LIFE-STORIES OF FAMOUS MEN

CHARLES DARWIN

LEONARD HUXLEY

WATTS AND CO.

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John van Wyhe
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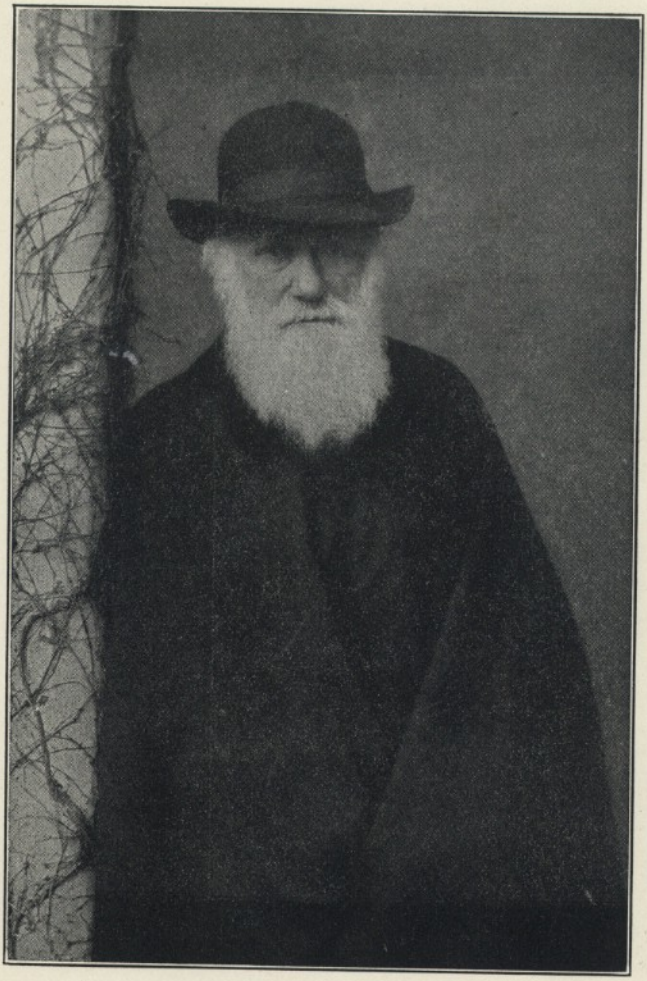

CHARLES DARWIN (1881)

Frontispiece

CHARLES DARWIN

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I

ANCESTRY

THE date 1809 invariably calls up recollections of a singularly quaint and picturesque personage. John Stuart Blackie, perfervid Scot and idealist Grecian, a silver-haired Professor in a poet's plaid, loved to acclaim that wonderful year. "It is the *Annus Mirabilis* of the century," he would cry; "the year in which more great men entered this world than any other. It was the birth year of Gladstone and Lincoln, and Tennyson and Fitz-Gerald and Wendell Holmes, and Poe and Mendelssohn and Chopin and Darwin and Monckton Milnes—and—John Stuart Blackie." I think the blue eyes used to flash more over the recital of so renowned a list than they twinkled at the conclusion, which was so unexpected by the hearer. Be that as it may, the list includes more men than one who profoundly modified the world into which they were born. In music, indeed, the revolutionaries were not yet; but Tennyson, with his exquisitely polished lute, gave his countrymen not only new beauties of word and song, but a new poetic interpretation of Nature in relation to man and his latest knowledge. Gladstone forwarded a democratic expansion which has incalculably altered the balance of constitutional power in our own country. Lincoln, with blood and tears, founded a new and more hallowed Union of the Great West. Darwin, by finding a *vera causa* for the majestic processes of creative nature, initiated a

revolution of thought confined to no country and to no continent.

Within Darwin's own lifetime it was possible to look back over a field of intellectual change unparalleled since the Renaissance. As then there were a new learning, new discoveries of buried knowledge, new sources to be opened up—all contributing to the marvellous new illumination, every voyage among the old-new books like a venture to one of the newly-open quarters of the globe, with measureless possibilities of treasure in golden ideas and revelations beyond the narrow limits of the accepted doctrines—so now a great and fertile idea, once established, burst the dykes of ancient orthodoxy that hemmed thought in. It gave coherence to the incoherent accumulations of natural knowledge; it stimulated research to find further proof or disproof of its own validity; it made advance possible by providing an intelligible line along which to work. All the natural sciences were affected by it; all responded to its vivifying touch. Science withal acquired a new dignity. It re-interpreted man's nature and man's destiny; it offered new clues to the relation between him and the universe in which he finds himself; it attempted to fling a bridge, however frail, over the dark chasm that severs the material and the mental worlds. It called out a new metaphysic and a new theology, profoundly affected by the fresh view of the universe without and of the world of psychology and ethics within. Natural science, therefore, shared in the honourable dignity so long accorded to these thought-sciences.

What manner of man was he who put this new and fruitful life into the ancient evolutionary idea? How was he equipped for the task by his natural birthright and his early education?

He came of two distinguished stocks. The Darwins

were a Lincolnshire family of good standing since 1500. From Commonwealth times, three generations of Darwins became barristers. The third of these, who early retired from practice to the life of a country squire on his estate at Elston, was the first of the family to show some taste for science, and this he passed on, enriched by his wife's literary gift, to two of his sons—especially to the famous Erasmus Darwin (1731–1802), in practice a physician, by taste and inclination at once poet, philosopher, and lover of natural history. Erasmus's mother, we suspect, was a bit of a blue-stocking—such "a wife that talketh Latine," as her husband prayed to be delivered from; but her gifts stood her son in better stead than her husband. His literary and philosophic fame became so well established that three years after the *Origin of Species* appeared an ordinary well-read person could be struck with the coincidence that the great Dr. Darwin's subject had been taken up by some one of the same name, and wonder whether there were any connection between the two.

As a younger son, Erasmus had to choose a profession, and his scientific bent doubtless led him to become a doctor. Settling at Lichfield, he attained remarkable success, and was even invited to leave that city for London and become the King's physician. In the intellectual circle which gave late eighteenth-century Lichfield a distinct literary reputation, he was a prominent member, and had among his friends Boulton, Watt, Wedgwood the potter, the Sewards, Edgeworths, and Day of *Sandford and Merton* fame. Dr. Johnson he also met; but, as a Radical and Freethinker, did not get on with that indomitable old Tory.

He was fifty-eight when he attained general fame by the publication of part of his scientific poem, *The Botanic Garden*, divided into "The Loves of the Plants"

and "The Economy of Vegetation." Written in the style of Pope, with rich word-painting, at once rhetorical and didactic, blending accurate scientific descriptions with fantastic personifications and imagery, it seems to modern judgment amazingly bad poetry, yet displaying a powerful mind. But it awakened interest in nature, and its vogue lasted more than half a generation till it was pricked by that brilliant parody in the anti-Jacobin, *The Loves of the Triangles*, and, as Jeffery put it, fine ladies and gentlemen ceased to talk of "gnomes, sylphs, oxygen, gossamer, polygynia, and polyandria."

The Botanic Garden was followed by his great prose work on Natural History, the *Zoönomia : Laws of Organic Life*, an evolutionary speculation, suggesting the modification of species by purposeful adaptations of individuals to their wants, and endowing plants with a kind of life and intelligence. If his speculations were insufficiently ballasted with the hard fact of experiment, he had glimpses of many principles and observations which were turned to account half a century later by his grandson.

There were many obvious likenesses between Erasmus and Charles Darwin. Charles inherited his height, but not his bulky figure nor his features. They had the same benevolence, sympathy, and charm of manner; the same indifference to fame and absence of self-conceit; the same swift anger over inhumanity or injustice; but, instead of Erasmus's love of mechanism and his literary and poetical temperament, Charles displayed a love of exercise and field sports, and a unique modesty and simplicity of character, entirely free from the touch of severity or caustic humour which Erasmus, though named "the benevolent," appears to have possessed.

Besides these deep-rooted differences of temperament, there was another profound intellectual difference which gave the greater value to Charles's work. With

the love of natural history and the scientific turn of mind which came out in him so strongly, Charles inherited his grandfather's "vividness of imagination," which led to "his overpowering tendency to theorize and generalize," but with the important qualification that in his own case "this tendency was kept in check by his determination to test his theories to the utmost." A part of this testing was the honesty and patience with which he would note down every objection or difficulty that arose in the course of his work, and try to seek out its explanation. Thus, when the *Origin* appeared, he found that there were very few critical objections made which he had not himself set forth and largely met in advance. Singular candour which frankly invited judgment on its difficulties instead of slurring them over!

Erasmus had been dead seven years before Charles was born, so that the younger evolutionist had no direct link with his grandfather—not even a childish memory. On the other hand, he had a very close personal tie with his father, Robert Waring Darwin. To him he owed much more through personal example than through intellectual inheritance. He, too, was a doctor, settled in Shrewsbury, and remarkably successful in his wide practice, for he possessed every qualification for healing men and winning their utmost confidence, although he at first had disliked doctoring and hated the sight of blood. With a quick and vivid temperament which loved society, and gave brightness, variety, and interest to the home life in which his family took their part, he was shrewd, sagacious, observant, extremely generous, full of sympathy alike in pleasures and pains. He had an almost disconcerting intuition in reading character as in diagnosis, so that people came to him in their private troubles as well as in sickness. Charles Darwin, who otherwise put little faith in doctors, retained an unlimited belief in his

father's medical instinct and methods, and would quote some maxim or hint of his in most cases of illness. Indeed, his recollection of everything connected with his father was peculiarly distinct, and he spoke of him frequently, generally prefacing an anecdote with some such phrase as: "My father, who was the wisest man I ever knew....." And, as he wrote to Hooker, in an intimate letter: "I do not think any one could love a father much more than I did mine, and I do not believe three or four days ever pass without my still thinking of him."

But, though Dr. Darwin inspired this "boundless and most touching reverence" in his son, he contributed little intellectually, except that his habit of forming a theory for almost everything which occurred may have reappeared in his son's larger passion for accurate generalization, urged on by an "intolerable desire not to be baffled." His grasp was for details: though thus theorizing, he had not a scientific mind, and did not try to arrange his knowledge under wide laws; man, rather than nature, was his chief study, and his only taste in the direction of natural history was the love of plants in his garden. The heritage of devotion to natural history and the scientific turn of mind were more apparent in his brothers.

On the other side, Charles Darwin came of the Wedgwood stock. His mother, who died before he was eight years old, was the daughter of the famous potter, Josiah Wedgwood, of Etruria—a man not less celebrated for his high character than for his artistic, inventive, and practical powers. On this side Charles Darwin may well have derived further strains of both moral and intellectual power.

With such strands woven into his nature—sound stuff of warp and weft—we should expect to find in Charles

Darwin a high potentiality of both character and capacity; an expectation justified by the high average of such qualities displayed in the various ramifications of the families to which he belonged.

For the student of intellectual inheritance it is interesting to note how the vigorous capacity of the Darwin-Wedgwood stock, re-inforced by Charles Darwin's own marriage to a Wedgwood, took a strong mathematical turn in more than one of his sons. This particular re-casting of intellectual power I believe to be not infrequent; nor is it matter of surprise, considering that the power of numeration—as indeed is evidenced by the popular phrase of "putting two and two together"—lies at the very root of reasoning thought, and the higher mathematical faculty is but a concentration of this most general faculty.

II

CHILDHOOD AND EDUCATION

HE was a docile child, strong and active, and, though loving sport, far humaner than most such boys; in mind not precocious, but fond of long, solitary walks, absorbed in his own thoughts, with imagination enough to enjoy romancing about himself and his doings for the sake of producing an effect; but he was cured of this habit by his family taking no notice of his romances. He had a strong taste already for natural history, and especially for collecting—a taste innate in him, for his brother and sisters did not share that "passion for collecting which leads a man to be a systematic naturalist, a virtuoso, or a miser." The first trial of regular education, however, which was given him was scarcely suited to bringing out his special capacities. It was almost the antipodes of such an education as would be given now to a youth who intended to follow natural science, although to-day there are still to be found men of linguistic and abstract training, and covered with distinction in very different fields, who, in their later years, without regular training, have turned to and done even detailed anatomical work in little-explored fields of natural science.

But in those days natural science was not sufficiently organized to be a school subject; nor, indeed, was a scientific training contemplated for Charles Darwin. Shrewsbury School, to which he was sent at the age of nine, was then, and long continued to be, the shrine of

the classics kept undefiled, as far as might be, by contact with baser studies, such as modern languages or science, mathematical or physical. Darwin found it bad for the development of his mind, and of no help to the training of his powers of language. Though he had strong and diversified tastes, with zeal for whatever interested him, and pleasure in understanding any complex subject or thing, that zeal and pleasure were in things outside the school curriculum. Clearly he loved knowledge better than learning.

He enjoyed the clear geometrical proofs of Euclid, learnt with his tutor. He used to sit for hours reading the historical plays of Shakespeare, generally in an old window in the thick walls of the school. Other poetry, too, he read, such as Thomson's "Seasons" and the newly-published poems of Byron and Scott.

He records how, in his later schooldays, he plunged into practical chemistry, helping his elder brother, who had rigged up a very fair laboratory in the tool-house in their garden at home, and reading several books on the subject. This out-of-school work, which gained him the nickname of "Gas" in school, he afterwards held to be the best part of his boyhood education, since it showed him practically the meaning of experimental science. But the reputation for taking interest in such an unprofitable and unscholastic subject brought down on him a public rebuke from his headmaster, Dr. Butler, who dubbed him a *poco curante*. "As I did not understand what he meant," adds the victim, "it seemed to me a fearful reproach."

No doubt this more organized work helped to give breadth and depth to his boyish bent for collecting anything out of which a collection could be made —whether flowers, seals, coins, or minerals (it was too early for postage stamps)—a bent which chiefly

exercised his keen powers of observation, and when turned to natural history, and especially insects, before long made him an ardent and indefatigable coleopterist. Unboylike, however, he almost made up his mind to begin collecting only the insects he could find dead, for on consulting his sister he concluded that it was not right to kill insects for the sake of making a collection. The reading of White's *Selborne* also led him to watch birds, and even to make notes upon them, wondering the while, in his simple delight, why every gentleman did not become an ornithologist. Strongest of all was an absorbing passion for shooting. "How I did enjoy shooting!" he writes. "But I think I must have been half-consciously ashamed of my zeal, for I tried to persuade myself that shooting was almost an intellectual employment; it required so much skill to judge where to find most game and to hunt the dogs well." With a curious appropriateness, the sport which had sharpened his powers of observation passed into the background during his travels in South America, when the ever-widening scope of observation in its turn became engrossing.

Thus, to the official eye, his school career at Shrewsbury was a failure, and in a moment of irritation Dr. Darwin once exclaimed to him: "You care for nothing but shooting, dogs, and rat-catching, and you will be a disgrace to yourself and all your family." "But my father," Charles Darwin continues, "who was the kindest man I ever knew, and whose memory I love with all my heart, must have been angry and somewhat unjust when he used such words."

Accordingly, Dr. Darwin wisely took him away from the classical school, where he was doing no good, and sent him at the age of sixteen and a-half to study medicine at Edinburgh University. His brother Erasmus

CHARLES DARWIN AND HIS SISTER CATHERINE (1817)

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was finishing his medical course, so that for Charles's first year the brothers were together. The lad had already been attending some of the poor folk in Shrewsbury as his father's unofficial assistant, and the old doctor—"who," says his son, "was by far the best judge of character I ever knew"—declared that he would make a successful physician, the chief element of success being the gift of inspiring confidence. Nevertheless, two years at Edinburgh convinced him that he was not cut out for a doctor. The same tenderness of heart which had made him hesitate to kill living insects for his collections revolted from the operations he saw—for anæsthetics were not; his sensitiveness recoiled from the dissecting room; and the lectures, save those on chemistry, were dull and dry. *Materia Medica* recalled only "cold, breakfastless hours on the properties of rhubarb"; and a course on geology—one of his favourite subjects—produced only the rash determination never to open a book again on the subject as long as he lived. At the same time he had begun to realize that, his father being well off, there was no imperious necessity to seek his livelihood by a profession he did not like. Still, though the second educational experiment failed, he gained from the contact with other minds in Edinburgh, as he was to gain yet more at Cambridge; and even in pure science he made a long step forward through his friendship with Dr. Grant, afterwards Professor of Zoology at University College, with whom he often used to go down to the seashore and collect marine creatures from the tidal pools. These he dissected, with an unpractised hand, indeed, and a "wretched microscope," despite which drawbacks he arrived at two little interesting discoveries—one that the so-called ova of *Flustra* were in fact larvæ, the other that what were supposed to be the young state of *Fucus loreus* were the egg-cases of a small, worm-like creature

He read papers on these subjects before the Plinian Society—the students' scientific society. As the society did not publish the papers read before it, the seventeen-year-old student did not have the satisfaction of seeing his name in print—a delightful experience which was to befall him first at Cambridge, when a rare beetle which he had caught was mentioned in Stephens's *Illustrations of British Insects*, with "the magic words, 'Captured by C. Darwin, Esq.'"

So he exchanged Edinburgh for Cambridge—the prospect of medicine for the prospect of the Church. Brought up to believe in the strict and literal truth of every word in the Bible, he easily satisfied his youthful scruples and hesitations by reading *Pearson on the Creed*, and similar theological works; while, in other respects, the life of a country clergyman promised to be congenial to the athletic young man, whose love of a country life was so strong that he seemed in danger of drifting into the character of an idle, sporting man. Since leaving school, however, he had not opened a classical book, and now found that he had forgotten every scrap of his classics, even to some few of the Greek letters of the alphabet. Instead of going up in the October term, he had to brush up his knowledge with a tutor, and did not go into residence at Christ's till the Spring of 1828.

This third educational experiment also failed to bring Charles Darwin his true education.

The three years at Cambridge bore fruit very differently from what his father had hoped. The regular academical training for a pass degree, with its moderate allowance of classics and mathematics, made little impression upon him, though he managed to take a respectable place among the "poll." Part of his time was spent in careless jollity among his sporting friends; but he made many friends also of a more intellectual

type—artistic, musical, and scientific. High spirits and the love of outdoor life were reflected in his gallops across country, his shooting excursions in the fens, his *vingt-et-un* supper parties—idle, no doubt, but of happy memory for their good fellowship. Science in its open-air form inspired his indefatigable pursuit of rare beetles and his triumphs as a collector and a coleopterist; his inborn artistic tastes, not yet submerged beneath the insistent flood of life-long sickness, took him to the treasures of the Fitzwilliam Gallery to serve his apprenticeship as a connoisseur of engravings, and to King's College Chapel for the exquisite music that used to "send a shiver down his backbone." Strange that the response to music evoked thus, or by the playing of his friends, should have been so deep in one who was really destitute of musical ear, and unable to perceive a discord or hum a tune correctly—a state which his musical friends soon detected, and used to amuse themselves now and then by putting him through an examination to see how many tunes he could recognize when played rather faster or slower than usual—a sore puzzle.

Noteworthy, too, is the fact that older men of high academical standing, including the famous Dr. Whewell, then tutor and soon to be Master of Trinity, must have found something unusual in the natural gifts of this young undergraduate. Chief of these older friends—and through whom, indeed, Darwin came to know them—was Henslow, then Professor of Botany, as formerly of Mineralogy. To Henslow he owed a great debt for personal friendship and intellectual encouragement. For Henslow was a teacher who did more for his young men outside the lecture-room than in it. Not only was he their guide in botanical and geological excursions, but in the weekly gatherings at his own house, where they were

admitted to a frank and simple intimacy of a rare quality, possible only with a man who united the mastery of half-a-dozen branches of science with modest candour, and the large simplicity of a strong and steadfast character with unfailing interest and courtesy towards all. Affection and admiration early knitted up a friendship which was to be life-long. In the later half of his time at Cambridge it became Darwin's habit on most days to take long walks with his friend, so that he was known to some of the dons as "the man who walks with Henslow." What impression the personal charm, enthusiasm, and real ability of the younger man made on his senior may be judged from Henslow's speedy selection of him for a post which demanded all these qualities.

Gradually the latent scientific instinct in him came out. His entomology was hardly so much as a scientific pursuit; it was the collector's passion. He touched on more scientific method when he attended Henslow's admirable lectures on botany, and when, in his last year, he enjoyed the close logical argument of Paley and Euclid. Finally, at Henslow's advice, he revived his geology during the two extra terms which, his examinations over, he had to keep before taking his degree, since he had originally come into residence at the bye term. It was through Henslow, too, that he was invited to accompany Sedgwick, the geologist, on a geological excursion in North Wales. Above all, he was fired by reading Herschel's *Introduction to the Study of Natural Philosophy* and Humboldt's *Personal Narrative*, on the strength of which he planned an excursion to see the glories of Teneriffe. "My whole course of life," he declared afterwards, when sending a message to Humboldt, "is due to having read and re-read, as a youth, his *Personal Narrative*." Thus his project of entering the Church was never formally abandoned, and, indeed, had not taken

such definite form as to present an insuperable obstacle when the young man of scientific interests and private means was offered the post of unpaid naturalist on the voyage of the *Beagle*. Thus, finally, when his career was definitely determined by the work done on that epoch-making voyage, the former project silently lapsed.

III

THE VOYAGE OF THE "BEAGLE"

THIS great opportunity was very nearly lost. On coming home at the end of August from his geological excursion with Sedgwick he found a letter from Henslow, who had been asked on behalf of Captain FitzRoy to recommend a young man for the post, and immediately mentioned the name of his pupil. "I have stated that I consider you to be the best qualified person I know of who is likely to undertake such a situation. I state this, not on the supposition of your being a *finished* naturalist, but as amply qualified for collecting, observing, and noting anything worthy to be noted in Natural History. .....The voyage is to last two years, and if you take plenty of books with you anything you please may be done" (I, 193). Charles Darwin was all eagerness to go; adventure called; the modest scientific duties were not above his powers; but his father strongly objected to a plan which presumably would unsettle him for his clerical career. Fortunately for his son, he added the words: "If you can find any man of common sense who advises you to go, I will give you my consent." So Darwin wrote on the 30th refusing the offer. Next day, however, he found the "sensible man" in his uncle Josiah Wedgwood, second of that name, to whose house at Maer he had gone in readiness for partridge shooting on the 1st. Josiah, whom Charles Darwin greatly revered as the very type of an upright man, with the clearest judgment, silent and reserved though he generally

was, thought it would be wise to accept the offer, as providing a valuable experience which would not necessarily be unsettling for a clerical career, and offered to drive Charles the fourteen miles to Shrewsbury and talk to Dr. Darwin. But it was simpler and more dispassionate to write—a pity also, maybe, to miss the shooting; and the letters from Charles and his uncle which the groom took to Shrewsbury are printed in the *Life* (I, 196–9). Dr. Darwin, who always maintained that Josiah was one of the most sensible men in the world, at once consented in the kindest manner, but not without a subsequent touch of humour. For when Charles, having been rather extravagant at Cambridge, said, to console his father, that he would be deuced clever to spend more than his allowance when on board the *Beagle*, he replied with a smile: "But they tell me you are very clever."

One obstacle was surmounted; another, none the less formidable because ridiculous, awaited him. He posted off to Henslow at Cambridge; then to London to interview FitzRoy, a youthful captain of six-and-twenty, whose family connections had ensured early recognition of his abilities. Long afterwards, when they had become close friends, he learnt that he had been within an ace of being rejected on account of the shape of his nose! Fitz-Roy, an ardent disciple of Lavater, was convinced that he could tell a man's character by the outline of his features; and he doubted whether any one with such a nose could possess sufficient energy and determination for the voyage. No doubt he was well satisfied afterwards that the poor nose had spoken falsely.

The voyage was indeed epoch-making. It was at once Darwin's real opportunity and his education; it turned him to a scientific career, while teaching him to use his scientific powers.

To quote from the Darwin *Obituary* :—

> A fourth educational experiment was to be tried. This time Nature took him in hand herself, and showed him the way by which, to borrow Henslow's prophetic phrase, "anything he pleased might be done."
>
> The conditions of life presented by a ship-of-war of only 242 tons burthen would not, *prima facie*, appear to be so favourable to intellectual development as those offered by the cloistered retirement of Christ's College. Darwin had not even a cabin to himself; while, in addition to the hindrances and interruptions incidental to sea-life, which can be appreciated only by those who have had experience of them, sea-sickness came on whenever the little ship was "lively"; and, considering the circumstances of the cruise, that must have been her normal state. Nevertheless, Darwin found on board the *Beagle* that which neither the pedagogues of Shrewsbury nor the professoriate of Edinburgh nor the tutors of Cambridge had managed to give him. "I have always felt that I owe to the voyage the first real training or education of my mind" (I, 61); and in a letter written as he was leaving England he calls the voyage on which he was starting, with just insight, his "second life" (I, 214). Happily for Darwin's education, the school-time of the *Beagle* lasted five years instead of two; and the countries which the ship visited were singularly well fitted to provide him with object-lessons, on the nature of things, of the greatest value.

On him, as on his two chief henchmen a few years later, the long sea-voyage produced a definite effect, with its withdrawals from the familiar distractions of life, its fresh and varied scientific interests, its opportunities for absorbing fresh impressions and drawing far-reaching conclusions from them, untrammelled by the pressure of conventional opinion. Such isolation is comparable to the withdrawal of the prophet-to-be into the wilderness

for meditation and for the crystallization of the thoughts still, as it were, in solution. On this voyage he found himself. It dawned upon him gradually that he could be something more than a collector of specimens and of facts to be used by the great men. The home appreciation of the collections he sent back to England was a genuine, if delightful, surprise. "You rank my natural history labours far too high," he had written to J. M. Herbert. "I am nothing but a lion's provider. I do not feel at all sure that they will not growl and finally destroy me."

He found, too, that he could write. The careful keeping of a journal afforded good practice in expression. Later, his captain, FitzRoy, asked him to read some of the Journal, and declared it would be worth publishing. To continue from the *Obituary*:—

> While at sea he diligently collected, studied, and made copious notes upon the surface Fauna. But with no previous training in dissection, hardly any power of drawing, and next to no knowledge of comparative anatomy, his occupation with work of this kind—notwithstanding all his zeal and industry—resulted, for the most part, in a vast accumulation of useless manuscript. Some acquaintance with the marine *Crustacea*, observations on *Planariæ* and on the ubiquitous *Sagitta*, seem to have been the chief results of a great amount of labour in this direction.
>
> It was otherwise with the terrestrial phenomena which came under the voyager's notice, and Geology very soon took her revenge for the scorn which the much-bored Edinburgh student had poured upon her.

Among the books which he took out with him was the first volume of Lyell's *Principles of Geology*, recently published, a book which Henslow, excellent conservative, had bidden him read, but by no means to accept its conclusions. The former injunction he obeyed, but not

the latter. He had some knowledge of the subject already, but in this book he found a new revelation. The doctrine of uniformity routed the catastrophists. "The very first place which I examined," writes Darwin, "namely, St. Jago, in the Cape de Verd Islands, showed me clearly the wonderful superiority of Lyell's manner of treating Geology, compared with that of any other author whose works I had with me or ever afterwards read"—a conviction only strengthened by his subsequent study of the tertiary deposits and the terraced gravel beds of South America.

It was at this same St. Jago, the first land at which the ship touched since leaving England three weeks before, that Darwin was at first stirred with a sense of his own power to do something more than mere collecting. His mind was full of geological thought, and the tokens of volcanic action and of upheaval before his eyes made a deep impression. A noble subject opened before him; he might write a book on the geology of the countries he was to visit. "And this," he tells us, "made me thrill with delight. That was a memorable hour to me, and how distinctly I can call to mind the low cliff of lava beneath which I rested, with the sun glaring hot, a few strange desert plants growing near, and with living corals in the tidal pools at my feet."

Geology, then, became his primary concern. His letters from South America refer to few scientific topics except geology; and even his theory of the formation of coral reefs was suggested by the extent of the slow changes in the South American coast level, of which he found evidence. In his own words: "No other work of mine was begun in so deductive a spirit as this; for the whole theory was thought out on the West Coast of South America, before I had seen a true coral reef. I had, therefore, only to verify and extend my views

by a careful examination of living reefs" (I, 70). His enthusiasm for the subject appears in a letter written from Lima in 1835, when he was about to start to the Galapagos Islands, in which he recommends his cousin W. D. Fox to take up geology: "There is so much larger a field for thought than in the other branches of Natural History. I am become a zealous disciple of Mr. Lyell's views, as made known in his admirable book. Geologizing in South America, I am tempted to carry parts to a greater extent even than he does. Geology is a capital science to begin with, as it requires nothing but a little reading, thinking, and hammering." Geology was still in a simple stage of development! A year later, also, Darwin speaks of being "much more inclined for geology than the other branches of Natural History" (I, 275).

What was this "carrying geology further than Lyell himself"? He proceeds: "I have a considerable body of notes together; but it is a constant subject of perplexity to me whether they are of sufficient value for all the time I have spent about them, or whether animals would not have been of more certain value." One may reasonably suspect a reference to the fossils of the Pampas and their relation to existing species, which, as will be seen later, raised the first questionings about the mutability of species.

Thus the energetic young man who, his father had feared, was likely to degenerate into an idle, sporting man, gradually discovered that the pleasure of observing and reasoning was a much higher one than that of skill and sport. Though for the first two years of the voyage his old love of shooting survived in almost full force, and he himself shot almost all the birds and animals needed for his collection, he gradually gave up his gun to his servant, as shooting interfered with his other work,

especially with making out the geological structure of a country. Step by step the love of science began to preponderate over every other taste.

It is curious to note how much he was just the gifted amateur, how little the professionally-trained man of science. He speaks in his letters of his ignorance of botanical detail in relation to his collection of plants; of his loss of time and trouble over the dissection and description of marine animals on the voyage, from his inability to draw, and his insufficiency of anatomical knowledge. Indeed, he was naturally hampered by a certain clumsiness of finger, and at Edinburgh had not struggled with the difficulties of dissection. This was, in part, remedied later by the careful systematic work upon the Cirripedes, spread over eight years—a wonderful piece at once of research and of self-discipline, arising out of his discovery in Chile of a curious new form, to understand the structure of which he had to examine and dissect many of the common forms, while this in turn led him on to write a monograph on the whole group, living and extinct. One recalls, also, his enthusiasm, when an elderly man, over his great follower's *Elementary Lessons in Physiology*, and his lament that he had not been brought up on some such book. Even in geology—the subject of which he had, perhaps, best knowledge, and which he continued to study enthusiastically in Lyell's *Principles* on board the *Beagle*—we find him writing to Henslow for information about various fundamental points, especially in connection with field work. In the event, his very notable studies in geology, especially in South America, though not of the same epoch-making kind as his biological researches, gave a powerful impulse to the general acceptance of Lyell's teaching by the way in which he gathered facts in its support from every part of the world.

IV

THE "BEAGLE" WORK

THE voyage lasted five years, and ten more years passed before the results were all arranged and published. The famous journal, which was to become a classic among books of travel, was at first smothered by appearing in 1839 as the third volume of King and Fitz-Roy's official narrative of the surveying voyages of H.M.S. *Adventure* and *Beagle* between 1826 and 1836, although the *Quarterly Review* did pick it out for the "charm arising from the freshness of heart which is thrown over these virgin pages of a strong, intellectual man and an acute and deep observer." It only emerged into popularity when published separately in 1845.

As Naturalist to the expedition, it was also Darwin's business to edit and superintend the Zoological Reports of the Voyage, five in number, which were worked up by specialists and published between 1840 and 1843.

The Geological Report was his own work, and consisted of three volumes: *Coral Reefs* (1842), *Geological Observations on Volcanic Islands* (1844), and *Geological Observations on South America* (1846).

Darwin's theory of the formation of coral reefs was suggested by the evidence he had observed of great subsidences on the coasts of South America. While he also contemplated the formation of reefs upon shoals rising by accumulation of marine deposits, he conceived the effective cause of oceanic coral islands to be the sub-

sidence of a central eminence and the gradual conversion of a barrier reef into a fringing reef, and the fringing reef into an atoll or ring reef with no central eminence left above water, but with steep walls of coral descending precipitously into the depths, their upper part still living and growing to the surface, the lower part dying as it is drawn down by continued subsidence below the level at which coral can live.

The theory, accounting so well for the phenomena displayed in the different types of coral reef and for the existence of cliffs extending far below the range of living coral, was accepted by the leading geologists of the day. But the alternative process, as originally put forward by Chamisso and later expanded by the researches of Murray and others, has been shown to have a wider range than Darwin contemplated. This theory rejects subsidence as the condition under which coral islands are formed; it sees the underwater highlands and peaks raised higher by an accumulating cap of organic débris, and so brought near enough to the surface for the corals to build upon them. It sees the fragments torn off by the breakers forming a talus below the coral wall, and the outer face of the reef growing vigorously in the wash of the open sea, while a lagoon is eaten out by the chemical action of sea water dissolving away the dead coral on the still inner side of the reef. At present it seems that neither theory covers the whole field.

The tenth year, then, saw the direct scientific results of the voyage worked out. A further work that grew out of the voyage, though not shaped from materials then collected, was the monograph, already mentioned, on the Cirripedes, begun in 1846 and completed in four volumes published in 1851–54.

Eight years of plodding, laborious work among minute details, the anatomy and classification of a single group!

Was this the right occupation for a great thinker who, as we shall see, had long been shaping a vast biological theory? True, he had accused himself of presumption in attacking the species question without having worked out his due share of species. There were moments in later life when Darwin himself doubted whether the work was worth the amount of time spent upon it, though acknowledging that it was of considerable use to him when, in the *Origin*, he had to discuss the principles of a natural classification and what naturalists meant by a species. And research impressed on him practically "the variability of every part, in some slight degree, of every species" (II, 37). But at the time, Sir Joseph Hooker tells us, he certainly appreciated its value to him as systematic training, for he "recognized three stages in his career as a biologist—the mere collector at Cambridge, the collector and observer in the *Beagle* and for some years afterwards, and the trained naturalist after, and only after, the Cirripedes work. That he was a thinker all along is true enough, and there is a vast deal in his writings previous to the Cirripedes that a trained naturalist could but emulate.....He often alluded to it as a valued discipline, and added that even the 'hateful' work of digging out synonyms, and of describing, not only improved his methods, but opened his eyes to the difficulties and merits of the works of the dullest of cataloguers. One result was that he would never allow a depreciatory remark to pass unchallenged on the poorest class of scientific workers, provided that their work was honest and good of its kind. I have always regarded it as one of the finest traits of his character—this generous appreciation of the hod-men of science and of their labours.....and it was monographing the Barnacles that brought it about" (*Life of Charles Darwin*, I, 346–7).

From the same source I quote Professor Huxley's letter on the subject to Sir F. Darwin:—

> In my opinion, your sagacious father never did a wiser thing than when he devoted himself to the years of patient toil which the Cirripede book cost him.
>
> Like the rest of us, he had no proper training in biological science, and it has always struck me as a remarkable instance of his scientific insight, that he saw the necessity of giving himself such training, and of his courage, that he did not shirk the labour of obtaining it.
>
> The great danger which besets all men of large speculative faculty is the temptation to deal with the accepted statements of fact in natural science as if they were not only correct, but exhaustive; as if they might be dealt with deductively, in the same way as propositions in Euclid may be dealt with. In reality, every such statement, however true it may be, is true only relatively to the means of observation and the point of view of those who have enunciated it. So far it may be depended upon. But whether it will bear every speculative conclusion that may be logically deduced from it is quite another question.
>
> Your father was building a vast superstructure upon the foundations furnished by the recognized facts of geological and biological science. In Physical Geography, in Geology proper, in Geographical Distribution, and in Palæontology, he had acquired an extensive practical training during the voyage of the *Beagle*. He knew of his own knowledge the way in which the raw materials of these branches of science are acquired, and was therefore a most competent judge of the speculative strain they would bear. That which he needed, after his return to England, was a corresponding acquaintance with Anatomy and Development and their relation to Taxonomy, and he acquired this by his Cirripede work.

Thus, in my apprehension, the value of the Cirripede monograph lies not only in the fact that it is a very admirable piece of work and constituted a great addition to positive knowledge, but still more in the circumstance that it was a piece of critical self-discipline, the effect of which manifested itself in everything your father wrote afterwards, and saved him from endless errors of detail.

So far from such work being a loss of time, I believe it would have been well worth his while, had it been practicable, to have supplemented it by a special study of embryology and physiology. His hands would have been greatly strengthened thereby when he came to write out sundry chapters of the *Origin of Species*. But, of course, in those days it was almost impossible for him to find facilities for such work.

V

THE OPENING OF THE SPECIES QUESTION

So much, then, for the actual material collected on the voyage and the subsequent self-training by Darwin. Let us return to the voyage itself as the well-spring of Darwin's main work, and note the salient points in it which specially tended to fix his line of thought.

The remote oceanic islands visited were standing question-marks to all theories of creation and distribution; the proof of recent vast continental upheavals and subsidences afforded by the geology of the Andes illustrated the variety of conditions affecting development as well as distribution: with his own eyes he had seen man in natural savagery amid Fuegian desolation.

He set out on the voyage a believer in the current doctrine of the fixity of species. In that belief, generally speaking, he wrote his Journal, though as time goes on questionings creep in. Doubtless the first questionings sprang from the influence of Lyell's *First Principles of Geology*. In the autumn of 1832, nearly a year after leaving England, he was collecting fossils on the Pampean plains when the second volume of Lyell, which most concerns biological questions, reached him. He became aware that mutability of species is the logical outcome of Lyell's doctrines, though this was not acknowledged by Lyell himself. "If the extermination of a species is no more catastrophic than the natural death of an individual, why should the birth of a species be any more miraculous than the birth of an individual?" So the current of

his thought began to set in the direction of Descent, stimulated by the deep impression made upon him "by discovering in the Pampean formation great fossil animals covered with armour like that on the existing armadillos, and by the manner in which closely allied animals replace one another in proceeding southwards over the Continent."

As he put it in the first note-book on the species question which he opened in 1837, propagation explains why modern animals are regularly of the same type as extinct animals.

The evidence from fossils which strongly impressed him from 1832 no doubt made a much fuller impression when these were arranged and described after his return; but the third point which he records in his Autobiography as having set him on the evolutionary track was of more immediate effect. This was "the South American character of most of the productions of the Galapagos archipelago, and more especially the manner in which they differ slightly on each island of the group; none of the islands appearing to be very ancient in a geological sense." There was less need to wait for the final examination of the specimens at home; he tells in the Journal how his attention was "thoroughly aroused" by comparing the birds shot by himself and others on board. The Galapagos thrust themselves upon him as a world of evolution in miniature.

From this time on the whole subject "haunted him." When he returned to England late in 1836, and began to prepare his Journal for press, he saw how many facts indicated the common descent of species; and in March, 1837, he records with some particularity, four months before he sat down to grapple seriously with the subject, how the South American and Galapagos evidence came home to him with especial force. He was so far

shaken that in the printed Journal (which was not actually published till 1839, a couple of years later, as part of the official account of the voyage) he omitted a passage of the original MS. which takes special creation for granted, though elsewhere, speaking generally, he gives his observations without insisting on their evolutionary bearing. He had not yet secured a firm enough base for his haunting thought. But in the second edition of 1845, when his theory had passed beyond its first hesitating and tentative stage into the clear outline of the 1844 sketch, and he felt more confident, he dotted the "i's" and crossed the "t's" in several passages, noting where modification must have occurred, introducing the question of the fossil predecessors of existing forms and "a strong statement of the intensity of the struggle for life," and giving much wider generalizations on geographical distribution.

Much had happened in the interval. Geology yielded to the more absorbing interest in the biological problem it had raised. By September, 1838, when his routine business was getting ready the scientific reports of the voyage of the *Beagle*, we find him half apologizing to Lyell for "idling"; that is to say, working strenuously at a more engrossing subject. This "idling" had been going on for over a year, ever since the Journal was off his hands. In his own words: "In July [1837] opened first note-book on Transmutation of Species. Had been greatly struck from about the month of previous March on character of South American fossils and species on Galapagos Archipelago. These facts (especially latter) origin of all my views" (I, 276). The problem which was to be his chief concern for the rest of his life first came to him as a problem mainly of distribution. "Why do species present certain relations in space and in time? Why are the animals and plants of the Galapagos

Archipelago so like those of South America and yet different from them? Why are those of the several islets more or less different from one another? Why are the animals of the latest geological epoch in South America similar in *facies* to those which exist in the same region at the present day, and yet specifically or generically different?"

The orthodox view of special creation was neither proven nor capable of giving a rational explanation of the existing distribution of species. Spontaneous generation —creation without a creator—was equally unsupported by any evidence. For those who rejected both hypotheses remained the obvious alternative—creation of species by modification of previous species. From the time of the ancient Greeks down to Darwin's own day his idea had been repeatedly propounded. Within recent times the study of life had brought a number of thinkers to accept evolution in some form. Buffon believed in it; Erasmus Darwin had speculated on it with much imagination but little fact; Lamarck had propounded his speciously attractive idea of modification by effort, impossible among plants, inadequate among animals, and as implying necessary progression, accounting neither for retrogression nor for persistence without change.

But though the evolutionists had laid hold of a far-reaching and indeed probable idea, they could not show any effective cause at work to bring it about. They offered no practical clue to their successors. Their evolution remained an empty speculation, instead of a fruitful hypothesis.

Thus Darwin had no help from his predecessors; even Lyell, the most advanced of his contemporaries, who held that natural causes are sufficient to produce the phenomena of the inorganic world, in regard to life was

a creationist until converted, a score of years later, by the *Origin* itself. The discoverer had to build anew. It was evident that the many facts he had observed on the voyage could only be explained on the supposition that species had been gradually modified. But it was equally evident that neither the action of the environment nor the will of the organisms, especially in the case of plants, could account for the innumerable and beautiful adaptations of organisms to their habits of life. Until these could be explained it seemed to him almost useless to endeavour to prove by indirect evidence that species have been modified.

VI

THE KEY OF THE PROBLEM AND THE FIRST SKETCHES OF THE THEORY

WHERE, then, was direct evidence to be sought? With wise instinct Darwin turned to the work of the breeders and horticulturists. Here the most diverse new varieties had been produced from the old, and it should be possible to argue from the known to the unknown, and find a clue to the maze of accumulated facts. Accordingly, he opened his note-books in the expectation "that by following the example of Lyell in geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject.....I worked on true Baconian principles, and without any theory collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed inquiries, by conversation with skilful breeders and gardeners, and by extensive reading. When I see the list of books of all kinds which I read and abstracted, including whole series of Journals and Transactions, I am surprised at my industry." His sagacity was rewarded. One half of the secret was speedily revealed.

"I soon perceived that selection was the keystone of man's success in making useful races of animals and plants. But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me."

In this first note-book Sir Francis Darwin tells us there is already a forecast of the importance of the

survival of the fittest, and Charles Darwin would no doubt have arrived in due course at a formulation of the principle, when, in October, 1838, fifteen months after beginning the systematic inquiry, his thought on the point was suddenly crystallized by his happening "to read for amusement Malthus *On Population*."

"Being well prepared," he writes, "to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of plants and animals, it at once struck me that, under these circumstances, favourable variations would tend to be preserved and unfavourable 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."

This was to be the master-key, which gave not only a probable theory of the origin of living species, but also the long-wanted explanation of the phenomena of adaptation. The process of natural selection depends on adaptation; the best adapted are the best fitted to survive. And it followed "that even the most complicated adaptations might result from the summation of a long series of simple favourable variations." Neither Erasmus Darwin nor Lamarck had discerned any such *vera causa* at work.

Meantime Darwin had already wrought out various subsidiary keys to parts of the problem. A month before he read Malthus he wrote to Lyell, in a letter discussing his geological work: "I have lately been sadly tempted to be idle—that is, as far as pure geology is concerned—by 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. Note-book after note-book has been filled with facts which begin to group themselves *clearly* under sub-laws" (I, 298).

By the beginning of 1839, then, twenty years before the publication of the *Origin*, he had fairly shaped out the framework of his great theory. But even then he would not be precipitate; he would not surrender himself to his theory till he had tested it for another three and a-half years and weighed it against ever more facts. As he recorded:—

> I had at last got a theory by which to work; but I was so anxious to avoid prejudice that I determined not for some time to write even the briefest sketch of it. In June 1842 I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil in thirty-five pages, and this was enlarged during the summer of 1844 into one of 230 pages, which I had fairly copied out and still possess.

Comparing the two sketches with the *Origin of Species* itself, Sir Francis Darwin notes it as very remarkable that in this pencil sketch of 1842, seventeen years before the finished book, he should have been able to write out so complete a sketch of his future work, and yet more remarkable that he should have written the longer 1844 sketch from memory alone—an achievement "which perhaps renders more conceivable the still greater feat of the writing of the *Origin* between July 1858 and September 1859."

Noticeable also, he tells us, is a certain freshness in the 1844 sketch which gives it a character of its own, though it was never finally polished up for publication. It has "an air of freedom, as if the author were letting himself go, rather than applying the curb," for it was an expansion of the previous sketch. The *Origin*, as a condensation of a much bigger book, gained in force and conciseness, but occasionally reveals a chafing against the author's self-imposed limitation.

While the main arguments and the illustrations em-

ployed are the same in sketch and book, in the former "much more weight is attached to the influence of external conditions in producing variation, and to the inheritance of acquired habits than in the *Origin*."

Looking back on the 1844 sketch, Darwin found its most serious flaw to be the "almost incomprehensible" overlooking of "one problem of great importance"—namely, the divergence in character of the descendants of a common ancestor, which enables them to seize on many and diversified places in the polity of nature, and so to increase in numbers. To the end of his life he could recall the spot on the road near Down where there had flashed across his mind the essential need of its clear enunciation. Nevertheless, there are passages in both sketches which approach this point of view. And a set discussion on the point was the less indispensable because "descent with modification implies divergence, and we become so habituated to a belief in descent, and therefore in divergence, that we do not notice the absence of proof that divergence is in itself an advantage" (*Life*, II, 15–16).

Still, the principle of divergence was of great importance and demanded clear statement, and this explains why, when Darwin's hand was forced in 1858, he sent to the Linnean Society along with Wallace's paper not only the Natural Selection chapter of the 1844 essay, but also a letter written to Asa Gray, the only appropriate document which included a discussion on the subject.

The collection and sifting of his material involved unexpected difficulties, and sometimes led Darwin into curious company. He was dealing with species; but species, unlike mathematical quantities, have no fixed proportions. Some naturalists frankly regarded them as conventional groups; it was once said, with cynical humour, that a species was anything to which a specific name could be given. Even those who most firmly

believed in their fixed reality had no agreement as to their boundaries. Species being most simply described as groups defined by some constant morphological characteristics not dependent on sexual peculiarities, a naturalist with an eye for differences would make, say, twenty species of brambles where another, with an eye for likenesses, would make three or four with several varieties. Nature provides no regular scale of structural differentiation—so much marking off a species, so much only a variety. And in a genus some species appear separated by large, others by small, differences.

Thus, in the absence of a natural scale, another practical criterion was currently accepted. Breeds and species differ not only in structure, but also in physiological characters, although physiological tests for species could hardly ever be applied by practical naturalists. Chief among such physiological differences stood the current generalization that varieties breed with one another, while species do not. But examination of the evidence as to hybridism, especially in experiments with plants, showed many curious exceptions to this generalization. Subsequent research, stimulated by Darwin's work, has considerably modified former ideas as to the amount and the causes of sterility when allied species are crossed; still, it remained that such sterility, however acquired, was a true quality of a separate species, lacking which it would become merged with the rest and lose its separate identity. Acceptance of this axiom, as we shall see, affected the logical completeness of Darwin's theory, which could be regarded as theoretically perfect only when experiment should produce varieties sterile with one another, and therefore by definition species. So far, breeding can produce structural divergences as great as those of species, but has not produced equal physiological divergences.

If these and similar difficulties were to be met in the field of botany, where the records with regard to plants had been put in orderly form by professed botanists, with regard to animals he found by bitter experience that no résumé was to be trusted; facts must be collected from all original sources. As he wrote to T. H. Huxley:—

> The inaccuracy of the blessed gang (of which I am one) of compilers passes all bounds. *Monsters* have frequently been described as hybrids without a tittle of evidence. I must give one other case to show how we jolly fellows work. A Belgian baron (I forget his name at this moment) crossed two distinct geese and got *seven* hybrids, which he proved subsequently to be quite sterile; well, compiler the first (Chevreul) says that the hybrids were propagated for *seven* generations *inter se*. Compiler second (Morton) mistakes the French name, and gives Latin names for two more distinct geese, and says *Chevreul* himself propagated them *inter se* for seven generations; and the latter statement is copied from book to book.

As to the facts about breeding: he picked up most by reading really numberless special treatises and *all* agricultural and horticultural journals—a work of long years. "The difficulty is to know what to trust. No one or two statements are worth a farthing; the facts are so complicated."

To get first-hand knowledge, not only did he breed fancy pigeons himself at Down, but, joining the Columbarian and Philoperistera Clubs, gained unique experience from the practised devotees of the art. "A noble and majestic pursuit," he assures his schoolboy son at Rugby, "and beats moths and butterflies, whatever you may say to the contrary." Thus he writes in 1859:—

> For instance, I sat one night in a gin palace in the Borough among a set of pigeon fanciers, when it

> was hinted that Mr. Bull had crossed his Pouters with Runts to gain size; and if you had seen the solemn, the mysterious, and awful shakes of the head which all the fanciers gave at this scandalous proceeding, you would have recognized how little crossing has had to do with improving breeds, and how dangerous for endless generations the process was. All this was brought home far more vividly than by pages of mere statements.

To illustrate the "extreme attention and close observation" indispensable to a good fancier, he loved to quote from a treatise by one of them "devoted to the Almond Tumbler alone, which is a sub-variety of the short-faced variety, which is a variety of the Tumbler, as that is of the Rock-pigeon," involving the pursuit of an ideal so delicate and so complex that it seemed "almost beyond the human intellect to attend to *all* the excellences of the Almond Tumbler!"

His favourite passage ran as follows:—

> If it was possible for noblemen and gentlemen to know the amazing amount of solace and pleasure derived from the Almond Tumbler when they begin to understand their (i.e., the Tumblers') properties, I should think that scarce any nobleman or gentleman would be without their aviaries of Almond Tumblers.

"My father," writes Sir F. Darwin, "was fond of quoting this passage, and always with a tone of fellow-feeling for the author, though, no doubt, he had forgotten his own wonderings as a child that 'every gentleman did not become an ornithologist.'"

There remained before him a dozen years of laborious work, toiling over the Cirripedes, making experiments in breeding pigeons, amassing material, working out subsidiary problems, noting difficulties and seeking explanations of them, before he began to write the actual book which was to embody his theory. Meanwhile, in a life

of unceasing ill-health, the ten or twelve years after 1840 perhaps involved the worst suffering. In the belief that he might not live to complete his work, he drew up a letter of instructions to his wife soon after he had finished the 1844 essay, setting aside a sum of money for the publication of the work, after revision and, if possible, completion from the unused material, by some competent editor—Lyell, or Lyell and Hooker together for choice, or, as amended many years later, when the latter had become chief confidant in his work, Hooker alone.

The book he began at Lyell's instigation in 1856, writing it on a scale three or four times as extensive as that afterwards allowed for the *Origin*.

VII

FRIENDSHIP WITH HOOKER

THE history of the *Origin* is not only a history of science; it is the history of a great friendship. Darwin, among his rich gifts of mind and heart, had the gift of making and keeping friends. Their knowledge as well as their goodwill was always at his disposal. In the forefront of his scientific friends stood Lyell, the teacher whose disciple he acknowledged himself to be—the expert counsellor in whom he confided. But the closest intimacy, the fullest confidence, came to be given to Hooker, the great botanist and son of a botanist. Hooker, his junior by eight years, had first met him in 1839, when he, in his turn, was about to set forth on a four years' voyage of scientific discovery. During the voyage they were in touch through the Lyells, and immediately after his friend's return from lands which they had both visited, in December, 1843, Darwin began their long correspondence with inquiries as to the distribution and connections of the Antarctic flora, offering his own collections of plants from Patagonia, Fuegia, and the Galapagos, for his examination.

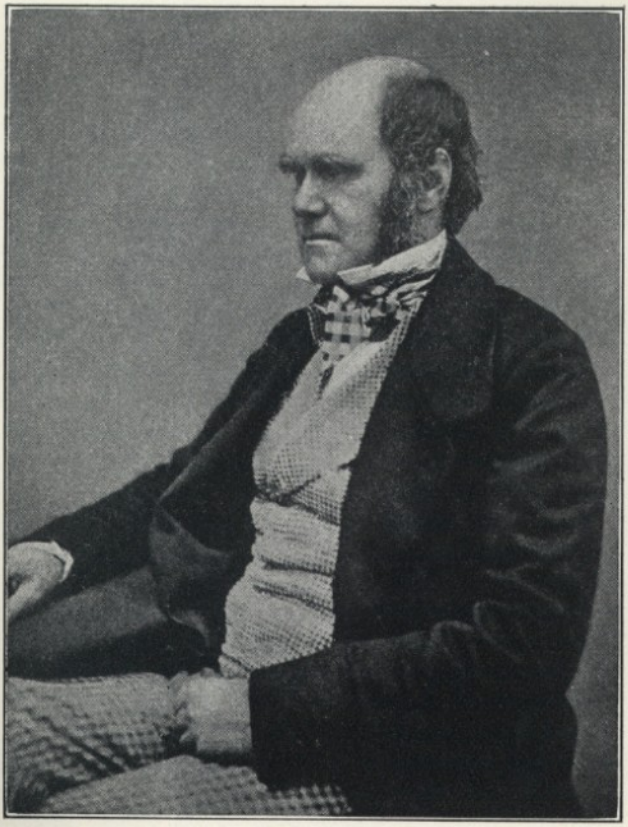
Acquaintance swiftly ripened into friendship. Community of tastes soon made Darwin feel as if he had known his friend "for fifty years." Pre-eminent in botany, Hooker could easily have won eminence in several other branches of science; and he had a mind which ranged beyond scientific details to larger principles, while preserving a cautious sanity of judgment. Darwin recog-

nized his quality from the first. "After having read a few of your letters, I never once doubted the position you will ultimately hold among European botanists..... It is absurdly unjust to speak of you as a mere systematist." And, in the wider range of research, Hooker shared his belief that "Geographical Distribution will be the key which will unlock the mystery of the species."

But the true distribution of plants could not be ascertained without accurate floras. The existing works were incomplete and uncorrelated.

Hooker was sustained by his shared enthusiasm in the laborious task of making complete and accurate classifications according to nature as an essential step towards finding the key to it all. Each had the generous conviction that he had received from his friend far more than he had given. For fifteen years Hooker, in the intimacy of friendship, gave sympathy and encouragement, responded to every call upon his vast and carefully digested store of botanical knowledge, gave frank and judicious criticism. Darwin stimulated his friend to redoubled energy in his researches, which, without any sacrifice of originality, led him independently to similar views of evolution among plants—views published immediately before the *Origin*.

Their constant correspondence was supplemented by visits to Darwin's home at Down, when Hooker would come, it may be, for several days, bringing his own work with him, and for the allotted half-hour each morning —all that Darwin's health allowed—discussing the questions of the moment which, to save time, Darwin had noted down ready. Of these discussions Hooker records (*Life of Charles Darwin*, II, 27): "I, at any rate, always left with the feeling that I had imparted nothing and carried away more than I could stagger



CHARLES DARWIN (ABOUT 1854)

Facing p. 42

under." Darwin, on the other hand, after one of these visits, writes: "For my part, I learn more in these discussions than in ten times over the number of hours reading." And, after reading Hooker's *Antarctic Flora*, he speaks of having "extracted more facts and views from you than from any other person."

Nor was such help confined to the spoken or written word, or loans of rare books. Hooker carried out many botanical experiments for him at Kew. On one occasion, in sceptical mood as to the power of seeds to survive long immersion in sea-water, on which Darwin had been experimenting with apparent success, he plants some seeds which have been carried by the Gulf Stream from America to Norway. They germinate perfectly, and his pride has an "unutterable mortification," over which Darwin chaffs him genially, with the consolation that the result really favours his own botanical views.

If Hooker, with all his belief in the variability and migration of species, was slow to accept Darwin's full theory, it was partly because actual transition had not been observed, and he had failed to find transitional forms in the Himalayas, where arctic and tropical floras meet, partly from the caution of a judicial mind which calls for proof entire and incontrovertible. But his criticisms led not to destruction, but to reconstruction. "You never make an objection without doing much good," says Darwin, and "fighting a battle with you clears my mind wonderfully," with the result that, "although we are very apt, I have observed, at the first approach of a subject, to take different views, we generally come to a near approach after a talk." Accustomed as he was to being quizzed by his non-naturalist relations and meeting opposition and even contempt, he marked out Hooker as the one living soul from whom he had constantly received sympathy.

And, finally, when in 1863 Hooker disclaimed the large credit for his services to the Darwinian theory publicly ascribed to him by Lyell in his *Antiquity of Man*, Darwin replied:—

> What a candid, honest fellow you are; too candid and too honest. I do not believe one man in ten thousand would have thought and said what you say about your own work in your letter. I told Lyell that nothing pleased me more in his work than the conspicuous position in which he very properly placed you.

VIII

PUBLICATION OF THE "ORIGIN"

FOR two years, then, from May, 1856, Darwin was hard at work on his great book, and completed ten chapters of it. Species were not primordial and immutable existences, but phases of existence. They were not independently created, but were only strongly-defined varieties, surviving from among their ever-varying brethren through being better adapted to the surrounding conditions, and isolated by the extinction of their less-adapted fellows. As among domestic animals careful selection, point by point, had produced such diverse breeds as the bulldog and the greyhound, the drayhorse and the racer, so in the state of nature automatic selection took place on a vaster scale and left these isolated groups we call species, which remain permanent as long as their determining conditions remain unchanged.

But while Darwin toiled to complete his work on a vast and convincing scale, there happened what Lyell had often warned him was likely to happen. Another man working in a different field enunciated the identical theory of Natural Selection as the definer of species.

Darwin had already corresponded with Alfred Russel Wallace in the Malay Archipelago. He had written in 1857 noting the similarity in their lines of thought and some of their conclusions, and telling also of his twenty years' work on the subject now being shaped into a book. Stirred by such sympathy, Wallace sent him in 1858 a paper, "On the Tendency of Varieties

to Depart Indefinitely from the Original Type," asking him to get it published. Basing himself on the natural history of the Malay Archipelago, with features like that of the Galapagos Islands, which had so deeply impressed Darwin twenty-eight years before, Wallace arrived at exactly the same idea of Natural Selection depending on the struggle for existence. "I never saw a more striking coincidence," exclaims Darwin to Lyell; "if Wallace had my MS. sketch written out in 1842, he could not have made a better short abstract!"

That eighteenth of June Darwin seemed to have met his Waterloo. He had no hesitation about having his unknown correspondent's paper published with no comment of his own, though well aware that, with his own book unpublished, he would be robbed of all claim to originality, even though the book itself, if it should have any value, would not be deteriorated, since all the labour of such a work consists in the application of the theory.

However, although his feeling was that he would rather burn the whole book than that Wallace or any other man should think that he had behaved in a paltry spirit, Lyell and Hooker, who had so long been his confidants, instantly reassured him on the point of honour, and insisted on a more equitable course. Wallace's paper, extracts from the sketch of 1844, and part of the letter to Asa Gray of September 5, 1857, under the joint names of Darwin and Wallace, were communicated to the Linnean Society on July 1, 1858, by Lyell and Hooker, whose prefatory note made it clear that they did so "not solely considering the relative claims to priority of himself and his friend, but the interests of science generally."

When the paper was read under countenance of a few words from the chief geologist and the chief botanist of the day, who alone were well acquainted with the subject, there was no semblance of a discussion. "The interest

excited was intense, but the subject was too novel and too ominous for the old school to enter the lists before armouring."

Wallace was as generous-minded as Darwin. As soon as he realized the vast work already done by Darwin to establish the theory on an incomparably broader basis than the observations which had suggested the same theory to himself, he waived all claim to priority. As soon as he read the *Origin* he frankly declared it was a book such as he himself could never have written. In a world where jealousy and self-seeking are too common, the magnanimity of these men, who laid aside rivalry and became allies, offers an inspiring spectacle.

Nevertheless, while Darwin rehabilitated the ancient doctrine of evolution and secured its foundations among half-a-dozen branches of science, it is curious to note that neither he nor Wallace could claim absolute priority in regard to the doctrine of Natural Selection. The success of the *Origin* brought into the field Patrick Matthew, who pointed out that he had enunciated the same principle in the unhappy obscurity of an appendix to a book on Naval Timber. Thereafter he printed on his title-page "Discoverer of the Principle of Natural Selection." However, his claims in turn were extinguished when it was found that Dr. Wells, in his Essay on Dew, read before the Royal Society in 1813, but not printed, most distinctly applied the principle of natural selection to the Races of Man.

Forced into earlier publication by Wallace's intervention, Darwin set about preparing what he first called an Abstract from the half-written book and his accumulated notes. His first intention was to issue this as a series of papers before the Linnean Society; but the work grew too long for this, and it was submitted, with a recommendation from Lyell, to John Murray for pub-

lication in book form. It is interesting to note that just as Darwin found his ideas more easily assimilated as a rule by intelligent people who were not professed naturalists—since the latter, steeped in their own working terminology, had "a bigoted idea of the term 'species'"—so Mr. Murray was led by his knowledge of the world to have the MS. "read" not by a naturalist, but by a lawyer friend of wide culture and sane judgment—Master George Pollock, the King's Remembrancer.

"Murray was extremely sceptical as to the soundness of the work, and thought 500 copies as large a number as it was prudent to print. He remarked that the Darwinian theory was as absurd as though one should contemplate a fruitful union between a poker and a rabbit. George Pollock read the book, and remarked that the contents were probably beyond the comprehension of any scientific man then living. But he advised publishing 1,000 copies, because Mr. Darwin had so brilliantly surmounted the formidable obstacles which he was honest enough to put in his own path. This is an interesting example of the way in which a man of good general ability, accustomed as a lawyer to apply broad principles of reason to different kinds of subject-matter, may arrive at sounder conclusions than a specialist."[1]

It is curious to add that years afterwards, apropos of the Variation book being "read" for John Murray by a literary friend, Darwin remarked (III, 60): "I think if you had sent the *Origin* to an unscientific man, he would have utterly condemned it.....I must add that my *Journal of Researches* was seen in MS. by an eminent semi-scientific man, and was pronounced unfit for publication."

[1] *Master George Pollock*, by E. S. P. Haynes, in the *Cornhill Magazine* for August, 1916.

Accordingly, the book was published on November 24, 1859.

When Darwin first put pen to paper for this volume, he fixed in his mind three judges on whose decision he determined mentally to abide. These three were Lyell, Hooker, and Huxley. Lyell, who long hung back, loth to accept the inevitable extension of the theory to the descent of man, could no longer stand out; Hooker, very much of an evolutionist already, found the full argument in the book infinitely more convincing than all his discussions before; and, on receiving a little later Huxley's powerful and discriminating letter of support, Darwin wrote: "I am now contented, and can sing my *nunc dimittis*."

The book caused an instant stir. The first edition of 1,250 copies was sold to the trade the first day; the second edition of 3,000 copies, which Darwin had to prepare as best he might while undergoing a "cure," appeared on January 7. The success of the book exceeded his wildest hopes; and by success he did not mean the sale, but the impression it made on the three judges whom he had chosen in advance.

For years Darwin had been known as a man of science who might do anything, if only he had health. Expectation, aroused by the Darwin-Wallace paper and a reference in Lyell's speech as President of the Geological Section at the 1859 meeting of the British Association, was far surpassed by the event.

Opponents, basing themselves partly on scientific difficulties, but mostly on theological grounds, were ready to attack. Huxley's prophecy to Darwin was as abundantly fulfilled as his promise to aid:—

> I trust you will not allow yourself to be in any way disgusted or annoyed by the considerable abuse and misrepresentation which, unless I greatly mis-

> take, is in store for you. Depend upon it, you have earned the lasting gratitude of all thoughtful men. And as to the curs which will bark and yelp, you must recollect that some of your friends, at any rate, are endowed with an amount of combativeness which (though you have often and justly rebuked it) may stand you in good stead. I am sharpening up my claws and beak in readiness.

It is not necessary to repeat here the details of the battle over the *Origin of Species;* the valiant support so willingly rendered by his friends; the famous duellos of debate which focussed popular attention. They can be read in full elsewhere.[1] Suffice it to quote from the chapter "On the Reception of the *Origin of Species*," which his foremost champion wrote for the *Life of Charles Darwin*:—

> It was badly received by the generation to which it was first addressed, and the outpouring of angry nonsense to which it gave rise is sad to think upon. But the present generation will probably behave just as badly if another Darwin should arise and inflict upon them that which the generality of mankind most hate—the necessity of revising their convictions. Let them, then, be charitable to us ancients; and if they behave no better than the men of my day to some new benefactor, let them recollect that, after all, our wrath did not come to much, and vented itself chiefly in the bad language of sanctimonious scolds. Let them as speedily perform a strategic right-about-face, and follow the truth wherever it leads. The opponents of the new truth will discover, as those of Darwin are doing, that, after all, theories do not alter facts, and that the universe remains unaffected even though texts crumble. Or, it may be, that, as history repeats itself, their happy

[1] See *Life and Letters of Charles Darwin*, Vol. II, chaps. vi and vii, especially p. 320; *Life of T. H. Huxley*, I, chaps. xiii, xiv, xv; *Life of Sir J. D. Hooker*, I, 510–27.

> ingenuity will also discover that the new wine is of exactly the same vintage as the old, and that (rightly viewed) the old bottles prove to have been expressly made for holding it.

To those who were pledged to think otherwise, the book was a challenge. It was first denounced; then, as denunciation made no difference to the facts adduced, it was gradually assimilated. To those who were not so pledged it provided a working clue to the processes of nature; it brought evidence from one field of science after another, from the fossilized tokens of geology to the living picture of telescoped evolution revealed in embryology; it threw light on countless problems of life, and gained additional strength from each new solution. The argument had gained by the twenty years' delay in its enunciation. The *Origin* was the fourth shape into which that argument had been cast by alternate expansion and condensation; the abstract of a much larger work based upon the preceding sketches. "Cramful of matter and reasoning," it was, Hooker declared, the very hardest book to read to full profit that he ever tried. Unprefaced by this, he adds, the bigger work "would have choked any naturalist of the nineteenth century, and certainly have softened my brain in the operation of assimilating its contents." And the *Origin* was aided by the characteristic quality of the man which came out in his writings. Unlike many enthusiastic speculators who ride a theory to death, airily overleaping the hedges and ditches of inconvenient fact, Darwin's patience and love of truth, rather than eagerness to push a pet theory at all hazards, led him to follow what he called a golden rule—

> namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results to make a memo-

> randum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favourable ones. Owing to this habit, very few objections were raised against my views which I had not at least noticed and attempted to answer.

This was only one side of a magnanimity and greatness of character in him which knew not the meaning of the word "assumption." In debate, as in research, the merely personal was always subordinated to the impersonal verities of nature and fact. One of his closest friends could write thus of him:—

> I have often remarked that I never knew any one of his intellectual rank who showed himself so tolerant to opponents, great and small, as Darwin did. Sensitive he was in the sense of being too ready to be depressed by adverse comment, but I never knew any one less easily hurt by fair criticism, or who less needed to be soothed by those who opposed him with good reason.
>
> I am sure I tried his patience often enough, without ever eliciting more than a "Well, there's a good deal in what you say, but—" and then followed something which nine times out of ten showed he had gone deeper into the business than I had.

IX

THE ARGUMENT OF THE "ORIGIN"

THE substance of Darwin's theory is summed up as follows in his *Obituary* by Professor Huxley (*Coll. Ess.*, II, p. 287) :—

> Observation proves the existence among all living beings of phenomena of three kinds, denoted by the terms heredity, variation, and multiplication. Progeny tend to resemble their parents; nevertheless, all their organs and functions are susceptible of departing more or less from the average parental character, and their number is in excess of that of their parents. Severe competition for the means of living, or the struggle for existence, is a necessary consequence of unlimited multiplication; while selection, or the preservation of favourable variations and the extinction of others, is a necessary consequence of severe competition. "Favourable variations" are those which are better adapted to surrounding conditions. It follows, therefore, that every variety which is selected into a species is so favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals. In other words, every species which exists, exists in virtue of adaptation, and whatever accounts for that adaptation accounts for the existence of the species.

Selection, then, is the method by which every species has originated; but it can operate only if variation occurs. To put it in another way, the origin of species in general lies in variation; the origin of any particular

species lies, firstly, in the occurrence and, secondly, in the selection and preservation of a particular variation:—

> Clearness on this head will relieve one from the necessity of attending to the fallacious assertion that natural selection is a *deus ex machina*, or occult agency.
>
> Those who confuse the operation of the natural causes which bring about variation and natural selection with what they are pleased to call "chance" can hardly have read the opening paragraph of the fifth chapter of the *Origin* (1st ed., p. 131): "I have sometimes spoken as if the variations.....had been due to chance. This is, of course, a wholly incorrect expression, but it seems to acknowledge plainly our ignorance of the cause of each particular variation."
>
> Another point of great importance to the right comprehension of the theory is that, while every species must needs have some adaptive advantageous characters to which it owes its preservation by selection, it may possess any number of others which are neither advantageous nor disadvantageous, but indifferent, or even slightly disadvantageous (*ibid.*, p. 81). For variations take place not merely in one organ or function at a time, but in many; and thus an advantageous variation, which gives rise to the selection of a new race or species, may be accompanied by others which are indifferent, but which are just as strongly hereditary as the advantageous variations. The advantageous structure is but one product of a modified general constitution which may manifest itself by several other products, and the selective process carries the general constitution along with the advantageous special peculiarity. A given species of plant may owe its existence to the selective adaptation of its flowers to insect fertilizers; but the character of its leaves may be the result of variations of an indifferent character. It is the origin of variations of this kind to which Darwin refers in his frequent reference

to what he calls "laws of correlation of growth" or "correlated variation."

These considerations lead us further to see the inappropriateness of the objections raised to Darwin's theory on the ground that natural selection does not account for the first commencements of useful organs. But it does not pretend to do so. The source of such commencements is necessarily to be sought in different variations, which remain unaffected by selection until they have taken such a form as to become utilizable in the struggle for existence.

It is not essential to Darwin's theory that anything more should be assumed than the facts of heredity, variation, and unlimited multiplication, and the validity of the deductive reasoning as to the effect of the last (that is, of the struggle for existence it involves) upon the varieties resulting from the operation of the former. Nor is it essential that one should take up any particular position in regard to the mode of variation—whether, for example, it takes *per saltum* or gradually; whether it is definite in character or indefinite. Still less are those who accept the theory bound to any particular views as to the causes of heredity or of variation.

That Darwin held strong opinions on some or all of these points may be quite true; but, so far as the theory is concerned, they must be regarded as *obiter dicta*. With respect to the causes of variation, Darwin's opinions are, from first to last, put forward altogether tentatively. In the first edition of the *Origin* he attributes the strongest influences to changes in the conditions of life of parental organisms, which he appears to think act on the germ through the intermediation of the sexual organs. He points out, over and over again, that habit, use, disuse, and the direct influence of conditions have some effect; but he does not think it great, and he draws attention to the difficulty of distinguishing between

effects of these agencies and those of selection. There is, however, one class of variations which he withdraws from the direct influence of selection—namely, the variations in the fertility of the sexual union of more or less closely allied forms. He regards less fertility, or more or less complete sterility, as "incidental to other acquired differences" (*ibid.*, p. 245).

Considering the difficulties which surround the question of the causes of variation, it is not to be wondered at that Darwin should have inclined sometimes rather more to one, and sometimes rather more to another, of the possible alternatives. There is little difference between the last edition of the *Origin* (1872) and the first on this head. In 1876, however, he writes to Moritz Wagner: "In my opinion, the greatest error which I have committed has been not allowing sufficient weight to the direct action of the environments (i.e., food, climate, etc.), independently of natural selection..... When I wrote the *Origin*, and for some years afterwards, I could find little good evidence of the direct action of the environment: now there is a large body of evidence, and your case of the Saturnia is one of the most remarkable of which I have heard" (III, p. 159). But there is really nothing to prevent the most tenacious adherent to the theory of natural selection from taking any view he pleases as to the importance of the direct influence of conditions and the hereditary transmissibility of the modifications which they produce. In fact, there is a good deal to be said for the view that the so-called direct influence of conditions is itself a case of selection. Whether the hypothesis of Pangenesis be accepted or rejected, it can hardly be doubted that the struggle for existence goes on not merely between distinct organisms, but between the physiological units of which each organism is composed, and that changes in external conditions favour some and hinder others.

The problem of variation is part of the problem of heredity. Darwin's own speculation of Pangenesis, confessedly unproven, while providing a mass of facts suggestive of some such explanation as he threw out, was also designed to provoke further research, especially by comparative morphologists, into the physical basis of heredity, and the nature of the way in which new qualities appear are fixed and expanded. Then the re-discovery of Mendel's law in 1900 led to an immensity of experimental research in breeding and embryology which has provided an analysis of the subject unsuspected in Darwin's day. A profound distinction has been drawn between variations in individuals acquired during life as the effect of external conditions, and the variations which appear as the result of pre-natal changes in the germ cells. The name "modification" has been restricted to the former; to the latter the name of "mutation" has been assigned. Research seems to show that acquired characters (modifications) do not recur in subsequent generations when the external stimulus ceases; they are not, properly speaking, inherited as a permanently specific character. Save in the case of certain intrusive poisons, the external stimuli have not been shown to affect the germ plasm from which the next individual springs. So far as a species is conceived of as taking permanently heritable form through the accumulation of "acquired characters" in this sense, the conception needs to be revised.

Experiment also seems to show that, when a new character has been developed in a group of organisms, its mean strength does not tend to increase cumulatively. Such progressive increase depends on the occurrence of fresh mutations, great or small, which can be picked out by the process of selection. Nor is there any lack of material from which to select, for these mutations, or

changes in the heritable constitution itself, are of frequent occurrence. Moreover, crossing will continually be throwing up new combinations of the hereditary factors already existing, and these new combinations are of great importance for the evolution of new species. In their nature they resemble those variations which are strongly enough marked to be called "sports" by the breeders, and from which, as is well known, distinct varieties have been established by artificial selection. The mechanism is still unknown by which certain of the resultant characters are emphasized, although in many instances it is probable that further mutations, which modify or intensify the effect of the original change, have arisen.

But though Darwin could see no difficulty in natural selection producing the most exquisite structure, if such structure can be arrived at by gradation, he held any large gap suspect; and, in default of sufficient evidence as to the survival of these marked sports in a state of nature, he was, if anything, too cautious in holding to the maxim, *Natura non facit saltum*, and, indeed, confessed later that he might have laid more stress upon the larger variations.

That Darwin unnecessarily hampered himself by adhering so strictly to this favourite axiom was the opinion of T. H. Huxley, who remarked: "We greatly suspect that she [Nature] does make considerable jumps in the way of variation now and then, and that these saltations give rise to some of the gaps which appear to exist in the series of known forms."

One of the first and most notable reviews of the *Origin* pointed out with clearness and discrimination how Darwin satisfactorily showed that the modifying causes and the selective power he appeals to do exist in nature, and how his theory had this immense advantage

over any of its predecessors, that it supplies a reason for many apparent anomalies in the distribution of living things in time and space, for the existence of rudimentary forms, for persistence or retrogression as well as progressive development. Moreover, unlike some speculators on the subject, Darwin yoked his scientific imagination to plodding fact. He "abhors mere speculation as nature abhors a vacuum. He is as greedy of cases and precedents as any constitutional lawyer, and all the principles he lays down are capable of being brought to the test of observation and experiment."

Thus, the reviewer notes, the combined investigations of another twenty years may perhaps enable naturalists to say whether the causes invoked by Darwin and shown by him to exist are sufficient to produce all the effects he ascribes to them, or whether he has over-estimated the value of natural selection as greatly as Lamarck over-estimated his operative cause of modification by exercise.

Twenty-one years later, and still in Darwin's lifetime, the same writer was able to record in *The Coming of Age of "The Origin of Species,"* that these investigations, stimulated by Darwin's thought, had spoken, and that both from palæontology and from embryology an overwhelming mass of evidence had been accumulated in support of the process of Evolution.

X

THE EVOLUTIONARY IDEA

MUCH of the immense popularity which the *Origin* at once acquired "must be assigned to a somewhat delusive simplicity of style, which tends to disguise the complexity and difficulty of the subject, and much to the wealth of information on all sorts of curious problems of natural history, which is made accessible to the most unlearned reader" (T. H. H., *Obit.*, p. 286).

But it was not only as a great study in natural history that the book was eagerly read, while Mudie's was thronged and copies snatched from the hands of the assistants. The theory, as applied to the brute creation alone, would have left the public unmoved; the excitement began in its application to the origin, and therefore to the destinies, of man. In the Western world the very ancient doctrine of Evolution had been bound hand and foot and cast into outer darkness during the long centuries of theological domination, and still the ecclesiastical thunderbolts impended over those whose reasonings led them outside the Hebrew cosmogony, which so long led physical science captive. Now, the half-guesswork of ancient philosophy arose in new armour of proof; its new array of evidence compelled a hearing. For though the uniformitarian geology set forth by Lyell led definitely to some form of evolution among living creatures, and the accumulating facts of palæontology linked with embryology (most important evidence of all) would ultimately have established a theory of

evolution if Darwin had never existed, yet up to his time Evolution was unsupported by sufficient evidence. The minute period of geological time included in human history pointed to the permanence rather than the mutation of species. The development theory of Lamarck was not well based, and its failure only weakened the position of evolutionary thinkers. Thus the pre-Darwinian, though feeling that some proof of Evolution would one day be established, was forced to confront both parties with the same question and the same lack of satisfactory answer: "What evidence have you for your assertions?"

When Darwin came forward he was faced on the one hand by the literal interpretation of the first chapter of *Genesis*; on the other by the doctrine of final causes. To study nature in its purely physical aspect, apart from its moral and metaphysical aspect, was, in the words of Adam Sedgwick, to be deep in the mire of folly. To ignore the link between material and moral, as given by final cause, was to degrade humanity. Development might be frankly admitted, but only on these terms, by the men who had felt the pressure of the advance in geological knowledge, yet clung to long-dominant theological conceptions. Nevertheless, those men of science who were both able to appreciate and, unlike the "Father" in that remarkable book, *Father and Son*, free to accept the theory, rapidly perceived that it gave a real reason for Nature's selection of the future parents of the species in those individuals among the varying multitude best suited to their place, as inevitably as the pebbles are sorted out according to their size by the waves along the Chesil beach. The theory spread among these men all the more rapidly because they already had in their minds many facts bearing on the question, only awaiting the key which was to set them all in order and relation to one another.

Granted that the logical proof of Natural Selection is, strictly speaking, incomplete until mutually infertile species are produced experimentally—a thing, perhaps, impossible under the conditions at our command and in view of the change in the reproductive powers produced by domestication; granted that in dealing with the nature of variation and its causes, the closer study of which has sprung from his work, Darwin hampered himself unnecessarily by adopting the principle *Natura non facit saltum*, and so treating all variations as something minute instead of laying stress on the larger mutations also, as he originally proposed to do; still, new discoveries as to the causes of variation and heredity do but widen the field of evolutionary operations. Whatever the modifications in detail required by further development of knowledge, whatever changes are found necessary in Darwin's conception of the methods whereby Nature effects her ends, the main issue remains unshaken—nay, rather buttressed—by the subsequent discoveries it has itself stimulated. The main thing is that Evolution is a firmly established and fertile theory, and that scientific method —a matter of more value than its results at any given moment—has vindicated its worth. For the future all general conceptions, all reasoned thought and fruitful speculation, must rest on the idea of continuity in change, of the orderly succession which we call laws of nature, and the exclusion of irrational breaks in this order.

Indeed, Darwin himself urged this view: the doctrine of Descent is "the turning-point"; and Natural Selection is, in comparison, "utterly unimportant," and this though personally he "cared much about Natural Selection." For while any theory of descent will connect a vast number of generalizations in natural history by an intelligible thread of reasoning, he claimed at least that no theory so well explains them as that of Natural

Selection; nor has any other satisfactory explanation ever been offered of the almost perfect adaptation of all organic beings to each other, and to their physical conditions of life. "Whether the naturalist believes in the views given by Lamarck, by Geoffroy St. Hilaire, by the author of the *Vestiges*, by Mr. Wallace and myself, or in any other such view, signifies extremely little in comparison with the admission that species have descended from other species, and have not been created immutable; for he who admits this as a great truth has a wide field opened to him for further inquiry" (III, 22).

Unreason in every form is the enemy of scientific method, and the victory of science which we associate with the name of Darwin means the gradual banishment of unnumbered bogeys and fanciful superstitions—offspring of strong sensibilities and false reasoning. With these, also, go many fancies and myths and fairy-tales, which survived to form a beautiful, if misty, background to every-day thought. Is it, then, true, as the lovers of the day before yesterday deplore, that the march of evolutionary science has robbed the world of its illusions, its beauty, its aspirations, and given in their stead naked fact, mechanical order, pedestrian reason? It is true, rather, that each new ideal, each new generalization, pushes out the old, ruthlessly tearing the fair fabric of imagery and allegory which drapes it round. Man cannot live without some ideal, any more than he can live without some sense of beauty; but it is with the ideal as it is with beauty, for beauty does not rest in untruth, nor is the loveliness of a landscape less appreciated by reason of a knowledge of perspective. The knowledge that destroys false beauties enthrones new ones; while it brings certain desirable and ideal conditions nearer present realization.

In the eternal problems of "fate, fore-knowledge, and

freewill," the old predestinarianism is in some measure reinforced by what may be termed scientific Calvinism, which, by the way, is by no means inconsistent with the argument of ultimate design, though the "general providence" admissible is, of course, inconsistent with "special providences." But two points must be noted: one, that such determinism lays stress on the value of motives and their cultivation, leaving responsibility in a practical form, and insists anew from the positive side on the value of education and training; the other, that it is a popular fallacy to suppose that the trend of evolution is to a society where the strong and ruthless alone shall possess the land. In the state of nature, no doubt, natural selection works by battle and extermination; but as society takes shape the unit in the struggle is changed. It ceases to be the individual; it is the organized group, family, tribe or nation, special association, or general league. Within such a group the war of physical extermination is suspended; the social and helpful qualities prevail with mutual aid, and by co-operation lend new strength to the larger unit. Social selection, if one may coin the term, takes the chief place instead of natural selection in determining the line of human evolution. That line follows mental rather than physical development; it runs so far counter to the process from which it has sprung that human society in the midst of the cosmos may be compared to an eddy in a river—it is part of the main stream, yet with its own swirling current running counter to the immediate direction of the parent stream.

To hold that the evolutionary process accounts for the origin of morality is by no means the same thing as to hold that the principle of Evolution in general can be adopted as an ethical principle, implying that morality is guided by the physical struggle for existence.

Like the other observed sequences of physical fact which have unhappily received the name of "laws" of nature, the "law" of natural selection is not a legal obligation nor a moral injunction. Legality and morality begin with the association of humankind, or possibly of creatures lower in the scale of consciousness. The pirate, whose honour extends only to his own thievish band; the robber state, which admits only force and cunning in its relations with other state units—these both, on a smaller or a larger scale, go back upon the fundamental principle of typically human development. When they justify themselves they do so on what they call Darwinian principles. The strong and cunning, they say, are the fittest; it is right for them to survive—let the rest perish.

But this is not really the teaching of Darwin. There is an ambiguity in the word "fittest." Standing thus alone, it suggests a moral quality in the "fit" to survive which is far from the original meaning—namely, that those individuals which are best fitted, best adapted, to the conditions around them will survive where the less well-adapted will perish. In a new glacial period the *Protococcus nivalis* would be the best fitted to survive, though neither strong nor cunning, nor gifted with the social sympathy which creates moral associations in a cosmic stream that in itself is neither moral nor immoral, but simply a physical process.

Had old Sir Thomas Browne been living in these days, he would, I think, have included this summary interpretation of the "law" of the survival of the fittest among his Vulgar Errors along with the once popular beliefs that since, by the "law" of gravitation as commonly observed, iron sinks in water and solid bodies drop through the air, therefore to build iron ships and to attempt to fly are not merely impossible, but contrary to

nature, and so actually indeed immoral, and in the last resort impious. The fallacy—need it be said?—lies in failure to determine the complete scope and bearing of the conditions involved, which from time to time settle what qualities have the best survival value: here for the individual within the group, there for the group among other groups. And right, if the word has any meaning, only begins with the conscious limitation of power.

The quickening leaven which Darwin contributed to thought worked in two ways. By establishing the evolutionary idea, it brought life and the problems of living history into the ambit of scientific method; and, by the very struggle to establish that idea, wrung from the dominant powers of obstruction the right of free utterance of thought. Indeed, this first victory cleared the way for the expansion of research and the unshackling of the individual; so that of all the services rendered by Darwin to his own and future generations none, perhaps, was greater than this: the battle of freedom of thought was fought and won over the *Origin of Species*. Sneered at, stormed at, denounced, even ostracized for a time, the Galileos of the new theory were not compelled to recant, even with the tempering ejaculation, "*E pur si muove.*" They included fighters as well as thinkers. Scorn met with scorn—the scorn of burning knowledge for chartered ignorance. In the end, indeed, the success of the new ideas was such that the next generation in the ecclesiastical world began to accept some form of this fundamental principle in the physical world as a doctrine alleged to have been virtually held by their Community all the time, while the accounts of resistance and persecution were regarded with incredulity.

The result was the more sweeping because all the thinking public were interested. Old teachings about the origin and destiny of man were in the balance. Dogmas,

those high decisions which do not wait upon poor material proof, were shaken. Historical criticism found collateral support. When the key of the position fell, when at the touch of the Ithuriel spear of science the creation myth and its sequel, misused as scientific arguments, were revealed in their true proportions, the result was felt far and wide. With them crumbled the logical framework of the Pauline theology which has overlaid and modified the teaching and the presentment of the Founder of Christianity. Freedom of thought, once conceded in the corner of physical science which touched so closely on religious and moral questions, was exercised in other quarters. No longer was it anathema to range beyond an anthropocentric world, to deal as freely with comparative religion as with comparative anatomy, to seek the root and beginnings of the moral faculties among the brutes, to find the secret of original sin, not in the fall of the first man from an imaginary state of primitive innocence, but in the selfish impulses inherited from the ancestral struggle for existence under the long, sub-human cosmic process, and surviving inharmoniously in the altruistic communities founded by man. The progress already made and the reasonable hope of yet further betterment gave a new cast to the idea of human destiny before set irrecoverably on the slopes of deterioration, while as to the mystery of suffering it was strikingly said: "I cannot but think that he who finds a certain proportion of pain and evil inseparably woven up in the life of the very worms will bear his own share with more courage and submission, and will, at any rate, view with suspicion those weakly amiable theories of the divine government which would have us believe pain to be an oversight and a mistake, to be corrected by-and-by."

Again a touch of scientific Calvinism, and with it

comes a recognition of the stern severity of Nature: of the fact that she is neither moral nor immoral, but simply non-moral, so that morality is justified only of her children in the conscious world, and religious arguments based on the moral or immoral tendencies of Nature fall to the ground, and with them the detailed interpretation of natural processes by final causes. This scientific view is based upon and restricted by its constantly-tested evidence, compounded of observation, experiment, and verification. It only ventures to describe the "why" indirectly in terms of the "how" which it investigates—namely, as the result of causes found to be at work. As to the ultimate "why," it has no trustworthy evidence, and can only suspend judgment.

The conception of the uniformity of nature, the inviolable sequence of cause and effect without external interference, had already entered into the realm of physical science. Under all the complexity of details so hard to unravel appeared the ramifications of the same fundamental causes familiar to the ordinary mind in the daily and less complicated experiences of time and space, mass and motion and number and the rest. But vital phenomena had always been far more difficult and elusive. Life begins in mystery; life ends in mystery. Life is poised between matter and thought, between the unconscious and the conscious. It is sustained by both in varying degrees. They blend; but what is the relation between them? Is the one but the efflorescence of the other, or does it use that other as a medium for its own development?

Yet even to this realm of uncertain forces the evolutionary theory brought steadily the conception of universal causation, and justified the efforts to make this clear in every province of that realm, in mind as well as the more obvious bodily processes. As the idea of unbroken

causation progresses, the notion of events as produced by spiritual intervention shrinks. The thought of primitive man, acquainted with his own volition as a cause of action, adjudged a similar cause to phenomena external to himself. Storm, wind, sun, and river, bringers of destruction or of harvest, moved at the will of in-dwelling gods. The West African sees a spirit in his homeliest domestic utensils; the logic of ancient Rome extended this idea from objects to actions in the abstract. The Roman parent would invoke Educa to teach the weanling to eat and Potina to drink; Iterduca to take the children safely out, and Domiduca to bring them safe home again. In the Western world, at all events, before Evolution took its new shape, the country parson knew better than to pray for rain during a spell of east wind; it remained for the conception so strongly refounded by Darwin to make a reasoned advanced towards including the most elusive and mysterious provinces of experience under the unbroken scheme of causality that was felt to rule the physical realm. His work provoked research into every corner of life, into every process that might further elucidate the weaving of the web of life; and on the remotest border of that web, where impalpably subtle threads of consciousness enter into the fabric, psychology, ethics, the religious impulse, all underwent a new analysis and a new synthesis. But knowledge has its limits; our faculties, our evidences, leave an unknown—perhaps an unknowable—tract to our imagination.

Immortality it leaves where Archbishop Whately found it, recent years having provided much in the way of psychological by-products, but nothing of sure value on the main question. Nevertheless, it would aim at no stunting or starving of this life for purely transcendental reasons. "Is there no second life? Pitch this one high" it might

cry with the poet, and it ventures to find positive grounds for many virtues formerly justified on intuitive or transcendental grounds. Indeed, it reconciles the intuitive basis of morals with the experiential, since it finds the origin of such intuition, and of the necessary forms of thought, as it finds the origin of instinct, in the age-long impressions of experience which by selection and inheritance have affected structure. The experiences of the ancestor have, so to say, become the presuppositions of the descendant.

Fuller understanding of the past, new hope for the future in intellectual achievement and practical betterment, further effort for the predominance of reason over unreason while freeing aspiration from its harmful and pretentious accretions—these at least offer ideals not wholly past the scope of man to carry out, whatever lies beyond, though he works within the limits imposed by time and space, undazzled by any millennial anticipations.

XI

FURTHER SPECIES WORK

DARWIN was past fifty when the *Origin of Species* was published. Happier in this than many discoverers, he lived to see his discoveries accepted by the thinking world. He beheld his great thought spreading in a wide flood, and, like the waters of the Nile, leaving behind its fertilizing traces in every field it touched. The book itself, besides being translated into almost every civilized tongue, was repeatedly revised and re-issued, reaching its sixth edition in 1872, and its twenty-fourth thousand in 1882, the year of Darwin's death.

His main work on Species, however, was by no means finished with the publication of the *Origin*. That book, it must be remembered, was only an abstract of the vast material he had accumulated. It remained for him to assemble further *pièces justificatives* from this material in *The Variation of Animals and Plants under Domestication*, his principal task for eight years until its publication in 1868. Vol. I gave all his observations and an immense number of facts collected from various sources about our domestic productions. Vol II discussed the causes and laws of variation, inheritance, and so forth, as far as existing knowledge permitted, while towards the end of the work he put forward his "well-abused" provisional hypothesis of Pangenesis to account for the phenomena of heredity—viz., that all the cells of the body, if not too highly differentiated, throw off characteristic gemmules, which, being specially concen-

trated in the reproductive elements, ultimately give rise to cells like their parent cells.

Writing to Asa Gray, Darwin remarks: "The chapter on what I call Pangenesis will be called a mad dream, and I shall be pretty well satisfied if you think it a dream worth publishing; but at the bottom of my own mind I think it contains a great truth."

When Wallace, admiring the chapter, remarks: "It is a *positive comfort* to me to have any feasible explanation of a difficulty that has always been haunting me, and I shall never be able to give it up till a better one supplies its place, and that I think hardly possible," Darwin tells Hooker: "His words express my sentiments exactly and fully, though perhaps I feel the relief extra strongly from having during many years vainly attempted to form some hypothesis.....[At least] this hypothesis serves as a useful connecting-link for various grand classes of physiological facts, which at present stand absolutely isolated" (III, 81–2).

These hypothetical gemmules, invoked as the mechanism of heredity, were admittedly "not proven"; but, like other assumed mechanisms which escape the senses, could do good service as an orderly basis for future observation, for by such a theory "an astonishing number of isolated facts can be thus connected together and rendered intelligible." In the event, subsequent research established the separation of the future reproductive cells from the cells which are forming the body at an early stage of embryonic development; but it remains true in the words of a contemporary critic (Hooker), that, whatever the scientific value of the gemmule notion might turn out to be, the statement of the theory was "the clearest and most systematic résumé of the many wonderful phenomena of reproduction and inheritance that has yet appeared." The

two volumes of this work, of which a second edition appeared in 1875, came into action as an army of reserve, driving home the spearhead of the *Origin*, and falling into rank and order according to the lines of its argument, which had already been assimilated. Without the preceding "Abstract," so fortunately necessitated by Wallace's appearance in the field, the work as originally projected to contain the substance both of the *Origin* and the *Variations* would assuredly have been bewildering in its vastness, and would have failed to make the keen and immediate impression of the one volume which had been condensed and hammered to a sharp edge.

This completed, he proceeded to fulfil the hint given in the *Origin* as to the descent of man, that by the work "light would be thrown on the origin of man and his history." From the first moment, in 1837 or 1838, that he had become convinced that species were mutable productions, he could not avoid the belief that man must come under the same law. For his own satisfaction, and for a long time with no intention of publishing, he collected notes on the subject. To treat it properly would have required a book to itself; the evidence could not possibly be included in the "abstract" of his views, and to have paraded his conviction as to man's origin without giving any evidence would have been both useless in itself and injurious to the success of the book. Thus, in his own words: "Although in the *Origin of Species* the derivation of any particular species is never discussed, yet I thought it best, in order that no honourable man should accuse me of concealing my views, to add that by the work 'light would be thrown on the origin of man and his history.'"

The corollary to the theory of the *Origin* was immediately perceived, and sprang into the forefront of debate with the untenable assertions of the great anatomist

Owen and the impolitic jeers of the Bishop of Oxford in 1860. The fact that anatomically there is less difference between man and the apes than between the apes and other simians was triumphantly shown by Huxley and his friends in a long scientific battle. Happily for Darwin's tranquillity, these champions drew the enemy's fire. The odium of touching such a question was much spent before Darwin was ready with his fuller work, while the irrefragable proofs of structural affinity in body and brain with the apes prepared the ground for the reception of the subsequent work. Considering that by this time many naturalists fully accepted the doctrine of the evolution of species, it seemed fitting to follow up the general mass of evidences in the Variation book by giving the evidence as to Man. The brief avowal in the *Origin* had not spared him the taunt that he concealed his views on the point, engrossed though he was with long years of work on Variation. The latter had fatigued him so much that, although he had the MS. for a further volume on the Variation of Species in a State of Nature almost ready for a long time, he gladly turned aside to refresh and "amuse" himself by working up his store of notes and writing a treatise—he expected it to be a short one—on the Descent of Man, the most domesticated of Animals. It took him three years to write (1st ed. 1871; 2nd, largely corrected, 1874), for it branched out into collateral subjects.

As has been noted, the evolutionary idea, with its corollary regarding man's descent, had by 1871 found firm standing ground. The publication of the *Origin*, and then of T. H. Huxley's *Man's Place in Nature*, had drawn the enemy's fire. Opponents had declared themselves at once; but the arguments stood so firm, the evidence was so abundant, that the ranks of supporters grew, and scientific doubters of the older school began to

come in. When at last Darwin had time to fulfil his promise of throwing new light upon the origin of man by the same process as in the case of other species, the implicit idea had lost its startlingness, and appeared as a logical expansion of the widely accepted conception of a general evolution. Haeckel's great book, also, had recently appeared discussing the scheme of evolution from the Monad to Man so fully that, if the *Descent of Man* had not been already written, Darwin declared he would not have put pen to paper.

The argument of his book was, first, to put forward the evidence of man's descent from some lower form, as shown by correspondence in structure, in the process of development, and in many special details, including rudimentary structures; next to show that man varies, multiplies beyond immediate subsistence, especially in contest with rival tribes, and thus comes under the process of natural selection. Drawing largely in all this from the *Origin* and from the book on Variation, he shows the great importance of man's adopting an erect posture. It affords the hands freedom to use every form of implement, instead of leaving the organism to develop more limited adaptations of some bodily structure to its fresh needs. It gives the concrete, as language gives the abstract, basis for mental advance, which leaves so great an intellectual gap between civilized man and the savage, and so vastly greater a gap between the lowest savage and the highest ape, albeit animals share to a limited extent in our emotions and such faculties as curiosity, imitation, attention, memory, imagination, and reason.

Intellectual advance, thus secured, is the cardinal point in man's subsequent development and his dominance over the whole world. At the same time, instinct weakens in man because the general growth of mental power means

freer connection between the several parts of the brain, the separated functioning of which lies at the root of instinct and its limited completeness.

While the evidence shows that civilized nations were once barbarous, natural selection, with its penalty of extinction, comes at last to apply less to individuals within the tribal group than to the group in competition with other groups where superiority consists in superior mental development.

In man as a social animal the more enduring social instincts tend to conquer other less persistent instincts. With the growing power of the intellect to look before and after, so as to compare and balance the feelings involved in satisfying or disobeying the claims of the society of which man is a unit, there come later the beginning of conscious morality and the growth of the personal virtues as well as the herd virtues so strongly marked in some animal communities. Subsequently the moral tendencies, being socially selected, largely become innate in the new generation, whatever the primary pressure under which they originated, and require only the stimulus of sympathy, not the laboured weighing of advantage or disadvantage, to call them into action.

Finally, the book discusses the affinities and genealogy of man, and his position in the animal series. He does not constitute a separate kingdom, which implies something more than a difference in degree, however great. To make him a separate sub-class of the mammalia in virtue of the development of one character alone, and that a very variable one—namely, the brain—while ignoring a multitude of resemblances in other points, runs counter to the recognized and justifiable procedure in determining the proper place in a genealogical system. Even to rank man as a separate Order seems excessive. Darwin is content, with all additional evidence at hand, to follow

Linnaeus in placing man in the same Order with the Quadrumana (the apes and monkeys) under the title of the Primates, and to rank him not even as a Sub-order, but as a Family or possibly even a Sub-family. By the sum of anatomical likenesses, he is an offshoot from the Old World Simian stem, not from that of the New World.

Though the existing man-like apes form a distinct sub-group, it is a broken group, containing forms which depart widely from any common type. Man, in short, as Mivart remarks, "is but one of several exceptional forms of Primates," and is descended not from any one of them which we know, but from a common ancestor far back, just as other sub-groups of Old World monkeys probably have a common ancestor in certain fossil forms intermediate between them.

A certain schoolboy's answer in a General Knowledge paper some five-and-twenty years ago admirably illustrates the popular misbelief as to Darwin's theory of descent, no less than the loose meaning so often attached to the definite word "proof." Asked what he knew about Charles Darwin, the boy wrote: "Charles Darwin proved that men are descended from monkeys. N.B.—Not generally believed." Instructors of the popular mind used to pay no heed to Darwin's express words (p. 155):—

> We must not fall into the error of supposing that the early progenitor of the whole Simian stock, including man, was identical with, or even closely resembled, any existing ape or monkey.

Except for the essential fact that man's birthplace must be in the Old World, excluding Australia and any oceanic island, Darwin thought it then useless to speculate on the precise locality. While our existing man-like apes live in the tropics, fossils show early man-like apes existing in Miocene Europe.

In his sketch of the chain of life which culminates in man, Darwin confines himself to a few general remarks on the lower stages in the genealogy, such as that no true bird or reptile intervenes in the direct line of mammalian descent, though links exist which bind together groups now utterly distinct. But, in following up the evidence, he comes to the lancelet, that very primitive vertebrate, its established affinities with the Ascidians, and the recent investigations into the larvæ of the Ascidians, with their developmental relations to the Vertebrates. Thereupon he says :—

> If we may rely on embryology, ever the safest guide in classification, it seems that we have at last gained a clue to the source whence the Vertebrata were derived. We should then be justified in believing that at an extremely remote period a group of animals existed, resembling in many respects the larvæ of our present Ascidians, which diverged into two great branches—the one retrograding in development and producing the present class of Ascidians, the other rising to the crown and summit of the animal kingdom by giving birth to the Vertebrata.

Most important of the collateral subjects into which the *Descent of Man* branched out was the treatise on "The Expression of the Emotions in Men and Animals," which he had intended to make one chapter of the larger book. But his notes, which had been accumulating since the birth of his first child in 1839, were too extensive for this, and finally took shape as a separate book. The greater part of the year 1872 was occupied with writing this and getting ready the sixth edition of the *Origin*. It appeared in November, and was awaited with such interest that over 5,000 copies were sold on the day of publication.

The interest of the book lies not only in the fact that

from the anatomical point of view it sums up and amplifies the results of its predecessors touching the mechanics of expression—that is to say, the work of the several facial muscles separately or in combination which bring about facial expression of different emotions—or that it co-ordinates Darwin's own careful observations begun thirty years before with the observations of his forerunners and with the reports of many observers replying to a printed list of questions he circulated among them, in regard especially to children, insane persons, and uncivilized races, whose mode of expression would be a matter of nature, not of convention. All this was of great value, especially as establishing the close similarity of the natural modes of expression in all sections of mankind; but far more valuable were the general results he deduced from his material, which applied to gesture as well as facial expression. He was able to lay down definite principles which in general determine what set of muscles shall be called into play on each occasion.

His three principles he enunciates as follows (*Expression of the Emotions*, p. 28):—

1. *The principle of serviceable associated Habits.*—Certain complex actions are of direct or indirect service under certain states of the mind, in order to relieve or gratify certain sensations, desires, etc.; and whenever the same state of mind is induced, however feebly, there is a tendency, through the force of habit and association, for the same movements to be performed, though they may not then be of the least use. Some actions ordinarily associated through habit with certain states of the mind may be partially repressed through the will, and in such cases the muscles which are least under the separate control of the will are the most liable still to act, causing movements which we recognize as expressive.

In certain other cases the checking of one habitual movement requires other slight movements; and these are likewise expressive.

2. *The principle of Antithesis.*—Certain states of the mind lead to certain habitual actions, which are of service, as under our first principle. Now, when a directly opposite state of mind is induced, there is a strong and involuntary tendency to the performance of movements of a directly opposite nature, though these are of no use; and such movements are in some cases highly expressive.

3. *The principle of actions due to the constitution of the Nervous System, independently from the first of the Will, and independently to a certain extent of Habit.*—When the sensorium is strongly excited nerve-force is generated in excess, and is transmitted in certain definite directions, depending on the connection of the nerve-cells and partly on habit; or the supply of nerve-force may, as it appears, be interrupted. Effects are thus produced which we recognize as expressive. This third principle may, for the sake of brevity, be called that of the direct action of the nervous system.

Pre-evolutionary writers on the subject had been curiously restricted in their outlook by their belief in special creation. The great anatomist Sir Charles Bell, who so thoroughly explored the facial muscles, was moved to declare that these were a special divine gift to man for the truly human purpose of expressing emotion. Others came to the conclusion that the causes of the several expressions are inexplicable. One of the most careful observers could aver that, if the phrase might be forgiven, it was a divine whim on the part of the creator to put into action such and such muscles when he wished the characteristic sign of emotion to flit across the countenance, and in order to render this language of the

face permanent and unchangeable it sufficed for him to implant in every human being the instinct to express his feelings through the contraction of these muscles.

In these conclusions sentiment had the upper hand of science. Darwin, however, as Herbert Spencer had already begun to do, was able to attack the question fully from the evolutionary standpoint. He not only traced expression through the different grades of mankind, but by parallel analysis in detail showed that the modes of expression in animals, with whatever differences in act from those of men, follow the same principles. In the simians, indeed, they come closest in form, and in many instances are affected by those very muscles of the face which sentiment claimed as the sole prerogative of mankind.

Here, then, once more, evolution proved the key to an unsolved problem, and with this key in his hand Darwin found that the chief interest of the subject lay in following out the wonderfully complex results to be drawn from studying the relation of mind and body as shown in conscious or reflex acts and the inheritance of habit.

XII

BOTANICAL WORK

SUCH, then, was the central current of Darwin's scientific work for the forty-one years since he set out on the *Beagle*—first, the scientific reports of the voyage of the *Beagle* with the theory of coral reefs, the later monograph on the Cirripedes, and the Journal with its wider and more personal sweep and its first hints of doubt as to the fixity of species; then the upbuilding of the evolutionary theory and the collection of evidence as to the moulding of the succession of forms by the inexorable pressure of causes visibly at work, at long last set forth in the *Origin*, the work on Variation among domesticated species, and the Descent of Man, with its offshoot on the Expression of the Emotions.

But this enormous mass of work was by no means all the work of that life of invalidism. Just as Darwin first spoke of his excursion into the species question as a bit of "idleness" relieving the stricter duty of getting ready the *Beagle* reports, so, when the vast labour of his evolutionary work became his main task, we find him constantly seeking "amusement" and "idleness" in Natural History observations and carefully conducted investigations, especially among plants, which are most easily experimented on and observed in a quiet country home such as Darwin's. From these sprang a series of studies—holiday work, so to say, from his main enterprise—which were sufficient to place him in the front rank of botanical discoverers and pioneers of new research and theory.

The first of these to appear was his little book *On the Various Contrivances by which Orchids are Fertilized by Insects*. This was a direct offshoot from his work on the *Origin*. When speculating on the subject he had very early concluded that crossing played an important part in keeping specific forms distinct, and as far back as the summer of 1839, if not earlier, began specially to look into the cross-fertilization of flowers by insects. Summer after summer he continued his observations, being greatly stimulated a couple of years later by reading Sprengel's long-neglected book of 1793 on the sexes in plants, following the advice of Robert Brown, the foremost botanist of the day, who in giving this advice performed one of his most notable services to science. Not content with reading the book, he verified many of Sprengel's statements: "it may be doubted whether there was a living botanical specialist, except perhaps Brown, who had done as much" (*Obit.*, p. 296).

Fascinated by the wonderful contrivances among the orchids for securing cross-fertilization, he resolved to write as complete a treatise as he could on this one group, as more effective for his purpose than to utilize the great mass of material which he had already collected about other plants. Following various papers on insect fertilization of plants published in the *Gardener's Chronicle* from 1857 onwards, the Orchid book appeared in 1862—an admirable example of Darwin's peculiar faculty for discovering new treasure in matter already worked over by others. And it had the happy consequence of setting other specialists to work on the fertilization of other flowers, with far completer results than could have been reached by one man alone.

But cross-fertilization must possess some advantage, if adaptations to that end were to be secured by natural

selection. He found by experiment that cross-fertilization produced greater fertility in the parent and greater vigour in the offspring. The more perfect the mechanism for cross-fertilization, the greater the advantage. "Thus the way lay open for the operation of natural selection in gradually perfecting the flower as a fertilization-trap. Analogous reasoning applies to the fertilizing insect. The better its structure is adapted to that of the trap, the more will it be able to profit by the bait, whether of honey or of pollen, to the exclusion of its competitors. Thus, by a sort of action and reaction, a two-fold series of adaptive modifications will be brought about" (*Obit.*, p. 297).

So important was the bearing of this subject on his whole theory that from 1865 Darwin undertook a "great series of laborious and difficult experiments on plant fertilization," which lasted eleven years, and furnished unexpected evidence for the favourable effects of crossing in these adapted groups, analogous to the invigorating effect on other groups of wide dispersion and amid fresh and varying surroundings. The resultant book was *The Effects of Cross and Self Fertilization in the Vegetable Kingdom*, which appeared in 1876.

Another and even more admirable example of Darwin's faculty for discovering what had been missed by others appeared in the same year, 1862, as the book on orchids, again as the result of thirteen or fourteen years' investigation. This was a paper in the Journal of the Linnean Society "On the Two Forms, or Dimorphic Condition of Primula," which was followed in the next five years by five other papers on plants with two and even three forms in their reproductive organs. Every one knew the pin form and the thrum form of primula—the long pistils with short stamens, the short pistils with long stamens; no one had the slightest inkling of their use or

significance. Careful experiments in fertilizing similar and dis-similar forms confirmed and extended a striking biological principle. "I do not think anything in my scientific life," he writes, "has given me such satisfaction as making out the meaning of the structure of these plants. I had noticed in 1838 or 1839 the dimorphism of *Linum flavum*, and had at first thought it was merely a case of unmeaning variability. But on examining the common species of primula I found that the two forms were much too regular and constant to be thus viewed. I therefore became almost convinced that the common cowslip and primrose were on the high-road to become dicœcious [i.e., with the sexes in different plants]; that the short pistil in the one form, and the short stamens in the other form, were tending towards abortion. The plants were therefore subjected under this point of view to trial; but as soon as the flowers with short pistils, fertilized with pollen from the short stamens [in the other form], were found to yield more seeds than any other of the four possible unions, the abortion theory was knocked on the head. After some additional experiment it became evident that the two forms, though both were perfect hermaphrodites, bore almost the same relation to one another as do the two sexes of an ordinary animal. With Lythrum we have the still more wonderful case of three forms standing in a similar relation to one another. [Of the eighteen possible unions, only the three same-length unions were fully fertile.] I afterwards found that the offspring from the union of two plants belonging to the same forms presented a close and curious analogy with hybrids from the union of two distinct species," the result of the "illegitimate" crossing having, as he believed, an important bearing on the sterility of hybrids. All this work on hetero-styled flowers, corrected and enlarged, was gathered up into a book, *The Different*

Forms of Flowers on Plants of the Same Species, 1877 (second edition, 1880).

Third in this series stands the work on *Climbing Plants*. Unlike its two predecessors, this was not a direct offshoot from his species work. His attention was caught by a short paper on the subject which Asa Gray, the well-known American botanist, published in 1858. The Autobiography tells how—

> He sent me seeds, and on raising some plants I was so much fascinated and perplexed by the revolving movements of the tendrils and stems—which movements are really very simple, though appearing at first sight very complex—that I procured various other kinds of climbing plants and studied the whole subject. I was all the more attracted to it from not being at all satisfied with the explanation which Henslow gave us in his lectures about twining plants—namely, that they had a natural tendency to grow up in a spire. This explanation proved quite erroneous. Some of the adaptations displayed by climbing plants are as beautiful as those of orchids for ensuring cross-fertilization.

This work first took shape as a long paper in 1864, which was sent to the Linnean Society. The paper, he confesses, was little noticed; he was so unwell when he received the proofs that he was forced to leave them very badly and often obscurely expressed. But when in 1875 it was corrected and issued as a book, it was widely read.

A sequel to this was the work of larger scope on *The Power of Movement in Plants*, written in conjunction with his son Francis, and published in 1880.

"This," he writes, "was a tough piece of work. The book bears somewhat the same relation to my little book on *Climbing Plants* which *Cross-Fertilization* did to the

Fertilization of Orchids, for, in accordance with the principle of Evolution, it was impossible to account for climbing plants having been developed in so many widely different groups unless all kinds of plants possess some slight power of movement of an analogous kind. This I proved to be the case, and I was further led to a rather wide generalization—viz., that the great and important classes of movements, excited by light, the attraction of gravity, etc., are all modified forms of the fundamental movement of circumnutation. It has always pleased me to exalt plants in the scale of organized beings, and I therefore felt an especial pleasure in showing how many and what admirably well-adapted movements the tip of a root possesses."

Next comes the work on *Insectivorous Plants*, again spread over some sixteen years. It began as an outdoor occupation when he was "idle and resting" at Hartfield in 1860, recovering from a bad bout of illness. There the sundew abounds with its rosette of pretty reddish leaves, each like a flat little spoon on a long handle, and spangled with dewy drops on the tips of its ring of tentacles.

Noticing that numerous insects had been entrapped by the leaves, he took some plants home, and, on giving them insects, saw the movements of the tentacles. There seemed to be some connection here, some special purpose in catching the insects. The tentacles would curl up to catch other substances laid upon them, but speedily let these go. "Fortunately," he says, "a crucial test occurred to me—that of placing a large number of leaves in various nitrogenous and non-nitrogenous fluids of equal density, and as soon as I found that the former alone excited energetic movements, it was obvious that here was a fine new field for investigation."

In the years that followed, whenever he had leisure. he pursued these experiments, not only on English but on foreign plants, receiving great help from Sir Joseph Hooker and Sir W. Thiselton Dyer, with their store of tropical plants at Kew. It was not till 1875 that his work was completed and published, as usual exciting wide interest, for, as he remarked, "the fact that a plant should secrete, when properly excited, a fluid containing an acid and ferment, closely analogous to the digestive fluid of an animal, was certainly a remarkable discovery."

As to the length of time that elapsed between the beginning and the end of his research, he makes the pertinent remark that "the delay in this case, as with all my other books, has been a great advantage to me, for a man, after a long interval, can criticize his own work almost as well as if it were that of another person."

XIII

GEOLOGY REVIVED: EARTHWORMS

Now, also, came a revival of his early geological interest, long thrust into the background by his other work.

He was astonished to find that thirty years after their publication his books on *Volcanic Islands* and *South America* were still consulted by geologists, and new editions were required. These were re-issued in a single volume, *Geological Observations on the Volcanic Islands and Parts of South America Visited During the Voyage of H.M.S. Beagle*, in 1876.

This revival of his old geological interest possibly led to his recording some observations on the origin of the angular gravels in the south of England. These, in the words of Professor James Geikie, who published them in his *Prehistoric Europe*, solve one of the most difficult problems in Quaternary Geology, and had already attracted the attention of German geologists.

The return to geology as well as the series of observational works, which Darwin undertook as by-tasks and reliefs from his central labours, is completed by the book on Earthworms, the full title of which is *The Formation of Vegetable Mould, through the Action of Worms, with Observations of their Habits.* This, the last of his published books (1881), had its forerunner in one of his very earliest papers, "On the Formation of Mould," read before the Geological Society on November 1, 1837. The point of contact between this and his other books was, as a reviewer remarked, the cumulative importance of the

infinitely little. It was a geological problem which first confronted him. How did it come to pass that, after a few years, fragments of burnt marl, cinders, and so forth, which had been thickly strewn over the surface of meadows, were to be found some inches below the turf, but still forming a layer? He found the answer by working out the suggestion of his uncle, Josiah Wedgwood (of Maer), that worms, by bringing earth to the surface in their castings, must undermine objects lying on the surface and cause an apparent undermining.

The interest never left him, and years afterwards he went on devising new ways of checking his estimates of the amount of work done by the worms; and from his friend Dr. (afterwards Sir George) King, at the Calcutta Botanical Gardens, he obtained much information as to the work of the earthworms in the tropics.

The old interest was vividly re-awakened in 1877, when his friend Sir Thomas (afterwards Lord) Farrer discovered close to his garden the remains of a Roman-British building, and Darwin was able to see with his own eyes the effects produced by the earthworms on the solid floors and walls.

Then, when he was passing through the press his book on *The Power of Movements in Plants*, he set regularly to work on the Earthworm book, putting together his notes with all the enthusiasm of riding a new hobby—intensely interesting to himself, however doubtful he might be of its interesting others. But in this he was delightfully mistaken. He found it "received with almost laughable enthusiasm." In the first three years it sold 8,500 copies, a sale "relatively greater than that of the *Origin*."

"It is not difficult," writes Sir F. Darwin, "to account for its success with the non-scientific public. Conclusions so wide and so novel and so easily understood,

drawn from the study of creatures so familiar, and treated with unabated vigour and freshness, may well have attracted many readers." And he quotes a reviewer as saying: "In the eyes of most men.....the earthworm is a mere blind, dumb, senseless, and unpleasantly slimy annelid. Mr. Darwin undertakes to rehabilitate its character, and the earthworm steps forth at once as an intelligent and beneficent personage, a worker of vast geological changes, a planer down of mountain sides..... a friend of man.....and an ally of the Society for the Preservation of Ancient Monuments."

XIV

HEALTH AND METHODS OF WORK

THE preceding chapters have shown an amount of work done by Darwin which would have been remarkable in a strong man, but which in a man who "for nearly forty years never knew one day of the health of ordinary men" is nothing less than amazing. Leaving evolutionary theory out of consideration, his achievements merited the highest scientific honours even in the eyes of his opponents, when opposition to the *Origin* ran highest. The breakdown in health began soon after his return to England in December, 1836. The three and a-half years that he continued in London after his marriage he describes as the period during which he did less scientific work, though he worked as hard as he possibly could, than during any other equal length of time in his life. Life in London becoming impossible, he migrated to Down in September, 1842—the last year in which he was able to go on a geological excursion.

The cause which originated his disabling headaches and sickness can hardly have been the frequent sea-sickness on the *Beagle*, for the amount of work he got through during the voyage shows that he was habitually in full vigour, and remained the untiring and athletic youth that he had set out. His power of endurance must have been exceptional, for on one shore excursion, "when all were suffering from want of water, he was one of the two who were better able than the rest to struggle on in search of it." The cause probably was a mysterious illness in South America, which affected every secretion of the

body. For nearly forty years, as has been said, he never knew one day of the health of ordinary men. Sometimes, for months at a time, the weary strain of sickness prevented him from working at all. In 1863–4 his "only approach to work" was "to look at tendrils and creepers" of the climbing plants on which he was experimenting. A little later he writes: "I am able most days to work for two or three hours, and this makes all the difference in my happiness."

Thus his ordinary days were carefully mapped out. Between a 7.45 breakfast, preceded by a short turn in the garden, and dinner at 7.30, three spells of work and one of letter-writing—even to the answering of "foolish and unscrupulous people" who insisted on bombarding him with letters—were sandwiched in between three separate hours of rest and two constitutionals round the "Sandwalk." In these hours of rest he was read to—novels chiefly, or other non-scientific books—in the afternoons smoking a cigarette the while, for he smoked little, his indulgence in tobacco running mostly to the exhilarating pinch of snuff. In the evenings came his regular games of backgammon with Mrs. Darwin, the reading, till he was tired, of some scientific book, and, finally, the refreshment of some music. Though he was in bed by 10.20, his nights were generally bad; he often lay awake for hours, wearied by the activity of his thoughts that went on wrestling with some problem on hand, or fretted by any vexation of the day—even, it may be, the venial sin of having left an intrusive letter unanswered.

At the best, then, he could manage between four and five hours of work at his own subjects—work done in each minute that could be snatched for it up to the very limit of his strength—so that he would suddenly stop in the midst of dictating, with the reluctant

admission, "I believe I mustn't do any more." It was work done with swift movements and care and patience, such as spelt speed because little or nothing had to be done over again. If a heavy book were on the stocks or proofs had to be corrected, tedium was relieved by some piece of experimental research and observation, profound probably in its novel results, but so personally delightful that he called it "idleness." If, however, he were really idle at times apart from his regular resting-hours, it was a sure sign that he was not well, and Mrs. Darwin would persuade him, however unwilling to lose a possible day of work, to take a short holiday of a week in town, staying with his married daughter or with his brother Erasmus, to whom he was devotedly attached—a man with all the Darwin charm, but held back by bad health from active life.

This undeviating routine of work, broken only by bouts of severe illness, became an essential part of the background of daily life to the Darwin children, especially during the eight years when his foremost task was the monograph on the Cirripedes or Barnacles; so much so that when Dr. Hooker came to stay at Down one of the children gravely asked him whether he had brought his Barnacles with him. The only consolation he could find for the yoke of ill-health was that it preserved him from the distractions of society and amusements.

In addition to his hatred of losing precious time, doubly precious to one so checked and curbed, various other points may be noted in regard to his way of working. First, his love of experiment. This was part of what Wallace, writing of him in 1873 (*Life*, III, p. 172), calls his "insatiable longing to discover the causes of the varied and complex phenomena presented by living things," and adds that in him "the restless curiosity of the child to know the 'what for?' the 'why?' and the

'how?' of everything" seems "never to have abated its force."

If he met with a long piece of deductive reasoning in a biological work, he always desired to see it tested by experiment; and of his own work he remarked: "I find that my mind is so fixed by the inductive method that I cannot appreciate deductive reasoning. I must begin with a good body of facts and not from a principle (in which I always suspect some fallacy), and then as much deduction as you please" (to John Fiske, December 8, 1874). As he remarks elsewhere: "I believe the cause to lie in the frequency with which I have found first-formed theories to be erroneous."

His persistence in refusing to be beaten by an experiment was noteworthy. "It's dogged as does it" was a favourite saying of his, and he stuck to his work with "his almost fierce desire to force the truth to reveal itself," albeit half-conscious of his strong inclination to go beyond the right point at which a man ought to give up an inquiry.

His habit of instantly jotting down every objection to a theory, every exception to what was regular or expected, has already been mentioned. This quality, which belongs to many discoverers, he possessed in especial strength. In his experiments even small exceptions were never passed over, but became the starting-point for further inquiry, often rewarded by new discoveries. Thus theory provoked observation, and accurate observation checked theory or supplemented it where incomplete with a new theory. Hence his remark (III, 215) that if a man is to be a first-rate observer "this implies, as I always think, a sound theorizer," which was a favourite aphorism of his (I, 149). To quote the passage that follows in the *Life*:—

> It was as though he were charged with theorizing power, ready to flow into any channel on the slightest disturbance; so that no fact, however small, could avoid releasing a stream of theory, and thus the fact became magnified into importance. In this way it happened that many untenable theories occurred to him; but, fortunately, his richness of imagination was equalled by his power of judging and condemning the thoughts that occurred to him. He was just to his theories, and did not condemn them unheard; and so it happened that he was willing to test what would seem to most people not at all worth testing. These rather wild trials he called "fool's experiments," and enjoyed extremely.

In dealing with his written material he was most methodical—a habit no doubt forcibly impressed upon him by the scantiness of the space at his disposal in his cramped quarters on board the *Beagle*. When he read a book he would mark and index all the passages bearing on his work, and finally make a rough abstract of the book from these passages under the chief subjects. These abstracts were then stored away in the proper portfolio, among the thirty or forty that he kept classified. Thus, with the help of the portfolios and the small indexes, from which a special list of relevant facts could be drawn, he was easily able to assemble the material for the subject on which he was about to write.

When he came to the writing of one of his large books he spent much time in first building up a skeleton of the whole. "I first make the rudest outline in two or three pages, and then a larger one in several pages, a few words or one word standing for a whole discussion or series of facts. Each one of these headings is again enlarged, and often transferred, before I begin to write *in extenso*." The plan was directed not so much to the building up of the argument, which apparently was

clearly shaped in his mind, as to the presentment of it and the marshalling of his facts.

He was not a facile writer, and, what with his intense desire for accuracy and his faculty for noting exceptions, he had a way, in talk as well as in writing, of introducing "the qualifying clause before it was clear what it was to qualify." Thus he says of himself: "There seems to be a sort of fatality in my mind leading me to put at first my statement or proposition in a wrong or awkward form. Formerly I used to think about my sentences before writing them down; but [in later] years I have found that it saves time to scribble in a vile hand whole pages as quickly as I possibly can, contracting half the words, and then correct deliberately. Sentences thus scribbled down are often better ones than I could have written deliberately" (*Life*, I, 99).

The last stage of putting a book into final shape and correcting it for press was very laborious, and it was at such times especially that he turned for relief from this drudgery to the refreshment of botanical observation. He found the labour "killing," and the book itself "intolerably dull," although he did not think so when writing it, and exclaims feelingly: "A naturalist's life would be a happy one if he had only to observe and not to write" (III, p. 65)—a sentiment repeated years after with no less fervour: "What a splendid pursuit Natural History would be if it were all observing and no writing" (III, p. 75).

Nor was he in other respects a good linguist. French, it is true, he read with ease, always admiring the lucidity of French writers. German he simply learned by hammering it out with a dictionary; and, though his only failures were with really difficult sentences, to make out the meaning of the many German books he read was a slow and toilsome process. Indeed, German

literary style is generally involved and clumsy, and so he used to call German the "verdammte." Dr. Hildebrand could write clearly; it was a grievance that others would not take trouble to do the same. He loved to tell of Hooker's witty retort to his boast in early days that he had begun German: "Ah, my dear fellow, that's nothing. I've begun it many times." However, he did not learn to speak the language, and his general knowledge of German was so slight that it became a standing joke when he once asked in perplexity: "Where is this place Wien, where they print so many books?"

It was this frequent intractability of his pen which provoked a humorous criticism from his intimate friend and supporter, after reading the *Origin* "for the *n*th time" preparatory to writing the Royal Society Obituary Notice: "Exposition was not Darwin's *forte*, and his English is sometimes wonderful. But there is a marvellous dumb sagacity about him, like that of a sort of miraculous dog, and he gets to the truth by ways as dark as those of the heathen Chinee."

Yet there was something more than simply intellectual power which prevailed over the unremitting obstacle of ill-health, the obscurity of the subject, and this intractability of his pen.

In a letter written just after Darwin's death, this same intimate friend, speaking of his intellectual equipment, instantly finds its driving power in the moral quality with which it was inseparably mated: "'Colossal' does not seem to me to be the right epithet for Darwin's intellect. He had a clear, rapid intelligence, a great memory, a vivid imagination; and what made his greatness was the strict subordination of all these to his love of truth."

As Wallace remarked in 1874: "If there is one thing more than another for which Mr. Darwin is pre-eminent

among modern scientific and literary men, it is for his perfect literary honesty, his self-abnegation in confessing himself wrong, and the eager haste with which he proclaims and even magnifies small errors in his works, for the most part discovered by himself."

XV

SOME PERSONAL CHARACTERISTICS

NOR was this love of truth confined to his scientific work. It permeated his private life and displayed itself in ways that would hardly have occurred to most men, who are not so punctilious in removing the veriest possibility of misinterpretation. Thus the Rev. J. Brodie Innes, for many years Vicar of Down, tells the following story:—

"Allied to the extreme carefulness of observation was his most remarkable truthfulness in all matters. On one occasion, when a parish meeting had been held on some disputed point of no importance, I was surprised by a visit from Mr. Darwin at night. He came to say that, thinking over the debate, though what he had said was quite accurate, he thought I might have drawn an erroneous conclusion, and he would not sleep till he had explained it. I believe that if on any day some certain fact had come to his knowledge which contradicted his most cherished theories, he would have placed the fact on record for publication before he slept."

Indeed, such action would have been quite in keeping with his usual habit, which has already been mentioned —namely, that whenever a difficulty or an objection occurred to him he made a point of noting it down instantly, because he found hostile facts so easily passed out of mind.

His simple and unaffected modesty in regard to his own achievements comes out very clearly in the words with which, in his old age, he thanked the workers and

observers in natural science, one group in Germany and another in Holland, who had sent him as a birthday gift in 1877 an Address with an album containing their photographs :—

> I am well aware that my books could never have been written, and would not have made any impression on the public mind, had not an immense amount of material been collected by a long series of admirable observers ; and it is to them that honour is chiefly due. I suppose that every worker at science occasionally feels depressed, and doubts whether what he has published has been worth the labour which it has cost him ; but for the few remaining years of my life, whenever I want cheering, I will look at the portraits of my distinguished co-workers in the field of science and remember their generous sympathy.

As for his profound and fruitful discoveries in the life of plants, made almost as a side-show during his other work, he rated them so simply that, when Sir Joseph Hooker dwelt upon them in his Presidential Address at the British Association in 1868, Darwin wrote: "I cannot get over my amazement at what you say about my botanical work." Nor is he less astonished to learn from a Hungarian geologist of the bearing of his own work on the methods of geology. "What a wonderful change in the future of geological chronology you indicate, by assuming the descent theory to be established and then taking the graduated changes of the same group of organisms as the true standard ! I never hoped to live to see such a step even proposed by any one!" (III, 234).

His sense of the debt he owed to Hooker has already been noted. Of his debt to Lyell he wrote, on the death of his old friend : "I never forget that almost everything which I have done in science I owe to the study of his great works."

This supreme love of truth was allied with no carping judgment of others or self-righteous exaltation, but with a warm and constant generosity of spirit. It has been seen how instant and unreserved was his impulse to waive his essential priority to Wallace in the matter of Natural Selection. His published correspondence bears abundant testimony to his good-humoured appreciation of a well-turned piece of criticism or even satire directed against himself, and to his conviction of the entire honesty of the most severe or the most uncomprehending among his scientific critics, save only in three cases, where the critics were obviously disingenuous. It was no more than justice when, in a review of the book on Variation, G. H. Lewes spoke of "the rare and noble calmness with which he expounds his own views, undisturbed by the heats of polemical agitation which those views have excited, and persistently refusing to retort on his antagonists by ridicule, by indignation, or by contempt. Considering the amount of vituperation and insinuation which has come from the other side, this forbearance is supremely dignified." And further: "Nowhere has the author a word that could wound the most sensitive self-love of an antagonist; nowhere does he, in text or note, expose the fallacies and mistakes of brother investigators.....but, while abstaining from impertinent censure, he is lavish in acknowledging the smallest debt he may owe, and his book will make many men happy."

Nothing was more alien to his nature than unkindness, and nothing would make him fire up more than any display of unkindness towards animals. And if he conceived that he himself had been betrayed into an unkind act, he could not rest till he had made amends for it.

His eldest son tells the following characteristic story of him, in 1866, when the whole country was bitterly

divided over the prosecution of Governor Eyre for his high-handed suppression of a negro disturbance in Jamaica: "With respect to Governor Eyre's conduct in Jamaica, he felt strongly that J. S. Mill was right in prosecuting him. I remember one evening, at my uncle's, we were talking on the subject, and, as I happened to think it was too strong a measure to prosecute Governor Eyre for murder, I made some foolish remark about the prosecutors spending the surplus of the fund on a dinner. My father turned on me almost in a fury, and told me, if these were my feelings, I had better go back to Southampton—the inhabitants having given a dinner to Governor Eyre on his landing, but with which I had had nothing to do. Next morning, at seven o'clock or so, he came into my bedroom and sat on my bed, and said that he had not been able to sleep from the thought that he had been so angry with me, and after a few more kind words he left me."

Darwin's magnanimous patience with critics who were soon to find out that he had gone deeper into the subject than they had themselves has already been noticed. It it interesting to see the same point made by the late Lord Farrer, who tells how in 1869 he corresponded with Darwin, making a particular suggestion about the fertilization of the Passion-flower. "What interested me," he writes, "was to see that on this as on almost any other point of detailed observation Mr. Darwin could always say: 'Yes; but at one time I made some observations myself on this particular point, and I think you will find, etc., etc.' That he should after years of interval remember that he had noticed the peculiar structure to which I was referring in the *Passiflora princeps* struck me at the time as very remarkable."

While his orderly mind delighted in careful business habits, he was extremely generous in money matters.

After receiving the Bressa prize from the Royal Academy of Turin, he gave £100 for a needed piece of apparatus to the Zoological Station at Naples. He provided for the translation into English of costly foreign scientific works. When in 1880 he heard that his correspondent Fritz Müller had barely escaped with his life from a flood in Brazil, where he was on a scientific expedition, he instantly wrote to Müller's brother Hermann offering to make good any loss of books or instruments "for the sake of science." When his children grew up, he had a pleasant way of distributing his surplus income among them at the year's end; and, indeed, he was so little greedy of this world's goods that he urged an admirer who had made a will in his favour to leave his fortune to another man of science who was less well off.

As a final benefaction to science, marking his especial gratitude to Kew, he provided for what he had himself long desired, the vast *Index Kewensis*, with its 375,000 entries cataloguing the various names given to all the known genera and species of plants with their habitats.

To the humble lover of science struggling to make his professional way, to an unrewarded colleague or a sick friend, his generous sympathy made his friend's judgment no empty praise: "Darwin is in all things noble and generous—one of those people who think it a privilege to let him help."

Let me mention his substantial aid in getting a botanical post in India for John Scott, a valued observer, whose enthusiasm for natural history had brought him to the Botanical Gardens at Edinburgh as a working gardener; his successful initiative in stirring Mr. Gladstone to confer a Civil List pension on Wallace; his exquisite tact and affection as of an elder brother in conveying to his friend Huxley, a man of proud and sensitive spirit, prostrated by overwork, the gift of his

friends that was to bring him rest and renewed health. Of the letter which Darwin wrote, it is difficult to say whether it does more honour to him who sent it or to him who received it. The proudest of men could not say "No" to such words from the oldest and most venerated of his friends. He could but exclaim: "What have I done to deserve this?"

Down, Beckenham, Kent,
April 23, 1873.

My dear Huxley,

I have been asked by some of your friends (eighteen in number) to inform you that they have placed, through Robarts, Lubbock, and Company, the sum of £2,100 to your account at your bankers. We have done this to enable you to get such complete rest as you may require for the re-establishment of your health, and in doing this we are convinced that we act in the public interest as well as in accordance with our most earnest desires. Let me assure you that we are all your warm personal friends, and that there is not a stranger or mere acquaintance among us. If you could have heard what was said, or could have read what was, as I believe, our inmost thoughts, you would know that we all feel towards you as we should to an honoured and much-loved brother. I am sure that you will return this feeling, and will therefore be glad to give us the opportunity of aiding you in some degree, as this will be a happiness to us to the last day of our lives. Let me add that our plan occurred to several of your friends at nearly the same time, and quite independently of one another.

My dear Huxley,
Your affectionate friend,
CHARLES DARWIN.

It was Madame Sismondi (Fanny Allen), Mrs. Darwin's aunt, herself a woman of remarkable charm and personality, who once described Charles Darwin

as "fresh and sparkling as the purest water"—a poetic description with the illuminating power of poetic insight. It sums up the primary impression of personal intercourse with him better than a whole page of analysis. And while Darwin's friends could not but uphold a thinker, whether he were a friend or not, whose views they found so true and so fruitful, affection for a man of such utter sincerity, such selfless respect for truth, and warm personality, led them, when his views were stupidly or maliciously attacked, to take more trouble in his defence and support, and to strike out much harder at his adversary, than they would otherwise have done. At the outset of his career he had been warned by Lyell against getting involved in controversy, with all its waste of time and temper—advice which, with only one or two exceptions, he studiously followed. But, indeed, his other friends were well assured that the scanty time which his health allowed for work was far too precious to be wasted in controversy; for his own sake and for the sake of the calm atmosphere in which a great theory should be worked out, they thought that the battling on a lower plane should be left to them. His "general agent," as he humorously called his active friend Huxley, declared: "You ought to be like one of the blessed gods of Elysium, and let the inferior deities do battle with the infernal powers." And again: "If I say a savage thing, it is only 'pretty Fanny's way'; but if you do, it is not likely to be forgotten." Hence a dash of personal pleasure was infused into the duty of upholding and defending the bringer of new light.

To his visitors in general he was a singularly attractive host. Their presence stimulated him, and under this stimulus he appeared to his best advantage, full of light-hearted humour and fun as well as serious matter, yet

with a total absence of pose or pontification, even if he often had most of the talk to himself. "It was this happy absence of pose, and the natural and simple way in which he began talking to his guests so as to get them on their own likes, which made him so charming a host to a stranger. His happy choice of matter for talk seemed to flow out of his sympathetic nature and humble, vivid interest in other people's work." Unlike a certain great talker of the past, omniscience was neither his *forte* nor his foible; on the contrary, he was so genuinely modest as to the range of his own knowledge, vast as it was, that he would almost embarrass a younger man to whom he would ascribe the knowledge he lacked himself.

All his guests, whether old friends who came for the week-end, or relatives who stayed for longer periods, or foreigners and other strangers who came down for lunch and went away in the afternoon, felt his charm as a host and the warmth of his welcome. "It was pleasant," writes Sir F. Darwin, "to see the way in which he shook hands with a guest who was being welcomed for the first time; his hand used to shoot out in a way that gave one the feeling that it was hastening to meet the guest's hands. With old friends his hand came down with a hearty swing into the other hand in a way I always had satisfaction in seeing. His good-bye was chiefly characterized by the pleasant way in which he thanked his guests, as he stood at the door, for having come to see him"—all the more because the journey to Down was, in his invalid's experience, something not to be undertaken lightly. The native warmth that kindled into hot indignation over anything cruel or mean came out in ordinary intercourse as eager emphasis of speech and a tincture of light-hearted gaiety which suffused his constant courtesy.

Thus, over and beyond the upwelling cordiality of his welcome that charmed the strangers who were his guests, it is not difficult to conceive how fully he was able to win and to keep the warm affection of his intimate friends, whether scientific or not. As Nelson among his captains, so was Darwin among the men to whose work and thought he had given a new illumination and a new direction. Not admiration only, but affection, gave him something more than followers—gave him a band of brothers.

Darwin is often held up as a "horrid example" of science causing the atrophy of the artistic sense. True it is that his early love of literature, prose, and poetry, his love of pictures and his emotional "thrill" in listening to music, faded away after his thirtieth year; that the man who carried Milton with him through the wilderness came to find Shakespeare intolerable, and even his sublime delight in fine scenery waning. And he deeply regretted this loss of æsthetic sensibility. But to ascribe it to the devastating effect of science on the feelings, as though its dry light left no scope for the warmth of emotion, or as if it were a kind of moral quicklime, desiccating away the flesh and blood of the finer sensibilities, is not true. The ultimate cause lay in the oppression of his forty years' ill-health. As he remarks in one connection after another, nothing availed to make him forget his daily discomfort except the joy and excitement of his scientific work. And when he was actually ill, he was sustained and kept going by this interest. As he writes when on the sick-list in 1869: "It seems, as soon as the stimulus of mental work stops, my whole strength gives way" (III, p. 106). Thus the essential bent of his mind became all-predominant; his other faculties fell into the background. In this he differed from his chief scientific friends, whose æsthetic

faculties continued to have full play; but it is noteworthy that the good people who pillory this one "horrid example" ignore the numerous instances to the contrary in Darwin's own circle.

It is worth recording these instructive passages in the *Life and Letters*. They are not, I think, collected elsewhere:—

> My chief enjoyment and sole employment throughout life has been scientific work, and the excitement from such work makes me for the time forget, or drives quite away, my daily discomfort (I, p. 79).

The following is the *locus classicus* for the change in his mind:—

> This curious and lamentable loss of the higher æsthetic taste is all the odder, as books on history, biographies, and travels (independently of any scientific facts which they may contain) and essays on all sorts of subjects interest me as much as ever they did. My mind seems to have become a kind of machine for grinding general laws out of large collections of facts; but why this should have caused the atrophy of that part of the brain alone on which the higher tastes depend I cannot conceive (I, pp. 100–1).

Clearly Darwin did not, when penning this passage with its frank account of his own deficiency, think of accounting for it by the disuse he mentions elsewhere:—

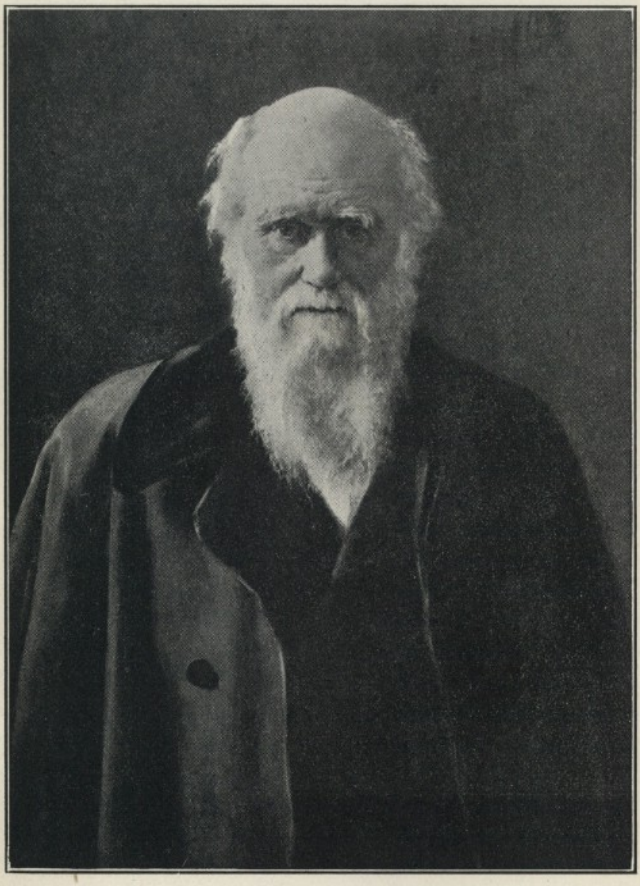
> It is really a great evil that from habit I have pleasure in hardly anything except Natural History, for nothing else makes me forget my ever-recurrent uncomfortable sensations (III, p. 75).
>
> It is a horrid bore to feel, as I constantly do, that I am a withered leaf for every subject except Science. It sometimes makes me hate Science, though God knows I ought to be thankful for such a perennial interest, which makes me forget for some hours every day my accursed stomach (III, p. 92).

XVI

HOME LIFE

PERHAPS it may be permitted to one of the (alas!) diminishing band who, though but in boyhood, spoke to Charles Darwin, who knew him in his habit as he lived, who felt in direct contact with his personality what others may perceive only as in a mirror, to recall from the past the sense of benignance which lay about his greatness, and, for those near him, despite his physical ills, consistently furnished "the warm precincts of the cheerful day" to dwell in with serenity. It was felt assuredly by the child mind that had not grown to capacity of appraising or analysing its nature. Memory leaps back half a century to certain weeks of spring merging into summer when a whole tribe of us were transplanted from London to the country delights of the old house at Down. The air of peace and happy interest still seems to issue from the wise and kind personalities of our host and hostess, pervasive but never invasive, as hosts in excess of hospitable zeal may sometimes be towards children. I can but hope that we in return did not spill ourselves too riotously outside our appointed quarters into the corner of the house sacred to quiet and study.

At breakfast-time each morning Darwin would come in to greet us—tall, white-bearded, impressive, his kind blue eyes beaming on us from under the penthouse of his brows—the incarnation of Socratic benevolence; and he would pat the curliest-headed youngster on the head and playfully bid him "make himself at home and take large



CHARLES DARWIN (1881)

Facing p. 110

mouthfuls." Before lunch and in the afternoon the same tall and venerable figure would emerge, clad this time in wide cloak and soft felt hat, for the appointed constitutional of so many turns round the "Sandwalk," his white terrier Polly trotting at his heels. Were we playing at Red Indians in the plantation, armed with javelins of hazel from the gardener's store of bavins behind the pigeon-house, or, maybe, roasting potatoes in the embers of a real gipsy fire under the responsible instruction of one of the sons of the house: the old man would always have a cheery word for us.

Serenity, again, was the note of the evenings after dinner, then and on later visits, when a half-hour was devoted to the invariable match of backgammon between Mr. and Mrs. Darwin. I can see them now sitting over the board near a bright lamp. The dice are rattled, the box descends with a rap: Darwin playfully gloats over a good throw, or bewails a cruel hazard that leaves him with a "blot" exposed to the enemy. The regulation games played, out came the little book wherein was entered the record of wins and losses, for curious examination at the year's end.

Indeed, the whole household ran with a quiet flow, smoothed by the genial air of personal courtesy and loyal interest. Among the faithful servants memory calls up the round figure, white-haired and apple-faced, of Parslow the butler—a veritable pillar of the house, without whom one could hardly picture the place. The atmosphere of serenity, thus felt even by a child visitor, sprang essentially from the sympathetic blending of two fine characters. In 1842 Darwin had been driven from London by ill-health, seemingly doomed to fail of accomplishing the high success promised by his early work and the pure fervour of his work for science. In his half-exile the tender care and thoughtfulness of

Mrs. Darwin, arranging her own quiet days to sustain him and carry out an indispensable routine, made his life not only possible, but happy and fruitful in its lamed energies. And he knew it. The bond between them grew ever more exquisitely close and responsive, while their sympathies were renewed and repeated in their own family and among their friends. In the second volume of *Emma Darwin* there are preserved for reverent eyes certain letters that passed between husband and wife, which bring out both her high solicitude for his spiritual happiness and his utter reliance upon her sustaining power of hand and heart. "In her presence he found his happiness; and through her his life, which might have been overshadowed by gloom, became one of content and quiet gladness."

Darwin's home life was rich in the warm and happy relation in which he stood to his family, whether as children or in their later life. His tender devotion to his own children comes out most perfectly in the exquisite sketch of his ten-year-old daughter Annie, his pet companion, written a few days after her death (I, 132).

To them all he was the most delightful playfellow and story-teller, the most perfect sympathizer, unbounded in patience when necessity overcame their knowledge that it was wrong to go in during work-hours, and even suffering them to make raids into the study when they "had an absolute need of sticking-plaster, string, pins, scissors, stamps, foot-rule, or hammer"—things to be found of a certainty in that orderly study.

One of the prettiest illustrations of the terms on which he stood with the children is the story of how one of his sons, when about four years old, tried to bribe him with sixpence to come and play in working hours. Working hours were sacred, "but that any one should resist sixpence seemed an impossibility." Or, again, the story

of how once he came into the drawing-room and found his son Leonard dancing about on the sofa—a thing forbidden for the sake of the springs. Exclaiming, "Oh, Lenny, Lenny, that's against all rules," he received for answer: "Then I think you'd better go out of the room."

"I do not believe," writes Sir F. Darwin, "he ever spoke an angry word to any of his children in his life; but I am certain it never entered our heads to disobey him."

In another passage he writes: "The *Expression of the Emotions* shows how closely he watched his children; it was characteristic of him that (I have heard him tell), although he was so anxious to observe accurately the expression of a crying child, his sympathy with the grief spoiled his observation. His note-book, in which are recorded sayings of his young children, shows his pleasure in them. He seems to retain a sort of regretful memory of the childhoods which had faded away, and thus he wrote in his *Recollections*: 'When you were very young it was my delight to play with you all, and I think with a sigh that such days can never return.'"

His affectionate manner towards them and his deep interest in all their plans and doings were unceasing. During the illness of one of his boys, who was a keen stamp collector, he was at pains to cheer him by asking friends for any out-of-the-way stamps that might come to them. I have seen a charming letter he wrote to his earliest publishers, Smith and Elder, begging, if it were not impossibly troublesome, that he might be enabled to procure for the boy some of the special stamps which he had heard were used by them in their Indian traffic. He even roused himself to detailed concern over pets of theirs, such as cats, for which he had no special taste, as he had for dogs. Yet with all this active interest he never invaded their sense of

individual freedom. At the same time, he was too great and too simple of character for such intimacy to weaken respect or obedience. The children felt that he always put his whole mind into answering any of their questions, and with his utmost sincerity and modesty of statement it came to pass that whatever he said was absolute truth and law to them. As his daughter wrote: "He always made us feel that we were each of us creatures whose opinions and thoughts were valuable to him, so that whatever there was best in us came out in the sunshine of his presence." Nor did he fail to appreciate their powers—too highly, they often felt, in the warmth of his sympathy. Yet it was sympathy that made them not puffed up, but humble and grateful before the radiant simplicity of his greatness. He was happy in his children and the great gifts, personal and intellectual, which they inherited and developed. They readily gave help in his experiments, were secretaries in his correspondence and literary work—functions which came to centre in the son whose career led him to botany; while Mrs. Darwin and the unmarried daughter were always at hand to read or to aid as amanuenses or literary critics on the domestic hearth—daily services which never failed to evoke an expression of gratitude.

The rare attendance at public functions, such as a meeting of a learned society or a scientific congress, was a severe exertion for which he paid heavily. The quiet home routine was varied only by visits to "cures," by summer holidays with the family, by brief visits away to his brother, and by the coming of visitors to Down, as already described. As for his summer holidays, unless he was suffering the reaction from overwork, the release from the close tension of every day left him ready to enter on them with an infectious youthfulness of enjoyment which brought him very close to his young folk.

When in London he was able to pay brief visits to his scientific friends, choosing the early morning, which was the only time when he could make an effort of the kind without suffering for it. I well remember his arrival now and then at my father's house, somewhere between half-past nine and ten, and the special preparations made for him by placing a hassock in the biggest armchair, for, with his long legs, he found sitting low uncomfortable. But half-an-hour was the limit of his stay; to talk longer meant a headache and nervous prostration; and in order to economize time he would have ready written on a slip of paper the particular questions he wished to discuss. Similarly, he limited scientific discussion with his visitors at Down. In the years when the *Origin* was in the making it was a great pleasure to get his close friend Sir Joseph Hooker to come for a long stay, bringing his work with him, so that they could have their half-hour's talk each day over the problems on which he was at work.

Mention has been made of Darwin's dog Polly. It was not only that he was fond of his dogs, but dogs had a curious instinctive devotion to him. The great affection between Polly and her master in his old age is described with sympathetic detail in the *Life*. Before Polly there was a large black-and-white half-bred retriever called Bob (or by the village folk, I believe, Bob Darwin), whom I can remember as a faithful son of the house. The *Life* tells, moreover, of Darwin's power as a young man of stealing away the affections of his sisters' pets, and at Cambridge of his cousin W. D. Fox's dog. Before he set out on the *Beagle* he had a surly dog, devoted to him, but unfriendly to every one else. When he returned after his five years' absence the dog remembered him, but in a curious way. "He went into the yard and shouted in his old manner; the dog rushed out and set

off with him on his walk, showing no more excitement or emotion than if the same thing had happened before."

With other animals, too, he must have had something of the same gift. Once when he was silently watching a family of squirrels the young ones ran up his back and legs, while the mother barked at them in an agony from the tree. He had a keen eye for rare birds, and to the end of his life sustained his reputation for a "special genius" in finding birds' nests; and this sympathy of the observer, it may be remarked, is commonly linked with an individual sympathy with the creatures observed. One more incident may illustrate this side of his character. As a boy he used to throw stones with remarkable accuracy of aim; in later life, never thinking that he had kept his old skill, he idly threw a stone at a crossbill, and killed it. He was so unhappy at this useless taking of life that it was years before he could mention it.

XVII

CONCLUSION

No one can look at the life and character of Charles Darwin without seeing in him an example of the fact that moral greatness and its most refined ideals which blend with the religious sphere can exist in the human heart independently of the particular doctrines which so often claim the sole parentage of morality. He was a man simple, compassionate, tender, generous, full of consideration for others, instinct with love; easily moved to righteous indignation by anything savouring of cruelty; free from worldly ambition or guile, bearing a life-long burden of pain patiently and indeed cheerfully; and, despite this intense burden, resolutely carrying on a giant's work. By one of the curious ironies of history, the man whose researches were to unsettle the foundations of contemporary orthodoxy went to Cambridge with a view of taking orders, since medicine, as set before him at Edinburgh, repelled him. Had this intention been fulfilled, we might have known another and a greater Gilbert White of Selborne. But his studies led him away from simple acceptance of the articles of the Church, and the project died a natural death. With reflection on the evidence adduced, with the gradual shaping of the evolutionary theory which, on the one hand, dissolved Paley's "argument from design" along with the conception of perfect beneficence in the creative power, and, on the other, reconciled teleology with morphology through the idea of selective

adaptation, he passed slowly, and without distress in the process of change, from orthodoxy and a belief in revelation to a vague Theism, finally reaching the conclusion "that the whole subject is beyond the scope of man's intellect; but man can do his duty." The circle of his personal life is an exemplar of high endeavour and beautiful practice, without the adventitious aid of the unproved, and often indeed the improbable. And if his labours did not move in the half-hypnotic lights of the ordinary religious imagination, their path was towards that fullness of humanity in truth and action upon which this imagination moulds the ideals it would realize.

In the full two-and-twenty years that passed between the publication of the *Origin* and Darwin's death the world had come to recognize the greatness of his work and had assimilated his quickening thought. The official —the national—seal of his recognition is his grave in Westminster Abbey, near that other builder of the new way, his friend and guide Lyell.

Yet for us, remembering that the simple attainment of truth was his main end, and not merely the attainment of truth as he saw it, but as his own vision might be bettered by further research, there can be no more fitting epitaph with which to round off this sketch than those inspiring words that concluded the speech of his stedfast friend at the unveiling of the Darwin statue in the Natural History Museum at South Kensington, requesting the trustees to accept the statue :—

> We do not make this request for the mere sake of perpetuating a memory; for so long as men occupy themselves with the pursuit of truth the name of Darwin runs no more risk of oblivion than does that of Copernicus or that of Harvey.
>
> Nor, most assuredly, do we ask you to preserve

the statue in its cynosural position in this entrance hall of our National Museum of Natural History as evidence that Mr. Darwin's views have received your official sanction ; for science does not recognize such sanctions, and commits suicide when it adopts a creed.

No; we beg you to cherish this memorial as a symbol by which, as generation after generation of students enter yonder door, they shall be reminded of the ideal according to which they must shape their lives if they would turn to the best account the opportunities offered by the great institution under your charge.

The movement which issued from Darwin's work has swept away much that hampered or distorted human development. If at the same time it swept away some things which seemed to make life worth living in its own despite, it has given a solid base from which to proceed anew. Not least, it has furnished fine types of character. One of its finest assets is the spirit in which the work was done. The achievement was great because the man was yet greater. The work is built deep into the foundations of the future ; the worker stands out as an example of the ideal by which his successors also must shape their work and life. Therefore it is that praise of his intellectual achievement is not enough, but a warm and stirring personal note must always mingle with the commemoration of Charles Darwin.

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