural haunts of species to which their structural adaptations bind them. We have already noticed the anomalous influence of human interference in altering the innate instincts of the lower animals, thereby unfitting them to pursue the mode of life followed by their wild progenitors. It would be needless to amplify on the concomitant effects produced by domestication on the changes in the physical constitution and adaptations of the corporeal frame of animals, which oftentimes render them dependent on human assistance for continuous support, in the degree of their domesticity. Such changes are equally imposed on the vegetable world by cultivation; and they everywhere mark the progress of man, and exhibit in indisputable characters the diversity of his influence over the inferior ranks of creation, from any mutual and reciprocal influence observable among these latter.

I may cursorily allude to hybridism also, as a phenomenon, as far as can yet be shown, at least in animals, where fecundation cannot happen fortuitously, in every instance referable to human interference. As yet, I have failed to meet with a single satisfactory instance, wherein commixture of species could not be directly traced to man's agency, in superimposing a change on the constitution of the female parent. This is a subject of exceeding interest; and I am glad to avail myself of every occasion to endeavour to incite some to undertake its further investigation. There can be little doubt that certain of our domestic races, as the common fowl, are derived from a plurality of species, which, however, do not blend in wild nature; so that their union (assuming the hypothesis to be correct) may here, at least, be fairly ascribed to domestication. Still, when we consider that separate species (i.e. races not descended from a common stock) exhibit, as is well known, every grade of approximation, from obviously distinct to doubtfully identical, there appears, I think, sufficient reason at least to suspect that circumstances may sometimes combine to induce those nearest allied to commingle. That the mixed progeny, too, would in some instances be mutually fertile, I know in the case of the hybrid offspring of the A'nser cygnöides, and the common goose; but, in birds generally, the converse nevertheless obtains, as is particularly instanced, I have learned, by the hybrid Fringíllidæ reared in confinement; and also the mule betwixt the common fowl and pheasant; the males of all which appear (from a variety of instances I have been fortunate in collecting) to have been incompetent to fecundate the eggs produced.* Perhaps the

[129] This paragraph is a very clear and precise statement of the conservative aspect of natural selection as it was seen by Blyth. L. C. E.

* Since writing this, I have been informed of a solitary instance of a male goldfinch mule producing offspring with a hen canary.

superior size, too, of these hybrids generally to that of either of their parent species may be explicable on the principle which occasions the large growth of capons. However, none of the species here alluded to are by any means so closely allied as many that are known to exist; and, therefore, as in the vegetable world the degree of fertility in hybrids is in the ratio of that of affinity between the parents, those derived from very approximate species being, apparently, quite as prolific as the pure race, analogy would lead us to infer that the same law holds in the animal creation. At present, we have no proof of it: and I may conclude the subject by observing that the cases of supposed union (apart from human influence) betwixt the carrion and hooded crows, so often insisted on, are inconclusive, inasmuch as it does not appear that the individuals were ever examined and compared, although black varieties of Córvus Córnix have been several times known to occur. Indeed, I have myself examined a female specimen, on which were several black feathers intermingled with the ordinary ash colour on the back.*

The agency of the human race has been likened to that of brutes, in the particular that, as man effects the destruction of one species, he necessarily advances the interests of another.† How far he may permanently benefit the latter, might be discussed on principles that have been already expounded. More able writers, however, have put the enquiry whether man, by taking certain plants, for instance, under his protection, and greatly extending their natural range by cultivation, does not thereby unintentionally promote the welfare of the various species which subsist upon them. But, will it be argued that man, by vastly increasing the breed of sheep, is unconsciously labouring for the advantage of the wolves? As little can it be concluded, regarding the human race as progressive (in which it differs from all other species), that any race hostile to man's interests can be permanently benefited by his agency. The question, in short, resolves itself into one of time.

It has already been intimated, that man is the only species that habitually destroys for other purposes than those of food. This leads me to a few remarks on the extinction of species. Without alluding, however, to the more direct agency of the human race, towards accomplishing the destruction of every terrene species which conduces not in some way to our enjoyment, we will merely consider the natural causes which suffice to extirpate all other races, but are inadequate to effect the extinction of the human species. We have already seen that brute animals, in a state of nature, are merely beings of locality, whose agency tends to perpetuate the surrounding system of which they are members. It tends to do so, but is insufficient to effect this permanently; because, in the immensity of time important changes are brought about in every locality, by causes ever in operation, to which the faculties of the inferior animals are blind. They must, therefore, perish with their locality, unless distributed beyond the influence of the change; for their adaptations unfit them to contend for existence with the more legitimate habitants of diverse haunts, in proportion as they were suited to their former abode: and it must be necessary for creatures of instinct to be thus expressly organised in relation to their specific haunts, even to all the minutiæ we perceive, in order to enable them to perform efficiently their destined office; which exquisite adaptation, however, cannot but of course disqualify them for maintaining their existence elsewhere. In man only we discover none of these partial adaptations, further than that he is intended to exist upon the ground; and the human race alone, in opposition to all other animals, takes cognisance of the progressive changes adverted to, and, from reflection, intentionally opposes obstacles to their course, or systematically endeavours to divert their energy. Man's agency, indeed, tends everywhere to alter, rather than to preserve, the indigenous features of a country; those features which natural causes combine to produce: in short, he strives against the united efforts of all other agents, insomuch that, wherever he appears, with his faculties at all developed, the aspect of the surface becomes changed: forests yield to his persevering labours; the marshes are drained, and converted into fertile lands; the very climate, accordingly, changes under

* A friend informs me that he has repeatedly noticed, in Aberdeenshire, the pairing of a black crow with an ordinary individual of C. Córnix; and he further assures me that, to judge from its most commonly sitting, the former was in every instance the female bird. (Are not the black individuals noticed in Ireland, and assumed to be C. Corône, in reality varieties of C. Cornix?) It may be added, that the circumstances occasioning the alleged union, stated by Temminck, betwixt the Motacílla lùgubris and M. álba require much additional investigation.

† Vide IX, p. 613.

his influence, which every way inclines to extirpate the indigenous products of the soil, or to reduce them, by domestication, to a condition subservient to the promotion of human interests. Does not, then, all this intimate that, even as a mundane being, man is no component of that reciprocal system to which all other species appertain? a system which for countless epochs prevailed ere the human race was summoned into being. His anomalous interference, therefore (for this work most aptly expresses the bearings of human influence upon that system), essentially differing from the uniform agency of all the rest, in not conducing to the *general* welfare, is thus shown to be in no way requisite to fill a gap in the vast system alluded to. All rather tends alike to indicate him a being of diverse, of higher destiny; designed, in the course of time, with the aid of physical causes ever in operation, and the presumed cessation of the creative energy, to revolutionise the entire surface of our planet. I will presently recur to this subject as regards marine productions. It is sufficiently evident, that, as the human species is bound to no description of locality, but alike inhabits the mountain and the plain, and is, by self-contrivance, enabled to endure the fervid heats of tropical climes, equally with the withering blasts of a polar winter, it is consequently proof against the undermining effects of those surface changes which suffice to effect the extermination of every other.* Its future removal, then, from this scene of existence, whenever that shall happen, will probably be brought about on another principle: how, it would be most useless to enquire. There is no reason, however, hence

* There is no occasion, here, to follow out all the causes which combine to bring about the extirpation of species; but I will mention one which appears not to have been duly considered by those who have written on the subject. We have every reason to believe that the original germ of an animal may be developed into either male or female; and it is certain, that external circumstances exercise a very considerable influence in determining the sex of the future being. Now, the results of experiments instituted on sheep by the Agricultural Society of Séverac fully warrant the conclusion, that, where species exist under circumstances favourable for their increase, a greater number of that sex is produced, which, in polygamous animals is most effectual for their multiplication; whereas the contrary obtains, probably, in proportion to the difficulty of obtaining a livelihood. The relative age and constitutional vigour of the parents is likewise an important element in this problem; and, combined with the former, will enable us to calculate an average with tolerable precision. I have collected some very curious facts bearing upon this subject, some of which are extremely difficult of explanation. Mr. Knapp, in his *Journal of a Naturalist*, has the following, which is worthy of close attention:—"The most remarkable instance," he observes, of variation in the relative proportion of the sexes, "that I remember of late, happened in 1825. How far it extended I do not know; but, for many miles round us, we had in that year scarcely any female calves born. Dairies of forty or fifty cows produced not more than five or six; those of inferior numbers in the same proportion; and the price of female calves for rearing was greatly augmented. In a wild state," he justly observes, "an event like this would have considerable influence upon the usual product of some future herd." (Note to p. 138.) This occurred in Gloucestershire. The character of the preceding season is not stated; but, most probably, it was one of scarcity to the parent animals. The following list exhibits the proportion of the sexes in the annual produce of generally six cows, of the Ayrshire breed (four being the same individuals throughout, the remainder their produce), kept in a park in this neighbourhood. It commences with the year in which the present superintendent took charge of the stock; and there is no question but that, if the stock-books of other persons who have the care of cattle were to be duly looked over for a series of years, many similar and equally interesting facts would be brought to light:—

In 1826, from 6 cows, were born 6 male calves, 0 females.

1827	–	6	–	–	6	–	–	0
1828	–	6	–	–	6	–	–	0
1829	–	5	–	–	4	–	–	1
1830	–	6	–	–	3	–	–	3
1831	–	5	–	–	0	–	–	5
1832	–	5	–	–	0	–	–	5
1833	–	6	–	–	0	–	–	6
1834	–	6	–	–	0	–	–	6
1835	–	6	–	–	3	–	–	3
1836	–	6	–	–	2	–	–	4

Thus it appears that, for the first four years, but one female calf was produced out of twenty-three births; that in the succeeding year the proportions were equal; that in the next four years, out of twenty-two births, there was not a single male; and that in the following year, again, the sexes were in like proportions. The present season, alone, has formed an exception to this remarkable regularity, which I have in vain endeavoured to solve by making every enquiry concerning the male parents. There is some reason, also, to suspect that the same phenomenon will be found to obtain among wild birds. The Hon. and Rev. W. Herbert remarks, incidentally, that he has found in the nests of whitethroats (Currùca cinèrea) a great predominance of males, and the contrary in those of whinchats and stonechats; which latter I have also noticed myself; but cannot say that I have remarked it in a sufficient number of instances, nor over a sufficient extent of ground, nor for a sufficiently protracted period, to be enabled to deduce any general or satisfactory conclusion: the fact can, in most instances, be only ascertained (without slaughtering a great number) by raising them to maturity in confinement. But the young stonechat may be readily distinguished even in the nest: the immature males have a large pure white spot above their wings, which in the females is pale brown. The subject is extremely worthy of further investigation, and it is needless to point out its important bearings in wild nature.

to anticipate that supernatural means must *necessarily* be resorted to, as a malignant disease might suffice to level all ranks in the dust. It is enough for my present purpose, to indicate in this the diversity of the human from all other species.

Some have argued the connexion of man with the reciprocal system to which the inferior animals pertain, because, forsooth, he sometimes is annoyed by parasites. Without dwelling upon this topic, I may be allowed to say that it remains to be shown that any are peculiar to the human species. The certain fact, that different races of mankind are infested by distinct species, rather points to the conclusion, that, as the bed cimex can subsist and thrive away from human habitations, so also may even those species which abide on the person.*

Vol. 1 (n.s.) (1837), pp. 131–141: ART. VI. *On Psychological Distinctions between Man and all other Animals; and the consequent Diversity of Human Influence over the inferior Ranks of Creation, from any mutual and reciprocal Influence exercised among the Latter.*

(*Concluded*)

I WILL now proceed to notice, and follow to its bearings, that mysterious impulse (if possible, even more incomprehensible than ordinary instinct) which guides a migrant animal to its destined haven; which also propels a bee towards its hive, and a pigeon homeward from one extremity of Europe to another; a principle, as will be shown, not wholly absent from the human constitution. The migrative restlessness displayed so forcibly by birds of passage, even when raised in confinement, and plentifully supplied with the nourishment they have been accustomed to (thus showing that insufficiency of food is not the predisposing cause, as is also intimated by the early departure of certain species from their summer haunts, after performing the duties of the season), is merely on a par with all other instinctive manifestations: and I may cursorily remark that, from much careful and attentive observation, I have determined, at least to my own satisfaction, that, as a general rule, it is in autumn mainly influenced by decline of temperature, and in spring by the breeding stimulus: the period of the incidence of which latter (though, undoubtedly, somewhat affected by temperature) is primarily dependent on specific peculiarity, and, secondarily, on constitutional vigour.* It is not the erratic impulse, however, so much as the guiding principle, that we have here to do with; that wonderful, most inexplicable principle, on which a diurnal migratory bird is not only, and by night, enabled to soar for even thousands of miles, over seas and continents, surmounting every obstacle, even lofty mountain ridges, in its course, impelled always in one unvarying direction, till it arrives at the proper winter quarters of its species; but, at the ensuing season, is also led back to its former abode, to the precise locality that it had previously set out

* It is amusing to observe how gravely the loss of these parasites is commented on in Vol. IX. p. 612. as a necessary consequence of the extermination of human beings. Let us suppose they were to perish; what then? Have not myriads upon myriads of every class of beings become extinct, as species, without affecting at all the workings of the mighty system? Why, then, should the *dreaded* loss of a few parasites, the sphere of whose influence cannot be supposed to extend beyond that of the species to which their adaptations link them?

* The direct influence of decline of temperature in prompting the equatorial movements of the feathered race, may be observed in the fluctuations in intensity of the erratic impulse, throughout the greater part of winter, exhibited by migrant birds in a state of confinement; such variations being constantly found to accord with thermometrical changes. It may be added, that the degree of temperature which incites them to migrate varies considerably in different species; and in some instances, also, it must not be concealed, that the impulse to quit the breeding station is entirely independent of decrease of temperature; as is exemplified by the swift and adult cuckoo retiring southward at the hottest period of the year: so powerful, too, is this impetus in the first-named species, and others of the Hirundínidæ, that these have been many times known to leave a brood of half-fledged nestlings to perish. As regards the polar movement, the proximate cause will appear on consideration of the following facts:— It is known that, in the feathered race, the enlargement of particular organs in spring superinduces, in most groups, some considerable change in the external aspect; frequently altering, for instance, the colour of the bill, and occasioning (in single-moulting species) the rapid disappearance of those deciduous edgings to the feathers, which oftentimes conceal, for a while, the brighter tints of summer; which latter, also, are, in addition, commonly more or less heightened at this period. Now, all these changes are observable in two nearly allied species, the chaffinch and bramble finch, both of which pass the winter in the same localities; but it uniformly happens that the vernal change takes place in the former species several weeks earlier than in the latter. In the beginning of March, every chaffinch is found to exhibit its complete summer aspect; whereas, late in April, I have watched, with a glass, a flock of bramble finches feeding on elm blossoms, in none of which had the bill acquired its blue colour; coincident with which change this species always leaves the country. The fact is equally noticeable when they

from, having been known even to return to confinement. I conceive it unnecessary to detail observed instances of this astonishing fact, because, in the feathered race, it is well known to every student of natural history. It will be enough to mention, that I have an instance, on indisputable authority, of a lame redstart returning regularly for sixteen summers to the same garden.

Among mammalians, numerous instances have been recorded, resting on unexceptionable testimony, of animals returning straight to their accustomed haunts, over pastures and across streams they could not possibly have ever traversed before, and by a nearer and very different route from that by which they had been driven or carried. To these I will add the following, which occurred to the personal knowledge of my informant. A cat, from the centre of an intricate and populous seaport town*, was shipped on board of a vessel bound for the Brazils; and, after performing the voyage to and fro, contrived to escape, on returning to its native port, and found its way, through several streets to its former domicile.

Mr. Jesse, in the third series of his *Gleanings*, has related a like anecdote of a reptile. Of a number of turtles, captured on Ascension Island, chanced to be an individual which, to use the technical phrase, had lost one of its *fins*. It was marked in the ordinary manner on the under shell, which marks are well known to be indelible. The vessel, on arriving in the Channel, was long detained by contrary winds, during which time a great mortality took place among the turtles; these dying one after another so fast, that it was at length resolved to cast what few remained of them, including the lame one, into the sea, to give them, as was said, a chance for their lives. Three years afterwards, this same turtle, with its three fins, and the marks of the hot iron beneath, was found again upon Ascension Island.

It is sufficient to refer to the results of numerous experiments which have been instituted on the fry of the Salmónidæ, to be convinced of the prevalence of the same surprising impulse also among fishes.

In the invertebrate animals, we have, apparently, proof of the existence of this principle, in the fact of the great distances to which many hymenopterous insects are known to range for food. A decisive experiment, however, is still needed to render the inference conclusive; and I venture to suggest, to whoever may have the opportunity and inclination, that of marking a number of bees from the same hive, and suffering them to fly from, say, a hundred miles' distance. There is hardly a doubt that they would be found to regain their abode; and it would be interesting to ascertain the time they would require to do so.

Some migratory birds are observed to resort annually to the exact same winter quarters; for illustrative proof of which, refer to Bewick's description of the woodcock. Other species would seem to wander through the winter, of which the waxwing may be cited as an example. They all, however, appear to return to their former breeding haunts, where dispersion is effected, in those species which do not nidificate in society, by the older individuals (which are always the first to return) driving away their young of a former year; which latter, however, do not commonly retire farther than they can help, as I have had occasion to notice in some instances.

The bearings of this law on the geographical distribution of species do not appear to have been sufficiently taken into consideration. For instance, Mr. Selby remarks, as an extraordinary circumstance, tending to show within what abrupt boundaries the natural range of particular species is confined, the abundance of the white stork in Holland, and its excessive rarity on the opposite English coast. In Holland, be it remembered, it meets with encouragement; whereas, in this country, no sooner does an individual make its appearance, than it is immediately shot down. Once allowed to settle,

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are kept in confinement. Fieldfares and redwings, also, linger in our fields till long after their resident congeners have been engaged in breeding; and it is found, on dissecting these, at this period, that they are comparatively very backward in their seasonal developments, the attainment of which immediately prompts the migrative impulse. Of course, the breeding station is the proper home of a species, and thereto do all its adaptations directly refer; and thus we find that even the genial influence of a more equatorial abode fails to excite the breeding energies of migrant birds, until such time as their distant summer haunts become fitted for their reception. To conclude this subject, it may be added, that the migratory restlessness in caged birds does not dissipate in spring, at the time of the reappearance of their wild brethren, but is occasionally evinced throughout the summer, till its cessation follows the decrease of those organs which had all along stimulated its manifestation; a constitutional change which likewise puts a stop to song, and brings about the autumnal renovation of plumage.

* St. Helier's, Jersey.

it would doubtless soon colonise our fenny counties.

Some years ago, a considerable flock of spoonbills settled in a part of Aberdeenshire; whereupon the whole neighbourhood uprose in arms, till every bird of them was killed. Here, probably, we have an instance of another phenomenon in the animal world, which should not be overlooked in this treatise. When a species increases numerically in any habitat beyond what the latter is adequate to sustain (a circumstance which, in the higher groups, can hardly happen, except in those of social habits), either their ranks are mysteriously thinned by what is termed *epizooty*, or an erratic impulse (unrestrained by the localising principle we have been considering) instinctively prompts a portion of them to seek fresh quarters. This is observed more in mammalians than in birds, but is particularly noticeable in the insect tribes; various species of which, though solitary in their usual habit, have been known to assemble at times in prodigious multitudes, prompted by a general impulse, which, however, appears to be less conferred with intent to extend the previous range of their distribution, than to preserve the species within due bounds in its native locality; for the numerous dangers with which these wanderers are necessarily every where beset absolutely appear to suffice, in most instances, to prevent their permanently establishing themselves in other places; a remarkable fact, notorious to all who have attended to the subject. So many causes are there in operation which combine to circumscribe the geographic range of species.

A variety of important considerations here crowd upon the mind; foremost of which is the enquiry, that, as man, by removing species from their appropriate haunts, superinduces changes on their physical constitution and adaptations, to what extent may not the same take place in wild nature, so that, in a few generations, distinctive characters may be acquired, such as are recognised as indicative of specific diversity? It is a positive fact, for example, that the nestling plumage of larks, hatched in a red gravelly locality, is of a paler and more rufous tint than in those bred upon a dark soil.* May not, then, a large proportion of what are considered species have descended from a common parentage?

I would briefly despatch this interrogatory, as abler writers have often taken the subject in hand. It is, moreover, foreign to the professed object of this paper. There are many phenomena which tend, in no small degree, to favour the supposition, and none more so than what I have termed the localising principle, which must occasion, to a great extent, what is called "breeding in and in," and, therefore, the transmission of individual peculiarities. We have seen, however, the extreme difficulties which most species have to encounter when occurring beyond the sphere of their adaptations; difficulties which must require human aid, in general, to render surmountable. But, without re-entering into the details of this subject, it will be sufficiently clear to all who consider the matter, that, were this self-adapting system to prevail to any extent, we should in vain seek for those constant and invariable distinctions which are found to obtain. Instead of a species becoming gradually less numerous where its haunts grade imperceptibly away, we should discover a corresponding gradation in its adaptations; and, as the most dissimilar varieties of one species (those of the dog, for instance) propagate as readily together as individuals of the same variety, producing offspring of blended characters*, so much so, that human interference is requisite to preserve a breed unadulterated, the unbending permanency of the distinguishing characteristics of all wild animals becomes of double import. Moreover, the characters in which these differ are of a diverse kind from these observable in any but the most distant of mere varieties; for they rarely agree in the relative proportions of parts, which are the most fixed of all specific distinctions. It is,

* Such is, at least, the uniform result of my experience; though I could never discern a corresponding difference in the adults. This curious fact was first intimated to me by a person who had a number of young larks for sale, among which were two nests of very rufous birds, and three of a much darker colour: the former, he assured me, were found in a gravelly situation; the others on a dark soil. Some cases I have since noticed have verified the observation. On another occasion, I may probably bring together a number of analogous facts, in the form of a paper; but it would occupy too much space to do so here. It may, however, be added, that the agency of many species confers a reciprocity of adaptation; thus, the mode in which sheep graze has a decided tendency to reduce a country to that bare and bleak state which suits best with their healthy condition. Hence would accrue a necessary return of varieties to their normal characters.

* Individuals of very diverse breeds mostly do so: where the parents more nearly approximate, the young often entirely resemble one or the other.

therefore, advisedly that we are enabled to state that the raven of the Cape is distinct from the raven of South America; that both are again different from that of the South Sea Islands and from that of Europe. The common jay is diffused over a wide range of latitude. but is the same in Italy as in Sweden: this would not be were it affected by locality or climate; the very trivial distinctions, therefore, which characterise it apart from that of Japan, and from that of Asia Minor, we are warranted in esteeming of specific value. Until the jays of intervening localities present inosculant characters†, or until precisely analogous diversities are, in wild nature, observed to be produced by locality or climate, the above conclusion is as irresistible as it is incontrovertible.

When, too, we perceive that species so very general in their adaptations as the typical Córvidæ are limited in their range, it behooves us to be most cautious in assuming the specifical identity of the most similar animals from widely separated localities. Let it be remembered that no reason can be assigned why those originally distinct should not exactly resemble. Human agency apart, I do not think there is a single species which even approximates to universal distribution. Of course, we can only judge from probability and analogy. Great locomotive power, even the maximum, by no means necessarily implies an extensive distribution: witness the common swift, and its American analogue*, neither of which have been known to straggle across the Atlantic, like many birds of far less power of wing, but are bound by the localising principle. It is true, this principle can apply only to such species as are locomotive; but it is equally true, that other causes analogously restrain the undue diffusion of those which are individually fixed. Thus we hear of the agency of sea currents in transporting seeds, which must abundantly be carried out into the ocean by the action of rivers; but it appears not to have been remembered that steeping in sea water destroys the vital principle; that moisture induces germination, which, once excited, can only be checked by the final cessation of the vital functions.† Analogy would lead us to infer that such antagonist principles obtain throughout creation, whether or not human observation may have yet detected their existence. It would be easy to point out additional hindrances to the more extensive spread of species of fixed habit, by treating on the fraction which are allowed to attain maturity, even in their normal habitat, of the multitude of germs which are annually produced; and in what ratio the causes which prevent the numerical increase of a species in its indigenous locality would act where its adaptations are not in strict accordance will sufficiently appear, on considering the exquisite perfection of those of the races with which it would have to contend. If there is a probability that any species has become naturally of general distribution, it is in the case of two lepidopterous insects, Acherón-tia A'tropos and Cýnthia cárdui, both of which are of peculiarly erratic habits; and it is said that these are found throughout the world: yet the differences which exist in specimens from diverse localities are hard to reconcile with specific variation, at least to judge from what tropical specimens I have seen of the former; and an eminent entomological friend has remarked to me, in conversation, that he is equally sceptical, judging from his own experience, of many Cýnthiæ assumed to be cárdui. It will be borne in mind, however, that man has unintentionally carried with him the seeds of the very prolific plants on which the painted-lady butterfly feeds, wherever he has introduced the Cereàlia.

But to return to that mysterious guiding principle, so important, as we have seen, in regulating the distribution of species; and which I have asserted to be not wholly absent from the human constitution. It has been stated of many savages, and more particularly of the aborigines of Australia, that they are enabled to return for even hundreds of miles to their homes, though totally unacquainted with the route, being led by an intuitive impulse that they cannot explain. This seems incredible: but we know that diurnal birds will return by night from the heart of Africa to their former abode,

† Here the very remarkable fact, however, is not to be overlooked, that the solitary African species of trogon presents a combination of those colours and markings which uniformly distinguish apart its numerous congeners in the Oriental isles from those of South America.

* Chætùra pelásgica.

† This observation is, however, intended to apply merely to those of inland plants; for some maritime species, as the Pandàneæ and cocoa palms, have their seeds encased in sea-proof coverings, especially adapted for floating uninjured on the waves: the restricted distribution of such vegetables is provided for on another principle.

marked individuals having done so; and we also know that a pigeon, carried from Paris to Constantinople, has flown back to the former city: these facts will tend to diminish our scepticism. I have two instances, however, of the manifestation of this principle by Europeans, when in a state of insensibility; for both of which I am indebted to the parties themselves, gentlemen of unimpeachable veracity: both of them returned, in this condition, to their temporary homes (one in the dark, and for upwards of a mile, having been thrown from his horse, which remained on the spot till found next morning), by routes with which they were quite unacquainted. I am not disposed to enlarge at present on this subject, by enquiring to what extent numerous phenomena recorded of somnambulists may be explicable on this obscure principle. We hear continually of surprising instances of blind men finding their way, with a degree of accuracy very difficult to comprehend; and, also, of drunkards stumbling home, when apparently unobservant of external objects. It will be sufficient if these hints serve to awaken the reader's attention, and so, peradventure, elicit some additional facts.

We have now traced to their ultimate results certain of the bearings of the intuitive information conferred on brutes, which, in wild nature, mainly influences their actions. We have seen that man is denied innate knowledge of the properties of objects, and is, therefore, necessitated to observe and reflect; in a word, to learn. Hence the necessity of a long infancy and superior intelligence; hence that progressiveness which so eminently distinguishes him from all other races. I have nowhere denied that other animals are capable of reflection; but I assert that, unrestrained by human influence, their inherent instincts sufficing to insure their weal and maintenance, these, in consequence, supersede the necessity of habitual observation; whence their reasoning even may be independent of experience. Indeed, it is hard to instance a case wherein the conduct of truly wild animals may not be satisfactorily referred to instinctive motives; but that such cases do occur is shown by eaves swallows (Hirúndo úrbica) having been known to immure a sitting sparrow that had usurped their nest*; which fact is proved sufficiently to be in nowise referable to instinct, inasmuch as it is contrary to the ordinary habit of the species upon such occasions. It will be readily admitted, however, that such instances are extremely rare exceptions to the general rule; and I imagine there are few who will be disposed to refer the ordinary habits of any species of the lower animals to aught else than original intuition.

I have yet another phenomenon, which is now, I believe, for the first time introduced to notice. It is the occurrence, in domesticated animals, of what is analogous to idiocy in the human race. Of this I have several instances in poultry, and one in a sheep. It consists in the privation of more or less of that intuitive knowledge which is needed to enable an animal to maintain its existence amid the numerous dangers with which it is naturally surrounded; dangers against which no experience could suffice to fortify it. The creatures I allude to evinced a listlessness in their deportment strikingly similar to what is commonly observed in human idiots: they sought not the society of their companions; and one of them, a hen (of which only I can speak from personal observation), would heedlessly wander close before the kennel of a fierce dog, which the other fowls constantly avoided. Whether the dog would have attacked another fowl, I cannot say; very likely not: but it is a well known fact, that the most savage of the canine race will never attack a human idiot, nor a child, nor a person stupified by intoxication: of the truth of which latter, a most remarkable exemplification lately happened in this neighbourhood; a drunken stranger having been absolutely permitted to share the straw of a very fierce watch-dog, which those it knows can hardly approach with safety.

In the foregoing pages, I have all along been considering the diversity of human influence from that of all other organised races, rather in its effects towards these latter, than by taking the higher ground of natural theology in reference to human kind, and recognising, in the grand aggregate of all that has been effected in past ages by the joint influence of every cause that has been in operation, not only a gradual prospective adaptation to the welfare of each succeeding race, but an ulterior object in capacitating the globe for the residence of human beings. A new era commenced with the introduction of man upon this world: a secondary intelligence was permitted to assume the domin-

* Even here it might be objected that man's influence could alone have brought these species into contact; so hard is it to disentangle ourselves entirely from the meshes of human interference. Such an objection would, however, in this instance, be frivolous.

ion over matter, in so far as, by experimenting upon its properties, it can elucidate the unvarying laws which regulate these, a knowledge of which is indispensable to empower intellect to direct their operation.* To man it was given to "conquer the whole earth and subdue it;" and who can venture to aver the ultimate limits of those changes which he every where superinduces; changes which, in conjunction with the physical laws which wear away the land and uplift the bed of the ocean, may, in time, be gradually fatal to the normal condition of every other race, and to the existence even of by far the greater number? that is, assuming, what there is every reason to infer, that the human species was the last act of creation upon this world, and that it will continue to be so until its removal. It is needless to add, that a prodigious lapse of time is here required; and, to judge from data which the past history of the globe abundantly furnishes, in legible records, wherever we turn our eyes; to judge from the progressiveness of human intellect, and the long, long while that must yet transpire ere man can hope to assume that rank, as a consistent being, for which his faculties clearly show that he was intended, the duration of his existence upon this planet would appear likely to bear proportion to that immense period that the globe will continue fitted for his reception; a period, it may be presumed, that will abundantly suffice to alternate the land and sea, as we know has repeatedly happened heretofore, and which may sweep from existence the inhabitants of the present ocean, as those of which the exuviæ occur in the chalk have become extinct before them.† The past affording the only record from which we are competent to judge rationally of the future, this inverse analogy would argue a continuance of the refrigeration of our planet, till it shall be again unfitted for the existence of organic beings. As by a catenation of obvious and palpable facts we can trace back the history of this world to a state of fusion, if not of general nebulosity, so are we warranted in anticipating its future congelation, when old age shall have come over it as barrenness, and the gases shall have solidified by intensity of cold; when, it may be, the sun himself shall have grown dim, and nature, in so far as this system is concerned, have sunk in years; when the stupendous cycle shall have been accomplished. Then, and with humble reverence let the mighty acts of Supreme Omnipotence be spoken of, it may be that the eternal and ever-glorious Being which willed matter into existence shall pronounce on it the final doom of annihilation; and

> ——"the great globe itself
> . . . . . shall dissolve,
> And, like the baseless fabric of a vision,
> Leave not a rack behind."

Or, what is far more probable, to judge from the universal analogies of all that is within our grasp, its elements shall again be called forth into light and life, and blaze forth the recommencement of the same system.

It is inconsistent with our notions of divine benevolence, to suppose that the human race shall be suffered to linger here upon earth, till such secondary causes as we have been considering should suffice to gather the last man to his fathers.

Tooting, Surrey, Dec. 1. 1836.

* "Homo, naturæ minister et interpres, tantum facit et intelligit quantum de naturæ ordine re vel mente observaverit; nec amplius scit aut potest."—*Lord Bacon.*

† Except man shall have domesticated some of these, and artificially transferred them to new localities.

APPENDIX B

MEMOIR OF EDWARD BLYTH BY ARTHUR GROTE, PUBLISHED IN THE JOURNAL OF THE ASIATIC SOCIETY OF BENGAL, AUGUST, 1875

More than one obituary notice of Blyth and his scientific labours, by competent and kindly pens, has already appeared in the columns of those Journals to which he had been in the habit of contributing. This seems a fitting place for collecting in a brief memoir such particulars as are obtainable of his early life, and such as I can myself supply of his long career in our Society's service. My personal acquaintance with him commenced on my return to India from furlough in 1848. He had then been Curator of our Museum for seven years, and my official connexion with the Society, combined with a taste for his pursuits, brought me into frequent and close relations with him. Of the incidents of his pre-Indian life some knowledge has been obtained from his sister, who has kindly given me access to such of his letters as are in her possession.

The carbon print which accompanies this memoir has been prepared by the Autotype Company from a photograph taken of Blyth when he visited Dublin some ten years ago.

It has been kindly contributed to me by Miss Blyth and her relative Mr. R. Loder, of High Beeches, Crawley, Sussex.

Edward Blyth was born in London on the 23rd December, 1810. His father was of a Norfolk family, and from him the son appears to have inherited both his taste for nature and the retentive memory for which he was so remarkable. Blyth's father died in 1820, leaving four children, whose care and education now devolved on the widow, a Hampshire lady, who at once sent Edward, the eldest boy, to Dr. Fennell's school at Wimbledon. Here the boy seems to have made unusual progress in his books, but the school reports describe him as of truant habits, and as being frequently found in the woods. He left school in 1825, and his mother seems at first to have intended him for an University career, and ultimately for the Church, but at Dr. Fennell's suggestion she sent her son to London to study chemistry under Mr. Keating, of St. Paul's Churchyard. He did not, however, long persevere in this study, being dissatisfied with his instructor's mode of teaching. His enthusiasm for Natural History pursuits disinclined him for any ordinary employment, and on coming of age he embarked the little means he had in a druggist's business at Tooting. To this he seems to have given little personal attention. The management of the business was left to another, while Blyth devoted all his time to the study which engrossed his thoughts. "Never," says his sister, "was any youth more industrious; up at three or four in the morning, reading, making notes, sketching bones, colouring maps, stuffing birds by the hundred, collecting butterflies, and beetles—teaching himself German sufficiently to translate it readily, singing always merrily at intervals." He took a room in Pall Mall, to have readier access to books, and passed much of his time in the British Museum, in which, or in some kindred institution, he tried hard to find employment.

Naturally the Tooting business did not thrive under such fitful management. Blyth soon found himself in serious difficulties; such literary work as offered itself in his own special line of study supplied him with but precarious means. In the Introduction to his edition of White's 'Selborne,' which bears date from Lower Tooting, 1836, he alludes to the anxieties which then surrounded him, though "his mind," he adds, "cleaves to its favourite pursuit in defiance of many obstacles and interruptions, and eagerly avails itself of every occasion to contribute a mite to the stock of general information." Young as he was, Blyth had at this time earned for himself a reputation as a diligent and accurate field observer, and he corresponded with many of the leading naturalists of the day. He seems to have been a contributor to both Loudon and Charlesworth's series of the Magazine of Natural History from 1833 till his departure for India, and in one of his papers of the volume for 1838 he proposed a new arrangement of Insessorial birds. Rennie enlisted him as a writer in the "Field Naturalist," and he was associated with Mudie, Johnston, and Westwood, in an illustrated translation of Cuvier, which was published by Orr and Co. in 1840. Blyth undertook the Mammals, Birds, and Reptiles in this work, adding much original matter of his own, which is inclosed within brackets. A new and enlarged edition of the work appeared in 1854, with important additions to the Molluscs and Fishes by Dr. Carpenter.

The Proceedings of the Zoological Society from 1837 to 1840 contain a few papers read by Blyth at their meetings. One of these, on the Osteology of the Great Auk, observes on the distinctive characters of Auks and Penguins. In another he draws attention to peculiarities in the structure of the feet of the Trogons. But the most important of these contributions was his Monograph of the genus "Ovis," read in 1840.* He here describes fifteen species of Sheep, including the then newly discovered *O. poli*, from Pamir. At the same meeting he exhibited drawings and specimens of the Yak, Kashmir Stag, Markhur, Himalayan Ibex, and other Indian ruminants, his remarks on which show the attention which he had already begun to give to the Zoology of India.

Just at this time our Society had obtained from the Court of Directors a grant for a paid Curator of its Museum, which had grown into a collection beyond what was manageable by the honorary office-bearers who had hitherto looked after it. The labours of Hodgson, Cantor, M'Clelland, and others, had filled it with valuable Zoological specimens, which with important

* Proc. Zool. Soc., July 28. This was an "Amended List" of species, of which he had enumerated nine in a summary Monograph in the previous February. This paper was reprinted in Taylor's Mag. of Nat. Hist. in 1841, and again with additional matter in J.B.A.S. vol. x. pt. 2, p. 858.

fossil and other contributions were falling into great disorder. Prof. H. H. Wilson, then our honorary agent in London, was asked to select a competent man to undertake the general charge of the Museum, and the appointment was offered to and accepted by Blyth, then in weak health, and professionally advised to seek a warmer climate. Provided with passage and outfit by the Court of Directors, the latter arrived in Calcutta in September, 1841. His letter to Mr. H. Torrens, published in our Society's Proceedings for that month (vide Journ. Vol. X. Pt. 2, p. 756), expresses the diffidence with which he entered on the charge of the Mineral Department of the Museum; but of this duty he was largely relieved in the following year on the appointment of Mr. Piddington to all the Departments of Economic Geology. He still retained the custody of the Palæontological specimens.

One of the duties impressed on him by our then President, Sir E. Ryan, was that of furnishing monthly reports at the Society's meetings; and in October, 1841, he accordingly submitted the first of that long series of useful reports which appear in our Proceedings with scarcely any intermission for the next twenty years. Each of the monthly issues of this Journal for the remainder of 1841 contains a paper by Blyth. In the first of these, 'A general review of the species of True Stag,' etc., he committed himself to an opinion, shared with him by Ogilby, regarding Hodgson's *Cervus affinis*, which, as Jerdon has pointed out (Mamm. p. 252), he did not recant till 1861.

Many of Blyth's reports fill from fifteen to twenty pages, and his remarks on the various contributions which reached him were just what were wanted by the field observers who supplied them. The active correspondence which he set on foot with these and with sportsmen, all more or less naturalists, throughout India, encouraged their useful pursuits, and brought him a large accession of specimens. He received in July, 1846, the thanks of the monthly meeting of our Society for his exertions "in opening out new channels of scientific intercourse."* He had already found it necessary to apply for assistance in his Museum duties, but the Society had not the means of supplementing the Government grant beyond the small allowance which they gave him for house rent. Had Blyth been less devoted to the special service in which he had engaged, there were not wanting to him opportunities of finding far more remunerative employment in other quarters. The Dutch authorities in Java seem to have about this time made him a very tempting offer.

The Proceedings of the Zoological Society for 1841 and 1842 contain two letters from Blyth, of which one was written on the voyage out to India,* and the other shortly after his arrival.† The latter contained remarks on various species of birds found in India and Europe. Nothing from his pen appears in the Calcutta Journal of Natural History, of which the publication had just commenced when he reached India, and which was brought to a close in 1847. He found time, however, to send home several papers for the Annals of Natural History in 1844–48, as will be seen in the List appended to this Memoir, in which I have endeavoured to collect the titles of all his published writings.

The unpleasant episode in regard to the publication of the Burnes Zoological drawings with Dr. Lord's notes had occurred before I joined the Society. The materials, which consisted of certain wretched figures by a native artist, and some descriptions of already well-known species, the Afghanistan localities of which were alone new, had been made over to us by the Government before Blyth became our Curator. The lithographer's death had brought the work to a stand, and when inquiry was made in 1844, the notes which were to furnish the letterpress were not forthcoming. Blyth's explanation of his share in their disappearance will be found in our Proceedings of October, 1844.‡ This was followed by a controversy with Mr. Torrens,§ then our Secretary; and the financial embarrassments of the Society soon afterwards necessitated the abandonment of the publication. Of the fourteen coloured copies of the completed plates, I possess one, and I quite agree with Blyth that their issue would have brought ridicule on the Society.

The heavy outlay incurred on this undertaking, and on the publication of Cantor's Chusan drawings, was unfortunately the cause, not only of the embarrassments just noticed, but of a temporary estrangement between the Philological and Physical classes of our members. Funds which had been assigned by the Govern-

* J.B.A.S. xv. p. 51.

* P.Z.S. 1841, p. 63.
† *idem.* 1842, p. 93.
‡ J.B.A.S. xiii. pt. 2, p. 51.
§ *idem.* xiv. pt. 2, p. cvi.

ment for furthering Oriental literature had no doubt been appropriated to other objects. Blyth came in for a share of this discontent on the part of the Orientalists, and some Naturalists also complained that he was enriching the Mammal and Bird departments of the Museum at the expense of those of the shells, fossils, and insects. The want, too, of a Catalogue of the collections had been long felt, and the Curator had been repeatedly urged to supply it. The Council refers to his delay in performing this duty in their Report* of 1848, while commending "his regularity of attendance and remarkable industry." His application for increased pay and a retiring pension was referred to the Society at large with the following guarded remarks:—"It must be admitted that for any scientific man capable of discharging the duties on which Mr. Blyth is employed, and of performing them with activity and zeal, for the advancement of science, etc., the [monthly] salary of 250 rupees is a very inadequate compensation. But the Council cannot but regard the present as an inauspicious period to address the Honourable Court in furtherance of any pecuniary claim. The diversion of the Oriental grant to so large an amount as has but lately been brought to notice, cannot be regarded with indifference by them, nor can it have disposed them to entertain with much favour any fresh demand on their munificence preferred by the Society." The application was then referred for report to the Natural History Section, and notwithstanding the stout struggle made on his behalf in the Section, their report was unfavourable to Blyth's claims, which were finally negatived at the July† meeting in 1848.

In the following year Blyth published his Catalogue of Birds, which had in fact long been ready for issue in a form which would have satisfied the Council. It had been constantly kept back for the Appendices, Addenda, and "Further Addenda," which disfigure the volume, and seriously detract from its value as a work of reference. This habitual reluctance of his to part with his compositions till he had embodied in them his latest gained information is conspicuous throughout his contributions, and it is in fact partly due to this habit that these Burman Catalogues form a posthumous publication.

Blyth availed himself of every opportunity which offered of escape from his closet studies to resume his early habits of field observation. Frequent mention will be found in his reports of the little excursions into the country which he thus made, and of the practical results obtained from them. The geniality of his disposition and the large store of general information at his command insured him a warm welcome in all quarters. One of his favourite resorts was Khulna, on the edge of the Jessore Sunderbuns, where the indigo factory of an intelligent and untiring observer‡ offered him a favourable station for field pursuits.

Several contributions from Blyth on his special subject will be found in the pages of the different sporting Journals which have appeared in Calcutta. He was on the regular staff of the 'Indian Field.' In the 'India Sporting Review' he published a sketch of 'The Osteology of the Elephant,' and a series of papers on 'The Feline Animals of India.' For the 'Calcutta Review' he wrote an article on the 'Birds of India.' It gives the results of his latest experience on the subject of the communication made in 1842 to the Zoological Society, which has been noticed above, and shows that of 353 species of birds admitted by Yarrell into the English avifauna, no less than 140 are found in India.

In 1854 Blyth was married to Mrs. Hodges, a young widow whom he had known as Miss Sutton, and who had lately come out to join some relatives in India. This step on his part necessarily aggravated the embarrassments entailed on him by his inadequate income, and on completing his fourteenth year of service in 1855, he memorialized the Court of Directors for an increased salary and for a pension "after a certain number of years' service." In the second paragraph of his memorial he observes, "that however desirous the Asiatic Society might be of augmenting your memorialist's personal allowances, the ever-increasing demands on its income, consequent on the extension of its collections among other causes, altogether disables it from so doing." On this memorial being submitted to the meeting* of May, 1856, it was agreed to forward the document to Government, "with the expression of the high sense entertained by the Society of the value of Mr. Blyth's labours in the Department of Natural

* J.B.A.S. xvii. pt. 1, p. 10.
† J.B.A.S. xvii. pt. 2, p. 122.

‡ Our common friend Robert Frith, whose name is of frequent occurrence in the Curator's reports.
* J.B.A.S. xxv. 237.

History, and of its hope that the memorial may be favourably considered by the Honourable Court."

The extract just given will show, in Blyth's own words, that he had no complaints to make of our Society's treatment of him. Mr. A. Hume, who seems to have first joined our Society in 1870, has gone somewhat out of his way in his 'Rough Notes'† to do justice to Blyth's merits as Curator, at the expense of older members. The language used is in Mr. Hume's characteristic style, and is as offensive as the charge brought against the Society is unjust. The same charge is implied in the use of the words "neglect and harshness" in the "In Memoriam" with which vol. ii. of 'Stray Feathers' opens, and which, with this exception, describes with much truth and feeling the life-long struggle in India, as at home, which Blyth's scientific ardour supported him in maintaining against the most depressing obstacles.

That nothing came of this memorial is due probably in some measure to the movement which commenced in 1857 for transferring our collections to an Imperial Museum, but mainly to the great convulsion which shook our empire in that year. I find no record in our Proceedings of any reply having been made to our recommendation, and the negociations for the foundation of the new museum were not resumed for some three years.

Blyth made a short tour in the N.W. Provinces in July, 1856. He spent some six weeks in Lucknow, Cawnpore, Allahabad, and Benares. Oude had just been annexed, and the sale of the Royal Menagerie at Lucknow had been determined on. The tigers were the finest caged specimens in the world, and to one who understood their value in the European market, the inducement to buy and ship the animals was irresistible. A German friend joined in the speculation, and found the necessary funds. Blyth was to do the rest, and as no competitors offered, he bought the bulk of the collection for a trifle. Eighteen magnificent tigers were sold at 20 rupees (£2) a head! Some casualties occurred on the passage down the river; but his collection, when exhibited in Calcutta, contained sixteen tigers, one leopard, one bear, two cheetas, three caracals, two rhinoceroses, and a giraffe, which carried a saddle and was daily ridden. Difficulties unfortunately occurred in finding ships for the transport of the animals, and their detention in Calcutta caused further casualties and heavy charges, which his partner would not face. The speculation collapsed, but one of the tigers which reached England realized £140.

In December, 1857, Blyth had the misfortune to lose his wife. His short married life had been of the happiest, and the blow fell heavily on him. His letters to his sister for the early months of 1858 are painful to read. The shock proved too much for him, and brought on a serious attack of illness; it threatened paralysis of the heart, and he seems to have been subject to partial returns of similar attacks for the rest of his life. His health too suffered much from the isolation imposed on him by his straitened means, and from want of proper exercise. Some distraction for his thoughts was luckily afforded at this time by the opening up of a new fauna in the Andaman Islands, which Dr. Mouatt had been sent to report on before their occupation as a penal settlement. To this Report Blyth contributed an interesting chapter on the Zoology of the Islands, so far as it was then known.

The China expedition of 1860 was considered both at home and in India a good opportunity for obtaining information regarding the natural history of North China. Blyth's name was put forward as that of a naturalist readily available and eminently qualified for the post of naturalist to the expedition. Replying to Lord Canning's objections that scientific observations in a hostile country would have to be carried on at much personal risk, our Council,* while urging the importance of the mission in a scientific point of view, stated on Blyth's behalf that "he was quite willing to encounter the danger, whatever it might be." The application, however, failed: no naturalist was appointed. This result was to be regretted, as it affected Blyth personally, for his health was failing, and the sea-voyage, with the stimulus afforded by so interesting a mission, would have been most beneficial to him, and would probably have averted the utter breakdown which was now at hand. It is doubtful whether he was equal to the more laborious task which he offered to undertake in the following year, when the

† See note to 'My Scrap Book or Rough Notes on Indian Oology and Ornithology,' No. 1, p. 181.

* J.B.A.S. xxix. p. 82.

scientific expedition into Chinese Tartary was projected by the Government.

Blyth was a staunch adherent of Darwin's views, and an opportunity of thus declaring himself offered at our November meeting in 1860, when Mr. H. Blanford read his paper on the well-known work of Dr. Broun on the laws of development of organized beings. The value attached by Darwin to Blyth's observations is shown by the frequent reference made to them, more especially in his 'Animals and Plants under Domestication.' His first citation of Blyth in the latter work describes him as an "excellent authority," and the many quotations that follow in these interesting volumes show how carefully he read and noted all that fell from Blyth, even in his contributions to sporting journals.

In 1861 Blyth's health fairly gave way, and in July of that year a second memorial was submitted to Government† with a view to obtaining a reconsideration by the Secretary of State for India of his claims to a pension. Lord Elgin, the New Viceroy, took up the subject warmly, and pressed it on the attention of the Home authorities as a special case:‡ "the case," as he observed, "of a man of science who had devoted himself for a very small salary to duties in connexion with the Asiatic Society, a body aided by and closely identified with the Government of India, from which the public have derived great advantage." After describing Blyth as "the creator of the Natural History Museum, which has hitherto supplied the place of a public museum in the Metropolis of India, and which will probably soon be made over to Government as part of a national museum," and referring to the importance of Blyth's labours in zoology in maintaining and extending the character and standing of our Society, this dispatch concludes thus: "His Excellency in Council considers, therefore, that if under such circumstances Mr. Blyth should, after twenty years' service, be compelled to retire from ill health, brought on very much by his exertions in pursuit of science, it would not be creditable to the Government that he should be allowed to leave without any retiring pension."

Meanwhile, Blyth was only enabled to remain at his post by the facilities which the Council afforded him of making short successive visits to Burma. He was for some five months in that province, from which, and more especially from the Yonzalin River, he communicated several interesting letters. His camp life there agreed with him, and he had kind friends like Phayre, Fytche, and Tickell to associate with and take care of him. His return to Calcutta was always attended by a relapse, and the hot season of 1862 brough him to a state for which there was no alternative but instant departure for Europe. As yet, however, no orders had been received from home in regard to the pension. It was clear that for these it would not do to wait, and the Council* under the emergency gave Blyth a year's leave on full pay. He had hardly gone when the expected reply was received and this, notwithstanding the Viceroy's strongly expressed opinion, proved† an unfavourable one. Eventually‡ a pension of £150 a year was conceded, owing, I believe, mainly to the untiring efforts made in London on Blyth's behalf by the late Sir P. Cautley and Dr. Falconer.

By the end of 1864 our Society's negociations with the Government for the transfer of its collections to the Indian Museum had been brought to a successful close, and at the November meeting the following just tribute was paid to our late Curator in the form of a resolution, which, on the Council's proposition, was carried unanimously:—

"One the eve of transferring the zoological collections of the Society to Government, to form the nucleus of an Imperial Museum of Natural History, the Society wishes to record its sense of the important services rendered by its late Curator, Mr. Blyth, in the formation of those collections. In the period of twenty-two years during which Mr. Blyth was Curator of the Society's Museum, he has formed a large and valuable series of specimens richly illustrative of the ornithology of India and the Burmese Peninsula, and has added largely to the Mammalian and other vertebrate collections of the Museum; while, by his numerous descriptive papers and catalogues* of the Museum specimens, he has made the materials thus amassed by him subservient to zoological science at large, and especially valuable to those engaged in the

† J.B.A.S. xxxi. 60.
‡ *Idem.* xxxi. 430.

* The Council's action in anticipation of the vote of a meeting was cordially approved at our annual meeting of 1863, but was protested against as illegal by Mr. Oldham.
† J.B.A.S. xxxii. 32.
‡ J.B.A.S. xxxiii. 73.
* Blyth's Catalogue of Mammalia was published in 1863, its last sheets being carried through the press by his friend Jerdon.

study of the vertebrate fauna of India and its adjoining countries."†

Blyth was elected an Honorary Member of the Society in the following year. The Museum was now under a Board of Trustees, and a new Curator, better paid, and with all the prospective advantages of a government official, had taken charge of it. Writing to me from Malvern, in June, 1865, Blyth says: "I had always a presentiment that my successor in the Museum would be more adequately remunerated, beginning with just double what I had after more than twenty years' work, with an additional £50 yearly, and house accommodation! How very much more could I have accomplished with such an income!" With this mild explosion he brushed off discontent, and strove to make the most of his small means. His letters to me, and these were frequent up to the time of my leaving India in 1868, were full of his own special subject; some of them are published in our Society's Proceedings.

In January, 1864, Blyth visited Dublin, where he read two papers before the Royal Irish Academy. The first of these was 'On the True Stags or Elaphine division of genus *Cervus*,' and does not appear to have been printed *in extenso* in the Academy's Proceedings.‡ His other paper, 'On the Animal Inhabitants of Ancient Ireland,' was published at length in the Academy's Proceedings§ of January 25th. What the extraordinary bones were which he exhibited at the meeting, and which he referred to as "probably Tibetan," was not explained in any of his letters.

At a meeting of the Geological|| Society of Dublin, he made some remarks on a paper of Professor Haughton's 'On Geological Epochs,' and expressed his concurrence in Dr. Carte's identification of the bones of the Polar Bear discovered in Lough Gur, in County Limerick. On further examination, however, these bones have been pronounced by Mr. Busk to be indistinguishable from those of *Ursus ferox*.

The question of zoological distribution will be found to have been treated by Blyth, in a paper which he contributed to 'Nature' in 1871 (March 30). He had been led to consider it while drawing up the introductory chapter which was to preface these catalogues, for in a letter to me dated 15th July of that year he refers to this MS. as follows:—

"I suppose that Phayre showed you my sketch of what I conceive to be the true regions and sub-regions of S. E. Asia, and I expected that he would have modified somewhat my notions with regard to the provinces into which I venture to divide the *Indo-Chinese sub-region*, but he seems to have assented to them altogether. Only yesterday I received the 'Proceedings of the Asiatic Society' for April and May last, and the 'Journal of the Asiatic Society' for April and May last, and the 'Journal of the Asiatic Society of Bengal,' Part II., No. 1, 1871, and in p. 84 of the 'Proceedings' I find some remarks by Stoliczka which quite confirm my views, only that I think that, with regard to the extension of the Malayan fauna into India, he should rather have said *Southern* India, because the African affinities of Central and Northern India, inclusive of the Siwâlik Deposits, are of ancient date, as shown by the occurrence of *Bos namadicus* in Central India, which is barely separable from the European *B. primigenius* (a type of *Bos* which is elsewhere only known from Europe), and by the presence of giraffes and of antelopes of African type in the Siwâlik Deposits. I have such an enormous mass of valuable facts to deal with, that I gave over making them public in driblets at the meetings of the Zoological Society; and I have now time and undisturbed leisure to treat of them in a work which I am preparing on 'The Origination of Species,' a subject upon which I think I can throw some light."*

As pointed out in a note, Blyth's 'Austral-Asian region' is generally the same with Dr. Sclater's 'Indian region,' *minus* Hindustan proper or the plains of Upper India east and south of the north-west desert—the Dukhun or tableland of the Peninsula with the intervening territory, inclusive of the Vindhyan Ghats—the Coromandel Coast and the low northern half of Ceylon—all of which Blyth places in his Ethiopian region. What remains of India after this large deduction Blyth distributes through three sub-regions, viz. the Himalayan, Indo-Chinese,

† J.B.A.S. xxxiii. 582.

‡ Vol. viii. Jan. 11, 1864, p. 458.

§ *Id. qu. sup.* p. 472.

|| Proceedings G. S. D. for January 13, 1864, Journ. p. 173.

* Among the papers left by Blyth is one headed 'Origination of the Various Races of Man,' which he may have intended to form part of the book here referred to. It contains nothing original, but brings together numerous points of resemblance and contrast observable in the several groups of the order Primates.

and Cinghalese. India cannot, he argues, be treated as a natural zoological province: it is a border-land in which different zoological regions meet, and one, therefore, "of extraordinarily complex zoological affinities." Burma of course falls within his Indo-Chinese subregion, which extends southward as far as Penang and Province Wellesley, where his Malayan sub-region commences.

The interest which Blyth had always taken in the Rhinoceros group was revived by the safe arrival at the Zoological Gardens of the Chittagong individual, the *Ceratorhinus crossei* of the present Catalogue. In his paper contributed to the 'Annals' in 1872, he argues against Gray's assignment of this species to *Rhinoceros sumatrensis*, and in favour of its identity with the fine Tavoy specimen shot by Col. Fytche, and figured in this Journal, vol. xxxi. p. 156. Blyth's conjecture that the Arakan Hills is one of the habitats of this species is borne out by the letter in which Capt. Lewin, the superintendent of the Hill Tracts of Chittagong, first reported to me in 1867 the capture of the animal.* After giving her measurements, which were then 6 feet from crown of head to root of tail, and 4 feet 2 inches in height, and otherwise minutely describing her horns, Capt. Lewin adds: "You are mistaken I think in supposing that she has come from the Tenasserim Provinces—the two-horned species is found in my hills. I have seen one alive, and several of my men have seen a dead one."

In the Journal of Travel and Natural History, No. 2,† of 1868, will be found a letter from Blyth in explanation of some remarks which he had made at the Zoological Society on the occasional shedding or loss by violence of rhinoceros' horns, followed by their renewal. In this he takes the opportunity of pointing out the tendency which some species have to develope a rudimentary horn on the forehead, and argues for the possible explanation in this manner of cases of three-horned rhinoceroses being reported by travellers.

The connexion which Blyth established, first with 'Land and Water,' and later with the 'Field,' gave him interesting literary occupation; and the 'Naturalist' columns of both these journals abound in scraps by 'Zoophilus,' which did real service to the advancement of scientific truth. No pen so ready as his to expose current fallacies or sensational announcements in works of travel of the results of loose and careless observations. Very many of his 'scraps' are worthy of being collected and preserved, for such use as we see they have been turned to by Mr. Darwin. These columns occasionally contained more elaborate papers, such as the series in the 'Field' for 1873, on 'Wild Animals dispersed by human agency,' and 'On the Gruidæ or Crane family.' This monograph, for such it amounts to, was its writer's last utterance. He had long been ailing, and in the autumn of this year he became very ill, and went to Antwerp for a change. On his return he called on me, feeling, as he said, better, though complaining of great prostration. He seemed full of what he had seen in the Antwerp Zoological Garden, where he thought he had found another new species of Rhinoceros. This was our last interview. Though nursed by a tenderly-attached sister, his weakness increased, and he died of heart disease on the 27th of December, within a day or two of his 63rd birthday.

More competent authorities than I can pretend to be have done justice to the high intellectual powers which Blyth displayed from the outset of his career as a naturalist, to the wonderful capacity and accuracy of his memory, which, unassisted by any systematic notes, assimilated the facts once stored in it, and enabled him readily to refer to his authority for them; to his great power of generalization, and to the conscientious use which he made of it. Abundant proof of the high respect with which his opinions were always listened to, and of the careful consideration given to them even where they were not accepted, is to be found in the published works of his brother naturalists. No higher testimony to his habitual scientific caution need be adduced than that of Mr. Darwin, but it is equally borne by Jerdon throughout his published writings. Gould* refers to him as "one of the first zoologists of his time, and the founder of the study of that science in India." I confine myself here to putting on record the tribute of an old and intimate friend, to the excellent qualities of heart possessed by Blyth. The warmth and freshness of his feelings which first inspired him with the love of Nature clung to him through his chequered life, and kept him on good terms

* The date of capture is erroneously given, both by Mr. Blyth and by Dr. Anderson in his cited communication to the Zoological Society.

† Page 130.

* 'Birds of Asia,' Pt. XXVI. *Trochalopteron blythii.*

with the world, which punished him, as it is wont to do, for not learning more of its wisdom. Had he been a less imaginative and a more practical man, he must have been a prosperous one. Few men who have written so much have left in their writings so little that is bitter. No man that I have ever known was so free as he was from the spirit of intolerance; and the absence of this is a marked feature in all his controversial papers. All too that he knew was at the service of everybody. No one asking him for information asked in vain. Among the many pleasurable reminiscences of my own long residence in India, few are more agreeable than those which recall his frequent Sunday visits to me.

A. GROTE.

LONDON, *August* 27, 1875.

The manuscript of the article by Dr. Eiseley was submitted to the Society, September 4, 1958.