

Journal, vol. ix. p. 271, 1899). The examination of the photographs has resulted in the possibility of arranging ten of the stars in a series indicating progressive evolution, and the four given are sufficiently representative to show the changes indicated. These are—

- I. 280 Schjellerup = DM $50^{\circ}28'10''$ (Magn. 7.8).
 II. 273 „ = 19 Piscium (Magn. $5.5 \pm$).
 III. 132 „ = U Hydrae (Magn. $5.5 \pm$).
 IV. 152 „ = (Magn. 5.5).

The presence of *bright lines* formerly announced is confirmed by these photographs, and some of these are identical with those observed visually by Prof. Duner at Upsala. Any attempt to establish a connection between these stars and those of other types must include these bright lines, but as yet no star is known intermediate in character between these red stars and other groups. In the absence of a suitable instrument for detecting such bodies at the Yerkes Observatory, advantage has been taken of an offer from Prof. Pickering to photograph suspected objects with the objective prism, and in case this indicates a body of new constitution, the 40-inch refractor and stellar spectrograph will be employed for its detailed examination.

The photographs extend from $\lambda 5150$ to $\lambda 5850$, the carbon fluting with maximum about $\lambda 563$ being specially distinct in the spectra of 19 Piscium and U Hydrae.

Bulletin No. 9, in the same number of the *Journal*, p. 273, contains a plate illustrating a later attempt to find some position for these stars of Class III. *b* in the stellar constitutional system. The stars compared are—

- I. The Sun (Type II.).
 II. μ Geminorum (Type III.).
 III. 132 Schjellerup (Type IV.).

In the region extending from b_4 to about $\lambda 5300$, the spectra of μ Geminorum (Type III.) and 132 Schjellerup (Type IV.) are almost identical, while in the region slightly less refrangible there are many common lines. Further towards the red the spectra become very dissimilar, the strong flutings of carbon seen in 132 Schjellerup being entirely wanting in μ Geminorum, although there are a few common features sufficient for comparison. Other photographs in the region H β to H γ show similar coincidences. These photographs, it is stated, show a decided connection between the two classes of red stars, and the observation of more of them may bring out other links in their relationship.

REMINISCENCES OF DARWIN—SIR JOSEPH D. HOOKER.

A STATUE of Charles Darwin by Mr. Hope Pinker, presented to the University of Oxford by Prof. Poulton, Hope Professor of Zoology, was unveiled at the University Museum on the 14th inst., and Sir Joseph D. Hooker delivered the following address, which we reprint from the *Times*, upon the occasion:—

The Vice-Chancellor of your University has done me the honour of asking me to address you on the occasion of the installation of the statue of the great naturalist which now adorns your museum, and has expressed his opinion that a few personal reminiscences would be more acceptable to you from me than an *éloge* of Mr. Darwin's researches and discoveries, of which latter indeed an excellent reasoned *résumé* is well known to you as the work of your Hope professor of zoology. In accepting the task of giving personal reminiscences, I am reminded of the fact that narrators of an advanced age are not only proverbially oblivious, but are too often the victims of self-deception in respect of what they think they remember, to which must be added that where a dual personification is attempted the narrator is apt to assume the more prominent position. I have thus many snares to avoid, and must hope for a lenient judgment on what follows.

EARLY FRIENDSHIP WITH DARWIN.

The fact of our having commenced our scientific careers under very similar conditions favoured the rapid growth of a bond of friendship between Mr. Darwin and myself. We both of us, immediately after leaving our respective Universities, commenced active life as naturalists under the flag of the Royal Navy; he as a volunteer eight years before me, who was an official. We both sailed round the world, collecting and observing often in the same regions, many of them at that time seldom visited and

since made accessible to science by his researches—the Cape Verde Islands, St. Helena, Rio, the Cape of Good Hope, the Falkland Islands, Tierra del Fuego, Tasmania, and New Zealand. On returning to England we both enjoyed the rare advantage of the counsel and encouragement of one of the greatest leaders in science of the time—Mr., afterwards Sir Charles, Lyell. It was through the father of Sir C. Lyell, the translator of the “*Vita Nuova*” of Dante, and a friend of my father, that I first heard of Mr. Darwin. The “*Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of the *Beagle**” was then passing through the press, and the proof sheets were being submitted to Sir C. Lyell for his information and criticisms. These were passed on to Sir Charles's father, himself a naturalist, who was permitted to lend them to me for perusal, because I was then preparing to accompany Sir James Ross as a naturalist on the Antarctic expedition (1839–43). At that particular time I was engaged upon engrossing hospital duties, and I slept with the proofs under my pillow that I might at once, on awaking, devour their contents. They impressed me profoundly, I may say despairingly, with the genius of the writer, the variety of his acquirements, the keenness of his powers of observation, and the lucidity of his descriptions. To follow in his footsteps, at however great a distance, seemed to be a hopeless aspiration; nevertheless they quickened my enthusiasm in the desire to travel and observe. A copy of the complete work was a parting gift from Mr. Lyell on the eve of my leaving England, and no more instructive and inspiring work occupied the bookshelf of my narrow quarters throughout the voyage. In the interval I had been introduced to Mr. Darwin, on a casual meeting in Trafalgar-square by a brother officer who had accompanied him in the *Beagle* to Rio, when I was impressed by his animated expression, heavy beetle brow, mellow voice, and delightfully frank and cordial greeting to his former shipmate. Shortly after the arrival in England of the Antarctic expedition (in 1843) I received from Mr. Darwin a long letter, warmly congratulating me on my return to my family and friends, directing my attention to the importance of correlating the flora of Fuegia with those of the Cordillera and of Europe, and inviting me to study and publish the botanical collections which he had made in the Galapagos Islands, Patagonia, and Fuegia.

VISITS TO DARWIN AT DOWN.

This led to an interchange of views on the subject of geographical distribution, followed by an invitation to visit him at what he used to call his inaccessible home at Down, which was then eight or ten miles distant from the nearest railroad station. This I joyfully accepted; and then commenced that friendship which ripened rapidly into feelings of esteem and reverence for his life, works, and character that were never clouded for one instant during the forty subsequent years of our joint lives. In the admirable biography of his father by my friend, Prof. Frank Darwin, are recorded the subjects, especially botanical and geographical, which were for many years the subjects of conversation and correspondence between us. During the many visits to Down which followed, he laid before me without reserve, not only his vast stores of knowledge, but his mature and immature speculations and theories, describing how they originated, and dwelling on their influence on the progress of his researches. Among these, so long ago as 1844, was his sketch of “*The Origin of Species*,” which I was the first to see of the few friends to whom he ever showed it. At that very early period of my own studies I failed to grasp its full significance, *à propos* of which I may mention that I have been reproached for this by friends who have wondered, not only that I did not assimilate it at once, but that I did not apply it to my earliest essays on the distribution of plants. My friends overlooked the fact that the communication was a confidential one, of a hypothesis which its author hoped to establish as a tenable theory by an accumulation of facts in support of it, which he was engaged in collecting with a view to future publication. On the occasions of many other visits it was Mr. Darwin's practice to ask me, shortly after breakfast, to retire with him to his study for twenty minutes or so, when he brought out a long list of questions to put to me on the botanical subjects then engaging his attention. These questions were sometimes answered offhand, others required consideration, and others a protracted research in the Herbarium or in the gardens at Kew. The answers were written on slips of paper, which were deposited in bags or pockets that hung against the wall within

reach of his arm, each of them a receptacle devoted to a special object of inquiry. To me this operation of "pumping," as he called it, was most instructive. I could not but feel that any information that I could give him was comparatively trivial, while what I carried away was often as much as I could stagger under. As his health fluctuated or declined, and especially during his sharper attacks of illness, these interviews became intermittent, and on such occasions he would ask me to bring my own work with me to Down, where I pursued my studies free from the distractions of Kew, and with the advantages of his counsel and aid whenever desired. These morning interviews were followed by his taking a complete rest, for they always exhausted him, often producing a buzzing noise in the head, and sometimes what he called "stars in the eyes," the latter too often the prelude of an attack of violent eczema in the head, during which he was hardly recognisable. These attacks were followed by a period of what with him was the nearest approach to health, and always to activity. Shortly before lunch I used to hear his mellow voice under my window, summoning me to walk with him, first to inspect the experiments in his little plant-houses, and then to take a precise number of rounds of the "sand-walk," which he trudged with quick step, staff in hand, wearing a broad-brimmed straw hat and light shooting coat in summer, and a felt hat and warm cape in winter. This walk was repeated in the afternoon; on both these occasions his conversation was delightful, animated when he was well enough, never depressing however ill he might be. It turned naturally on the scenes we had witnessed in far-away regions and anecdotes of our seafaring lives, and on the discoveries in science, then, as now, hurrying onwards and treading on one another's heels in their haste for recognition. In the evening we had books and music, of which latter Mr. Darwin was, during the first few years of our friendship, almost passionately fond. I well remember now, at the 1847 meeting of the British Association in this city, his asking me to accompany him to hear the organ at New College Chapel, and, on coming away, saying to me, "Hooker, I felt it up and down my back;" and I find in the "Life and Letters" that when a student at Cambridge, after hearing a beautiful anthem, he made use of a similar expression to a friend who had accompanied him. It is a curious fact that music should have had in after life no charm for him—that "it set him thinking too energetically at what he had been at work on instead of giving him pleasure."

AN ESTIMATE OF DARWIN'S CHARACTER.

If I were asked what traits in Mr. Darwin's character appeared to me most remarkable during the many exercises of his intellect that I was privileged to bear witness to, they would be, first, his self-control and indomitable perseverance under bodily suffering, then his ready grasp of difficult problems, and, lastly, the power of turning to account the waste observations, failures, and even the blunders of his predecessors in whatever subject of inquiry. It was this power of utilising the vain efforts of others which in my friend Sir James Paget's opinion afforded the best evidence of Darwin's genius. Like so many men who have been great discoverers, or whose works or writings are proofs of their having intellects indicating great originality, he was wont to attribute his success to industry rather than ability. "It is dogged that does it" was an expression he often made use of. In his autobiography he says of himself, "My industry has been nearly as great as it could have been in the observation and collection of facts"; and, again, "of the complex and diversified mental qualities and conditions which determined my success as a man of science, I regard as the most important the love of science—unbounded patience in long reflecting over many subjects—industry in observing facts, and a fair share of invention, as well as of common sense." In this introspection he has, if my judgment is correct, greatly undervalued "invention"; that is originality or that outcome of the exercise of the imagination which is so conspicuous in every experiment he made or controlled, and in the genesis of every new fact or idea that he first brought to light. Referring to his disregard when possible of his bodily sufferings, I remember his once saying to me that his sleepless nights had their advantages, for they enabled him to forget his hours of misery when recording the movement of his beloved plants from dark to dawn and daybreak. For those other qualities of head and heart that endeared Mr. Darwin to his friends I must refer you to the "Life and Letters." There is

only one upon which I would comment, it is that passage of his autobiography where he says, "I have no great quickness of apprehension or wit." Possibly the "of" and "or" are here transposed; whether or no, my impression of his conversation has left the opposite as characteristic of him. It is, at any rate, inconsistent with the fact that in arguing he was ever ready with repartee, as I many times experienced to my discomfort, though never to my displeasure; it was a physis so thoughtfully and kindly exhibited. And I may conclude these fragmentary records with an anecdote which goes, I think, to support my view, and which I give, if not verbally correctly, as nearly as my memory of so ancient an episode permits. I was describing to him the reception at the Linnean Society, where he was unable to be present, of his now famous account of "The two forms or dimorphic condition of *Primula*," for which he took the common primrose as an illustration. On that occasion an enthusiastic admirer of its author got up, and in concluding his *loge* likened British botanists who had overlooked so conspicuous and beautiful a contrivance to effect cross-fertilisation to Wordsworth's "Peter Bell," to whom

"A primrose on the river's brim
A yellow primrose was to him,
And it was nothing more."

When I told Mr. Darwin of this he roared with laughter, and, slapping his side with his hand, a rather common trick with him when excited, he said, "I would rather be the man who thought of that on the spur of the moment than have written the paper that suggested it."

"AMERIND"—A SUGGESTED DESIGNATION FOR AMERICAN ABORIGINES.

A PART of the *Proceedings* of the Anthropological Society of Washington, at a meeting on May 23, seem destined to produce permanent influence on ethnologic nomenclature; this part of the proceedings taking the form of a symposium on the name of the native American tribes. The discussion was opened by Colonel F. F. Hilder, of the Bureau of American Ethnology, with a critical account of the origin of the misnomer "Indian," applied by Columbus to the American aborigines; he was followed by Major J. W. Powell, who advocated the substitution of the name *Amerind*, recently suggested in a conference with lexicographers. A communication by Dr. O. T. Mason followed, in which the various schemes of ethnologic classification and nomenclature were summarised and discussed. Contributions to the symposium were made also by Dr. Albert S. Gatschet, Dr. Thomas Wilson, and Miss Alice C. Fletcher. At the close of the discussion the contributions were summarised by President McGee as follows:—

(1) There is no satisfactory denotive term in use to designate the native American tribes. Most biologists and many ethnologists employ the term "American"; but this term is inappropriate, in that it connotes, and is commonly used for, the present predominantly Caucasian population. The term "Indian" is used in popular speech and writing, and to a slight extent in ethnologic literature; but it is seriously objectionable in that it perpetuates an error, and for the further reason that it connotes, and so confuses, distinct peoples. Various descriptive or connotive terms are also in use, such as "North American Savages," "Red Men," &c.; but these designations are often misleading, and never adapted to convenient employment in a denotive way.

(2) In most cases, the classifications on which current nomenclature are based, and many terms depending on them for definition, are obsolete; and the retention of the unsuitable nomenclature of the past tends to perpetuate misleading classifications.

(3) While the name "Indian" is firmly fixed in American literature and speech, and must long retain its current meaning (at least as a synonym), the need of scientific students for a definite designation is such that any suitable term acceptable to ethnologists may be expected to come into use with considerable rapidity. In this, as in other respects, the body of working specialists form the court of last appeal; and it cannot be doubted that their decision will eventually be adopted by thinkers along other lines.

(4) As the most active students of the native American tribes, it would seem to be incumbent on American ethnologists to propose a general designation for these tribes.