SCIENCE AND SCIENTISTS IN THE
NINETEENTH CENTURY

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BY THE REV.
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WITH AN INTRODUCTION BY
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LONDON
THE SHELDON PRESS
NORTHUMBERLAND AVENUE, W.C.
NEW YORK AND TORONTO: THE MACMILLAN CO.
Printed in Great Britain

First published 1925

TO THE

LADY ARDILAUN

FOR WHOSE STIMULATING FRIENDSHIP

I CAN NEVER FEEL SUFFICIENTLY

GRATEFUL

PREFACE

FIVE-AND-TWENTY centuries have passed since the greatest of all Greek historians, Thucydides, wrote: " People do not distinguish; without a test they take things from one another: even on things of their own day, not dulled by time, Hellenes are apt to be all wrong. So little pains will most men take in search for truth: so much more readily they turn to what comes first."* The Greek applied these mournful words to history. It is the purpose of this book to apply them to science. The scientist should be a man willing to listen to every suggestion, to every hypothesis, but should also be determined to be the slave of neither suggestion nor hypothesis. With an open mind, uninfluenced by preconceived ideas, he sets out on his quest for truth inspired by the desire of ascertaining what Virgil deemed the fortunate lot of him who found out the causes of events in the world of matter, just as the historian seeks the causes of events in the world of affairs. In " Gott und Welt " Goethe launched a magnificent ideal:

Wide of world and broad in living,
Long years' single-hearted striving,
Ever seeking, fathoming ever,
Rounding oft, concluding never,
Oldest truth in fealty keeping,
Newest truth in gladness greeting,
Mind serene, and pure ambition:
Make good faring on life's mission.†

* Thucydides, i. 20: οὕτως ἀταλαίπωρος τοῖς πολλοῖς ἡ ζήτησις τῆς ἀληθείας, και ἐπὶ τὰ ἕτοιμα μᾶλλον τρέπονται.

† Weite Welt und breites Leben,
Langer Jahre redlich Streben,
Stets geforscht und stets gegründet,
Nie geschlossen, oft geründet,
Aeltestes bewahrt mit Treue,
Freundlich aufgefasstes Neue,
Heitern Sinn, und reine Zwecke:
Nun! man kommt wohl eine Strecke.

Such a scientist is one of the greatest benefactors of the human race, and to him I pay my tribute of sincerest respect for his patient observation and his persistent inquiry. If, in addition to these gifts, he possesses insight and imagination raised to the highest degree, we are fortunate to meet with a Newton in the past or a Poincaré in the present. The biography of a Faraday or a Pasteur is enough to show what years of labour a man will give when the love of knowledge and the joy of discovery take possession of him. The life devoted to the exploration of nature is one that commands my admiration increasingly, and no one is more conscious than myself of how many scientific men have aims and ideals as noble as any which stimulate human endeavour.

Science, in the old sense, meant knowledge, and this knowledge might wear many forms as well as that of the laboratory. Any investigator is such simply because he puts truth high above everything. Some scientists—they are not the greatest—seem to think that love of truth actuates a man in their ranks more than anyone else. If one reads such a tenth-rate book as J. W. Draper's *History of the Conflict between Religion and Science* or even such a book as A. D. White's *History of the Warfare of Science with Theology in Christendom,* one is conscious that both authors assume unquestioningly that the theologian is moved by prepossessions, whereas the man of science is moved by nothing else than the desire to ascertain the facts as they actually are. Would that it were so with all men of science! It might have occurred to these authors that the history of science bears no testimony to the accuracy of their assumption, and indeed one main purpose in writing this book has been to prove that there are just as many preconceived notions in science as there are in theology. These pages have been written in the hope that scientists will read them in order to detect the presence of hypotheses that are inflicting grave injury on the progress of their several departments. In a sense my book forms an assault upon science, or, to put it more correctly, upon the preconceptions that lie at its base far more than most F.R.S.s are aware. Take the story told of Herbert Spencer. He replied to an argument with the words, "That can't be true, for otherwise my *First Principles* would have to be re-written—*and the edition is stereotyped.*" Is it true that much that passes under the name of science is also stereotyped?

In logic two blacks do not make a white, but in life they sometimes do. Convince a man that he is acting upon hypothesis, not upon ascertained truth, and he is your debtor. I have enough faith in the candour of men of science to think that if—it is a big if—it is possible to convince them that there are every whit as many prepossessions in their departments as there are in theology, we shall hear less of the warfare between science and theology. For a similar warfare is characteristic of EVERY form of human knowledge. There are schools of science in, say, biology just as there are such schools in mathematics; e.g. some mathematicians will not allow the use of quaternions in any form, while to others they are indispensable. Discoverers are not simply discoverers: fundamentally they share the æsthetic temperament. The historian can only see truth, as it were, through the hundred facets of a cut diamond; and he sits patiently, mentally turning the diamond till he notes the gleam of which he is in search. Nor is the attitude of the scientist a whit different.

I have confined my attention to the nineteenth century, and in the careers of the men investigated I stop my account of them ten years after they effect their chief contribution to their particular corner of the domain of knowledge.* Had I gone to, say, the eighteenth century and studied Newton's career, I could have made my account a thousandfold stronger. In order to be quite fair, I determined to concentrate my attention on the nineteenth century.

Another matter calls for comment. There are many biographies of scientists in English, but there are few in German and fewer in French. For this reason I have been compelled to pay more attention to England than to Germany or France, simply because I have not the material for European scientists. In France there are, of course, the *éloges* of the French Academies. In Germany there are memoirs of Varnhagen von Ense and of Perthes; there are R. Haym's great biographies of Hegel, Wilhelm von Humboldt, and Herder; and there are Justi's *Winckelmann* and Dilthey's *Schleiermacher*. None of these, however, are really scien-

* Cf. p. 307.

tific in character. For that we have to fall back on the incomparable *Reden* which E. Du Bois Reymond and K. E. von Baer have bequeathed to us.

References to the utterances of the scientists under discussion are so desirable that in some cases I have employed a good many quotations. The following publishers generously allowed me to use extracts from books published by them, and I desire to thank them most cordially: E. Arnold & Co. (Lord Rayleigh, *Life of Lord Rayleigh*); the Clarendon Press (Sir R. J. Godlee, *Life of Lord Lister,* and L. Koenigsberger, *Hermann von Helmholtz*); Constable & Co. (O. Metchnikoff, *Life of Elie Metchnikoff,* and R. Vallery-Radot, *Life of Pasteur*); Longmans, Green & Co. (J. Tyndall, *Faraday as a Discoverer*); Macmillan & Co. (L. Huxley, *Life and Letters of T. H. Huxley,* T. H. Huxley, *Darwiniana,* and S. P. Thompson, *Life of Lord Kelvin*); John Murray (F. Darwin, *Life and Letters of Charles Darwin,* Mrs. Horner, *Life of Sir Charles Lyell,* and Sir Ronald Ross, *Memoirs*); the Council of the Royal Society (Lord Rayleigh's Presidential Address in 1907); and the S.P.C.K. (the Rev. R. H. Murray, *Erasmus and Luther: their Attitude to Toleration*).

Nothing remains save to express my gratitude to those who have helped me in what has necessarily been a long task. I desire to give my sincere thanks to the following, who have made valuable suggestions: The Earl of Balfour, Sir W. F. Barrett, Mr. Havelock Ellis, Sir R. J. Godlee, Lord Rayleigh, and Hon. Bertrand Russell. Alas! Sir W. F. Barrett and Sir R. J. Godlee, like some others, are beyond the reach of my acknowledgment. The Rev. Dr. Tennant most kindly went through the original book with me, and I owe much to his advice. Six other Cambridge Fellows were kind enough to give me the benefit of their accurate knowledge. The reader of my publishers and the Rev. W. K. Lowther Clarke have bestowed the utmost pains upon my manuscript. The Master of Christ's College, Cambridge (Sir A. E. Shipley), and the Master of Trinity College, Cambridge (Sir J. J. Thomson), read through my proofs and made many helpful suggestions. I feel specially grateful to Sir Joseph Larmor for his searching criticism of my proofs. Sir Oliver Lodge aided me in numberless ways, and I thank him most cordially for all his assistance and for the Introduction he has been good enough to write for me. Last of all, my wife has given me constant encouragement and constant assistance. Indeed it is a genuine pleasure to me to thank the many friends who have given me both counsel and criticism.

ROBERT H. MURRAY.

BROUGHTON RECTORY,
HUNTINGDON.

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INTRODUCTION

It is not often that men of science are looked at with the eye of an historian, and their merits and demerits parcelled out with an impartial hand. I am not sure that the operation is altogether pleasant. We like to be thought devotees of truth uninfluenced by prejudice, as open-minded and serene students of nature, free from presuppositions and welcoming every fact that comes within our ken. Yet, in the past, history has testified against us, and posterity has found it needful to mingle some condemnation with its praise. It is unlikely that the present generation is immune. Obituary notices and biographies naturally and properly aim at emphasising positive merits and are niggard of blame; but our letters and contemporary utterances, some of which we should like to forget, stand ready to condemn us in the eyes of a future generation. For each generation, though doubtless it has difficulties of its own, has been enlightened by the placid progress of knowledge, so that the strivings and jealousies and bigotries of the past seem perverse and difficult of comprehension.

We may find it difficult to realise that quite similar prejudices and bigotries are not extinct to-day. We are not historians, and sometimes we seem incapable of learning from the past. When the errors of our predecessors are forced upon our notice we may lament them or be amused at them or may seek to excuse them, but that the same lamentations and excuses may some day have to be made for us we can hardly think possible. Yet though our predecessors were doubtless as single-eyed for truth as we are, they stood forth as champions of scientific orthodoxy, they condemned heresies unheard even when supported by asserted facts, and they resisted novelties because they seemed incredible. Their patiently acquired knowledge had grown so great that if confronted with phenomena which were unpalatable or inadmissible, they felt certain that they could not be facts and might be safely rejected without examination.

In the past we see the supporters of new doctrines, the detectors of unwelcome facts, coming forward apologetically, humbly presenting their credentials, and we see them immediately snuffed out or else browbeaten and ridiculed by the High Priests of Science. Surely that sort of thing cannot happen to-day!

The suggestion that there may be a branch of inquiry which even now has to run the gauntlet of fierce denial and unbending hostility, and yet may be accepted by posterity as a matter of course, is humiliating. Truth may be trusted to prevail in the long run, but meanwhile the run is long, and many shrewd blows may be received by the unfortunate custodian of new truth. In theology the persecution of the heretic has always been bitter, though a later age may canonise the martyr. The world is full of prejudices and presuppositions, and the birth of truth is seldom accomplished without the pains of parturition.

Surely the world of science is free from prejudice and is ever ready to welcome truth! So we try to think. But is it so? Does history bear this out? Are we in better case than our colleagues in any other department of human activity? The Author of this book, surveying the unparalleled development of scientific knowledge in the nineteenth century as a theme for historical inquiry, implicitly answers this question, and answers it in the negative. Keenly admiring the work of the pioneer, the Author finds that he had to encounter in every case a lamentable hostility, a serious opposition; and then he sadly recognises that when this same pioneer has at length been received and honoured and exalted, in too many cases the quondam sufferer metes out the same treatment to the pioneer of the next generation.

Thus it can be claimed that science has no more open-mindedness than any other profession. It can also be urged that scientific method has no monopoly of the avenues of truth. A vivifying hypothesis may dawn on the mind intuitively, æsthetically, and in other ways; and verification may be forthcoming from common experience.

A claim like these must be confronted with the history of scientific discovery and with the records of all experience. Few there are who can take a wide enough survey of human knowledge even to enter on the discussion; but that is what the Author has attempted. The range of study exhibited in this book is remarkable. The Author must have been largely dependent on biographies and obituary notices, supplementing his own scientific knowledge. To extract from these rather sugary sources their essential essence can have been no easy task. An immense range of subjects is competently and ably dealt with, and the work of continental as well as of English-speaking discoverers is passed under review. The chapter on Helmholtz, too little known in this country, is specially useful, though even so the Author has hardly succeeded in the almost impossible task of conveying to the general reader an adequate conception of the exceeding brilliance and wide scope of the 1847 thesis of Helmholtz on the Conservation of Energy; still less is it possible to deal with that epoch-making paper on Vortex theory, which may be said to have initiated modern hydrodynamics. Mathematics, Biology, Geology, and Medicine are all included in the survey, and the chapter on forgotten scientists resuscitates the memory of many workers whose meritorious contributions were either rejected or but slightly appreciated by their contemporaries.

On the whole it may be said, without any exaggeration, that the Author has succeeded in producing an eminently readable book, full of personal reminiscences and biographical details about the great men of science of the nineteenth century, with their mixed strength and weakness; a book also which is a serviceable storehouse of reference for the coming generation of students in all branches of science, to whom those stirring times and the personalities who enriched and vivified them are fast becoming little better than a tradition. Judiciously selected extracts and anecdotes make it often amusing as well as interesting, and it may be safely commended to a wide circle of readers.

OLIVER LODGE.

CHAPTER I

JENNER AND VACCINATION

EDWARD JENNER (1749—1823) was born at Berkeley, Gloucestershire, of which place his father was vicar. His mother's name was Head, and her father had also been vicar of Berkeley. The celibacy of the clergy of the Church of Rome stands in need of defence. The marriage of the clergy of the Church of England stands in need of none, for the volumes of the *Dictionary of National Biography* attest how many men in the front rank have come from her rectories. On his father's death when he was five, the education of Edward Jenner fell into the hands of his eldest brother, Stephen. When eight years old he was sent to school at Wotton-under-Edge under a clergyman named Clissold. Next he was placed under the tuition of the Rev. Dr. Washbourn at Cirencester. There he acquired a respectable knowledge of the classics, and he also acquired—what was no less important—some of those friends who bulked largely in his life. The boy quite commonly is not the father of the man. He was, however, in Jenner's case. Cirencester is a neighbourhood abounding in fossils, and the lad of nine collected them, as he also collected the nests of the dormouse.

Jenner's profession was to be medicine, and accordingly he was apprenticed to Daniel Ludlow of Sodbury, a surgeon of note. At the end of his apprenticeship he was fortunate enough in 1770 to become a resident pupil of John Hunter, who was to become the head of the surgical profession. As an investigator, as a stimulator of thought, and as an original thinker Hunter left an exceedingly deep impression on his contemporaries. With a profound ignorance of books he possessed an intimacy with facts. Hobbes was of the opinion that if he had read as much as other people, he would know as little as they, and this was an opinion that Hunter cordially shared. His pupils included John Abernethy, Anthony Carlisle, Thomas Chevalier, Henry Cline, Astley Cooper, Everard Home, and James Macartney. Distinguished as these men became, Jenner easily held his own with them, and Hunter regarded him as a favourite pupil. When the two met, the master was in his forty-second year and the student in his twenty-first. The first time a young man comes into contact with a genius of a commanding order marks a distinct epoch in his mind. So it proved to young Jenner, who met daily a penetrating thinker devoted to the mastery of facts. For Hunter loved science with all the passion of a devotee. He ardently pursued truth at all costs, and succeeded in communicating this ardour to his pupil. The two years Jenner spent under Hunter's roof left an imperishable impression on him. Both men were direct and straightforward in conduct, and both possessed an unquenchable desire for knowledge. The relation of master to pupil was replaced by that of friend to friend, and the correspondence between them ranged over the wide circle of Hunter's researches, lasting till within a short period of Hunter's death in 1793. Jenner attached a great value to these letters, carefully preserving them in a cover inscribed in his own handwriting, "Letters from Mr. Hunter to E. Jenner."

Already the brain of Jenner was actively at work, and even so early as 1770 he mentioned to his teacher facts bearing on cow-pox. Hunter possessed the quality, rare in a professor, of never damping the ardour of an investigator by suggesting doubts or difficulties. His practice was to extend encouragement, to urge that the matter in hand ought to be brought to the test of experiment, and to advise absolute accuracy and faithfulness in the procedure adopted. In cases of this kind he would say, "Don't think, but try; be patient, be accurate." Hunter infected Jenner with this Newtonian dislike of hypotheses. William Clift describes Hunter as "standing for hours, motionless as a statue, except that, with a pair of forceps in each hand, he was picking asunder the connecting fibres of some structure he was studying," and Jenner too proved capable of absorption for hours in thought.

In 1771, when Jenner was living with Hunter, Captain

Cook returned from his first voyage of discovery. Valuable specimens of natural history had been collected by Sir Joseph Banks, and Hunter recommended his favourite pupil for their arrangement and preparation. Jenner evinced so much dexterity and knowledge in this task that he was offered the appointment of naturalist in the next expedition, which sailed in 1772. Such a temptation enticed Darwin away, but it failed to entice Jenner, whose heart was set on returning to his old home in the country when he had finished his studies at St. George's Hospital. At first sight it may seem as if the decision of a young surgeon to bury himself in a country village was unwise. In the light of his after-life the wisdom of his choice fully justified itself, for the existence of such an affection as cow-pox was known only in a few districts. Hunter required the metropolis for his ever-growing inquiries; his pupil, on the other hand, required a village for the range to which he restricted himself. The genius of men like Hunter and Jenner encourages investigation: the man of lesser type asks Lord Melbourne's famous question, Why can't you let things be? This is precisely what the man with an inquiring turn of mind cannot possibly do. Problems fascinated Jenner, though he quickly found that they in nowise fascinated his professional brethren. Repeatedly he tried to arouse their attention to them, and just as repeatedly he failed. He became a member of a society he called the Medico-Convivial, which met at the Fleece Inn, Rodborough, and of the Convivio-Medical, which met at Alveston, a village about ten miles from Bristol. Dr. Baron, the biographer of Jenner, was a medical man, and he testifies: "Dr. Jenner has frequently told me that at the meetings of this society (i.e. the Convivio-Medical) he was accustomed to bring forward the reported prophylactic virtues of cow-pox, and earnestly to recommend his medical friends to prosecute the inquiry. All his efforts were, however, ineffectual; his brethren were acquainted with the rumour, but they looked upon it as one of those vague notions from which no accurate or valuable information could be gathered, especially as most of them had met with cases in which those who were supposed to have had cow-pox had subsequently been affected with small-pox. These discouragements . . . did not suppress the ardour of Jenner's mind. He often recurred to the subject in these meetings; at length it became so distasteful to his companions, that I have many times heard him declare that they threatened to expel him if he continued to harass them with so unprofitable a subject." * Dr. Fewster of Thornbury was a member of the Convivio-Medical, and he persistently undervalued the efforts of Jenner even two years after Jenner was able to show, in 1796, that cow-pox is protective against small-pox. In a letter to Mr. Rolph, surgeon, of Peckham, dated October 11, 1798, Fewster still writes: "I think it (i.e. the cow-pox in the natural way) is a much more severe disease in general than the inoculated small-pox. I do not see any great advantage from inoculation from the cow-pox: inoculation for the small-pox seems to be well understood, so that there is very little need of a substitute. It is curious, however, and may lead to improvements." †

The maxim, "Youth will be served," is the maxim of the prize-ring, and yet sometimes it applies to science. Bacon surveyed his "Temporis Partus Maximus" and Newton unfolded his doctrine of light and colours before either of them had reached his twentieth birthday, and Jenner at the same age contemplated the removal of one of the direst scourges that afflicted the human race. He might have met with hope: he met with despair. He might have met with encouragement: he met with discouragement. Dr. Baron sums up the state of feeling of those medical men who had heard of the reported virtues of cow-pox: "We have all heard (they would observe) of what you mention, and we have even seen examples which certainly do give some sort of countenance to the notion to which you allude; but we have also known cases of a perfectly different nature,—many who were reported to have had the cow-pox, having subsequently caught the small-pox. The supposed prophylactic powers probably, therefore, depend upon some peculiarity in the constitution of the individual who has escaped the small-pox; and not on any efficacy of that disorder which they may have received from the cow. In short, the evidence is altogether so inconclusive and unsatisfactory that we put no value on it,

* J. Baron, *Life of Jenner,* I, p. 48.
† *Ibid.,* p. 48.

and cannot think that it will lead to anything but uncertainty and disappointment."* When Mohammed attained fame, he married many wives. One of them was young and beautiful, and she slighted Khadijah, the first and now elderly wife of the prophet, saying to him, "Am I not dearer to you than Khadijah?" "No, by God, you are not. For Khadijah believed in me when none else did." Not one of his brethren played this part to Jenner, and it is not easy to think of all that mankind might have lost had it not been for the indefatigable pursuit of truth that characterised him.

That John Hunter continued to afford stimulus to his old pupil is apparent from Drewry Ottley's *Life of John Hunter* and John Baron's *Life of Edward Jenner.* The letters passing between the master and the student render it obvious that each entertained a lively respect for the other. Critics like Dr. E. M. Crookshank and Dr. Creighton minimise the abilities or at least the originality of Jenner. It is not usual, however, for a genius like Hunter to write so often to an ordinary member of the medical profession. Some men in the front rank owe part of their position to a combination, at the same time, in their careers, of the advantages of youth and age. Some of them have old heads on young shoulders, and others have young heads on old shoulders. Either peculiarity confers a marked advantage on its owner. Jenner had an old head on his young shoulders. "I don't know any one," Hunter tells him in 1776, "I would as soon write to as you. I don't know anybody I am so much obliged to." On January 18, 1776, he writes: "I have but one order to send you, which is, to send everything you can get, either animal, vegetable, or mineral." The next letter speaks for itself, for on December 17, 1777, Hunter says: "I am always plaguing you with letters, but you are the only man I can apply to." The correspondence of the two men bears trace after trace of the stimulus given by Hunter to Jenner. He stirs up the country doctor to make observations on the temperature of animals and the problems suggested by eels. At Hunter's instigation Jenner investigated the migration of birds, with the result that he disproved the view that ascribes their disappearance to a state of hibernation; and he notably anticipated Darwin in exposing the action of earth-

* Baron, *Life of Jenner,* I, p. 125.

worms in rendering the earth readily fertile. Analogy was his favourite guide, and it misled him into opposition to the theory of population promulgated by the Rev. Thomas Malthus. The young physician generally carried a large pocket-book with him; and recorded his thoughts as they occurred. These thoughts naturally are now and then without any connection save that in the mind of the recorder. There is reason to believe that, where he could test his ideas by experiment, he was ready to do so. Once he was dining with a large number at Bath and the conversation turned on the question whether the temperature was highest in the centre of the flame of a candle or at some small distance from its apex. Various opinions were hazarded. Jenner at once settled the matter. He placed the candle before him, and, inserting his finger into the middle of the flame, he retained it there for a short time. He then placed it a little above the flame, but was compelled immediately to withdraw it. "There, gentlemen," he observed, "the question is settled."

Before the Medico-Convivial Jenner read papers on angina pectoris, ophthalmia, and valvular disease of the heart. Letters from Hunter continued to arrive, asking him to forward salmon-spawn, porpoises, cuckoos, and fossils. The young man's mind turned to science. He was disappointed in love, and wrote to tell Hunter of his disappointment. Just as Richard Cobden urged John Bright when he lost his wife to throw himself into the abolition of the corn-laws, similarly Hunter wrote on September 25, 1778: "Let her go, never mind her. I shall employ you with hedgehogs." The parties given by his wife Anne may have induced the great anatomist to take this view of matrimony. It was not till March 6, 1788, that his friend married Catharine Kingscote, and during the intervening ten years Hunter's questions combined with his own provided him with sufficient mental occupation. For several years to come Hunter sent him questions on the problem of the winter-sleep of the hedgehog, the autumnal storing of fat, the consumption of it during the winter, and the like. In 1787 Jenner wrote a paper on the "Natural History of the Cuckoo," publishing it in the following year in the *Philosophical Transactions* of the Royal Society. It sets forth the habits of the cuckoo at some length, observing the contents of the stomach in the young bird, the small size of the egg, the number of eggs in the oviduct of the cuckoo, the fierce behaviour of the young cuckoo when inspected in its nest, and the hedge-sparrow's, or other foster-parent's, habit of ejecting its own eggs from the nest after the cuckoo has deposited hers. Waterton, in his "Essay on the Jay," has demonstrated that this last statement is absurd. It seems that Jenner asked his nephew to conduct some of the observations, and he, after the manner of boys, was too indolent to furnish a correct report. In 1788, in spite of this mistake, Jenner was elected a Fellow of the Royal Society, before he was forty years of age.

As practice comes before theory, it is not surprising to learn that it is to a Greek slave we owe the art of inoculation, to an African the value of quassia, to the Jesuits the Peruvian bark, to the barbers the bold use of mercury, and to the remark of a dairy-maid one of the causes that turned the attention of Jenner to the cure of small-pox. So far back as the days when he was pursuing his professional education with Daniel Ludlow of Sodbury, a country girl came to consult him. On mentioning small-pox to her, she immediately observed: "I cannot take that disease, for I have had cow-pox." This subject was in his thought, and of course this incident fastened it on his memory. After his recovery from typhus fever in 1794 he continued his investigations into the protective power of cow-pox against small-pox. The dairy-maid had discovered this empirically. What were the causes underlying this empiricism? Jenner's country life led him to observe his horses with care, and he came to the conclusion that grease, a disease of the feet in horses, and cow-pox were the same disease. This conclusion was erroneous. Still, the more he worked the more he felt certain that cow-pox acted as a preventive of small-pox.

How large a toll was taken by small-pox is clear from a few facts. Dr. Jurin examined the London bills of mortality for a period of forty-two years, and he calculated that one in fourteen, of all that were born, died of small-pox. Of persons of all ages taken ill of this disease, in the natural way, he showed that one in five or six died. Dr. Heberden reckoned that during the last thirty years of the eighteenth century the proportion of deaths due to it was 95 in 1,000. In 1777 and 1781 the deaths were 2,567 and 3,500 respectively. As the population of England was then under ten millions, it is easy to note how widespread was this disease. In Europe it was every whit as prevalent. The deaths in Sweden are given in the following table:

The Year	The Number
1779	15,000
1784	12,000
1800	12,000
1801	6,000
1822	11
1823	37

There was evidently urgent need for a skilled physician to stand between the living and the dead. Jenner made notes of a few cases of immunity from small-pox after cow-pox which he had encountered. In 1778 he inoculated with small-pox a Mrs. H., but the result was a failure. This he thought due to her having had cow-pox when very young. In 1782 Simon Nichols had cow-pox, and "some years afterwards" inoculation failed. In 1795 Jenner failed to inoculate Joseph Merret, who had had cow-pox in 1770. He did not shrink from trying experiments on his own family. In November 1789 he had inoculated his eldest son Edward, who was then about one year and a half old, and in March 1792 he inoculated him again.

One day Jenner was riding with Edward Gardner on the road between Gloucester and Bristol, near Newport, when the conversation of the two friends turned—as it had so often done before—on the natural history of cow-pox. The mortality returns were much in their mind, and as Jenner perceived the possibility of reducing them, he remarked: "Gardner, I have entrusted a most important matter to you, which I firmly believe will prove of essential benefit to the human race. I know you, and should not wish what I have stated to be brought into conversation; for should anything untoward turn up in my experiments I should be made, particularly by my medical brethren, the subject of ridicule—for I am the mark they all shoot at."

In May 1796 cow-pox occurred in a farm near Berkeley, and a dairy-maid, Sarah Nelmes, caught the disease. On May 14 matter was taken from a sore on her hand and inserted by means of two superficial incisions in the arm of James Phipps, a healthy boy about eight years old. This inoculation succeeded. The result was described as much the same as after inoculation with variolous matter, except that the usual efflorescence had more of "an erysipelatous look." The whole died away, leaving "scabs and subsequent eschars." May 14, 1796, used to be an annual festival in Berlin to commemorate the day on which Jenner performed this experiment. The day when Harvey discovered the circulation of the blood, the day when Newton discovered the law of gravitation, the day when Columbus discovered the New World—these are all memorable days. When John Keats first looked into Chapman's *Homer* he felt impelled to pen these words:

> Much have I travelled in the realms of gold,
> And many goodly states and kingdoms seen;
> Round many western islands have I been
> Which bards in fealty to Apollo hold.
> Oft of one wide expanse had I been told
> That deep-browed Homer ruled as his demesne:
> Yet did I never breathe its pure serene
> Till I heard Chapman speak out loud and bold:
> Then felt I like some watcher of the skies
> When a new planet swims into his ken;
> Or like stout Cortez when with eagle eyes
> He stared at the Pacific—and all his men
> Looked at each other with a wild surmise—
> Silent, upon a peak in Darien.

The arm of James Phipps brought to Jenner the same deep-seated satisfaction that the sight of the Pacific brought to Cortez. S. T. Coleridge once planned a poem on Jenner's discovery, but, like so many of his plans, it came to nothing. Of course Jenner could not rest satisfied with the case of Phipps, for, decisive as it was, it was only a single instance. He waited a year in order to add the cases of William Rodway, and of Sarah and Elizabeth Wynne. In 1796 or 1797 he sent his paper to a correspondent who was in the confidence of Sir Joseph Banks, President of the Royal Society. Jenner's paper on the habits of the cuckoo had already been published in the *Transactions* of this Society, and he entertained no doubt that a paper on so epoch-making a matter as inoculation by cow-pox would meet with a similar welcome. Informally his paper was circulated, and Sir Joseph Banks showed it to Lord Somerville, President of the Board of Agriculture. Sir Everard Home also looked through it. The perusal of the cases and experiments produced no conviction on the Council of the Royal Society. On the contrary Jenner received a friendly hint that, as he had gained some reputation by what we now know to be his incorrect paper on the cuckoo, it was inadvisable to present what we now know to be his correct paper, which would injure his established credit. In 1809 Jenner wrote: "I explained in conversation, as I said before, all that passed respecting my first paper on the cow-pox intended for the Royal Society. It was not with Sir Joseph, but with Home; he took the paper. It was shewn to the Council, and returned to me. This, I think, was in the year 1797, after the vaccination of one patient only; but even this was strong evidence, as it followed that of the numbers I had put to the test of the small-pox after casual vaccination." *

Jenner performed a few additional experiments, and in June 1797 he wrote: "I have shown a copy of my intended paper on the Cow Pox to our friend, Worthington, who has been pleased to express his approbation of it, and to recommend my publishing it as a pamphlet, instead of sending it to the Royal Society." Edward Gardner and Henry Hicks were often consulted about it, and the circle of the discoverer read it repeatedly. Woodville criticised the "grease" origin of small-pox. In spite of this criticism, Jenner persisted in adhering to his belief. In London in 1798 he enjoyed the opportunity of meeting his professional brethren, and he brought before them the subject that occupied his thoughts. To his infinite regret the whole time he was in the metropolis he was unable to procure a single person on whom he could exhibit the results of inoculation. Some of the virus he carried home with him, presenting it to Henry Cline, who successfully inoculated it into the hip of a patient by two punctures.

In June 1798 appeared a slim quarto of seventy-five pages, dedicated to Dr. C. H. Parry of Bath. Its title was "*An*

* J. Baron, *Life of Jenner*, II, p. 364.

Inquiry into the Cause and Effects of the Variolæ Vaccinæ, a Disease discovered in some parts of the Western Counties of England, particularly of Gloucestershire, and known by the name of the Cow-Pox."* This booklet contained a fuller account of his observations and conclusions. There are some coloured plates, and one gives the hand of Sarah Nelmes showing the vaccine pustules upon it. Twenty-three cases are described, and the author sums up that "the cow-pox protects the human constitution from the infection of small-pox." That this summing up is sound the experience of a century lends ready testimony. An Alfred Russel Wallace may mock at it, but the majority of civilised mankind is content to adopt it, knowing full well that life proves its value. Woodville had pointed out the objections to the "grease" theory. Jenner weighed them, but remained unconvinced. Accordingly as there are truth and untruth in his cuckoo paper, so there are truth and untruth in his *magnum opus.*

An Inquiry into the Cause and Effects of the Variolæ Vaccinæ met with a mixed reception. One lady, of no mean influence among its author's own townsfolk, met him soon after it appeared. She accosted him in true Gloucestershire dialect. "So, your book is out at last. Well, I can tell you that there be'ant a copy sold in our town; nor shan't neither, if I can help it." On another occasion as she heard of some rumours of failures in vaccination she came up to the doctor with keen eagerness, and said, "Shan't us have a general inoculation now?" That he possessed that saving quality, a perfect good-humour, is evident from the glee with which he used to relate these two anecdotes.

That there were failures arose from the fact that vaccinators were sometimes careless and that occasionally small-pox pustules were ignorantly used. A medical man called at the Small-pox Hospital in London in order to obtain from Mr. Wachsel, the apothecary, leave to charge some threads with vaccine virus, as he wished to distribute them to his medical correspondents. Mr. Wachsel chanced to be called out of his room. During his absence the doctor selected a patient, and was busily engaged in charging the threads. Mr. Wachsel observed on his return that he had fixed on a patient who had

* A facsimile edition was published in 1924 in London and Milan by H. K. Lewis and A. Lier respectively.

a general sprinkling of small-pox pustules, and inquired whether he intended to furnish his friends with the virus of small-pox as well as of cow-pox. He replied, "With the virus of cow-pox only." "Then, sir," said Mr. Wachsel, "you know not what you are doing. You are taking the virus of small-pox." The threads thus charged, had it not been for Mr. Wachsel's vigilance, would have been distributed as vaccine virus!

On March 23, 1799, Jenner saw Dr. Woodville in London, who informed him that in one of his cases the cow-pox had been communicated by effluvia, and the patient had it in the confluent way. In the same month, Woodville published his reports, in which he concluded that cow-pox manifested itself sometimes in an eruptive disease of great severity, for three or four cases out of five hundred had been in considerable danger, and one patient had died. Was there variolous matter which had crept into the constitution of the vaccine? This was the natural question that came into the mind of the discoverer. Bent on answering this question, he procured some lymph from the London dairies, and sent it to Mr. Marshall by his friend Mr. Tanner, who used it on 127 cases without any eruptions resulting. Jenner therefore concluded that in Woodville's cases the eruptions were due to variolation, and in this conclusion Drs. Woodville and Pearson afterwards concurred. If blunders were committed in the country of the discoverer, it was certain that they would be committed elsewhere. Accordingly when we examine the report that in Austrian Poland the vaccine inoculation was in a backward state, we find that the village matrons and barbers employed a very malignant kind of false cow-pox.

The opposition Jenner experienced is nothing new in the history of science. When Harvey announced his discovery of the circulation of the blood, he met with vigorous denunciation on the part of his professional brethren and loss of practice on the part of the public. When Jenner lived down opposition, he had to face men like Pearson in England and Rabaut in France, who claimed to have anticipated him. Dr. Baron philosophises on the hard fate of the distinguished inventor. "A fact which has been lying common and at waste, floating on the very surface of daily experience, is seized upon by some penetrating and inquisitive mind. Its relations to the different branches of human knowledge are examined and defined: it throws a light all around, and is a lamp to the feet of the inquirer, while he surveys other regions. Having thus explored a terra incognita, up starts one, and says,—Sir, you have not the whole merit of this discovery; I knew that such a land, as you have visited and explored, existed, for I saw it, but did not approach it. Another says,—I was actually cast away upon the coast; I noticed some things which you have described. I did not examine them minutely, but I remember, from your description that such things did exist, and I therefore am entitled to the merit and reward which you claim.

"A process similar to this marked the discussion regarding the origin of vaccination. The subject had been forced upon the attention of many individuals; but as far as they were concerned all the information relating to it might have remained in its original and unsatisfactory state. All the pretensions, therefore, of the men that became wise by the labours of Jenner, who achieved what they were unable to accomplish, instead of detracting from his fame ought to raise it still higher."* So thirty-three eminent physicians and forty surgeons of the metropolis felt, for they signed in 1799 a testimony to the value of vaccination. Dr. Erasmus Darwin, the famous author of *Zoonomia,* wrote to Jenner on February 24, 1802: "In a little time it may occur that the christening and vaccination of children may always be performed on the same day."

The day when vaccination would be warmly and universally welcomed was not yet to dawn. In a speech when proposing a vote of thanks to Pasteur at the International Congress, held in London in 1881, Sir James Paget, the eminent surgeon, said, in his silvery tones: "Jenner had to fight for the benefit of men's lives against a vehement opposition; to that for the benefit of cattle, which are human property, there is no such opposition. It is truly a fact that we may well remember; though it is a novelty to many in our profession, who have frequent opportunities for seeing how much more valuable a man feels his own property to be than his neighbour's health. . . . Property and healthy life may

* J. Baron, *Life of Jenner,* I, pp. 563-4.

soon be regarded as more nearly equivalent than they have been hitherto." This view did not hold good in 1881, and it certainly held less good in 1799.

The contest Jenner was forced to wage was mainly with men of his own profession. The most formidable of his antagonists was Dr. Ingenhousz of Vienna, who left honourable mention of his labours in vegetable physiology and electricity. In the autumn of 1798, being then in his seventieth year, he came to pay a visit to Lord Lansdowne at Bowood. When *An Inquiry into the Cause and Effects of the Variolæ Vaccinæ* was published, he read it, dissenting from its conclusions. Taking advantage of his residence in Wiltshire, he investigated the "extraordinary doctrine" of protection by cow-pox. Jenner confided in his friend Edward Gardner: "My friends must not desert me now. Brickbats and hostile weapons of every sort are flying thick around me; but with a very little aid, a few friendly opiates seasonably administered, they will do me no injury.

"Ingenhousz has declined my offer of receiving my letter in print—so that must be modelled anew. We must set off by impressing the idea that there will be no end to cavil and controversy until it be defined with precision what is, and what is not cow-pox." Ingenhousz found out from Mr. Alsop of Calne, Dr. Pulteney of Blandford, and Major-General Hastings that persons had contracted small-pox after cow-pox.

It was plain that there was need of further inquiry. Jenner replied frankly to Ingenhousz that his own observations had been few, and no doubt they needed the confirmation of other observers. As the opposition increased he published on April 5, 1799, his *Further Observations on the Variolæ Vaccinæ or Cow-pox,* in which he endeavours to meet the points raised against his ideas. He continued to work at his subject at Berkeley and at Cheltenham, and in 1800 he published *A Continuation of Facts and Observations relative to the Cow-pock.* He added two continuations of the same subject. The first was entitled *On the Origin of Vaccine Inoculation* (1801), and the second was *On the Varieties of the Vaccine Pustule occasioned by an Herpetic State of the Skin.*

Dr. Pearson informed Jenner that "you cannot imagine how fastidious the people are with regard to this business of the cow-pox. One says it is very filthy and nasty to derive it from the sore heel of horses! Another, O my God, we shall introduce the diseases of animals among us, and we have too many already of our own." It is easy to understand the impatience Jenner felt when he wrote on March 7, 1799, to Gardner: "I am beset on all sides with snarling fellows, and so ignorant withal that they know no more of the disease they write about than the animals which generate it. The last philippic that has appeared comes from Bristol, and is communicated by Dr. Sims of London. Sims gives comments on it in harsh and unjustifiable language. It is impossible for me, single-handed, to combat all my adversaries."

Dr. Benjamin Moseley, physician to Chelsea Hospital, thought fit in a treatise on sugar to turn aside to attack the artificial introduction of cow-pox. In Jamaica he had rendered valuable service in military operations by his skill as principal medical officer. He had published a standard work on tropical diseases and the climate of the West Indies that had reached three editions, and his versatility is clear from the fact that his treatise on coffee had reached five editions. On his return to London he gradually acquired a large practice among the upper classes in St. James's. On the appearance of *An Inquiry,* he spoke of it as a portent in the heavens, whose significance is not altogether clear. "Some pretend that a restive, greasy-heeled horse will kick down all the old gally-pots of Galen. . . . To preserve, as far as in me lies, the genesis of this desirable, this excelling distemper to posterity, I mention that it is said to originate in what is called the greasy-heel distemper in horses. . . . The virtues of this charming distemper are said to be an amulet against the small-pox. . . . In this cow-mania it is not enough for reason to concede that the cow-pox may lessen, for a time, the disposition in the habit to receive the infection of the small-pox; all cutaneous determinations, catarrhal fevers, and every disease of the lymphatics do the same. . . . The small-pox and the cow-pox are not analogous, but radically dissimilar. . . . Can any person say what may be the consequences of introducing the lues bovilla, a bestial humour, into the human frame after a long lapse of years? . . . The doctrine of engrafting distempers is not yet comprehended by the wisest men; and I wish to arrest the hurry of public credulity until the subject had undergone a deep, calm, dispassionate scrutiny; and to guard parents against suffering their children becoming victims to experiment."

The efforts of Dr. Moseley to deepen prejudice were seconded by Dr. William Rowley and Dr. Squirrell. They actually published prints representing the human visage in the act of transformation, and assuming that of a cow. There was a "Master Jowles, the cow-poxed, ox-cheeked young gentleman," and "Miss Mary Ann Lewis, the cow-poxed, the cow-managed young lady," exhibited in clever caricature by Dr. Rowley. Nor did he deem it unworthy to collect absurdities like this and to term his work "a solemn appeal, not to the passions of mankind, but to the reason and judgment of all who were capable of deep reflection." There was wisdom in his method, for the cartoon and the caricature are the most effective of weapons. The late G. W. Curtis used in *Harper's Weekly* to attack Tammany Hall as it was run by William Marcy Tweed. Tweed felt the sharpness of the weapon directed against him. He said once: "I don't care a straw for your newspaper articles: my constituents don't know how to read, but they can't help seeing them damned pictures."

The King's Reader in Physic at Cambridge, Sir Isaac Pennington, and John Birch, Surgeon Extraordinary to the Prince of Wales and Surgeon of St. Thomas's Hospital, opposed the new practice, and the latter published in 1806 a temperate survey of Jenner's arguments, which by no means consisted of sheer abuse. In Edinburgh the reception of the new remedy was decidedly tepid. Nor was the welcome warm in the United States, where some medical men, using small-pox pustules by mistake, spread the very disease they were trying to check. At home the Moseleys spread their satire. The Continent, however, was more eager to give Jenner a chance than England, though Ehrmann of Frankfort attempted to prove from quotations of the prophetical parts of Scripture and the writings of the Fathers that the vaccine was nothing less than Antichrist.

That curious woman, Lady Mary Wortley Montagu, had introduced inoculation into England, and accordingly we find that Lady Frances Morton, Lady Peyton, and Princess Louisa of Prussia followed the excellent example she had set. Lord Egremont (the patron of J. M. W. Turner), Lord Hervey, the Earl of Aylesbury, the Earl of Ossory, Lord Elgin, and the Earl of Berkeley were also favourably disposed to Jenner's idea. The Duke of York recommended vaccination in the army. On March 27, 1800, Queen Charlotte asked Jenner many questions relative to the progress of cow-pox.

For the most part the clergy showed themselves fully alive to the importance of the cure placed before them. Men like Mr. Holt, Rector of Finmere, near Buckingham, the Rev. Dr. Booker of Dudley, the Rev. T. A. Warren of Kensworth, near Dunstable, and the Rev. T. T. A. Reed of Leckhamstead, proved warm advocates of vaccination. The Rev. Dr. Booker did more than recommend the practice from the pulpit. He printed pamphlets arguing in its favour, and one of these he gave to everyone who brought a baby to be baptised, also distributing regularly about twenty a week. At home Bishop William Cleaver of Chester and abroad a Danish Bishop, Balles, extended their valuable support. The Rev. J. Plumptre of Hinxton, Cambridgeshire, and the Rev. Dr. Ramsden of Grundisburgh, Suffolk, preached before the University of Cambridge, seizing the occasion to eulogise Jenner. Plumptre employed at his own cost a medical man to vaccinate the poor, and at times took that office upon himself. He also printed and circulated largely songs and ballads calculated to impress the peasantry. For he was emphatically of the belief of Andrew Fletcher of Saltoun: "I knew a very nice man . . . that believed if a man were permitted to make all the ballads, he need not care who should make the laws of a nation."

The World War has rendered us all familiar with the conception that a battle may be won in the munition factory as well as at the front. It is no new idea. Arkwright and Cartwright did their share every whit as efficiently as Nelson and Wellington in defeating Napoleon. Nor can the labours of the discoverer of remedies against disease be ignored. What Sir Almroth Wright did in our generation, Edward Jenner did in his. The sailors of our fleet were vaccinated in 1801, when the medical officers presented a gold medal to

Jenner. On it Apollo presents a vaccinated sailor to Britannia, who holds a civic crown inscribed "Jenner," and the reverse bears an anchor with the names of the King and Earl Spencer, First Lord of the Admiralty. Ours is an age when the forces of internationalism are supposed to be gaining in strength, yet five of the episodes of the Napoleonic Wars may well set us thinking. As Sir Humphry Davy received a prize from the French Government while the war was raging for his invention of the safety-lamp, so Napoleon transcended his particularism in 1804 when he struck one of the most beautiful of his medals in order to show his estimate of the value of vaccination. The next year Jenner addressed to the Emperor a petition, begging the release of two of his friends, men of science and literature. Just as Napoleon was about to reject the proffered petition, Josephine uttered the name of Jenner. Her husband paused for an instant, and exclaimed, "Jenner! ah, we can refuse nothing to that man." In 1808, while the war had still seven years to rage, the National Institute of France elected him a corresponding member, and three years later the same distinguished body conferred upon him a still higher honour by placing him on the list of its foreign associates. Could any of these five episodes have occurred during the World War?

On June 2, 1802, our House of Commons proposed, on the motion of the Prime Minister, Addington, that ten thousand pounds be given to the discoverer for his splendid services, and this motion was carried. Parliament appointed a committee to report on Jenner's claims before the motion was proposed. Before its members Dr. Pearson endeavoured to show that the discovery was not Jenner's but merely a part of common knowledge, and in our day Dr. Crookshank and Dr. Creighton prefer similar charges. That a man's foes are those of his own household is common knowledge, and it was certainly so with the discoverer of vaccination. In 1814 some members of the College of Physicians of London felt that his name would do honour to a bead-roll on which were the names of Linacre, Caius, and Harvey. Oxford University had conferred upon him its honorary M.D. "No," held some of the fellows of the College of Physicians, "it is true that Dr. Jenner, coming from Oxford as he does, may, if he chooses, claim admission into our body, but he can only take his place with us after undergoing the usual examination."

It is the custom of the Roman Catholic Church, on the proposal to canonise a man distinguished for his holiness, to appoint an advocatus diaboli. His duties are largely perfunctory. Jenner felt the treatment to which he had been subjected. "I told Dr. Moseley that in his assertion against it [i.e. vaccination] he had acted the part of the devil's advocate at a canonisation, who was to say all the harm he could against the saint in order that his life might be thoroughly scrutinised, and his merits appear all the more conspicuous." Jenner erred. It was the rôle of the Moseleys, the Rowleys, and the Squirrells to scrutinise his life thoroughly in order that his demerits might appear the more conspicuous. The century that has followed his death exposes his merits. For the whole of Immunology, which has now become an independent science, arises from vaccination. The work of Pasteur, Lister, and Koch, and the whole of the modern therapeutical movement are based as naturally on Jenner as is astronomy on Copernicus.

CHAPTER II

SIMPSON AND CHLOROFORM

"EVERY Scottish man has a pedigree," says Sir Walter Scott in his autobiography. "It is a national prerogative as inalienable as his pride and his poverty." Accordingly James Young Simpson (1811—1870) has one, the ramifications of which his biographer, Dr. J. Duns, spends some space in detailing. His maternal grandfather, John Jarvey, farmer at Balbardie, near Bathgate, Linlithgowshire, claimed descent from a Huguenot family. John Jarvey had married Mary Cleland, whose mother was a Cleland of Auchinlee, the representative branch of the Clelands of Cleland. If Robert Burns is right in maintaining that an honest man is the noblest work of God, Simpson as well as possessing gentle blood on his mother's side had noble blood on his father's. His father was the village baker, who, we may feel assured, gave his customers excellent value. James Simpson was the seventh son borne by his mother, who during her short life gave her family that sound Puritan training which has done so much to make Scotland what it is. There is, as we all realise, an evil side to Puritanism, but no one can read the family record of the Simpsons without perceiving how it steadied them to face the responsibilities of life. Puritanism not only gave their mother that force of character which impressed all who met her, but it also gave her the quiet and loving heart that endeared her to many. If George Herbert is correct in thinking that "a good mother is worth a hundred schoolmasters," then the Simpsons were fortunate young people. She believed that *orare est laborare,* and, as her family knew well, regularly retired to pray. When Simpson was only nine, he lost her, and her death was a grievous loss. His brothers and his sister Mary were devoted to him. "My second mother," he wrote of his sister, "the only mother in later days I knew." If Mary was a mother to him, his eldest brother Sandy was a father. Of his Benjamin Sandy always thought, "I aye felt he would be great some day." Clearly this youngest brother, unlike many, invariably had honour in his own country and in his father's house.

In the early decades of the nineteenth century superstition flourished in Linlithgowshire. Simpson's grandfather Alexander, in order to bring to an end a murrain of cattle, interred a cow alive. His uncle Thomas bought a little farm, Gormyre, and enclosed a small triangular corner of one of the fields within a stone wall. This corner, which remained cut off, was called the "Gudeman's Croft." It was a species of tithe to the Spirit of Evil in the hope that the foul fiend would abstain from ever blighting or damaging the rest of the farm. Of course the laird of Gormyre gave Auld Clootie the most worthless piece of land on the whole farm.

Lessons proved easy to the lad and he was generally dux of his class at school. His master was Mr. MacArthur, and among the boys was John Reid, afterwards Professor of Anatomy at St. Andrews. Simpson loved knowledge and he loved facts on which to base his knowledge. When he became a man, it was remarked in his presence that "the Bible and Shakespeare are the best books in the world." Simpson made a characteristic addition: "The Bible and Shakespeare and Oliver and Boyd's Almanac! At least I know the Almanac would have been the greatest prize for me when a boy." This "lad o' pairts" entered Edinburgh University when he was only fourteen. "Very young, very young and very solitary, very poor and almost friendless," he said forty years later to his fellow-citizens, when receiving the freedom of the City of Edinburgh from its Lord Provost, Dr. William Chambers, "I came to settle in Edinburgh and fight amongst you a hard and uphill battle of life for bread, and name, and fame; and the fact that I stand here before you this day so far testifies that in that arduous struggle I have *won*." Yes, he had won, though nature by the shortness of the winner's life had exacted her price for the victory. On his arrival at Edinburgh he joined John Reid, and the two lads lodged with Mr. MacArthur, who had now taken his medical degree. When they had been a short time with him, Dr. MacArthur said to Simpson's brother Sandy, "I can now do with four hours' sleep, John Reid can do with six, but I have not been able to break in James yet." He did, however, break in James. He himself was a man of indefatigable energy who ever prophesied a brilliant place for his two former pupils: "If only they would work." When Reid and Simpson attained position he used to remark: "Yes, but *how* they worked!" MacArthur belongs to the class of man that does as an agent what he never could have done as a principal.

The life of Simpson at Edinburgh University was the life of a thrifty and hardworking student. He felt that expenses at home were heavy, and the least he could do for his family was to be as little burdensome as possible. His means were so scanty that he was forced to reckon his expenses even in pennies. In the entries in his undergraduate note-book we meet with: "Subject £2, Spoon 6*d*., and Bread and Tart, one shilling and eightpence," and "Fur Cap 14 sh., Mary's Tippet 2 sh. and 6*d*." In the blank leaf of his little cash-book he wrote:

> No trivial gain nor trivial loss despise,
> Mole-hills, if often heaped, to mountains rise.
> Weigh every small expense, and nothing waste,
> Farthings long saved amount to pounds at last.

If the poetry of the lines leaves something to be desired, the sentiment they express leaves nothing to be desired.

The interest taken by Simpson in his studies is not simply that taken by a young man anxious to attain a leading position in the medical profession. Nor were his studies merely of the bread-and-butter order, to use the apt German phrase. For he read his botany, zoology, geology, and meteorology with one eye upon his examinations and the other fixed upon weaknesses in the explanations of his lecturers. These weaknesses might be logical or due to contradiction of facts the undergraduate had observed for himself. No young man who cared merely for passing his examinations, even with credit, would wish to find out "a law determining the appearance of stragglers, as well as of the birds which regularly visit this country at particular seasons." Just as Jenner noted every phenomenon of which he had no adequate explanation, so did Simpson. He admired Sir Isaac Newton, probably the greatest man of science our race ever produced, and his admiration is based in part upon his gigantic genius and intellectual strength and in part upon his powers of patient thought and industry. If Sir Walter Scott could toil terribly, so could young Simpson. In fact, throughout his life he leant to the view that between men who attain unusual distinction and those who do not there is not more than a one per cent. difference, but it is just this one per cent. difference that counts. Of course he had sleepless diligence, but so have many Scots. He had originality of thought, and he took notes at college, as Opie is said to have mixed his colours, "with brains."

Simpson flowered early, and was cut off early. Eager for intellectual distinction, and for the rewards which would enable him to repay the sacrifices made in his home, it seems to us that he was careless of health. The strenuous work of Edinburgh University is seldom undertaken by the undergraduates of either Oxford or Cambridge, nor is it altogether desirable that this should be so. For there is manifold truth in the saying of Toppfer that a year of downright loitering is a desirable element in a liberal education. Such an element was entirely unknown to Simpson at any period of his life. To toil terribly was part and parcel of his character. He never had a margin in his life of hurried—and unceasing—thought. To him "work was master and the lord of work, who is God." He began his medical studies in 1827 and graduated M.D. in 1832. He shrank so much from the sight of suffering that at one time it did not seem as if he would be able to remain a medical student. When he witnessed the awful agony of a poor Highland woman under amputation of the breast, he left the class-room with the firm intention of seeking employment as a writer's clerk. It is a feeling common to not a few sensitive medical students, but he was unusually sensitive. On second thoughts, he returned to the study of medicine, asking, "Can anything be done to make operations less painful?" The note of this question is never absent from the rest of his career. In 1836 he demanded, "Cannot something be done to render the patient unconscious while under acute pain, without interfering with the free and healthy play of natural functions?" For a time he turned his attention to mesmerism, a direction in which he possessed wonderful powers. When he was created a baronet in 1866, he took for his crest the healing rod of Æsculapius, and for his motto "Victor Dolore"—a motto he was amply entitled to assume.

Is early disappointment an element of later success? The cases of Lord Tenterden and Simpson suggest that this is so. When the former was about the age of fourteen, his father put him forward as a candidate for a place as singing-boy in Canterbury Cathedral. But as his voice was husky, another lad was elected. In after-years, as Lord Chief Justice, he went the home circuit with Mr. Justice Richardson, and visited the cathedral with his fellow-judge. Pointing to a singer still in the choir, he said, "Behold, brother Richardson, that is the only human being I ever envied. When at school in this town we were candidates for a chorister's place; he obtained it; and if I had gained my wish, he might have been accompanying you as Chief Justice, and pointing me out as his old schoolfellow, the singing-man." When Simpson obtained his surgical diploma he sought a situation as surgeon to the village of Inverkip, on the Clyde. As he had some local influence, he deemed his chances good. "When not selected," he informs us, "I felt perhaps a deeper amount of chagrin and disappointment than I have ever experienced since that date. If chosen, I would probably have been working there as a village doctor still. But like many other men I have, in relation to my whole fate in life, found strong reason to recognise the mighty fact that assuredly—

> There's a Divinity that shapes our ends,
> Rough-hew them how we will."*

Simpson's doctoral dissertation was on death from inflammation, and his examiner was Dr. John Thomson, the Professor of Pathology, who was so much struck with this Latin † dissertation that he engaged Simpson as his assistant

* J. Duns, *Memoir of Sir James Y. Simpson*, p. 33.

† Simpson was among the last graduates examined through the medium of Latin; cf. H. Laing Gordon, *Sir James Simpson*, p. 39.

at the salary of fifty pounds a year. Thomson steadily suggested that his assistant should turn from pathology to obstetrics, conduct that was on his part very self-denying. The assistant took the proffered advice on the spot. Nevertheless, he gave his lectures on pathology with readiness and fluency as well as with knowledge. The success of his lectures was immediate, and his success came as the reward of his hard work. He cared for his subject, and this care was at once evident to the men who attended his first lectures. He was an attractive lecturer whose mesmeric presence and pleasing voice were undoubted assets. Distinction in the academic world foreshadowed distinction in the larger world of the Scots metropolis, and in 1835 he was appointed senior president of the Royal Medical Society of Edinburgh when only in his twenty-fourth year.

In 1839 Dr. James Hamilton resigned the Midwifery Chair. A year or two previously Simpson had remarked to some ladies he was escorting to the "capping" of the graduates: "Do you see that old gentleman? well, I intend to have his gown." There were obstacles in the way. When are there not? The candidate was young and he was a bachelor. The latter obstacle was removed by his marriage to Miss Jessie Grindlay of Liverpool on December 26, 1839. Time every day was removing the former. As Robertson Smith and William Thomson (afterwards Lord Kelvin) felt that their youth was a barrier in their way when they desired chairs, so Simpson felt his. He longed for his brother John's prematurely whitened head to give him at any rate the appearance of the weight of years. The contest lay between Dr. Evory Kennedy, who enjoyed the support of much of the University, and Simpson. The electors were Edinburgh Town Council, a body that on the whole has shown discernment in its appointments. Its members were vigorously canvassed, and documents of portentous length—the testimonials of one candidate extended to more than 150 large octavo pages—were sent in to them for their consideration. On February 4, 1840, the election took place. Out of the thirty-three present, seventeen voted for Simpson and sixteen for Kennedy. Immediately after the election he wrote to Mrs. Grindlay: "My dearest Mother,—Jessie's honeymoon and mine begins to-morrow. I was elected

Professor to-day by a majority of one. Hurrah! Your ever affectionate son, J. Y. Simpson." *

His private practice was commensurate with his public distinction. His fee-book testified how increasingly patients sought his services. Indeed he became in such demand that there was not time enough even for them. He burst out with an "O that there were double twenty-four hours in the day!" Even this allowance would not have sufficed for the demands constantly made upon him. Thrifty as he had been in his student days, he was not thrifty towards himself in the collection of his fees. It is possible to give a turn to the old saying of "Street angel, house devil," for a man may serve after-generations, and not his own. It is not the least enviable jewel of Simpson's crown of success that he served his own generation as faithfully as after-generations. There were more names in his books than there was money in his pocket. The generosity of the medical profession is known to its members and to the clergy, who realise most the amount of good done by stealth by the surgeon. Simpson's relatives and friends often urged him so to regulate the management of his practice as to ensure his fee. His answer was: "I prefer to have my reward in the gratitude of my patients."

A Lammermoor shepherd hopes "the Lord will bless him and his for all his kindness to Jean." A shoemaker tells him: "Tammy's been another callant since you saw him; we thank God for such a doctor." A minister writes, enclosing a fee, which was returned: "To you, under God, I feel indebted that I have still her who is the light of my heart and hearth. We cease not to remember you in our prayers." Nor did he forget other professional struggling men. Many such a man could call to mind that when he was bidding farewell to his old Professor, Simpson would follow him to the door with the apologetic inquiry if he had enough money to start his professional career. If not, the loan was forthcoming with the words, "for I was poor too, and if my brothers hadn't helped me without stint, I would not be where I am to-day."

There have been men who have been great obstetricians

* E. B. Simpson, *Sir James Y. Simpson,* p. 40.

and nothing more. Simpson was not one of these, for the range of his curiosity was of the widest. The feeling that the cobbler should stick to his last is even yet fairly widespread. We see it when Huxley endeavoured to confine Lord Kelvin to mathematical physics to the exclusion of geology, and we see it in our own time. If the ramifications of midwifery led Simpson to considerations of surgery, then he was not to be deterred by the warnings of surgeons that he should confine himself to his own proper subject. He was a man whose aim was the healing of humanity, and no object that could compass this end was foreign to him. Fife caves, cup-marked stones, ancient sculptures, the provision of medical officers for the Roman army—these are a fraction of the subjects that surged in his ever-active brain. *The Lancet* might call him to task for his dabbling in séances, but the all-embracing explanation he tendered was that this was a subject that deserved a fair trial. He always tried to preserve an open mind. His foresight was at least as remarkable as his insight. In advance of his time he anticipated the development of ovariotomy, and he prophesied in his graduation address the discovery of the Röntgen rays. "Possibly," we learn, "even by the concentration of electrical and other lights we may render many parts of the body, if not the whole body, sufficiently diaphanous for the inspection of the practised eye of the physician and surgeon." For such a suggestion to have been made thirty-five years before the actual discovery by Röntgen is very surprising. He was also before his day in the welcome he extended to the pioneers of the lady doctors.

In January 1847 Simpson was appointed one of Queen Victoria's Physicians for Scotland, and the Duchess of Sutherland added to his natural pride when she informed him that her mistress deemed that this was a post "which his high character and abilities make him very fit for." * To his brother Sandy he wrote: "Flattery from the Queen is perhaps not common flattery, but I am far less interested in it than in having delivered a woman this week without any pain while inhaling sulphuric ether. I can think of naught else." † Ever since he had witnessed the agony of the poor

* J. Duns, *Memoir of Sir James Y. Simpson*, p. 201.
† *Ibid.*, p. 202.

Highland woman, the problem of pain, of unnecessary pain, had never been far from his thoughts. It is intelligible, therefore, that he should write, " I can think of naught else." Ether was one way out of it. Were there not other drugs? Of course there were, and equally of course experiments must be made with them, for there were disadvantages as well as advantages in the use of ether. Faraday in our country and Godman in America had realised the effects of the inhalation of the vapour of this drug.

Perchloride of formyle, or chloroform, was first discovered and described at nearly the same time by Soubeiran in 1831 and by Liebig in 1832. The first to ascertain accurately its composition was the French chemist Dumas, and this he did in 1835. None of these three, however, entertained any idea that chloroform could be turned to the relief of pain under an operation. Yet when Sir Humphry Davy had applied nitrous oxide he wrote in 1830: " It appears capable of destroying physical pain. It may be used with advantage during surgical operations in which no great effusion of blood takes place." Sound requires an atmosphere, and there was no atmosphere for this fertile conception of Davy. For forty years it lay dormant. As Mr. Colton was lecturing on laughing-gas in Hartford, Connecticut, he had among his auditors Dr. Horace Wells, dentist. He saw a person, who inhaled it, fall and bruise himself badly, and remain unaware of the fact. Next day Mr. Colton administered the gas to Dr. Wells, and Dr. Riggs extracted one of his teeth. " A new era in tooth-pulling!" he exclaimed. " It did not hurt me more than the prick of a pin." This was the first anæsthetic operation in the United States, and it took place in 1844. Dr. Riggs drew six teeth from another patient, at one sitting, without any suffering. Dr. Wells informed Drs. Warren, Heyward, Jackson, and Morton of his discovery. There was then to be a public demonstration before the Medical School of Boston and some surgeons of the Massachusetts Hospital. There was a single slip in the single experiment allowed Dr. Wells, and that sufficed to end his career in hisses and hoots. The truth was that he had not given a sufficiently large dose.

In 1842 Mr. Crawford Long of Georgia employed the vapour of ether.* Dr. Morton, a daring dentist of Boston, proceeded to experiment on nitrous oxide gas along the line of Dr. Wells. He applied to Dr. Charles T. Jackson, who took a keen concern in the proposed experiment. Jackson recommended Morton to try ether. On September 30, 1846, Morton inhaled it, with the result that he was eight minutes unconscious. He at once drew the conclusion that operations were possible by means of ether. He asked for a public trial of it at Massachusetts General Hospital on October 16, 1846, and, to quote the words of Oliver Wendell Holmes, "by this priceless gift to humanity, the fierce extremity of suffering has been steeped in the waters of forgetfulness, and the deepest furrow in the knotted brow of agony has been smoothed for ever." In a letter written by Simpson to Morton on November 19, 1847, we read: "Of course the great thought is that of producing insensibility, and for that the world is, I think, indebted to you." The Americans died without wealth or honour and with worry and disappointment, Wells dying insane.

Throughout the autumn and summer of 1847 Simpson and his assistants, Dr. Matthews Duncan and Dr. George Keith, tried narcotic drug after narcotic drug. On November 4, 1847, a red-letter day in the history of the alleviation of suffering, the first trial of chloroform as an anæsthetic occurred. The experimenters were Simpson and his two assistants. The witnesses were Mrs. Simpson, her sister Miss Grindlay, her niece Miss Petrie, and her brother-in-law Captain Petrie. Simpson wrote to Mr. Waldie, who had thought chloroform worth a trial: "I am sure you will be delighted to see part of the good results of our hasty conversation. I had the chloroform for several days in the house before trying it, as, after seeing it such a heavy, unvolatile-like liquid, I despaired of it, and went on dreaming about others. The first night we took it, Dr. Duncan, Dr. Keith, and I all tried it simultaneously, and were all 'under the table' in a minute or two." Dr. Keith told Miss Simpson, the daughter of the discoverer, in 1891 that: "Dr. Miller, in the appendix to his work on surgery, published soon after, gives a pretty full account of the scene. It is

* H. H. Young, "Long, the discoverer of Anæsthesia," *Johns Hopkins Hospital Bulletin,* 1897, VIII, pp. 174-84.

pretty correct, only he says we all took chloroform at once. This, with a new substance to try, would have been foolish, and the fact is, I began to inhale it a few minutes before the others. On seeing the effects on me, and hearing my approval before I went quite over, they both took a dose, and I believe we were all more or less under the table together, much to the alarm of your mother, who was present." *

Professor Miller, who used to appear every morning to see if the experimenters were still in the land of the living, says that "these experiments were performed after the long day's toil was over, at late night or early morn, and when the greater part of mankind were soundly anæsthetised in the arms of common sleep." He describes how, after a weary day's labour, the three sat down and inhaled various drugs out of tumblers, as was their custom, and chloroform was searched for and "found beneath a heap of waste paper, and with each tumbler newly charged, the inhalers resumed their occupation. . . . A moment more, then all was quiet, and then a crash. On awakening Dr. Simpson's first perception was mental. 'This is far stronger and better than ether,' he said to himself. His second was to note that he was prostrate on the floor, and that among the friends about him there was both confusion and alarm. Of his assistants, Dr. Duncan he saw snoring heavily, and Dr. Keith kicking violently at the table above him. They made several more trials of it that eventful evening, and were so satisfied with the results that the festivities of the evening did not terminate till a late hour, 3 a.m." †

Miss Grindlay used often to speak of Dr. Keith's ghastly expression when, ceasing to kick, he raised his head to the level of the table and stared with unconscious eyes on the onlookers. It was fitting that the first woman to feel the influence of this new agent was Miss Petrie, the niece of the discoverer. The first child born under its influence was the daughter of a medical contemporary of Simpson's. As the first child in all the Russias that was vaccinated was baptised Vaccinoff, so this baby girl was baptised Anæsthesia. "You will be pleased to hear the Queen [Victoria] had chloroform

* E. B. Simpson, *Sir James Y. Simpson*, p. 58.

† *Ibid.*, pp. 58-9.

administered to her during her late confinement. Her Majesty was greatly pleased with the effect, and she certainly never has had a better recovery," Sir James Clark, one of Victoria's physicians from London, wrote in April 1853.

The experiments did not cease with those of that epoch-making day, November 4, 1847. Miss Grindlay says that her brother-in-law came into the dining-room one afternoon, holding a little bottle in his hand, and his words were, "This little bottle will turn the world upside-down." He then poured some of the contents into a tumbler, breathed it, and fell unconscious. Miss Petrie mentions that he "tried everything on himself first." Once, after swallowing some concoction, he remained insensible for two hours. Dr. Keith recalled another experiment when he tried a compound of carbon which brought on such irritation in breathing that he had to be kept under chloroform in order to relieve him. Sir Lyon Playfair (afterwards Lord Playfair) in 1883 told the House of Commons no more than the bare truth when he asserted that in experimenting upon himself Simpson was ever "bold even to rashness." Lord Playfair was speaking of vivisection, and told how Sir James, still searching for something better, came to his laboratory, and Playfair put in his hand a new liquid. On the spot he wanted to inhale it. Playfair, however, insisted that this time the experiment should first be made on two rabbits who speedily succumbed. "Now was not this," he asked the House, "a justifiable experiment on animals? Was it not worth the sacrifice of two rabbits to save the life of the most distinguished physician of his time, who by the introduction of chloroform has done so much to mitigate animal suffering?"

No true scientist is ever content with what he has achieved, and accordingly the discoverer persisted in his never-ending search for something better than chloroform.* His butler, Clarke, entertained a high opinion of the properties of "chlory." On one occasion Clarke found Simpson in a Lethean sleep, the outcome of yet another experiment. "He'll kill himsel' yet wi' thae experiments; an' he's a big fule, for they'll never find onything better nor chlory." Though Simpson was unable to speak at the time, he heard

* Sir J. Paget, "Escape from Pain; the History of a Discovery," in *The Nineteenth Century,* VI, 1879, pp. 1119-32.

distinctly his butler's remark, and it decidedly helped to rouse him. Among other experiments Simpson prepared an effervescing drink made with chloric ether in aerated water. As a beverage a dinner party pronounced it pleasant but rather heady. The butler gave some of it to the cook, telling her it was champagne. Soon after drinking it, she fell flat on the floor. Rushing to the dining-room, Clarke cried out, "Come doon, come doon, Doctor! I've pushioned the cook deid." Naturally Clarke fell back on his old opinion. "I tell ye, chlory's the best."

No one who knows the world of science will dream that this wonderful discovery of chloroform was at once accepted. What Simpson really achieved was clearly stated by Mr. Lawson Tait at the British Medical Association in 1890 when he pointed out that "we are apt to ignore the fact that all our brilliant advancement to-day could never have been arrived at but for chloroform. We could not have developed the splendid work of the modern ophthalmic surgeon, and the modern development of abdominal surgery never would have been dreamed of, but for the genius and indomitable fighting qualities of James Young Simpson, who threshed out the victory of anæsthesia, and gave us the anæsthetic which has held its own against all comers." In 1895 in *The Times* the same surgeon, as he reviews the delicate operations now daily performed, asks: "But where should we have been without anæsthetics? No human being could undergo, in a conscious state, such operations as I have spoken of: I doubt if any human being could nerve himself to perform them. At the head of the list for whom I claim the true credit, I place the name of Simpson, the greatest genius our profession had produced for centuries. He fought the fight of anæsthesia."

Theological opposition Simpson disarmed by his exegesis of the passages bearing on the Fall and also by the fact that he was fortunate enough to procure the adhesion of the great Dr. Chalmers, then the foremost name in the ranks of the ministry in Scotland. Dr. P., a medical practitioner, curiously enough raised the question of the primary curse.* Simpson was easily able to show that the word translated "sorrow" is truly "labour" or "toil." That

* J. Duns, *Memoir of Sir James Y. Simpson*, p. 215.

is, Adam was to eat of the ground with labour, which does not necessarily mean physical pain. Though the ground was cursed to bear thorns and thistles, yet we pull them up without dreaming that it is a sin. He proceeded to argue that as Christ in dying hath "borne our griefs and carried our sorrows," He definitely removed "the curse of the law, being made a curse for us." The deep sleep into which Adam fell is ingeniously introduced as an argument on behalf of the new use of the drug. "Those who urge, on a kind of religious ground, that an artificial or anæsthetic state of unconsciousness should not be induced merely to save frail humanity from the miseries and tortures of bodily pain, forget that we have the greatest of all examples set before us. He follows out this very principle of practice. I allude to that most singular description of the preliminaries of the first surgical operation ever performed on man, which is contained in Genesis ii. 21: 'And the Lord God caused a deep sleep to fall upon Adam, and he slept, and He took one of his ribs, and closed up the flesh instead thereof.'" *

Dr. Chalmers took such a liberal interpretation of Holy Writ that he could not at first conceive that anyone could raise objections on the ground that it enabled women to avoid one part of the primeval curse. When Professor Miller at length succeeded in convincing him that such ground had been taken, Dr. Chalmers thought quietly for a minute or two, and then added that if some "small theologians" really took such an improper view of the subject, he should certainly advise Dr. Miller "not to heed them." † His attitude was probably the commonsense one that if God has given us the means of mitigating the agonies of childbirth, it is clearly His intention that we should employ those means.

As Jenner in 1798 when he first announced vaccination had to encounter the opposition of members of his own profession, so Simpson had to encounter precisely the same kind of opposition. In the deliberate words of Dr. Duns, "the leaders of the opposition were professional men," ‡ and by professional men medical doctors are meant. "I dare not try it upon the rich," wrote a leading practitioner of

* J. Duns, *Memoir of Sir James Y. Simpson,* p. 259.
† *Ibid.,* p. 260.
‡ *Ibid.,* p. 248; cf. H. Laing Gordon, *Sir James Simpson,* p. 113.

Dublin, "for my own sake, nor upon the poor for their sake, until I see something more definite about its dangers and safeguards."* On February 15, 1848, a Dr. G. thought that chloroform "is almost sure to be used as the means of debauching innocent women, which goes greatly to strengthen the argument for Legislative interference. . . . For the Burkes and Hares chloroform is the readiest implement they could desire, if it can be got without restriction."† A French physiologist, M. Magendie, held "it was a trivial matter to suffer, and a discovery, whose object was to prevent pain, was of slight interest only." ‡ "Should I," writes Dr. Greig, "exhibit it to a thousand patients merely to prevent physiological pain, and *for no other motive;* and should I, in consequence, destroy only one, the least of them, I should feel disposed to clothe me in sackcloth and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the life and health of one in a thousand in a questionable attempt to abrogate one of the general conditions of man?" Other doctors thought that the cold steel of the surgeon formed a good tonic. Dr. George Wilson describes his own feelings when in the hands of the surgeons, and his powerful letter to Dr. Simpson is a painful revelation of what an operation used to mean. § His two opening sentences reveal the fight Simpson had to make: "I have recently read, with mingled sadness and surprise, the declarations of some surgeons that anæsthetics are needless luxuries, and that unendurable agony is the best of tonics. Those surgeons, I think, can scarcely have been patients of their brother surgeons, and jest at scars only because they never felt a wound; but if they remain enemies of anæsthetics after what you have written, I despair of convincing them of their utility."

Dr. Magnus Retzius, a Danish obstetrician of high scientific standing, in a letter of April 6, 1847, succinctly stated two of the main objections to the use of ether, and these objections were the very ones to be raised with warmth against the use of chloroform. These two objections were,

* J. Duns, *Memoir of Sir James Y. Simpson*, p. 271.
† *Ibid.*, pp. 271-2.
‡ E. B. Simpson, *Sir James Y. Simpson*, p. 65.
§ *Horæ Subsecivæ*, p. 377.

first, the influence on the will of the patient, and, secondly, the beneficial influences of the action of a creative law. The first objection wore a serious form in the forties. Is there not danger to the individual, it was asked, in the employment of anything that suspends will-power or that destroys self-consciousness? The second objection appeared in all sorts of forms. One was that unnecessary interference with the providentially arranged process of healthy progression is sure sooner or later to be followed by injurious and fatal consequences. So Dr. Ashwell argued on March 11, 1848, and so argued men like Bransby Cooper, Gull, and Nunn. Simpson's retort was irresistible. "If you refuse to interfere with a natural function because it is natural—why do you ride, my dear Doctor? you ought to walk, in order to be consistent. Chloroform does nothing but save pain, you allege. A carriage does nothing but save fatigue. Which is the more important to get done away with?—your fatigue, or your patients' screams and tortures? To confess to you the truth, my blood feels chilled by the inhumanity and deliberate cruelty which you and some members of your profession openly avow. And I know that you will yet, in a few years, look back with horror at your present resolution of refusing to relieve your patients, merely because you have not yet had time to get rid of some old professional caprices and nonsensical thoughts upon the subject." * Another form of the second objection was the question of the Irish lady, "Is it not against nature to take away the pangs of labour?" "Is it not," he answered, "unnatural for you to have been carried over from Ireland in a steamboat against wind and tide?"

One moral aspect appealed to Simpson, and quite another moral aspect appealed to a Liverpool surgeon in the paper he read to the Liverpool Medical Society in 1847. He regarded the whole question as a branch of medical ethics, and argued against the use of chloroform on high moral ground. His arguments are best given in his own language. "I contend that we violate the boundaries of a most noble profession when, in our capacity as medical men,

* E. B. Simpson, *Sir James Y. Simpson*, p. 65; cf. J. Duns, *Memoir of Sir James Y. Simpson*, p. 257. Contrast Galen's aphorism, "Dolor dolentibus inutile est."

we urge or seduce our fellow-creatures, for the sake of avoiding pain alone—pain unconnected with danger—to pass into a state of existence the secrets of which we know so little at present. I say secrets, because from the dark chambers of that existence we have as yet had presented to us but fitful and indistinct gleamings, and these so little to encourage the gaze of a thoughtful and modest eye, that I should be sorry to expose any human being unnecessarily—much less one whom I esteemed or loved—to influences whose nature I more than suspect.

"What right have we, even as men, to say to our brother-man, 'Sacrifice thy manhood—let go thy hold upon that noble capacity of thought and reason with which thy God hath endowed thee, and become a trembling coward before the mere presence of bodily pain'?

". . . In connection with this part of the subject, I was struck with a remark in Professor Simpson's last pamphlet, where it is said, 'that patients themselves will force this remedy upon us.' Now, I ask, is not this a mark of disorder? Are they to be suffered for a moment to decide upon such a subject? and are we to be influenced to give up our judgment and place because they forsake theirs? . . .

"I have said nothing upon the natural or physical merits of this preparation; that has yet to be determined by time; but I cannot help suggesting to those who recommend it so indiscriminately, how they would feel in the event of death being clearly traced to its use in an ordinary labour, or during or after some surgical operation, when it was merely employed to relieve pain, and when there was no dangerous disease. If one death took place out of every five hundred, and that one was caused by the remedy, would it not be something to meditate upon? Besides, we have as yet had no time to watch other consequences; but one, I fear, in particular, will become more common—I mean insanity. I wish I may be mistaken, but I greatly fear it." * We have travelled far from this frame of mind when we remember that the doctor, who is also a psychologist, aims at re-creating the whole of the past life of the patient, in nervous cases, in the attempt to under-

* J. Duns, *Memoir of Sir James Y. Simpson*, pp. 221-2.

stand his complexes, and thus arrive at a cure. To-day Simpson's fame as the discoverer of chloroform stands securely. On a bead-roll of medical men with Harvey and Jenner he claims a place alongside them, and this claim has been universally conceded.*

In gynæcology Simpson laid the foundation of the structure now raised. He gave new power in diagnosis, he gave new power in precision, and he gave new power in instruments. In diagnosis the uterine sound and the sponge tent bestowed upon the practitioner the ability to carry out treatment hitherto impossible. His amazing care with his patient gave his fellow-obstetricians a new sense of the saying of Carlyle that a quality of genius was the infinite capacity for taking pains. In the use of the obstetric forceps and of the various methods of ovariotomy his work was original. On December 19, 1859, he read a paper on acupressure before the Royal Society of Edinburgh. Acupressure was an invention by which veins were pinned, and thereby the use of ligatures after amputations was rendered unnecessary. The point was the avoidance of the danger of festering flesh. Objections were urged by Professors Miller, Erichsen, Neudöver, Spence, Ferguson, and his old enemy, Syme, who resented the intrusion of the gynæcologist into the operating theatre. That this idea of Simpson has perished we all know. Paracelsus, when he dissented from the conclusions of an author, used to burn the particular writing. Syme took the pamphlet on acupressure by Simpson, his colleague in Edinburgh University, and in the presence of his class he tore it in two, and gave the pieces to his assistant to be consigned to the sawdust box with other surgical remains. A young student, Joseph Lister by name, aimed at achieving by antiseptic treatment what Simpson achieved by acupressure. At the Dublin meeting Simpson had made some disparaging remarks on the use of antiseptics. Unguardedly Lister wrote to *The Lancet* a letter in which occurred the following passage: "The truth is, that the treatment which I advocate has arrived at the second stage of its progress in professional confidence. So lately as the Association meeting in Dublin a feeble attempt

* It was estimated that no less than £80,000 a year was lost to the hotel, lodging-, and boarding-house keepers of Edinburgh when he died; cf. H. Laing Gordon, *Sir James Simpson,* p. 163.

was made to decry it as useless; and now it is represented as not original. Trusting that such unworthy cavils will not impede the adoption of a useful procedure, I am, Sir, yours, etc." *

In a temperate letter of June 16, 1865, Simpson urged upon Lister the merits of acupressure.† We fear that Simpson in an anonymous letter to the *Edinburgh Daily Review* attacked Lister on the ground that his work had been antedated by Dr. Lemaire of Paris and by others. Lister had never read a line of Lemaire's and indeed had never heard of him. In *The Lancet* Simpson continued his attack, accusing Lister of almost culpable ignorance.‡ The writer did bring out the fact that Lemaire had employed carbolic acid in the treatment of compound fractures, wounds, and abscesses. He brought it out, however, in such a fashion that the principle on which Listerian treatment was based was ignored. In 1867 Simpson occupied the position of a field-marshal in the medical army and Lister was the latest recruit. The greeting the senior officer extended was anything but friendly. Simpson himself had suffered so much at the hands of objectors that one would have thought that he would have had a fellow-feeling for Lister.

* Sir R. Godlee, *Lord Lister*, p. 200; cf *Lancet*, 1867, II, p. 444.
† Sir R. Godlee, *Lord Lister*, pp. 201-2.
‡ *Lancet*, 1867, II, p. 546.

CHAPTER III

LYELL AND UNIFORMITARIANISM

At the beginning of the nineteenth century geologists were divided into two hostile camps who waged against each other a keen and even embittered contest. On the one hand were the followers of James Hutton of Edinburgh, a man of a singularly original and active mind, called from him Huttonians, sometimes also Vulcanists or Plutonists; on the other, the disciples of A. G. Werner of Freiburg in Saxony, who were naturally called Wernerians or Neptunists.* The strife lasted long, and deflected the current of geological thought. The Huttonians maintained, as their fundamental doctrine, that the facts of our planet in the past are to be explained by what we can learn of it at present. The first condition of the earth and all its subsequent phases they regarded as outside their scope. Hutton declared with all the emphasis at his command that the rocks around us can never reveal to us any trace of the beginning of things. As nature to him never made a leap, he summoned the fall of rain, the flow of rivers, the dash of waves, the slowly-crumbling decay of mountain, valley, and shore as witnesses of the slow and silent fashion in which even the most stupendous changes are wrought. Living in a land devoid of fossil remains of plants and animals, devoid of such rocks as are found in Italy, Hutton ignored the long succession of life upon the earth just as he ignored the fact that nature—to our surprise—sometimes does make a leap. Still, he ascertained that the great mass of the rocks which form the visible part of the crust of the earth was formed under the sea, as sand, gravel,

* For a recent reminiscence of the strife, cf. Sir A. Geikie, *A Long Life's Work*, p. 143.

and mud are laid down there now; and that these ancient sediments were consolidated by subterranean heat, to be contorted and upheaved one day into dry land. He found that portions of the rocks had been in a fused state, establishing the former molten condition of granite and of many other crystalline rocks, and he maintained that the combined influence of subterranean heat and pressure upon sedimentary rocks could consolidate and mineralise them, thus converting them into crystalline masses.

If it is the mark of genius to unite in one common origin phenomena very different in their nature, then James Hutton was a genius. He opened out a new path that has been trodden by many men since. Of course he exaggerated the extent to which his conceptions could be applied, but is not this the commonest fault of genius? Hutton himself was as strong in principles as he was weak in details. His followers were deficient in accurate mineralogical knowledge. In spite of this, the whole of modern geology testifies to the influence of the Huttonian school.

From his infancy Abraham Gottlob Werner was familiar with stones. When he did his lessons with proficiency, his father used to allow him to look over a small collection of minerals which he kept in a box. As an old man Werner could vividly recall the very minerals that were the playthings of his childhood—various ores and spars, as well as some varieties of which his father did not know the names. Naturally he betook himself to the Mining Academy at Freiburg, visiting all the chief Saxon mines, especially those of note in the Freiburg district. He came to know his own locality intimately, satisfying himself by repeated excursions of the order and history of the rocks in it. Jowett used to maintain that logic was neither a science nor an art but simply a dodge. Werner held some such opinion, for despite the laws of logic he reasoned from the particular to the universal, concluding that the various rocks of the rest of the globe were modelled on those of Saxony. Unlike the Huttonians, he felt impelled to begin at the beginning. He supposed that the earth had been originally covered with the ocean, in which the materials of the minerals were dissolved. Out of this ocean he conceived that the various rocks were precipitated in the same order as that in which he found those of

Saxony to lie. Obviously, on the retirement of the ocean, certain universal formations spread over all the globe, and assumed at the surface various irregular shapes as they consolidated.

Werner enjoyed the advantage of being a good mineralogist. Previous cosmological systems produced chaos, not the cosmos of the Saxon geologist. His system was as neat and precise as he himself was. Besides, it could be readily applied to other countries. Observations had been disconnected, isolated, and heterogeneous. Under Werner's skilful method they proved to be connected, unisolated, and homogeneous. What Linnæus effected for botany he effected for mineralogy. This subject is narrow save in the hands of a Werner. With him mineralogy embraced the whole of human history, the whole pursuits and tendencies of mankind. From a few pieces of stone, placed almost at random on the table before him, he would pour forth an eloquent exposition of the influence of minerals and rocks upon the geography and topography of the earth's surface. He could contrast the mountainous scenery of the granites and schists with the less wild landscapes of the sandstones and limestones. Was not the development of the arts and the industries guided by the distribution of minerals? Were not the successes of a Napoleon or a Wellington dependent not on their strategic skill but on the distribution of minerals? In fact, Werner anticipated the school of political economists of our own day who trace everything in history to £ *s. d.* It seemed in Werner's day as if efficient training for the affairs of life was only to be found at the Mining School of Freiburg.

There were undoubted weaknesses in the Wernerian school of geology. In what school of thought are there not weaknesses? He made the mistake of transferring the formation and the aqueous origin of Saxon rocks to explain the formation of all rocks. Still, he had grasped a seminal idea in his chronological grouping of strata and had noticed that the remains of plants and animals imbedded in the strata became fewer in number, and more unlike living forms, the older the rocks in which they occur. The theory—an entirely unsupported one—of a primeval universal ocean formed the basis of his teaching. From the prominence given to the sea in his geognosy, his followers were styled Neptunists, while those of Hutton, who stressed the potency of the internal fire of the earth, were dubbed Plutonists or Vulcanists. Obviously, there is this question to be answered by the Wernerians, What has become of the immense volume of water that once covered and stood so high over the whole earth? Robert Jameson, a typical Scots Wernerian, announced that "although we cannot give any very satisfactory answer to this question, it is evident that the theory of the diminution of the water remains equally probable. We may be convinced of its truth, and are so, although we may not be able to explain it. To know from observation that a great phenomenon took place, is a very different thing from ascertaining how it happened."* The moment we examine this answer we see that it resolves itself into a scientific creed: I believe in Werner and you had better believe in him too. The gravamen against this creed is that it announces as a body of ascertained truth conclusions about which it was held that there could be no further doubt or dispute, and such a position implicitly denies the possibility of progress.

The unreasoning rivalry of the Wernerians and the Huttonians was at its height when Charles Lyell was born on November 14, 1797, in the family mansion of Kinnordy, Forfarshire. His father was a botanist with diverse interests. He is another example of that hereditary genius on which Galton placed such stress. "The front of heaven," as Lyell himself wrote in a spirited fragment of autobiography, was not "full of fiery shapes at his nativity,"† but the season was so exceptionally warm that his mother's bedroom-window was kept open all night—an appropriate birth-omen for the future geologist, who had a firmer faith than some of his successors in the value of work in the open air. Scots by birth, English by education, Lyell was to turn out cosmopolitan in his range over the earth. For twenty-eight years his family lived on the edge of the New Forest, a situation that afforded the lad many opportunities of gratifying his taste for watching the habits of aquatic insects. "I had," he confesses, "no companion to share this hobby with me, no one to encourage me in following it up, yet my love for it continued always to increase, and it afforded me a

* R. Jameson, *Geognosy*, p. 82.
† Horner, *Life of Sir C. Lyell*, I, p. 2.

most varied source of amusement."* Like Darwin at Shrewsbury, he received "from almost everyone else beyond my home, either ridicule, or hints that the pursuits of other boys were more manly."† Mr. Lytton Strachey, in his curious study of Thomas Arnold of Rugby, seems to imagine that the boys of our day enjoy considerably less elasticity in their amusements than those of Arnold's day. A study of the early life of either Lyell or of Darwin would, on this point at least, considerably disabuse him. To be sure, his essay would not have quite so much point: it would, however, have a good deal more truth.

As a lad Lyell had met with a copy of Bakewell's *Geology* on the shelves of his father's library, and this induced him while he was an undergraduate at Exeter College, Oxford, to attend in 1817 the lectures of Professor Buckland, who then enjoyed the height of his popularity. On July 28 of that year he wrote to his father, and his letter —remarkable as that of a lad of nineteen—contains the germ of his future book on the *Principles of Geology*. "Dr. Arnold and I examined yesterday the pit which is dug out for the foundation of the Nelson monument, and found that the first bed of shingle is eight feet down. Now this was the last stratum brought by the sea; all since was driven up by wind and kept there by the 'rest-harrow' and other plants. It is mere sand. Therefore, thirty-five years ago the Deens were nearly as low as the last stratum left by the sea; and as the wind would naturally have begun adding from the very first, it is clear that within fifty years the sea flowed over that part. This, even Mr. T. allows, is a strong argument in favour of the recency of the changes. Dr. Arnold surprised me by telling me that he thought that the Straits of Dover were formerly joined, and that the great current and tides of the North Sea being held back, the sea flowed higher over these parts than now. If he had thought a little more he would have found no necessity for all this, for all those towns on this eastern coast, which have no river god to stand their friend, have necessarily been losing in the same proportion as Yarmouth gains—viz. Cromer, Pakefield, Dunwich, Aldborough, etc. etc. With Dunwich I believe it is Fuit Ilium."‡

* Horner, *Life of Sir C. Lyell,* I, p. 14. † *Ibid.,* p. 16. ‡ *Ibid.,* p. 43.

At twenty Lyell had seized hold of the germinal notion that was to change the future of geology, at the same age at which Pasteur became immersed in the puzzle of the right- and left-handed crystals of tartaric acid. At eighteen Perkin discovered the first aniline dye, mauve. At the same age Einstein conceived the idea of his theory of relativity. At twenty-two van't Hoff and at twenty-seven Le Bel simultaneously carried the ideas of Pasteur on his crystals a marked stage along the road to completer knowledge. At twenty-three Emil Fischer discovered what led ultimately to the discarding of the type theory. At twenty-four Svante Arrhenius devised the electrolytic theory of solution that all salts are decomposed in water into positive and negative elements. At the same age Berthelot discovered his synthesis of benzene compounds. At twenty-six that brilliant scientist Henry Moseley found a way to analyse the elements by the reflection of X-rays from their atoms, one of the most important generalisations in the history of chemistry since Mendeleef's Periodic Law. What a bullet in Gallipoli cost us, we can read in the measured estimate Sir J. J. Thomson furnished of Moseley's career in the *Transactions* of the Royal Society. At twenty-eight Niels Bohr conceived the atom as a sort of solar system in which the sun is represented by a nucleus of positive electricity and the planets by particles of negative electricity revolving around it with astonishing speed. At twenty-eight Kekulé formulated ideas which led to the discarding of the prevalent type theory. At twenty-nine Crookes discovered thallium, a new metal by a new method, that of the spectroscope.

The undergraduate's father was a man of means, and fortunate is it for geology that this was so. For during the long vacation of 1818 young Lyell accompanied his father, mother, and two eldest sisters on a continental tour. They drove in a ramshackle carriage. The slow pace of the carriage enabled Lyell *inter alia* to note the nodular flints in the limestone of the Jura, the contrast between these mountains and the Grampians of his native land, the rapid advance of the glaciers in general and of the Glacier des Bossons in particular, the action of the torrent of the Alpbach and its effects in carrying liquid mud and shattered slate. If we agree with Heine that what we see depends on our powers of sight, then it is clear that the possibilities of the powers of this young man were in excess of those of most men of his standing.

On his return to Exeter College, he found that few of his old acquaintances had come up after the long vacation. "It will be less difficult," he thinks, "therefore, to acquire what Paley so strongly recommended to his pupils, 'courage to be alone.'"* This courage Lyell readily acquired, and he acquired it with all the more ease because he was forming the purpose to devote his life to the furtherance of geological discovery. In the spring of 1827 his ideas as to his future work appear to be assuming a definite form. To Dr. Gideon A. Mantell he writes that he has been reading Lamarck, and is not convinced by that author's theories of the development of species, "which would prove that men may have come from the ourang-outang," though he makes this admission: "After all, what changes a species may really undergo! How impossible will it be to distinguish and lay down a line, beyond which some of the so-called extinct species have never passed into recent ones. That the earth is now quite so old as he [i.e. Lamarck] supposes, has long been my creed, and I will try before six months are over to convert the readers of the *Quarterly* to that heterodox opinion. . . . I am going to write in confirmation of ancient causes having been the same as modern, and to show that those plants and animals which we know are becoming preserved now, are the same as were formerly preserved. E.g. scarcely any insects now, no lichens, no mosses, etc., ever get to places where they can become imbedded in strata. But quadrupeds do in lakes, reptiles in estuaries, corals in reefs, fish in sea, plants wherever there is water, salt or fresh, etc. etc. Now have you ever in Lewes levels found a bird's skeleton or any cetacea? If not, why in Tilgate and the Weald beds? In our Scotch marl, though water birds abound in those lakes, we meet with no birds in the marl; and they must be at least as rare as in old freshwater formations, for they are much worked and examined. You see the drift of my argument—ergo, mamalia existed when the oolite and coal, etc., were formed."† There were these differences between successive

* Horner, *Life of Sir C. Lyell*, I, p. 52.
† *Ibid.*, p. 169.

fauna. What was their origin? What was the cause of their extinction? Answers to questions like these constituted the prelude to the understanding of the relations between existing genera and species.

As these ideas were revolving in the mind of Lyell he was fortunate to meet a man slightly senior to himself, one Roderick Murchison by name. Murchison had served at Vimeiro, and had shared in Sir John Moore's Spanish campaign and his famous retreat to Corunna. On the conclusion of the war he married a thoughtful and affectionate woman who so altered him that he pursued science with as much ardour as he had formerly pursued the fox. Healthy and wealthy, he became wise with the wisdom of geological lore. His strength lay neither in the philosophic spirit nor in the imaginative power that marked out Lyell, but in his rapid comprehension of the leading features in the geology of a district. He possessed a patient and sagacious faculty of gathering facts and marshalling them, thereby paving the way for Lyell to draw conclusions from these facts. Murchison never quite escaped from the influence of the Wernerian school. To the end of his long life he maintained that the present inequalities of the land are due to subterranean action. In the Silurian system he certainly confused the Llandovery rocks with the Caradoc sandstone. As one of the main articles in his scientific creed was the occurrence of former convulsions of nature, he inevitably offered stout opposition to the views of the evolutionists. There were limitations to Murchison's outlook, but some of these limitations supplemented deficiencies in the equipment of Lyell. With Mr. and Mrs. Murchison, in May 1828, he toured by carriage through Clermont Ferrand, one of the most interesting geological districts in Europe. Visiting Pontgibaud and the gorge of the Sioul, they discovered a section that afforded them a demonstration that a lava stream had dammed up the course of a river by flowing down into its valley, and had converted the part above into a lake. This in turn had been drained as the river had carved for itself a new channel, partly in the basalt, partly in the underlying gneiss. Was it not obvious that a river could cut out a path for itself? Was it not equally obvious that such a force was still in operation? Surely, this was Lyell's conclusion, such forces, given time enough, could sculpture the features of the crust of the earth. So saw Lyell, though Murchison saw quite otherwise. At the Euganean Hills the Murchisons and Lyell at the end of a four months' elaborate investigation parted.

Lyell wended his way southward to Naples and Sicily. On the coast of the Maritime Alps he encountered huge beds of conglomerate, parted one from another by laminated shales full of fossils, most of which were identical with creatures still living in the Mediterranean. These masses attained a thickness of 800 feet, and were displayed in the sides of a valley fifteen miles in length. Did not the torrents from the Maritime Alps, as they plunged into the Mediterranean, build up these masses of stratified pebbles? Did not similar torrents form the conglomerates and sandstones of Angus in his native country? If rain and rivers could excavate valleys, the sea could slowly raise fossiliferous deposits. One outcome of the tour was the three papers Murchison and he wrote. On January 21, 1829, he tells his sister Marianne: "My letters from geological friends are very satisfactory, as to the unusual interest excited in the Geological Society by our paper on the excavation of valleys in Auvergne. Seventy persons present the second evening, and a warm debate. Buckland and Greenough furious, contra Scrope, Sedgwick, and Warburton supporting us."* Buckland was an eloquent expounder of the view that the remains of animals found in caves afford the means of judging the inhabitants of the earth before the universal deluge. Greenough was the true founder and first president of the Geological Society, yet he displayed an obstinate scepticism towards new opinions, being a kind of staunch geological Tory. Poulett Scrope believed in putting notions to the test. When he put Werner's ideas to the test of the evidence afforded by nature in the case of volcanoes, he proved them to be mere "idols of the cave." Sedgwick was an equal believer in laying up a store of facts from which he could extract brilliant deductions. Warburton was the very soul of caution.

The reception of this joint paper proved to Lyell the need of accumulating further facts in support of his views, and this meant more travelling. In later years he held: "We

* Horner, *Life of Sir C. Lyell,* I, p. 238.

must preach up travelling as the first, second, and third requisites for a modern geologist."* What he preached he practised, and accordingly we find him climbing Vesuvius, seeing there for the first time the lava-streams and piles of scoria of a volcano still active. The sections of the old crater of Somma furnished a link between the living present and the remote past—between Italy and Auvergne. Visiting Ischia, he ascended its old volcano, Monte Epomeo, to find at a height of two thousand feet above the sea marine shells which belonged to the same class as those in the lower regions of Ischia. He discovered these fossils, and at once it struck him, Had not the land been elevated two thousand feet without any appreciable change in the fauna inhabiting the Mediterranean? Was there not here another proof of the slow work of nature? In spite of the bad roads, the poor fare, and the miserable accommodation he met with, the heart of the geologist was rejoicing, for his results, as he told Murchison, "exceeded his warmest expectations in the way of modern analogies." On February 26, 1829, he confides in his sister Caroline: "I will build up a system on data never before obtained, by comparing the contents of the present with more ancient seas, and the latter with each other."†

To the older school nature was always making a leap, whereas to Lyell she seldom, if ever, made a leap. At all times and in all places, he held, nature remained constant in her operation. In April 1829 he tells Gideon Mantell: "A splendid meeting last night. Sedgwick in the chair. Conybeare's paper on Valley of Thames, directed against Messrs. Lyell and Murchison's paper, was read in part. Buckland present to defend the 'Diluvialists,' as Conybeare styles his sect, and us he terms 'Fluvialists.' Greenough assisted us by making an ultra speech on the importance of modern causes. No river, he said, within times of history, has deepened its channel one foot! It was great fun, for he said, 'Our opponents say, "Give us time, and we will work wonders." So said the wolf in the fable to the lamb: "Why do you disturb the water?" "I do not: you are further up the stream than I." "But your father did." "He never was here." "Then your grandfather did, so I will murder you.

* Horner, *Life of Sir C. Lyell*, I, p. 233.
† *Ibid.*, I, p. 252.

Give me *time,* and I will murder you." So say the Fluvialists!' Roars of laughter, in which Greenough joined against himself. What a choice simile! Murchison and I fought stoutly, and Buckland was very piano. Conybeare's memoir is not strong by any means. He admits three deluges before the Noachian and Buckland adds God knows how many *catastrophes* besides, so we have driven them out of the Mosaic record fairly."*

To Mantell on June 7, 1829, he writes: "The last discharge of Conybeare's artillery, served by the great Oxford engineer against the Fluvialists, as they are pleased to term us, drew upon them on Friday a sharp volley of musketry from all sides, and such a broadside at the finale from Sedgwick, as was enough to sink the *Reliquæ Diluvianæ* † for ever, and make the second volume shy of venturing out to sea."‡ In a letter of June 10, 1829, he continues the account of the Diluvialist *v.* Fluvialist controversy, ending his letter with these words: "I am preparing a general work on the younger epochs of the earth's history, which I hope to be out with next spring. I begin with Sicily, which has almost entirely risen from the sea, to the height of nearly 4,000 feet, since all the present animals existed in the Mediterranean!"§

The summer of 1829 Lyell spent at Kinnordy exploring the quarries of Kirriemuir and the neighbouring districts and encouraging the workmen to look out for the remains of plants and the scales of fishes. What he was doing at home Murchison and Sedgwick were doing abroad. For they were exploring the geological structure of the Eastern Alps and the basin of the Danube. Throughout their labours they kept in touch with Lyell, who derived satisfaction from obtaining results that he felt must keep Murchison sound in the uniformitarian faith and must turn Sedgwick to that faith. On October 31, 1829, Lyell writes: "Sedgwick and Murchison are just returned, the former full of magnificent views. Throws overboard all the diluvian hypothesis; is vexed he ever lost time about such a complete humbug; says he lost two years by having started as a Wernerian. He says primary rocks are not primary, but, as Hutton supposed,

* Horner, *Life of Sir C. Lyell,* I, p. 252.
† Buckland's book bore this title.
‡ Horner, *Life of Sir C. Lyell,* I, p. 253.
§ *Ibid.,* I, p. 254.

some igneous, some altered secondary. Mica schist in Alps lies *over* organic remains. *No rock* in the Alps older than lias! Much of Buckland's dashing paper on Alps wrong. A formation (marine) found at foot of Alps, between Danube and Rhine, thicker than all the English secondaries united. Munich is in it. Its age probably between chalk and our oldest tertiaries. I have this moment received a note from C. Prévost by Murchison. He has heard with delight and surprise of their Alpine novelties, and alluding to them and other recent discoveries, he says, 'Comme nous allons rire de nos vieilles idées! Comme nous allons nous moquer de nous-mêmes!' At the same time he says, 'If in your book you are too hard on us on this side the Channel, we will throw at you some of old Brongniart's "metric and peponary blocks," which float in that general and universal diluvium, and have been there "depuis le grand jour qui a séparé, d'une manière si tranchée, les temps ante-des-temps Post-Diluviens."' "* It is inevitably a triumphant letter, though all the statements in it have not been borne out by subsequent investigation. Beyond all doubt there are many rocks in the Alps older than the Trias. The modification and mineral changes in the Secondary rocks of the Alps are quite different from the metamorphism of the crystalline schists, which are the older rocks.

The more he investigated the complicated processes by which the rocky crust of the earth has been built up and by which the present varied contour of the surface of the earth has been produced, the more Lyell was satisfied that the slow, yet persistent, operations of nature provided an all-sufficient explanation. The study of the existing economy of the world revealed the history of our planet in early ages. Nor would he allow other matters to divert him from his self-imposed task. He was so engrossed with the writing of his *magnum opus, Principles of Geology: being an Attempt to explain the Former Changes of the Earth's Surface by Reference to Causes now in Operation,* that he refused to stand for the vacant chair of Geology and Mineralogy in the newly-established London University. He meant it to convince the ordinary reader that there was an absolute uniformity in the order of nature. It was a con-

* Horner, *Life of Sir C. Lyell,* I, p. 256.

clusion entirely opposed to the prevailing system of geological thought, but it was a conclusion to which he had been forced by the evidence he had gathered during the course of his repeated travels. He fully expected, as he wrote his pages, that the publication of his book would bring a hornet's nest about his head, but he had determined that, when his first volume was attacked, he would waste no money on pamphleteering, but would work steadily at the second volume. Then, if the book was a success, he would labour at the second edition, for "controversy is interminable work."

Henry Milman had just published his *History of the Jews*, and by this book he had incurred the disapproval of the orthodox, who resented the idea of the past of the chosen nation being treated like the history of any other nation. Its publication pleased Murchison, who was far-seeing enough to note that Milman's volumes would create a diversion in Lyell's favour. For he was attempting to treat geology like any other science, relying entirely on the evidence of the facts, not on pre-conceived theories. Poulett Scrope was to review Lyell's two volumes in *The Quarterly Review*, and in a letter of June 14, 1830, to him he insists that the climate of a region depends not only upon its latitude, but also upon its geography, the distribution of land and sea, and the coincidence of time between zoological and geographical changes in the past. These, he thought, were the most novel ideas in the book.

No one can peruse the epoch-making three volumes, which appeared from 1830 to 1833, without perceiving many new ideas. The strength of them lies not in their details but in the views so convincingly elaborated. Was the method of nature uniform, a process? Was it sudden, a leap? The longer he looked facts in the face, the more they demonstrated to him the gradual process by which nature proceeded. Deluges were the favourite resort of the catastrophic geologist, and deluges Lyell could not discover on a universal scale in the past. He laughs in one of his letters at the idea of a French geologist that a sudden upheaval of South America could have been the cause of the Noachian flood. True, there were, as the catastrophists urged, breaks in the succession of the strata, but these breaks were, in his judgment, largely local in character. In the record of the rocks he could discern no signs of a general destruction of living creatures. In a word, geology afforded no corroboration of the Mosaic cosmogony. Lyell's task was the interpretation of nature, and his clue to the handiwork of nature in the present was the study of it in the past history of the earth.

The Wernerians faced the problem of origins. Such a problem Lyell refused to face. He told Poulett Scrope that "probably there was a beginning—it is a metaphysical question, worthy a theologian—probably there will be an end. Species, as you say, have begun and ended—but the analogy is faint and distant. Perhaps it is an analogy, but all I say is, there are, as Hutton said, 'no signs of a beginning, no prospect of an end.' Herschel thought the nebulæ became worlds. Davy said in his last book, 'It is always more probable that the new stars become visible, and then invisible, and pre-existed, than that they are created and extinguished.' So I think. All I ask is, that at any given period of the past, don't stop inquiry when puzzled by refuge in a beginning, which is all one with 'another state of nature,' as it appears to me. But there is no harm in your attacking me, provided you point out that it is the proof I deny, not the probability of a beginning. Mark, too, my argument, that we are called upon to say in each case, 'Which is now most probable, my ignorance of all possible effects of existing causes,' or that 'the beginning' is the cause of this puzzling phenomenon?"*

When Macaulay had finished his history, he at once took down his Thucydides. The master of the present turned to the master of the past. Lyell took down his master figuratively, for he once more travelled abroad to note if the examination of fresh facts would confirm or destroy his leading hypothesis. He describes the scenery of the Pyrenees, contrasting it with that of the Alps, and analysing the causes of this contrast. Moving on to Spain, he visits Olot, a region of extinct volcanoes. Poulett Scrope, the well-known authority on volcanoes, had advised this visit, and here Lyell manifests his caution in refusing to assign dates for eruption in bygone ages—unless remains of quadrupeds or other organic substances be found. Evidence, not Wernerian theory—this is what he insistently demands. In the course of his wanderings he met with remains suggesting

* Horner, *Life of Sir C. Lyell*, I, p. 269.

the gradual approximation of the fauna preserved in the Tertiary deposits to that which still exists, and tending to settle, as he hopes "for ever, the question whether species come in all at a batch or are always going out and coming in." Could he oppose the diluvialists by seeing any instance where nature allowed visible growth? Joyfully he heard of the eruption of Graham's Island. A few months before there has been a depth of eighty fathoms, as sounding on the site of this island proved. Now the cone "is 200 feet above water and is still growing. Here is a hill 680 feet, with hope of more, and the probability of much having been done before the *Britannia* sounded." Nature herself was coming round to his side by bestowing "her approbation of the advocates of modern causes! Was the cross which Constantine saw in the heavens a more clear indication of the approaching conversion of a wavering world?" Precisely so, but it also suggested another consideration, Does nature make a leap? Yet this question does not seem to have crossed the mind of Lyell. His evidence suggested proof of his hypothesis, and, like many another investigator, it never occurred to him that this evidence—when scanned by eyes other than his—might point in quite a contrary direction.

During his tour he met that staunch Wernerian, Jean François d'Aubuisson de Voisins, and the Frenchman foresaw that the contempt manifested by the Huttonians for mineralogy must impede the progress of geology. D'Aubuisson came to find out that Wernerian conception of primitive rocks was a pure myth. Lyell was so strongly possessed by his big idea that he records that D'Aubuisson "thinks the interest of the subject greatly destroyed by our new invention, especially our having almost cut mineralogy and turned it into a zoological science. In short, like all men, he dislikes that which destroys his early and youthful associations, and he has too much to do as an engineer to keep up with the subject."* No one who knows the state of geology, in England at any rate, from 1840 to 1870 can avoid noticing that the forecast of D'Aubuisson proved painfully correct. Nor were the controversial articles directed by Elie de Beaumont and others against his system more to Lyell's taste. That there is an *odium scientificum* Edward Jenner

* Horner, *Life of Sir C. Lyell*, I, p. 275.

and Sir James Simpson attest. That there is an *odium geologicum* Lyell was soon to find out.

The years 1830 to 1833 were marked by the publication of the first edition, in three volumes, of Lyell's *Principles of Geology: being an Attempt to explain the Former Changes of the Earth's Surface by Reference to Causes now in Operation.* His theory is said to have been suggested by the gradual growth of the British Constitution. For five years its author had steadily concentrated on its production, embodying in it facts from physical phenomena, facts from botany and zoology as well as facts from geology, in all parts of the world and from observers of all ages. By wealth of illustration just as much as by cogent reasoning, he drove home the doctrine of uniformity on all who read his pages. His three volumes deserve to be styled by that much-abused adjective, epoch-making. To adapt the tribute Lord Bryce paid to Lord Acton, Lyell wrote like a man inspired, seeming as if, from some mountain summit high in air, he saw beneath him the far-winding path of geological evolution from dim Cimmerian shores of prehistoric shadow into the fuller yet broken and fitful light of modern time. Unlike either Sedgwick or Murchison, he added no new chapters to geological history. His function was far different, for he was the philosopher of geology, possessing the rare faculty of perceiving the connection of scattered facts with each other. He wrote little, but his ideas have been as the grain of mustard seed in the parable. As A. C. Ramsay once remarked to Sir Archibald Geikie, " We collect the data, and Lyell teaches us to comprehend the meaning of them." Lyell's destruction of catastrophism in geology prepared the mind for Darwin's destruction of catastrophism in the animal kingdom. As Blaise Pascal points out, " Qu'on ne dit pas que je n'ai rien dit de nouveau: la disposition des matières est nouvelle." For originality lies as much in perception of opportunity or fresh disposition of material as in invention. Lyell saw, perhaps too vividly, the new conception of uniformity that was looming along the horizon of the geological world. The essential point is not that he saw too vividly, but that he saw at all. There were great masters of geology at home and abroad from 1820 to 1840, the very generation when Lyell was doing his most illuminating work. On its bead-roll were such men as Sedgwick and Murchison, De la Beche and Elie de Beaumont, Von Buch and Boue, Omalius d'Halloy. Before his fourth decade was completed Lyell assumed the front rank even with such formidable competitors. The influence of the catastrophic school of geologists had long been dwindling when the publication of the *Principles* administered the *coup de grâce.*

Edition after edition of Lyell's *magnum opus* was called for, and with the increase of knowledge he had "found it necessary," as he states in his preface, "entirely to rewrite some chapters, and recast others, and to modify or omit some passages given in former editions." Naturally he was delighted to enforce the doctrine of uniformitarianism with examples produced from forces still at work upon the crust of the earth. The accounts of Vesuvius and Etna, of the vicissitudes of climate in the past, the connection between climate and the geography of the surface of the earth, the influence of astronomical causes on changes of climate—these were among the matters considerably reinforced by the additional facts the author was able to adduce. There were additions of serious importance. Convinced by the biological evidence of Charles Darwin and A. R. Wallace and by the botanical evidence of Sir J. D. Hooker, Lyell at last saw his way to get rid of the objections raised by Lamarck, and champions the theory of evolution. The historian of the early history of geology will still find in the first five chapters an account which is not superseded by the works of Sir Archibald Geikie or of K. A. von Zittel. With the process of revision the *Principles* lost not a little of their literary charm. For books, like children, are apt to lose some of their beauty as they increase in size and strength.

Like all men who conceive an original idea, Lyell pushed it far, very far indeed. The Wernerian school had urged that practically there were nothing but catastrophes, so now the Lyellian school was to urge that there was nothing but uniformity. This school attained a dominant position with Lyell as its high priest, and its creed was almost universally believed, notably in England. The high priest taught us all to substitute for catastrophes glacial action, the slow denudation by rivers, subsidence and elevation. In a word, there is an orderly process. Uniformitarianism accounts for many matters, but does it account for all? Does it account for the volcanoes as it does for the glaciers, for the aberrations of the atoms? Such questions suggest themselves, though we can quite understand Darwin pronouncing in his autobiography the verdict that "the science of geology is enormously indebted to Lyell—more so, I believe, than to any other man who ever lived." * Galileo Galilei, according to tradition, maintained that the earth still moved, and in a far different sense Lyell demonstrated that this truth was the very foundation of his system. According to Terence, *Homo sum, humani nihil a me alienum puto.* Adapting this *obiter dictum,* Lyell held that though he was a geologist, yet he regarded nothing in physics and natural history as foreign to his purpose.

In March 1831 Lyell informed Gideon Mantell that he had just been elected Professor of Geology at King's College, London, then recently founded by members of the Church of England. The electors to this chair were the Archbishop of Canterbury, the Bishops of London and Landaff, and two "strictly orthodox doctors," D'Oyley and Lonsdale. The Bishop of Landaff showed some hesitation, but the Rev. W. D. Conybeare, though opposed to Lyell's theories on scientific grounds, vouched for his orthodoxy. The prelates declared "that they considered some of my doctrines startling enough, but could not find that they were come by otherwise than in a straightforward manner, and (as I appeared to think) logically deducible from the facts, so that whether the facts were true or not, or my conclusions logical or otherwise, there was no reason to infer that I had made my theory from any hostile feeling towards revelation." † This wise caution manifests, on the whole, that attitude to science that has often marked the attitude of the Church of England towards new discoveries. Nor is there much cause for surprise at the action of the electors, for Hooker in his consideration *Of the Laws of Ecclesiastical Polity* manifests in the days of Elizabeth as warm an appreciation of the reign of law in nature as Lyell himself. As a comment on the moderation indicated by his election, Lyell says that a friend in the United States affirms that there

* *Life and Letters of C. Darwin*, I, p. 76.
† Horner, *Life of Sir C. Lyell,* I, p. 316.

"he could hardly dare to approve of the doctrines even in a review, such a storm would the orthodox raise against him. So much for toleration of Church Establishment and No Church Establishment countries." * Advocates of the disestablishment of the Church of England ought to bear in mind that the course of the years has not diminished the worth of this conclusion.

The young lady who figures in *Pride and Prejudice* as a student of literature reappears in *Sybil* as a student of astronomy or geology. Ladies flocked to hear Lyell's lectures at King's College. George Eliot, in her evangelical days, describes herself as "revelling in Nichol's *Architecture of the Heavens*." † Harriet Martineau points out that, in the period following the Waverley novels, "the general middle-class public purchased five copies of an expensive work on geology to one of the most popular novels of the time." ‡

So long as Lyell lived he learnt. High priest as he was of the doctrine of uniformitarianism, he was always willing to make such modification of his opinions as the progress of scientific inquiry demanded. Ready to receive new impressions, he was every whit as ready to correct old views. Nine editions of the *Principles of Geology* had appeared, and in every one of them he had maintained the doctrine of Special Creation. The only explanation which at the time seemed possible to him of the perpetual change of life revealed by the successive strata was, that when the material conditions of any district became so changed that the old inhabitants died out, a new creative fiat went forth, by virtue of which the district was again peopled with fresh inhabitants especially adapted to its new conditions. When Darwin showed that causes were at work which slowly and gradually modified the characters of plants and animals, so that they became adjusted by a self-adapting process to the changing circumstances around them, he gladly adopted a view which was so much in harmony with his general principles. Accordingly in his tenth edition he renounced the doctrine of Special Creation and adopted that of Evolution. Sir J. D. Hooker, in his address to the British Association at Norwich, adduced

* Horner, *Life of Sir C. Lyell*, I, p. 317.
† *Life*, I, p. 89.
‡ *History of England*, II, p. 334.

as a signal example of heroism the fact that an author could thus abandon "late in life, a theory which he had for forty years regarded as one of the foundation-stones of a work that had given him the highest position attainable among contemporary scientific writers." Yet, if the facts upset the theory so completely as Lyell imagined, is there anything really heroic in his action? Or does Hooker think that when a scientist holds a theory for a long time, the scientist, in spite of facts, will persist in maintaining it? Even Darwin wrote, "Considering his age, his former views, and position in society, I think his conduct has been heroic on this subject." * T. H. Huxley once met Herbert Spencer in the Athenæum. Wearing a lugubrious expression Spencer remarked, "Oh, Huxley, there has been a tragedy in my house this morning." Without waiting to hear its nature, Huxley at once retorted, "Oh, I know what has happened. A beautiful scientific theory has been killed by one nasty inconvenient fact." If one may judge from the words of praise bestowed on Lyell by Hooker and Darwin, the real tragedy is that with little regard for facts, not a few scientists persist in expressing opinions that ignore the "one nasty inconvenient fact." It was part of the good fortune of Sir Charles Lyell that he had suffered much from opposition. He had learnt toleration by bitter experience in early life. He had endured the criticisms of the Wernerians, who, obsessed by their own leading conceptions, refused to admit that other views were possible. Lyell had shared the fate which usually falls to—

> Teachers whose minds move faster than the age,
> And faster than society's slow flight.

Philosophic to the core in his outlook on the world, he ever retained that mental plasticity which seldom attends the scientist. John Henry Newman held that "in a higher world it may be different. But here below to live is to change, and to be perfect is to have changed often." In that sense Lyell was always advancing towards perfection. From such a point of view it is surely obvious that when Lyell discarded the doctrine of Special Creation, he was acting in no manner to occasion surprise. That it occasioned surprise is enough to testify that in the struggle for truth the scien-

* *Life and Letters of C. Darwin*, II, p. 326.

tist is swayed just as truly as any other thinker by the prepossessions of the doctrines in which he has been brought up.

There was a time when it was easy to jeer at the omniscience of the Rev. William Whewell, Master of Trinity College, Cambridge, yet he displayed keen sympathy with the views enounced by Lyell. In spite of the pronounced opposition of the geologists, Whewell announced in November 1830 to the startled University that Lyell had discovered a new set of powers in nature which might be termed geological dynamics. Whewell set to work to write an article for *The Quarterly Review*, and in *The British Critic* he warmly entered into the new conception of geology, seeing the bearing of uniformity on the subject, and explaining it in a clear way.

The storm soon burst on the devoted head of Lyell. He was afraid of the hostility of Oxford University, asking that Poulett Scrope's article in *The Quarterly Review* on this account may be toned down. Hard as Conybeare had worked to secure Lyell the chair of geology at King's College, London, he fired what Lyell termed " an explosion " against the *Principles of Geology*. The friend of Elie de Beaumont and Sir Henry de la Beche, Conybeare was sufficiently eminent in geology to render his opposition serious. Steadfast as he had once been on the Wernerian side, C. G. B. Daubeny joined forces with Conybeare, and his open-mindedness lent strength to all he said.

In his letter of April 7, 1831, to his sister Marianne Lyell plainly thought that the attack of Adam Sedgwick was the severest. Admirable as an observer, lucid and brilliant as an expositor, he had been Woodwardian Professor of Geology at Cambridge since 1818, and in 1831 attained the dignity of the presidency of the Geological Society. Enthusiastic and earnest, Sedgwick had a keen eye for the testimony of the rocks, and could shed the charm of his own genial nature over all his observations. The vigour, the originality, and the eloquence with which he could set forth his views were enough almost to daunt the courage of Lyell. Just as Dean Milman had helped by his *History of the Jews* in giving the geologist a hearing, so Archbishop Whately helped by his lectures on Political Economy to diffuse the cold light of reason as the only test of truth. Whately said that the cry against economics is "louder 'than against geology,' because people will admit that the sacred writings were not to teach us physics, but say that a science connected with human concerns should be in accordance even with the letter." * As Lyell notes this on August 13, 1831, it is plain that the historian and the political economist, who were both in Holy Orders, were extending assistance to the new views of the geologist. The mental atmosphere was, then, altering. Though Sedgwick had discarded the Wernerian hypothesis, yet in 1830 from the presidential chair of the Geological Society he criticised the leading argument of the *Principles of Geology* in no friendly spirit. He considered that "my [i.e. Lyell's] mode of explaining geological phenomena, or my bias towards a leading doctrine of the Huttonian hypothesis, had served like a false horizon in astronomy to vitiate the results of my own observation." † Nor did Sedgwick ever see reason to change his attitude, for in a letter of October 6, 1855, Lyell writes: "Sedgwick's attempt to take the Lower Silurian into his Cambrian is even worse than Murchison claiming all that is older than the Devonian as appertaining to his Silurian." ‡

Much of the early labours of Lyell and Murchison had been in common, and it is intelligible that Lyell was taken aback when he found he had to face the active dissent of his sometime colleague. Not many scientists retain complete possession of their youthfulness and pliability of mind at the close of a long life. Lyell was one of them, and Murchison was certainly not another. Lyell and Murchison, like Darwin and Lecky, were wealthy men able to devote themselves to the pursuit of truth. That pursuit demands *inter alia* pliability, plasticity of mind, and this was not a quality possessed by Murchison. With characteristic obstinacy he fought against the uniformitarian doctrines in the organic as well as in the inorganic history of the world to his death in 1871. The concluding pages of the last edition of his *Siluria* reiterate his faith in a former greater intensity of the operations of nature. From the chair of the Geographical Society, as Sedgwick from the chair of the Geo-

* Horner, *Life of Sir C. Lyell,* I, p. 322.
† *Ibid.,* II, p. 4.
‡ *Ibid.,* II, p. 206.

logical Society, the Lyellian views were proscribed, though naturally his greatest vigour of denunciation was reserved for his private correspondence. Nor indeed can we deny that the weight of official authority was employed for crushing the new "heresy," for so Murchison regarded it.

Murchison never used the argument commended by the solicitor to the barrister, "No case: abuse your opponent." He attached weight to the part played by glacier-erosion. The force of the evidence brought forward by Lyell had constrained him to yield somewhat of the old exclusiveness with which he had fought for his icebergs. Some of his points had to be modified. He consented—rather reluctantly—to admit the powers of glaciers to polish and score the face of a country, and to pile up huge moraine-mounds. This was, however, the extreme limit of his concessions. He felt himself free to set his foot down firmly and refuse to go a step further in the way of excavation than his friends the "ice-men" would have him go.

Once upon a time Murchison had laughed at the stubborn adherence of men like Greenough to the *antiquas vias*. The day came when he himself was to stand just as firmly in the old ways. A letter he wrote to Sir William Denison on October 6, 1864, forms saddening reading: "In my Anniversary Address to the Geographical Society you would see the pains I have taken to moderate the ice-men, who would excavate all the rock basins by glaciers eating their way into solid rocks. . . . In seconding the motion of thanks to Lyell for his address at Bath, I felt bound to say a few words in defence of my opinions as to the grander intensity of causes in old geological times than in the present or Man period; and as Lyell had used the words 'some great convulsion and fracture,' to account for the great rent and fault out of which the hot Bath water flows, I said I was happy to receive that indication of the right view, and that I should in future range my friend Sir Charles along with myself among the 'convulsionists.' And again, I entirely disagree with him when he adverts with triumph to the discovery of animal life in the old Laurentian rocks of North America, that this is any indication that we have here 'no trace of a beginning.' On the contrary, the only animal which has been found, being a zoophyte, adds nothing and changes nothing in the general argument founded on the indisputable facts recorded all over the world, viz. that there has been a progression of creation from the lowest grades of animal life up to man." * Facts he had gathered all his life with that untiring perseverance that does him honour. His Silurian system had thrown light on these facts. The pity was that for the rest of his days all facts must be interpreted by their bearing on the Silurian system, and facts are too much for such a theory.

The ranks of Conybeare and Daubeny, of Sedgwick and Murchison, were joined by W. H. Fitton, who had laid down the proper succession of the strata between the oolite and the chalk; by William Lonsdale, joint originator with Murchison and Sedgwick of the theory of the independence of the Devonian system; and by Henry Samuel Boase, F.R.S., who investigated Cornish geology. Nor would Agassiz give up the catastrophe system. Fitton regretted from the historical standpoint that the fact that James Hutton had anticipated the *Principles of Geology* was inadequately noticed. Lyell retorted that Steno in 1669, Hooke in 1705, and Moro in 1740 deserved as much credit as Hutton, and that his earlier chapters dealt equally with all. Fitton felt natural indignation at the unpardonable neglect with which the French and Germans had treated Hutton, and he thought he perceived similar neglect on the part of Lyell. Lonsdale laid stress on the arguments put forward by Elie de Beaumont and by Sedgwick, though Lyell succeeded in modifying this objector's views.

If opposition was vocal in England and America, it was also vocal in France and in Germany. Elie de Beaumont in 1868 still believed in the general sudden formation of the organic world and of the particular sudden formation of mountain chains. In 1857 there was a vacancy in the French Institute and de Beaumont thought that Lyell was anxious for election. De Beaumont frankly opposed Lyell, writing to him "to let me know, in return for my enmity to his opinions (or as they always say in Paris, to himself, 'mes ennemis,' etc., meaning my theoretical opponents) that he had the will and power to thwart me in what he really imagines is the great object of everyone's ambition. His message did not open my

* Sir A. Geikie, *Memoir of Sir R. Murchison*, II, p. 318.

eyes to his course in the election, for I knew that before, but was a gratifying testimony to the existence of a party in my favour." *

Another opponent was Joachim Barrande, who had investigated the extraordinary abundance and variety of Silurian fossils in Bohemia. This keen observer noted the equivalents of Murchison's Upper and Lower Silurian series, and he also noted below that series a still older group of strata. He discovered a 'colony' of Upper Silurian fossils 3,400 feet deep, in the midst of the Lower Silurian group. These fossils he regarded as "colonies" which reappeared at higher horizons. This constituted a break in the theory formulated by Lyell. With that loyalty to truth characteristic of him Lyell investigated these fossils in 1856. "I never," he confessed, "saw Silurian fossils in such abundance except in a few strata in Sweden; but here they pass through many thousands of feet. Yet the whole fossiliferous area is only equal to one-sixtieth part of the Adriatic. As Barrande himself has calculated this, I wonder he remains such a finality man. I remember at the Geological Society when Sedgwick and Murchison used to argue with me exactly on the grounds now taken up by Barrande in proof of a beginning of life on this globe, founded on the notion that no fossils would ever be found below the stiper stones. Now that a totally distinct fauna has turned up, and that the transformation of some are traced from the egg to the adult, the discoverer is just as sure that here at least we have the true beginning." †

The German critics, Lyell notices on June 1, 1836, were attacking him vigorously. They held that by impugning the doctrine of spontaneous generation, and substituting nothing in its place, he had left them nothing but the direct and miraculous intervention of the First Cause, as often as a new species is introduced. Hence, in their judgment, he had overthrown his own doctrine of revolutions, carried on by a regular system of secondary causes. The tenth edition was to meet attacks like this. Serious as was the opposition of Murchison and Sedgwick in England, the opposition of Leopold von Buch in Germany was just as serious. He was

* Horner, *Life of Sir C. Lyell*, II, p. 243.
† *Ibid.*, II, p. 226.

the most illustrious geologist that Germany had produced With Alexander von Humboldt he had attended the lectures of Werner at Freiburg. Conservative by nature, von Buch possessed width of knowledge and shrewdness of observation. To physical geography and palæontology, to dynamical and stratigraphical geology, he made original contributions. He was quite as philosophic and almost as travelled as Lyell himself. "We must," held Lyell, "preach up travelling as the first, second, and third requisites for a modern geologist," * a doctrine to which von Buch would readily have subscribed. He resembled de Beaumont in conceiving personal hostility towards those who did not embrace the theoretical doctrines which he published. He experienced difficulty in breaking with all that Werner had taught him as to the aqueous origin of basalt, and all that he himself thought he had perceived in his extended journeys through his fatherland. The whole doctrine of the chemical precipitation of the rocks of the earth's crust was at stake. If he surrendered at one point, where was he to stop? The sight of the volcanoes and basalt-hills of Italy and Central France and the proofs of the recent uprise of Scandinavia widened his geological horizon, which was still dominated by the sun of Werner. Glacial geology of the type propounded by Lyell he could not bear. When Louis Agassiz put forward such a view, von Buch "could hardly contain his indignation mingled with contempt, for what seemed to him the view of a youthful and inexperienced observer." † On March 24, 1855, Lyell writes: "It is strange what influence von Buch exerted, for he had made both Ewald and Beyrich entirely disbelieve all the glacial hypothesis. The other day I told Mitscherlich I would convert them both, and he said (both of them being present and laughing at the joke), 'No you will never do that, for the one' (pointing to Beyrich) 'is like a stone, and the other like india-rubber; you think you are making a great impression, and then find next day that up he comes again just in his former shape.'" ‡

Lord Kelvin no more accepted in its entirety the uniformitarian doctrine than he accepted its supplement, the evolu

* Horner, *Life of Sir C. Lyell*, I, p. 233.
† E. C. Agassiz, *Louis Agassiz, his Life and Correspondence*, I, p. 264
‡ Horner, *Life of Sir C. Lyell*, II, p. 203.

tionist doctrine. He introduced from Kant's *Collected Works* his remarks in the following parable: "A large proportion of English popular geologists of the present day have been longer contented than other scientists to look upon the sun as Fontenelle's roses looked upon their gardener. 'Our gardener,' they say, 'must be a very old man; within the memory of roses he is the same as he has always been; it is impossible he can ever die, or be other than he is.'" *

* S. T. Thompson, *Life of Lord Kelvin*, I, p. 539.

CHAPTER IV

HELMHOLTZ, JOULE, AND THE CONSERVATION OF ENERGY

THE advocates of the advantages of a mixed ancestry can turn with considerable confidence to the career of Hermann Ludwig Ferdinand von Helmholtz. He was the son of Ferdinand Helmholtz, a teacher of philology and philosophy in the Potsdam Gymnasium, a man of high culture and high intelligence. His mother was the daughter of a Hanoverian artillery officer of the name of Penne, a lineal descendant of William Penn, the Quaker who founded Pennsylvania. She possessed the faculty of penetrating obscure points by intuition, a faculty she transmitted to her son. The grandmother on his mother's side sprang from a family of French refugees, of the name of Sauvage. Thus Helmholtz had the blood of England and France as well as of Germany coursing through his veins when he was born on August 31, 1821. The width of the literary studies of the father is perhaps seen in the comprehensive scientific tastes the son was to develop. Is there a trace of any hereditary aptitude for mathematics?

The little we know of his early life was revealed by Helmholtz in a speech delivered in 1891, in reply to the toast of his health at a banquet given in honour of his seventieth birthday. For the first seven years of his life he was a weakly boy, confined for long periods to his room, and frequently to his bed; but he was fond of such amusements as were possible, and he evinced wonderful activity of mind. Thanks to his wooden bricks, he already knew, like Pascal, all the facts the masters of his geometry class expected him to learn. Frau von Bernuth, his father's cousin, daughter of Surgeon-General Mursinna of Berlin, assured the parents by the example of Alexander von Humboldt, who learnt nothing before he was eight, "and now the King has made him President of the Academy of Sciences, with the title of Excellency, and a big yearly stipend—and this is what I predict for your son." The lad's health improved by degrees with gymnastics and daily bathing, and his keen love of nature was developed by his regular walks with his father in the beautiful environs of Potsdam. His father encouraged him to study languages and literature. The quality of his mind, however, did not fit him for following in his father's steps. While the class read Cicero or Virgil, which did not interest him, he was often engaged beneath the table in working out the passage of rays through the telescope, or in learning some of the optical theorems that served him in good stead later on in the construction of the ophthalmoscope. It is curious to find that in the textbooks he read on physics and chemistry no attention had been paid to the discoveries of Antoine Laurent Lavoisier and Sir Humphry Davy. Phlogiston still played its part and galvanism ended with the voltaic pile.

At school he met with no difficulty in learning off by heart the poems of the great masters, though he found the task far from easy when the verses were by second-rate poets. History, as it was taught in those days, was beyond him. It was a real torture to him to commit prose extracts to memory. Indeed, in the lower classes, he felt hampered by the want of a clear recollection of facts if they were disconnected. He even found it hard to distinguish between left and right. It is a remarkable indication of the breadth of his early education that he was able to read the fables of Lökman in the original Arabic when he was twelve years of age. His father exercised him in the composition of essays and verses, and Helmholtz remarks that although the verses showed that he was a poor poet, the practice proved invaluable to him in the way of training him to the proper use of forms of expression. He also mentions that he used to listen to the philosophical discussions between his father and his friends, thus growing familiar with some of the problems of metaphysics as enunciated by Kant and Fichte.

The home of the boy was decidedly intellectual, if it was not scientific. Indeed it may well be that such an atmosphere was best fitted to develop the many-sidedness of the future scientist. Algebra and geometry were the keys with which he hoped to unlock the secrets of physical phenomena that increasingly attracted him. Of course he performed experiments, to the detriment, he confessed, of his mother's linen and furniture. He constructed optical apparatus with a few spectacle glasses and a small botanical lens belonging to his father. Filled with a passionate enthusiasm for the causes of phenomena, he never felt satisfied with the apparent solution of any problem, if there were still doubtful points in it, and these he invariably endeavoured to clear up by bringing them fairly before his mind. Caring for art and science, he also cared for music and poetry. Nor is it devoid of significance that some of the greatest masters of mathematical physics, like Kelvin in the past and Einstein in the present, have been fervently devoted to music.

While still in the second class of the Gymnasium, Helmholtz announced to his father that he had found his vocation in life, and that it was undoubtedly science. The philosopher was forced to tell him that he had to educate four more children, explaining that he could not afford to provide him with instruction in physics unless he also took up the study of medicine. The lad acquiesced in this decision. As early as 1835 his father applied for his admission to the Royal Friedrich-Wilhelm Institute of Medicine and Surgery in Berlin, which gave considerable assistance to medical students. For it guaranteed them a complete course of study and means of livelihood in return for a certain number of years' service as army surgeons. Thanks to the practical assistance of his relative, Surgeon-General Mursinna, he obtained admission as bursar to this Institute in 1838. Work was strenuous. There were forty-eight lectures in the week. There were six on Chemistry by Mitscherlich, six on General Anatomy, four on Splanchnology, three on Osteology, three on the Anatomy of the Sense-organs. In addition to these there were lectures on Osteology in the anatomical theatre. Hecker gave two lectures on General Medicine. Then there were four on Physics by Turte, two on Logic by Wolf, three on History by Preuss, two on Latin by Hecker, and one on French by Pastor Gosshauer. Beside these the student had twelve hours of revision classes. In spite of the severity of his attendance at classes, his spare time was spent on music, even on the worst days practising for an hour. He played sonatas of Mozart and Beethoven, to whom he was as devoted as Bismarck, and any new pieces he got hold of. In the evenings of his first year he read Byron and Goethe, and sometimes for a change the integral calculus. In his list of lectures there is no mention of mathematics, yet his thoughts often strayed to this subject. No one, however, fostered his talent for it, and it is significant of the silence with which he pursued it that some of his early friends, such as Brücke and Du Bois Reymond, who were his fellow-students, remained unaware of the attention he was bestowing on the problems of analytical geometry. He had no teacher save his own genius. One wonders what might not have happened had he had a teacher of such transcendent gifts as William Hopkins or Edward John Routh.

For the development of his mathematical powers he had to rely on himself. He continued during his scanty hours of leisure to devote himself to music and poetry. He read Homer and Byron with the same avidity as he devoured Biot and Kant. He found time to take part in amateur theatricals, enjoying a splendid performance of *Euryanthe* and admiring also Seydelmann's Mephistopheles and Clara Stich's Gretchen. Nor did he stand aside from the growing national feeling, watching its developments with eager interest. Indeed in what was he not interested? More and more he felt drawn to the teaching of that master physiologist, Johannes Müller, the greatest living force then in the University of Berlin, the Cuvier of Germany.* What John Hunter † accomplished in England, he accomplished in Germany, for he became the most outstanding biological teacher of his time. He left the deepest impression upon all who were fortunate to come into contact with him. Among his pupils were Schwann and Henle in anatomy, Brücke and Du Bois Reymond in physiology, and Virchow in pathological anatomy. Nor was his influence confined to Germany.

* E. Du Bois Reymond reprints his *Gedächtnissrede auf Joh. Müller*, with extensive notes, in his *Reden*, II, pp. 143-334, especially 219 ff.; cf. T. von Billroth, *Lehren und Lernen der medicinischen Wissenschaften*, pp. 307-66.

† Audubon, Cuvier, Benjamin Franklin, Gladstone, von Humboldt, John Hunter, Samuel Johnson, Lord Kitchener, Scott, and Wagner had fathers of over forty.

Donders in Holland; Claude Bernard and Vulpian in France; Sir William Bowman, William Sharpey, and William Benjamin Carpenter in England; and Allen Thomson and John Goodsir in Scotland, all acknowledged the seminal ideas Müller was sowing. What William Harvey and Charles Bell did for physiology in England, Albrecht von Haller (1708—1777) and Johannes Müller (1801—1858) did for it in Germany. Helmholtz lived entirely in the circle of Müller's pupils, since he had already formed a friendship with the physiologists Brücke and Du Bois Reymond, who were two years senior to himself, and like him devotedly attached to their suggestive professor. What this intercourse meant to him Helmholtz owned when he said, half a century later—so lasting are the impressions of youth—"Whoever comes into contact with men of the first rank has an altered scale of values in life. Such intellectual contact is the most interesting event life can offer."

Suggestive as the intercourse with Müller undoubtedly was, the intercourse with the students of his day at the Institute was no whit less powerful. For while the matured mind can stimulate, young mind in touch with young mind can kindle the flame as nothing else can. Du Bois Reymond, who in due time became Professor of Physiology in the University of Berlin, Brücke, who was to hold the same chair in the University of Vienna, and Virchow, who was to become Professor of Pathology in the University of Berlin, were among the young men who felt that their master was unquestionably right in the emphasis he laid on the investigation of biological problems by the methods of physical and chemical science. Fortunately at this time the chair of Physics in the University of Berlin was held by Gustav Magnus, who felt repelled by the assumptions of the metaphysical school and attracted by the experiments of the school of Johannes Müller. Clausius and Heintz, Gustav Karsten and Knoblauch, Kirchhoff and Werner Siemens, Quincke and Wiedemann, Beetz and Tyndall in physics or in chemistry advocated the methods employed by Brücke, Du Bois Reymond, and Helmholtz in physiology. They formed "the Physical Society," and in it members from the physical and the physiological sides met on equal terms, though as Brücke, Du Bois Reymond, and Helmholtz approached physiological problems from the physical standpoint the physicists tended to gain supremacy. The metaphysicians had assumed that the fundamental problem of vital action was quite beyond the domain of experimental science, and "the Physical Society" implicitly set out to combat ideas that could not be demonstrated.

Helmholtz never believed that the years he spent in medical study were wasted. Nor were they. In a lecture on "Thought in Medicine," delivered in 1871, he remarked: "My own original inclination was towards physics; external circumstances obliged me to commence the study of medicine. It had, however, been the custom of a former time to combine the study of medicine with that of the natural sciences, and whatever in this was compulsory I must consider fortunate; not merely that I entered medicine at a time in which anyone who was even moderately at home in physical considerations found a virgin field for cultivation, but I consider the study of medicine to have been the training which preached more impressively and more convincingly than any other could have done, the everlasting principles of all scientific work; principles which are so simple and yet are ever forgotten again; so clear and yet always so hidden by a deceptive veil." *

In 1842 Helmholtz, at the age of twenty-one, presented his inaugural thesis, entitled *De Fabrica Systematis nervosi Evertebratorum,* or "The Structure of the Nervous System in Invertebrates," and it was naturally dedicated to Johannes Müller. In 1833 Von Ehrenberg discovered in ganglia, which are usually small, more or less rounded swellings on nerves, often situated at the apparent junction of several trunks, peculiar cells or corpuscles. These cells are also found in all nerve centres, such as the spinal cord and brain, and they lie in a fine variety of tissue, while numerous nerve centres pass through the ganglia, apparently in close proximity to the cells. Was there a connection between the nerve cells and the nerve fibres? Müller had taught that there was in all probability a connection. It was reserved for Helmholtz, with a very simple and primitive form of a compound microscope, to discover in the ganglia of leeches and crabs that the nerve fibre originates from one of the cor-

* *Popular Lectures,* 1881, p. 202.

puscles. What the master had guessed the pupil had discovered, thus furnishing a first-rate contribution to minute anatomy. It was the first of the long series of discoveries which added to our knowledge of no less than seven sciences. As each of seven cities contended for Homer, so seven sciences, mathematics, physics, chemistry, physiology, medicine, philosophy, and æsthetics, claimed Helmholtz. From 1842 to 1894, the year of his death, paper after paper flowed from his indefatigable pen. With the exception of one year, 1849, he always published at least one important paper, usually three or four, each year, so that he had to his credit a grand total of no fewer than 217 distinct papers and books. It is a record just as impressive as Mommsen's for its quality as for its quantity.

His discovery in 1842 showed what he might have accomplished in the realm of anatomy. He was a man who took the practical side of medicine as seriously as did Sir James Paget. In his lecture on "Thought in Medicine" he points out that "perhaps only he can appreciate the immense importance and the frightful practical scope of the problems of medical theory, who has watched the fading eye of approaching death, and witnessed the distracted grief of affection, and who has asked himself the solemn questions, Has all been done which could be done to ward off the dread event? Have all the resources and all the means which science has accumulated become exhausted?" *

The scientific atmosphere in which he had lived with Johannes Müller and his congenial friends he exchanged for his duties as Hussar-Surgeon attached to the regiment of Red Hussars, stationed at his old home, Potsdam. Such was his work from 1842 to his appointment to the chair of Physiology in Königsberg in 1849. Private practice he never had, and all his time off duty he devoted to science. He arranged a small laboratory for physics and physiology in the barracks, where he was frequently visited by Du Bois Reymond and Brücke, who came out from Berlin to discuss their plans for the future reconstruction of physiology. True, the instruments were as elementary as those employed by Michael Faraday, but the mind is what really matters. Besides, there was the constant advice and assistance which

* *Popular Lectures,* 1881, p. 203.

Du Bois Reymond tendered, who, he writes, " tended me like a mother, to enable me to attain a scientific position." At once he set out to investigate the metabolism in muscular activity, to embark on a series of laborious experiments on the conduction of heat in muscle, and the rate of transmission of the nervous impulse.

There were metaphysical presuppositions in the mind of his father, and even Müller was not free from the older quasi-metaphysical position, though in his later years he also adopted the views of Ernst Heinrich Weber. He was the first to demand an explanation of the phenomena of life by examination of these phenomena by physical methods and by the application of physical laws. Helmholtz was close to his own home, and he increasingly felt that his whole scientific attitude was irreconcilable with the wholly speculative philosophy of his father. Ferdinand Helmholtz admitted only the deductive method in science and refused to admit the inductive. His father, Hermann, on the other hand, proclaimed with all the vehemence of a young discoverer that the use of inductive reasoning constituted the salvation of science in general and of the physical sciences in particular. This is the clash of two generations which Edmund Gosse has so pathetically portrayed in *Father and Son,* and it is also the clash of men influenced by two sets of conflicting presuppositions. We meet it in the case of Clara Schumann, whose father opposed her marriage with the famous musician because his son-in-law-to-be belonged to a school of music different from his own, and he foresaw that when his daughter married she would fall not under her father's influence but under her husband's. Treitschke the father was devoted to Saxony, while his son was devoted to Prussia with consequences which can still be felt. The outcome in the case of Helmholtz was that he came to shun discussion of his experiments with his father. The old man felt the altered nature of the relationship keenly, but he came at last to submit to it.

Definite and methodical experiment was the only method by which he could advance the general principles of science. " Vital forces "—if there were such forces—must be brought within the scope of the laboratory. The Physical Society, like Bacon, took all knowledge for its province. Müller had again and again raised such questions as whether the life of organisms was the effect of one special, self-engendered, definitely directed force, or merely the sum of the forces that are effective in inorganic nature also, modified only by the manner of their occurrence. Baron von Liebig, with all the authority of his commanding position, transformed these questions into the far more concrete problem of whether the mechanical energy and the heat produced in an organism could entirely result from its own metabolism, or not. Behind these questions Helmholtz perceived that there was a common bond in the validity of that law of Conservation of Energy which had for years seemed incontestable to his mode of thought. Proof was wanting. Nevertheless, he felt convinced of its validity. The outer world was devoid of his intuition, and for it there was necessary the proof to be derived from endless experiments in different regions of physics and physiology. Of the truth of his mathematico-physical conception he entertained little doubt. In 1845 he set about testing the accuracy of his physical conceptions upon a highly complex physiological problem, and the result was the paper he published in Müller's *Archiv,* entitled "Metabolism during Muscular Activity."

A paper like this cannot be understood nor its significance appreciated unless we know something about the previous history of the attempts to solve the problems of fermentation and putrefaction. From time immemorial, as Sir Rickman J. Godlee observes in his fine biography of *Lord Lister,* it was common knowledge that grape juice turned into wine, that beer was made from malt, and vinegar from wine, by similar processes. The fermentations which lead to the production of wine and bread have been known since the prehistoric period, and must have been among the first of natural phenomena which man learnt to control and adapt to his needs. They attracted the attention of philosophers who, in a succession from the early alchemists through Paracelsus to Stahl, played with ideas and did little more than show the absurdity of the older ideas which they in turn replaced by new verbal fantasies. Then came the era of chemists, who had learned how to handle gases and to distinguish one from the other. The nature of fermentation was supposed to be highly mysterious, and accordingly the scientific men up to the beginning of the nineteenth century passed it by. The chemists of the eighteenth century had spent precious time in disputes about the number of the alchemical principles and the phlogistic theory. It occurred to but few of them that they ought to test their pre-conceived notions by experiment. The ferment of the French Revolution shortened the days of Lavoisier, a founder of modern chemistry, who was the first to study fermentation scientifically. The Republic proclaimed it had no need for savants of the type of Lavoisier or Condorcet, Bailly or Cousin, Vicq-d'Azyr or Dionis du Lejour, and exterminated them, though nothing like so ruthlessly as the early Russian Revolutionists. Lavoisier regarded fermentation as a purely chemical process and felt only interested in observing the chemical changes he perceived. He introduced the chemical balance, and showed that when a solution of sugar was fermenting under the influence of a little yeast from beer, the sum of the weights of the alcohol and carbonic acid that were produced was very nearly equal to the weight of sugar that was destroyed. He carried the analysis further, and came to the wrong conclusion that if it were possible to recombine the alcohol and the carbonic acid, the sugar would be recovered. He had already shown by other experiments that a man who is doing work requires more oxygen than a man at rest. Obviously certain ponderable or imponderable substances were consumed in the production of mechanical effects, and were renewed by vegetative vital processes. He also noted that the amount of excreted nitrogenous matters was increased by muscular activity. The matter to be investigated was, What were data concerning the initial and the intermediate steps of the process? What precisely was the seat of its occurrence?

Gay-Lussac, the pupil and the friend of Berthollet, had turned his attention in manhood to physics, and in middle life he changed to chemistry. In 1810 he carried on the experiments stopped by the execution of Lavoisier in 1794.* They had been suggested to him by what M. Appert, who had no scientific education, had found out. Gay-Lussac published the results of a series of these experiments on fermentation.

* *Annales de Chimie,* 1810, LXXVI, p. 245. Here my indebtedness to Sir Rickman J. Godlee is very heavy.

M. Appert, a confectioner or food-purveyor, had preserved alimentary substances by the received empirical methods, such as desiccation or pickling, and "having spent 45 years in this business," he records, "I have been able to avail myself, in my process, of a number of advantages which the greater number of those persons have not possessed who have devoted themselves to the art of preserving." In his book on *The Art of Preserving all Kinds of Animal and Vegetable Substances for Several Years* he sets forth his results. By order of the French Minister of the Interior in 1810 it was published in a report of the Board of Arts and Manufactures. Appert's plan was to place the substances he wished to preserve in very carefully corked bottles, and to keep them for a longer or shorter time at the temperature of boiling water. The bottles were filled nearly full. Practically, the results were almost always successful. In fact Appert anticipated the modern methods of bottling fruit and vegetables. To his extreme surprise Gay-Lussac found that grape juice thus preserved for a year fermented in a few hours if decanted into another vessel. He therefore assumed that oxygen was necessary for starting the fermentation of grape juice, though he was forced to own that it might not be necessary for carrying on the process when it had once been set going, or even for starting the growth of the yeast.

Gay-Lussac appealed to the experiments conducted by Lavoisier, Fabroni, and Baron Thenard. The last was not merely an experimenter but also belonged to that rare class of great professors. He could truthfully boast that he had had 40,000 pupils, and that he had left the impress of his winning personality on all of them. The results of the experiments of these three men had been to show that for the development of alcoholic fermentation it is necessary to bring together a saccharine matter and a particular ferment "de nature animale." Gay-Lussac goes on to say that it has been asserted that this can take place in the absence of oxygen. But can it? For such an argument assumes that all living ferments are identical, a proposition he vigorously contests. By a series of experiments he found he was right, and he succeeded in convincing Thenard that he was right. Gay-Lussac concluded that "the ferment of the grape is not of the same nature as the yeast of beer, or rather that they are not by any means both of them in the same condition." * A step had been taken, but that was all. Gay-Lussac was only too well aware that "fermentation still seems to me one of the most mysterious of chemical processes; especially because it only operates gradually, and because we cannot understand why, when the ferment and the sugar are intimately mixed together, they do not act upon one another more rapidly. One would be tempted to believe that it is partly due to a galvanic process, and that it has some analogy with the mutual precipitation of metals." † Plainly, he was straying away from the path of the true solution.

From 1810 to 1835 nothing further was done. Pasteur was a boy of fifteen and Lister was only eight, when from two or three quarters interest in the problem revived. The darkness of the subject was pierced by a momentary ray of light in 1836. Another distinguished French chemist, Cagniard-Latour, presented his observations in a paper sent to the Académie des Sciences on June 12, 1837. He was the first to call in aid the use of the microscope. He had been working for twenty-five years, he said, at first with very imperfect instruments, but more recently with better ones by Georges Oberhauser and Giovanni Battista Amici which magnified 300 and 400 diameters.

Cagniard-Latour tabulated the result of his investigations as follows:

1. Yeast is a mass of small globules which, as they can reproduce themselves, are organic and not simply a chemical substance, as was before supposed.

2. These bodies appear to belong to the vegetable kingdom and to reproduce themselves in different manners.

3. They appear to act on a saccharine solution as long as they retain their vitality, "from which it is fair to conclude that very probably it is some effect of their vegetation which sets free the carbonic acid whilst converting the sugar into a spirituous liquor."

To these main conclusions he added three other minor propositions:

1. That yeast can develop and increase under some circum-

Annales de Chimie, 1810, LXXVI, p. 246.

† *Ibid.*, p. 247.

stances with great speed, even in the presence of carbonic acid, as in the brewer's vats.

2. That the manner in which it grows is different from that previously observed in the case of similar microscopic organisms.

3. That it retains its vitality even after exposure to the low temperature obtainable from solidified carbonic acid.

Gay-Lussac had been puzzled by the slow and progressive action of a living ferment. Cagniard-Latour solved the puzzle when he demonstrated that this action depended upon the gradual growth of an organism. The ferment of beer called yeast was, in short, composed of cells "susceptible of reproduction by a sort of budding, and probably acting on sugar through some effect of their vegetation." Lavoisier, Fabroni, Baron Thenard, Gay-Lussac, and Cagniard-Latour were all Frenchmen who approached the problem from the physical angle. Now it was the turn of Germans to approach it from the physiological angle, though it was to be reserved for the Physical Society to approach problems from all angles.

How indispensable a new line of approach was is clear when a man like J. B. Dumas, who was to be the academic sponsor for Pasteur, said that perhaps there might be a sequel to Cagniard-Latour's statement. So late as 1853, Anglada, in his book *On Contagion,* expressed himself thus: "M. Dumas, who is an authority, looks upon the act of fermentation as strange and obscure; he declares that it gives rise to phenomena the knowledge of which is only tentative at present. Such a competent affirmation is of a nature to discourage those who claim to unravel the mysteries of contagion by the comparative study of fermentation. What is the advantage of explaining one through the other since both are equally mysterious!" There was evidently comfort to be derived when you labelled phenomena obscure or mysterious, and greater comfort when you labelled them both obscure and mysterious. Berzelius, the Swedish chemist, thought that fermentation was due to contact. Was there not supposed to be a catalytic force? In his opinion, what Cagniard-Latour believed he had seen was but "an immediate vegetable principle, which became precipitated during the fermentation of beer, and which, in precipitating, presented forms analogous to the simpler forms of vegetable life, but formation does not constitute life."

The heart of Helmholtz rejoiced to know that the first German physiologist to get close to the solution of the vexed question of fermentation was a friend and pupil of Johannes Müller, Theodor Schwann. Working independently of Cagniard-Latour—and in complete ignorance of his labours —Schwann wrote his first paper a few months before that of the French physicist, though it was not published till a few months later.* Schwann gave the first real proof that the vegetable cells caused fermentation. Of course his proofs encountered opposition from all who held the widely diffused belief in spontaneous generation. The facts he adduced went a long way to prove the truth of his conclusion. In true scientific spirit, he admitted that he had met with nasty, inconvenient facts which he could not account for. It is of course the only justifiable method of presenting conclusions, but it is not exactly the way to produce conviction. In spite of the corroboration of his striking facts put forward by men like Franz Schülze and Schroeder, the scientific world remained unconvinced.

From the days of Lavoisier to those of Schwann there had been marked advance. One of the then great men in the world of science was Baron von Liebig, who was wont to make oracular pronouncements on many questions. He contemptuously brushed aside the arguments of Cagniard-Latour and Schwann, and as his reputation was world-wide men paid him attention. In his paper, " Sur les Phénomènes de la Fermentation et de la Putréfaction, et sur les causes qui les provoquent," he allows that the microscope reveals the presence of certain globules in the deposit that takes place during fermentation. He goes on to point out that "the appearance they present in these circumstances has induced certain savants to adopt the view that the ferment consists of organised living beings, plants or animalcules, which, in order that they may be able to develop, assimilate the elements of the sugar and give them off as excrement in the form of carbonic acid and alcohol; this is how they explain

* "Vorläufige Mittheilung betreffend Versuche über die Weingährung und Fäulniss," von Dr. Th. Schwann im Berlin; *Annalen der Physik und Chemie,* 1837, XLI, p. 184.

the decomposition of the sugar and the increase in the amount of the ferment during the formation of the must of beer. This hypothesis is self-destructive." * Fermentations to him were simply chemical processes.

We must do Liebig justice, for he has his own explanation of both fermentation and putrefaction. They take place by what he calls eremacausis, which is a species of slow combustion. By eremacausis he understood certain changes that organic substances undergo at normal or slightly raised temperatures, and which only occur in the moist state and in the presence of oxygen. His explanation of putrefaction is that it is "a kind of eremacausis which takes place without the influence of atmospheric oxygen; it is a combustion of one or of many of the elements of the organic substance at the expense of its own oxygen, or possibly of that of the water, or possibly even at the expense of the oxygen of the organic matter and of the water at the same time." These theories were adopted, taught, and were to be found in all treatises on chemistry. Curiously enough, Liebig insisted that it was the dead portion of the yeast and not the living which, being an extremely alterable organic substance, "decomposed, and in decomposing set in motion by the rupture of its own elements the molecules of the fermentative matter." †

Helmholtz set himself the task of investigating the modifications produced in the chemical constitution of the muscle by its own activity. Resorting to the frog, "that ancient martyr to science," he succeeded by means of the electrical machine he had constructed, and by a Leyden jar, in showing that the components within a muscle undergo chemical transformation during its activity in virtue of the chemical processes he had described in his account of fermentation and putrefaction. For a time these experiments on metabolism, in spite of his other duties, engrossed him. But was there not a preliminary step? What were the relations between muscular action and the heat therein developed? With the smaller issue there was inextricably connected the larger one of a theory of animal heat. Is the material theory of heat any longer tenable? All his thoughts led him to say, No. Must a kinetic theory be substituted for it? All his thoughts

* *Annales de Chimie et de Physique*, 1839, LXXI, p. 187.
† R. Vallery-Radot, *The Life of Pasteur*, I, p. 105.

were tending to make him say, Yes. Heat, he was coming increasingly to hold, originates in mechanical forces, either directly by friction, or indirectly from an electrical current produced by the motion of magnets. At the beginning of October 1846 he sent a "Report on Work done on the Theory of Animal Heat for 1845," at the request of Du Bois Reymond, to the *Fortschritte der Physik,* issued by the Physical Society, and in the course of it he maintained that his conception of heat as motion involves the conclusion that mechanical, electrical, and chemical forces must always be the definite equivalent of one and the same energy, whatever the mode by which one force is transformed into another. Such a far-reaching conception lacked confirmation. Obviously the physicists and the physiologists of the Physical Society must set out with their experiments.

The study of fermentation and putrefaction is clearly one that takes the student far afield. Helmholtz was able to show that the oxygen produced by electrolysis in a sealed-up tube containing boiled fermentable fluid did not cause fermentation. Then he placed a bladder full of boiled grape juice in a vat of fermenting juice, and found that the fluid in the bladder did not ferment. Beyond question this ingeniously proved that the cause of fermentation could not pass through the wall of the bladder. Liebig held that if the fermentation were excited by a substance formed by the yeast cells, and presumably soluble, one would have expected it to pass through the wall of the bladder. But Helmholtz proved that it did not. On the other hand, if the process were caused by the small yeast cells, then one can understand why fermentation was not excited, as the yeast cells could not pass through the membrane. In effect, he had prepared the way for the conclusion that the living organisms in the air and in the yeast are the causes of putrefaction and fermentation.

The investigation had begun with the phenomena of animal heat and it widened into the consideration of the causes of the changes observable in energy. The subject continued to agitate his mind from 1844 to 1848, and he returned to it in 1850, 1852, 1855, and 1859, a singularly epoch-making year in the history of science. In order to judge the worth of his contribution to one of the most far-reaching conceptions in modern science, we must retrace our steps to note its slow growth. Others had been working more or less—chiefly more—vaguely in the field before Helmholtz.

Bacon, in his *Novum Organum,* states his conviction that "the very essence of heat is motion and nothing else." Descartes affirmed the doctrine of the constancy of the quantity of motion in the world. Boyle, in his book *On Cold,* published in 1665, when discussing the primum frigidum, says: "For if a body's being cold signify no more than its not having its insensible parts so much agitated as those of our sensories, there will be no cause to bring in the primum frigidum . . . it suffices that the sun, or some other agent which agitated more vehemently its parts before, does now either cease to agitate them, or agitate them very remissly." John Locke makes a similar statement, but all these statements are merely speculative, wanting any experimental verification. Newton was in possession of the principal facts of the conservation and the transformation of energy.*

The first experiments of value were those of Count Rumford about 1798. The scientists of his day held that heat is an imponderable fluid, caloric, which flows from a body at a higher temperature to one at a lower, much as water flows from a place of higher to a place of lower level. They also spoke of substances having different capacities for heat. Lavoisier had demonstrated the truth of the conservation of matter. Rumford set to work to develop the consequences of this conservation. He reasoned that if heat is a fluid, it can neither be created nor destroyed. Hence either the same amount of heat must be present in the hot chips and cannon as in the unbored metal or else heat must have reached the cannon from outside. He produced by friction sufficient heat to raise 26·58 pounds of water from its freezing-point to its boiling-point. Heat, he deduced, could not be a material substance. It is a form of energy: it is motion. Black had shown that heat could disappear as temperature and become latent, that is, heat not discoverable by the thermometer. Still, in his view heat was a material substance. Rumford's experiments threw doubt on this conception. He was in fact the first to suggest definitely the convertibility of heat into mechanical work. It was not the disappearance of

* P. G. Tait, *Lectures on Recent Advances in Physical Science,* lect. ii, p. 27.

heat but its appearance when mechanical work was performed which attracted his attention. His conclusion was that "it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated in those experiments, except it be motion."

Sir Humphry Davy showed that ice could be melted by friction, even in a vacuum, when everything else in the neighbourhood was at the freezing-point. His view was that heat is not matter, but "may be defined as a peculiar" motion, probably a vibration of the corpuscles of bodies tending to separate them. Davy states that his experiments on the generation of heat "were made long before the publication of Count Rumford's ingenious paper on the heat produced by friction." The pity is that he did not pursue such a promising line of investigation. If he had taken his statements into account with the second interpretation of Newton's third law, he would have anticipated Joule and Helmholtz.*

Thomas Young, physician, physicist, and Egyptologist, directed his many-sided attention to this problem. We draw attention to Young because he bears a striking resemblance to Helmholtz. Both were remarkable for versatility and originality; both possessed a vast extent of knowledge; both were physicists and physiologists; and both conducted fundamental researches. Helmholtz says of this Somersetshire scientist: "He was one of the most clear-sighted men who have ever lived, but he had the misfortune to be too greatly superior in sagacity to his contemporaries. They gazed on him with astonishment, but could not always follow the bold flights of his intellect, and thus a multitude of his most important ideas lay buried and forgotten in the great tomes of the Royal Society of London, till a later generation in tardy advance re-made his discoveries and convinced itself of the accuracy and force of his inferences." When Young discussed the experiments of Rumford he inferred that "heat

* Here I want to say, once for all, what a mine of wealth I find in J. T. Merz, *A History of European Thought in the Nineteenth Century*. His four volumes are marked by an insight of understanding and a penetration of thought rare in books.

is a quality and that this quality can only be motion." He refers to Newton's view that "heat consists in a minute vibratory motion of the particles of bodies," and to his own undulatory theory of light. As radiant heat possessed the same properties of reflexion, refraction, and polarisation as light possessed, the analogy of this form of heat with light for a long time served to unify the speculations of those who were inclined to embrace a mechanical or kinetic view of the nature of heat. James Prescott Joule was the first to emancipate himself from it.

We met Liebig when we were considering the problem of fermentation, and we meet him again in the attempt to ascertain the nature of the correlation of forces. As his position was amazingly commanding, this is what we should expect. Vaguely the notion floated before the mind that there was some sort of connection between heat and motion. The circulation of matter, its fermentation, the phenomenon of animal heat, its origin, and the part it plays in the living organism attracted his attention. His width of interests naturally compelled him to advocate an alliance of the different sciences. This meant that he speculated about the connection between them in general and the interdependence of the various forces of nature in particular. He is in fact one of those invaluable men in science whose general aims are of more importance than their particular objects. Johannes Müller also powerfully stimulated thoughts leading in the direction of the correlation of all the physical forces of nature. Naturally such a teacher stamped this conception, directly and indirectly, on the impressionable minds of his pupils.

Clearly we need, as well as the idea, the mind fit to receive it. As Heine puts it, what we see depends on our powers of sight. The ideas that floated before the minds of Liebig and Müller germinated in the mind of that remarkably original thinker, Karl Friedrich Mohr. In 1837 in a Viennese scientific periodical appeared his short memoir "On the Nature of Heat." The following are startling words: "Besides the known fifty-four chemical elements there exists in nature only one agent more, and this is called 'Kraft'; it can under suitable conditions appear as motion, cohesion, electricity, light, heat, and magnetism." The pith of the point is undoubtedly in this quotation, yet its publication remained unknown, even to the author himself, and fell on deaf ears throughout the scientific world for more than thirty years. He had offered his paper to Poggendorff, who refused it. A dread of introducing speculative matter into the *Annalen* was the cause of this refusal, and of the refusal of the later papers of Julius Robert Mayer and of Helmholtz. Mohr then sent his manuscript to one who was interested in theoretical physics, Baumgartner of Vienna, who printed it in the *Zeitschrift für Physik,* and never informed the author that he had done so. Curiously enough, Mohr himself did not note the wonderful discovery he had made, contenting himself with inserting a mere abstract of it in the *Annalen der Pharmacie.** The illuminating conception he held, but it does not seem to have occurred to him that he ought to measure the amount of energy appearing in diverse forms.

J. R. Mayer took five years later the next step. In a paper published in 1842 he showed that he clearly conceived the convertibility of falling force, or of the vis viva, which is its equivalent, into heat, which again can disappear as heat by re-conversion into work or vis viva as the case may be. Sir Gabriel Stokes points out that Mayer drew attention to the mechanical equivalent of heat as a fundamental datum, like the space through which a body falls in one second, to be obtained from experiment.† He went further. When air is condensed by the application of pressure, heat is produced. Taking the heat so produced as the equivalent of the work done in compressing the air, Mayer obtained a numerical value of the mechanical equivalent of heat, which, when corrected by employing a more precise value of the specific heat of air than that accessible to Mayer, does not differ much from Joule's result. Sir Gabriel Stokes admits that this was a bold idea. He proceeds, however, to observe that one essential condition in a trustworthy determination is wanting in Mayer's method: *the portion of matter operated on does*

* Vol. XXIV, p. 141. On the controversies over the conservation of energy, the following works of G. Helm are fair-minded, especially the second: *Die Lehre von der Energie; Die Energetik nach ihrer geschichtlichen Entwickelung; Das Princip der Erhaltung der Energie.*

† Sir J. Larmor, *Sir G. G. Stokes,* II, p. 51; P. G. Tait, *Recent Advances,* pp. 53, 60; J. J. Weyrauch, *Kleinere Schriften . . . von R. Mayer,* pp. 407, 408; G. Helm, *Energetik,* p. 24; E. Mach, *Wärmelehre,* p. 248.

*not go through a cycle of changes.** Mayer reasons as if the production of heat were the sole effect of the work done in compressing air. But the volume of the air is changed at the same time, and it is quite impossible to say a priori whether this change may not involve what is analogous to the statical compression of a spring, in which a portion or even a large portion of the work done in compression may have been expended. In that case the numerical result given by Mayer's method would have been erroneous, and might have been widely erroneous. Hence the practical correctness of the equivalent given by Mayer's method must not lead us to shut our eyes to the merit of Joule in being the first to determine the mechanical equivalent of heat by methods which are unexceptionable, as fulfilling the essential condition that no ultimate change of state is produced in the matter operated upon. The happy generalisation of Mohr and the numerical estimate of Mohr alike remained unnoticed by contemporary philosophers, and indeed similar neglect, for a time, fell to the lot of Joule. From 1841 to 1847 he laboured without receiving attention from the men of science of his day.

From that distinguished chemist John Dalton, Joule of Manchester received his first introduction to chemistry.† In 1841 he read his first paper "On the Electric Origin of the Heat of Combustion," before the Manchester Literary and Philosophical Society. In a paper of the year before, "On the Production of Heat by Voltaic Electricity," he showed that he had grasped the great importance of the law of electrolytic equivalence as affording the means of accurately measuring chemical processes. He gave definite expression to the vaguer ideas supported by Faraday and others that force was indestructible, and that the different elements of nature were mutually convertible. In magneto-electricity he discerned an agent capable, by simple mechanical means, of destroying or generating heat. Sir Humphry Davy, so far back as 1821, had observed the fact that a current produced heat in a con-

* The italics are Stokes's.

† A valuable account of Joule's life and work, by Osborne Reynolds, will be found in the Joule volume of the Manchester Literary and Philosophical Society. It is a matter of regret that there is no adequate biography.

ductor through which it had passed.* Davy had experimented on wires of different materials but of the same dimensions, arranging them in order according to the magnitude of the heat produced. Joule took a great step in advance. For he announced in his 1840 paper—and he was the first so to announce—the definite law that "when a current of voltaic electricity is propagated along a metallic conductor the heat evolved in a given time is proportional to the resistance of the conductor multiplied by the square of the electric intensity," i.e. the electric current.† In the same paper he showed that the law applies, when proper allowance is made for certain disturbances, to heat produced in electrolytes. The paper also contained the first reference to a "standard of resistance"; this consisted of a coil of 10 feet of copper wire ·024 inch in thickness. Obviously, we are coming close to the conception of the equivalence of heat and energy.

The nearer he was coming to the goal, the more certain he determined to make his approaches. He persisted in his experiments with the electromotive forces of various forms of voltaic cells and the heats of combination of the material of the cells. The results of his experiments down to 1843 are summed up in a paper "On the Heat evolved during the Electrolysis of Water." ‡ Here are some of his conclusions: "Third—Hence it is that, however we arrange the voltaic apparatus, and whatever cells of electrolysis we include in the circuit, the whole caloric of the circuit is exactly accounted for by the whole of the chemical changes. Fourth—As was discovered by Faraday, the quantity of current electricity depends upon the number of atoms which suffer electrolysis in each cell, and the intensity § depends upon the sum of chemical affinities. Now both the mechanical and heating powers of a current are (per equivalent of electrolysis in any one of the battery cells) proportional to its intensity. Therefore the mechanical and heating powers of the current are proportional to each other. Fifth—The magnetic electrical machine enables us to convert mechanical power into heat by

* *Phil. Trans*, 1821; cf. H. L. F. von Helmholtz, *Ueber die Erhaltung der Kraft*, I, p. 33.

† *Proc. R.S.*, Dec. 17, 1840; G. Helm, *Energetik*, p. 34.

‡ *Mem. Manchester Lit. and Phil. Soc.*, vol. VII.

§ i.e. the electromotive force.

aid of the electric currents which are induced by it, and I have little doubt that by interposing an electric magnetic engine in the circuit of a battery a diminution of the heat evolved per equivalent of chemical change would be the consequence, and that in proportion to the mechanical powers obtained."

The experimental question referred to in the fifth head was soon submitted to a further test, and on August 21, 1843, a paper "On the Calorific Effects of Magneto-Electricity and on the Mechanical Value of Heat" was read before the British Association at Cork.* This paper was as wonderful for its detailed knowledge as for its sweeping conclusions. In it Joule described a number of experiments in which a small electro-magnet was rotated in water in a magnetic field produced either by permanent magnets or by a fixed electro-magnet. The current induced in the moving coils, the total heat generated, and the energy used in maintaining the motion were all measured. It was proved that the energy used and the heat produced were both proportional to the square of the current. Thus a constant ratio exists between the heat generated and the mechanical power used in its production, so that, in Joule's words, "The quantity of heat capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale is equal to . . . a mechanical force capable of raising 838 pounds to a perpendicular height of one foot." † We may say here that the correct result of after-years is 778 foot-pounds. In a postscript to his paper Joule adds: "I have lately proved experimentally that heat is evolved by the passage of water through narrow tubes. . . . I thus obtain one degree of heat per pound of water from a mechanical force capable of raising 770 pounds to the height of one foot. I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are by the Creator's fiat indestructible, and that wherever mechanical force is expended an exact equivalent of heat is always obtained."

The details were ample, and the conclusion was no less ample. Heat had long been suspected to be a form of energy, and at last it had been verified to be a form of energy. From

* *Phil. Mag.*, 3rd ser., vol. XXIII; *Collected Papers*, I, p. 123.

† G. Helm, *Energetik*, p. 34; J. P. Joule, *Scientific Papers*, p. 328; H. L. F. von Helmholtz, *Ueber die Erhaltung der Kraft*, I, p. 33.

at least the days of Lavoisier men had been working at this hypothesis, and now their work was practically over. One pictures with what satisfaction the scientists welcomed this striking verification of what they had long dreamt. It is melancholy to relate that Joule's wonderful conclusion was received with entire incredulity. The fate of Mohr was also the fate of Joule.

The Manchester investigator plainly perceived that more experiments were required. Entirely unaware of a suggestion of Mayer, he proceeded in 1844 to inquire into the changes of temperature due to the rarefaction and condensation of the air. In another fashion he arrived at a fresh determination of the mechanical equivalent of heat. In this paper he combated the views of Carnot and Clapeyron, and put forth once more his own conclusion that the steam in the cylinder of an engine loses heat while it is expanding and doing work, and that on condensation of the steam the heat thus converted into power is not given back. He prepared a paper for the Royal Society, but the Royal Society rejected it. His own discoveries cleared up the only point that was really obscure in Carnot's cycle,* though he failed to perceive this. Like Mohr, Joule lived too near to his own results to be able to do justice to their many-sidedness.

The chilling reception of his paper at Cork and the rejection of another paper by the Royal Society would have been enough to daunt many men. Joule quietly pursued his experiments at his small laboratory at Pendlebury and later at the one his father built for him at Whalley Range. Two years passed, and Joule once more tried his fortune with a paper read before the British Association at Oxford. In the new apparatus brass paddles revolving in a fluid were propelled by the descent of weights. Joule's account of the circumstance by which he attracted attention, written in 1885, is worth recording:

"It was in the year 1843 that I read a paper 'On the Calorific Effects of Magneto-Electricity and the Mechanical

* In this cycle Carnot conceived that heat should be given to a prescribed quantity of air or steam; then the hot fluid should expand, doing work, but becoming cooler as it expanded, and giving out its heat to the refrigerator; then it should be compressed to its original volume and brought to the same pressure and temperature to recommence the cycle. This cycle is reversible.

Value of Heat' to the Chemical Section of the British Association assembled at Cork. With the exception of some eminent men, among whom I recollect with pride Dr. Apjohn, the president of the section, the Earl of Rosse, Mr. Eaton Hodgkinson, and others, the subject did not excite much attention; so that when I brought it forward again at the [Oxford] meeting in 1847 the chairman suggested that, as the business of the section pressed, I should not read my paper, but confine myself to a short verbal description of my experiments. This I endeavoured to do, and discussion not being invited, the communication would have passed without comment if a young man had not risen in the section, and by his intelligent observations created a lively interest in the new theory. The young man was William Thomson, who had two years previously passed the University of Cambridge with the highest honour, and is now probably the foremost scientific authority of the age." *

Thomson's version, given in 1882, is also worth recording:

"I made Joule's acquaintance at the Oxford meeting, and it quickly ripened into a life-long friendship. I heard his paper read at the section, and felt strongly impelled to rise and say that it must be wrong, because the true mechanical value of heat given, suppose to warm water, must for small differences of temperature be proportional to the square of its quantity. I knew from Carnot's law that this must be true (and it *is* true; only I now call it 'motivity' in order not to clash with Joule's 'mechanical value'). But as I listened on and on I saw that (though Carnot had vitally important truth not to be abandoned) Joule had certainly a great truth and a great discovery, and a most important measurement to bring forward. So instead of rising with my objection to the meeting, I waited till it was over, and said my say to Joule himself at the end of the meeting. This made my first introduction to him. After that I had a long talk over the whole matter at one of the conversaziones of the Association, and we became friends from thenceforward. However, he did not tell me that he was to be married in a week or so; but about a fortnight later I was walking down from Chamounix to commence a tour of Mont Blanc, and whom should I meet walking up but Joule, with

* *Collected Papers*, II, p. 215; Lord Kelvin, *Popular Lectures*, II, p. 556.

a long thermometer in his hand, and a carriage with a lady in it not far off. He told me that he had been married since we parted at Oxford! and he was going to try for elevation of temperature in waterfalls. We trysted to meet a few days later at Martigny, and look at the Cascade de Sallanches to see if it might answer. We found it too much broken into spray. . . . Joule's paper at the Oxford meeting made a great sensation. Faraday was there and was much struck with it, but did not enter fully into the new views. It was many years after that before any of the scientific chiefs began to give their adhesion. It was not long after when Stokes told me that he was inclined to be a Joulite.

" Miller and Graham, or both, were for many years quite incredulous as to Joule's results, because they all depended on fractions of a degree of temperature, sometimes small fractions. His boldness in making such large conclusions from such very small observational effects is almost as noteworthy and admirable as his skill in extorting accuracy from them. I remember distinctly at the Royal Society, I think it was either Graham or Miller saying simply he did not believe in Joule because he had nothing but hundredths of a degree to prove his case by." *

Endowed as he was with the magnificent audacity of youth, Thomson found himself at first, and for many months to come, unable to accept what Joule had laid down. To his brother James he wrote after the meeting: " I enclose Joule's papers, which will astonish you. I have only had time to glance through them as yet. I think at present that some great flaws must be found. Look especially to the rarefaction and condensation of air, where something is decidedly neglected in estimating the total change effected in some of the cases." Thomson felt quite convinced that work could be turned into heat. But could heat be turned into its equivalent of work? This he could not, in spite of Joule's reasoning, accept. Dominated by the reasoning of Carnot, Thomson saw that heat could furnish motive power when being let down from a higher to a lower temperature, or when passing from a hotter to a colder body. Unlike Joule, he could not as yet perceive that this would still be true even

* S. P. Thompson, *Life of Lord Kelvin*, I, p. 264; *Nature*, XXVI, p. 618.

if during the transference some portion of heat disappeared, as heat, to be converted into its equivalent in work. In another account of the Oxford meeting given by Thomson in 1893, at the unveiling of the Joule statue in Manchester, he declared that he was "tremendously struck with the paper," and added, "This is one of the most valuable recollections of my life, and is indeed as valuable a recollection as I can conceive in the possession of any man interested in science." * Nevertheless, his attitude in 1847 was one of warm approval mixed with disapproval.

On his return to Manchester, Joule continued his experiments on the production of heat by friction. The results were communicated to the Royal Society by Faraday on June 21, 1849, and this time the Council printed his paper "On the Mechanical Equivalent of Heat." † Joule mentions in it the series of observers who preceded him. As the outcome of fresh experiments, conducted with the utmost care, he concludes that "the quantity of heat capable of increasing the temperature of a pound of water (weighed in vacuo, and taken at between 55° and 60° Fahr.) by 1° Fahr. requires for its evolution the expenditure of a mechanical force represented by the fall of 772 lb. through the space of one foot." For nearly thirty years this result of Joule's stood alone as the one satisfactory determination of a most important physical constant. Writing in the *Proceedings* of the American Academy for Arts and Sciences on June 11, 1879, Professor Rowland of Baltimore points out: "We find that the only experimenter who has made the determination with anything like the accuracy demanded by modern science, and by a method capable of giving good results, is Joule, whose determination of thirty years ago, confirmed by some recent results to-day, stands almost, if not quite, alone among accurate results of the subject." Professor Rowland undertook fresh experiments, and concluded that the difference between his own results and those of Joule is "not greater than 1 in 400, and is probably less." So thought an American expert in 1879, but so did not think the observers in 1850.

Lord Rayleigh in his biography of his father, the eminent

* S. P. Thompson, *Life of Lord Kelvin*, I, p. 265.
† *Phil. Trans.*, 1850, pt. i; *Collected Papers*, I, p. 298.

scientist, reveals the limitations of Thomson in arresting fashion. He lived in a pre-Joule world, and he found it excessively difficult to assimilate the ideas of that world with those he had learnt at Cambridge. In spite of the experiments of Joule, Thomson declares in 1848 that "the conversion of heat (or caloric) into mechanical effect is probably impossible, certainly undiscovered." * In actual engines for obtaining mechanical effect through the agency of heat, we must consequently look for the source of power, not in any absorption and conversion, but merely in a transmission of heat. In a footnote appended to the word "impossible" in the sentence quoted, he adds: "This opinion seems to be universally held by those who have written on the subject. A contrary opinion, however, has been advocated by Mr. Joule of Manchester; some very remarkable discoveries which he has made with reference to the generation of heat by the friction of fluids in motion, and some known experiments with magneto-electric machines, seeming to indicate an actual conversion of mechanical effect † into caloric. No experiment, however, is adduced in which the converse operation is exhibited; but it must be confessed that, as yet, much is involved in mystery with reference to these fundamental questions of Natural Philosophy." He had either not read the evidence of Joule's experiments or else, in fear of Carnot's reasoning, he doubted their relevancy.

Thomson was an unbelieving Thomas, but he was also an investigating Thomas. He expounded in a paper read before the Royal Society of Edinburgh the Carnot cycle in detail, studying the writings of Regnault and of Joule.‡ He persists in employing the term "mechanical effect" for work performed, and persists in accepting as axiomatic "the ordinarily received and almost universally acknowledged" principle that in the cycle of operations as much heat must leave the body as entered it. Though he admits the urgent necessity of a most careful examination of the entire experimental basis of the theory of heat and though he turns to Joule and to the evidence which previously he had ignored, he sums up thus: "In the present state of science, however, no operation is

* Lord Kelvin, *Mathematical and Physical Papers,* I, p. 102.

† This expression denotes work done.

‡ *Trans. R. Soc. Edin.*, XVI, p. 541; Lord Kelvin, *Mathematical and Physical Papers,* I, p. 113. Cf. also *Ann. de Chimie,* XXXV, p. 248, 1852.

known by which heat can be absorbed into a body without either elevating its temperature, or becoming latent, and producing some alteration in its physical condition; and the fundamental axiom adopted by Carnot may be considered as still the most probable basis for an investigation of the motive power of heat."

Thomson was evidently still an unbeliever, denying any conversion of heat or caloric into mechanical effects. In inquiring into the thermal agency spent in conducting heat through a solid, Thomson asks, What becomes of the mechanical effect which it might produce? Nothing, he heartily believes, can be lost in the operations of nature, for no energy can be destroyed. What effect then is produced in place of the mechanical effect which is lost? He continues: "A perfect theory of heat imperatively demands an answer to this question, yet no answer can be given in the present state of science. A few years ago a similar confession must have been made with reference to the mechanical effect lost in a fluid set in motion in the interior of a rigid closed vessel, and allowed to come to rest by its own internal friction; but in this case the foundation of a solution of the difficulty has been actually found, in Mr. Joule's discovery of the generation of heat by the internal friction of a fluid in motion. Encouraged by this example, we may hope that the very perplexing question in the theory of heat by which we are at present arrested, will, before long, be cleared up. It might appear that the difficulty would be entirely avoided by abandoning Carnot's fundamental axiom. . . . If we do so, however, we meet with innumerable other difficulties—insuperable without further experimental investigation, and an entire reconstruction of the theory of heat from its foundation. It is in reality to experiment that we must look—either for a verification of Carnot's axiom, and an explanation of the difficulty we have been considering; or for an entirely new basis of the theory of heat." The very experiments he desired had already been supplied by Joule, covering the exact point raised by Thomson. Blinded by pre-conceived notions Thomson could not perceive the whole meaning either of the papers or of the experiments of Joule.*

* For Thomson's later views, cf. S. P. Thompson, *Life of Lord Kelvin*, I, pp. 440, 529.

We have seen that the papers of Joule met with entire incredulity at Cork by men of the scientific rank of Dr. Apjohn, Lord Rosse, and Eaton Hodgkinson; with rejection at the hands of the Royal Society of London; and with hostility for many years on the part of the scientific chiefs. The fate of Joule was indeed to be the fate of Helmholtz.

We left our study of Helmholtz at the stage when the consideration of the causes of the changes observable in energy was taking hold of his thoughts. He returned to his labours on animal heat again and again, knowing nothing of the labours of Mayer* and Joule. He watched the exchanges of matter that occur in connection with muscular contractions, noting that such exchanges are invariably accompanied by the disengagement of heat. Did not this indicate that animal heat, as produced by a muscle, arises from the chemical phenomena occurring in the muscle? Just as Joule and Thomson watched the mechanical engine, so Helmholtz watched the human engine. He was able to establish that the heat of the combustion of food, as determined by a calorimeter, is equal to the heat given off by an animal. In effect, an animal is a living calorimeter in which foodstuffs are oxidised or burnt.

The researches on muscular motion and on heat inevitably led the investigator far afield. What, for instance, is the relation of the forces of nature to each other? What is the relation of these forces to the phenomena of life? What is life? Is it to be explained by the interplay of the mechanical processes at work in the outer world? Was there what thinkers of the older and idealistic schools aimed at by employing such terms as the "vis viva" of Leibniz, the "vital force" of Stahl and Bichat, the "purpose and finality" of Kant, the "nisus formativus" of Blumenbach, "the Idea" of Hegel and Claude Bernard, or the "inherent tendency" of von Baer? Between the two extremes of considering life as a universal property of all matter and of considering it as a casual and accidental occurrence attached to rare and exceptional conditions, there was room for much variety of opinion. Vitalists like Bichat insisted on the independence,

* Cf. J. J. Weyrauch, *Kleinere Schriften und Briefe von Robert Mayer*, which forms a supplement to the edition by the same author of R. Mayer's *Schriften*, entitled, *Die Mechanik der Wärme*.

the incommensurability, and the originality of life which he defined as the totality of those functions which resist death. According to Claude Bernard, " L'élément ultime du phénomène est physique; l'arrangement est vital." * In accepting a mechanical conception, we must be careful not, he thinks, to fall into the common mistake of trying to explain vital processes as due directly to mechanical causes. In 1783 Lavoisier and Laplace had presented a memoir to the Paris Academy of Sciences, in which they attributed the generation of animal heat mainly to a process of combustion which took place by the conversion of oxygen into fixed air during the process of respiration. Other chemists, stimulated by this memoir, applied the new science of chemistry to questions of the individual and collective life of organisms. The ideas of Liebig led to the extension of the idea of " Stoffwechsel " (that is, the continual change of matter connected with maintenance of form in all living things) so as to embrace heat, light, electricity, and the like. In the world of Helmholtz, accordingly, there was the view that the living body was only a minute portion of the mechanism of the cosmos, and there was also the view that there was something beyond, some spiritual fuel continually being added to its vital fires. Helmholtz thought that if life was fed from such force of external energy, then the living body was an example of a perpetuum mobile, a perpetual motion, an idea he had often heard ridiculed in the philosophical discussions that were not infrequent in his father's home.

Considerations like these induced Helmholtz, in his twenty-sixth year, to write his paper " Ueber die Erhaltung der Kraft"—on the Conservation of Energy. In the days to come Clerk Maxwell was to acclaim it just as the generation *after* the printing of Joule's paper acclaimed his. Clerk Maxwell wrote: " To appreciate to the full the scientific value of Helmholtz's little essay on the Conservation of Force, we should have to ask those to whom we owe the greatest discoveries in thermo-dynamics and other branches of modern physics, how many times they have read it over, and how often during their researches they felt the weighty statement of Helmholtz acting on their minds like an irresistible

* C. Bernard, *Leçons sur les phénomènes de la vie,* II, p. 524.

driving power." As Lavoisier had rendered the persistence of matter the fundamental principle of chemistry, so Helmholtz rendered the conservation of force the fundamental principle of physics.* He showed how it could be considered as an extension of the theorem known in abstract dynamics as the conservation of the vis viva of a mechanical system. Sharply distinguishing between active (work) forces and mere tensions (dead forces), Helmholtz proceeds to draw all other forces of nature into his consideration, showing, in the case of the phenomena of heat, electricity, galvanism, and magnetic induction, how the different agencies can be brought into comparison with mechanical ones by measuring the work they perform. Referring to the attempts to fix the mechanical value of heat, he ends with the words: "I think in the foregoing I have proved that the abovementioned law does not go against any hitherto known facts of natural science, but is supported by a large number of them in a striking manner. I have tried to enumerate as completely as possible what consequences result from the combination of other known laws of nature, and how they require to be confirmed by other experiments. The aim of this investigation, and what must excuse me likewise for its hypothetical sections, was to explain to natural philosophers the theoretical, practical, and heuristic importance of the law, the complete verification of which may well be looked upon as one of the main problems in the near future."

It is sad to have to record that the prophet was not received with honour in his own country. Mohr and Mayer, Joule and Helmholtz all met with the keenest hostility. As soon as the paper was read, Helmholtz sent it to Gustav Magnus. He declined to express an opinion on its worth, as he thought there should be a distinction between mathematical and experimental physics. He warned Helmholtz against undue partiality for mathematics, and the attempt to bring remote provinces of physics together by its means. We have it on the authority of Du Bois Reymond that only Jacobi, who himself had already done excellent work in mechanics, saw its merit. Helmholtz, referring in after-years to this opposition, said he was met by some of the older men by such a remark as

* H. L. F. von Helmholtz, *Gesammelte Abhandlungen*, I, p. 67. Cf E. T. Whittaker, *History of the Theories of Aether*, pp. 240-3.

this: "This has already been well known to us; what does this young medical man imagine when he thinks it necessary to explain so minutely all this to us?" * Poggendorf thought that the subject-matter was not in his opinion sufficiently experimental to justify him in publishing it in the *Annalen*. The older physicists of Berlin like Dove and Reuss would not admit the principle of the conservation of energy. They seemed to fear that the speculations of the paper would revive the phantasm of Hegel's "nature-philosophy," against which they had fought so long, and in the end so successfully. Distinguished mathematical colleagues like Eisenstein and Lejeune-Dirichlet seconded the opposition of Dove and Reuss.

Some of this opposition sprang from the confusion in the use of the term "force." Force might mean pressure or dead force, in the Newtonian sense, or it might mean acting force, vis viva in the Leibnizian sense. One way out of this difficulty was to employ the term "work," as Clausius suggested in 1850, or the term "energy," as Thomson suggested in 1852 in his adaptation of Young's already exact terminology.

Mohr,† Mayer, and Helmholtz gave few new experimental facts, and as the German physicists were just escaping from the dominance of "nature-philosophy" they felt afraid that they had expelled it in one form only to witness its re-entry in another. Such a consideration is of no avail, however, when we come to the papers of Joule, who provided amply experimental verification of the views he put forward. There is, according to the old proverb, none so blind as those who won't see, and it is melancholy to behold men refuse to accept the truth vouched for by experiment after experiment.

Those who wished to disparage what Helmholtz had brought to light said that he had borrowed the idea from Mayer, but the singular matter is that Helmholtz had never heard of Mayer's or Joule's papers. The law of the conservation of energy, like most scientific laws, had to face the opposition of the scientists who found it contradicted some of

* Cf. his *Reden*, II, p. 46; his *Wissenschaftliche Abhandlungen*, I, p. 73.

† Cf. his *Allgemeine Theorie der Bewegung und der Kraft*, p. 82; J. J. Weyrauch, *Kleinere Schriften . . . von R. Mayer*, p. 100; H. L. F. von Helmholtz, *Wissenschaftliche Abhandlungen*, I, p. 71; his *Vorträge und Reden*, pp. 39, 69; J. von Leibig, *Die organische Chemie*, p. 183.

the principles they held.* The day was to come when they were to admit it. That day, however, was not yet. Kirchhoff estimated this universal principle of Helmholtz as the most important contribution to natural science made in our era. This estimate came twenty years later. Hertz, the discoverer's greatest pupil, could say that by it "physical research had been diverted into an entirely new channel. Under the overmastering influence of Helmholtz's discovery of the conservation of energy, its object was henceforward to refer all phenomena in the last resort to the laws which govern the transformation of energy." At the close of 1853, Clausius published in the *Annalen* an attack upon Helmholtz's memoir on the conservation of energy, conveying the impression to non-mathematical physicists that his conclusions were erroneous. "The die is cast," wrote Kepler, "I have written my book. It will be read; whether in the present age or by posterity matters little. It can wait for its readers." Helmholtz could—and had to—wait for his readers. He became convinced of the universal validity of the law of energy for all natural processes of the non-living as of the living world, and thus arrived at the law of the conservation of energy. Its discovery rendered a coherent structure of theoretical mechanics possible. The concept of force retreated into the background; mass and energy emerged as indestructible physical quantities.

We can scarcely read of the far-reaching discovery of the German scientist without noting how little share experiment played in it. Nor need we greatly wonder at this. He delighted in describing how Faraday had with a mysterious instinct made the most pregnant discoveries in natural science, though he was unable subsequently to give any lucid account of the train of ideas that led to them.† The intuition of the Irish scientist was shared in no mean degree by the German. This comes out in the reference of the latter to the former. "It is," Helmholtz confesses, "very hard to define new abstractions in universal propositions, so as to avoid misunderstandings of all kinds. It is, as a rule, much harder

* Cf. the application Lord Balfour makes in his *Theism and Humanism*, pp. 220 ff. E. Meyerson, in his *Identité et Réalité*, is singularly illuminating.

† L. Koenigsberger, *Hermann von Helmholtz*, p. 248.

for the creator of such a new idea to make out why others fail to understand him, than it had been to discover the new truth. I will not disparage Faraday's contemporaries, because his words appeared to them uncertain and dark sayings. I remember too well how often I have sat gazing hopelessly at one of his descriptions of lines of force, of their number and tensions, or have sought to puzzle out the meaning of some law in which the galvanic current is treated as an axis of force, and so on. A Clerk Maxwell was required, a second man of the same depth and independence of insight, to build up in the normal forms of our systematic thinking the great structure whose plan was present to Faraday's mind, which he saw clear before him, and endeavoured to render apparent to his contemporaries."*

To this insight, this intuition, Helmholtz harks back. In the preface to his translation of Tyndall's *Fragments of Science,* he describes in clear and beautiful language the importance of the classics in the development of a moral and æsthetic sense, and in the evolution of an intuitive knowledge of human sensations, ideas, and conditions of civilisation.† In his speech on the commemoration of his seventieth birthday, November 2, 1891, he pointed out: "There are many narrow-minded people who admire themselves enormously if they have one stroke of luck, or think that they have had one. A pioneer in science, or an artist, who has a repeated run of happy accidents, is indubitably a privileged character, and is recognised as a benefactor of mankind. But who can count or weigh such lightning flashes of the mind? Who can trace out the secret threads by which our conceptions are united? For—

Was vom Menschen nicht gewusst,
  Oder nicht bedacht,
Durch das Labyrinth der Brust
  Wandelt in der Nacht.

"I must confess that the departments in which one has not to trust to lucky accidents and inspirations have always had the greatest attraction for me. Yet as I have often been in the predicament of having to wait on inspiration, I have had some few experiences as to when or how it came to me,

* L. Koenigsberger, *Hermann von Helmholtz,* p. 293.
† *Ibid.*, p. 212.

which may perhaps be of use to others. Often enough it steals quietly into one's thoughts and at first one does not appreciate its significance; it is only sometimes that another fortuitous circumstance helps one to recognise when, and under what conditions, it occurred to me; otherwise it is there, without effort, like a flash of thought. So far as my experience goes it never comes to a wearied brain, or at the writing-table. I must first have turned my problem over and over in all directions, till I can see its twists and windings in my mind's eye, and run through it freely, without writing it down; and it is never possible to get to this point without a long period of preliminary work. And then, when the consequent fatigue has been recovered from, there must be an hour of perfect bodily recuperation and peaceful comfort, before the kindly inspiration rewards one. Often it comes in the morning on waking up, according to the lines I have quoted from Goethe (as Gauss also noticed, *Works*, V, p. 609: Law of Induction discovered January 23, 1835, at 7 a.m. before rising). It came most readily, as I experienced at Heidelberg, when I went out to climb the wooded hills in sunny weather. The least trace of alcohol, however, sufficed to banish it. Such moments of fertile thought were truly gratifying, but the obverse was less pleasant when the inspiration would not come. Then I might worry at my problem for weeks and months, till I felt like the creature on the barren heath—

> Von einem bösen Geist im Kreis herumgeführt,
> Und ringsumher ist schöne grüne Weide.

Sometimes nothing but a severe attack of headache could release me from my spell, and set me free again for other interests." *

On April 5, 1881, Helmholtz delivered the Faraday lecture to the Chemical Society "On the Modern Development of Faraday's Conception of Electricity," in the course of which he gave not merely an estimate of the Irish physicist but also threw light on the question of insight. His words in opening the lecture are so characteristic of Helmholtz as to merit quotation:

"The facts which he [i.e. Faraday] discovered are uni-

* L. Koenigsberger, *Hermann von Helmholtz,* p. 208.

versally known. Every physicist, at present, is acquainted with the rotation of the plane of polarisation of light by magnetism, with dielectric tension and diamagnetism, and with the measurement of the intensity of galvanic currents by the voltameter, while induced currents act on the telephone, are applied to paralysed muscles, and nourish electric light. Nevertheless, the fundamental conceptions by which Faraday was led to these much admired discoveries have not received an equal amount of consideration. They were very divergent from the trodden path of scientific theory, and appeared rather startling to his contemporaries. His principal aim was to express in his new conceptions only facts, with the least possible use of hypothetical substances and forces. This was really an advance on general scientific method, destined to purify science from the last remnants of metaphysics. Faraday was not the first, and not the only man, who has worked in this direction, but perhaps nobody else at his time did so radically. But every reform of fundamental and leading principles introduces new kinds of abstract notions, the sense of which the reader does not catch in the first instance. Under such circumstances, it is often less difficult for a man of original thought to discover new truth than to discover why other people do not understand and do not follow him. This difficulty must increase in Faraday's case, because he had not gone through the same common course of scientific education as the majority of his readers. Now that the mathematical interpretation of Faraday's conceptions regarding the nature of electric and magnetic forces has been given by Clerk Maxwell, we see how great a degree of exactness and precision was really hidden behind the words, which, to Faraday's contemporaries, appeared either vague or obscure; and it is in the highest degree astonishing to see what a large number of general theorems, the methodical deduction of which requires the highest powers of mathematical analysis, he found by a kind of *intuition,** with the security of instinct, without the help of a single mathematical formula. I have no intention of blaming his contemporaries, for I confess that many times I have myself sat hopelessly looking upon some paragraph of Faraday's description of lines of force, or of the galvanic

* I italicise this word.

current being an axis of power, etc. A single remarkable discovery may, of course, be the result of a happy accident, and may not indicate the possession of any special gift on the part of the discoverer; but it is against all the rules of probability that the train of thought which has led to such a series of surprising and unexpected discoveries, as were those of Faraday, should be without a firm, although perhaps hidden, basis of truth. We must also in his case acquiesce in the fact, that the greatest benefactors of mankind usually do not obtain a full reward during their lifetime, and that new ideas need the more time for gaining general assent the more really original they are, and the more power they have to change the broad path of human knowledge." *

* Helmholtz, *Vorträge und Reden*, II, p. 277.

CHAPTER V

THE PRECURSORS OF DARWIN

THE conception of evolution occupies no mean part in theories of the universe so far back as six centuries before our era. To the early philosophers of India no less than to those of Greece, our globe, without haste and without rest, was changing. Talking with Matthew Arnold in 1871, J. W. Judd heard him say, "I cannot understand why you scientific people make such a fuss about Darwin. Why, it's all in Lucretius!" On Judd replying, "Yes! Lucretius guessed what Darwin proved," he mischievously rejoined, "Ah! that only shows how much greater Lucretius was—for he divined a truth, which Darwin spent a life of labour in groping for."

The intuition of the classical poets and writers is as evident in the history of atomism as it is in evolutionism. Democritus (450 B.C.) confidently asserts that the world consists of atoms, and that its infinite variety is due to the motions and positions of immutable and imperceptible units, which, if they are not exactly alike, at least differ less than do the visible objects into which they are comprehended. He offered no proof for this theory, which, to say the least, is not self-evident. Century after century this theory persisted, and was held with unabated confidence. With the Renaissance of science during the sixteenth century it acquired fresh vigour. Bacon believed it, and so did Boyle and Gassendi. Boyle held it stoutly, and Newton assumed it without question and without proof. The first proof of the atomic theory was given by John Dalton in 1802. For over twelve centuries his views had been more or less held without reasons for holding them. When they were put forth, it is in keeping with the history of science that Sir Humphry Davy and Berthollet were the most conspicuous objectors to them. Of course we all admit to-day that

Dalton started a new era in chemistry. Taken over by the physicists, the atomic theory lies at the root of the modern theory of gases or liquids, the modern theory of matter, the modern theory of heat, and the modern theory of electricity.

This is a very strange story, which becomes no less strange when we find the same sort of story in the history of the theory of evolution. Haeckel terms Anaximander the Milesian (610—547) the prophet of Kant and Laplace in cosmogony and of Lamarck and Darwin in biology.* Anaximander assumed the idea of the Infinite, or the Unconditioned, and on the basis of this assumption he put forth a crude form of the nebular hypothesis and of the evolution idea. He assumes that matter is primitive and indeterminate, that there is necessarily in it eternal energy and movement, and that through this energy and movement the two original contraries of heat and cold separate. What is cold falls to the centre and forms the earth. What is hot arises to the circumference and forms the bright fiery bodies of the heavens, which are only fragments of what once existed as a complete sphere. In process of time this sphere burst, forming the stars. The action of the sun's heat on the cold earth generated films or bladders, out of which proceeded different kinds of imperfectly organised beings: they gradually developed into the animals now existing. This is quite unlike Epicurus (342—270) and Lucretius (95—55), who both imagined animals arising directly out of the earth, much as Milton's lion long afterwards pawed its way out. The pedigree of man with Anaximander goes back to the fishlike creatures which dwelt originally in muddy waters, and only as the sun slowly dried up the earth did they become by stages fit for life on dry land. He, however, confines his conception of progress to the evolution of animals and man. He holds that there is a plurality of worlds, and according to him one world springs out of another.† This idea is also to be found in Heraclitus of

* Throughout my account of the slow growth of evolutionism, I desire to state how ample is my debt to Mr. H. F. Osborn's stimulating book, *From the Greeks to Darwin*.

† Cf. p. 406 ff. in the appendix on "The Conception of Progress in Classical and Renaissance Writers" in my *Erasmus and Luther: their Attitude to Toleration*.

Ephesus (513—c. 473), who maintains that out of the universal conflagration will issue a new world, and this process will continue indefinitely. Nevertheless, in spite of this continual transformation, Heraclitus does not speak of any amelioration in the lot of man. The cardinal fact to him was the ceaseless movement in the universe and the utter hopelessness of it. With thoughts of the World War in our mind, it is easy for us to hold that development includes retrogression. There are many side currents as well as the main current in the stream of evolution. As Huxley pointed out, " So far from any gradual progress forming any necessary part of the Darwinian creed, it appears to us that it is perfectly consistent with indefinite persistence in one state or with a gradual retrogression. Suppose, for example, a return of the glacial period and a spread of polar climatical conditions over the whole world." *

Xenophanes (c. 540—c. 500), a pupil of Anaximander, agreed with his master's view of the beginning of man. He traced the ultimate origin of life to spontaneous generation, believing that the sun in warming the earth produced both animals and plants.† The physicists like Heraclitus and Empedocles, Democritus and Anaxagoras, developed these notions.

The poetic view of Empedocles of Acragas (c. 490—430) is not unlike the scientific conception of Anaximander, who was the very first to teach the doctrine of abiogenesis, believing that eels and other aquatic forms are directly produced from lifeless matter. Empedocles assumes the four elements of earth, air, fire, and water. Out of the conflict between love and hate emerge plants, animals, and man in succession. The greater number of the members of the animals was generated by chance.‡ After endless efforts on the part of the organs to unite, the present shapes are evolved.§. Empedocles finds the origin of life in abiogenesis or spontaneous generation: centaurs, chimæras, and other creatures he brings under the operation of this law.|| In a crude form he lays down the

* T. H. Huxley, *Darwiniana*, pp. 90-1.

† Stobæus, *Eclog.*, I, 224.

‡ *Parts of Animals*, book i.

§ Mullach, "Empedoclis Carmina," 314-16, in *Frag. Philos. Græc.*; Aelian, *H.A.*, XVI, 29; Aristotle, *Physics*, II, 8.

|| Lucretius, V, 860.

theory of natural selection. Aristotle (384—322) in his *Physics* is careful to inform the reader that he derived his theory from Empedocles, who merely held the germ of an all-important conception. Strangely enough, the idea of one stage giving origin to another was absent from his thought.

Empedocles tries to provide a human beginning for the centaurs and chimæras of Greek mythology. Lucretius interpreted this teaching:

> Hence, doubtless, Earth prodigious forms at first
> Gendered, of face and members most grotesque:
> Monsters half-man, half-woman, not from each
> Distant, yet neither total; shapes unsound,
> Footless and handless, void of mouth or eye,
> Or from misjunction, maimed, of limb with limb:
> To act all impotent, or flee from harm,
> Or nurture take, their loathsome days t'extend.
> These sprang at first and things alike uncouth
> Yet vainly; for abhorrent Nature quick
> Checked their vile growths; . . .
> Hence, doubtless, many a tribe has sunk suppress,
> Powerless its kind to gender. For whate'er
> Feeds on the living ether, craft or speed,
> Or courage stern, from age to age preserves
> In ranks uninjured: . . .
> Yet Centaurs lived not; nor could shapes like these
> Live ever, from two different natures reared,
> Discordant limbs and powers by powers reversed.

Compare these lines with those of John Milton:

> The Earth obeyed, and straight
> Opening her fertile womb, teemed at a birth
> Innumerous living creatures, perfect forms,
> Limbed and full grown. Out of the ground up rose
> As from his lair, the wild beast, where he wons
> In forest wild, in thicket, brake, or den;
> Among the trees they rose, they walk'd;
> The cattle in the fields and meadows green:
> Those rare and solitary, these in flocks
> Pasturing at once, and in broad herds upsprung.
> The grassy clods now calv'd; now half appear'd
> The tawny lion, pawing to get free
> His hinder parts, then springs, as broke from bonds,
> And rampant shakes his brindled mane.

If we understand evolution primarily to mean the series of slow successive transformations, infinitesimal changes which, when taking place, alter the condition of a plant or an animal, then there is scarcely a trace of such a doctrine in Plato (c. 428—347). He, then, from our standpoint, makes but a small contribution to our theme. Still, it is a fact of enormous significance that the groundwork of modern science, the evolution theory, was laid not by the early naturalists or the speculative writers but by the modern philosophers, by Descartes (1596—1650) and by Leibniz (1646—1716). What Plato failed to accomplish directly he himself accomplished indirectly. He is one of the sources of that spirit of mysticism which tends to merge the particular in the universal, the temporal in the eternal. He is also one of the sources of that idealism which checks the evil side of mysticism, for Plato sought the ideal in the real world, the world of ideas.* The ancients did not realise the modern conception of indefinite progress in a *continuous* direction. If one may use an Irishism, the thought of progress backwards was more familiar to them than progress forwards.

Plato conceived society dynamically: Aristotle conceived it statically. Order to the latter was heaven's first law. To trace the general plan of the human evolution of the human race is a task which does not concern him: his is the humbler labour of showing under what conditions the City-State can realise happiness. Its size, its site, its nearness to the sea, its aloofness from the stranger—these are the matters in his mind. His closest approach to the consideration of the ideal is his criticism of the Platonic conception. Has he more than a glimpse of scientific progress when he discusses changes in medicine which have modified the art of healing? † Though he has not the resources of palæontology at his command, he entertains a general conception of the origin of higher species from lower. In his consideration of the factors of evolution it is amazing to note that he discusses the "survival of the fittest" hypothesis, which he states quite plainly, and dismisses a theory of adaptive structures in animals surprisingly similar to that laid down by Darwin almost twenty-three centuries later. In spite of Empedocles,

* The world of (sensible) experience was to him not real, though things partook of the universals, or ideas, which alone were real, and contributed, of themselves, a quite different world, viz. the intelligible or noumenal.

† *Politics,* VII, 13.

he held that adaptive structures are not produced by natural selection.

Aristotle's view of the development of life ultimately led to the correct interpretation of the Mosaic account of the creation, and his view St. Augustine (354—430) cordially accepted. Indeed, if the teaching of the African doctor, in this respect at least, had remained the teaching of the Church, the triumph of the theory of evolution might have been anticipated by fourteen centuries.* St. Augustine was an observer; Aristotle was a scientific observer. The latter distinguished no fewer than five hundred species of mammals, birds, and fishes. Underlying these and other species he conceived of a single chain of events, which is among the greatest of his achievements: it completely passed out of the ken of man till the middle of the nineteenth century. Nature, he maintains, proceeds constantly by the aid of gradual transitions from the most imperfect to the most perfect, while the numerous analogies we find in the various parts of the animal scale show that all is governed by the same laws. That is, all nature is essentially one in the matter of causation. The ascent is from the inorganic to the organic, and then comes man, who reaches the highest point in one long and continuous process.

Details were in the mind of Aristotle: so too were illuminating principles. He notices the effects of heredity, of the influence of one parent or stock, of atavism, of reversion. In the *Generation of Animals* † he analyses the heredity theories of Hippocrates (460—357) and Heraclitus, which were not unlike those of Democritus, who noted design in nature and admired her adaptations. The variety of nature moved him just as much as the variety of atoms. Aristotle describes the difference between the vegetable and the animal world, and marks off the organic world from the inorganic. He clearly grasps the principle of adaptation, understanding the physiological division of labour in the different parts of an organism. Life to him is not a separate principle: it is the function of the organism, a view which anticipates the doctrine of epigenesis in embryonic development discovered by Harvey (1578—1657).

* Cf. *De Trin.*, III, 8, 9; IV, 21; *De Genes. ad Lit.*, I, 39; *De Doct. Christ.*, II, 46.

† *Generation of Animals*, I, sec. 35.

How did Aristotle arrive at these notable advances? Unlike Plato, who trusted intuition, he sometimes trusted experiment and deduction,* though at others he revelled in a priori thought, to the neglect of fact or observation, like a Platonist. To a man with his scientific bent it was impossible to believe in the operation of chance. Nothing, he holds, which occurs regularly, can be the result of accident. The adaptation manifested in the world obliged him to believe in an intelligent First Cause. This theistic tinge influenced the early Christians, especially St. Augustine, and in time the authority of Aristotle in the Mediæval Church was elevated to a position as exalted as that of the Bible itself. The lover of truth must regret that the conquests of Philip (382—336) and Alexander (356—323) and the loss of national independence checked the love of free physical inquiry among the Greeks, which promised to be so fruitful. The dynasties founded by Alexander's generals left the City-State a mere pawn in the game of militarism; the all-conquering arm of Rome completed her destruction. In the post-Aristotelian period Francis Bacon is right in thinking that for the ancients moral philosophy supplied the place of religion. The new school of thought is subjective and individualistic. Ethical conceptions replace science. The Stoics or the Epicureans came into possession of the vacant field. The happiness of man was no longer bound up with the welfare of the State. For the first time it became possible to lead a private life: Diogenes (c. 412—323) and Aristippus (c. 428—350) were no longer singular in their conduct.

The Oriental doctrine of vast chronological cycles forms a fundamental tenet of the Stoic school. With its philosophers the pantheistic notion that God is the creative soul of the world was a commonplace. He is the eternal force which forms and permeates the world, the spirit of ever-acting and living fire, which manifests itself outwardly as matter when its heat declines, and burns up matter when its heat is intense.† Zeno (c. 362—264), the founder of Stoicism, believed that the world would be reabsorbed into the fiery

* Aristotle, *History of Animals*, I, 6.

† Nemesius, *De Nat. Hom.*, c. 38; Polybius, *Hist.*, VI, c. V ff.; II, pp. 462, 575 ff.; Cicero, *De Nat. Deor.*, II, 20, 46, 51; Origen, *Con. Cels.*, IV; Origen, *De Principiis*, III, 6.

ether, which is Reason and God. But how could Reason be identified with a material substance which could be burnt? Is this absorption final? The mind of man is so constituted that it refuses to derive satisfaction in the conflagration of the world. There was one way out of the difficulty, and that was to make the movement circular. What had happened once could happen again. When the period of unification was ended, Zeno forecast the beginning of another world-process which would follow the same course as its predecessor, ending, like it, in fire. And for ever there lay before men the prospect of this unvarying round. To us such a notion is abhorrent; still, we ought to remember that men not only in Greece and in India but even in modern Europe acquiesce in it.

From Zeno and other teachers the conclusion was drawn that in a necessary and endless succession world after world was created and destroyed, each world being exactly like its predecessor, and all things in it without exception running round in the same order from beginning to end. Aristotle maintained that all the arts and sciences have been found and lost an infinite number of times already.* Stoicism, in some of its aspects, reflects the hopelessness and world-weariness which sees in modern progress only "an endless effort, and, if need be, by endless pain." The same sombre tendency sees no evolution but rather a long series of cycles of death and revival, of endless mutations in constant progressions: *tout lasse, tout passe, tout se refait.* Going round in a circle, however, is in no wise the equivalent of going on. It is easy to understand Seneca's *tædium vitæ* when he thought of it, to employ a modern phrase, as an infinite recurrent series. He was really to finish nothing, for in the revolution of the circle it must come again and again to him.†

At the very time of the early Stoics, Epicurus (342—270) was developing a conception of progress, and his philosophy contains more than the beginning of the doctrine professed by

* *Politics,* II, 5, 1264A, 1-5; IV (vii), 10, 1329B, 25-7; *De Cælo.,* I, 3, 270B, 16-20.

† Seneca, *Ad Lucilium,* Ep. XXIV. Cf. *De Tranquillitate Animi,* ch. i and ii; Horace, *Carm.,* 11, 18, 15; Lucretius, *De Natura Rerum,* III, 920-50.

the Sophists.* Were it not for Lucretius† our knowledge of Epicurus would be scanty, but he provides us with a full account of a notable attempt to get rid of the supernatural. The mind of Epicurus conceives the social state not as it ought to be but as it actually is. For him, as for Lucretius, the important matter is the survey of knowledge and of civilisation through past ages. Familiar as he was with Empedocles, Epicurus knows that human life has passed from the darkness of ignorance to the light of knowledge, the source of all quietism and happiness.‡ The mind of man has at last passed the superstitious stage.§ In truth philosophy has taken the place formerly occupied by superstition. Much as Lucretius admires Epicurus, he lets fall hints which show, inconsistently enough from his standpoint, that the past was better than the present. Did not in olden times matters come easily to men? Did they not possess simple joys? Did voyages at sea, did war, did luxury claim so many victims as to-day? || Is not Nature right to tell man, greedy of pleasure and novelty, that she can devise nothing new, for everything returns as before? ¶ Will not the universe one day be destroyed? ** These questions no doubt are not the bedrock of the thought of the poet; still, they are in his poem.†† On the other hand, he argues that as the world is not the handiwork of the gods, its increase in intelligence and industry affords evidence of progress.‡‡ Advance, he argues, in material comfort is not synonymous with advance in happiness, just as one might argue that our material progress is nothing more than an extra-flooding wave of an ebbing tide. Lucretius is strikingly clear that material and even artistic improvement does not increase the happiness of man.§§

The view set forth by Lucretius in the fifth book of the *De Natura Rerum* is that the general law of existence is

* Cf. the speech of Callicles in Plato's *Gorgias*.
† *De Nat. Rer.*, V, 1181 ff.
‡ *Ibid.*, 10.
§ *Ibid.*, I, 62 ff.
|| *Ibid.*, II, 1157-74; V, 935 ff., 988-1010.
¶ *Ibid.*, III, 945.
** *Ibid.*, II, 1148-74; V, 93-6.
†† Cf. Ovid, *Met.*, I, 89-150; I, 256-8.
‡‡ *De Nat. Rer.*, II, 181.
§§ *Ibid.*, V, 1379 ff.; especially 1410.

change.* Nothing remains as it was in the beginning: one thing disappears and is replaced by another: what was formerly is to-day impossible, and what has never been will yet be realised.† In Lucretius the materialistic and agnostic tendencies of Empedocles, Democritus, and Epicurus are revived. Aristotle regarded the world as an organism, Lucretius as a mechanism. Aristotle is teleological, Lucretius is nothing of the kind. The former carries his conception of nature into the law of the gradual development of organic life; the latter does not. Lucretius, like Parmenides (flor. 513 B.C.), Democritus, and Anaxagoras (500—428), thinks that plants arise directly from the earth.‡ From Epicurus he takes the idea of the survival of the fittest: some men were out of harmony with their surroundings, died, and were replaced. Still, Aristotle is an evolutionist, and Lucretius is just as certainly not. The latter does not believe in gradual development by the ascent of the higher forms from the lower, though he believes in the successive appearance of different forms of life. The animals and plants of Lucretius, unlike those of Aristotle, spring from the earth in their present form: with them Nature makes a leap. This is not evolution in the true sense, yet, curiously enough, it was one day to take a great share in the growth of the idea. To Aristotle the process of evolution was like the emergence of the plant from under the ground, where its germinative forces have been slowly maturing, whereas Lucretius conceived it to be the light of a spark for which the explosive train had not long been laid.

Generation succeeds generation: there is no break. The notion of continuity means more to Lucretius than to Epicurus. The latter is not content with change in nature: he believes that there are times in the span of existence better than others. Lucretius was well aware that in early society force was the only remedy: laws did not exist.§ By virtue of mind men left this condition behind them.|| No Prometheus brought the fire which the ingenuity of human beings discovered.¶ Genius has accomplished much: so too have the numberless groping efforts of ordinary

* *De Nat. Rer.*, V, 828-36.
† *Ibid.*, III, 964; V, 855-77.
‡ *Ibid.*, V, 780.
§ *Ibid.*, V, 959.
|| *Ibid.*, V, 1107, 1187.
¶ *Ibid.*, I, 208-14.

men. Steady work renders better what was primitive, mediocre. According to Lucretius and Epicurus necessity has always been the principal agent in progress. To need, for example, we owe the names of things: this is the origin of language.* To chance was due the first union of men and women, and in time conjugal love succeeded. Kings built fortified towns, and the cause of progress was served by the necessity of avoiding aggression.† On the death of kings democracy succeeded, and then came magistrates and laws with justice in their hand. Force as the only remedy disappeared indefinitely, and the reign of law was ushered in.‡

At the same time human industry took its rise. The first instruments of man were his hands, his nails, his teeth, then stones, then branches of trees, afterwards the flame and the fire.§ This is the closest approach Lucretius perhaps makes to the doctrine of evolution. Later came the metals—brass, gold, iron, money, lead. The discovery of iron combined with the discovery of fire permitted man to manufacture implements adapted to agriculture and to war,|| and it assisted in the improved clothing the tailor devised.¶ Stage by stage man developed, and in the course of his development nature suggested experiments to him. What nature was doing of herself suggested to imitative man the art, for example, of grafting. The sighs of the wind through the reeds invited man to invent the flute.** Once the stage in which physical strength counted for everything was past, music and song were cultivated. For the future there is an æsthetic as well as a material side to life.†† Throughout all these changes and chances man is travelling along the road to improvement of his mechanical appliances, the amelioration of his earthly lot. Time is required for this advance, for all growth is by infinitesimal steps.‡‡ Men do not become men at once; trees are only shrubs in their early life. To Lucretius, as to Diogenes Laertius (c. 412—323), time produces growth in everything. Little by little experience taught man to note the regular movements of the heavens and the return of the

* *De Nat. Rer.*, V, 1450.
† *Ibid.*, V, 1109-20.
‡ *Ibid.*, V, 1143-55.
§ *Ibid.*, V, 1028-90.
|| *Ibid.*, V, 1281-1307.
¶ *Ibid.*, V, 1350-60.
** *Ibid.*, V, 1382.
†† *Ibid.*, V, 1391.
‡‡ *Ibid.*, V, 181 ff. Cf. I, 310 ff.

seasons, foreshadowing the true nature of things. Once the will of the gods was deemed sufficient to account for everything, whereas now it is abundantly evident that there are natural causes at work. Once chance ruled all things in heaven and earth, whereas now clearly there is a sameness, an orderliness in the phenomena all around us.

The speculations of Lucretius are evidently expressed in the following lines:

And first the race she reared of verdant herbs,
Glistening o'er every hill; the fields at large
Shone with the verdant tincture, and the trees
Felt the deep impulse, and with outstretched arms
Broke from their bonds rejoicing. As the down
Shoots from the winged nations, or from the beasts
Bristles or hair, so poured the new-born earth
Plants, fruits, and herbage. Then, in order next,
Raised she the sentient tribes, in various modes,
By various powers distinguished: for nor heaven
Down dropped them, nor from ocean's briny waves
Sprang they, terrestrial sole; whence, justly, Earth
Claims the dear name of mother, since alone
Flowed from herself whate'er the sight surveys.
E'en now oft rears she many a sentient tribe,
By showers and sunshine ushered into day
Whence less stupendous tribes should then have risen
More, and of ampler make, herself new-formed,
In flower of youth, and Ether all mature.
Of these birds first, of wing and plume diverse,
Broke their light shells in springtime: as in spring
Still breaks the grasshopper his curious web,
And seeks, spontaneous, foods and vital air.

. . . . .

Hence the dear name of mother, o'er and o'er,
Earth claims most justly, since the race of man
Long bore she of herself, each brutal tribe
Wild-wandering o'er the mountains, and the birds
Gay-winged, that cleave, diverse, the liquid air.

The originality of the conception of Epicurus and Lucretius is so remarkable that it is not till the *Esquisse* of Condorcet (1743—1794) that we meet with a similar theory. Evolution there has been in the past: with that Lucretius stops. Evolution in the future he scarcely contemplates. He catches glimpses of the truth through the clouds, but there is no clearness in his vision.

The elder Pliny (23—79) counsels us "firmly to trust that the ages go on incessantly improving." * He, however,

* *Hist. Nat.*, XIX, 1-4.

feels more interest in collecting anecdotes than in collecting facts. About twelve years after Seneca's death he published his book on *Natural History.* He claims to have read 2,000 volumes of 100 authors, and in his Latin list he omits Seneca. The preface to the *Epitome of Roman History* which Florus (c. 60—138) has written anticipates ideas afterwards developed by Postel (1510—1581) and Lessing (1729—1781). The historian is clear that nations pass through a succession of ages similar to those of the individual. "If any one," he points out, "will consider the Roman people as if it were one man, and observe its entire course, how it began, how it grew up, how it reached a certain youthful bloom, and how it has since, as it were, been growing old, he will find it to have four degrees and stages." It is not important to consider these four degrees; it is important to see that an author in the reign of Trajan has been able to perceive them.

Of all the Roman writers on science none has greater claims on our attention than Seneca. For real learning he feels a genuine interest, but not for the study of what he regards as "useless letters," leaving to the one side such questions as whether the same poet wrote the *Iliad* and the *Odyssey,** or whether Homer or Hesiod was the earlier.† Stoic in the main as he was, he makes fun of the *grammatici.*‡ Learning was apt to become logomachy, philosophy to become philology.§ He has a sovereign contempt for the 4,000 volumes acquired by Didymus (52—128), for do they not discuss such questions as the birthplace of Homer, the moral character of Sappho and of Anacreon, and the like? ||

When he speaks of the restoration of the world, Seneca holds that when it pleases God it will produce things. Will there not be then the opening of a very happy era in which man, born under better auspices, will be ignorant of all crimes and will be innocent? ¶ This era, or rather this improved world, will, on the Stoic hypothesis, pass away, being replaced through fire by another. The conflagration notion made a strong appeal to the feelings, for with it the perpetual

* *Dial.,* X, 13, 1-9; cf. *Quæst. Natur.,* IV, 13, 1.

† *Dial.,* X, 13, sect. 6.

‡ *Ibid.,* sect. 3. § *Ibid.,* sect, 23. || *Ibid.,* sect. 24-34.

¶ *Quæst. Natur.,* III, chap. xxvii; cf. Plutarch, *De Commun. Nat.;* J. Lipsius, *Physiologiæ Stocicorum,* libri três, p. 258.

struggle between good and evil ceased. In the interval between the appearance of a new world the Deity enjoys a period of rest, during which he can leisurely meditate upon the universe that has vanished into smoke * and plan improvements in the one he is about to create. † The universe used to be happy and innocent.‡ Men lived together in the distant past in societies, willing to obey the strongest and wisest of their number §; none were tempted to wrong their neighbour. The "return to nature" notion is plain in his account of men dwelling in natural grottos or in the stems of trees, and obtaining nourishment from tame animals and wild fruits. In process of time they develop the arts, learning to bake, to build, and to make use of the metals. According to Seneca, his own age is one far removed from primeval simplicity, though it is no worse than others.|| It is necessary to distinguish between moral and material progress. Seneca, following Posidonius, believes that man had made progress in science and in the material arts of life, but that this advance in learning had been accompanied by a moral decline. The political economist to-day reckons that the awakening of human beings to the need of satisfying their wants is a mark in advance, and the greater the range of these wants the greater is the advance, whereas to Seneca the reverse of the conception held good.¶ There used to be no struggle for existence: the earth supplied sufficient food for all.** The moment gold was discovered, happiness fled: the love of it was indeed the root of all evil.†† A crowd now is an assembly of savage beasts, a spectacle of vice incarnate.‡‡ This pessimistic outlook on life is in no wise peculiar to Seneca: it is characteristic of first- and second-century thought.§§ There seems no indication that movement was

* Seneca, *Ep.*, 9, 16.

† *Quæst. Natur.*, III, 28, 7.

‡ Ep., *ad Lucilium*, 90; Horace, *Odes*, III, 14.

§ Seneca, *Ep.*, 90, 5.

|| *De Benef.*, I, 10. ¶ *Ep.*, 90, sect. 42. ** *Ep.*, 90, sect. 38.

†† *Ibid.*, 90, sect. 5, sect. 12, sect. 19, sect. 36.

‡‡ *De Ira*, II, 8, sect. 1; cf. II, 8, 9; *Ad Marc.*, II, 11, 17, 20.

§§ Lucretius, *De Nat. Rer.*, II, 1150, 1174; V, 66-7, 1429-30; Horace, *Odes*, I, 2, 14; III, 6; Tac., *Hist.*, I, c. iii; II, 37; Tac., *Ann.*, III, cc. xviii and xxvii; IV, c. i; XVI, c. xvi; Cicero, *De Opp.*, I, c. xxv; II, c. viii; III, c. xvii; *Tusc. Quæst.*, II, c. ii; Juvenal, *Satires*, 6, 10, 12, 13, 15; Seneca, *De Ira*, II, 8, 9; Seneca, *Quæst. Natur.*, II, c. xxxv; III, c. xxx.

thought of as a spiral and not as an unvarying round. There was not what Wordsworth called " the sweet air of futurity," or what George Meredith calls " the rapture of the forward view."

The security afforded by the Empire was sufficient to overcome internal disorders. With the *pax Romana* around him Seneca could indulge in speculations on progress. With Huxley he holds that though there are many clever men, honest folk are as scarce as ever; and this thought Rousseau (1712—1778) borrowed.* Still, Seneca maintains the sciences progress and their applications become more extensive. The sagacity of men contrives inventions.† We can live without science, for nature has allowed animals to exist; but as we create needs we devise arts to satisfy them. We receive these discoveries from our forefathers, and when we transmit them to our descendants we transmit an enlarged inheritance. " There remains yet and there will remain much to do; and the man who will be born a thousand years hence will not refuse the opportunity of adding something more." ‡

The *Natural Questions* goes far to explain the action of Gian Galeazzo in making not only Dante (1265—1321) but also Seneca have chairs founded in their memory and for discussion of their work. True, it is characterised by hypothesis not founded on experiment. True, the author is a moralist first, a physical scientist afterwards. To him there were no natural phenomena compared with the fascination virtue exercised over his soul.§ To him as to Kant (1724—1804) there is a bond between the starry heavens above and the moral law within. Throughout the *Natural Questions* he is well aware of the necessity of procuring correct data. Men like Lucilius suffered through Seneca's desire to have phenomena recorded accurately, especially when they were rare. He is anxious to be just to his predecessors: " First of all I feel bound to say in general terms that the old views are crude and inexact. As yet men were groping their way round truth. Everything was new to those who

* *Ep. ad Lucilium,* 95; Rouss., *Discours sur les sciences et les arts,* I, p. 20; cf. Montaigne, *Essais,* bk. I, chap. xxiv.

† Seneca, *Ep.,* 90; cf. Cicero, *De Legibus,* I, c. ix.

‡ Seneca, *Ep.,* 64.

§ *Ibid.,* 73, sect. 13.

made the first attempt to grasp it; only later were the subjects accurately investigated. But all subsequent discoveries must nevertheless be set down to the credit of those early thinkers. It was a task demanding great courage to remove the veil that hid nature, and, not satisfied with a superficial view, to look beneath the surface and dive into the secrets of the gods. A great contribution to discovery was made by the man who first conceived the hope of its possibility. We must therefore listen indulgently to the ancients. No subject is perfect while it is but beginning. The truth holds not merely of the subject [i.e. earthquakes] we are dealing with, the greatest and most complex of all, in which, however much may be accomplished, every succeeding age will still find something fresh to accomplish. It holds alike in every other concern: the first principles have always been a long way off from the complete science." * We are here far removed from the Platonic notion that the whole body of truth has been discovered. If there is the hope of the possibility of discovery, there is also the hope of the possibility of evolution.

This is more evident in the next quotation from Seneca: "It is not a thousand years since Greece 'counted the number of the stars and named them every one.' And there are many nations at the present hour who merely know the face of the sky and do not yet understand why the moon is obscured in an eclipse. It is but recently that science brought home to ourselves certain knowledge on the subject. The day will yet come when the progress of research through long ages will reveal to sight the mysteries of nature that are now concealed. A single lifetime, though it were wholly devoted to the study of the sky, does not suffice for the investigations of problems of such complexity. And then we never make a fair division of the few brief years of life as between study and vice. It must therefore require long successive ages to unfold all. The day will yet come when posterity will be amazed that we remain ignorant of things that will seem to them so plain." †

This book of Seneca's was the last word on science spoken by the classical world, and it is the only work of importance

* *Quæst. Natur.*, bk. VI, 5.
† *Ibid.*, bk. VII, 25.

bearing on science that has come down to us in Latin. Herein he possessed a marked advantage over Aristotle, whose *Physics* was written in Greek, a tongue much less familiar to the mediæval world. True, Lucretius unfolds ingenious speculations in the direction of evolution, but Seneca possesses true method that makes the discovery of evolution possible.

The *Physics* of Aristotle became a text-book of science to the men of the Middle Ages. It has been the infinite loss of mankind that the two following passages have not sunk deeply into the mind of Europe. "Aristotle has finely said," remarks Seneca, "that we should never be more reverent than when we are treating of the gods. We enter a temple with all due gravity, we lower our eyes, draw up our toga, and assume every token of modesty, when we approach the sacrifice. How much more is all this due when we discuss the heavenly bodies, the stars, the nature of the gods, lest in ignorance we make any assertion regarding them that is hasty or disrespectful; or lest we unwittingly lie. Let us not be surprised that what is buried so deep should be unearthed so slowly. . . . But all these questions [i.e. on comets] are foreclosed by my statement that they are not accidental fires, but inwoven in the texture of the universe, directed by it in secret, but not often revealed. And how many bodies besides revolve in secret, never dawning upon human eyes? Nor is it for man that God has made all things. How small a portion of His mighty work is entrusted to us!" * He then proceeds to draw attention to the new discoveries: "How many animals we have come to know for the first time in our days. Many too that are unknown to us the people of a coming day will know. Many discoveries are reserved for the ages still to be, when our memory shall have perished. The world is a poor affair if it do not contain matter for investigation for the whole world in every age. Some of the sacred rites are not revealed to worshippers all at once. Eleusis contains some of his mysteries to show to votaries on their second visit. Nature does not reveal all her secrets at once. We imagine we are initiated in her mysteries: we as yet but hanging around her outer courts. These secrets of hers are not open to all indiscriminately. They are withdrawn and shut up in the inner shrine. Of one of them this

* *Quæst. Natur.*, bk. VII, 30.

age will catch a glimpse, of another the age that will come after." *

In all the classical writings there are no four quotations so plain in their views of all that the future holds for the man of science. Were such statements much read? Take the evidence of Quintilian (40—100), who obviously thought Seneca an overrated man and placed Cicero far above him.† He has no doubt of the popularity of Seneca in his own times.‡ Moreover, was he not a Christian who corresponded with St. Paul? The Fathers reckoned him one of themselves. Jerome (345—420) frankly gave him rank among recognised ecclesiastical writers. His statements must therefore be orthodox. In the Middle Ages he was famous as the author of the *Natural Questions*, and still more so as a moralist. Dante terms him " Seneca morale." § He is quoted by writers like Albert Magnus (c. 1193—1280), Vincent of Beauvais (c. 1200—1264), Walter Burlay (1275—1357), John of Salisbury (c. 1110—1180), and Friar John of Wales (died c. 1285), who were acquainted with the *Natural Questions*, and by writers such as Otto of Freisingen (died 1158) and Giraldus Cambrensis (c. 1146—1220) oftener than Cicero or " Cato." Some of the manuscripts of the *Natural Questions* only contain books I-IV, and this was probably the only part generally known. || Books VI and VII, which give us the four prophetic quotations, were largely unknown. It is therefore not surprising that the only mediæval writer who quotes passages from the *Natural Questions* with a distinct consciousness of the possibility of future progress in discovery is Roger Bacon (1214—1292).¶ Walter Burlay and John of Salisbury knew it indirectly. The latter recommends expressly its perusal** and uses terms borrowed from it.†† In the *Annales Colimenses Maximi*,‡‡

* *Quæst. Natur.*, bk. VII, 31.

† Inst. *Orat.*, X, 1, 125-8. ‡ *Ibid.*, 125. § *Inf.*, IV, 141.

|| There is a copy in the library of St. Augustine's Abbey, Canterbury (cf. M. R. James's *Ancient Libraries of Canterbury and Dover*, p. 305) and at Eton (cf. M. R. James's *Catalogue of MSS, at Eton College*, p. 31).

¶ Bacon's *Metaphysics*, which is in Charles's monograph, quotes *Quæst. Natur.*, VI, 5, sects. 2, 3; VII, 25, sects. 3, 4.

** *Policraticus*, II, 320 (Webb).

†† *Ibid.*, I, 70; John of Salisbury borrows from *Quæst. Natur.*, I, 11, 1-2. ‡‡ *Mon. Germ. Script.*, XVII.

A.D. 1235, there is a reference to the section of the *Natural Questions* discussing halves.* It is therefore practically certain, however, that all the mediæval references to this book quote it as an authority for natural phenomena except in the case of Roger Bacon, who discerns in it an incentive to future progress.

If any one is anxious to understand the originality of St. Paul's conception of the future, the ideal method is to peruse some of the authors here cited. As one reads them one wonders that all save Seneca stop short at the very point which is of the greatest interest, the nature of the future, the sort of evolution to which it will give rise. A perusal of the *Natural Questions* and then a perusal of the Epistle to the Ephesians enable one to grasp in some measure the originality of St. Paul. Indeed the true idea of progress is a creation of Christianity, forming one of its finest achievements. The transition from the Apostle of the Gentiles to St. Augustine is easy, for the thoughts of the two men were singularly kindred. Theologian as St. Augustine primarily is, the invasions of the barbarians forced him to become an observer. Society, according to him, is divided into two orders: one is the ordinary society of men, the other is the society of men who live according to God. Paganism represents one city, Christianity the other.† He views the history of Rome in the light of the establishment of the "Civitas Dei." This establishment constitutes progress for humanity.‡ Christianity, however, is no radical innovation without roots in the past; the ages have been a preparation for it. In spite of digressions, the *Civitas Dei* is devoted to the moral evolution of history. The providence of God in the life of the world is the burden of its message.§ St. Augustine stood as firmly for this belief in the Christian world as the Stoic did in the ancient world. This conception is indeed the consummation of the moral and religious evolution of humanity. The light of God appears everywhere: it shines under Moses and the prophets; it flickers under the patriarchs; and it enlightens the world in Jesus

* In *Apocrypha Anecdota* (ed. M. R. James), Cambridge, 1893.
† *De Civ. Dei,* XVI, 1.
‡ Cf. Vincent of Beauvais, XXVI-XXX.
§ *De Civ. Dei,* V, 11; cf. V, 1.

Christ, greater than the patriarchs, greater than the prophets. With Clement of Alexandria (150 or 160—c. 213) St. Augustine recognises that other beliefs, other ideas, prepared the time for Him who is the Light of the World.* The world advances, thanks to Christianity, towards perfection. From God alone comes such a consummation; from Him we hope for eternal life. As the world therefore advances, to St. Augustine the cycle theory is sheer madness. Jesus Christ died once: He will die no more, for death hath no more dominion over Him.

It is noticeable that St. Augustine does not ignore the development of industry † through the ages, and makes a notable application of it in his consideration of the destiny of man. He can allow no activity to be outside or apart from God. There is a complete gradation of nature: there is also a complete gradation of soul. There is, he observes, a wide difference between the evolution of humanity and the evolution of the individual. Old age is perfect in the former: it is feeble and decadent in the latter. Here is the germ of the idea which lies in the background of all the philosophy of progress in the seventeenth and eighteenth centuries. St. Augustine perceived it clearly. Limiting himself to the study of civilisation which has preceded Christianity, he compares the education of the human race to that of a single man; it must follow the progressive succession of the ages in order to raise itself, by degrees, from time to eternity, from the visible to the invisible.‡ "Divine Providence, which guides marvellously all things, governs the succession of generations, from Adam to the end of the ages as a single man." § In observing the action of God in history St. Augustine also observes the successive epochs of humanity, the steps towards progress. There are three epochs: youth, characterised by the absence of law, from Adam to Abraham; the virile age from Abraham to Christ, which is the epoch of law; at last, old age, which is the era of Christianity and the epoch of grace. || In each of these three epochs there

* *De Civ. Dei,* II, especially 28.

† *Ibid.,* XXII, 24. ‡ *Ibid.,* X, 14; cf. XXII, 24.

§ *De quæstionibus,* octoginta tribus, quæstio 58.

|| *De Civ. Dei,* XV-XIX. Cf. Godet's striking remark: "L'histoire du monde dans essence se résume dans trois mots: il vient; il est venu; il revient" (*Etudes Bibliques N.T.,* p. 292).

are subdivisions, and, following the procedure of the Jewish schools, he seeks parallels in other eras. He compares the six epochs of the world to the six days of creation, seeking analogies between the events of each period and the works of each day of creation. For example, the third epoch is distinguished by the separation of the people of God from other peoples: similarly the third witnessed the separation of the earth from the waters. In *De Genesi contra Manichæos* he returns to a consideration of the ages of the world, adding a seventh to correspond to the seventh day.* Then the Lord will stand forth in clearness; then will those find rest with Christ to whom He said, "Be ye therefore perfect, as your Father in heaven is perfect." This seventh day will not be quite like the other six: there will be no night. The perfection then attained in Christ will be eternal.

St. Augustine formulates a serious contribution to the growth of humanity, and the steps he traces there were not devoid of stimulus to those who tried to trace evolution in the kingdom of animals as well as in the kingdom of men.† Like Gregory of Nyssa (332—395), he adopts an explanation of the Creation which is in part naturalistic. He does not dream of thinking of the six days of the Creation as in any wise equivalent to the solar days. In commenting on the passage, "In the beginning God created the heaven and the earth," he writes: "In the beginning God made the heaven and the earth, as if this were the *seed* of the heaven and the earth, although as yet all the matter of heaven and of earth was in confusion; but because it was certain that from this heaven and the earth would be, therefore the material itself is called by that name." ‡ He thinks of Creation as of things in process of coming into due order, "not by intervals of time, but by series of causes, so that those things which in the mind of God were made simultaneously might be brought to their completion by the sixfold representation of that one day." § In his view of the origin of life he stands midway between biogenesis and abiogenesis. It is perhaps too much

* *De Genesi contra Manichæos,* I, 24; cf. Tertullian, *De Virginibus relandes,* c. i.

† Cf. W. Cunningham, *S. Austin,* p. 114.

‡ *De Genesi contra Manichæos,* bk. I, sect. 2 (vii).

§ *De Genesi ad Litteram,* bk. V, sect. 12 (v).

to say that he put forward a theory of evolution, but he plainly rejected the doctrine of special creation.

In the twelfth century Hugh de St. Victor (1098—1141), inspired by St. Augustine, considers evolution, in a loose sense, as the universal law of creation; even the angels make advances towards perfection. All creatures share in these advances till the day of judgment, when all will share the immutability and perfection of God Himself. Of course, with some thinkers like Gregory of Tours (539—593), Lambert of Hersfeld (flor. c. beginning of the eleventh century), and Otto of Freisingen, the view of a catastrophic end of the world prevails. With St. Hugh de St. Victor the painful march of the race towards perfection is in no wise a consequence of the fall. There exists a trace of the golden age hypothesis in the notion that all things were perfect in the very principle of creation in so far as God *directly* called them into being. Everything else arriving after the first process of creation is subject to the law of gradual growth, beginning with imperfection and ending with perfection. This is clear in the vegetable and animal worlds, and is no less clear in the world of the human race.* Hugh de St. Victor and St. Thomas Aquinas (1227—1274) insist that all truth is one, that there is a progressive revelation of it, that as the coming of the Saviour drew near the knowledge of the truth increased.† The sacraments of the law of nature shadowed forth the truth; those of the law of Moses were its image; and those instituted by Jesus Christ are the reality. That is, the early is a preparation for the later, but all are fundamentally one.‡

According to St. Thomas Aquinas, "it is natural for human reason to arrive by degrees from the imperfect to the perfect. Hence the early philosophers taught imperfect truth, which afterwards was more clearly discovered by their successors." It is exactly the same with the practical sciences; from many standpoints the early inventions were defective, later these defects were corrected, with the result that machines were improved.§ He maintains, however, that faith remains

* Hugo de Sancto Victore, *Summa,* lib. I, part VI, c. xiv.

† Hugo de Sancto Victore, *De Sacramentis,* lib. I, part XI, c. vi; St. Thomas Aquinas, *Summa contra Gentes,* IV, 57; *Op.,* IX, p. 493.

‡ Hugo, *De Sacramentis,* lib. I, part XI, c. vi; lib. I, part XII, c. iii.

§ *Summa Theologica,* prima secundæ, quæst. 97, art. 1.

constant as thoroughly as Newton believed in the law of change, of development. Dogmas are seemingly increasing in number. In reality it is not so, for the germ of them lies in the creeds of the primitive Church.* To us dogma suggests a superfluous garment which trammels and incommodes the mind. The Stoics and St. Thomas Aquinas realised the bitter need of dogma felt by minds which have been stripped to the winds of heaven. They were acutely aware that an unsolved enigma means intellectual discomfort. Therefore St. Thomas bends all his energies to the removal of the unsolved. There is another method of overcoming the difficulty: truth is unchanged, though its aspects are always changing.† The law of Moses was good, argues St. Thomas in the spirit of St. Augustine and Prudentius, but it was not perfect. Was not, for example, grace lacking? ‡ He holds the outline of the doctrine of development, but he holds it as an ecclesiastic. Take an example. Why, he asks, was not the New Law of Christ bestowed upon men from the dawn of creation? The answer is St. Augustine's: "The Gospel has not been preached to the first men because it contains the law of perfection; now perfection cannot exist in the very beginning of things." § If we compare the law of Moses with that of Christ, the former is unquestionably imperfect; but if we compare it—and it is the only proper comparison—with the needs of the men for whom it has been provided, it was relatively perfect.|| The Mosaic law is the germ of the law of Christ just as much as the seed contains the essence of the tree.¶ Here the comparative standpoint is adopted, and had its consequences been realised it would have constituted one of the greatest forward steps that man has ever taken. Its consequences, alas! were not realised till the days of our own fathers.

St. Thomas Aquinas possessed the Stoic passion for definition. It is possible to meet with passages in the *Summa Theologica* which may be taken to mean that he had a vague conception of something that, in the hands of a

* *Summa Theologica,* Secundæ scunda, quæst. 1, art. 7.
† *Ibid.,* quæst. 16, art. 8.
‡ *Ibid.,* prima secundæ, quæst. 97, art. 1.
§ *Ibid.,* quæst. 106, art. 3.
|| *Ibid.,* quæst. 98, art. 2.
¶ *Ibid.,* quæst. 107, art. 3.

dialectician, might be called a theory of evolution, just as in his *De Regimine Principum* he has the idea of a contract made between the king and the people. Influenced by the teaching of St. Augustine, he lays down: "As to production of plants, Augustine holds a different view, . . . for some say that on the third day plants were actually produced, each in his kind—a view favoured by the superficial reading of the Scripture. But Augustine says that the earth is then said to have brought forward grass and trees causaliter; that is, it then received power to produce them." When he discusses Genesis ii. 4, he remarks that "in those days . . . God made creation primarily or causaliter, and then rested from His work." From passages like these it is evident that it would be as fair to call St. Thomas an advocate of Whiggism or of democracy as an evolutionist. Indeed a candid perusal of the *Summa Theologica* at once reveals the fact that the mind of this great thinker was pre-scientific. The idea that there might be endless knowledge, the view of Seneca, was outside his scheme of things. The field of learning was strictly bounded, and his mind was quite competent to explore every part of it. Dean Colet (1466—1519) protested, not against the ignorance of St. Thomas Aquinas—for no one could accuse the great Italian of lack of information—but against his confidence in thinking that he could define everything.

Europe went through—and required to go through—three Renaissances: the first in the eighth century; the second in the twelfth; the third in the fourteenth and fifteenth centuries. The first reintroduced something of the old Roman education; the second introduced Aristotle and the learning of the Arabs; the third resuscitated the whole culture of the classical world. The first prepared the way for the second; the second for the third. The third originated that new birth of the human spirit which we emphatically call the Renaissance. Admiration for antiquity became its hall-mark. Art and Literature threw off the forms of mediævalism and looked for all their inspiration to the models of the ancient world. Platonic societies were formed in Italy, and Plato was found to be a theologian, a prophet. The New Learning tended in many quarters to place Plato on the pedestal formerly occupied by Aristotle. That is, the scholar substituted for the works of a thinker with possibilities of scientific progress foreshadowed, the works of one whose ideal lay in the past. In political circles, as well as in literary and scientific, it was not rare to meet with the ancient notion of the circular theory of the movement of peoples and civilisations. If on the one hand there are the names of Rabelais (c.1495—1553), Campanella (1568—1639), and Francis Bacon (1561—1626), on the other there are the no less renowned names of Machiavelli (1469—1527), Bodin (1530—1596), and Montaigne (1533—1592).

The geographical discoveries of the age brought into prominence cycles of another kind, the cycle of incessant movement—growth, expansion, short-lived conquest, followed by shrinkage, defeat, expulsion, or absorption by another set of migrants. The written history of mankind is to be read largely in the shiftings of peoples, now going forward, then thrusting back. Society was approaching a dynamic stage, though of course it never is static. The great service Copernicus (1473—1543) rendered to mankind was the conception of perpetual motion of this world. Motion there is in the worlds above, and incessant motion there is in the worlds beneath. Petrarch (1304—1374) is sometimes called the first modern man, and on the literary side a case may be made out for this designation. He was, however, as blind as Dante (1265—1321) to the forces about him which made for political and scientific progress. What was fatal to the work of Dante was the work of Copernicus. There was no longer any distinction between the heavens and the earth. True, the earth became a heavenly body, but for all time to come the substance of the heavenly was precisely the same as that of the earthly. It was no longer possible to credit the belief that the stars influenced the destiny of man, for their motions were governed by the same laws as that of the globe we inhabit. Man was once more a mote in the unfathomable universe. Four generations after Copernicus, Blaise Pascal (1623—1662) could say, "Le silence éternel de ces espaces m'effraie." The first modern man was the astronomer, the first to cherish a scientific conception of evolution in the heavens.

Naturalists like Leeuwenhoek, Malpighi, and Swammerdam contributed during the second half of the seventeenth and the beginning of the eighteenth century to the study of the smaller organisms. They provided facts, but they did not provide principles. These were set going from the days of St. Augustine, who suggested that there were stages in the history of the world just as there were stages in the history of the animals, the relations of man. Seneca was every whit as much a Stoic as he was a scientist, and it is out of our power to separate the thought of evolution in theology from the thought of evolution in science. The remarkable fact in modern times is that the stimulus given to evolutionary studies came much more from the moral philosophers than from the scientists proper. As Mr. Osborn takes care to show: "It is a very striking fact, that the basis of our modern methods of studying the Evolution problem was established not by the early naturalists nor by the speculative writers, but by the Philosophers. They alone were upon the main track of modern thought. It is evident that they were groping in the dark for a working theory of the Evolution of life, and it is remarkable that they clearly perceived from the outset that the point to which observation should be directed was not the past but the present mutability of species, and further, that this mutability was simply the variation of individuals on an extended scale. Thus Variation was brought into prominence as the point to which observation should be directed."* Bacon pointed out the evidence for variation in plants and animals and the bearing of this upon the production of new species. Leibniz advanced beyond Bacon's position in indicating that the evolution of life was a necessary part of a system of cosmic philosophy. Kant's conception of evolution is one of the most comprehensive, embodying in it the views of philosophers from Aristotle onwards.

Francis Bacon grasped the idea of the renewal of the modern world by the aid of the intellectual labours of successive generations.† He is often reproached with making no real contribution to science. The criticism is just, but it is not well founded. His rôle was that of a herald. "I am but

* H. F. Osborn, *From the Greeks to Darwin*, p. 87.

† *De Dign. et Augm. Scientiarum*, I, 20; II, 23; IV, 111; V, 114; VIII, 287-8 (1638 ed.). Cf. *Novum Organum*, I, aphor. 56, 78, 84, 92; *De Sapientia Veterum*, 315-6 (1638 ed.); *Nova Atlantis*, 367 ff. (1638 ed.).

a trumpeter," he proclaimed, "not a combatant." Scientific investigators work, as a rule, on facts and observations they collect. Bacon urged them to amass facts and evolve cosmos out of chaos. His method is wrong; still there is no mistaking the enthusiasm of the man who writes that "without such a natural and experimental history . . . no progress worthy of the human race in Philosophy and the Sciences could possibly be made; whereas if such a history were once provided, and well ordered, with the addition of such auxiliary and light-giving experiments as the course of Interpretation would itself suggest, the investigation of Nature and of all the Sciences would be the work of only a few years." * In this fashion he hopes to get rid of the ancient hypothesis that men are condemned to return always in a circle.

Did not the schoolmen employ experience? Truly they did, but it was not to consult her as an adviser, but to drag her at their chariot-wheels as a captive. In his *Historia Vitæ et Mortis,* Bacon is well aware of the utility of provisional hypotheses. In the preface to his *magnum opus* Copernicus had announced, "Neither let any one, so far as hypotheses are concerned, expect anything certain from Astronomy; since science can afford nothing of the kind." Bacon attacked the Copernican discovery, and no doubt some of his hostility was prompted by the circumstance that the astronomer was pragmatic in his outlook. To Bacon science was making such progress that he could not bear this pessimistic philosopher. In his *Advancement of Learning,* which he published in 1605, he insists on the wisdom of providing readers in science and of providing the expenses of the experiments these men undertake. The foundation of the Royal Society was one day to be the outcome of his ideas. It is scarcely three centuries since the idea of the possibility of indefinite progress through man's own conscious efforts first emerged in the minds of a few thoughtful persons. It is to Bacon the glory is due of first popularising this seminal idea, one of the greatest single ideas in the whole history of mankind in the vista of possibilities it opens before us.

Bacon's *Novum Organum* is filled with hope. He raised

* Preface to *Parasceue ad Historiam Naturalem et Experimentalem.*

the problem of the mutability of species as a possible result of the accumulation of variations. "In the eighth rank of prerogative instances," he remarks, "we will place deviating instances, such as the errors of Nature or strange and monstrous objects, in which Nature deviates and turns from her ordinary course. For the errors of Nature differ from singular instances, inasmuch as the latter are the miracles of species, the former of the individuals. Their use is much the same, for they rectify the understanding in opposition to habit, and reveal common forms. For with regard to these, also, we must not desist from inquiry till we discern the cause of the deviation; the cause does not, however, in such cases rise to a regular form, but only in the latent process towards such a form, for he who is acquainted with the path of Nature will more readily observe their deviations, and vice versa, he who has learnt her deviations will be able more accurately to describe her paths." * There is no reason to believe that the investigator is speaking here: it is the prophet with which we are concerned, the man who could divine that "plants sometimes degenerate to the point of changing into other plants."

In his *Novum Organum* he proceeds to hint that man can produce variations experimentally, and that living objects are well adapted to experimental work: "They differ again from singular instances, by being much more apt for practice. For it would be very difficult to generate new species, but less to vary known species, and thus produce many rare and unusual results. The passage from the miracles of Nature to those of Art is easy; for if Nature be once seized in her variations and the cause be manifest, it will be easy to lead her by Art to such variation as she was first led to by chance; and not only to that, but others, since deviations on the one side lead and open the way in every direction."

In the following passage his acumen enables him to perceive the presence of transitional forms in Nature: "In the ninth rank of prerogative instances we will place bordering instances, which we are also wont to term participants. They are such as exhibit those species of bodies which appear to be composed of two species, or to be the rudiments between one and the other. They may well be classed with the

* *Novum Organum,* bk. II, sect. 29.

singular or heteroclite instances; for in the whole system of things, they are rare and extraordinary. Yet from their dignity they must be treated of and classed separately, for they point out admirably the order and constitution of things, and suggest the causes of the number and quality of the more common species in the Universe, leading the understanding from that which is, to that which is possible. We have examples of them in Moss, which is something between putrescence and a plant; in some Comets, which hold a place between stars and ignited meteors; in Flying Fishes, between fishes and birds; and in Bats, between birds and quadrupeds."

What an Englishman suggested a German proceeded to develop. Gottfried Wilhelm Leibniz (1646—1716) was familiar with the writings of Bacon, who enforced on him his views on variation. Influenced by Aristotle, Leibniz expressed the law of continuity as applied to life: "All natural orders of beings present but a single chain, in which the different classes of animals, like so many rings, are so closely united that it is not possible either by observation or imagination to determine where one ends or begins." His conception of continuity is clear in the following: "All advances by degrees in Nature, and nothing by leaps, and this law as applied to each, is part of my doctrine of Continuity. Although there may exist in some other world species intermediate between Man and the Apes, Nature has thought it best to remove them from us, in order to establish our superiority beyond question. I speak of the intermediate species, and by no means limit myself to those leading to Man. I strongly approve of the research for analogies; plants, insects, and Comparative Anatomy will increase these analogies, especially when we are able to take advantage of the microscope more than at present." Huxley quotes a passage from the *Protogæa* * which proves that Leibniz had not merely a law of continuity, a law of perfectibility, but also thought on the mutability of species. In discussing the fossil Ammonites related to the living Nautilus he notes: "Some are surprised that there are to be seen everywhere in rocks such objects as one might seek for in vain elsewhere in the known world, or certainly, at least, in

* *Protogæa*, XXVI; cf. T. H. Huxley, *Darwiniana*, p. 208.

his own neighbourhood. Such are the horns of Ammon (Ammonites), which are reckoned a kind of Nautilus, although they are said to differ always both in form and size, sometimes indeed being found a foot in diameter, from all those animal natures which the sea exhibits. Yet who has thoroughly searched those hidden recesses or subterranean depth? And how many animals hitherto unknown to us has a new world to offer? Indeed it is credible that by means of such great changes (of habitat) even the species of animals are often changed." In his world there are endless monads and each of them is the centre of an endless evolution.

Most questions in moral philosophy have been so altered by the thought of Immanuel Kant (1724—1804) that one is not altogether unprepared to discern his potency in the world of evolution. By his grand nebular hypothesis he suggested the possible development of stars, suns, planets, and satellites by the slow contraction of diffuse and incandescent haze-clouds. If it be true that great minds think alike, we can grasp the fact that about the same time there occurred to Buffon and him ideas of selection and adaptation, of environment and inheritance. Following in the steps of Newton, who noted the uniformity of structure which pervades animal types, and Leibniz, who noted the possible perfectibility of monads, Kant in 1755 published his *The General History of Nature and Theory of the Heavens*, which endeavoured to reconcile Newton and Leibniz from the mechanical and teleological standpoints. Influenced also by Lucretius, Kant adopted an attitude unlike that of his former book when he published in 1780 his *The Teleological Faculty of Judgment*. In the former he considers the world to be under the domain of natural causes. In the latter he divides the world into the inorganic in which natural causes prevail, and the organic in which the teleological principle prevails. His awe of the starry heavens predisposed him to think that it was next to impossible for the mind of man to discover all the laws of the universe.

In 1763 he traced back all the higher forms of life to simpler elementary forms. He notes the changes produced in man by migration, differences of climate, and deduces the law of degeneration from the originally created types of species. What is true of the world of animals is just as true of the world of man. Is there not stage after stage in both? Is not man in fact an animal? In 1790 Kant wrote a pregnant passage: "It is desirable to examine the great domain of organised beings by means of a methodical comparative anatomy, in order to discover whether we may not find in them something resembling a system, and that too in connection with their mode of generation, so that we may not be compelled to stop short with a mere consideration of forms as they are—which gives us no insight into their generation—and need not despair of gaining a full insight into this department of Nature. The agreement of so many kinds of animals in a certain common plan of structure, which seems to be visible not only in their skeletons—so that a wonderfully simple typical form, by the shortening and lengthening of some parts, and by the suppression and development of others, might be able to produce an immense variety of species—gives us a ray of hope, though feeble, that here perhaps some results may be obtained, by the application of the principle of the *mechanism of Nature,* without which, in fact, no science can exist. This analogy of forms (in so far as they seem to have been produced in accordance with a common prototype, notwithstanding their great variety) strengthens the supposition that they have an actual blood relationship, due to derivation from a common parent; a supposition which is arrived at by observation of the graduated approximation of one class of animals to another, beginning with the one in which the principle of purposiveness seems to be most conspicuous, namely man, and extending down to the polyps, and from these even down to mosses and lichens, and arriving finally at raw matter, the lowest stage of Nature observable by us. From this raw matter and its forces, the whole apparatus of Nature seems to have been derived according to mechanical laws (such as those which resulted in the production of crystals); yet this apparatus, as seen in inorganic beings, is so incomprehensible to us, that we feel ourselves compelled to conceive for it a different principle. But it would seem that the archæologist of Nature is at liberty to regard the great *Family* of creatures (for as a Family we must conceive it, if the above-mentioned continuous and connected relationship has a real foundation) as having sprung from the immediate results of her earliest revolution, judging from all the laws of their mechanisms known to us or conjectured by him." *

The classical and the mediæval conception of evolution had been preserved by Bacon, Leibniz, and Kant. Bacon was a philosopher and nothing else. Leibniz and Kant were philosophers and mathematicians. During the eighteenth century we meet men who are partly philosophers, partly naturalists. Huxley has drawn attention † to Benoit de Maillet (1656—1738), whose *Telliamed* was written before the time of Haller and Bonnet, of Linnæus and Hutton. Influenced by Empedocles, de Maillet traces a theory of transmission of acquired characters. Habit and transformation are his leading explanations of all metamorphoses. All terrestrial animals have their origin in marine forms. Birds come from flying-fishes. Lions came from sea-lions, and man from l'homme marin, the husband of the mermaid! Huxley points out that de Maillet entertains a definite conception of the plasticity of living things, but he omits to mention that de Maillet is capable of thinking that this plasticity can take place in a single life and he also omits de Maillet's pedigree of man. Like St. Augustine, de Maillet interprets the days of Genesis as so many gradual periods or epochs.

Pierre Louis Moreau de Maupertuis (1698—1759) bore traces of Greek thought in his speculations, for Democritus and Anaxagoras left their mark upon him. There are to Maupertuis psychical properties of the higher organisms in all material particles, and these constitute the link between the organic and the inorganic worlds. As he assumes that non-living matter holds properties of living matter, he can easily derive the latter from the former. He finds an origin of new species in supposing that the elementary particles may not always retain the same order: there may be chance combinations producing differences which result in the infinite variety of species.

Denis Diderot (1713—1784) continued the teaching of Empedocles in his anticipation of the doctrine of natural

* Schultze drew Mr. Osborn's attention to this notable passage, and Mr. Osborn drew mine.

† *Darwiniana*, p. 208.

selection. His reasoning he puts into the mouth of one Saunderson. As to anterior states, Saunderson tells us * : "You have no witnesses to confront with me, and your eyes give you no help. Imagine, if you choose, that the order which strikes you so profoundly has subsisted from the beginning. But you leave me free to think that it has done no such thing, and that if we went back to the birth of things and scenes, and perceived matter in motion and chaos slowly disentangling itself, we should come across a whole multitude of shapeless creatures, instead of a very few creatures highly organised. If I have no objection to make to what you say about the present condition of things, I may at least question you as to their past condition. I may at least ask of you, for example, who told you—you and Leibniz and Clarke and Newton—that in the first instances of the formation of animals, some were without heads and others without feet? I may maintain that these had no stomachs, and those no intestines; that some to whom a stomach, a palate, and teeth seemed to promise permanence, came to an end through some fault of heart and lungs; that the monsters annihilated one another in succession, that all the faulty (*vicieuses*) combinations of matter disappeared, and that *these only survived whose mechanism implied no important misadaptation (contradiction), and who had the power of supporting and perpetuating themselves.*

"On this hypothesis, if the first man had happened to have his larynx closed, or had not found suitable food, or had been defective in the parts of generation, or had failed to find a mate, then what would have become of the human race? It would have been still enfolded in the general depuration of the universe; and that arrogant being who calls himself Man, dissolved and scattered among the molecules of matter, would perhaps have remained for all time hidden in the number of mere possibilities.

"If shapeless creatures had never existed, you would not fail to insist that none will ever appear, and that I am throwing myself headlong into chimerical hypotheses. But the order is not even now so perfect, but that monstrous products appear from time to time."

Saunderson continues to enlarge his views. "I con-

* I use Lord Morley's translation on p. 94 ff. of his *Diderot,* I.

jecture then," he proceeds, "that in the beginning when matter in fermentation gradually brought our universe bursting into being, blind creatures like myself were very common. But why should I not believe of worlds what I believe of animals? How many worlds, mutilated and imperfect, were peradventure dispersed, then re-formed, and are again dispersing at each moment of time in those far-off spaces which I cannot touch and you cannot behold, but where motion combines and will continue to combine masses of matter, until they have chanced on some arrangement in which they may finally persevere! O philosophers, transport yourselves with me on to the confines of the universe, beyond the point where I feel, and you see, organised beings; gaze over that new ocean, and seek across its lawless, aimless heavings some vestiges of that intelligent Being whose wisdom strikes you with such wonder here!

"What is this world? A complex whole, subject to endless revolutions. All these revolutions show a continual tendency to destruction; a swift succession of beings who follow one another, press forward, and vanish; a fleeting symmetry; the order of a moment. I reproached you just now with estimating the perfection of things by your own capacity; and I might accuse you here of measuring its duration by the length of your own days. You judge of the continuous existence of the world, as an ephemeral insect might judge of yours. The world is eternal for you, as you are eternal to the being that lives but for one instant. Yet the insect is the more reasonable of the two. For what a prodigious succession of ephemeral generations attests your eternity! What an immeasurable tradition! Yet shall we all pass away, without the possibility of assigning either the real extension that we filled in space, or the precise time that we shall have endured. Time, space, matter—all, it may be, are no more than a point."

Diderot sent a copy of his work to Voltaire. The poet replied with courtesy, but declared his dissent from the conclusions of Saunderson, "who denied God, because he happened to have been born blind." And indeed there is colour-blindness on the part of not a few scientists. Huxley himself asked Professor Haughton of Trinity College, Dublin, could he account for the circumstance that he was an agnostic and Haughton was a Christian scientist. Haughton reflected for a moment, and then replied, "Perhaps, in this matter, you are colour-blind." "Of course," answered Huxley, "if I were colour-blind I should not know it." We might contend that Diderot, one of the heroic sceptics of the eighteenth century, would very likely disown many of those who profess his scepticism now. For scepticism, when a virtue, is always an opportunist virtue, and it may become a vice when the circumstances that have justified it have passed away. The man who, like Diderot, is an heroic sceptic when the world is cumbered with a mass of false doctrine, might in another age affirm instead of denying and might oppose the sceptic of that age every whit as eagerly as he opposed the dogmatists of his own time. For scepticism itself may be a false doctrine instead of the enemy of false doctrines. It may become an end instead of a means, and a drug to the intellect rather than a spur. In fact there are two kinds of sceptics; and it is very easy for the baser kind to flatter themselves that they are followers of the nobler. Diderot was a sceptic, but he was made one by his passion for truth; and that passion was not the least sceptical. For years he toiled at the *Encyclopædia* at a salary of about £120 a year, and mere disbelief would never have impelled a man of such gifts to make so great a sacrifice for it. He disbelieved many things because his faith in truth and in the value of knowledge was so strong; he was content to destroy because he believed that truth would prevail when error was swept away.

There are sceptics who do not believe in truth or that it can ever prevail, and, unlike Diderot, they find it easy to accept beliefs readily enough just because they do not believe in them. In an age of dogmatism they are comfortable dogmatists; in an age of scepticism they are equally comfortable sceptics. But in any case their dogmatism is sceptical and their scepticism dogmatic. When they seem to affirm anything they are only denying that the effort after truth is worth making; when they seem to deny anything they are only affirming that truth cannot be discovered. To them scepticism it not a means to an end, but an end in itself. It is an animal lethargy of the mind which they flatter with a philosophic name. Low forms of life seem to have adapted themselves more perfectly to circumstance than the higher forms; and there is the same perfection of adaptation in this low kind of scepticism. Whether it wears the guise of belief or disbelief, its aim is always mere comfort; and it can only be stirred to anger by anything which threatens its comfort. The Sadducees, the comfortable sceptics of their time, joined with the Pharisees against Christ; and they did so, no doubt, not because He destroyed, but because He affirmed. The baser sceptic tolerates an old affirmation because he knows by experience that it will not interfere with his comfort; but he hates a new one because he does not know how uncomfortable it may make him. He is as hostile to the passion for truth as to the passion for righteousness; for both of these try to answer questions, and he likes to ask them only because he is sure that they cannot be answered.

A passionate sceptic like Diderot falls into the habit of destruction, because in his time there is so much that needs to be destroyed. Like every man who would do great things, he becomes a specialist and sacrifices some of his own virtue to his specialism. He knows well enough that there must be reconstruction, but he leaves that to the future when the destruction is accomplished. Unfortunately his habit of destruction is often mistaken by his followers for his peculiar virtue, and they persist in it out of mere imitation. They think that they are heroic revolutionaries like him when they continue to deny from mere conservatism. Revolutions, in thought as in politics, cannot continue for ever; and the effort to continue them is merely an effort to maintain anarchy. A sceptic like Diderot is a soldier who practises the arts of war in thought; but when he has won his victories it becomes necessary to practise the arts of peace. He, because he was a great soldier of thought, would have known this, but the mechanical sceptic who persists in fighting the errors of the past does not know it. He still enjoys slaying the slain and winning easily victories that once were hard and glorious. He is brave against superstitions that no one now believes in and against old evils which he does not recognise in a new form. He is, in fact, obsolete himself, and not the less so because he is the attacker, not the defender, of lost causes. There are White Rose leagues of scepticism as well as of loyalty; and they do not even possess the advantage of being romantic. They deny safely what no one affirms;

but Diderot denied at his peril what the whole world affirmed, and by the peril of his denial proved that there was affirmation behind it. For men are generally right in what they affirm and wrong in what they deny—at least after the Diderot stage has been negotiated.

Charles Bonnet (1720—1793) champions the Greek doctrine of pre-existing germs, holding that all living things proceed from them. It is only in a loose sense that he can be called an evolutionist, though he was the author of this term, deriving it from *e-volvo*. Following the law of continuity of Leibniz, he came to the conclusion that no such thing as generation, in the strict sense of the term, occurs in nature.*

J. B. René Robinet (1735—1820) followed in the steps of Leibniz in holding the law of continuity and of de Maillet, pursuing his line of thought, though more soberly. Borrowing a mistaken interpretation of Aristotle, he conceived evolution in the Master's large-minded fashion. Like Maupertuis, he minimises the differences between organic and inorganic, reaching an "échelle des êtres" which embraces all matter. Unlike Bonnet and de Maillet and like Leibniz, he was a uniformitarian long before the days of Lyell. Holding Leibniz's law of continuity, he imagines that Nature has for her aim a movement towards the perfection of each type. From the very beginning she meant to produce man, her *chef-d'œuvre*, and the higher apes appear as her last efforts before she triumphed in making man. Fossils, minerals, dogs, horses, orang-outangs—are not these the experiments of nature? Man is simply the last of the series, and even he may be replaced. Curiously enough, like Lucretius, Robinet has no true idea of the gradual change of the lower form into a higher.

Lorenzo Oken (1776—1851) held a fixed scientific creed, and its articles determined his attitude to all the conceptions of his day. In his sea-slime theory and spontaneous generation views he harks back to the ideas of Anaximander. He bases his whole philosophy upon the spherical form of his metaphysical "All." The skull, for example, he held to be one of these manifestations of the archetypal sphere. The

* C. O. Whitman, *Bonnet's Theory of Evolution* (Woods Hall Biological Lectures, 1894), pp. 225-40; cf. also pp. 205-24 on evolution and epigenesis.

cell of course is also a sphere. His *Philosophy of Nature* appeared in 1802, the very year in which Lamarck and Treviranus independently outlined their theories of biology and evolution. In spite of the praises lavished by Haeckel on him, there is no comparison between their methods and their results with his methods and his results. In 1805 appeared his work upon Generation—*Die Zeugung*—containing his *Ur-Schleim* doctrine. "Every organic thing," we learn, "has arisen out of slime, and is nothing but slime in different forms. This primitive slime originated in the sea, from inorganic matter, in the course of planetary evolution. The origin of life occurred upon the shores, where water, air, and earth were joined." The *Ur-Schleim* took the form of minute bladders containing fluid. This infusorium, as he calls it, develops. The whole organic world, in fact, consists of infusoria, and both plants and animals are merely modified infusoria. Generation, in his view, is the synthesis of organic spheres. With Robinet, he holds that it is the synthesis of germs. With Maupertuis and Diderot, he holds that it is the synthesis of particles. His conceptions of the beginnings of life vary, nor is he afraid to be inconsistent. "All life," he declares, "is from the sea; the whole sea is alive. Love arose out of sea-foam." Such a conclusion might have stood in the days of Anaximander, but it could not stand at the beginning of the nineteenth century. He is even able to throw over his celebrated *Ur-Schleim* hypothesis when he lays down that "man has not been created, but developed, so the Bible itself teaches us. God did not make man out of nothing, but took an elemental body then existing—an earth-clod or carbon; moulded it into form, thus making use of water; and breathed into it life—namely, air—whereby galvanism or the vital process arose." This, by the way, will indicate the sort of teaching in which Helmholtz grew up.

The philosophers like Bacon, Leibniz, and Kant step off the scene, and so too do the quasi-philosophers like de Maillet, Maupertuis, Diderot, Bonnet, Robinet, and Oken. The philosophers from Bacon to Kant had been the forerunners of the naturalists of the rank of Linnæus and Buffon.

The change in attitude effected by the naturalists is apparent in the opening words of Charles Dickens's novel *Hard Times*. "Now, what I want," says Mr. Gradgrind, "is Facts.

Teach these boys and girls nothing but Facts. Facts alone are wanted in life. Plant nothing else, and root out everything else. You can only form the minds of reasoning animals upon Facts; nothing else will ever be of any service to them. This is the principle on which I bring up my own children, and this is the principle on which I bring up these children. Stick to Facts, sir!" The wish of Mr. Gradgrind was anticipated during the eighteenth century by some naturalists. Views there had been in abundance, but had not the time come to put facts before investigators?

Linnæus (1707—1778), it is not unfair to say, founded the school of facts, and Buffon and Cuvier adopted his attitude. His *magnum opus* on the *Systema Naturæ* contained masses of facts. He saw behind the facts he diligently collected the world of organic life as composed of so many well-demarcated types, each separate, distinct, and immutable, each capable of producing its life *ad infinitum,* and each unable to vary from its central standard in any of its individuals, except perhaps within very narrow and unimportant limits. To him every species was exactly intermediate between two others: "We reckon as many species as issued in pairs from the hands of the Creator." By defining a kind as a group of plants or animals so closely resembling one another as to give rise to the belief that they might all be descended from a single ancestor or pair of ancestors, he implicitly gave the sanction of his weighty authority to the Creation hypothesis, and to the doctrine of the unchangeability of organic forms. Such were his views before 1751. Observance of facts proved too much for this attitude. He dropped the idea of the absolute fixity of species, allowing for an increase of species when he remarked that "all the species of one genus constituted at first (that is, at the Creation) one species, ab initio unam constituerint speciem; they were subsequently multiplied by hybrid generation, that is, by intercrossing with other species." In the last edition of his *Systema Naturæ,* which appeared in 1766, the fundamental proposition of his early years, *nullæ speciæ novæ,* quietly disappears. He also suggests that degeneration was the result of the influence of climate or environment.

Like Linnæus, Georges Louis Leclerc Buffon (1707—

1788) collected facts with the utmost care, and, like Linnæus, believed at first in the absolute immutability of species, and, also like him, came to disbelieve in this immutability. The early edition of Buffon's *magnum opus, L'Histoire Naturelle,* contains words that might be a quotation from the early edition of Linnæus's *Systema Naturæ*: "In animals, species are separated by a gap which Nature cannot bridge. . . . We see him, the Creator, dictating his simple but beautiful laws and impressing upon each species its immutable characters." The nasty, inconvenient fact of the pig stood so early as 1755 in the way of the acceptance of the Special Creation theory. How could the pig have been formed upon an original, special, and perfect plan? Was it not a compound of other animals? Had it not parts like the toes which are of no service to it?

Anticipating Goethe, Buffon refused to perceive a purpose in every part. Six years later he came to hold a belief in the frequent mutability of species. Plants and animals may freely vary in every direction from a common centre, so that one kind may gradually and slowly be evolved by natural causes from the type of another. He points out that, underlying all external diversities of character and shape, fundamental likenesses of type occur in many animals, which irresistibly suggest the notion of common descent from a single ancestor. Thus regarded, he maintains, not only the ass and the horse but even man himself, the monkeys, the quadrupeds, and all vertebrate animals, might be viewed as merely forming divergent branches of one and the same great family tree. Every such family, he believed, whether animal or vegetable, might have sprung originally from a single stock, which after many generations had here developed into a higher form, and there degenerated into a lower and less perfect type of organisation. Granting this—granting that nature could by slow variation produce one species in the course of direct descent from another unlike it (for example, the ass from the horse)—then, Buffon observed, there was no further limit to be set to her powers in this respect. We might reasonably conclude that from a single primordial being she has gradually been able in the course of time to develop the whole continuous gamut of existing animal and vegetable life. It is the old Aristotelian notion worked out upon a wide basis of facts. The relations of species he leaves to the one side as a problem beyond our reach: "Nous ne pourrions nous prononcer plus affirmativement si les limites qui séparent les espèces, ou la chaîne qui les unit, nous étaient mieux connus; mais qui peut avoir suivi la grande filiation de toutes les généalogies dans la nature? Il faut être né avec elle et avoir, pour ainsi dire, des observations contemporaines."

To students of heredity it is a fact of surpassing fascination to find that Dr. Erasmus Darwin * (1731—1802), grandfather of the great naturalist, proves to be a poet of evolution, following Empedocles and Lucretius, and followed by Goethe. He wrote the *Botanic Garden* and *Loves of the Plants,* two volumes of verse published about 1788, his *Zoonomia,* published in 1794, and his *Temple of Nature,* published after his death in 1802, a memorable year in the annals of works published on evolution. Poor as his poetry is, it everywhere evinces the working of an original mind. Where Charles Darwin was to catch views, Erasmus Darwin only caught glimpses. These glimpses leant towards the ideas afterwards put forth by that outstanding scientist Lamarck. Darwin held that modifications spring from within by the reactions of the organism, and he also endowed plants with sensibility and, going further than Lamarck, attributed their evolution to their own efforts towards the attainment of certain structures.

In the opening verses of the *Temple of Nature* Erasmus Darwin revives the Greek doctrine of the spontaneous origin of life:

> Hence without parents, by spontaneous birth,
> Rise the first specks of animated earth.
>
> . . . . .
>
> Organic life beneath the shoreless waves
> Was born and nurs'd in ocean's pearly caves;
> First, forms minute, unseen by spheric glass,
> Move on the mud, or pierce the watery mass;
> These, as successive generations bloom,
> New powers acquire and larger limbs assume;
> Whence countless groups of vegetation spring,
> And breathing realms of fin and feet and wing.

In the transition from sea to dry land came the amphibious,

* Throughout this section I make much use of E. Krause's *Erasmus Darwin,* and Mr. H. F. Osborn's *From the Greeks to Darwin.*

and then the terrestrial forms of life. He notes the development of the tadpole into the frog that surprised Canning so much. He quotes Buffon to the effect that many features in the anatomy of man point to a former quadrupedal position. Man may have arisen from a single family of monkeys in which, accidentally, the opposing muscle brought the thumb against the tips of the fingers, and this muscle gradually increased in size by use in successive generations. That is, he discerns the part taken by the survival of an accidental variation. The hand of man indeed stirs him:

> The hand, first gift of Heaven! to man belongs;
> Untipt with claws, the circling fingers close,
> With rival points the bending thumbs oppose,
> Trace the nice lines of Form with sense refined,
> And clear ideas charm the thinking mind.

The development of the human faculties next receives attention. He describes the fierce struggle for existence in verses that remind us of Tennyson's lines upon nature red in tooth and claw. Animal destroys animal, and plant destroys plant. Plants engage in an endless contest among themselves for soil and air, for light and moisture. This bitter contest results in the checks administered to the naturally rapid increase of life. What Darwin put poetically, we may put by means of the more prosaic medium of figures. If we assume each plant to occupy a foot and if we assume the dry surface of the earth to be 51,000,000 square miles, this provides us with 1,421,798,400,000,000 square feet, room enough, we should think, for reasonable expansion on the part of plants. Take the process for nine years, and we find:

Plants					Plants
1 × 50	in	1st	year	=	50
50 × 50	,,	2nd	,,	=	2,500
2,500 × 50	,,	3rd	,,	=	125,000
125,000 × 50	,,	4th	,,	=	6,250,000
6,250,000 × 50	,,	5th	,,	=	312,500,000
312,500,000 × 50	,,	6th	,,	=	15,625,000,000
15,625,000,000 × 50	,,	7th	,,	=	781,250,000,000
781,250,000,000 × 50	,,	8th	,,	=	39,062,500,000,000
39,062,500,000,000 × 50	,,	9th	,,	=	1,953,125,000,000,000

That is to say, at the end of the ninth year there are 531,326,600,000,000 square feet less than the descendants of a single plant, unchecked, require. As Dr. Krause remarks, Darwin just misses the connection between this fierce struggle and the survival of the fittest.

From such predecessors as Aristotle and Leibniz, Buffon and Helvetius, Linnæus and Blumenthal, Erasmus Darwin draws, and draws heavily. Darwin confesses that "this idea of the gradual formation and improvement of the Animal world seems not to have been unknown to the ancient philosophers."

In his *Zoonomia* Darwin defends the idea of individual development by successive additions of parts to the embryo. Individual life begins from a single filament. "Shall we conjecture," he asks, "that one and the same kind of living filament is and has been the cause of all organic life? . . . I suppose this living filament, of whatever form it may be, whether sphere, cube, or cylinder, to be endowed with the capability of being excited into action by certain kinds of stimulus." This excitability gives rise to the changes in the structure of plants and animals, and these changes are transmitted to their descendants. Coming to the largest problem, he believes that "when we revolve in our minds the metamorphoses of animals, as from the tadpole to the frog; secondly, the changes produced by artificial cultivation, as in the breeds of horses, dogs, and sheep; thirdly, the changes produced by conditions of climate and of season, as in the sheep of warm climates being covered with hair instead of with wool, and the hares and partridges of northern climates becoming white in winter: when, further, we observe the changes in structure produced by habit, as seen especially in men of different occupations; or the changes produced by artificial mutilation and prenatal influences, as in the crossing of species and production of monsters; fourthly, when we observe the essential unity of plan in all warm-blooded animals, we are led to conclude that they have been alike produced from a similar living filament."

As he reviews some of the arguments for mutability, he speculates upon the causes of these changes. "From their first rudiments," he thinks, "or primordium, to the termination of their lives, all animals undergo perpetual transformations; which are in part produced by their own exertions in consequence of their desires and aversions, of their pleasures and their pains, or of irritation, or of associations; and many of these acquired forms or propensities are transmitted to their posterity." * Here is the theory of the inheritance

* *Zoonomia,* I, p. 506.

of acquired modifications enunciated, and enunciated for the first time. Nor is it a stray remark of Darwin's, for he repeatedly recurs to it.

Acquired characters are transmitted by animals. Discussing their wants, he arranges them from the point of view of sexual characters. Horns and spurs, he thinks, are developed for purposes of combat and for the procuring of females. Though he misses the idea of the sexual selection of the horns developed as ornaments to the male, he hits upon the idea of protective colouring. For we learn that "there are organs developed for protective purposes, diversifying both the form and colour of the body for concealment and for combat." He admits the limitations of his view of evolution when he writes that "the final cause of these colours is readily understood, as they serve some purpose of the animal, but the efficient cause would seem almost beyond conjecture."*

The geologists had not then begun to ask for those æons with which men like Lyell have made us so familiar. Darwin anticipates them when he holds: "From thus meditating upon the minute portion of time in which many of the above changes have been produced, would it be too bold to imagine, in the great length of time since the earth began to exist, perhaps millions of ages before the commencement of the history of mankind, that all warm-blooded animals have arisen from one living filament, which the first great Cause imbued with animality, with the power of acquiring new parts, attended with new propensities, directed by irritations, sensations, volitions, and associations, and thus possessing the faculty of continuing to improve by its own inherent activity, and of delivering down those improvements by generation (i.e. inheritance) to posterity, world without end?" This is a statement at least as far-reaching as that of Kant, based of course on a profounder knowledge of plants and animals.

All forms of life, Darwin holds stoutly, proceed from a single filament. Irritability, excitability, sensibility—these stimulate the growth of this filament. "The most essential parts of the system are first formed by the irritations (of hunger, thirst, etc., above mentioned) and by the pleasurable

* *Ibid.*, p. 510.

sensations attending those irritations, and by exertions in consequence of painful sensations, similar to those of hunger and suffocation. . . . In confirmation of these ideas, it may be observed that all parts of the body endeavour to grow or to make additional parts of themselves throughout our lives." * Carrying this idea of sensibility and irritability into plant life as well as into animal, he frames a theory of plant evolution similar to that of animal evolution. On the origin of plants he mentions the suggestion of Linnæus: "And that from thence, as Linnæus has conjectured in respect to the vegetable world, it is not impossible but the great variety of species of animals which now tenant the earth, may have had their origin from the mixture of a few natural orders." Be this as it may, the plants possess the sensibility and the irritability of animals. When he digested these views we can well understand Charles Darwin writing: "It is curious how largely my grandfather, Dr. Erasmus Darwin, anticipated the views and erroneous grounds of opinion of Lamarck in his *Zoonomia*."

The most outstanding name in the long history of the theory of evolution between the days of Aristotle and Charles Darwin is that of Jeanne Baptiste Pierre Antoine de Monet, otherwise known as the Chevalier de Lamarck (1744—1829). If ever there was a fair-minded man, it was Charles Darwin, yet he never refers to either Lamarck or his work except with disdain. His verdict on him is given in the following passage of a letter to Sir Charles Lyell, written in March 1863: "Lastly, you refer repeatedly to my view as a modification of Lamarck's doctrine of development and progression. If this is your deliberate opinion there is nothing to be said, but it does not seem so to me. Plato, Buffon, my grandfather, before Lamarck and others propounded the *obvious* view that if species were not created separately they must have descended from other species, and I can see nothing else in common between the *Origin* and Lamarck. I believe this way of putting the case is very injurious to its acceptance, as it implies necessary progression, and closely connects Wallace's and my views with what I consider, after two deliberate readings, as a wretched book, and one from which (I well remember to my surprise) I gained nothing."

* *Zoonomia,* XXXIX, 3.

"But," adds Darwin, with a little touch of banter, "I know you rank it higher, which is curious, as it did not in the least shake your belief * . . . to me it was an absolutely useless book." † There are not many instances where posterity has refused to confirm such a judgment of the man popularly supposed to be the father of evolutionary doctrine. In this instance the stepfatherly attitude is more apparent than the fatherly.

The career of Lamarck is one of the most singular in the whole history of science, for where, with the possible exception of Kant, shall we meet with a man whose leading ideas were given to the world after the age of fifty? He first turned his attention towards botany. He travelled in Europe as a companion to Buffon's son, but, unlike Darwin and Hooker and Huxley, he never travelled outside Europe, and this proved a lasting loss to biological science. Devoid of that rich experience of his three successors, there were gaps in his knowledge which no mere reading could atone for. Lyell regarded travel as the first, second, and third requisites for a geologist, and one of the gravest blows to biology lies in the fact that Lamarck never travelled save in Europe. This meant that in the days to come he committed mistake after mistake when he employed illustrations to confirm the truth of his leading illuminating conceptions. Men argued that because the illustrations were faulty, so too were the conceptions. It proved not difficult to ignore the man who explained the webbed feet of birds as due to their being attracted to swampy ground by hunger, to their then making efforts to swim, thus spreading their toes, and stretching the skin between them. It proved not difficult to ignore the man who explained the origin of the horns in ruminant animals by the efforts they have made to butt their heads together in their periods of anger, thus giving rise to a secretion of matter upon their forehead.‡ It proved not difficult to ignore

* F. Darwin, *Life and Letters of Charles Darwin,* III, p. 14.

† *Ibid.*, p. 16; cf. pp. 23, 29, 39. Packard says that Darwin attributes to Lamarck statements which so careful a student of Lamarck's writings as Packard cannot trace. Cf. Packard, *Lamarck*, p. 74. As well as Packard's important book on Lamarck, cf. also E. Périer, *La Philosophie Zoologique devant Darwin.*

‡ Cf. Cuvier, "Eloge de Lamarck," November 26, 1832, *Mém. de l'Acad. des Sciences,* XIII, p. 20. Lyell criticises Lamarck fairly in his *Principles of Geology,* II, bk. III, chaps. i-iv.

the man who explained the rapid movements of the deer by saying that the fleet types of ruminants have been exposed to the attacks of carnivorous animals, and therefore have been forced to fly. Take his account of the limbs of the snake: "The snakes sprang from reptiles with four extremities, but having taken up the habit of moving along the earth and concealing themselves among bushes, their bodies, owing to repeated efforts to elongate themselves and to pass out through narrow spaces, have acquired a considerable length out of all proportion to their width. Since long feet would have been very useless, and short feet would have been incapable of moving their bodies, there resulted a cessation of use of these parts, which has finally caused them totally to disappear, although they were originally part of the plan of organisation in these animals."

Men found it easy to note flaws in his illustrations, and they jumped to the conclusion that there were just as many flaws in his ideas. This of course was not the case, but the scientists of his time choose to imagine that it was. At the age of forty-nine the Directory transferred him from botany to the chair in zoology at the Jardin des Plantes. As Huxley turned from physiology to palæontology, so Lamarck changed from botany to zoology. At the same time Geoffroy Saint-Hilaire assumed charge of the vertebrates, he assumed charge of the invertebrates. Forty-nine is usually an age when a man's big work is done. With Lamarck, on the contrary, his big work lay before him. Poor as Diderot, he was as devoted to truth as the famous editor of the *Encyclopædia*. Too poverty-stricken to buy expensive instruments, his inferior instruments combined with his devotion to the small forms of life gradually deprived him of the use of his sight, and in 1819 he became totally blind. There is a touch of pathos in the introduction to the last edition of his *Animaux sans Vertèbres* that might have moved the heart of Darwin. Lamarck lacked appreciation abroad, for his books found but few readers, but he never lacked appreciation at home. His daughter was so devoted to him that when his grievous calamity overtook him, she never left him. The last two volumes of the first edition of his *Histoire Naturelle des Animaux sans Vertèbres,* begun in 1816 and finished in 1822, he dictated to his daughter. If there were poverty of goods in his home, there was no poverty of affection or spirit. If there was uncommonly plain living, there was uncommonly high thinking. The thoughts were met with a disdain that rivalled Charles Darwin's and with a neglect that forms a permanent disgrace to science.

His *Recherches sur les Causes des Principaux Faits Physiques* was written in 1766, presented to the Academy in 1780, and published in 1794, the year of the publication of the *Zoonomia*. In it he exhibits his strong belief in the immutability of species and his equally strong disbelief in the theory of the spontaneous origin of life. These were his botanical days, not his zoological ones. Just as it is possible to maintain that there were two Carlyles (the one who wrote before 1850, and the other after that date), so it is quite possible to maintain that there were two Lamarcks (the one who wrote before 1793, and the other after that date). The second Lamarck published in 1802 his *Hydrogéologie,* anticipating Lyell in suggesting uniformitarian ideas in geology, and proposing the term "biology" for the first time for the sciences of life. The year 1802 was surely a memorable one in biology, for during it Lamarck also put forth his *Recherches sur l'Organisation des Corps Vivants.* Employing the works of Aristotle, he lays down two main principles * : first, it is not organs which have given rise to habits, but habits, modes of life, and environment which have given rise to organs. This he illustrates by the blindness of the mole, by the presence of teeth in mammals and by the absence of teeth in birds. His second principle is that life is an order and condition of things in the parts of all bodies which possess it, which renders possible all the organic movements within. At the very time Lamarck was proclaiming these views, Erasmus Darwin was proclaiming similar ones. Why not? Just as Charles Darwin and Alfred Russel Wallace were simultaneously working at the theory of evolution, just as John Couch Adams and Leverrier simultaneously discovered the planet Neptune, so Lamarck and Erasmus Darwin were working out similar evolutionary conceptions which were nothing short of revolutionary.

* I use the convenient summary of Osborn, p. 160.

Lamarck's *magnum opus* was his *Philosophie Zoologique,* published in 1809, but it made little stir, for Cuvier opposed it with all the powers of his great genius. In it combined with his later *Histoire Naturelle* we possess his generalisations on the mutability and variability of species, on the influence of the environment on the habits, and through them and inheritance on the forms of living creatures. There is a complete break with the doctrine of the fixity of species and with the permanence and recurrence of types.* In a word, there had been a statical or morphological attitude towards problems, and it was now replaced by a kinetic or genetic one. He distinctly lays down the doctrine that man is descended from an ape-like ancestor, which gradually acquired an upright position, not even yet wholly natural to the human race. We may, if we like, lay stress on the factor of adaptation put prominently forward, on the idea of the dependence of living things on their milieu, on the view that in the graduated scale of living things there is an increasing independence with regard to the external environment, or on the modifying influences which Lamarck emphasised. The vital matter is what he expressed in the words: "All that Nature has caused individuals to acquire or lose by the influences of environment to which they have been long exposed, and consequently by the influence of the predominant employment of a certain organ, or by that of the continued lack of use of the same part—all this Nature conserves by generation to the new individuals which arise, provided that these acquired variations (*changements*) are common to both sexes, or to those which have produced these new individuals." This is the law of the inheritance of acquired characters, and was substantially enunciated by Erasmus Darwin. In his *Philosophie Zoologique* Lamarck put his doctrine concisely: "But great changes in environment bring about changes in the habits of animals. Changes in their wants necessarily bring about parallel changes in their habits. If new wants become constant or very lasting, they form new habits, the new habits involve the use of new parts, or a different use of old parts, which results finally in the pro-

* Cf. J. V. Carus, *Geschichte der Zoologie*, p. 723; K. E. von Baer, *Reden und wissenschaftliche Abhandlungen,* II, p. 258. Cf. Horner, *Life of Sir C. Lyell,* I, p. 168; II, p. 365.

duction of new organs and the modification of old ones."

He saw with the utmost clearness that lower types like the Molluscs had given way to the higher, but he failed to see that higher types like the Mastodon and the Paleotherium could also be extinguished. He demonstrated the persistency of these lower types. When his colleague, Geoffroy Saint-Hilaire, brought back his collection of mummied cats and other animals from the tombs of Egypt, it was at once evident that they were identical with the actual living representatives of the same species. This seemed rebutting evidence against his Transmission theory. His reply was that in Egypt, that land of surprises, there had been no substantial alteration in the environment, the soil and the climate remaining unchanged. Under these circumstances, animals naturally retained their old habits. Why should they change them? No cause was alleged for their doing so. Therefore the persistence of their characters was readily demonstrated. If he could only have explained away so satisfactorily his explanation of the shape of the snake!

Johann Wolfgang Goethe (1749—1832) was great as a scientist, though the world will persist in forgetting this aspect of his many-sided intellect. The recognition Lamarck never received in his lifetime would have been his had Goethe only happened to have glanced at the ignored *Philosophie Zoologique*. Goethe, to the permanent loss of science, abandoned Loder for Schiller, and Linnæus for Shakespeare. Yet he discovered the vertebrate theory of the skull and made his studies on the metamorphoses of the plants. In his *Metamorphoses of Plants* Goethe anticipated Lamarck as an evolutionist, and to the very last he always evinced the liveliest interest in scientific thought. In his *Metamorphoses of Animals*, published in 1819, he writes:

> All members develop themselves according to eternal laws,
> And the rarest form mysteriously preserves the primitive type.
> Form, therefore, determines the animal's way of life,
> And in turn the way of life powerfully reacts upon all form.
> Thus the orderly growth of form is seen to hold
> Whilst yielding to change from externally acting causes.

It is tragical to have to record that the poet died three years earlier than Lamarck, and yet had never heard of the blind zoologist!

CHAPTER VI

DARWIN AND EVOLUTION

CHARLES ROBERT DARWIN (1809—1882) was the fifth child and second son of Robert Waring Darwin and Susannah Wedgwood, and was born on February 12, 1809,* at Shrewsbury, where his father was a physician with a large practice. No doubt he inherited from his grandfather Erasmus the inborn tendencies to look at nature in the same observant way. Erasmus Darwin had defined a fool to his friend Edgeworth as "a man who never tried an experiment in his life." Curiously enough, though he tried them, he was wanting in that rigorous and patient inductive habit that was to mark his grandson Charles. Sir Francis Darwin records that R. W. Darwin had no pretensions to the character of a man of science, no tendency to generalise his knowledge, and "though a successful physician he was guided more by intuition and everyday observation than by a deep knowledge of his subject."† One of our facile generalisations is to remark, in this and sundry other like cases, that the mental energy skips a generation. People have said so in the case of the intermediate Mendelssohn who was the son of Moses Mendelssohn, the philosopher, and father of Felix Mendelssohn-Bartholdy, the composer—that mere link in a marvellous chain who was wont to observe of himself in the decline of life, that in his youth he was called the son of the great Mendelssohn, and in his old age the father of the great Mendelssohn. Is actual skipping possible in the nature of things? We gravely doubt it. In the particular instance of R. W. Darwin we may feel pretty confident that the distinctive Darwinian strain lay latent rather than dormant. Attaining in his time sufficient eminence to become a Fellow of the

* Abraham Lincoln was also born on February 12, 1809.
† F. Darwin, *Life of C. Darwin*, p. 1 (1892 ed.).

Royal Society, an honour rarely accorded to a country doctor, Charles Darwin records of him, " He was incomparably the most acute observer whom I ever knew." Nor is there any reason to think that this is simply a tribute paid by filial piety. For potentiality is wider than actuality: what a man does is no certain criterion of what he can do. When Charles Darwin records that his father " formed a theory for almost everything that occurred,"* we are not left in much doubt on the influence of heredity.

Among the brothers of R. W. Darwin were Charles, the eldest (1758—1778), who gave the highest promise, studied medicine at Edinburgh, received the medal of the Æsculapian Society, and died from a wound received in dissecting. Among the cousins of Charles Darwin are Hensleigh Wedgwood, the philologist; the late Sir Henry Holland; and Francis Galton, the author of that essentially Darwinian book, *Hereditary Genius*. Among the sons of Charles Darwin are Sir George Howard Darwin, Plumian Professor of Astronomy and Experimental Philosophy at the University of Cambridge, and brother to three other Darwins who have distinguished themselves in mathematics, engineering, botany, and geographical science. Though the bent of the Darwins was to natural science, the mind of Sir George, tinged by the mentality of his mother's family, the Wedgwoods, and more distantly by that of the Galtons, was mathematical. R. W. Darwin married Susannah Wedgwood, daughter of Josiah Wedgwood, the potter, who, by his marked originality and force of character, succeeded in turning the current of national taste towards wares of a higher type of artistic workmanship. His trials of method and materials were carried out in the exhaustive spirit of true scientific inquiry, and conduced to many improvements. He possessed a great power of adaptation, and an inventive faculty which revealed itself not only in new methods and new materials, but in the origination of new forms. Can we doubt that an ancestor who was a practical thrower,† an expert modeller, and an ingenious designer left his mark upon his grandson Charles?

In the summer of 1818 Charles Darwin entered as a

* F. Darwin, *Life and Letters of C. Darwin,* I, p. 20; cf. I, p. 103.
† In handicraft.

boarder at Shrewsbury school under Dr. Butler. The education given was on old-fashioned classical lines, and the lad gained little from it. In his *Autobiography* he tells us that he had much zeal for whatever interested him, and he worked pretty hard at Euclid and practical chemistry in an extemporised laboratory. His enthusiasm for chemical studies kept him late at work, earning for him the nickname of "Gas" from his schoolfellows, and also earning for him a public rebuke from the headmaster, "for this wasting my time on such useless subjects; and he called me very unjustly a *poco curante,* and as I did not understand what he meant, it seemed to me a fearful reproach." * He learnt many lines of Homer and Virgil off by heart, admiring greatly the odes of Horace. He spent hours over the historical plays of Shakespeare, Thomson's *Seasons,* and the recently published poems of Byron and Scott. The pleasure he derived from reading poetry conspired to arouse in him in 1822 a vivid delight in scenery. Early in his schooldays a boy lent him a copy of the *Wonders of the World,* which he often read, and disputed with other boys about the veracity of some of the statements; "and I believe that this book gave me a wish to travel in remote countries, which was ultimately fulfilled by the voyage in the *Beagle.*" †

At school he had made himself notable by his love of collecting—the first nascent symptom of the naturalistic bent. He collected everything: shells, eggs, minerals, coins, even, since postage stamps had not then been invented, franks. He has himself described the zeal with which, as a boy and a young man, he gave himself up to shooting, a passion which only gradually faded before his stronger delight in unravelling the geology of an unknown country. As it was intended that he should follow his father's profession of medicine, in 1825 he joined his brother Erasmus at Edinburgh University. With the one exception of Hope, the Professor of Chemistry, Darwin found them all "intolerably dull."' We learn that the Professor of Anatomy made his lectures "as dull as he was himself." In spite of his early interest, the prælections of the Professor of Geology and Zoology were so "incredibly dull" that they produced on their hearer the

* F. Darwin, *Life of C. Darwin,* p. 10 (1892 ed.).
† *Ibid.,* p. 11 (1892 ed.).

resolution never "to read a book on geology or in any way to study the science."* Jameson was a Wernerian geologist who spent his time sneering at the Huttonians. The outcome in the subject of anatomy was particularly unfortunate, for as Darwin never practised dissection he was continually handicapped in his future researches. Twice he attended the operating theatre, and the ensuing nausea in one of these cases was such that he felt obliged to rush away before the surgeon had completed his task. As there was no chloroform in those days, it is not hard to imagine that these two cases fairly haunted the young medical student for many a long year. He gave, however, distinct evidence of his tastes by contributing to the Plinian Society at the beginning of 1826 a paper on the floating eggs of the common sea-mat, in which he succeeded in discovering for the first time organs of locomotion. Like many an undergraduate, he experienced more pleasure and profit in meeting his fellow-undergraduates than in meeting the professors.

On the conclusion of two years at Edinburgh, medicine was abandoned as a profession, and Darwin contemplated taking Holy Orders. To his dismay he found that he had forgotten his classics, and had to learn almost everything, "even to some few of the Greek letters." † Working with a private tutor in Shrewsbury, he soon recovered his school standard, and was able to translate easy Greek books, such as Homer and the Greek Testament, with moderate facility. As he must possess a degree in arts to be ordained, he entered Christ's College, Cambridge, and came up in the Lent term of 1828. Christ's College claims that on its bead-roll it has the name of John Milton, the writer of the epic of the special creation theory, which another of its sons, Charles Darwin, destroyed. He read the Thirty-nine Articles and he studied Pearson's great book on *The Creed,* and the study of other books, as well as these, left him convinced that he could conscientiously present himself for ordination. He read intently Paley's *Moral Philosophy,* his *Evidences of Christianity,* and his *Natural Theology,* and the perusal of the last two gave him as much delight as did Euclid. ‡ Was it the case that

* F. Darwin, *Life and Letters of C. Darwin,* I, p. 14.
† *Ibid.,* p. 17.
‡ *Ibid.,* p. 47.

just as Paley proleptically accepted the doctrine of evolution, so Darwin was getting ready to accept it? It is rather remarkable that three of the men who most influenced his thought were all clergymen. For Paley, Malthus, the author of *Essay on the Principle of Population,* and Henslow, the Professor of Botany at Cambridge, were all three of them in Holy Orders. Darwin's Edinburgh lecturers were not clergymen, and they repelled him, whereas his Cambridge lecturers were clergymen, and they attracted him. Nor was this attraction due to the circumstance that he then thought of becoming ordained, for the Rev. Dr. Sedgwick and the Rev. J. S. Henslow were Professors of Geology and Botany respectively in the University of Cambridge. Darwin's natural taste for geology, chilled by his Edinburgh teachers, revived during an excursion with Professor Sedgwick, who insisted that "science consists in grouping facts so that general laws and conclusions may be drawn from them." * Both Sedgwick and Henslow used to take their pupils field excursions, on foot or in coaches, to distant places, and they lectured on fossils and plants to the utter content of their audience. Henslow was a singularly attractive character who introduced into his parishes the voluntary study of botany with signal success, and he also introduced cricket and athletic clubs, allotments and parish excursions, benefit clubs and horticultural shows in the fifties, when such institutions were very uncommon. Nor was his enthusiasm confined to his successive parishes. In Cambridge he proved that he could communicate to his men the zeal and the knowledge that eminently characterised him. Darwin, his favourite pupil, always manifested the deepest regard for him, calling him on his death in 1861 his "dear old master in Natural History." †

While at Edinburgh, Darwin had taken a vow to cease to think of geology. It was Henslow who induced him to break it. Through Henslow the undergraduate obtained permission to accompany Sedgwick on one of his excursions in Wales. Above all, Henslow recommended him to buy and to study the then recently published volume of Lyell's *Principles of Geology.* With this recommendation there

* F. Darwin, *Letters of C. Darwin,* p. 24.
† F. Darwin, *Life and Letters of C. Darwin,* II, p. 217.

went also the admonition not to allow himself to be swept off his feet by the fascination of Lyell's views. This warning was unheeded, for Darwin writes: "After my return to England it appeared to me that by following the example of Lyell in Geology, and by collecting all the facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown upon the whole subject [of the origin of species]." * It was through Henslow, and at his suggestion, that Darwin was offered the appointment to the *Beagle* as naturalist. What Helmholtz experienced when he came into contact with Johannes Müller, Darwin, in his own fashion, felt when he came to know the character as well as the mind of John Stevens Henslow.

Darwin's passion for collecting renewed itself in Cambridge. In childhood it had been damped by the moral scruples of a sister, as to the propriety of catching and killing insects for the mere sake of possessing them. The neighbouring fens afforded him the opportunity of capturing beetles. It was, he confesses, the passion of collecting for its own sake, for he did not dissect them, and rarely compared their external characters with published descriptions. "I will give a proof of my zeal: one day, on tearing off some old bark, I saw two rare beetles, and seized one in each hand; then I saw a third and new kind, which I could not bear to lose, so that I popped the one which I held in my right hand into my mouth. Alas! it ejected some intensely acrid fluid, which burnt my tongue so that I was forced to spit the beetle out, which was lost, as was the third one." † His delight lay in the capture of a species which turned out to be rare or new, for then he could read in print the magic words, "Captured by C. Darwin, Esq." Obviously this was his old love of sport simply assuming a new form, and his father believed that "he cared for nothing but shooting, dogs, and rat-catching." ‡

If men like Sedgwick and Henslow influenced him, so too did the books of men like Paley, Humboldt, and Sir J.

* F. Darwin, *Life and Letters of C. Darwin*, I, p. 83. Cf. Darwin's dedication of the second edition of the *Journal of a Naturalist*.

† F. Darwin, *Life of C. Darwin*, p. 20 (1892 ed.).

‡ F. Darwin, *Life and Letters of C. Darwin*, I, p. 32.

Herschel. Humboldt's *Personal Narrative* and Herschel's *Introduction to the Study of Natural Philosophy* stirred the undergraduate reader to think that he too might make a modest contribution to the noble structure of Natural History. The attractions of science increasingly prevailed over the labours of the ministry. "My whole course of life," says Darwin in sending a message to Humboldt, "is due to having read and re-read, as a youth, his personal narrative." * The description of Teneriffe moved him so strongly that he was seized with a lively desire to visit the island, inquiring about ships and the like.

While Darwin was turning over in his mind his project of a trip to Teneriffe, the Government decided to send a brig of 235 tons, the *Beagle,* under command of Captain FitzRoy, to complete the unfinished survey of Patagonia and Tierra del Fuego, to map out the shores of Chili and Peru, to visit several of the Pacific archipelagoes, and to carry a chain of chronometrical measurements round the whole world. This was an essentially scientific expedition, and Captain FitzRoy, afterwards famous as the meteorological admiral, was an officer of the finest type. He was anxious to be accompanied on his cruise by a competent naturalist who would undertake the collection and preservation of the animals and plants discovered on the voyage, for which purpose he generously offered to give up a share of his own cabin accommodation. Professor Henslow seized upon this opportunity of recommending young Darwin, "grandson of the poet." In his letter of August 24, 1831, he writes: "I have stated that I consider you to be the best qualified person I know 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." † Darwin gladly volunteered his services without salary, and partly paid his own expenses on condition of being permitted to retain in his own possession the plants and animals he collected on his journey. The *Beagle* set sail from Devonport

* F. Darwin, *Life and Letters of C. Darwin,* I, p. 336.
† *Ibid.,* I, p. 193.

on December 27, 1831, and she returned to Falmouth on October 2, 1836. The opportunity that had been denied to Lamarck was now in the fullest measure to be Darwin's, and right nobly he used it.

Aristotle and Voltaire were never tired of dwelling on the small springs on which the greater events of history turn. Pascal continued this train of thought.* Does not Burke† inform us of the case of "a common soldier, a child, and a girl at the door of an inn" who "changed the face of fortune and almost of nature"? ‡ We are, however, getting tired of hearing that another sort of tilt to Cleopatra's nose, and the history of the world might have flown in a different channel. Soberly, nineteenth-century thought might have undergone a similar transformation, for Captain FitzRoy was on the point of rejecting Darwin on account of the shape of his nose. "The voyage of the *Beagle*," confesses Darwin, "has been by far the most important event in my life, and has determined my whole career; yet it depended on so small a circumstance as my uncle offering to drive me thirty miles to Shrewsbury, which few uncles would have done, and on such a trifle as the shape of my nose." §

The last year of a man reading for high honours is by far the most important, mentally speaking, in his whole academical career. Up to that time for the most part he takes the opinions offered to him by his tutors as a matter of course. During his last year he weighs opinions for the first time with critical care. The gulf separating a man just matriculated or even a man in his third year from a man in his last year is scarcely to be bridged by any mental effort. This experience did not come to Darwin, for he only took a pass degree. To the able man, however, such an experience comes sooner or later, and accordingly it came to Darwin on board the *Beagle*. "I have always felt," he owns, "that I owe to the voyage the first real training or education of my mind; I was led to attend closely to several branches of natural history, and thus my powers of observation were improved, though they were al-

* Pascal, *Pensées*, 1829 ed., p. 137.

† In the *Regicide Peace*.

‡ Cf. my *Erasmus and Luther: their Attitude to Toleration*, pp. 67-8.

§ F. Darwin, *Life of C. Darwin*, p. 27 (1892 ed.). Cf. N. Pirogoff, *Klinische Chirurgie*, p. 32.

ways fairly developed." * Nor is the parallel with college life without instruction when we note that he spent five years on the voyage. These were his Wanderjahre, the years on which Goethe lays such stress, when he investigated at close quarters the teeming life of the tropics.

It is significant that Darwin, Hooker, and Huxley began their scientific career by long voyages or travels with the Navy, Wallace and Bates in the South American tropics. With most of them the way to science proved long and difficult. But this was not all loss; strength grows in a man who

grasps the skirt of happy chance
And breasts the blows of circumstance
And grapples with his evil star.

The particular countries visited by the *Beagle* during the course of her long and varied cruise, as Mr. Grant Allen ably shows, happened to be exactly such as were naturally best adapted for bringing out the latent potentialities of Darwin's mind, and suggesting to his active and receptive brain those deep problems of life and its environment which he afterwards wrought out with such subtle skill and such consummate patience in the *Origin of Species* and the *Descent of Man.* The Cape Verde and the other Atlantic islands, with their scanty population of plants and animals, composed for the most part of waifs and strays drifted to their barren rocks by ocean currents, or blown out helplessly to sea by heavy winds; Brazil, with its marvellous contrasting wealth of tropical luxuriance and self-strangling fertility, a new province of interminable delights to the soul of the enthusiastic young collector; the South American pampas, with their colossal remains of extinct animals, huge geological precursors of the stunted modern sloths and armadillos that still inhabit the self-same plains; Tierra del Fuego, with its almost Arctic climate, and its glimpses into the secrets of the most degraded savage types; the vast range of the Andes and the Cordilleras, with their volcanic energy and their closely crowded horizontal belts of climatic life; the South Sea Islands, those paradises of the Pacific, Hesperian fables come true, alike for the lover of the picturesque and the biological student; Australia, that surviving fragment of an extinct world, with an antiquated fauna whose archaic

* F. Darwin, *Life of C. Darwin*, p. 27 (1892 ed.).

character still closely recalls the European life of ten million years back in the secondary epoch: all these and many others equally novel and equally instructive passed in long alternating panorama before Darwin's eyes, and left their images deeply photographed for ever after on the lasting tablets of his retentive memory. That was the real great university in which he studied nature and read for his degree, which assuredly this time was no pass degree. Our evolutionist was undergoing a thorough process of education.

At all the places he touched he investigated geological phenomena most carefully, for here he thought that reasoning comes into play. On first examining a new district, he tells us, nothing can appear more hopeless than the chaos of rocks; but by recording the stratification and nature of the rocks and fossils at many points, always reasoning and predicting what will be found elsewhere, light soon begins to dawn on the district, and the structure of the whole becomes more or less intelligible. The underlying causes plainly interested him, and here he had to make his choice between the catastrophic system of the Wernerians, of which his old Edinburgh professor, Jameson, was a leader, and the system of the Huttonians, of which Sir Charles Lyell was a master. His *Principles of Geology* he perused with the utmost attention, and, in spite of the cautious attitude advocated towards it by another old professor, Henslow, Darwin became convinced the moment he examined the very first place on the voyage, St. Jago, in the Cape Verde islands, of the amazing superiority of the Lyellian methods. Might he not apply the principles of the great Scotsman to the geology of the countries he was about to visit? Such a thought gave him a thrill of delight.* Even before the *Beagle* touched her first land he had observed that the impalpably fine dust which fell on deck contained no fewer than sixty-seven distinct organic forms, two of them belonging to species peculiar to South America. In some of the dust he found particles of stone so big that they measured "above the thousandth of an inch square"; and after this fact "one need not be surprised at the diffusion of the far lighter and smaller sporules of cryptogamic plants." May we not trace in these observations the hereditary tendencies of Josiah Wedgwood, whose minute

* F. Darwin, *Life of C. Darwin*, p. 29 (1892 ed.).

investigation and accuracy of detail were reflected in his pottery? May we not also trace in them the influence of his incomparable teacher, Henslow?

Henslow had candidly stated that though he considered his favourite pupil amply qualified for the task of collection, yet he did not suppose he was by any means a finished naturalist, and indeed some of Darwin's labours on the voyage sufficiently attest this. The young naturalist collected copious details upon the surface fauna. But as he had little knowledge of drawing, less knowledge of comparative anatomy, and least knowledge of dissection, it is not a matter of wonder that he accumulated much manuscript that was useless. True, he acquired acquaintance with the marine Crustacea, and made observations on Planariæ and on the ubiquitous Sagitta—and that was the tale of all he had accomplished. Thanks to the training of Henslow and to Lyell's book, the case was altogether different with his geology. The work begun at St. Jago was continued throughout a voyage that marked an epoch in his mental growth. His subsequent study of the tertiary deposits and of the terraced gravel beds of South America turned his thoughts increasingly in a geological direction. His letters from South America contain mostly geological references, and even then he had begun to think of his theory of the formation of coral reefs as due to the extensive and gradual changes revealed by the geology of South America. "No other work," he holds, "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." * On May 18, 1882, he wrote to Henslow: "One great source of perplexity to me is an utter ignorance whether I note the right facts, and whether they are of sufficient importance to interest others.

"Geology carries the day: it is like the pleasure of gambling. Speculating, on first arriving, what the rocks may be, I often mentally cry out 3 to 1 tertiary against primitive; but the latter have hitherto won all the bets." †

* F. Darwin, *Life and Letters of C. Darwin*, I, p. 70.
† F. Darwin, *Life of C. Darwin*, p. 134 (1892 ed.).

To an old Cambridge friend, the Rev. William Darwin Fox, he wrote in July 1835, when about to start from Lima to the Galapagos: "I am glad to hear you have some thoughts of beginning Geology. I hope you will; 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 known in his admirable book. Geologising in South America, I am tempted to carry parts to a greater extent even than he does. Geology is a capital science to begin, as it requires nothing but a little reading, thinking, and hammering. 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." *

Henslow thought so much of Darwin's letters and observations that he had them printed and circulated them privately, and had read some of them before the Philosophical Society of Cambridge. His collection of fossil bones, which he had sent to Henslow, had also excited considerable attention among palæontologists. Evidently Henslow wrote him an encouraging letter. For "after reading this letter, I clambered over the mountains of Ascension with a bounding step and made the volcanic rocks resound under my geological hammer. All this shows how ambitious I was; but I think that I can say with truth that in after-years, though I cared in the highest degree for the approbation of such men as Lyell and Hooker, who were my friends, I did not care much about the general public. I do not mean to say that a favourable review or a large sale of my books did not please me greatly, but the pleasure was a fleeting one, and I am sure that I have never turned one inch out of my course to gain them."†

The hearty praise of men in the position of Sedgwick and Henslow meant everything to the young traveller. While at Ascension he received a letter in which his sisters told him that Sedgwick had called upon his father, saying that "I should take a place among the leading scientific men." ‡ Nor was this merely praise to warm the parental heart. For

* F. Darwin, *Letters of C. Darwin*, p. 135 (1892 ed.).
† *Ibid.*, p. 30. ‡ *Ibid.*, p. 30.

Sedgwick wrote on November 7, 1835, to Dr. Butler, the headmaster of Shrewsbury who had called Darwin a *poco curante*, that the lad he had censured "is doing admirable work in South America, and has already sent home a collection above price. It was the best thing in the world that he went out on the voyage of discovery. There was some risk of his turning out an idle man, but his character will now be fixed, and if God spares his life he will have a great name among the naturalists of Europe." * We have heard so much of clerical opposition to Darwin ideas that it is refreshing, on investigating the facts, to ascertain that such devoted clergymen as Henslow and Sedgwick, with all the weight of their academic positions, did all they could to afford encouragement to Darwin while he was winning—not when he had won—his spurs. To his sister Susan he wrote in August 1836: "Both your letters were full of good news; especially the expressions which you tell me Professor Sedgwick used about my collections. I confess they are deeply gratifying. I trust one part at least will turn out to be true, and that I shall act as I now think—as a man who dares to waste one hour of time has not discovered the value of life. Professor Sedgwick mentioning my name at all gives me hopes that he will assist me with his advice, of which, in my geological questions, I stand much in need.†

On his return home he wrote to Henslow on October 6, 1836: "I am sure you will congratulate me on the delight of once again being home. The *Beagle* arrived at Falmouth on Sunday evening, and I reached Shrewsbury yesterday morning. I am exceedingly anxious to see you, and as it will be necessary in four or five days to return to London to get my goods and chattels out of the *Beagle*, it appears to me my best plan to pass through Cambridge. I want your advice on many points; indeed I am in the clouds, and neither know what to do or where to go. My chief puzzle is about the geological specimens—who will have the charity to help me in describing their mineralogical nature? Will you be kind enough to write me one line by *return of post*, saying whether you are now at Cambridge?" ‡ We give this letter

* F. Darwin, *Letters of C. Darwin*, p. 137 (1892 ed.).
† *Ibid.*, p. 137.
‡ *Ibid.*, p. 139.

in anticipation, for we desire to lay stress on the fact that though he was writing to a botanist of the standing of Henslow, yet his inquiries are not about botany but about geology. Indeed in 1836 he speaks of being "much more inclined for geology than the other branches of Natural History." * So little did Darwin, then in his twenty-seventh year, realise the true bent of his genius!

A passage in his *Autobiography* is of such supreme importance in indicating the doubt whether zoological studies might not have been more profitable that we transcribe it:

"During the voyage of the *Beagle* I had been deeply impressed by discovering in the Pampean formation great fossil animals covered with armour like that on the existing armadillos; secondly, by the manner in which closely allied animals replace one another in proceeding southwards over the Continent; and thirdly, by the South American character of most of the productions of the Galapagos Archipelago, and more especially by 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.

"It was evident that such facts as these, as well as many others, could only be explained on the supposition that species gradually become modified; and the subject haunted me. But it was equally evident that neither the action of the surrounding conditions, † nor the will of the organisms ‡ (especially in the case of plants) could account for the innumerable cases in which organisms of every kind are beautifully adapted to their habits of life—for instance, a woodpecker or a tree-frog to climb trees, or a seed for dispersal by hooks or plumes. I had always been much struck by such adaptations, and until these could be explained it seemed to me almost useless to endeavour to prove by indirect evidence that species have been modified.

"After my return to England it appeared to me 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. My first note-book

* F. Darwin, *Life and Letters of C. Darwin,* I, p. 275.
† This refers to Buffon's factor.
‡ This refers to Lamarck's factor, and misconceives it.

was opened in July 1837. 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. 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 October 1838, that is, fifteen months after I had begun my systematic inquiry, I happened to read for amusement Malthus on *Population,* and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, 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 a new species. Here, then, 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." *

We have travelled on with the scientist to the stage when he is able to perceive a view about the facts he is industriously collecting. Now we return to the collector as he was on his voyage. At St. Paul's Rocks, a mass of volcanic peaks rising abruptly from the midst of the Atlantic, he notes that feather- and dirt-feeding and parasitic insects or spiders are the first inhabitants to take up their residence on recently formed oceanic islands. Was there here not light on the problem on how new lands were peopled? As one problem leads on to another, next came the question, How were some of the singular species he met evolved? How, in fact, was a new species evolved?

* F. Darwin, *Life of C. Darwin,* pp. 39-40 (1892 ed.).

On the last day of February 1832 the *Beagle* came to anchor in the harbour of Bahia, and he caught sight for the first time of the self-strangling luxuriance of tropical vegetation. "Delight itself," he writes in his *Journal,* "delight itself is a weak term to express the feelings of a naturalist who for the first time has wandered by himself in a Brazilian forest. The elegance of the grasses, the novelty of the parasitical plants, the beauty of the flowers, the glossy green of the foliage, but above all the general luxuriance of the vegetation, filled me with admiration." Nor was this admiration unmingled with penetration. As yet he had had no distinct views of the forces at work, but had he not had glimpses? Was he not vaguely feeling that there was some principle of selection behind them? As he began, in however faint a measure, to grasp the scheme of nature he experienced that keenest of pleasures, the pleasure felt by the man of far-reaching brain who can perceive all minor details fall at once into their proper place, as component elements in one consistent and harmonious whole—a sympathetic pleasure akin to that with which a musician listens to the linked harmonies of the *Messiah* or the *Creation,* or an architect views the soul-satisfying interiors of those glories of eastern England, Ely and Lincoln Cathedrals.

At Monte Video kindred problems aroused his active interest. Here he was in the moist plain-land of Uruguay, and there was barely a tree. On the other hand, Australia, which had a far drier climate, possessed quantities of gum-trees. What part did climate play in this distribution? What part did geography play in distribution? Clearly when he was asking questions like these, he was attaining to more than a glimpse of the solution of such problems. In Uruguay he met the tucutuco, a true rodent with the habits of a mole. Here was another problem. "Considering the strictly subterranean habits of the tucutuco," he writes, "the blindness, though so common, cannot be a very serious evil; yet it appears strange that any animal should possess an organ frequently subject to be injured. Lamarck would have been delighted with this fact, had he known it, when speculating (probably with more truth than usual with him) on the gradually acquired blindness of the Aspalax, a gnawer living under the ground, and of the Proteus, a reptile living in dark caverns filled with water; in both of which animals the eye is in an almost rudimentary state, and is covered by a tendinous membrane and skin. In the common mole the eye is extraordinarily small but perfect, though many anatomists doubt whether it is connected with the true optic nerve; its vision must certainly be imperfect, though probably useful to the animal when it leaves its burrow. In the tucutuco, which I believe never comes to the surface of the ground, the eye is rather larger, but often rendered blind and useless, though without apparently causing any inconvenience to the animal: no doubt Lamarck would have said that the tucutuco is now passing into the state of the Aspalax and Proteus." There is a zeal manifested here, but not a zeal manifested on any true knowledge—or, rather, understanding—of Lamarck's ideas.

For the two years after her arrival at Monte Video, the *Beagle* was employed in surveying the eastern coasts of South America, and here there was ample scope for the investigating mind. He noted *inter alia* the absence of recent geological formations along the lately upheaved coast of South America; the strange extinction of the horse in La Plata; the affinities of the extinct and recent species; the effect of minute individual peculiarities in preserving life under special circumstances; the influence of insects and blood-sucking bats in determining the existence of the larger naturalised mammals in parts of Brazil and the Argentine Republic, the curious relationship between the gigantic fossil armour-plated animals and the existing armadillo and between the huge megatherium and the modern sloth, and the curious instincts of the cuckoo-like molothrus, of the owl of the Pampas, and of the American ostrich.

Though much of the keenness of Darwin ran to geology, still, it is evident that biology was never long out of his ken.* If he observes a certain singular group of South American birds, we hear that "this small family is one of those which, though from its varied relations to other families, although at present offering only difficulties to the systematic naturalist, ultimately may assist in revealing the grand scheme, common to the present and past ages, on which organised beings have been created." In 1670 Wood had found the agouti abun-

* His last book on *The Earthworm* was in a sense geological.

dant as far south as Port St. Julian, though Darwin could not find it there in his time. Here is material for another question, "What cause can have altered, in a wide, uninhabited, and rarely visited country, the range of an animal like this?" He felt the force of the analogies between the fossil armour-plated animals and the armadillo, between the megatherium and the sloth, between the colossal ant-eaters and their degenerate descendants, between the extinct camel-like macrauchenia and the modern guanco, as well as those between the fossil and the living species of South American rodents. This moved him to write: "This wonderful relationship in the same continent between the dead and the living will, I do not doubt, hereafter throw more light on the appearance of organic beings on our earth, and their disappearance from it, than any other class of facts."

The *Journal of the "Beagle"* shows up many specimens as well as geological ones. Of course we must bear in mind that on his return home, he rewrote it after he perused for the first time the *Essay on the Principle of Population* which the Rev. Thomas Robert Malthus (1766—1834) had written in 1798. For almost fifty years this epoch-making essay had been in the hands of the public before one of its most serious implicit prepositions had been revealed to the minds of both Darwin and Alfred Russel Wallace, for it is an astonishing fact that Malthus's book set both naturalists on the track of the principle of natural selection. Under the account of events observed in the year 1834 Darwin records: "We do not steadily bear in mind how profoundly ignorant we are of the conditions of existence of every animal; nor do we always remember that some check is constantly preventing the too rapid increase of every organised being left in a state of nature. The supply of food, on an average, remains constant; yet the tendency of every animal to increase by propagation is geometrical, and its surprising effects have nowhere been more astonishingly shown than in the case of the European animals run wild during the last few centuries in America. Every animal in a state of nature regularly breeds; yet in a species long established any great increase in numbers is obviously impossible, and must be checked by some means."

There are coincidences in the reading of great men, but is there anything in its long arm to equal the parallelisms of Darwin and Wallace? It is not altogether surprising that both should have read Chambers's *Vestiges of the Natural History of Creation* (1844) or Lyell's *Principles of Geology* (1830-33). Nor is it extremely surprising that they both should have read Humboldt's *Personal Narrative* (1814-18). But surely it is most amazing that both should have read Malthus's *Essay on the Principle of Population,* a book published so far back as the year 1798. How out of the common it is appears from the following quotations:

DARWIN

"In October 1838, that is, fifteen months after I had begun my systematic inquiry, I happened to read for my amusement, *Malthus on Population,* and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observations of the habits of animals and plants, 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; 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

WALLACE

"In February 1858 I was suffering from a rather severe attack of intermittent fever at Ternate, in the Moluccas; and one day, while lying on my bed during the cold fit, wrapped in blankets, though the thermometer was at 88° Fahr., the problem presented itself to me, and something led me to think of 'the positive checks' described by Malthus in his *Essay on Population,* a work I had read several years before, and which had made a deep and permanent impression on my mind. These checks—war, disease, famine, and the like—must, it occurred to me, act on animals as well as man. Then I thought of the enormously rapid multiplication of animals, causing these checks to be much more effective in them than in the case of man; and while pondering vaguely on this fact of my theory in pencil, in thirty-five pages, and this was enlarged during the summer of 1844 into one of 230 pages." *

there suddenly flashed upon me the *idea* of the survival of the fittest—that the individuals removed by these checks must be on the whole inferior to those that survived. In the two hours that elapsed before my ague fit was over, I had thought out almost the whole of the theory; and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin." †

As the Rev. Thomas Malthus is at least the grandfather of the evolution theory, it is well worth while to see what he actually taught.‡ He left a book which everyone abuses and nobody reads, a book that attempted to prick the bubble of an earthly paradise set before thoughtless folks by Thomas Godwin in his *Political Justice*. Godwin imagined a society where all were equally comfortable and equal in fact all round. Assuming that it could be established—and Malthus plainly deemed this an unwarranted assumption—it would inevitably crash through the growth of population. Malthus's proof is short and sharp. Population, when unchecked, increases in geometrical ratio, whereas subsistence increases only in arithmetical.§ Malthus points out, with remorseless logic, that "the race of plants and animals shrinks under this great restrictive law, and the race of man cannot by any efforts of reason escape from it. Among plants and animals its effects are waste of seed, sickness, and premature death, among men misery and vice." In old countries like Europe population is constantly checked by want of room and want of food, by

* F. Darwin, *Life of C. Darwin*, p. 40 (1892 ed.).
† A. R. Wallace, *My Life*, I, p. 232.
‡ E. Haeckel, *History of Creation*, chap. vi. Haeckel dwells at length on the connection of Darwin with Malthus. Cf. *The Journal of the Linnean Society*, III, p. 51.
T. R. Malthus, *Essay*, p. 14 (1st ed.).

vice and misery, and by the fear of vice and misery. In new countries like America there is room and there is food, but the price of the latter is toil, and the toil of the women, for example, will interfere with the rearing of the children. In old Europe people double their numbers once a century. In new America they perform the same feat, despite toil and the difficulty of rearing children, once in twenty-five years. Obviously in the happy society of Godwin,

> Where all are proper and well-behaved,
> And all are free from sorrow and pain,

the rate of increase will be infinitely faster than doubling the population once in twenty-five years.* Godwin and the whole French school, like the Socialists of our own day, are entirely wrong in attributing all inequality to human institutions. The passion of man and woman to reproduce themselves is the root cause of the whole difficulty,† and always will be—until, at least, we all become as wise as the eugenists want us to be. "Where goods are increased, they are increased that eat them." The "struggle for existence"—Malthus uses this very phrase—is a present fact, as it has been a past fact, and will be a future one. Nothing is gained by rhetorical references to the wideness of the world and the possibilities of the ages. People increase in numbers up to the limit of food, and a "great restrictive law" prevents them, as it prevents all other animals, from multiplying beyond that limit.‡

In a herd of animals the units are simply the fittest who have survived in the struggle for existence. The principle of population is in the foreground there; there is no check to it but famine, disease, and death, the very checks on which Malthus was the first to lay emphasis. We can therefore understand how the study of the *Essay on Population* led Charles Darwin and Alfred Russel Wallace to explain the origin of species by a generalisation which Malthus had known and named, though he did not pursue it beyond man. So much indeed is Sir Charles Lyell impressed by these considerations that he even denied the originality of Darwin

* T. R. Malthus, *Essay*, pp. 20, 173, etc. (7th ed.).
† *Ibid.*, pp. 17, 47-8 (1st ed.).
‡ *Ibid.*, pp. 15, 16 (1st ed.).

and Wallace.* Darwinism is Malthusianism on the largest scale: it is the application of the problem of population, animal and vegetable.

In the autumn of 1835 the *Beagle* made her way to the small and unimportant Galapagos Archipelago. Small and unimportant as these little equatorial islands are from the geographical and commercial point of view, they are anything but this from the biological. They form a group of tiny volcanic islets, never joined to any continent and never joined to one another, yet each of them possesses its own special zoological features. They contain no frogs and no mammal save mice brought to them likely by a passing ship. The only insects are beetles, which possess peculiar facilities for transportation in the egg or grub across salt water upon floating logs. There are a few snails and two kinds of snake, one tortoise, and four lizards. There is a genus of a gigantic and ugly lizard, the amblyrhyncus, unknown elsewhere, but here assuming the forms of two species, the one marine and the other terrestrial. On the other hand there are no less than at least fifty-five distinct species of native birds. Besides, the differences of fauna and flora between the various islands force one to think that each form must necessarily have been developed not merely for the group, but for the special island which it actually inhabits. Darwin's brain reeled as he contemplated the amount of creative force employed, and it also reeled as he thought of "its diverse, yet analogous, action on points so near to each other." The fauna and flora of the islands are different from one another, and they are also different from that of Ecuador, the nearest mainland. The law cares nothing for *de minimis*: Darwin cared everything for *de minimis*.

Darwin perceives that once again he is met by another question set by Nature. What is the key to the riddle of organic existence? It seems almost in his hand. He writes that "most of the organic productions are aboriginal creations, found nowhere else; there is even a difference between the inhabitants of the different islands: yet all show a marked relationship with those of America, though separated from that continent by an open space of ocean, between 500 and

* *Origin of Species,* ch. iii, p. 50. Cf. Sir C. Lyell, *Antiquity of Man,* ch. xxi, p. 456.

600 miles in width. . . . Considering the small size of these islands, we feel the more astonished at the number of their aboriginal beings, and at their confined range. Seeing every height crowned with its crater, and the boundaries of most of the lava-streams still distinct, we are led to believe that within a period geologically recent the unbroken sea was here spread out. Hence, both in space and time we seem to be brought somewhat nearer to that great fact—that mystery of mysteries—the first appearance of new beings on this earth."

In New Zealand he met with fauna and flora of the most amazing meagreness and poverty of species. In the woods he noted very few big birds, and he remarks with astonishment that so large an island—the same size as Great Britain—should not possess a single living indigenous mammal, save a solitary rat of doubtful origin. It is the most insular extensive mass of land in the whole world, constituting a wonderful contrast with the Galapagos Archipelago.

For nearly five years the young geologist had been travelling. Occasionally we meet with the young biologist, but he came home a geologist. As Lamarck altered the course of his studies, so Darwin altered the course of his. It may very well have been that Lyell had laid down principles that would go towards the solution of many geological problems. Where, on the other hand, were the principles of the biologists? Collection and observation had given him one-half the subject-matter of the *Origin of Species*. It was reserved for reflection and Malthus to give him the missing half. As naturalist to the *Beagle* he had been obliged to consider all sorts of problems as well as the geological ones that absorbed so much of his attention. Oceanic phenomena, the formation of coral islands and of icebergs, the transport of boulders, volcanic phenomena, the height of the snow-line, the climate of the Antarctic islands, the effects of slavery, the appearance of the Patagonian and other races—a thousand and one problems like these compelled the geologist to consider many matters outside the range of the narrow specialist. For the time being, at any rate, Darwin acted on the Baconian maxim of taking the whole world of science for his special problem. As he appreciated the labours of Sedgwick and Henslow on his behalf, so he appreciated those of Captain

FitzRoy, to whom he wrote after his return and settlement in London: "However others may look back to the *Beagle's* voyage, now that the small disagreeable parts are well-nigh forgotten, I think it far the *most fortunate circumstance in my life* that the chance afforded by your offer of taking a naturalist fell on me. I often have the most vivid and delightful pictures of what I saw on board the *Beagle* pass before my eyes. These recollections, and what I learnt on Natural History, I would not exchange for twice ten thousand a year." *

When he landed in England on his return from the five happy years spent on board the *Beagle,* he was nearly twenty-eight. When he published the first edition of the *Origin of Species* he was over fifty. The intervening years were mainly devoted to seeing where his material led him, and when he was sure of this direction he spent his time in proving to the satisfaction of all its truth. Like Lyell, Darwin was a man of independent means, and thus was spared the necessity of frittering away his intellectual powers in earning his living.

On board the *Beagle* he had believed in the permanence of species, though vague doubts occasionally flitted across his mind. On March 7, 1837, he took lodgings in London, and remained there for nearly two years, until he married his cousin, Emma Wedgwood. During those two years he finished his Journal of his travels, read several papers before the Geological Society, began preparing the manuscript for his *Geological Observations,* and arranged for the publication of the *Zoology of the Voyage of the "Beagle."* This affords clues to the position geology occupied in his thoughts. In July he opened his first note-book for facts in relation to his thoughts on the origin of species. The character of South American fossils and the many species on the Galapagos Archipelago had seriously struck him. The brilliant hypotheses of his grandfather, Erasmus, were to be replaced by twenty-two years of drudgery with fact after fact. The hard, dry, scientific mind dislikes speculation, and is impressed by masses of facts, and these Darwin prepared to gather and to go on gathering—for a generation if need be.

* F. Darwin, *Life of C. Darwin,* p. 139 (1892 ed.).

In after-days men compared him with Newton, and the comparison is a just one.* Newton discovered the law of gravitation and Darwin the law of natural selection. Newton also laid the foundations of dynamics and natural philosophy and Darwin introduced a conception of nature that viewed it as a scene of ceaseless conflict and ceaseless development.† If we like, we can continue the parallel by suggesting that as Newton was condemned by his contemporaries on the basis of the philosophy of Bacon, so Darwin suffered condemnation on the basis of the philosophy of Bacon and Newton. For the moment the item in the parallel that concerns us most is the enormous energy and accuracy both men gave to their main conception.

In a manuscript quoted in the preface to *A Catalogue of the Newton MSS., Portsmouth Collection,* written probably about 1716, Newton writes ‡: "In the beginning of the year 1665 I found the method for approximating series and the rule for reducing any dignity [power] of any binomial to such a series [i.e. the binomial theorem]. The same year in May I found the method of tangents of Gregory and Slusius, and in November had the direct method of Fluxions [i.e. the elements of the differential calculus], and the next year in January had the Theory of Colours, and in May following I had entrance into the inverse method of Fluxions [i.e. integral calculus], and in the same year I began to think of gravity extending to the orb of the Moon . . . and having thereby compared the force requisite to keep the Moon in her orb with the force of gravity at the surface of the earth, and found them to answer pretty nearly. All this was in the two years of 1665 and 1666, for in those years I was in the prime of my age for invention, and minded Mathematics and Philosophy more than at any time since." From 1666 to 1686 the problem of gravitation lay at the back of the mind of Newton, and from 1836 to 1859 the problem of the origin of species lay at the back of the mind of Darwin. The

* Contrast A. Wigand, *Der Darwinismus und die Naturforschung Newton's und Cuvier's,* III, p. 14.

† A. R. Wallace, *Darwinism,* p. 9. Cf. E. Du Bois Reymond, *Reden,* I, p. 216.

‡ Cf. Appendix to Rigaud, *Essay on the Principia,* pp. 20, 23; Letter to Leibniz, Oct. 24, 1676, No. LV in the *Commercium Epistolicum:* Pemberton, Preface to *A View of Sir Isaac Newton's Philosophy,* 1728.

perseverance of the two men is at least as remarkable as their modesty. Another experiment enables us to grasp in some measure the patient and painstaking plan of inquiry Darwin pursued. In order to test the reality of earthworm castings, in 1842 he began to spread broken chalk over a field in Down, his Kentish home, in which in 1871 a trench was dug to test the results. How could any matter-of-fact scientist resist the man who was capable of waiting twenty-nine years in order to ascertain the outcome of a single experiment?

Darwin used to say that no one could be a good observer unless he was an active theoriser, a circumstance that explains the failures—as well as the successes—of scientists. "I am a firm believer," as he stated, "that without speculation there is no good and original observation." * As the facts accumulated under his never-ceasing industry, Darwin in 1838 read his Malthus, and the *Essay on the Principle of Population* performed not the least of its services to mankind when it enabled Darwin to render the principles Malthus applied to man applicable to plants and animals as well. Without this Essay we might never have had the *Origin of Species* in 1859. The caution and the self-criticism of Darwin demanded the clearest evidence, rejecting the most welcome support if it be not flawless. With accuracy of statement went his sincerity of opinion. Yet all can seek truth and show sincerity without necessarily attaining correctness; for that, a power of logical reasoning, though no virtue, is a most necessary talent; it was a faculty that Darwin and his co-discoverer, Wallace, valued highly in themselves. Love of one's subject comes of course first, and no one can doubt the love of Darwin for geology and biology. Only because love must be first do we place truth second. Perhaps indeed truth is one facet of love; for love's self-effacement leads to that objective treatment, freed from sentiment and prejudice, which forms the very foundation of science. First of the virtues, for the man of science as for all men else, is love. Love of his fellow-creatures, so beautifully illustrated in the story of Pasteur and the first child he saved from hydrophobia, has led many an investigator through suffering and privation, even to his death. But without a burning love of his subject,

* F. Darwin, *Life and Letters of C. Darwin*, II, p. 95.

be it the life of men, or of animals, or of plants, or of stones, no man of science has achieved greatness, the greatness of a Jenner and a Simpson, the greatness of a Lyell and a Helmholtz. For the true lover the object of his affection is all, he himself is nothing: self-suppression is the hall-mark of the great discoverer.

There are memorials to Newton and to Darwin in their own colleges, Trinity and Christ's It is appropriate that a plaque modelled by T. Woolner, made by Josiah Wedgwood & Sons, is on Darwin's rooms in Christ's College. A singularly beautiful statue of Newton by Roubiliac was given to Trinity College by the Master, Dr. Robert Smith, in 1750, and is now in the ante-chapel. Wordsworth, in his "Prelude" (bk. III), detected in Newton's "silent face," as depicted in this work of art,

> The marble index of a mind for ever
> Voyaging through strange seas of Thought, alone.

The scientist in the front rank requires the power of such a voyage every whit as real as that in the *Beagle*. He must be able to live a lonely life with his idea, content to see men ignore it when it is put before them, content to believe with Kepler that if God can wait six thousand years for one to contemplate His works, the discoverer too can wait. Loneliness is the fate of genius. It is noteworthy that the profoundest book St. Paul wrote, the Epistle to the Ephesians, and the greatest work of uninspired religious genius, the *Pilgrim's Progress,* were written in the seclusion of a prison. Mohammed meditated his message on the mount above Mecca, Dante pondered his poem in the sylvan solitudes of Fonte Avellana, and Cervantes wrote the saddest book in the world in the seclusion of a prison. All men who have a message for their fellows come to realise the justice of the remark Dr. Copleston addressed to Newman, once meeting him taking his solitary walk, "Nunquam minus solus quam cum solus."

The solitary is by no means a figure confined to religion: he is the type of all whose labours endure. Love of one's subject, the desire of truth, the power to lead a lonely life —these are qualities of genius. But let us not forget hope— the hope that tramples on failure, "is baffled to fight better," and through mists of doubt presses forward to the goal. Hope is the virtue of youth; but the truly great man possesses perpetual youth, in mind if not in body: ever ready for new ideas, ever looking at the heights. Do we always realise that practically all Darwin's volumes on evolution were published after he was fifty? "The substance of things hoped for" is faith, and this too is a necessary virtue of the man of science. Faith assures him that what he is doing is worth while; faith gives him that singleness of purpose without which no great task was ever accomplished; faith endows him with that patience and industry which Darwin claimed as his chief qualities. On faith in the unity and meaning of creation depends that breadth of view without which few men make discoveries of fundamental importance; and surely it is "the evidence of things not seen" which kindles in the natural philosopher the fire of imagination—that "Phantasie" which the great physiologist Johannes Müller acknowledged to be "ein unentbehrliches Gut."

One eminent quality of a man in authority in the Church is the power of suffering fools gladly, and it is also an eminent quality of a man in authority in the world of science. In a super-eminent degree Darwin possessed this quality. In 1844 Robert Chambers (1802—1871) published anonymously his *Vestiges of the Natural History of Creation.* It was vivid, it was graphic, and it gave the world a glimpse of a theory of development. How prepared the public was for such a theory is witnessed by the fact that in nine years it leaped to no less than ten successive editions. Two years sufficed for Chambers, and we cannot wonder that Darwin thought it showed "a great want of scientific caution." Since 1838 he had been in possession of his main conception, and though there were adumbrations of it in Chambers's book he kept silence, and this was not the least notable of his triumphs. It was a triumph, however, accompanied with a warning that he might be forestalled.

Preoccupied as he was with his leading idea, he was far from idle in other directions. A second edition of his *Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of H.M.S. "Beagle"* appeared in 1845. His *Zoology of the Voyage of H.M.S. "Beagle"* appeared in 1840; his *Structure and Dis-*

tribution of Coral Reefs in 1842; his *Geological Observations on South America* in 1846; his *Monograph of the Fossil Lepapidæ or Pedunculated Cirripedes of Great Britain* in 1851; his *Monograph of the Tubeless Cirripedia, with figures of all the Species,* in 1851; his *Monograph of the Fossil Balanidæ and Verrucidæ of Great Britain* in 1854. No one could doubt that the sometime Secretary of the Geological Society, who had eight such tomes to his credit, was a man to be reckoned with. The average F.R.S. was sure to be impressed by such masterly marshalling of facts.

Darwin's *Autobiography* contains a vivid history of the process by which he was able to revivify "the oldest of all the philosophies—that of evolution." * In 1838, thanks to Malthus, he had grasped the idea of natural selection. In 1842 he first allowed himself to write out his progress in thirty-five pages. In 1844 he enlarged this sketch into one of 230 pages. Struggle, selection, sexual selection, and variation—all were in his mind, though he attached much more weight to the influence of external conditions and to the inheritance of acquired habits than he did later. Must man be included with other animals in his quest for the origin of species? Yes. So far back as 1837 or 1838 he collected facts on this point, and they convinced him of the "Descent of Man" from an animal, though the book with this title was not published till 1871. Early in 1859, on Lyell's advice, he began to write out his views on the origin of species on a scale three or four times as extensive as he did in 1856. By 1856 he had sent Hooker his manuscript. Swinging away from any sympathy with the theories of Buffon and Lamarck, he reached an extreme position on the work of natural selection. In the July of that year he gave a brief sketch of his theory in a letter to Asa Gray, the American naturalist, mentioning the cardinal conceptions of the *Origin of Species.* The formation of a species he thought almost wholly due to the selection of "chance" variations. Neither the "blind fortuity" of Empedocles nor the "progressive principle" of Aristotle is in his mind. What he means by "chance" variations is that they occur under unknown laws.

A fortunate accident forced the pace of the slowly-working mind of the investigator. In 1858 A. R. Wallace had *sud-*

* F. Darwin, *Life of C. Darwin,* p. 169 (1892 ed.).

denly reached a theory similar to Darwin's, and sent him a paper, written in February, "On the Tendency of Varieties to depart indefinitely from the Original Type." * Darwin's feelings are plain in what he writes: "If Wallace had my MS. sketch written out in 1842 he could not have made a better short abstract of it. Even his terms stand now as heads of my chapters. Please return me the MS., which he does not say he wishes me to publish, but I shall, of course, at once write and offer to send it to any journal. So all my originality, whatever it may amount to, will be smashed, though my book, if ever it will have any value, will not be deteriorated; as all the labour consists in the application of the theory." † He then doubted what course he ought to pursue. Urged by his friends to publish an abstract of his own views, he wrote to Lyell: "Wallace might say, 'You did not intend publishing an abstract of your views till you received my communication. Is it fair to take advantage of my having freely, though unasked, communicated to you my ideas, and thus prevent me forestalling you?' The advantage which I should take being that I am induced to publish from privately knowing that Wallace was in the field. It seems hard on me that I should thus be compelled to lose my priority of many years' standing, but I cannot feel sure that this alters the justice of the case. First impressions are generally right, and I at first thought it would be dishonourable in me now to publish."

The feelings of this sensitive soul were left to the judgment of his friends Lyell and Hooker, the latter of whom had read the sketch of 1844. He suggested, as an undoubtedly more equitable course than Darwin's first impulse to publish Wallace's essay without note or comment of his own, that extracts from the manuscript of 1844 and from the letter of Dr. Asa Gray should be communicated to the Linnean Society along with Wallace's paper. This was the fairest course, and it was the course pursued. Accordingly the two papers were read together on the memorable evening of July 1, 1858, and published under the title *On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection*.

* F. Darwin, *Life of C. Darwin*, p. 189.
† F. Darwin, *Life and Letters of C. Darwin*, II. p. 116.

It is a satisfaction to be able to record that Wallace was as generous-minded as his co-discoverer. As when Joule proclaimed his views, there was not the semblance of a discussion.* Darwin wrote in his *Autobiography*: "Our joint productions excited very little attention, and the only published notice of them which I can remember was by Professor Haughton of Dublin, whose verdict was that all that was new in them was false, and what was true was old. This shows how necessary it is that any new view should be explained at considerable length in order to arouse public attention." † Darwin worked for thirteen months more, and in November 1859 at last appeared his book *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.*

The world of science, on its publication, began to undergo a profound transformation that has affected every department of it. The situation reminds one of the campanile of St. Mark's at Venice in July 1902. The guardian of the tower wanted a few inches more elbow room in his little kitchen and took away a sort of lintel in order to enlarge the passage. Next day there was a crack in the wall above, and the week after the whole campanile sat down upon itself. Nothing was changed; the same bricks and mortar were there; only the situation was different. Darwin's *magnum opus* rendered the whole scientific situation wholly different. The idea of natural selection had been conceived by Wells in 1813, by Patrick Matthew in 1831, and by Wallace in 1858. That second-rate philosopher, Herbert Spencer, came close to it in 1852. It is remarkable that Wells and Spencer, as well as Wallace, based their ideas on the Malthusian principle. There is infinite variability in wild and domestic animals. There appears indeed to be hardly any limit to the almost infinite plasticity and modifiability of domestic animals. "It would seem," said a great sheep-breeder, speaking of sheep, "as if farmers had chalked out upon a wall a form perfect in itself, and then proceeded to give it existence."

Granting individual variability, then, how do species arise

* F. Darwin, *Life of C. Darwin,* p. 186 (1892 ed.).
† *Ibid.*, p. 41.

in nature? And how are all the exquisite adaptations of part to whole, and of whole to environment, gradually initiated, improved, and perfected?

Here the book of Malthus comes to our assistance. For it teaches that here and now the world is over-populated. It is not going to be over-populated, but is actually at this moment over-populated. Species perpetually outruns subsistence. Linnæus reckoned that if an annual plant had two seeds, each of which produced two seedlings in the succeeding season, and so on continually, in twenty years their progeny would amount to a million plants. The roe of a cod contains nearly ten million eggs. If each of these produced a young fish which arrived at maturity, the whole sea must immediately become a solid mass of codfish. There is, then, a struggle for existence between members of the same species —not between members of different species—and this struggle is never ending. This struggle is between cod and cod, tiger and tiger, snake and snake. *Homo homini lupus,* so runs the old proverb which Hobbes applied to his own purpose. *Lupus lupo lupus,* so runs the proverb Darwin might have coined for his purpose.

The three men Darwin looked to for judgment on his work were Lyell, Hooker, and Huxley, and they all signified their agreement with it.* That is, from the angle of geology, botany, and zoology, three men of mark were on the side of the hypothesis launched upon the scientific world. All of them had their reservations, but they cordially accepted the main idea. Hooker did not disguise his opinion that he thought it had been pressed too far,† holding that Darwin had ignored the view of the mutability of species held by G. Saint-Hilaire and Lamarck.‡ Huxley pointed out, for instance, that the logical foundation of the *Origin* was insecure so long as experiments in selective breeding had not produced varieties which were more or less infertile, and he thought that that insecurity remains. In his Romanes lecture, given in 1892, Huxley held that natural selection failed to explain the origin of our moral and ethical nature.§ Lyell shrank

* F. Darwin, *Life of C. Darwin,* pp. 214, 235 (1892 ed.).
† L. Huxley, *Life and Letters of Sir J. D. Hooker,* I, p. 512.
‡ F. Darwin, *Life of C. Darwin,* pp. 207, 254, 256.
§ L. Huxley, *Life and Letters of T. H. Huxley,* I, p. 170.

from accepting the Darwinian teaching, for he foresaw its inevitable extension to the descent of man, and that was repugnant to his feelings. Ultimately, he ceased to shrink, and Darwin thought this one of the noblest acts he knew. For Lyell "to have maintained in the position of a master, one side of the question for thirty years, and then deliberately give it up, is a fact to which I * much doubt whether the records of science offer a parallel."†

Sir Richard Owen (1804—1892) was a naturalist who occupied such a foremost position in science that he has been called the British Cuvier, and he could not see his way to accept the old-new view. Darwin's special doctrine of natural selection he never appreciated. He attacked it with acerbity in an anonymous article in the *Edinburgh Review* for April 1860.‡ Darwin believed him to have inspired the hostile notice given to this book by Samuel Wilberforce, Bishop of Oxford, in the *Quarterly Review* of the same date. There is also reason to think that Owen proved the source of inspiration of the speech made by Wilberforce at the meeting of the British Association at Oxford in 1860. That is, what has long been believed to have been an episcopal attack was in fact an attack by a scientist with a first-class reputation. Wilberforce had taken a first-class in mathematics, and when Owen assured him that the new theory was an untrue theory he believed his informant. His ignorance was pardonable—if he had not ventured to speak. Not only was he foolish enough to speak, but he was more foolish to sneer. Turning to his antagonist, Huxley, with smiling insolence, he begged to know, Was it through his grandfather or his grandmother that he claimed his descent from a monkey? No wonder Huxley exclaimed to Sir Benjamin Brodie, who was sitting beside him, "The Lord hath delivered him into mine hands!" With an effectively quiet manner, he retorted that he was not ashamed to have a monkey for his ancestor; but that he would be ashamed to be connected with a man who used great gifts to obscure the truth. "Close to them stood one of the few men among the

* Darwin.

† F. Darwin, *Life of C. Darwin*, pp. 212, 260 (1892 ed.). Cf. Horner, *Life of Sir C. Lyell*, II, p. 384.

‡ On Owen's attitude, cf. R. Owen, *Life of Professor Owen*, II, p. 91.

audience already in Holy Orders, who joined in—and indeed led—the cheers for the Darwinians."* An eye-witness adds, "I was much struck with the fair and unprejudiced way the black coats and white cravats of Oxford discussed the question, and the frankness with which they offered their congratulations to the winners in the combat." † The feeling of the audience was very hostile to the Bishop, and Simpson, who had been most anti-Darwinian, declared that if that was all that could be said in favour of the old idea, he was a convert. The President of the section, Henslow, adjourned the discussion until the following Monday, but it was then thought by the leaders on both sides that it had better be dropped, and so the matter rested. On the Sunday, at the University Church, Frederick Temple, the future Archbishop of Canterbury, treated his audience to a sermon on Darwinism, in which he espoused Darwin's ideas very fully.‡ Huxley, when describing the mammalian heart, used to remark on the difficulty of distinguishing the tricuspid valve, on the right side, from the bicuspid valve, on the left, which resembles a bishop's mitre. His rule was that as a bishop is never known to be on the right, the mitral valve is on the left. In 1860, at any rate, a bishop-to-be was on the right!

The 1860 meeting of the British Association has attained a high degree of notoriety. There was another meeting of this Association at Oxford in 1894 which is sometimes forgotten.§ The President then was Lord Salisbury, who proceeded to attack, in the presence of Huxley, conceptions of the evolution theory. With delicate irony he spoke of the "comforting word, evolution," passing to Weismannism. Lord Salisbury quoted Lord Kelvin against Darwin, implying that the diametrically opposed views so frequently expressed nowadays threw the whole process of evolution in doubt. This of course irritated Huxley, who naturally considered Lord Kelvin a non-expert witness on a biological question. When the President had finished his address, Lord Kelvin proposed a vote of thanks, and he did so with genuine conviction. For he saw grave difficulties from a physico-

* L. Huxley, *Life and Letters of T. H. Huxley,* I, p. 182; F. Darwin, *Life of C. Darwin,* p. 236 ff. (1892 ed.).

† F. Darwin, *Life of C. Darwin,* p. 241 (1892 ed.).

‡ A. F. R. Wollaston, *Life of A. Newton,* p. 118.

§ J. Mavor, *My Windows on the Street of the World,* I, p. 320.

mathematical point of view in reconciling the Darwinian hypothesis of evolution in biology with the physical data he had in his mind. Huxley contented himself with a formal speech. The triumph of 1860 was not destined to be repeated, for Lamarckianism *inter alia* was at last coming into its own. If the shade of Sir Richard Owen or of Samuel Wilberforce could have been present, how he would have chuckled at the retribution falling on their former antagonist!

Huxley had the rare good fortune to review the *Origin of Species* for *The Times*, and a notice in the leading newspaper of 1860 rendered incalculable service to the new contribution to thought. Still, writing in 1887, he writes: "There is not the slightest doubt that, if a General Council of the Church scientific had been held at that time, we should have been condemned by an overwhelming majority." * Not a few clergymen naturally followed the lead given them by a Bishop, but it occasions surprise to find that, in the opinion of a competent judge, the scientists would also have condemned Darwin. The severe criticism passed by the *Quarterly Review* of July 1860 rendered a notable disservice to the reception of the evolution theory. In the *Life and Letters of Charles Darwin* there is a letter written by Hooker which we quote: "Huxley has sent me the proof of his contribution to the *Life*.† I do not think it too severe. The *Quarterly* then held the highest place amongst the first-class Reviews and was most bound to be fair and judicious, but proved unjust and malicious and ignorant. It went indefinitely beyond 'severity' and into scurrility, and for all Huxley says he cites abundant proof. It is not for us, who repeat *ad nauseam* our contempt for the persecutors of Galileo and the sneerers of Franklin, to conceal the fact that our own great discoverers met the same fate at the hands of the highest in the land of Literature and Science, as represented by its most exalted organ, the *Q.R.*"‡ Poulett Scrope in two

* J. W. Clark and A. C. Seward, *Order of the Proceedings at the Darwin Celebration held at Cambridge, June 22-24*, 1909, p. 20.

† Of Darwin.

‡ L. Huxley, *Life and Letters of Sir J. D. Hooker*, II, p. 300; F. Darwin, *Letters of C. Darwin*, p. 242 (1892 ed.). Cf. *Ibid.*, p. 221. Hooker wrote to Darwin in 1859, "I saw a highly flattering notice [of the *Origin*] in the *English Churchman*, short and not at all entering into discussion, but praising you and your book, and talking patronisingly of the doctrine!"

luminous articles in the *Quarterly Review* did for Lyell what Huxley accomplished for Darwin in his famous review in *The Times*.* Sir Richard Owen controlled this great Review in 1860, and this created a world of difference.

It is one of the ironies of the situation that in their views Darwin and Wallace did not continue to see eye to eye. The longer Wallace worked practically, the more he perceived difficulties in the way. In his *World of Life* he states three of the most formidable of these. The first is that the slight beginnings of new organs would be useless, and could not therefore be preserved and increased by natural selection—to which it is answered that the usual method of evolution is to make apparent novelties by the transformation or specialisation of old-established structures. The second difficulty is that new adaptations imply a number of concurrent variations—to which it is answered that time is long and variability great, and that coincident variations are demonstrably numerous in connection with both difficulties. Professors Baldwin, Lloyd Morgan, and Osborn have suggested that adaptive individual modifiability may serve as a life-saving screen till hereditary germinal variations in the same direction have grown strong. The third difficulty is in the excessive development of characters, such as decorations and weapons, beyond the limits of utility, and the answer, we glean, is found in Weismann's ingenious hypothesis of germinal selection. Huxley and Hooker took the field with enthusiasm when Darwin extended his theory to the descent of man. From Wallace he received no support in this matter. Wallace admitted everything in regard to the morphological descent of man, but maintained, in a mystic manner, that something else, something spiritual, must have been added to that inherited from his animal ancestors.† Though he urged that natural selection accounted for the evolution of man's bodily frame from the simian stock, yet from this point of view some extraneous power had inspired him with his mentality, and with a future purpose in view had provided the mere savage with a brain disproportionate to his require-

* F. Darwin, *Letters of C. Darwin*, p. 221 (1892 ed.).

† Cf. G. Schwalbe in A. C. Seward's ed. *Darwin and Modern Science*, p. 116.

ments, whether compared with civilised man or with the brutes.*

The well-known *Essays and Reviews* was an able effort to contribute to one of the many reconciliations between science and religion or rather theology. In it a thoughtful observer,† the Rev. Baden Powell (1796—1860), Savilian Professor of Geometry at Oxford, records that: "Just a similar scepticism has been evinced by nearly all the first physiologists of the day, who have joined in rejecting the development theories of Lamarck and the *Vestiges*. . . . Yet it is now acknowledged under the high sanction of Owen that 'creation' is only another name for our ignorance of the mode of production, . . . while a work has now appeared by a naturalist of the most acknowledged authority, Mr. Darwin's masterly volume on the *Origin of Species,* by the law of 'natural selection,' which now substantiates on undeniable grounds the very principle so long denounced by the first naturalists—the origination of new species by natural causes: a work which must soon bring about an entire revolution of opinion in favour of the grand principle of the self-evolving powers of nature." These striking words appeared in 1860 in a study of the evidences of Christianity, and attest that the lead given by Temple at Oxford in 1860 was not altogether forgotten. Canon Henry Baker Tristram (1822—1906) was the first zoologist of any note who publicly accepted the Darwinian views in his paper in the *Ibis* of October 1859, though on mature thought he modified his language.

Lyell, Hooker, and Huxley had so often discussed the *Origin of Species* with Darwin that many of its difficulties and objections are met by anticipation. Of his three friends, Lyell and Hooker contributed most to this result. Darwin, in spite of all this preparation, found trouble in convincing naturalists who had "a bigoted idea of the term species." ‡ His ideas were more readily understood by intelligent people who were not professed naturalists. Among scientific men they were accepted most commonly by geologists, thanks to Lyell's *Principles of Geology,* next by the botanists, and least

* A. R. Wallace, *Contributions to the Theory of Natural Selection,* p. 359.

† *Essays and Reviews,* pp. 138-9.

‡ L. Huxley, *Life and Letters of Sir J. D. Hooker,* I, p. 508; F. Darwin and A. C. Seward, *More Letters of C. Darwin,* I, p. 175.

by the zoologists.* Darwin wrote to Lyell on December 2, 1859: "H. C. Watson tells me that one zoologist says he will read my book, 'but I will never believe it.' What a spirit to read any book in!"† Sir Roderick Murchison was a geologist who believed "that the evidence of the older formation lent no support to the views now enunciated. Darwin "will have no creation—no signs of a beginning—millions of living things before the lowest Silurian—no succession of creatures from lower to higher, but a mere transmutation from a monad to a man. His assumption of the position of the Lyellian theory, that causation never was more intense than it is now, and that former great disruptions (faults) were all removed by the denudation of the ages, is so gratuitous, and so entirely antagonistic to my creed, that I deny all his inductions, and am still as firm a believer as ever that a monkey and a man are distinct species, and not connected by any links." ‡

Darwin's old teacher, the Rev. Professor Henslow, made a stout stand on his side, thinking the matter a legitimate one for investigation.§ Some of the geologists relied for criticism on the assumed perfection of the geological record. On November 22, 1860, Darwin wrote to H. W. Bates, "As you say, I have been thoroughly well attacked and reviled (especially by entomologists—Westwood, Wollaston, and A. Murray have all reviled and sneered at me to their hearts' content), but I care nothing for their attacks." || We can understand Darwin writing: "I am actually weary of telling people that I do not pretend to adduce direct evidence of one species changing into another, but that I believe that this view in the main is correct, because so many phenomena can be thus grouped together and explained. But it is generally of no use; I cannot make persons see this. I generally throw in their teeth the universally admitted theory of the undulation of light,—neither the undulation nor the very

* L. Huxley, *Life and Letters of Sir J. D. Hooker,* I, p. 508; F. Darwin and A. C. Seward, *More Letters of C. Darwin,* I, p. 167.

† F. Darwin, *Life of C. Darwin,* p. 218 (1892 ed.).

‡ Sir A. Geikie, *Life of Sir Roderick Murchison,* II, p. 321. Contrast Sir A. Geikie, *A Long Life's Work,* pp. 71, 143.

§ L. Huxley, *Life and Letters of Sir J. D. Hooker,* I, p. 508. Cf. F. Darwin, *Letters of C. Darwin,* pp. 227, 234 (1892 ed.).

|| F. Darwin and A. C. Seward, *More Letters of C. Darwin,* I, p. 118.

existence of ether being proved, yet admitted because it explains so much." *

The influence of an hypothesis comes out in the repeated references made to the difficulty of convincing the naturalists. Botanists and geologists proved more reasonable people. "I am much pleased," Darwin records, "that the younger and middle-aged geologists are coming round, for the arguments from geology have always seemed strongest against me. Not one of the older geologists (except Lyell) has been ever shaken in his views of the eternal immutability of species." † On March 3, 1860, he wrote to Hooker: "One large class of men, more especially I suspect of naturalists, never will care about *any* general question, of which old Gray, ‡ of the British Museum, may be taken as a type; and secondly, nearly all men past a moderate age, either in actual years or in mind, are, I am fully convinced, incapable of looking at facts under a new point of view." § The same despair of middle and old age breaks out in a letter to Huxley on December 2, 1860: "I can pretty plainly see that, if my view is ever to be generally adopted, it will be by young men growing up and replacing the old workers, and then the young ones finding that they can group facts and search out new lines of investigation better on the notion of descent, than on that of creation." Throughout this correspondence we have instance after instance of elder men losing that elasticity of judgment and modifiability of intellect which are so indispensable for the reception of new and fundamental concepts. No wonder Hewett Cottrell Watson wrote to Darwin on November 21, 1859: "Now these novel views are brought fairly before the scientific public, it seems truly remarkable how so many of them could have failed to see their right road sooner. How could Sir C. Lyell, for instance, for thirty years read, write, and think on the subject of 'species and their succession' and yet constantly look down the wrong road?" || H. C. Watson might have gone on to ask, Why did the scientific public, after 1859, persist in still looking down the wrong road?

* F. Darwin and A. C. Seward, *More Letters of C. Darwin*, I, p. 184.
† F. Darwin, *Life of C. Darwin*, p. 230 (1892 ed.).
‡ *Ibid.*, p. 219.
§ *Ibid.*, p. 244.
|| F. Darwin, *Life of C. Darwin*, I, p. 352; II, p. 226.

The quarrels of the scientific men are satirised in *Public Opinion* of April 23, 1863, in a graphic account of a police-case. Mr. John Bull gave evidence that—

"The whole neighbourhood was unsettled by their disputes; Huxley quarrelled with Owen, Owen with Darwin, Lyell with Owen, Falconer and Prestwich with Lyell, and Gray the menagerie man with everybody. He had pleasure, however, in stating that Darwin was the quietest of the set. They were always picking bones with each other and fighting over their gains. If either of the gravel sifters or stone breakers found anything, he was obliged to conceal it immediately, or one of the old bone collectors would be sure to appropriate it first and deny the theft afterwards, and the consequent wrangling and disputes were as endless as they were wearisome.

"LORD MAYOR.—Probably the clergyman of the parish might exert some influence over them?

"The gentleman smiled, shook his head, and stated that he regretted to say that no class of men paid so little attention to the opinions of the clergy as that to which these unhappy men belonged." *

In Bret Harte's *Poems* there is "Truthful Sammy's" account of the row "That broke up our Society upon the Stanislow."

Now nothing could be finer or more beautiful to see
Than the first six months' proceedings of that same Society,
Till Brown of Calaveras brought a lot of fossil bones
That he found within a tunnel near the tenement of Jones.

Then Brown he read a paper, and he reconstructed there,
From those same bones, an animal extremely rare;
And Jones then asked the Chair for a suspension of the rules,
Till he could prove that those same bones was one of his lost mules.

Then Brown he smiled a bitter smile, and said he was at fault,
It seemed he had been trespassing on Jones's family vault;
He was a most sarcastic man, this quiet Mr. Brown,
And on several occasions he had cleaned out the town.

Now I hold it is not decent for a scientific gent
To say another is an ass,—at least, to all intent;
Nor should the individual who happens to be meant
Reply by heaving rocks at him, to any great extent.

Then Abner Dean of Angel's raised a point of order, when
A chunk of old red sandstone took him in the abdomen,

* F. Darwin, *Life of C. Darwin*, p. 259 (1892 ed.).

And he smiled a kind of sickly smile, and curled up on the floor,
And the subsequent proceedings interested him no more.

For, in less time than I write it, every member did engage
In a warfare with the remnants of the palæozoic age;
And the way they heaved those fossils in their anger was a sin,
Till the skull of an old mammoth caved the head of Thompson in.

Of course these lines form amusing reading, but is it not pathetic to note that what should have been a contest for truth in the eyes of the public was regarded as a personal matter? It is a relief to turn to a letter the Rev. Charles Kingsley wrote on December 18, 1859. Kingsley was no match for such a subtle dialectician as John Henry Newman, yet there is an honesty of outlook in the man that commands our hearty respect. He wrote to Darwin: " I have to thank you for the unexpected honour of your book. That the naturalist whom, of all naturalists living, I most wish to know and to learn from, should have sent a scientist like me his book, encourages me at least to observe more carefully, and think more slowly.

" I am so poorly (in brain), that I fear I cannot read your book just now as I ought. All I have seen of it *awes* me; both with the heap of facts and the prestige of your name, and also with the clear intuition, that if you be right, I must give up much that I have believed and written.

" In that care I little. Let God be true, and every man a liar. Let us know what *is*, and, as old Socrates has it, ἕπεσθαι τῷ λόγῳ—follow up the villainous shifty fox of an argument, into whatsoever unexpected bogs and brakes he may lead us, if we do but run into him at last.

" From two common superstitions, at least, I shall be free while judging of your book:

" (1) I have long since, from watching the crossing of domesticated animals and plants, learnt to disbelieve the dogma of the permanence of species.

" (2) I have gradually learnt to see that it is just as noble a conception of Deity, to believe that He created primal forms capable of self-development into all forms needful *pro tempore* and *pro loco*, as to believe that He required a fresh act of intervention to supply the lacunas which He Himself had made. I question whether the former be not the loftier thought.

"Be that as it may, I shall prize your book, both for itself, and as a proof that you are aware of the existence of such a person as Your faithful servant, C. Kingsley." *

The Vicar of Down, Darwin's parish church, was the Rev. J. Brodie Innes, of Milton Brodie, and he writes: "We never attacked each other. Before I knew Mr. Darwin I had adopted, and publicly expressed, the principle that the study of natural history, geology, and science in general should be pursued without reference to the Bible. That the Book of Nature and Scripture came from the same Divine source, ran in parallel lines, and when properly understood would never cross. . . .

"In [a] letter, after I had left Down, he [Darwin] writes, 'We often differed, but you are one of those rare mortals from whom one can differ and yet feel no shade of animosity, and that is a thing [of] which I should feel very proud if any one could say [it] of me.'

"On my last visit to Down, Mr. Darwin said, at his dinner-table, 'Innes and I have been fast friends for thirty years, and we never thoroughly agreed on any subject but once, and then we stared hard at each other, and thought one of us must be very ill." †

Darwin had to face the opposition of William Henry Harvey (1811—1866), an Irishman whom he terms "a first-rate botanist." For seven years Harvey worked hard at the botany of South Africa, and became the chief authority on algæ. In 1853 he visited India, Australia, and the South Sea Islands, a voyage of three years, covering much of the ground Darwin had covered. In 1859 he was forty-eight, not too advanced an age, one would have thought, to receive a new idea, yet it seems, as with most men of science of his time who were already in mental maturity, the *Origin* contained doctrines too revolutionary to be readily accepted by him.‡ So far from accepting Darwin's theory, he delivered an address against it, and he had this address printed. Subsequently he came round to accept some of the views propounded, and did his best to call in copies of the address, which to the end of his life he sincerely regretted having

* F. Darwin, *Life of C. Darwin*, p. 228 (1892 ed.).
† F. Darwin, *Life of C. Darwin*, p. 229 (1892 ed.).
‡ W. V. Ball, *Reminiscences and Letters of Sir R. Ball*, p. 44.

published. But he never became a real Darwinian. In one of his letters to Asa Gray he wrote, " A good deal of Darwin reads to me like an ingenious dream," * which is pretty much the attitude of Sedgwick. Harvey's own work lay in botanical discrimination, description, and illustration. Though ready to admit natural selection as a *vera causa* of much change, he would not go so far as to admit it a *vera causa* of species. He suspected that Darwin had ascribed too great efficacy to secondary causes and, as it were, deified natural selection.† Hooker, who was an old friend of Harvey, told him that he had been of the views of Darwin for fourteen years before he had adopted them, and that he had done so solely and entirely from an independent study of the plants themselves. Hooker wrote to Harvey, " I am profoundly indifferent to the sneers and contempt I have received from the opposite side of your passage [the Irish Sea]. Asa Gray alone has treated me with candour and fairness; all other botanists are either indifferent, hostile, or contemptuous."‡

John Hutton Balfour (1808—1884) was a Scots systematic botanist who conducted botanical excursions with pupils energetically, and extended them to almost every part of Scotland. Impartial in the breadth of his teaching, he was ever anxious to assimilate new knowledge. The knowledge of 1859 proved too much for him, and he found himself unable to accept it.§ Just as Harvey turned Irish opinion in general and Dublin University opinion in particular against Darwin, so Balfour turned Scots opinion in general and Edinburgh University opinion || in particular. Nor was the attitude of Oxford University more friendly. John Phillips (1800—1874) was Professor of Geology there. More than a hundred papers stand under his name in the Catalogue of the Royal Society, and the variety of their subjects attests the wide range of his knowledge. Attractive as a speaker and lecturer, he was, we learn, eminently judicious, ever courteous, genial, and conciliatory. It was serious for Darwin that he too stood on the side of opposi-

* W. V. Ball, *Reminiscences and Letters of Sir R. Ball*, p. 44.
† L. Huxley, *Life and Letters of Sir J. D. Hooker*, I, p. 26.
‡ *Ibid.*, I, p. 515.
§ *Ibid.*, p. 202.
|| *Ibid.*, p. 515.

tion. William Clark (1788—1869) was Professor of Anatomy in Cambridge University, and carried out his duties so efficiently that he laid the foundation of the school of biological science at Cambridge. He always lectured from the actual subject, which was not usually done in his time, and performed the dissections himself with singular neatness. Like Phillips, with Sedgwick he opposed the opinions of the *Origin.** We have given enough from the Universities of Dublin and Edinburgh, of Oxford and Cambridge, to prove that the scientific world displayed no undue eagerness to believe what was set before it. The three men who knew Darwin thoroughly well, Lyell, Hooker, and Huxley, accepted his ideas, but the members of the world of science —outside this narrow range—set their faces like a flint against them. Nor was Oxford University the only home of lost causes. The academic world felt pretty unanimous on this point.

Hugh Falconer (1808—1865) was a botanist and a palæontologist of more than common distinction. Marked by a penetrating intellect, he had the charm of a frank and winning disposition. Securing a nomination as assistant-surgeon on the Bengal establishment of the East India Company, he turned his attention to the two subjects in which he has won permanent fame. For twenty-five years he laboured at them, discovering the earliest fossil quadrumana, many species of the mastodon and elephant, several species of rhinoceros, new sub-genera of hippopotamus, the colossal ruminant sivatherium, species of ostrich, crocodiles, the enormous tortoise collossochelys, and numerous fishes. On his retirement from the Indian service in the spring of 1855, he resumed his palæontological researches, visiting almost every museum in Western Europe. Researches on the fauna of the ossiferous caves of Gower led him in 1860 to prove that elephas antiquus and rhinoceros hemitœchus were members of the cave fauna of England. In his latter years he spent much time in examining the evidences as to the antiquity of man, which he had been led to anticipate in India in 1844. In fact, every current question about fossil mammalia and prehistoric man Falconer investigated and

* Huxley, *Life and Letters of Sir J. D. Hooker,* p. 514.

commented upon in a patient, impartial, and candid spirit. He was always seeking fresh evidence and developing his ideas, many of which he never committed to writing, owing to the tenaciousness of his memory. Having returned hastily from Gibraltar to support the claims of Darwin to the Copley Medal, the highest award of the Royal Society, he suffered much from fatigue and exposure, and on January 31, 1865, he passed away. In 1861 he had been rambling through the north of Italy and Germany, and everywhere he heard Darwinism discussed—"the views of course often dissented from, according to the special bias of the speaker—but the work, its honesty of purpose, grandeur of conception, felicity of illustration, and courageous exposition, always referred to in terms of the highest admiration." * In spite of all this praise, he was among those who did not fully accept the views expressed in the *Origin,* but he could differ from its author without bitterness.†

Sir Edward Sabine (1788—1883) was a distinguished general of the Royal Artillery who also attained the position of President of the Royal Society. With his scientific capacity he possessed a personality as attractive as that of Hugh Falconer himself. His grace of manner and invincible cheerfulness rendered him universally popular. Astronomy, terrestrial magnetism, biology, and ornithology were his favourite studies. He was anti-Darwinian, and not unwilling to deliver a left-handed attack on the new Copley medallist. Darwin records that "some old members of the Royal [Society] are quite shocked at my having the Copley."‡ He goes on to say that "such a feeling existed is clear from the action of the Council in pointedly omitting from the grounds of their award the *theory* set forth in the *Origin.* That this book could within five years of its publication be valued by the Royal Society merely as a 'mass of observations, etc.,' is striking evidence of the slow progress of Evolution. It may perhaps be said that 1870 is the date at which the current of scientific opinion is seen to be definitely flowing in the direction of Evolution." ‡

* F. Darwin, *Life of C. Darwin,* p. 247.

† F. Darwin and A. C. Seward, *More Letters of C. Darwin,* I, p. 253.

‡ J. W. Clark and A. C. Seward, *Order of the Proceedings at the Darwin Celebration held at Cambridge, June 22-24,* 1909, p. 20.

At the Anniversary Meeting and Dinner of the Royal Society in 1864, Sabine wrote the only part of the address to Darwin: "Speaking generally and collectively, we have expressly omitted it [Darwin's theory] from the grounds of our award." * Hooker wrote to Darwin on December 2, 1864: "Have you heard of the small breeze at R.S. a propos of your reward? Busk told me thus: Sabine said, in his address, that in awarding you the Copley 'all consideration of your *Origin* was expressly excluded.' After the address, Huxley gets up and asks how this is, and being assured it is so, he insists on the Minutes of the Council being produced and read, in which of course there was no such exclusion or indeed any allusion to the *Origin*. Busk and Sabine were afterwards discussing the point, Sabine saying that no allusion = express exclusion, and shuffling as usual, when up comes Falconer, and to Busk's horror compliments Sabine's address unreservedly. Busk, thinking that Falconer had overheard the discussion, said nothing at the time, but calls Falconer to account, upon which Falconer is grievously put out at finding what he has done and forthwith goes and writes a letter to Sabine on the subject. May the Lord have mercy on Sabine, is all I can say; for Falconer will have none." †

It was reserved for a Professor of Engineering at Edinburgh to deliver what Darwin felt to be the most valuable criticism ever made on his views,‡ and this professor was Henry Charles Fleeming Jenkin (1833—1885). His taste in literature was as broad and unconventional as in science. His determinative work in electricity is of the highest value, while his varied originality as an inventor is testified by his thirty-five patents, and by his scientific papers. He criticised natural selection on mathematical grounds.§ It was, he urged, an infinitesimal chance that an individual with a particular variation should meet with a similar varying mate and so propagate the variation.

* L. Huxley, *Life and Letters of T. H. Huxley*, I, p. 254.

† L. Huxley, *Life and Letters of Sir J. D. Hooker*, II, p. 75; L. Huxley, *Life and Letters of T. H. Huxley*, I, p. 254; J. W. Clark and A. C. Seward, *Order of the Proceedings at the Darwin Celebration held at Cambridge, June 22-24*, 1909, p. 20.

‡ F. Darwin, *Life of C. Darwin*, p. 274.

§ L. Huxley, *Life and Letters of Sir J. D. Hooker*, II, p. 83.

St. George Jackson Mivart (1827—1900) was a brilliant biologist and anatomist who formally opposed Darwinism while he supported evolution by the side-issue of derivative creation. He consistently maintained an essential disparity between organic and inorganic matter, and between human reason and the highest faculties of the brutes. Natural selection he relegated to an extremely subordinate place, and attributed the formation of specific characters to a principle of individuation, which he postulated as the essence of life. He freely criticised the hypothesis of the great naturalist both in the *Quarterly Review* * and in a substantive essay "On the Genesis of Species," and this assertion of the right of private judgment led to an estrangement from both Huxley and Darwin. Huxley replied in an article in the *Contemporary Review* which was extremely effective.† Mivart had attempted to show that evolution, at least in his sense, had been accepted in advance by such authorities in his communion as the Jesuit Suarez. Turning up the references quoted, Huxley ascertained that the precise opposite was stated, and with delicious irony was able to pose as the defender of Roman Catholic orthodoxy against a heterodox son of the Church. At the same time he was full of cold anger against the man who was writing privately to express his friendship for Darwin, yet, as the anonymous critic in the *Quarterly Review,* was treating Darwin in a manner Hooker terms "alike unjust and unbecoming." ‡ For Mivart sneered at Darwin's candour, one of the most attractive of his many attractive qualities, and at the mutually generous relations between him and Wallace over the enunciation of natural selection. This Mivart criticism is one of the least edifying episodes of the whole controversy.

Asa Gray occupies a high position in the ranks of American men of science, and it is plain that Darwin was most anxious to enlist his adherence. Gray was impressed by the masterly manner and the no less masterly matter of the *Origin.* On January 23, 1860, Gray wrote to Darwin: "It naturally happens that my review of your book does not

* CXXXI, p. 47.

† T. H. Huxley, *Darwiniana,* pp. 120-86. It is one of the most pungent and penetrating articles Huxley ever wrote.

‡ L. Huxley, *Life and Letters of Sir J. D. Hooker,* II, p. 128.

exhibit anything like the full force of the impression the book has made upon me. Under the circumstances I suppose I do your theory more good here, by bespeaking for it a fair and favourable consideration, and by standing non-committed as to its full conclusions, than I should if I announced myself a convert; nor could I say the latter, with truth. Well, what seems to be the weakest point in the book is the attempt to account for the formation of organs, the making of eyes, etc., by natural selection. Some of this reads quite Lamarckian." * Any reference to the labours of Lamarck stirred Darwin to fury, and we can readily conceive how this letter must have made him writhe.

Louis Jean Rodolphe Agassiz (1807—1873) entered a college at Bienne at the age of 10, and from 1822 to 1824 he was a student at the Academy of Lausanne. He afterwards spent some years as a student in the Universities of Zürich, Heidelberg, and Munich, where he gained a reputation as a skilful fencer. It was at Heidelberg that his studies took a definite turn towards natural history. He took a Ph.D. degree at Erlangen in 1829. He published his first paper in *Isis* in 1828, and for many years devoted himself to ichthyology. During a visit to Paris he became acquainted with Cuvier and Alexander von Humboldt; in 1833, through the liberality of the latter, he began the publication of his *Recherches sur les Poissons Fossiles,* and in 1840 he completed his *Etudes sur les Glaciers.* In 1846 he went to Boston, where he lectured in the Lowell Institute, and in the following year became Professor of Geology and Zoology at Cambridge. During the last twenty-seven years of his life Agassiz lived in America, and exerted a wide influence on the study of natural history in the United States. In 1836 he received the Wollaston Medal of the Geological Society of London, and in 1861 was selected for the Copley Medal of the Royal Society. In an article on "Evolution and Permanence of Type" he repeated his strong conviction against the views embodied in the *Origin.* "A physical fact," to him, "is as sacred as a moral principle. Our own nature demands from us this double allegiance. . . . I hope in future articles to show, first, that, however broken the

* F. Darwin, *Letters of C. Darwin,* p. 225. This letter is better given in A. Gray, *Letters,* II, p. 457.

geological record may be, there is a complete sequence in many parts of it, from which the character of the succession may be ascertained; secondly, that, since the most exquisitely delicate structures, as well as embryonic phases of growth of the most perishable nature, have been preserved from very early deposits, we have no right to infer the disappearance of types because their absence disproves some favourite theory; and lastly, that there is no evidence of a direct descent of later from earlier species in the geological succession of animals." *

The high opinion Darwin † entertained of Agassiz comes out in a letter he wrote to Longfellow: "What a set of men you have in Harvard! Both our universities put together cannot furnish the like. Why, there is Agassiz,—he counts for three." ‡ And it is pleasant to record that of Darwin personally Agassiz had none but good words.

In a letter to Sir Philip de Grey Egerton, Agassiz wrote: "My recent studies have made me more adverse than ever to the new scientific doctrines which are flourishing in England. This sensational zeal reminds me of what I experienced as a young man in Germany, when the physio-philosophy of Oken had invaded every centre of scientific activity; and yet, what is there left of it? I trust to outlive this mania also. As usual, I do not ask beforehand what you think of it, and I may have put my hand into a hornet's nest; but you know your old friend Agassiz, and will forgive him if he hits a tender spot." § Nor were the effects of the criticism of Agassiz confined either to America or to England. The Professor of Zoology at Göttingen, Keferstein, wrote in 1862 in the *Göttinger Gelehrte Anzeiger:* "It gives great satisfaction to the earnest scientific worker to see a man like Agassiz, with an authority based on the finest zoological works, reject unreservedly a theory [i.e. Darwin's] that would discredit the whole work of classifiers for a century, and to see the views built up by several generations and the general consent of humanity hold a stronger position than

* C. F. Holder, *Louis Agassiz,* p. 181. Cf. F. Darwin, *Letters of C. Darwin,* p. 225.

† *Ibid.,* p. 180. Cf. J. Marcon, *Life, Letters, and Works of Louis Agassiz;* E. C. Agassiz, *Louis Agassiz, his Life and Correspondence.*

‡ C. F. Holder, *Louis Agassiz,* p. 180.

§ *Ibid.,* p. 180. Cf. the Smithsonian Report, 1873, p. 198.

the views of a single individual, however eloquently they may be cited." *

Agassiz, like Asa Gray, could not renounce his firmly-established conceptions, and the remark Hooker makes of Gray applies to him also, and alas! to many another scientist as well. Hooker records, "I did not follow Gray into his later comments on Darwinism, and I never read his *Darwiniana.* My recollection of his attitude after acceptance of the doctrine, and during the first few years of his active promulgation of it, is, that he understood it clearly, but sought to harmonise it with his prepossession—without disturbing its physical principles in any way." † A mind that has hardened down into the last stage of extreme maturity may assimilate fresh facts and fresh minor principles, but it cannot accept fresh synthetic systems of the cosmos. Besides, some of the senior thinkers were committed beforehand to opposing views, with which they lacked either the courage or the intellectual power to break. A scientist wants that state of mind Goethe defined as "Thätige Skepsis"—active doubt. What he possesses is not uncommonly passive acquiescence in accepted opinion. There is a scientific orthodoxy which works untold harm. Like the Bourbons, it neither learns nor forgets. It owns a scientific creed, and, though it commits intellectual suicide by so doing, imposes official punishments on those who refuse to accept it.

Among the English-speaking races in our own islands and in North America there was frank hostility displayed to the *Origin.* Nor is there any reason to believe that European opinion was very much more friendly. The Secrétaire Perpétuel, P. Flourens, published ‡ in 1864 his dull, weak, and shallow *Examination du Livre de M. Darwin sur l'Origine des Espèces.* The work of Jean Louis Armand de Quatrefages de Bréau was largely anthropological, and in his writings and lectures he always combated evolutionary ideas. § Gaspard Auguste Brullé, Professor of Zoology and Comparative Anatomy at Dijon, could not comprehend these ideas, and was in 1864 still unconvinced of their truth. ||

* W. Bölsche, *Haeckel, his Life and Work,* p. 148.
† L. Huxley, *Life and Letters of Sir J. D. Hooker,* II, p. 305.
‡ Cf. T. H. Huxley's delicious critique in his *Darwiniana,* pp. 98-106.
§ F. Darwin and A. C. Seward, *More Letters of C. Darwin,* I, p. 186.
|| *Ibid.,* I, p. 257.

François Jules Pictet, the palæontologist, in the *Archives des Sciences de la Bibliothèque Universelle,* Mars 1860, delivered himself, and of his deliverance Darwin wrote: "There has been one prodigy of a review, namely, an *opposed* one . . . which is perfectly fair and just, and I agree to every word he says; our only difference being that he attaches less weight to arguments in favour, and more to the arguments opposed, than I do. Of all the opposed reviews, I think this the only quite fair one, and I never expected to see one." * If there were a de Bréau and a Pictet on the one side, there was a Gaudry and a Ribot on the other. In 1878 Darwin was elected a Corresponding Member of the French Institute in the Botanical Section. In 1872 an attempt had been made to elect him in the Section of Zoology, when he only received fifteen out of forty-eight votes, and Lóven was chosen instead. The zoologists cherished their dislike of his ideas longer than the other scientists.

In all the labours of Jean Henri Casimir Fabre (1823—1915) there is the touch of the poet who can exclaim with Browning:

> The rest may reason and welcome: 'tis we musicians know.

His method of working combined with his patience in research rendered him what Darwin called him, "an incomparable observer." † Fabre came of a humble stock, like many other great men. But he had received a good education, partly in village schools and local colleges, partly the gift of his own resolute perseverance. He was something of a chemist, a mathematician who had conquered the binomial theorem, and a fair classical scholar. He was a schoolmaster at Carpentras, a professor at Ajaccio and Avignon, before whom the possibility of a chair at one of the greater universities lay open. The complete story of what led to his retirement, first to Orange and then to Sérignan, and to the need of supporting himself by his pen, is not told in his own books. It had something to do with politics, something with a quarrel with the Church of which he always remained a devout member, and something to do with the destruction of madder-growing by artificial dyes. But if it seemed a mis-

* F. Darwin, *Life of C. Darwin,* p. 231.

† A. Fabre, *The Life of J. H. Fabre,* p. 215. Darwin was acquainted only with the first volume of Fabre's *Souvenirs.* Darwin died in 1881, and the second volume of the *Souvenirs* appeared in 1883.

fortune at the time, he faced it bravely, and the world was the gainer. There are many who can make morphological studies and accomplish the systematic work on which Fabre had started, and which doubtless he would have continued in academic ease. But there have been very few observers of living things so untiring or so understanding. His peaceful yet arduous years at Sérignan provided him with the environment and the occupations most congenial to his natural bent. Had his early ambitions been realised, we should have had another great entomologist, but not the Virgil of the Insects. Fabre was an artist in words, as well as an incomparable observer. He adopted an attitude pleasing to himself and pleasing to his audience. Here am I, he continued to say, a poor peasant, in my sun-baked southern fields. I have neither the riches of the tropics nor the assistance of libraries and laboratories. But it is the truth I give you, and not the theories by which scientific men are beguiled. Great people, no doubt, but overwhelmed by their science, puffed up with pride! Let me show you what can be done in a field, with patience. Away with names and classifications and all their fusty jargon! In actual fact there was never anyone more careful to identify the creatures he was observing or to use their names correctly. This punctilious exactitude, combined with almost vehement disclaimer of it, has set pitfalls into which critics have fallen. And in the same fashion, his real obedience to the scientific method of observation, deduction, and confirming experiment, joined with a lyrical denunciation of science, has snared many an unwary commentator.

His stubborn opposition to evolution was a logical consequence of the evidence before him, as it was to Milne-Edwards, a French scientist who remained to the end unconvinced by the arguments of Darwin. The greater part of Fabre's life was concentrated on the study of instinct among creatures which show that quality in its highest form. He came on the theory of descent at a time when the effort was made to derive the higher structure or quality from the highest stage of the structure or quality next below it. Assuming that intelligence had arisen from instincts, the comparison was made between human intelligence and the highest types of instinct. We now know that to be a wrong method. Intelligence must be traced down to its lowest grades, and instinct must similarly be traced backwards until it is possible to compare the most primitive forms of each. One might as readily try to derive a cat from a canary as the intelligence of man from the instincts of the mason wasps. Fabre's contention against evolutionism, on its negative side, was sound. "Can the insect," he asked, "have acquired its skill gradually from generation to generation, by a long series of casual experiments, of blind gropings? Can such order be born of chaos; such foresight of hazard; such wisdom of stupidity? Is the world subject to the fatalities of evolution, from the first albuminous atom which coagulated into a cell, or is it ruled by an Intelligence? The more I see and the more I observe, the more does this Intelligence shine behind the mystery of things."

In 1861 Charles Naudin presented to the French Academy a paper, with coloured plates, on "Nouvelles Recherches sur l'Hybridité dans les Végétaux."* In it he proceeds to develop a mechanical theory of reproduction of the same general character as pangenesis. In the *Variations of Animals and Plants*,† Darwin states that in his treatment of hybridism in terms of gemmules he is practically following Naudin's treatment of the same theme in terms of "essences." Naudin, however, does not clearly distinguish between hybrid and pure gemmules, and makes the assumption that the hybrid or mixed essences tend constantly to dissociate into pure parental essences, and thus to lead to reversion. It is to this view Darwin refers when he says that Naudin's view throws no light on the reversion to long-lost characters.‡ Mr. Bateson takes occasion to point out that "Naudin clearly enunciated what we shall henceforth know as the Mendelian conception of the dissociation of characters of cross-breds in the formation of germ-cells, though he apparently never developed this conception." § The need of co-ordination in science is manifest when we

* *Nouvelles Archives du Muséum d'Hist. Nat.*, vol. I, p. 25. The second part only appeared in *Ann. Sci. Nat.*, XIX. A review of Naudin is in the *Natural History Review*, 1864, p. ix. George Bentham dealt with hybridism in *Proc. Linn. Soc.*, VIII, 1864, p. 50.

† Vol. II, p. 395 (2nd ed.).

‡ *Ibid.*, II, p. 595.

§ W. Bateson, *Mendel: Principle of Heredity*, p. 38.

say that Darwin never in any way came across Mendel's work. Remarkably enough, the late Mr. Laxton of Stamford was close on the track of Mendelian principle. Mr. Bateson writes that "had he [Laxton] with his other gifts combined that penetration which detects a great principle hidden in the thin mist of 'exceptions,' we should have been able to claim for him that honour which must ever be Mendel's in the history of discovery." Johann Gregor Mendel (1822—1884) carried out in the garden of the monastery of Brunn, in Bohemia, his plant-breeding experiments. Those on peas lasted for eight years, and he took as much care with them as Darwin himself. Then in 1865 he laid the results before the Brunn Society, and published them in 1866. They, however, attracted little attention from the savants and were simply forgotten for thirty-five years, and the neglect embittered the heart of the discoverer. What effect might they not have had on the mind of Darwin had he met with this discovery? *

Though Bronn translated the first German edition of the *Origin,* he thought of evolution as no more than a possibility. "From the first," declares Wilhelm Bölsche, "Darwin—Haeckel was the first to experience it—was branded with the anathemas of the two opposite schools of science in Germany. On the one hand the vigorous and exact workers declared that his teaching was pure metaphysics, because it sought to prove evolution and contemplated vast ideal connections. On the other hand the Dualist metaphysicians denounced him as an empiric of the worst character, who sought to replace the great ideal elements in the world by a few miserable natural necessities. It is significant to find that Schopenhauer, the brilliant thinker, regarded the *Origin of Species* as one of the empirical soapsud or barber books produced by exact investigation, which he thoroughly despised from his metaphysical point of view. And there were already (there are more to-day) whole schools of zoology and botany that looked upon Darwin's theoretical explanations as unscientific mysticism, metaphysics, and philosophy in the worst sense." †

* Yet contrast J. W. Clark and A. C. Seward, *More Letters of C. Darwin,* II, p. 339.

† W. Bölsche, *Haeckel, his Life and Work,* p. 132.

The geologist Otto Volger is not an unfair representative of views like these. Curiously enough, he was the man who preserved from destruction the venerable Goethe-house at Frankfort-on-the-Main, but the spirit of the poet-scientist in no wise rested on him. He declared that Darwinism in general was an unsupported hypothesis, but he made a concession. The species of animals and plants need not be absolutely unchangeable. The only thing that is impossible is a continuous upward direction in evolution. All the groups of living things, even the highest, may have been present together from the earliest days. Local changes in the distribution of land, water, etc., must have brought about a certain amount of variation in life forms. The proper symbol of the story of life is the wave that rises out of the sea and sinks back into it. The real image of human life is the analogy of its obvious development: youth, maturity, then old age and back once more. This conception, he urged, retained the idea of an "eternal becoming," which he deemed better than a rigid fulfilment. If there were a Volger and a Kölliker on the one side, there were the brothers Fritz and Hermann Müller and Rütimeyr on the other. Rudolph Albert von Kölliker attained fame for his researches in anatomy, embryology, and above all histology. In 1845 he demonstrated the continuity between nerve-fibres and nerve-cells of vertebrates. Three years later he isolated the elements of smooth muscle. In 1849 to 1850 he did fine work on the development of the skull and the backbone. He gave marked impetus to the cell theory, and he traced the origin of tissues from the segmenting ovum through the developing embryo. With such wealth of knowledge he faced the problems raised by the *Origin.** In his *Ueber die Darwin'sche Schöpfungstheorie, ein Vortrag* he enumerates and discusses eight objections†:

1. No transitional forms between existing species are known; and known varieties, whether selected or spontaneous, never go so far as to establish new species.

2. No transitional forms of animals are met with among the organic remains of earlier epochs.

* L. Huxley, *Life and Letters of Sir J. D. Hooker*, II, p. 57.

† I use the convenient summary of T. H. Huxley, *Darwiniana*, pp. 82-92.

3. The struggle for existence does not take place.

4. A tendency of organisms to give rise to useful varieties, and a natural selection, do not exist.

5. Pelzeln has also objected that if the later organisms have proceeded from the earlier, the whole developmental series, from the simplest to the highest, could not now exist; in such a case the simpler organisms must have disappeared.

6. Great weight must be attached to the objection brought forward by Huxley, otherwise a warm supporter of Darwin's hypothesis, that we know of no varieties which are sterile with one another, as is the rule among sharply distinguished animal forms.

7. The teleological general conception adopted by Darwin is a mistaken one.

8. The developmental theory of Darwin is not needed to enable us to understand the regular harmonious progress of the complete series of organic forms from the simpler to the more perfect.

Von Baer broke the spell laid by Cuvier on natural science. He broadened the principle of development beyond the limits of morphology and comparative anatomy to which Cuvier was confining it. From 1819 to 1837 he was engaged in the task of demonstrating in thorough fashion the truth of epigenesis. Development was to him the sole basis of zoological classification, bringing the study of living forms back to their origins. He persisted to the end of his long life in minimising the transformation of species, which is "very probable, but only to a limited extent." *

The bulk of the criticisms we have given appeared almost immediately after the publication of the *Origin of Species,* and we do not think that, on the whole, we have given any criticism of later date than the year 1864, within five years of its appearance. They leave on the mind of the candid reader the impression Huxley took the trouble to record in 1887: "There is not the slightest doubt that, if a General Council of the Church scientific had been held at that time [c. 1860], we should have been condemned by an overwhelm-

* K. E. von Baer, *Das Allgemeinste Gesetze der Natur in aller Entwickelung,* I, p. 60. Cf. pp. 37, 39. Cf. his *Ueber Entwickelungsgeschichte der Thiere Beobachtung und Reflexion,* 5th ed.

ing majority."* In 1885 he had written: "It is curious now to remember how largely, at first, the objectors predominated." † The case is strong enough, if we survey the American, French, and German evidence, to hold that any Ecumenical Council of the Church scientific would have reached precisely the same conclusion.

Of course it is not fair to employ against Darwinism developments of the doctrine since 1864. Work of the highest character has been carried out by men like Mendel and Weismann, De Vries and Pavlov of the University of Petrograd, who with others had introduced a new interest in inheritance of acquired characters and therefore a revival in Lamarckism. Take a particular instance. Dr. Patrick Geddes became assistant lecturer at University College, London, and he also became a disciple of Lamarck rather than of Darwin. He used to say that the hypothesis of evolution through natural selection accounted for our survival by explaining the deaths of our uncles and aunts, and that it was consequently rather a theory in necrology than in biology. Within twenty years the divines had become reconciled to the teaching of evolution given in 1859, while the biologists evinced a growing scepticism about some of the Darwinian conclusions.‡

The mutation theory of De Vries explains the origin of species by sudden and saltatory leaps rather than by gradual modification and is received by botanists and rejected by zoologists. The distinguished French palæobotanists C. Grand Eury and R. Zeiller,§ think that the facts of fossil botany lend weight to the view of the sudden appearance of new forms.||

* J. W. Clark and A. C. Seward, *Order of the Proceedings at the Darwin Celebration held at Cambridge, June 22-24, 1909*, p. 20.

† T. H. Huxley, *Darwiniana*, p. 249.

‡ In 1873 Kelvin, when off the coast of Madeira, said: "It has been impossible to keep off Darwinism, and although Madeira gave Darwin some of his most notable and ingenious illustrations and proofs(!), we find at every turn something to show (if anything were needed to show) the utter futility of his philosophy."—S. P. Thompson, *Life of Lord Kelvin*, II, p. 637.

§ R. Zeiller, "Les Végétaux fossiles et leurs Enchaînements," *Revue du Mois*, III, February 1907.

|| The present Lord Rayleigh asked his father in 1906 whether, on the whole, he could accept natural selection as a sufficient explanation of evolution. "Well, no," he said, "I don't think I can quite swallow it." Cf. Rayleigh, *Life of Lord Rayleigh*, p. 45.

Weismann has challenged the evidence that use and disuse have any transmitted effects at all. Expounding the theory of the continuity of the germ-plasm, he has changed the fashion of science, and attention is now directed to the chemico-physical processes of life, and to heredity merely in so far as it throws light upon these processes. Biologists of the standing of Sir E. Ray Lankester and Professor J. Arthur Thomson, of Mr. Francis Galton and Nägeli, regard this theory of the continuity of the germ-plasm as the most striking advance of evolutionary science. On the other hand, Herbert Spencer and Sir William Turner, Hertwig and Haeckel, Gegenbaur and Kölliker, are agreed in rejecting it.

Though this matter is not entirely pertinent to our purpose, we think it deserves a few words more. Darwin, like most of his contemporaries and predecessors, believed that characters stamped by environment on a living creature could be inherited by its descendants. There was indeed difference of opinion as to the extent of such influences and as to their total effect in producing permanent modifications such as might lead to a new species or to new adaptations. Reflective persons understood that the same environmental force, perchance the influence of heat or of light, would produce different effects on different organisms, the "acquired character" being a composite reaction between the inborn capacities of response or resistance to impinging forces and the direct effect of the forces themselves. But there was general acceptance that the characters could be inherited and were inherited.

No reasonable theory existed as to how these characters could be transferred to the reproductive organs and stored in their reproductive cells in such a fashion that when one of the latter grew into a new organism the character acquired by its parent would reappear in the progeny without the presence of external stimulus. Darwin appreciated the difficulty, and offered his theory of pangenesis not so much as a suggestion of what actually did occur as of the kind of machinery required to explain what appeared to be the facts. According to this theory, every part of the body discharged into the blood minute particles stamped with its qualities. These were collected by the reproductive organs, entered the reproductive cells, and thus formed a material link between parent and progeny. Other theories, similar in character, were propounded. But there was no exact evidence for the existence of any of these hypothetical "pangenes"; and increasing knowledge of the cellular details of fertilisation and development increased the difficulty of believing in them.

Then came Weismann's insistence on the distinctness of the body plasm and the reproductive plasm and the doctrine that the reproductive plasm formed a unicellar chain on which the individual lives hung as temporary pendants. His germ plasm rested on so large a body of visible evidence and joined so many hitherto discrete facts in apparent harmony that it gave a new direction to biological theory. Incidentally, it appeared to put out of court the inheritance of acquired characters—a view that was confirmed by careful examination of the rather vague evidence for this process. Darwin was out-Darwined, and the whole burden of evolution was thrown upon the principle of natural selection.

The exclusion of Lamarckian factors from evolution found no favour with many writers, whose chief interest in biology was its application to man, and to whom it seemed incredible that the influences of education and civilisation affected only the generation which experienced them. But it was also distasteful to some competent and experienced naturalists in France, America, and England, who continued to insist that natural selection was insufficient to explain the origin of species. Mr. J. T. Cunningham is one of the most persistent, ingenious, and well-informed of our "neo-Lamarckians." In his remarkably penetrating book on *Hormones and Heredity* he claims to have been one of the first to see the possible bearing of the new physiological conception of "hormones" on the theory of heredity. It seems certain that every organ and tissue of the body liberates chemical messengers into the blood, and that these may have a profound influence on some organ or tissue far removed from their place of manufacture. Hormones are not theoretical substances like pangenes; they are definite chemical bodies which in many cases have been isolated and experimented with. Why, therefore, should not the hormones of an organ or tissue become modified when the organ or tissue acquires a new character at the stimulus of a new environment?

CHAPTER VII

PASTEUR AND MICROBES

Louis Pasteur (1822—1895) came of simple country folk whose family had been for three generations tanners in the district of Dôle in the Jura. The name is to be found in the old registers in the province of Franche-Comté as far back as the early seventeenth century. His great-grandfather was the first freeman in the family, for he bought himself out of serfdom with four gold pieces of twenty-four livres. Claude Etienne Pasteur desired to be freed and succeeded in achieving this at the age of thirty, as is witnessed by a deed, dated March 20, 1673, drawn up in the presence of the Royal notary, Claude Jarry. Messire Philippe-Marie-François, Count of Udressier, Lord of Ecleux, Cramans, Lemuy, and other places, consented "by special grace" to free Claude Etienne Pasteur, a tanner, of Salins, his serf. The deed stipulated that Claude Etienne and his unborn posterity should henceforth be enfranchised from the stain of mortmain.*

Louis Pasteur's father, Jean Joseph, as a young man had been one of Napoleon's conscripts and had won the Cross of the Legion of Honour on the field of battle, for valour and fidelity. Jean Joseph attained the rank of sergeant in the 3rd Regiment, called "the brave among the brave." Though carefully brought up, he was without much learning. To be able to read the Emperor's bulletins in those days was considered ample for a man in his position in life. In later years he painted on an inner door of his house a soldier in an old uniform pausing in his digging to lean on his spade and dream of past glories. For him, as for so many other

* There is an admirable biography of Pasteur by René Vallery-Radot, and I feel much indebted to it. There is an excellent translation of it by Mrs. Downshire. M. Vallery-Radot is Pasteur's son-in-law.

Frenchmen, Napoleon had been a demi-god. On the resumption of his work in the tannery after the conclusion of peace in 1815, he came to know a family of gardeners. His tannery stood on the bank of the river Furieuse, and from the steps leading to the water he used to watch a young girl working in the garden at early dawn. She soon perceived that the old soldier of twenty-five was interested in her every movement. Her name was Jeanne Etiennette Roqui, a native of Marnot, a village about four kilometres from Salins. Like the Pasteurs, the Roquis came of old yet humble stock and of such warm affections that "to love like the Roqui" was a local saying. In 1815 they married, and on December 27, 1822, their son Louis was born. He at first attended the Ecole Primaire, attached to the college of Arbois. He in no wise distinguished himself, belonging merely to the category of good average pupils. He liked drawing and he liked fishing, but he did not extend his liking to lessons. His patriotism was kindled by such stories of local patriotism as the siege of Arbois under Henry IV, when the Arboisians held out for three whole days against a besieging army of 25,000. Though his father's language and manners were retiring, the lad felt impressed by his regular walk on Sundays. Then the sergeant, wearing a military-looking frock coat, spotlessly clean and adorned with the showy ribbon of the Legion of Honour—it was then worn very large—invariably walked out towards the road from Arbois to Besançon. Patriotism spelt duty for the father and it came to spell the same for the son.

The headmaster of Arbois college, M. Romanet, was the first to discover that hidden behind the face of his pupil there was genuine intelligence. The mind of the lad worked but slowly, for he could never bring himself to affirm anything of which he was not absolutely certain. Romanet, during their strolls around the college playground, tried to awaken the leading qualities of the boy's nature. He succeeded in firing him with the desire of going to the Ecole Normale, there to prepare himself to become a "professor," as schoolmasters are called in France. This Ecole Normale Supérieure was a training college, and candidates for it had to be between the ages of eighteen and twenty-one and be already Bachelors of Letters or of Science.

At the end of October 1838 he accompanied his dear school friend, Jules Vercel, to Paris to work for his "baccalauréat." The wrench of leaving home and his loved Jura proved too much for the boy of sixteen. When he arrived in Paris he was far from sympathising with Balzac's student hero, confidently defying the great city. The nostalgia so persisted that he avowed to Jules Vercel, "If I could only get a whiff of the tannery yard, I feel I should be cured." He was not, however, cured, and his father came after a month's struggle to take him home. To the Arbois college he returned, and on recovering from his home-sickness he settled down to read, grasping the fact that his education imposed a stiff charge on the family funds. As there was no "philosophy" class at the college at Arbois, he made up his mind to go to the college at Besançon—only twenty-five miles from home—where he could continue his studies, pass his "baccalauréat," and then prepare for his examinations of the Ecole Normale.

Besançon owned the Royal College of Franche-Comté, and on his arrival there Pasteur found that the science master, M. Darlay, was nothing like so good as the philosophy master, M. Daunas. As Pasteur grew interested in his science work, he asked questions that proved embarrassing to M. Darlay. He disapproved of saying, "I don't know," and used to try to keep his pupil in his place by telling him that questions were to be asked by the teacher, not by the scholar. On August 29, 1840, he took the degree of "bachelier ès lettres" with no particular brilliancy. The three examiners, doctors "ès lettres," put down his answers as "good in Greek on Plutarch and in Latin on Virgil, good also in rhetoric, medicine, history, and geography, good in philosophy, very good in elementary science, good in French composition." *

The character more than the degree of the young bachelor had impressed the college authorities, for at the end of the summer holidays the headmaster of the Royal College, M. Répécaud, offered him the post of preparation master, and the offer was gratefully accepted. For this work, as he boasted proudly to his parents, he received beside his board and lodging 300 francs a year. To his sisters at home he

* R. Vallery-Radot, *Life of Pasteur,* p. 14.

wrote: "Let me tell you again, work hard, love each other. When one is accustomed to work it is impossible to do without it: beside, everything else in this world depends on that. Armed with science, one can rise above all one's fellows." * At another time he wants to pay for the education of his little sisters, saying that he can easily do it by giving private lessons. This he had already been asked to do at the rate of 20 to 25 francs a month. His parents sensibly would not listen to his making this sacrifice, but wanted instead to give him a small allowance for extra coaching for himself.

If the thoughts of youth are long, long thoughts, the friends of youth are long, long friends. Michelet, in his recollections, tells of the hours of intimacy he enjoyed with a college friend named Poinsat, and thus expresses himself: "It was an immense, an insatiable longing for confidences, for mutual revelations." What Mutianus meant to his circle of admirers, what Melanchthon meant to Camerarius, what Montaigne meant to La Boëtie, what Goethe meant to Schiller, what Blücher meant to Gneisenau, Pasteur meant to Charles Chappuis, a fellow-student of the Besançon college. Save sympathy in scientific taste, everything else that friendship can give in generous strength and in brotherly confidences, everything that, according to Montaigne—who knew more about it than even Michelet, in spite of his ardent friendship for Quinet—"makes souls merge into each other so that the seam which originally joined them disappears," Pasteur and Chappuis felt for each other. Of all the gifts that college life affords, this gift of ardent interchange of ideas is the most valuable. To treat the masters of literature with or without the deference that is justly theirs, to elevate a minor poet of one's own discovery to their rank, to criticise all and sundry to one's heart's content—what joys of after-days can rival these? Does any man, to the end of the longest life, ever forget that proud and happy day when he first met his other self at college? There have been later successes—other first days, memorable in their way. The first day to make a scientific discovery,† the first on the Bench, the first speech in the House, the first command in

* R. Vallery-Radot, *Life of Pasteur,* p. 14.

† Pasteur's first scientific joy was to extract sixty grammes of phosphorus from bones. Cf. R. Vallery-Radot, *Life of Pasteur,* p. 31.

the field—but the *couleur de rose* had paled by that time. There is no colouring so bright as the long-faded colours of those wonderful early days! Who does not look back on them, realising only when they are long past how happy they were? One is very much wiser now, and richer, and, maybe, has reached the highest round of the ladder; but who does not remember the daisies that grew around the foot when one was climbing that lowest rung! Ah, but the old days were the best! So Pasteur and Chappuis believed, and, at any rate, so they experienced. With Chappuis he exchanged his thoughts and his ideals, and together they mapped out, with the happy confidence of youth, a life together. When Chappuis set out for Paris, the better to prepare himself for the Ecole Normale, Pasteur longed to set out with him. Chappuis wrote to him with that open spontaneity which forms such a charm of youth, "I shall feel as if I had all my Franche-Comté with me when you are here." Fearing a crisis like that of 1838, Pasteur's father, after some hesitation, refused his consent to an immediate departure. "Next year," he said.

Though master at the Royal College of Besançon, Pasteur never ceased to be a student, even to the last day he lived. In October 1841 he resumed his attendance of the classes for special mathematics. But he was constantly thinking of Paris, "where study is deeper," and where Chappuis was. "If I do not pass this year," he wrote to his father on November 7, "I think I should do well to go to Paris for a year. But there is time to think of that and of the means of doing so without spending too much, if the occasion should arise. I see now what great advantage there is in giving two years to mathematics; everything becomes clearer and easier. Of all our class students who tried this year for the Ecole Polytechnique and the Ecole Normale, not a single one has passed, not even the best of them, a student who had already done one year's mathematics at Lyons." *

Dry and exhausting as the young student found mathematics, he persevered with his studies. In spite of his application he passed even less brilliantly his examination, before the Dijon faculty on August 13, 1842, for the baccalauréat ès sciences than he had passed the baccalauréat ès lettres. In

* R. Vallery-Radot, *Life of Pasteur*, p. 19.

chemistry the examiners reported that he was simply "médiocre." Nor is there any reason to think that this report was not a correct one. For on August 26 when he entered the examination for admission to the Ecole Normale, he was only fifteenth out of twenty-two candidates. This place he considered too low, and he resolved to try again the following year. In order to redeem himself, he determined at last to set out for Paris, and this time his father consented. In company with Chappuis in October 1842 he arrived at the Barbet Boarding School, and the arrangement was that he only paid one-third of the pupil's fees, and in return he had to give the younger pupils some instruction in mathematics every morning from six to seven. "Do not be anxious about my health and work," he wrote to his friends a few days after his arrival, "I need hardly get up till 5.45; you see it is not so very early. . . . I shall spend my Thursdays in a neighbouring library with Chappuis, who has four hours to himself on that day. On Sundays we shall walk and work a little together; we hope to do some philosophy on Sundays, perhaps too on Thursdays; I shall also read some literary works. Surely you must see that I am not homesick this time." * Of all his new acquaintances there was none like Chappuis, who believed in those far-off days that his friend was bound to make his mark. "You will see what Pasteur will be," so he used to say and so he always maintained.

There were fine lecturers at the Ecole Normale and at the Sorbonne, and among the finest was Balard, the discoverer of bromine, and J. B. Dumas, a man with magnetic powers of attraction exercised over his students. Balard and Dumas had both begun life as pupils of an apothecary, and Dumas was wont to say in his grand manner, "Balard and I were initiated into our scientific life under the same condition." At the Sorbonne Dumas commanded an audience of six to seven hundred people, and there was a great deal of applause at his lectures. Dumas's allusions to science in other departments as well as his own chemical one seemed to open the doors to Pasteur into all sorts of roomy and spacious laboratories. It used to seem to him that Dumas habitually lived in a world that was bigger, brighter, and more entertaining

* R. Vallery-Radot, *Life of Pasteur*, p. 21.

than the ordinary world. Dumas was able to bring, like the wise householder, out of his chemical and other treasures things old and new; and Pasteur came to feel that he would like to have similar treasures in the background too.

Inspired by such really great chemists as Balard and Dumas, Pasteur began to do better even in his examinations. At the end of 1843 he took at the Lycée St. Louis two accessits and one first prize in physics, and at the Concours Général, an open competition held every year at the Sorbonne between the élite of the students of all the colleges in France, he won a sixth accessit in physics. At last he was admitted fourth on the list to the Ecole Normale. As a small token of his gratitude for past kindness, he offered to M. Barbet to give some lessons at the school of the Impasse des Feuillantines. "There is nothing more easy," Pasteur thought with that simpleness of character that invariably distinguished him, "than to come regularly at six o'clock on Thursdays and give the schoolboys a physical science class." * "I am very pleased," wrote his father, "that you are giving lessons at M. Barbet's. He has been so kind to us that I was anxious that you should show some gratitude; be therefore always most obliging towards him. You should do so, not only for your own sake, but for others'; it will encourage him to show the same kindness to other studious young men, whose future might depend upon it." * The only matter that worried the father was that his son would work so immoderately. Writing to Chappuis, Joseph Pasteur begged him: "Do tell Louis not to work so much; it is not good to strain one's brain. That is not the way to succeed but to compromise one's health."* In another letter of December 1843, to his son this time, he writes: "Tell Chappuis that I have bottled some 1834 bought on purpose to drink the health of the Ecole Normale during the next holidays. There is more within those 100 litres than in all the books on philosophy in the world; but as to mathematical formulæ, there are none, I believe. Mind you tell him that we shall drink the first bottle with him. Remain two good friends." * That Pasteur had a genius for friendship is obvious, for he owned the patience and the powers of trust such a genius implies. The letters between father and son reveal full con-

* R. Vallery-Radot, *Life of Pasteur,* p. 23.

fidence between them, so that Louis's own judgment was fostered, and his humour had free play. If it is a healthy home where the young and the old share the same joke, it is a no less healthy home where the old welcome the friends of the young, and such a home was Pasteur's.

Inspired by his teachers, Pasteur felt this impulse as he perused the biographies of either great scientists or great patriots, for few have loved "la patrie" as he loved it. There was always the impulse to research to be derived from men like Balard and Dumas. Was there a better way of spending a holiday than to be shut up the whole afternoon at the Sorbonne laboratory? Chappuis half-loved and half-feared this ardour of mind. Anxious to obey the injunctions of his friend's father, "Do not let him work too much!" he used to wait patiently—if not philosophically—sitting on a laboratory stool, until the experiments were finished for the time being. Conquered by the reproachful silence no less than by his patient attitude, Pasteur would take off his apron, saying half-angrily yet half-gratefully, "Well, let us go for a walk!" Just as nothing else thrives when a man is absorbed in a piece of work, so the conversation between the two languished when it turned in the direction of philosophy. But when it turned in the direction of science—that was a totally different matter!

When Sir James Paget was only nineteen he discovered the *Trichina spiralis*.* Dr. Cobbold has told the story of the several steps leading to the discovery and following it, in his work on the Entozoa. Paget's share was the detection of the "worm" in its capsule; and he justly ascribes it to the habit of looking-out, and observing, and wishing to find new things, powers he had acquired in his previous study of botany. All the men in the dissecting-rooms, teachers included, "saw" the little specks in the muscles: but Paget alone "looked at" them and "observed" them. He notes that no one trained in natural history could have failed to do so, but all up to his time had so failed. When Thomas Henry Huxley was also only nineteen he discovered a hitherto undiscovered membrane in the root of the human

* S. Paget, *Memoirs and Letters of Sir J. Paget*, p. 55. May I commend this extremely fine book? It is one of the most enjoyable biographies I know.

hair, which received the name of Huxley's layer, and this was the only discovery Huxley ever succeeded in making.* Not long after these two discoveries Pasteur was conducting investigations that were to lead to his first discovery. The first observations on the fact that for every compound which possesses the power of turning the plane of polarisation to the right, there is another which, while possessing the same composition, rotates equally to the left was beginning to occur to him while he was working in the wretchedly-equipped laboratory of the Sorbonne.

There are two saline combinations, tartrate and paratartrate of soda and ammonia, and in these two substances of similar crystalline form, the nature and number of the atoms and their distances are the same. The problem was to ascertain why dissolved tartrate rotates the plane of polarised light and paratartrate remains inactive. Mitscherlich and Biot had been puzzled by this curious difference, and it was now the turn of Pasteur to be also puzzled. Chappuis was then absorbed in the series of lectures on philosophy given by Jules Simon, but he was plainly affected when he saw his friend so upset by the optical inactivity of paratartrate. In the meantime Pasteur was writing his thesis for his doctorate on the applications of crystallography and physics to chemical problems. While labouring at this thesis, he was most anxious to turn aside to the behaviour of paratartrate. The father here at least proved wiser than the son. "Before being captain," thought the old sergeant-major, "you must become lieutenant." Accordingly Louis returned to his thesis on "The Phenomena relative to the Rotary Polarisation of Liquids," and duly won his doctorate in 1847.

At the end of 1846 a newcomer entered Balard's laboratory, which was as poorly equipped as that at the Sorbonne, and this man was Auguste Laurent, poet as well as scientist. Laurent asked Pasteur to assist him with his experiments, and Pasteur was so delighted with this proposed collaboration that he wrote at once to Chappuis to tell him of it. Though Laurent went off to the Sorbonne to become the assistant of Dumas, Pasteur continued his researches. On March 20, 1848, he read before the Académie des Sciences a portion of his paper on "Researches on Dimorphism." Some sub-

* L. Huxley, *Life and Letters of T. H. Huxley,* I, p. 21.

stances crystallise in two different ways. Sulphur, for instance, gives quite dissimilar crystals according as it is melted in a crucible or dissolved in sulphide of carbon, and a substance like it is termed dimorphous.

In the midst of his labours the Revolution of 1848 broke out, as European a Revolution as the memorable one of 1789. *La patrie* always moved the inmost fibre of his very being. On the spot he enrolled with his fellow-students. "What a transformation of our whole being," has written one who was then a candidate to the Ecole Normale, already noted by his masters for his sound sense, Francisque Sarcey. "How those magical words of liberty and fraternity, this renewal of the Republic, born in the sunshine of our twentieth year, filled our hearts with unknown and absolutely delicious sensations! With what a gallant joy we embraced the sweet and superb image of a people of free men and brethren! The whole nation was moved as we were; like us, it had drunk of the intoxicating cup. The honey of eloquence flowed unceasingly from the lips of a great poet, and France believed, in childlike faith, that his word was efficacious to destroy abuses, cure evils, and soothe sorrows." One day when Pasteur was crossing the Place du Panthéon he saw a gathering crowd around a wooden erection, inscribed with the magic words: "Autel de la Patrie." With more patriotism than prudence, he hurried back to the Ecole Normale and emptied all his hard-won savings into the Autel. "You say," wrote his father on April 28, 1848, "that you have offered to France all your savings, amounting to 150 francs. You have probably kept a receipt of the office where this payment was made, with mention of the date and place?" And considering that this action should be made known, he advised him to publish it in the journal *Le National* or *La Réforme* in the following terms: "Gift to the Patrie: 150 francs, by the son of an old soldier of the Empire, Louis Pasteur of the Ecole Normale."

From the days of 1848 for the moment we gaze ahead to the disastrous ones of 1871, and we experience no difficulty in grasping the emotions of Pasteur when he saw the terms the Germans forced on his beloved land in 1871. Men who feel inclined to commiserate the Germans in their plight in the Treaty of Versailles of 1919 are invited to turn their attention to the Treaty of 1871. The feelings that inspired Napoleon when he understood the work of Jenner were not the feelings of Bismarck when he understood the work of Pasteur—if ever he understood it. Just as the German artillery battered down the Cathedral of Rheims, so the German artillery fired on the Panthéon and other non-warlike buildings in Paris.

On January 9, 1871, Chevreul read the following declaration to the Académie des Sciences:

The Garden of Medicinal Plants, founded in Paris
by an edict of King Louis XIII,
dated January 1826,
Converted into the Museum of Natural History
by a decree of the Convention on June 10, 1793,
was Bombarded,
under the reign of Wilhelm I, King of
Prussia, Count von Bismarck, Chancellor,
by the Prussian Army, during the night
of January 8-9, 1871.
It had until then been respected by all parties
and all powers, national or
foreign.

When Pasteur read this protest, his regret that he had not been present to sign it was poignant. In 1868 the University of Bonn had conferred upon him its honorary diploma of Doctor of Medicine, acknowledging that "by his very penetrating experiments, he had much contributed to the knowledge of the history of the generation of micro-organisms, and had happily advanced the progress of the science of fermentation." Naturally Pasteur had been proud to receive this diploma.

"Now," he wrote on January 18, 1871, to the Head of the Faculty of Medicine, after recalling his former feelings of pride, "now the sight of that parchment is odious to me, and I feel offended at seeing my name, with the qualification of Virum clarissimum that you have given it, placed under a name which is henceforth an object of execration to my country, that of Rex Gulielmus.

"While highly asseverating my profound respect for you, Sir, and for the celebrated professors who have affixed their signatures to the decision of the members of your Order, I am called upon by my conscience to ask you to efface my name from the archives of your Faculty, and to take back that diploma, as a sign of the indignation inspired in a French scientist by the barbarity and hypocrisy of him who, in order to satisfy his criminal pride, persists in the massacre of two great nations." Pasteur's protest ended with these words: "Written at Arbois (Jura) on January 18, 1871, after reading the mark of infamy inscribed on the forehead of your King by the illustrious director of the Museum of Natural History, M. Chevreul."

"This letter," thought its writer, "will not have much weight with a people whose principles differ so totally from those which inspire us, but it will at least echo the indignation of the French scientists." *

That the great scientist is also a great artist we hold to be indubitable truth. Man has a heart as well as a head. Like many another great man, Pasteur thought through his feelings as well as through his brain. "La cœur a ses raisons," wrote Blaise Pascal in a pregnant saying, "que la raison ne connoît pas," † and the saying is eminently true of Pasteur. How deeply the calamities of his country stirred him is clear in his letter of September 17, 1870, to his pupil Raulin: "What folly, what blindness, there are in the inertia of Austria, Russia, England! What ignorance in our army leaders of the respective forces of the two nations! The real cause of our misfortunes lies there. It is not with impunity—as it will one day be recognised, too late—that a great nation is allowed to lose its intellectual standard. But, as you say, if we rise again from these disasters, we shall again see our statesmen lose themselves in endless discussions on forms of government and abstract political questions, instead of going to the root of the matter. We are paying the penalty of fifty years' forgetfulness of science, of its conditions of development, of its immense influence on the destiny of a great people, and of all that might have assisted the diffusion of light. . . . I cannot go on, all this hurts me. I try to put away all such memories, and also the sight of our terrible distress, in which it seems that a desperate resistance is the only hope we have left. I wish that France may fight to her last man, to her last fortress. I wish that the war may be prolonged until the winter, when, the elements

* R. Vallery-Radot, *Life of Pasteur*, p. 190.
† *Pensées*, p. 32 (1829 ed.).

aiding us, all these Vandals may perish of cold and distress. Every one of my future works will bear on its title-page the words: 'Hatred to Prussia. Revenge! Revenge!'" *

Unfortunately Pasteur did not live to see the glories won by his countrymen in the World War of 1914—1918. The bitter memories of 1870-1 were to be washed in the waters of Lethe by the experiences of 1914-18. We, however, return to the Revolution of 1848. After its days of national exultation, Pasteur turned once more to his crystals. Influenced by certain ideas, he considered that some objects, placed before a mirror, give an image which can be superposed to them. These, like a chair, possess a symmetrical plan. Other objects, placed before a mirror, give an image which cannot be superposed to them. These, like a spiral staircase, possess a dissymmetrical plan. If it turns to the right, its image turns to the left. Pasteur noticed that the crystals of tartaric acid and the tartrates had little faces, a matter that had escaped Mitscherlich. These faces, which only existed on one half of the edges or similar angles, constituted a hemihedral form. When the crystal was placed before a mirror the image that appeared could not be superposed to the crystal; the comparison of the spiral staircase was applicable to it. Pasteur proceeded to think that this aspect of the crystal might be an index of what existed within the molecules, dissymmetry of form corresponding with molecular dissymmetry. Clearly the deviation to the right of the plane of polarisation produced by tartrate and the optical neutrality of paratartrates could be explained by a structural law. All the crystals of tartrate proved to be hemihedral.

The next stage in the experiment was to examine the crystals of paratartrate. They, so he reasoned, could not be hemihedral. As a matter of fact, they were. Keenly disappointed, he cast about for a fresh explanation of the new difficulty, and he at last found it. Were not some of the faces of the crystal inclined to the right and others to the left? Here was food for thought. It then occurred to him to take up these crystals one by one and sort them carefully, putting on the one side those which turned to the left, and on the other those

* R. Vallery-Radot, *Life of Pasteur*, p. 183.

which turned to the right. He thought that by observing their respective solutions in the polarising apparatus, the two contrary hemihedral forms would give two contrary deviations. Then, by mixing together an equal number of each kind, the resulting solution would produce no action upon light, the two equal and directly opposite deviations exactly neutralising each other.

With beating heart he proceeded to carry out his experiment with the polarising apparatus and exclaimed with fervour, "I have it!" In his excitement he rushed out of his laboratory, not unlike Archimedes, embracing the first man he met in the corridor, the curator, Bertrand, as he would have embraced Chappuis. He dragged the puzzled curator out to the Luxembourg garden to explain his discovery. He had found out the relations between the crystalline forms of the several tartaric acids, and their action on polarised light led him to perceive the necessity of some kind of theory to account for the internal structure of the molecules of such compounds. If the atoms composing the molecule in one of such a pair of compounds be conceived as arranged in a particular order, then the atoms in the other must be arranged in the same order but inversely, so that if the atoms could be made visible they would be seen to exhibit the relation of an object to its image in a mirror. Twenty years after Pasteur's discovery in 1848 the subject again attracted attention, and after the study of lactic acids by Wislicenus, a theory was put forward, by the Dutch chemist van't Hoff and the French chemist Le Bel, which provided the necessary clue, and provided the basis for that large department of chemistry we to-day know as stereo-chemistry, or chemistry in space.

The mystery of the inactivity of paratartrates was a mystery no more. "How often," he wrote to Chappuis on May 5, 1848, as he thought of the difficulty of explaining his discovery, "how often have I regretted that we did not both take up the same study, that of physical science. We who so often talked of the future, we did not understand. What splendid work we could have undertaken and would be undertaking now; and what could we not have done united by the same ideas, the same love of science, the same ambition. I would we were twenty and with the three years of the Ecole before us." Yes, the old days were the happy days, even if "we who so often talked of the future, we did not understand." J. B. Biot was seventy-four when he heard of this discovery, affording mankind a first glimpse of molecular construction. During thirty years he had investigated the phenomena of rotatory polarisation, and his satisfaction was deep when he verified the results of a young man of twenty-five.

Mankind remembers Pasteur largely for his memorable labours on micro-organisms, and it might seem at first sight as if the inactivity of paratartrate was far removed from it. Yet his researches on this very matter were logically as well as actually connected with his practical researches on fermentation. He had examined racemic or paratartaric acid, which resembles tartaric acid in chemical composition, but has a different crystalline form and is as optically inactive as paratartrate. One hot summer day he noted in his laboratory that a tartrate solution had begun to ferment. Instead of throwing it away he examined it in the spirit of Faraday, who, when asked how he made so many discoveries, answered, "By always inspecting the refuse of my experiments." Similarly, Pasteur inspected his refuse, and it suggested to him the question, Would fermentation exercise any effect on racemic acid? To solve this problem, he set up fermentation in a racemate, ascertaining that the inactive liquid gradually became as a result optically active. Fermentation, in fact, separated the two active constituents, destroying the one and leaving the other. This discovery, in turn, led on to his wonderful work of fermentation. The study of one form of asymmetrical molecules led to the examination of another form of such molecules. He had found that the rotary power of a body disappeared when that body was chemically broken up, and that life alone seemed to be capable of producing new asymmetrical molecules. Step by step he had found experimental verification for the new ideas which his work on crystals had introduced into fermentation. He had reached the empirical construction of a solution containing only mineral substance and ammonia, in which yeast would grow and would begin to set up alcoholic fermentation. Fermentation was therefore due to the action of a living organism. We set out with racemic or paratartaric acid, and we end—or do we end?—with the epoch-making experiments on micro-organisms. At first sight the connection is remote, but of its reality no one can doubt. It is a tale that has been repeated again and again in the annals of science. What is the connection between the twitching legs of Galvani's frog and the flicking needle of the telegraph? What is the connection between Oersted in 1822 deflecting a magnetised needle and the electric telegraph? What is the connection between the walks of William Smith, the father of geology, and the finding of our mineral resources? What is the connection between the highly abstract work of Lagrange in mathematics and wireless telegraphy? What is the connection between Lord Kelvin's stiff piece of mathematical analysis, published in 1853, and the study of electric oscillations that led to the invention of wireless telegraphy? What is the connection between the discovery of Sir William Crookes in 1892 that a strong electric current produced nitrous and nitric acids and the fixation of nitrogen which enabled Germany to prolong the War? What is the connection between the dyes of Perkin and the industrial predominance of Germany before and since the War? What is the connection between the knowledge gained by the zoologists when they counted the hairs on the backs of flies and quarrelled over the specific distinctions between one gnat and another and the opening up of tropical Africa or the completion of the Panama Canal? The truth is, as Pasteur was to demonstrate in a hundred different ways, no generalisation of science is really remote from any other generalisation. If it appears to be remote, to-day or to-morrow may give birth to the wide conception that will unite such generalisations.

At the end of the summer of 1848 Pasteur was appointed Professor of Physics at the Dijon Lycée. The Minister of Public Instruction, at the request of Biot, consented to allow him to postpone his departure in order to let him finish some work. His appointment came as a real blow to Biot. "If at least," he indignantly remarked, "they were sending you to a Faculty!" He turned his wrath on to the Government officials. "They don't seem to realise that such labours stand above everything else. If they only knew it, two or three such treatises might bring a man straight to the Institut!" *

* R. Vallery-Radot, *Life of Pasteur*, p. 43.

In spite of this outburst, Pasteur had to go. He wrote to Chappuis on November 20, 1848: "I find that preparing my lessons takes up a great deal of time. It is only when I have prepared a lecture very carefully that I succeed in making it very clear and capable of compelling attention. If I neglect it at all I lecture badly and become unintelligible." * His two classes of first- and second-year pupils engrossed all his time and all his strength, and the outcome was that he could not possibly pursue his favourite studies. Biot appealed to Baron Thenard, who was Chairman of the Grand Council of the University, and in 1849 Pasteur became Professor of Chemistry at Strasburg. The new Professor met the Rector of the Academy of Strasburg, M. Laurent, and within a fortnight of meeting his daughter Marie he proposed marriage to her. He was so deeply in love with her that he forsook his laboratory—"I," he remorsefully adds, "who did so love my crystals." "I believe," he confided in Chappuis, "that I shall be very happy. Every quality I could wish for in a wife I find in her. You will say, 'He is in love!' Yes, but I do not think I exaggerate at all, and my sister Josephine quite agrees with me."† The union of the two lovers proved ideally happy, though there is a story that on the wedding day the bridegroom was so wrapped up in his experiments that he entirely forgot the ceremony and had to be fetched by a friend. "Why are you not a professor of physics or chemistry?" he asked Chappuis. "We should work together, and in ten years' time we should revolutionise chemistry. There are wonders hidden in crystallisation, and, through it, the inmost construction of substances will one day be revealed. If you come to Strasburg, you *shall* become a chemist; I shall talk to you of nothing but crystals." ‡ Was Pasteur wiser than even he knew? For behind his solution of the mystery of racemic acid lay the first real glimpse of the construction of the molecule. By dint of amazing trouble he succeeded in transforming tartaric acid into racemic acid. He found that one of the salts of racemic acid, paratartrate or racemate of ammonia, for instance, in the ordinary conditions of fer-

* R. Vallery-Radot, *Life of Pasteur*, p. 51.
† *Ibid.*, p. 44.
‡ *Ibid*, p. 54.

mentation, the dextro-tartaric acid alone ferments, the other remaining in the liquor. Why does the dextro-tartaric acid alone become putrefied? His answer was that the ferments of that fermentation feed more easily on the right than on the left molecules. By a wonderful coincidence, just at the very moment when his studies were bringing him more and more to the problem of fermentation he was appointed Professor and Dean of the new Faculty of Science at Lille, the country of distilleries, in September 1854. Here he opened a new chapter in the annals of science.

In the summer of 1856 a Lille manufacturer, M. Bigo, came to the young Dean for advice. Bigo had met with grave disappointment in the manufacture of beetroot alcohol. On his return to his primitive laboratory, Pasteur examined the globules in the fermentation juice, compared filtered with unfiltered beetroot juice, and conceived hypothesis after hypothesis to explain the puzzling phenomena. A long line of chemists had been trying to solve the problems of fermentation and putrefaction. Lavoisier, Fabroni, Thenard, Gay-Lussac, Cagniard-Latour, Dumas, Berzelius, Schwann, Liebig, Helmholtz—all had given time and thought to these perplexing problems. It was now the turn of Pasteur to give them his best attention.

Men of the school of Liebig and Berzelius rejected the idea of an influence of life in the cause of fermentations. In them Pasteur began to perceive phenomenon correlative to life.* In lactic yeast he discerned the budding, multiplying and offering the same phenomena of reproduction as beer yeast. He showed that the yeast plant assumes different stages in the fermentation of wine, thereby demolishing the teaching of Liebig which dominated the scientific world. He recognised for the first time the presence of a micro-organism in connection with the process of lactic acid fermentation. He showed that fermentation could be set up in vinegar by the addition of minute cultivations of the special organism connected with each particular process. He prepared a fluid consisting of a solution of sugar to which only mineral substances had been added, in which he could produce at will either the alcoholic or the lactic fermentation by inoculating

* Here and elsewhere I am deeply indebted to chap. xii of Sir Rickman Godlee's fascinating biography of Lord Lister.

it with the appropriate organism. Liebig persisted in regarding the processes of decay, decomposition, and fermentation as purely physico-chemical in character. "Those who," he held, "attempt to explain the putrefaction of animal substances by the presence of animalcules, argue much in the same way as a child who imagines that he can explain the rapidity of the Rhine's flowing by attributing it to the violent agitation caused by the numerous water-wheels at Mayence."

To Pasteur it seemed increasingly, on the other hand, that the living animalcules caused the phenomena he was rigidly investigating. Experiment after experiment confirmed the view of the French scientist. For a generation the old school fought against the new. That the new could show experiments that demonstrated the truth of their view mattered nothing. The old school persisted in holding that the chemical actions taking place during fermentation could only be explained in terms of molecular physics. Pasteur equally persisted in experimenting, and his experiments taught him that fermentation, putrefaction, decomposition, all are "acts of life, and in the absence of life do not take place." "A liquid really sterile, exposed to air really sterile, will remain sterile for ever," and in that condition will neither ferment nor putrefy. The truth is what Pasteur concisely stated: "La vie c'est le germe et le germe c'est la vie."

This was by no means all. He had proved that the minute living things called animalcules caused it to break up into simpler compounds. He proceeded to demonstrate that certain organisms grow, not in the presence of air, but in its absence. Such is the case with the organism which he described as associated with the butyric fermentation. He afterwards extended these observations and demonstrated that the butyric ferment is not an isolated example, but that there is a whole class of organisms which, though they cannot do without oxygen, are unable to flourish in the presence of free oxygen. They obtain their oxygen from the compounds of little stability which they decompose. This observation naturally led the investigator on to the study of putrefaction and to the development of a new theory of fermentation and decay. He determined the point that putrefaction does not occur independently of the agency of micro-organisms. As life was thought to be absolutely dependent on air for its maintenance, Pasteur's discovery of the possibility of anaerobic life, which grows by the absence of air, was received with incredulity. Belief in the existence of the anaerobes aroused a storm of criticism and opposition on the part of Pasteur's contemporaries.

The reply Pasteur made to the unbelief of scientists was to concentrate on his experiments. In December 1857 he recognised complex phenomena in alcoholic fermentation. The chemist had been content to say, So much sugar gives so much alcohol and so much carbonic acid. He wrote to the ever-loyal Chappuis in June 1858: "I find that alcoholic fermentation is constantly accompanied by the production of glycerine; it is a very curious fact. For instance, in one litre of wine there are several grammes of that product which had not been suspected." * He also recognised the normal presence of succinic acid in alcoholic fermentation. "I should be pursuing the consequences of these facts," he adds, "if a temperature of 36° C. did not keep me from my laboratory. I regret to see the longest days in the year lost to me. Yet I have grown accustomed to my attic [i.e. his poor laboratory], and I should be sorry to leave it. Next holidays I hope to enlarge it. You too are struggling against material hindrances in your work; let it stimulate us, my dear fellow, and not discourage us. Our discoveries will have the greater merit."

The link between many kinds of phenomena was the organism detected. Organisms produced fermentation and putrefaction. Organisms are carried on particles of dust floating in the atmosphere. These particles of dust can be destroyed by heat, or filtered off by cotton wool, or intercepted in the finely-drawn-out or tortuous necks of flasks, through which the free ingress and egress of air takes place owing to the diurnal variations in temperature. Conclusions of this nature shaped themselves in Pasteur's mind with that logical precision in which he delighted.

Particles of dust carried the organisms in which he was interested. But these particles of dust differed in the degree of their abundance. They were as conspicuously present in a dusty room as they were conspicuously absent in an undisturbed cellar or on a mountain top. Was air the cause

* R. Vallery-Radot, *Life of Pasteur*, p. 85.

of putrefaction? Was it not rather due to the presence of filtrable dust? Was not, in fact, the atmosphere the sole vehicle of all the harm done? This, of course, is not the case, but there was enough in it to set Pasteur thinking. It in no wise escaped him that germs not only people the air but are carried by it to all solid and liquid substances, and therefore will be found adherent to the hands of the experimenter, to the insides of bottles, to corks, and even to such unlikely materials as mercury, through which some were in the habit of passing their sterilised putrescible fluids and purified air. Along with all this the investigator proceeded to show that certain natural substances, such as blood and urine, are free from micro-organisms, and can be kept from decomposing for an indefinite length of time if received with proper precautions into previously sterilised vessels.

In fact, by varying the solution and the conditions of growth, Pasteur reached the conception of pure cultures, cultures in which only one kind of organism throve and one kind of fermentation took place. So he paved the way for the exact methods which turned brewing and the making of wines and vinegars into scientific industries. Above all, at the same time he rendered possible the vast progress which was to come in the transference of the new theories and methods of disease. For the preparation of culture media, the growth of pure cultures, and the possibility of associating particular organisms with particular diseases are the foundations of bacteriology.

As Darwin found that under his hands all questions widened, so Pasteur under his found exactly the same. On February 7, 1860, he wrote to his father*: "You know I have always told you confidentially that time would see the growth of my researches on the molecular dissymmetry of natural organic products. Founded as they were on varied notions borrowed from divers branches of science—crystallography, physics, and chemistry—those studies could not be followed by most scientists so as to be fully understood. On this occasion I presented them in the aggregate with some clearness and power and every one was struck with their importance.

"It is not by their form that these two lectures have de-

* R. Vallery-Radot, *Life of Pasteur,* p. 88.

lighted my hearers, it is by their contents; it is the future reserved to those great results, so unexpected, and opening with such entirely new vistas of physiology. I have dared to say so, for at these heights all sense of personality disappears, and there only remains that sense of dignity which is ever inspired by true love of science.

"God grant that by my persevering labours I may bring a little stone to the frail and ill-assured edifice of our knowledge of those deep mysteries of Life and Death where all our intellects have so lamentably failed.

"P.S.—Yesterday I presented to the Academy my researches on spontaneous generation; they seemed to produce a great sensation."

The history of the question of spontaneous generation goes so far back as the classical writers.* Thales of Miletus thought animals came from moisture. Anaximander believed life originated in inorganic mud. Aristotle and Lucretius, Virgil and Ovid, and Pliny all believe in it. Does not Virgil describe the way in which a swarm of bees can be made to originate from the rotting carcase of a young bull? Taking over the Virgilian belief, our ancestors thought that if you put a piece of beef in the sun, and allowed it to putrefy, they conceived that the grubs which soon began to appear were the outcome of the action of a power of spontaneous generation which the beef contained. And they could provide you with recipes for making various animal and vegetable preparations which would produce the particular kinds of animals you required. Thus Van Helmont (1577—1644), the Belgian physician, actually supplies a recipe for the spontaneous generation of the domestic mouse. His prescription consists in squeezing some soiled linen into the mouth of a vessel containing grains of wheat, whereupon, after the lapse of twenty-one days, thrice the mystic number seven, the wheat will be found to have been transformed into mice—adult ones to boot, with both sexes equally represented! Some time later an Italian, Buonanni, announced a fact no less weird. Certain timberwood, he said, after rotting in the sea, produced worms which

* J. Tyndall, "Spontaneous Generation" in *Popular Science Monthly*, XII, 1877, pp. 476-88, 591-604.

engendered butterflies, and these butterflies became in their turn birds!

Harvey, the discoverer of the circulation of the blood, like his contemporaries believed in spontaneous generation. A less credulous Italian, Francesco Redi (1626—1698), studied the phenomena. In order to demonstrate that the worms found in rotten meat did not appear spontaneously, he placed a piece of gauze over the meat. Flies, attracted by the colour, deposited their eggs on the gauze. From these eggs were hatched the worms, which up to the time of the experiment had been supposed to begin life spontaneously in the flesh itself. He demonstrated the fact that life begins from life, and for a while his demonstration was remembered. A medical professor of Padua, Vallisneri (1661—1730), also recognised that the grub in a fruit is hatched from an egg deposited by an insect before the development of the fruit. Not becoming part of the mental stock of mankind, the work of Redi and Vallisneri was overtaken by oblivion.

The microscope bolstered up this belief by disclosing to the gaze of men thousands of creatures, those infinitely small beings which appeared in rain-water as in any infusion of organic matter when exposed to the air. How could bodies capable of producing a million descendants in the course of less than forty-eight hours do so save by spontaneous generation? Diderot was intrigued by the problem of evolution, and he was also intrigued by the kindred problem of spontaneous generation. "Does living matter," he asked, "combine with living matter? how? and with what result? And what about dead matter?" Such questions appear in his *Interpretation of Nature,* which Comte, absurdly enough, placed beside Descartes's *Discourse on Method* and Bacon's *Novum Organum.* Still, it is startling in 1754 to find Diderot asking, What is the difference between living matter and dead? Does the energy of a living molecule vary by itself, or according to the quantity, the quality, the forms of the dead or living matter with which it is united? Questions such as these are samples of what he thought philosophers might perhaps count worthy of discussion.

About the time of Diderot two priests, one an Englishman, Needham, and the other an Italian, Spallanzani, took the subject up. Needham had studied with Buffon microscopic animalculæ. The force which Needham found in matter, a force which is called productive or vegetative, he regarded in 1745 as charged with the formation of the organic world. Buffon explained this force by holding that there are certain primitive and incorruptible parts common to animals and vegetables. These organic molecules cast themselves into the moulds or shapes which constituted different beings. When one of those moulds was destroyed by death, the organic molecules became free. Ever active, they worked the putrefied matter, appropriating to themselves some raw particles, and forming by their reunion a multitude of little organised bodies. All these bodies, according to Buffon, only existed through spontaneous generation.

Spallanzani studied in 1763 the infinitesimal beings through his microscope. Needham had affirmed that by enclosing putrescible matter in vases and by placing those vases on warm ashes, he produced animalculæ. Spallanzani shrewdly suspected firstly that Needham had not exposed the vases to a sufficient degree of heat to kill the seeds which were inside; and secondly that seeds could readily have entered those vases and given birth to animalculæ, for Needham had only closed his vases with cork stoppers, which are very porous. "I repeated the experiment with more accuracy," wrote the Abbé Spallanzani: "I used hermetically sealed vases. I kept them for an hour in boiling water, and after having opened them and examined their contents within a reasonable time I found not the slightest trace of animalculæ, though I had examined with the microscope the infusions from nineteen different vases." It would seem as if Spallanzani had anticipated one of the most renowned of the experiments of Pasteur with results that would have satisfied the French investigator.

Voltaire, after his wont, took an interest in this scientific dispute. In his *Singularities of Nature*, written in 1769, he laughed at Needham, whom he turned into an Irish Jesuit in order to amuse his readers. Jesting at this race of so-called eels which began in the gravy of boiled mutton, he said: "At once several philosophers exclaimed at the wonder and said, 'There is no germ; all is made, all is regenerated by a vital force of nature.' 'Attraction,' said one. 'Organised matter,' said another, 'they are organisable molecules which have found their casts.' Clever physicists were taken in by a Jesuit." In this effusion of Voltaire nothing remained of what he termed "the ridiculous mistake, the unfortunate experiments of Needham, so triumphantly refuted by M. Spallanzani and rejected by whosoever has studied nature at all." "It is now demonstrated to sight and reason that there is no vegetable, no animal but has its own germ." In his *Philosophic Dictionary,* at the word God, he remarks, "It is very strange that men should deny a creator and yet attribute to themselves the power of creating eels."

It would seem as if the experiments of the Abbé Spallanzani had proved that there was no such thing as spontaneous generation. Nor were his results suffered, like those of Abbot Mendel, to languish in the obscurity of a local scientific journal. Voltaire was as great a journalist as Jonathan Swift himself, and with the publicity given to Spallanzani's work in the *Singularities of Nature,* it might have been thought impossible that such a striking demonstration either would or could have been forgotten. Forgotten it undoubtedly was. After all, sound requires atmosphere, and, for that matter, results require their atmosphere. If a Joseph II lived before his own proper age, it is no less true that the Abbé Spallanzani lived before the age when scientists were likely to listen to what he had carried out.

The subject dropped in one form only to be revived in another. Experiments of course, in the eyes of men even in the middle of the nineteenth century, were rather absurd methods of demonstrating the truths of nature. A priori conceptions were a far more satisfactory fashion, so they believed. In 1846 a moralist called Ernest Bersot employed this plan when he wrote his book on Spiritualism: "The doctrine of spontaneous generation pleases simplicity-loving minds; it leads them far beyond their own expectations. But it is yet only a private opinion, and, were it recognised, its virtue would have to be limited and narrowed down to the production of a few inferior animals."

On December 20, 1858, a correspondent of the Institut, M. Pouchet, Director of the Natural History Museum of Rouen, sent to the Academy of Sciences a "Note on Vegetable and Animal Proto-organisms spontaneously generated in Artificial Air and in Oxygen Gas." The note began thus: "At this time when, seconded by the progress of science, several naturalists are endeavouring to reduce the domain of spontaneous generation or even to deny its existence altogether, I have undertaken a series of researches with the object of elucidating this vexed question." Declaring that he had taken excessive precautions to preserve his results from any cause of error, he showed apparently convincing experiments, demonstrating that organisms developed spontaneously when hay which had been previously heated to a high temperature was introduced into the atmosphere of pure oxygen. In 1860 the French Academy offered a prize for a series of experiments that would tend towards a solution of a question that had occupied the attention of mankind since the days of the classical writers.

The world of science discussed the whole question. Pasteur contented himself not with discussion but with tests of the results that M. Pouchet had claimed to be true, and therefore verifiable. At one of the extremities of the façade of the Ecole Normale in Paris, on the same line as the doorkeeper's lodge, a pavilion had been built for the school architect and his clerk. Pasteur succeeded in obtaining possession of this small building, and transformed it into a laboratory which any student of our day would describe as totally inadequate. For four years he experimented, and *en route* made many discoveries. For example, he made clear the efficiency of a cotton-wool plug in the neck of a flask as a means of preventing the entrance of air germs, and he invented a flask with a long-drawn-out neck, known to the researcher as Pasteur's flask. It is easy to understand why so exact an experimenter should write to Pouchet that the results he had attained were "not founded on facts of a faultless exactitude. I think you are wrong, not in believing in spontaneous generation (for it is difficult in such a case not to have a preconceived idea), but in affirming the existence of spontaneous generation. In experimental science it is always a mistake not to doubt when facts do not compel affirmation. . . . In my opinion, the question is whole and untouched by decisive proofs. What is there in air which provokes organisation? Are they germs? is it a solid? is it a fluid? is it a principle such as ozone? All this is unknown and invites experiment." *

* R. Vallery-Radot, *Life of Pasteur*, p. 94.

The more he laboured in the laboratory, the more Pasteur became convinced that there is nothing in the air itself able to produce life, and he gradually came to the conclusion that it was the germs in the air that did the mischief. Such an idea was preposterous to Pouchet. How many millions of loose eggs or spores, he derisively inquired, would then be contained in a cubic millimetre of atmospheric air if the Pasteurian hypothesis were true? He proceeded to advise investigators to accept the doctrine of spontaneous generation adopted of old by so many men of genius from the days of Aristotle downwards. Of course this is the argument of authority draped in scientific clothing, but it is authority emphatically that Pouchet invoked.

Into a series of flasks of a capacity of 250 cubic centimetres Pasteur introduced a putrescible liquid, such as yeast water, which he boiled. While the liquid was still boiling he closed, with an enameller's lamp, the pointed opening through which the steam had rushed out, taking with it all the air the vessel contained. If the air were pure, the contents of the flask remained pure. If the air were impure, the contents of the flask remained impure. On August 10, 1860, he wrote to Chappuis*: "I fear from your letter that you will not go to the Alps this year. . . . Besides the pleasure of having you for a guide, I had hoped to utilise your love of science by offering you the modest part of curator. It is by some study of air on heights afar from the habitations and vegetation that I want to conclude my work on so-called spontaneous generation. The real interest of that work for me lies in the connection of this subject with that of ferments which I shall take up again in November." He started for Arbois, taking with him seventy-three flasks. He opened twenty of them not very far from his father's old tannery on the road to Dôle. Of those twenty vessels, opened some distance away from any dwelling, eight yielded organised bodies. He then walked on to Salins and climbed Mount Poupet, 850 metres above sea-level. Out of the twenty vessels opened, only five were altered. Pasteur would have liked to charter a balloon in order to prove that the higher you go the fewer germs you will find, and that certain zones absolutely pure contain none at all. Instead, he ascended

* R. Vallery-Radot, *Life of Pasteur*, p. 96.

Montanvert at Chamonix. A mule carried the case of thirty-three vessels, followed very closely by Pasteur. The twenty flasks were brought to the Mer de Glace. Pasteur gathered the air with infinite precautions; he used to enjoy relating the details to those people who call everything easy—after it has been found out. After tracing with a steel point a line on the glass, careful lest specks of dust should become a cause of error, he began by heating the neck and fine point of the bulb in the flame of the little spirit-lamp. Then raising the vessel above his head, he broke the point with steel nippers, the long ends of which had also been heated in order to burn the specks which might be on their surface. These would have been driven into the vessel by the quick inrush of the air. Of those twenty flasks, closed again immediately, only one was altered. "If all the results are compared that I have obtained until now," he wrote, on March 5, 1880, when relating this journey to the Academy, "it seems to me that it can be affirmed that the dusts suspended in atmospheric air are the exclusive origin, the necessary condition of life in infusions." *

Bacon long ago reminded us that Saul set out to seek his father's asses, and instead of finding the asses he found a kingdom. It is a tale that is being constantly told in the realm of science. An investigator re-starts the old idea of spontaneous generation, and another investigator tries to seek the truth about the subject. He proves that there is no such thing as spontaneous generation, and incidentally he founds the brand-new subject of bacteriology. For Pasteur points out: "What would be most desirable would be to push those studies far enough to prepare the road for a serious research into the origin of various diseases." * It was dawning upon him that germs were not merely the active cause of putrefaction and of fermentation, but they were also the active cause of disease. Such was the far-reaching result of an attempt to discover the validity of the belief in spontaneous generation. The Pasteurisation of wine or of milk, the process of sterilisation, Listerism, and a thousand other -isms, all date their origin from these renowned results. Yet in 1860 a scientist could write in *La Presse*: "I am afraid that the experiments you quote, M. Pasteur, will turn against you

* R. Vallery-Radot, *Life of Pasteur*, p. 98.

. . . The world into which you wish to take us is really too fantastic. . . ." *

There was a vacancy in the Institut in 1861, and despite the efforts of Balard and Dumas the votes of the members elected Duchartre, not Pasteur. The following year he was elected on December 8, receiving thirty-six votes out of sixty. Biot did not live to see this outcome of his canvass on behalf of Pasteur. The very next morning, when the gates of the Montparnasse cemetery were opened, a woman walked towards Biot's grave with her hands full of flowers. It was Mme Pasteur, who was bringing them to him who lay there since February 5, 1862.

After his election to the Institut, in March 1863, the Emperor Napoleon III, who professed an interest in all that was going on in the small laboratory of the Rue d'Ulm, desired to speak with Pasteur. Dumas claimed the privilege of presenting his former pupil, and the interview took place at the Tuileries. Napoleon questioned Pasteur, who assured "the Emperor that all my ambition was to arrive at the knowledge of the causes of putrid and contagious diseases."† The spontaneous generationists, Pouchet, Joly, Professor of Physiology at Toulouse, and Musset, made fresh experiments which apparently verified their ideas. Ascending higher than Montanvert, they found alteration in their flasks. These, filled with a decoction of hay, showed germs. Pasteur's flasks, filled with yeast water, showed sterility. Our conclusion would be that the different results depended on the circumstance that the hay water contained the spores of the bacilli. Pouchet, Joly, and Musset of course thought nothing of the kind, and were convinced even in 1876—when Dr. Bastian raised the question again—that they were as indubitably right as Pasteur was as indubitably wrong. "Therefore," said Pouchet triumphantly,‡ "the air of the Maladetta [the glacier they ascended], and of high mountains in general, is not incapable of producing alteration in an eminently putrescible liquor; therefore heterogenia or the production of a new being devoid of parents, but formed at the expense of ambient organic matter, is for us a reality."§

* R. Vallery-Radot, *Life of Pasteur*, p. 99.
† *Ibid.*, p. 104. ‡ *Ibid.*, p. 105.
§ *Ibid.*, p. 105.

Before the world of fashion as well as before the world of learning in the lecture-room of the Sorbonne, on April 7, 1864, Pasteur gave his conclusions. Duruy and Alexandre Dumas, senior, George Sand and the Princess Mathilde, were present, as well as representatives of science. Beginning in his deep, firm voice, Pasteur said: "Great problems are now being handled, keeping every thinking man in suspense; the unity or multiplicity of human races; the creation of man a thousand years or a thousand centuries ago; the fixity of species, or the slow and progressive transformation of one species into another; the eternity of matter; the idea of a God unnecessary. Such are some of the questions that humanity discusses nowadays." * Then he came to his own subject. Can matter organise itself? Can living beings come into the world without having been preceded by beings similar to them? Explaining how the microscope had given a fresh lease of life to the idea of spontaneous generation, he narrated the results of his experiments, showing the part played by germs. "And, therefore, gentlemen, I could point to that liquid and say to you, I have taken my drop of water from the immensity of creation, and I have taken it full of the elements appropriate to the development of inferior beings. And I wait, I watch, I question it, begging it to recommence for me the beautiful spectacle of the first creation. But it is dumb, dumb since these experiments were begun several years ago; it is dumb because I have kept it from the only thing man cannot produce, from the germs which float in the air, from Life, for Life is a germ and a germ is life. Never will the doctrine of spontaneous generation recover from the mortal blow of this simple experiment." †

The fashionable and the scientific audience applauded these stirring words, which ended the lecture: "No, there is now no circumstance known to me in which it can be affirmed that microscopic beings came into the world without germs, without parents similar to themselves. Those who affirm it have been duped by illusions, by ill-conducted experiments, spoilt by errors that either they did not perceive or did not know how to avoid." ‡

* R. Vallery-Radot, *Life of Pasteur,* p. 107.
† *Ibid.,* p. 108. Cf. Sir R. Godlee, *Lord Lister,* p. 176
‡ R. Vallery-Radot, *Life of Pasteur,* p. 109.

George Eliot used to say that of all forms of mistake, prophecy was quite the most gratuitous. In 1876—twelve years after the date of the famous Sorbonne lecture—Pouchet and Musset, Joly and Bastian * alike announced experiments which, they claimed, proved the truth of the doctrine of spontaneous generation. Therefore this doctrine, in spite of Pasteur, did recover, though the recovery was a poor one.

Minute organisms were the *vera causa* of what was called spontaneous generation. Might they not be the *causæ* of much else? Might they not set up diseases in animals and men as well as fermentation in liquids? While these general conclusions were in his mind, J. B. Dumas asked him to turn his attention to the poisonous plight of the silkworms. In the reign of Louis-Philippe the income from the silk industry was 100,000,000 francs. In Pasteur's day the loss in one arrondissement of Alais for fifteen years was 120,000,000 francs. This huge income was in process of vanishing, for disease attacked the silkworms. The symptoms were serious in 1843 and 1845, and became increasingly so in 1849, 1853, and 1864. As the spots on the diseased worms resembled pepper grains, the disease was called "pébrine," from the patois word *pébré*. To the deep disgust of the silkworm cultivators, Pasteur studied hundreds of moths under the microscope. They thought that the services of a zoologist or one of themselves infinitely preferable to one who was a "mere chemist." Pasteur contented himself with saying, "Have patience," and continued experimenting. He ascertained that there were two distinct maladies at work. One was the real pébrine, due to a protozoal organism. The other was "flâcherie," due to an actively mobile bacillus. He diagnosed the causes and he provided a remedy for both, and this, in spite of the hostility or the scepticism of scientists and cultivators, he successfully accomplished. In the spirit of Faraday, he informed Napoleon and Eugénie, in the course of a private interview with them when they manifested some surprise that he did not apply his discoveries to his own profit, "that in France scientists would consider that they lowered themselves by doing so." † The real man comes out

* Cf. the remarks in M. Onslow, *Huia Onslow*, pp. 108-9.

† R. Vallery-Radot, *Life of Pasteur*, p. 129.

no less clearly in the answer he gave to Henri Sainte-Claire Deville when Claude Bernard and Pasteur tried blood taken from patients during the cholera epidemic of 1865. Deville remarked to Pasteur, " Studies of that sort require much courage." " What about duty? " said Pasteur simply, in a tone, said Deville afterwards, worth many sermons. Like Sir Walter Raleigh, he continued to toil terribly. Friends were tempted to make a comparison with the legendary demon of Michael Scott, who had to be found a task lest he should turn upon his master.

The old adversaries, who proclaimed their belief in spontaneous generation, and the new ones, who announced that there was nothing in " pébrine " or flâcherie," were extremely *en évidence* in 1867. Pouchet was as confident as ever, announcing that the question of spontaneous generation was being taken up in Italy and Germany, in England and America. In his illuminating biography of his father-in-law of which we have made so extensive use, M. Vallery-Radot records: " Now that the scourge [of the silkworms] was really conquered, Pasteur imagined that all he had to do was to set up a table of results sent to him. But, from the south of France and from Corsica, jealousies were beginning their work of undermining; pseudo-scientists in their vanity proclaimed that everything was illusory that was outside their own affirmations, and the seed merchants, willing to ruin everybody rather than jeopardise their miserable interests, ' did not hesitate (we are quoting M. Gernez) to perpetrate the most odious falsehoods.' Instead of being annoyed, saddened, often indignant as he was, Pasteur would have done more wisely to look back upon the history of most great discoveries and of the initial difficulties which beset them. But he could not look upon such things philosophically; stupidity astonished him and he could not easily bring himself to believe in bad faith. His friends in Alais society, M. de Lachadenède, M. Despeyroux, Professor of Chemistry, might have reminded him, in their evening conversations, of the difficulties ever encountered in the service of mankind.... But such comparisons had no weight with Pasteur; he was henceforth sure of his method and longed to see it adopted, unable to understand why there should be further discussions now that the silkworm industry was saved and the bread of so many families assured. He was learning to know all the bitterness of sterile polemics, and the obstacles placed one by one in the way of those who attempt to give humanity anything new and useful." *

The opposition Pasteur experienced was the opposition that Jenner had experienced when he proclaimed the truth of vaccination, it was the opposition that Simpson had experienced when he proclaimed the truth of chloroform effects, it was the opposition Lyell had experienced when he proclaimed the truth of uniformitarianism, it was the opposition Helmholtz and Joule had experienced when they proclaimed the truth of the conservation of energy, and it was the opposition Darwin had experienced when he proclaimed the truth of evolutionism. The doctrines change, the discoverers change, but the opposition never changes. Our studies in the conflict—if they teach us anything—of science with scientists during the nineteenth century teach us that the discoverer can always reckon on meeting with opposition from his fellow-scientists. True, the opposition may wear different forms, but *plus ça change, plus c'est la même chose.*

The case of Liebig adds to the truth of this grave French saying. Pasteur had met him on paper, and he had not convinced him of the truth of his ideas on fermentation. Pasteur, optimistically enough, believed that if he met the illustrious German chemist in the flesh he might convert him to the truth of his ideas. He forgot, foolishly enough, that if he converted Liebig to the truth of his ideas, he proved the falsity of Liebig's idea. In face of decisive experiments, how could Liebig affirm that the presence of decomposing matter should be necessary to fermentation? Had not Pasteur sown a trace of yeast in water, containing but sugar and crystallised salts, and had he not seen this yeast multiply itself and produce a regular alcoholic fermentation? How *could* Liebig deny the independent existence of ferments in their infinite littleness and their power of destroying and transforming everything? Besides, Liebig, who hailed Dumas as a master, had seen this master acknowledge the truth of the new views. Since then Pasteur had extended his theory to the disease infecting the silkworms and had shadowed forth its extension to the diseases of men as well

* R. Vallery-Radot, *Life of Pasteur,* p. 170.

as of animals. In 1870, full of hope, Pasteur paid his visit. "To convince Liebig," writes M. Vallery-Radot, "to bring him to acknowledge the triumph of those ideas with the pleasure of a true savant, such was Pasteur's desire when he entered Liebig's laboratory. The tall old man, in a long frock coat, received him with kindly courtesy; but when Pasteur, who was eager to come to the object of his visit, tried to approach the delicate subject, Liebig, without losing his amenity, refused all discussion, alleging indisposition. Pasteur did not insist, but promised himself that he would return to the charge." *

If law is silent during war, research is no less so. On the outbreak of the Franco-German War of 1870-1, Pasteur longed to serve, but an attack of paralysis had disqualified him for active service. The disasters of his native land fell on his ardent heart with the deepest pain and grief. "Should we not cry: 'Happy are the dead!'" he wrote when he heard of the surrender of Metz, the strongest in France, by Bazaine without a struggle.

Lister had been meditating on Pasteur's theory of germs with the outcome that he proclaimed himself a follower of the new ideas. He attempted the destruction of germs floating in the air by means of a vaporiser filled with a carbolic solution. But it occurred to no one in France—except Pasteur—to apply the Pasteurian conceptions to the cure of wounds.

Pasteur, as we have seen, returned his diploma of M.D., *honoris causa,* bestowed upon him by the University of Bonn. The reply was: "SIR,—The undersigned, now Principal of the Faculty of Medicine of Bonn, is requested to answer the insult you have dared to offer to the German nation in the sacred person of its august Emperor, King Wilhelm of Prussia, by sending you the expression of their *entire contempt.*—MAURICE NAUMANN. P.S.—Desiring to keep its papers free from taint, the Faculty herewith returns your screed."

Part of the reply of Pasteur was: "And now, Mr. Principal, after reading over both your letter and mine, I sorrow in my heart to think that men who, like yourself and myself, have spent a lifetime in the pursuit of truth and progress should address each other in such a fashion. This

* R. Vallery-Radot, *Life of Pasteur,* p. 175.

is but one of the results of the character your Emperor has given the war. You speak to me of *taint*. Mr. Principal, you may be assured that taint will rest until far-distant ages on the memory of those who began the bombardment of Paris, when capitulation by famine was inevitable, and who continued this act of savagery after it had become evident to all men that it would not advance by one hour the surrender of this heroic city."

The brilliant young painter, Henri Regnault, enlisted as a Garde Nationale, though exempt by law from any military service as he was a laureate of the Prix de Rome. In the last sortie attempted by the Prussians at Buzenval, the last Prussian shot struck him in the forehead. The Academy of Sciences rendered its homage to one whose coffin enclosed so many hopes of a great future in the world of art.

The Franco-German War forms an essential part of Pasteur's life not only as a Frenchman but also as a scientist. Accustomed to diagnosis in science, he extended his diagnosis to la Patrie. He put his finger on what he conceived to be the main fault, the forgetfulness, the disdain, that France had exhibited for intellectual men, especially in the domain of exact science. If in 1792 France was able to face danger on all sides, had she not Berthollet and Chaptal, Fourcroy and de Moreau, Monge and Lavoisier? The day after Lavoisier's execution, Lagrange said: "One moment was enough for this head to fall, and two hundred years may not suffice to produce such another." If Lavoisier and Condorcet perished, the Republic had many scientists behind it. Were there such men in 1870-1? The more he reflected on the plight of his beloved land, the more Pasteur felt that it was the most urgent of all calls to persistent research as much to redeem the reputation of France as to contribute to the cause of truth.

In a fever of anxiety, he set to work. He had improved the silk industry. Could he not seek means of making his seed-selecting process applicable by cultivators on a small scale as well as a large? Could not each village own its own microscope? Could not the village schoolmaster examine the moths? Italy and Austria adopted his plans for the diagnosis of the disease of the silkworms long before his native country, which was the very last to be convinced.

Austria offered him a handsome prize for his remedy against pébrine, and then it struck French sericultors that there was something in it. M. Vallery-Radot seems to think that the French character offers this strange contrast, that France is often willing to risk her fortune and her blood for causes which may be unworthy, whilst at another moment, in everyday life, she shrinks at the least innovation before accepting a benefit originated on her own soil. The French often wait until other nations have adopted and approved a French discovery before venturing to adopt it in their turn. Here we think that the biographer is too hard on his fellow-countrymen, for just as a prophet has no honour in his own country, the discoverer has every whit as little. When other countries become aware of the merits of the discoverer, then his own begins to appreciate them. R. Meldola, in his memoir of Sir William Henry Perkin (1838—1907), insists on this very point. Perkin discovered mauve, the first aniline dye which had created the important coal-tar dyeing industry and had revolutionised industrial processes in varied directions. The Germans, however, were more alive to the merits of mauve than the countrymen of its discoverer.

Inspired by patriotic motives, Pasteur hoped to make French beer capable of competing with German beer, and he sought for the causes rendering beer acid or putrid, sour or slimy. Were not these alterations due to germs in the air, or in the water, or in the utensils of the brewery? Whether he investigated silkworms, vinegar, or wine, germs always afforded the clue, the clue of which he was the real discoverer. As the silkworm cultivators had turned aside from his plans till their brethren in Italy and Austria had adopted them, so the beer manufacturers turned aside. English brewers received a visit from the French scientist in a more friendly spirit than the French, though this arises partly from the fact that the visitor was foreign. Pasteur felt glad of the opportunity of rendering assistance to the practical English brewers, who concluded that the stranger possessed ideas out of which they could reap profit. "We must make friends for our beloved France," he would say, and certainly men like him rendered possible the Entente of our own day. He combined in his person the faith of a patriotic apostle with the patience of a fervent scientist.

Pouchet, in his book on *The Universe: the Infinitely Great and the Infinitely Small*, published in 1872, condescended to admit that some "microzoa did fly about here and there," but as for the theory of germs, why, that was simply "a ridiculous fiction." Liebig had apparently recovered from the indisposition he felt at his interview with Pasteur in July 1870, for he published a long treatise disputing the facts put forward by Pasteur. He declared that in the German process of vinegar-making the chips of beech-wood placed in the barrels acted as supports for the *Mycoderma aceti*. Liebig, who consulted at Munich the chief of one of the largest vinegar factories, affirmed that he himself had not seen a trace of fungus on chips used in that factory for twenty-five years. Pasteur had offered to Pouchet, Joly, and Musset to bring their conflicting views to the test of experiment, and they had in effect refused. Pasteur offered to dry some of the chips rapidly in a stove and to send them to Paris, where a commission, selected from members of the Academy of Sciences, would decide this dispute. Liebig refused to submit to this test.

A member of the Academy of Sciences, M. Frémy, took part in what proved to be an almost interminable discussion on the origin of ferments. So far back as 1841 he had investigated lactic fermentation, "at a time when our learned colleague—M. Pasteur—was barely entering into science," and his conclusion was that ferments arose from organic bodies, not from dust. Some bodies, he held, by reason of the vital force with which they are endowed, go through successive decompositions, and give rise to new derivatives. Thus, only thus, are ferments engendered. His theory was in fact a form of the spontaneous generation view combined with the vital force of the school of Natural Philosophy, a school Helmholtz had to encounter.

M. Trécul, a botanist who sincerely sought the truth, insisted that he had witnessed a whole series of transformation of microscopic species each into the other. Besides, Pouchet, Joly, and Musset had proved the matter. Therefore, spontaneous generation must be true. Pasteur offered experimental proof—if Messieurs Frémy and Trécul cared to avail themselves of it. Like Liebig, like Pouchet, Joly, Musset, they did not care to avail themselves of it. Balard and Dumas implored Pasteur to continue his researches, disregarding the attacks made upon them. Pasteur felt as strongly as Huxley that he had been given truth, and that he must at all costs proclaim it. On his way to the Diet of Worms, Martin Luther, according to tradition, retorted to those who would have prevented his going there: "Here I stand, I can do no other. God help me. Amen." Here Pasteur stood, and he too could do no other. "What *you* lack, M. Frémy, is familiarity with the microscope, and you, M. Trécul, are not accustomed to laboratories." Trécul persisted that he had witnessed the transformation of cells or spores from one into the other. Yes, cordially agreed Pasteur, there was one transformation, that of *Mycoderma vini* into an alcoholic ferment, but there was certainly no other. The controversy dragged on. How could it help dragging on when Messieurs Frémy and Trécul were thrifty of experiment and extravagant with speech? Their opponent was extravagant with experiment and thrifty of speech. They were like two Euclidian parallel straight lines: they could never, in spite of Einstein, meet.

The experiments all through the fierce dispute went on. Pasteur observed the aerobiæ, requiring air to live, the anaerobiæ, perishing when exposed to air, and a class of organisms capable of living for a time outside the influence of air. Surely fermentation was simply life without air. The old researches joined on to the new, reinforcing the old conclusions by fresh examples which served to deepen their truth. Liebig and his school insisted that fermentation was a phenomenon of death. Pasteur equally insisted that it was a phenomenon of life—without air.

If germs are in the air, it would seem as if ideas were also in the air. Men by their intuition divine what they do not experimentally know. The theory of atomism and the theory of evolution were divined in classical times before there was a shred of real proof offered in support of either. Similarly, the theory of germs has a long history, though nothing like so lengthy a pedigree as either the theory of atomism or the theory of evolution. Robert Boyle (1627—1691), the seventh son and the fourteenth child of the "great" Earl of Cork, had announced that he who could probe to the bottom the nature of ferments and fermentation would probably be more capable than anyone of explaining certain morbid phenomena. Nor was this the only flash of Boyle's genius. Did he not demolish the peripatetic doctrine of the four elements, the Stagiristic doctrine of the *tria prima*? Did he not tentatively substitute the principles of a "mechanical philosophy" for that of mere hypothesis? Did he not suggest fresh ways of looking at the old atomic views? Did he not suggest the transmutability of differing forms of matter by the rearrangement of their particles? Views on the nature of fermentation and its bearing on disease he possessed. It remained for a man of the genius of Pasteur to win verification, by patient investigation, for what Boyle had divined. If Boyle was the Moses who led men to Mount Pisgah, Pasteur was the Joshua who led them to the Promised Land.

Medical men seem to resent with peculiar emphasis the intrusion of the criticisms of anyone who does not belong to their ranks. Darwin thought with grave reason that intelligent men, who were not naturalists, would grasp his ideas on natural history. Zoologists, botanists, and geologists, in a diminishing scale, would certainly offer stout opposition. His forecast was amply warranted by the hostile reception given by the naturalists in general and by the zoologists in particular to his *Origin of Species*. The microbe of conservatism in opinion had infected them. Medical men also possess this microbe in no scanty measure. So Jenner had found, so Simpson had found, so Lister had found, and so Pasteur was about to find. What is the cause of this attitude of conservatism to ideas? Is it because the surgeon and the physician feel themselves to be ruling powers? Is it because they are accustomed to offer daily advice to their patients? Is this the cause why they so frequently adopt the authoritative tone of "I ain't a-arguing with you. I'm a-telling you"?

There are of course many exceptions to the microbe of conservatism infecting medical men. Dr. Villemin, the physician of Vâle de Grâce, formed such an exception. Working patiently from 1860 to 1865, he came to the conclusion that tuberculosis was a specific and contagious disease. Dr. Pidoux, a typical representative of traditional medicine, with his gold-buttoned blue coat and his reputation equally outstanding in Paris and at the Eaux-Bonnes, declared that the idea of specificity was a fatal thought. As Matthew Arnold said of religion, that it is morality touched with emotion, so practice in medicine is science touched with emotion. The pity is that the emotion not seldom assumes the form of prejudice. "Le médecin artiste ne crée rien," said Claude Bernard, Pasteur's close friend; but surely he is wrong. For the doctor of the type of Pidoux creates the atmosphere of a stately practice, unassailable, fortified in authority. The surgeon of his class, when he possesses skill, comes to have a name that is a household word, his face known everywhere, his presence felt, his anger dreaded, his verdict final. Such a surgeon is sometimes the honour of his profession, sometimes the dishonour. His is the honour if with his experiences he preserves something of an open mind. His is the dishonour if with his experiences he holds a mind hermetically sealed with seven seals against the admission of all new ideas.

Pidoux was naturally a staunch supporter of the doctrine of diathesis and of the morbid spontaneity of the organism. To him "disease is in us, of us, by us." He was not even sure that smallpox could only proceed from inoculation and contagion. Did Villemin, in the true spirit of Pasteur, declare there were germs of tuberculosis? Then it was for Pidoux to declare that "then all we doctors have to do is to set our nets to catch the sporules of tuberculosis, and find a vaccine." If truth is sometimes spoken in jest, it is also sometimes spoken in sarcasm, and Pidoux spoke it in sarcasm. Spontaneous generation, we can imagine his saying, is right, always has been right, and always will be right. As for these new-fangled notions about germs, no doctor with any reputation to lose would dream of believing in them! Following in the steps of Villemin, Davaine, who had perused Pasteur's books on fermentation, had the audacity to put forth the idea that the filaments found in anthrax were bacteria.

As Pasteur had been attacked, as Villemin had been attacked, so Davaine was now attacked. Dr. Chassaignac, a prominent surgeon, spoke before the Academy of Medicine of what he called "laboratory surgery, which has destroyed very many animals and saved very few human beings." * He elaborated his ideas by pointing out that "laboratory results should be brought out in a circumspect, modest, and reserved manner, as long as they have not been sanctioned by long clinical researches, a sanction without which there is no real and practical medical science." * That is, the surgeon of his class should hear from the scientist what he had discovered, should impart his discovery to him, and then he himself should employ it in his operating theatre, and of course claim all the credit of the work of another. Besides, "everything," he held, "cannot be resolved into a question of bacteria!" All unconscious of the truth, he growled, "Typhoid fever, bacterisation! Hospital miasma, bacterisation!!" Dr. Piorry, with all his accustomed solemnity of tone, found not germs, but pus! Trécul still thought that the *vera causa* was his hypothesis of transformations.

The appreciation denied Pasteur at home was to be his in abundant measure abroad. If his ideas did not for the time bear fruit in the hospitals of Paris, they did in the hospitals of Edinburgh, as the following letter, dated from Edinburgh, February 13, 1874, testifies:

My dear Sir,—Allow me to beg your acceptance of a pamphlet, which I send by the same post, containing an account of some investigations into the subject which you have done so much to elucidate, the germ theory of fermentative changes. I flatter myself that you may read with some interest what I have written on the organism which you were the first to describe in your *Mémoire sur la Fermentation Appelée Lactique*.

I do not know whether the records of British "surgery" ever meet your eye. If so, you will have seen from time to time notices of the antiseptic system of treatment, which I have been labouring for the last nine years to bring to perfection.

Allow me to take this opportunity to tender you my most cordial thanks for having, by your brilliant researches, demonstrated to me the truth of the germ theory of putrefaction, and thus furnished me with the principle upon which

* R. Vallery-Radot, *Life of Pasteur*, p. 228.

alone the antiseptic system can be carried out. Should you at any time visit Edinburgh, it would, I believe, give you sincere gratification to see at our hospital how largely mankind is being benefited by your labours.

I need hardly say that it would afford me the highest gratification to show you how greatly surgery is indebted to you.

Forgive the freedom with which a common love of science inspires me, and

Believe me, with profound respect,
Yours very sincerely,
JOSEPH LISTER.

CHAPTER VIII

LISTER AND ANTISEPTICS

JOSEPH LISTER (1827—1912) came of that Quaker stock which has left such an indelible impress on the character of England. Originally of Yorkshire descent, John Lister came to London, becoming a freeman of the Bakers' Company. His son Joseph Jackson entered the wine business, and attained distinct business success. Marrying Isabella Harris, his fourth child and second son was born on April 5, 1827, in Upton House, a capacious old Queen Anne house with fields and gardens, at Upton in Essex. The atmosphere the child breathed was scientific from the very first. Between 1824 and 1843, in the intervals of business, his father found time to make mathematical calculations and to investigate the true shape of the red corpuscles of the blood. His work gained for him the Fellowship of the Royal Society in 1832, thus bringing him into contact with Airy and Herschel, Dr. Hodgkin and Sir Richard Owen. Unlike Darwin, his son learnt science at school, and at the age of fifteen he had already a sound all-round education, well grounded in mathematics and modern languages, natural science in general and comparative anatomy in particular. His bent towards surgery was unmistakable from the time he was a child, and this bent gradually became more confirmed.

He left school in the spring of 1844, and entered University College, London, to read for the B.A. degree. In the winter session of 1848 he began his preliminary medical studies under such men as Lindley, Professor of Botany; Graham, Professor of Chemistry; Grant, Professor of Comparative Anatomy; Ellis, Professor of Anatomy; Carpenter, Professor of Medical Jurisprudence; Wharton Jones, Professor of Surgery; and Sharpey, Professor of Physiology. All were competent men, and Jones and Sharpey were much more than competent.

Two of the finest biographies we have the pleasure of knowing are Mr. Stephen Paget's *Memoirs and Letters of Sir James Paget* and Sir Rickman Godlee's *Lord Lister.* * Among the many matters common to these two entrancing volumes there is the enthusiasm with which both speak of the teachers at their respective medical schools. It is a joy to note that when Paget and Lister looked back to their early days, both of them could testify to the abilities, the characters, and the workmanship of the men who lectured them. As they were at different medical schools, they do not naturally mention the same lecturers. This we regret, for as they both belonged to about the same period in surgery it would have been invaluable to compare the estimates they respectively formed. The rank Lawrence and Latham, Burrows and Stanley, occupied in Paget's eyes was filled in Lister's by Graham and Lindley, Wharton Jones and Sharpey. At University College, as at St. Bartholomew's, there was what Paget called "constant dissension and mischievous rivalry among the teachers." †

The extent and the precision of the knowledge of Wharton Jones combined with his investigations on the circulation of the blood and the phenomena of inflammation rendered him, in the opinion of Jenner, one of the greatest Englishmen who ever lived. Even making allowance for the enthusiasm of a fellow-student, this is extremely liberal praise, and Lister was fortunate in having such a man as a teacher, and in this capacity Huxley spoke warmly of the method and quality of his physiological teaching.‡ William Sharpey (1802—1880) was a few years older than Wharton Jones, and has been called the father of modern physiology, because he was the first to give a special course of lectures on this subject, which had formerly been treated as an appanage of anatomy. He had studied at Edinburgh and at Paris, where he worked at clinical surgery under Dupuytren and

* Once for all we acknowledge our vast indebtedness to Sir Rickman Godlee's book. We are glad to notice it has just gone into another new edition. Ours is the 1917 one.

† S. Paget, *Memoirs and Letters of Sir J. Paget,* p. 40.

‡ L. Huxley, *Life and Letters of T. H. Huxley,* I, pp. 20, 21, 26, 99. It is a pity there is no account of Jones in the *D.N.B.*

operative surgery under Lisfranc. At Berlin he dissected for nine months under Rudolphi, proceeding to Heidelberg to be under Tiedemann, and afterwards to Vienna. Commencing his medical studies in 1818, he did not settle in Edinburgh till 1829, a course almost unprecedented for thoroughness in those days. Appointed in 1836 to the chair of anatomy and physiology at University College, London, he taught there for thirty-eight years, exercising his undoubted power as a great teacher who was able to bind his pupils to himself both by ties of personal affection as well as by their common scientific interests. Among them were Michael Foster of Cambridge, and Burdon Sanderson of Oxford, and Lister.

The staff of University College Hospital included such well-known men as Sir John Erichsen and Sir William Jenner, who were careful observers. Thanks to J. Y. Simpson, the use of anæsthetics had robbed the operating theatre of much of its horrors. Yet the old days had left their mark, for operations were still performed with that breathless haste that characterised the pre-Jenner days. The need for the haste had disappeared, but the tradition of it remained, and the celerity of the operation was regarded as the outward and visible sign of a first-class surgeon. Pain no longer was the evil to be dreaded. Sometimes it almost seems as if in getting rid of one evil we have merely provided the opportunity for another to appear. If pain had largely gone, there were such subjects of dread as erysipelas and gangrene, pyæmia and septicæmia, and purulent infection. In the opinion of the surgeon Velpeau, so grave were these calamities that he said, "A pin-prick is a door open to Death." The terror expressed by ovariotomy was such that a physician declared that it ought to be "classed among the attributes of the executioner." Views like these impressed the young medical student with the risks undergone by the patient. If coming events sometimes cast their shadow before them, we can understand Lister reading a paper before the hospital Medical Society on hospital gangrene. In 1852 he won the M.B. of the University of London and the Fellowship of the Royal College of Surgeons, concluding his nine years at University College, and he also won that taste for original research which he owed principally to Wharton Jones and William Sharpey.

As Paget and Huxley when young students made their discoveries, so Lister made his. Kölliker of Würzburg, the leading histologist, had been the first to show that the contractile curtain in front of the lens of the eye was made of involuntary muscular tissue, and this tissue in turn was made of cells. Lister confirmed and extended Kölliker's observations, and for the first time demonstrated the existence of two distinct muscles in the iris, the dilator and sphincter, for enlarging and diminishing the size of the pupil.* Sir Richard Owen, an old friend of Lister's father, was pleased with this and another paper confirming observations of Kölliker on the involuntary muscular fibres of the skin.† Naturally that fine man, Kölliker, felt pleasure in deriving support from the young surgeon.

The circumstances of Quaker families are usually easy, and accordingly we find that Lister travelled at home and abroad. He felt interested in geology and architecture, in manners and customs, and he improved his knowledge of French, German, and even Dutch. As Sharpey had pursued his studies at different medical schools for eleven years, he recommended Lister to go to the famous one of Edinburgh, there to complete his studies by attending the practice of Syme. There was the James Syme (1799—1870) James Simpson knew, and there was the far different James Syme that Joseph Lister came to know with intimacy. Dr. John Brown, the author of *Rab and his Friends,* described Syme as " verax, capax, perspicax, sagax, efficax, tenax," and Lister plainly came to entertain an equally high opinion of him. Indeed, Lister thought that to enumerate all the contributions made by Syme during his career to the science and art of surgery was out of the question. In September 1853 Lister presented Sharpey's introduction to Syme, who at once received him most warmly.

The attraction between Syme and Lister was as great as the repulsion between Syme and Simpson. Lister had intended staying a month in Edinburgh, and he actually remained seven years. Syme made him house surgeon at the

* *Quarterly Journal of Microscopical Science,* 1853, I, p. 8; Lister, *Collected Papers,* I, p. 1.

† *Quarterly Journal of Microscopical Science,* 1853, I, p. 262; Lister, *Collected Papers,* I, p. 9.

hospital, and the two grew to be friends whose union was cemented when Lister married Agnes Syme on April 24, 1856, the daughter of the surgeon. As Pasteur had a real helpmate in his wife, Lister was fortunate enough to find one in his. Mrs. Lister sometimes wrote for her husband from dictation for seven or eight hours a day and "was most helpful in suggestion as to words and arrangement of sentences." By this marriage according to the strict customs of those days Lister was obliged to sever his connection with the Quakers, and he joined the Anglican communion. Here perhaps we ought to mention that in 1909 he wrote: "I have no hesitation in saying that, in my opinion, there is no antagonism between the religion of Jesus Christ and any fact scientifically." * There are many parallelisms between Paget and Lister, and their religious faith forms one of them. Jenner, Simpson, Lyell, Helmholtz, Joule, and Pasteur were all men who cared deeply for their common Christian religion. There is just one exception, and it is the outstanding name of Darwin.

There were dissensions within and rivalries without in the case of the medical schools of London. "Students of the medical history of the first half of the nineteenth century," remarked Sir Rickman Godlee, "cannot fail to be struck by the acrimony with which discussions were carried on, the amount of jealousy they excited, and the personal element which was constantly introduced. There is scarcely an author who does not speak of the *odium medicum*. It was a relic of the still more quarrelsome times of Mead, Jenner, and the Hunters, and indeed had been handed down from the long past." † None of this spirit animated Lister, who, strangely enough, thought that he should have less difficulty in avoiding it in Edinburgh than in London. Clearly he knew nothing of the fierce strife between Syme and Simpson, which was by no means unique, and he certainly had never heard of the even fiercer contests between the Wernerians and the Huttonians, waged most bitterly of all places in the University of Edinburgh herself. Is there a fieriness in the Celt that is lacking in the stolidity of the English? Be that as it may, neither Oxford, nor Cambridge, nor London can

* Sir R. Godlee, *Lord Lister*, p. 613.
† *Ibid.*, p. 31.

equal the scientific acerbity marking the annals of Edinburgh University during the first five decades of the nineteenth century. It is, therefore, with astonishment we read a letter of Lister to his father in which he writes: "I shall not have, as in London, to fight with jealous rivals, and contend or join ingloriously with quacks, but I shall be able, if all be well, to acquire a solid reputation in a legitimate manner, and then, if it seem desirable, move to London, and stand on my own ground there. I am by disposition very averse to quarrelling and contending with others, in fact, I doubt if I could do it, though I have never tried much, but at the same time I do love honesty and independence, which without contention would be almost impossible in London." *

Liston, Sir William Fergusson, and Sir Charles Bell had all done signally well in London, and Syme himself in 1848 had been Professor of Surgery at University College. Edinburgh was not an abode of quiet, yet the atmosphere of the place proved uncongenial to him. "I found," he said, "such a spirit of dispeace in the College as to forbid any reasonable prospect of comfort," and therefore he returned to a metropolis where he better grasped the conditions of hostility. Whatever foes Syme encountered abroad, he met none in his own home. Lister was his house surgeon, and the intimacy between the two grew with extreme rapidity. John Hunter was Lister's greatest hero, as he had been the hero of Edward Jenner before him. Nor did he ever fail in his allegiance to the man who revolutionised surgery by putting it on a scientific basis more than any other man before the introduction of anæsthetics and antiseptics. A proof of Sharp's engraving of the portrait of Hunter by Reynolds, which had belonged to Syme, hung in Lister's study. He set such store by it, that, when Sir Rickman Godlee asked to borrow it about a fortnight before he died, he said, in giving permission, "As I value it very highly, I should be glad to have it returned to its place at Park Crescent as soon as you have finished with it." †

Lister seldom spoke while he was operating, feeling as he said, that "to introduce an unskilled hand into such a piece of mechanism as the human body is a fearful responsibility."

* Sir R. Godlee, *Life of Lister*, p. 31.
† *Ibid.*, p. 597.

To some surgeons this body is a collection of sewers, but to Lister it was nothing short of the temple of the Holy Ghost. The point of view of the patient towards him is expressed by W. E. Henley in his sonnet "The Chief," written when he was under him in Edinburgh Infirmary:

His brow spreads large and placid, and his eye
Is deep and bright, with steady looks that still
Soft lines of tranquil thought his face fulfil—
His face at once benign and proud and shy.
If envy scout, if ignorance deny,
His faultless patience, his unyielding skill,
Innumerable gratitudes reply.
His wise, rare smile is sweet with certainties,
And seems in all his patients to compel
Such love and faith as failure cannot quell.
We hold him for another Herakles,
Battling with custom, prejudice, disease,
At once the son of Zeus with Death and Hell.

The friends of Syme were the friends of Lister and the enemies of Syme were the enemies of Lister. Syme had views of his own on the treatment no less than the pathology of club-foot and allied diseases. William Adams, a London surgeon, attacked them, and Lister defeated the attacks with a heat that was as lively as Syme's. Indeed he became so closely identified with him that there was some excuse for his father playfully suggesting, "nullius jurare in verba magistri." By June 1854 we see how he spent his day. He rose at 7; visited one of the hospitals from 8 to 10; breakfasted; made notes of what he had seen till 12; operated from 12 to 2.30 or 3; and devoted the rest of the day to correspondence, exercise, and reflection. On September 16, 1855, he is beginning to be immersed in his investigation of the early stages of inflammation.* As Pouchet and his school had spoken of spontaneous generation, so Lister in those days spoke of spontaneous inflammation. Of course Pasteur had not then conducted his researches into the nature of bacteria. Germs are so much in the atmosphere we breathe physically and mentally that it always comes upon us with a shock of surprise when we realise that in 1855 such knowledge was utterly unavailable.

Preoccupied as Lister was with the subject of inflamma-

* *Phil. Transactions,* 1858, CXLVIII, p. 645; Lister, *Collected Papers,* I, p. 209.

tion, he was also occupied with such subjects as the parts of the nervous system which regulate the contractions of the arteries * and the cutaneous pigmentary system of the frog.† On November 7, 1855, he delivered the first of a course of lectures on the principles and the practice of surgery. At first, like most lecturers, he read his remarks or had full notes. In time he trusted less and less to notes, and at last dispensed with them. His early pupils, just as much as his later ones, felt impressed by the personality of their teacher. John Stewart, writing in 1910, describes this magnetic influence: "The difficulty will be for any man to find language to express what our master was to us. We knew we were in contact with Genius. We felt we were helping in the making of history and that all things were becoming new." ‡ The admiration for the genius was mingled with love for the man. "Many of the students of my day," owns Dr. Malloch, "reading of the honours conferred upon their old teacher (late though they were in coming), have seen the page blurred before them and, while returning thanks for the great privilege that had been theirs, must have regretted that they had not made a better use of it." § "In the Hospital wards," confesses Mr. Roxburgh, the last of his Edinburgh house surgeons, "it was not only the healing art which was taught. They were a school of gentleness and human sympathy, and we can well remember the darkening of his countenance as, with stern severity, he rebuked an unthinking student for lifting a broken leg somewhat roughly. In his clinical lectures, which were models of pure English, such expressions as 'this poor man,' or 'this poor woman,' were much oftener heard than 'this case.'" ||

As all medical studies ramify, he soon found that the analysis of the problems of inflammation led him on to consider coagulation of blood which is closely related to it. The observations of even John Hunter did not altogether satisfy him. Lister's first article ¶ on coagulation of the

* *Phil. Transactions*, 1858, CXLVIII, p. 607; Lister, *Collected Papers*, I, p. 27.

† *Phil. Transactions*, 1858, CXLVIII, p. 627; Lister, *Collected Papers*, I, p. 48.

‡ Sir R. Godlee, *Lord Lister*, p. 604.

§ *Ibid.*, p. 604.

|| *Ibid.*, p. 605.

¶ *Edin. Med. Jour.*, 1858, III, p. 893; Lister, *Collected Papers*, I, p. 69.

blood was written in 1858, and his fifth and last * in 1891, a proof that once he took a matter in hand, though now and then the pressure of work forced him to lay it down, it still remained at the back of his mind—to be resumed when suitable opportunity presented itself. Nor was it in any wise a side issue. His labours on the blood proved indispensable to his study of inflammation, and the study of the causes and the prevention of inflammation in wounds formed his outstanding achievement. His paper on spontaneous gangrene contained a lucid account of the facts combined with an equally lucid survey of the principles to which the facts led the investigator. It was read before the Medico-Chirurgical Society of Edinburgh on March 18, 1858, "comfortably read," according to Mrs. Lister, "though unfortunately there was no one at the meeting who seemed capable of appreciating it, and the remarks made upon it were very poor. The President (Professor Millar) was not present, and in his absence the Vice-President, Mr. Benjamin Bell, was in the chair (there is no harm in giving names even though you don't know the people). He (Mr. Bell) said something about the 'ingenuity' of the paper and the valuable *suggestions* which Mr. Lister had thrown out. 'Suggestions!' when the paper contained perfectly clear demonstration of facts having the most important bearings. . . . When we went to dinner the paper was in a most incomplete state, and it required considerable exercise of faith to believe that an hour's more work could bring it nearly to a close. However, about 7 we resumed our labours, and how we did work, Joseph's dictating was really wonderful, keeping me writing as fast as I possibly could, and the sentences flowing out so smoothly, hardly a word having to be altered." † This is only a sample of the efficient assistance she tendered to her husband when he was pressed with work. His practice in Edinburgh did not attain large proportions, and his wife once referred to "poor Joseph and his one patient."

On January 28, 1860, the Crown appointed Lister Regius Professor of Surgery at Glasgow University. He had testimonials from Syme, his father-in-law, Dr. Gourlay, a student attending his lectures, and Dr. Joseph Bell, the

* *Brit. Med. Jour.*, 1891, I, p. 1057; Lister, *Collected Papers*, I, p. 189.
† Sir R. Godlee, *Lord Lister*, p. 72.

original of "Sherlock Holmes." Bell writes: "To the excellency of what you taught us your published papers and the approval of the scientific world bear witness; but to the manner in which it was taught none *can* testify so well as your students. Your Lectures were no mere prelections—the teacher's thoughtfulness compelled the student to think, and his enthusiasm urged his hearers to a like love of science. Neither were they mere scientific curiosities, but at every point the dry details were clothed with life and interest by the manner in which you pointed out the bearing of structural changes as affecting Surgical practice." * The big opportunity had at last come when he was only thirty-two, and he was fitted to seize it.

In Edinburgh Lister had been dwarfed by the personality of his father-in-law, and the numbers at his lectures used to be seven, and we even hear of his beginning one session with one student, who arrived on the opening day ten minutes late! It is accordingly easy to understand the anxiety of the young wife when her husband gave his inaugural lecture. While waiting at home she wrote to her mother-in-law, describing the theatre and "how nice it looks. All so clean and bright—the green baize on the three doors and the diagram-frame setting off the oak colouring, and the bright brass handles on the doors setting them off; and a very handsome slate on a frame on one side, and the skeleton nicely mounted on the other . . . now it is just about 12. Oh! I trust he may be blessed, and believe he will be. His gown will be going on for the first time except when I saw it tried on here. About 5 minutes past! he will be beginning! and how is he getting on?" † She need not have been anxious, for he got on remarkably well with his audience of close on two hundred students. It was a lecture delivered in the spirit of Paget—can one say more? One cannot say less. There was the quotation from Ambroise Paré, "I dressed him, God cured him," and there was the closing reference to the two requisites for the medical profession, first, a warm loving heart, and secondly, truth in an earnest spirit.

No doubt Agnes Lister wrote to her father. At any rate he wrote to his son-in-law one of his characteristic letters:

* Sir R. Godlee, *Lord Lister,* p. 82.
† *Ibid.*, p. 91.

"MY DEAR J.,

I am glad to hear from Ramsay that all went well. It being now established that you can please a large class as well as a small one—or I should rather say still better—the game may be considered in your own hands. Wishing you all comfort in playing it out.

Yours affectionately,

JAS. SYME.

Let me hear your numbers." *

As an examiner he proved as conscientious as he was as a lecturer. In March 1862 he asked the question, Explain the principles on which simple incised wounds ought to be treated? It is significant of the man that he analysed the answer he expected under nine heads with a total value of 15. It is no less significant of his mental growth then that he only allotted 1/30 of the marks to the subject of decomposition. In August 1861 he wrote an article on amputation that appeared in Holmes's *System of Surgery*. There is much in it about inflammation and coagulation of the blood. He observes that we can never be secure against the formation of some pus, and provides instruction as to what should be done in the case of the onset of erysipelas or hospital gangrene. For either was inevitably expected after an amputation in the sixties. In 1863-4 he devised a method of bloodless operating, and "it is remarkable," notes Sir Rickman Godlee, "that this very important advance in surgery made little impression till many years later." †

It is an astonishing fact that during the first half of the nineteenth century surgery had retrograded.‡ Earlier centuries had practised such amateur antiseptics as cauterisations by fir, boiling liquids, and disinfecting substances.

No doubt all such methods are very imperfect, but they were better than nothing. "Pus seemed to germinate everywhere," said a student of that time, "as if it had been sown by the surgeon." § M. Denonvilliers, a splendid surgeon of

* Sir R. Godlee, *Lord Lister*, p. 91.

† *Ibid.*, p. 99.

‡ H. L. F. von Helmholtz, *Vorträge und Reden*, I, p. 361. Cf. his discourse "Ueber das Denken in der Medicin," reprinted in his *Vorträge und Reden*, II, p. 178.

§ R. Vallery-Radot, *Life of Pasteur*, p. 235.

the Charité Hospital, a first-class operator, used to say to his pupils: "When an amputation seems necessary, think ten times about it, for too often, when we decide upon an operation, we sign the patient's death warrant" * Another surgeon declared: "There was no longer any precise indications, any rational provisions; nothing was successful, neither abstention, conservation, restricted or radical mutilation, early or postponed extraction of the bullets, dressings rare or frequent, emollient or excitant, dry or moist, with or without drainage; we tried everything in vain!" † Surgeons had come to think that purulent infection was the inevitable consequence of any important operation. Nor was the tale in Scotland a whit different from that in France. As certain as the course of the planets came with the operation erysipelas, gangrene, pyæmia, purulent infection, and septicæmia, with all their ghastly accompaniments. As early as 1872 Lister guessed that tetanus had a microbic origin, but in 1863 Pasteur had already arrived at a similar stage.

Sir James Simpson collected statistics of more than 2,000 amputations performed in hospitals and more than 2,000 in country practice. His analysis seemed to prove that the mortality was larger in hospitals than in private houses, and that it increased exactly in proportion to the size of the hospitals.‡ He asserted that "the man laid on the operating-table in one of our surgical hospitals is exposed to more chances of death than the English soldier on the field of Waterloo." His remedy lay in the replacement of the hospital by small iron huts to accommodate one or two patients each, and these were to be pulled down and re-erected periodically. The substitution of the hospital by the hut was a heroic remedy, but that it should be suggested at all proved the gravity of the issue. "The question of hospitalism," writes Sir Rickman Godlee, "however, was one of special interest for obstetric physicians (like Simpson) owing to the fearful mortality from puerperal fever in most of the large lying-in hospitals. The controversy with regard to the nature and cause of this disease had been carried on with unnecessary bitterness for twenty years and more; it was still raging, and was continued for many years to come. The

* R. Vallery-Radot, *Life of Pasteur*, p. 235. † *Ibid.*, p. 236.
‡ Sir J. Y. Simpson, *Works*, II, pp. 289-392.

storm centre was the doctrine of one of those unfortunate geniuses who happen to light upon a truth prematurely, but are not gifted by nature with the ability to proclaim it convincingly to the world. Many of those pioneers, whom succeeding generations glorified as heroes, have sunk under the burden of perverse misrepresentation and neglect." *

Some teachers have been so bound up with their own generation that they have been strangers in the outer world—like plants which flourish in one zone and die in the next. Their message may have been effectual, but it was provincial; their accent may have been forceful, but it was a dialect. Other teachers have had such a breadth of thought, such a grasp of principles, that their work could not be confined to a small corner of the earth. Such was Ignaz Phillip Semmelweis. Between the man and his time there must be a certain correspondence. Nothing is more pathetic than the experience of one who has arrived too soon, delivering a message which will be understood to-morrow, but which to-day is a dream; attempting a work which to-morrow the world will welcome, which to-day it considers madness. Such was the fate of Semmelweis. Nothing is more ironical than the effort of one who has arrived too late, for whom there was an audience yesterday, for whose cause there was an opportunity; but now the audience has dispersed, and the field is taken; he has missed his tide, and for him another will not come.

The tragedy of Semmelweis's life is that he arrived too soon, not too late. Born in Buda-Pesth in 1818, in 1846 he was assistant in the huge lying-in hospital in Vienna, to which about 7,000 women were admitted annually. It consisted of two divisions. In the first the male students were admitted, and in the second the midwives were admitted. In the latter the mortality was constantly much less than in the former. For the six years from 1841 to 1846 the average was 3.38 compared with 9.92. The monthly average reached the appalling total of 25 to 30 per cent. Naturally expecting mothers shrank from the first division. Semmelweis felt puzzled by the different rates of mortality. "All this reduced me," he owned, "to such an unhappy frame of mind as to make my life unenviable. Everywhere questions

* Sir R. Godlee, *Lord Lister*, p. 137.

arose; everything remained without explanation; all was doubt and difficulty. Only the great number of the dead was an undoubted reality." *

When pondering over the *veræ causæ,* a colleague died of septicæmia following a poisoned wound received while making a post-mortem examination, and Semmelweis perceived that the disease was identical with puerperal fever and due to the same cause, infection from without. "In my excited state of mind," he owns, "it flashed across me with irresistible clearness that the disease of which Kolletschka had died was identical with that from which I had seen so many hundreds of lying-in women perish."

Questions suggested themselves to the mind of Semmelweis. What forms of antisepsis should be used? Was the eighteenth century, in this respect at least, wiser than the nineteenth? Was chlorine water the best antiseptic? Was infective matter from a dead body the chief cause of disease as he met it? Was puerperal fever caused by decomposed animal organic matter regardless of origin, whether from the dead body or from a living person affected with a disease which produced a decomposed animal organic matter? He provided answers to these questions that ought to have satisfied the practitioners of his day. They remained unsatisfied. He was not an author. He was a scientist who had made a valuable discovery, but he was not able to clothe it with the words that won assent for it. His instructions were carried out by men with no hearty belief in them, and Lister was never more right than in Semmelweis's case when he laid down that two great requisites for the medical profession were a warm loving heart and the pursuit of truth in an earnest spirit. Semmelweis trusted the diffusion of his views to his pupils and his friends. Among the latter were three men of outstanding rank in Vienna, Hebra, Skoda, and Rokitanski. They adopted Semmelweis's ideas, but they did so from the special angle of cadaveric infection. His ideas became lop-sided, and lent themselves therefore to misapprehension. Ridicule and persecution befell Jenner and Simpson, and it befell Semmelweis. Deprived of his appointment, he left Vienna in 1850, and set out for Buda-

* "Die Aetiologie, der Begriff, und die Prophylaxis des Kindbettfiebers," Pest. Wien, u. Leipzig; C. A. Hartleben's *Verlags-Expedition,* 1861, p. 51.

Pesth, his native city. His work for the future was to lie on a smaller scale. His work had to be done, and he did it with that whole-hearted devotion that characterised him even in adversity. On his honeymoon in 1856 Lister visited Rokitanski. By then Semmelweis had left Vienna, and Rokitanski never mentioned him to Lister. It was one of those lost opportunities that do not return, for possibly if the two had met Lister's work in 1865 would have been anticipated by nine years. On what small events do great opportunities turn!

In 1861 Semmelweis* published his *magnum opus,* "Die Aetiologie, der Begriff, und die Prophylaxis des Kindbettfiebers," and the outcome of publication was the even fiercer attacks that its poor author had already encountered. Incensed by them, he met them by pungent "open letters" which simply deepened the antipathy to his views. For four years he continued, almost unsupported, to face misapprehensions of his labours, and the outcome was that his mind became unhinged. The body of Semmelweis was put under restraint. By a strange coincidence he passed away in August 1865, from the effects of blood-poisoning following a wound in his finger inflicted in the course of his duty. He was a martyr to his professional duty, in the ordinary sense of the term, and he was a martyr in a higher sense of the term. For he really died from the assaults of professional men who were neither afraid nor ashamed to be the means of the death of one of the heroic spirits of the nineteenth century. Virchow, even Virchow, had sneered at him as "Der Kerl der speculiert."

Prophets in science in their own day are for the most part unheard. After the lapse of a generation and a half the scientific world awoke to the greatness of the man who had been called "Pesther Narr." Then instead of hurtling stones at him, they placed one over his body. The thousandth part of the compliment paid to him in 1891 would have cheered the lonely fresh investigator to fresh efforts on behalf of mankind—had he heard them during his lifetime. The pity of it is that he never heard them. The last words of his "Die Aetiologie" are words still able to move us after

* On Semmelweis cf. Sir W. Sinclair's moving account in his *Semmelweis, his Life and Doctrine.*

the lapse of the sixty-four years since they were penned: " When I, with my present convictions, look back on the past, I can only dispel the sadness which falls upon me by gazing at the same time into that happy future when within the lying-in hospitals, and also outside of them, throughout the whole world, only cases of self-infection will occur. . . . But if it is not vouchsafed me to look upon that happy time with my own eyes, from which misfortune may God preserve me, the conviction that such a time must inevitably arrive sooner or later after I have passed away will cheer my dying hour."

One is irresistibly reminded of the dying words of Fulton, who was the first to apply the steam-engine to ships. In vain he besought Napoleon to give his invention a chance. Well was it for us that Napoleon was so blind, for Lord Acton thinks that the moment Fulton brought his invention to the greatest genius in war this earth has ever seen was the most dangerous moment of all the long struggle that raged from 1793 to 1815. Fulton asked to be buried near the waters of the Mississippi, for " I long to hear the plash of the wheels near my grave, though I shall never see them."

Oblivion fell upon Semmelweis as it fell upon Fulton for many a long day, and neither Pasteur nor Lister owed anything to him. Semmelweis undoubtedly anticipated Lister in proving that one form of blood-poisoning did not depend on unascertainable causes but on such ascertainable causes as abrasions and external wounds; and in proving that neither the size of the hospitals, nor their age, nor their crowded wards was due to the demerits of what Simpson termed hospitalism.

The recognition of sepsis is no new thing on the part of Semmelweis. The wine in the parable of the Good Samaritan owed its powers of healing to the antiseptic power of alcohol. Benzoin, Friar's Balsam, alcohol, glycerine, chlorine and its compounds, iodine, coal-tar with such derivatives as carbolic acid—all these had been employed. Jules Lemaire served as " pharmacien interne des hôpitaux de Paris," and then became a pharmaceutical chemist. Another pharmaceutical chemist, Le Beuf of Bayonne, drew his attention to coal-tar, and he thereupon conducted an arduous series of investigations into the properties of carbolic acid. He published his results in two books, and the second of them, *De l'acide phénique,* won so much notice that a second edition of it was called for in 1865, within two years of the publication of the first. Working pretty empirically, Lemaire employed carbolic acid for many different diseases, medical as well as surgical. He also used it for hygienic purposes as a disinfectant and for the preservation of foodstuffs. It is to his credit that in 1860 he proposed the use of a weak carbolic solution for the treatment of open wounds, and the following year Dr. Déclat proposed the same remedy.

Not a few surgeons in the sixties did not read any foreign language, save French, with any degree of fluency. This was not the case with Lister, for he read French, German, and some Dutch. He travelled abroad repeatedly. This makes it all the more remarkable that he never met with the articles or the book of Semmelweis. Nor did he meet with the writings of Pasteur independently. His colleague, Dr. Thomas Anderson, Professor of Chemistry, induced Lister to turn his attention to the work of Pasteur. Now in 1856 for his labours on crystallography Pasteur was of sufficiently outstanding position to receive the Rumford Medal of the Royal Society. Apparently had it not been for Anderson, Lister might not have read till much later than 1865 the writings of one who was preoccupied with the science of the problem that concerned Lister as an art.

Lister's study of inflammation had led him to certain definite conclusions regarding wound infection which for several years he had publicly taught:

1. That putrefaction or decomposition—which for him were then synonymous terms—caused suppuration and wound infection; and that wound infection did not occur without suppuration.

2. That decomposition was, in some unexplained way, set up by the air.

3. That the air alone—that is, the gases of the air—did not give rise to decomposition.*

Behind these three conclusions there lay the unanswered question, What is the mysterious relationship between the air and decomposition?

To Lister's absolute amazement he learnt that the missing

* I use Sir R. Godlee's summary in his *Lord Lister,* p. 163.

half of his conclusions lay in the possession of Pasteur. According to the French savant, putrefaction was a fermentation. Was it not caused by the minute growth of minute microbes which were carried everywhere by the dust floating in the air? Was it possible to free the air of this dust by any means? What about filtration? What about heat? What about some means? For there must be some means of freeing the air from pollution. There was room no longer for any mystery in the infection of wounds. Did not the air, or rather the germs in the air, start all the trouble? In a flash he saw that the proper time to employ antiseptics was *before* putrefaction was established, not after. Could he so purify the air before it gained access to the wound that putrefaction should not have a chance to begin? Such was the illuminating idea that began to dawn upon him.

Pasteur's discoveries had been nine years before the world that wanted them so badly. Erysipelas and gangrene, pyæmia and septicæmia were known in every hospital throughout the length and the breadth of Europe. Pasteur, moreover, had plainly indicated in his interview with Napoleon and Eugénie that the end he had in view was the extension of his discoveries in fermentation in wine to fermentation in wounds. "I assured the Emperor that all my ambition was to arrive at the knowledge of the causes of putrid and contagious diseases." Sir Rickman Godlee offers his explanation, which does not quite satisfy us. We read: "In any case it must be remembered that the question of fermentation was in the main a very technical one: of vital importance to brewers and wine-producers, but apparently far enough removed from that of wound-treatment. When at last it attracted the attention of scientific men it interested primarily chemists and only secondarily physiologists and botanists, and lastly, if at all, the medical profession. Busy surgeons, at all events, are not in the habit of reading abstruse chemical reviews; and physiology advances with such rapid strides that the practitioner of medicine makes no pretence to keep up with it. Lister himself"—he was then holding a chair of surgery—"was much engrossed with his own special occupation and investigations, upon each step of which he was in the habit of concentrating the whole of his attention for the time. No doubt they involved a fairly wide survey of recent physiological work, but they were not likely to entice him into the field of chemistry, and they certainly left him little time for promiscuous reading." * The defence would appeal to us more if Pasteur had not definitely indicated his intention of applying his conceptions to the world of disease. Putrefaction and decomposition in the wine cellar were to him precisely the same matter as putrefaction and decomposition in the cells of the human body, and his was the missing half of the knowledge that Lister sorely required.

Lister was ignorant of the work that was being done in his own decade, though that work vitally concerned him. In common with many scientists, even of the year 1925, he was equally ignorant of the genesis of such vital work in the labours of past scientists. Robert Boyle in 1662 had thrown out his flash of inspiration when he gave forth a form of the germ theory. Lavoisier, Fabroni, Thenard, Gay-Lussac, Cagniard-Latour, Schwann, Liebig, Pasteur—all had been in the field before Lister, and the experiments of Lavoisier ran back to the days of the French Revolution. From 1794 to Lister's time is surely a sufficient interval for men to become aware that germs count. Sir Rickman Godlee believes, with reason, that "perhaps the most remarkable characteristic [of Pasteur] was the intuition with which he saw how one discovery led to another. No doubt it is especially true in his case that, to quote his own words, 'in the field of observation chance only favours the mind which is prepared'; no doubt he explored the valleys as well as the heights, but his apparently infallible instinct suggests the quaint apophthegm of Nietzsche, 'in the mountains the shortest way is from summit to summit; but for that thou needest long legs.' Lister possessed such legs. Was their control intuitive? Or, was their control guided?" †

Pasteur was led on by apparently inevitable steps from investigations in crystallography to fermentation in alcohol in 1856. In his writings before 1865 Lister found the following points:

Putrefaction is a species of fermentation.

It is caused by the growth of micro-organisms and does not occur independently of their presence.

* Sir R. Godlee, *Lord Lister*, p. 164. Contrast p. 172 on Pasteur.
† *Ibid.*, p. 171.

The micro-organisms that produce fermentation and putrefaction are carried by the air on the dust that floats. They also occur on and in solid and liquid substances.

These micro-organisms can be destroyed by heat and other agencies or separated from the air by filtration.

Certain recognisable organisms produce definite and distinct fermentative processes.

All of these organisms require oxygen. Some of them flourish only in the presence of free oxygen (aerobic), others only in its absence (anaerobic). The latter acquire their oxygen from the bodies which, by their growth, they are causing to ferment and putrefy.

Many natural animal and vegetable products have no tendency to ferment or putrefy, even in the presence of oxygen, if collected with proper precautions and kept in sterilised vessels.

Spontaneous generation has never been observed to occur, and thus may be regarded as a chimera.*

Lister came to the conclusion, founded on these Pasteurian points, that vibrios or micro-organisms caused putrefaction and that they swarmed in the air. Such a conclusion seems inevitable to us to-day, but it was by no means inevitable then. The proof of this remark is easy, for Lister was really the only surgeon who drew it.† Besides, we have to bear in mind that Pasteur had just given rise to the subject of bacteriology. We are apt to think in terms of microbes to-day, but the men of 1865 were not at all so inclined to think. In fact, we have always to remember that the world of the sixties was a pre-Pasteur world. If we remember this steadily, we have some means of appreciating the grandeur of the simple Listerian conception. Lister showed the way, and once he showed the way it was quite straightforward for others to walk in his steps. He was, however, pre-eminently the pathfinder, and as such we hold him in honour. Men could make an egg stand on its end—after Columbus showed them how, but emphatically it was Columbus that showed them how. The honour is always to the pioneer. The antiseptic system is Lister's, we prefer to give it in his own words: " In the course of an extended investigation into the

* This is Sir R. Godlee's summary on p. 177.
† Spencer Wells is a possible exception.

nature of inflammation, and the healthy and morbid conditions of the blood in relation to it, I arrived several years ago at the conclusion that the essential cause of suppuration in wounds is decomposition, brought about by the influence of the atmosphere upon blood or serum retained within them, and, in the case of contused wounds, upon portions of tissue destroyed by the violence of the injury.

"To prevent the occurrence of suppuration, with all its attendant risks, was an object manifestly desirable; but till lately apparently unattainable, since it seemed hopeless to attempt to exclude the oxygen, which was universally regarded as the agent by which putrefaction was effected. But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended, not on the oxygen or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided without excluding air, by applying as a dressing some material capable of destroying the life of the floating particles.

"Upon this principle I have based a practice of which I will now attempt to give a short account." *

How was he to get rid of the germs present in the air when it approached wounds? There were three methods—heat, filtration, and treatment by a chemical antiseptic. He chose the last because it was at the moment the most practicable, and the chemical antiseptic he chose was carbolic acid, which his colleague, Dr. Thomas Anderson, supplied.

The very first occasion on which he used carbolic acid in the treatment of a compound fracture was in March 1865, but he had a much better instance of its worth in the spring of 1866. The pleasure with which he welcomed the relief his discovery gave is evident in the letter he wrote to his father on May 27: "There is one of my cases at the Infirmary which I am sure will interest thee. It is one of compound fracture of the leg: with a wound of considerable size and accompanied by great bruising, and great effusion of the blood into the substance of the limb, causing great swelling. Though hardly expecting success, I tried the application of

* *British Medical Journal,* 1867, II, p. 246; Lister, *Collected Papers,* II, p. 37.

carbolic acid to the wound, to prevent decomposition of the blood, and so avoid the fearful mischief of suppuration through the limb. Well, it is now 8 days since the accident, and the patient has been going on exactly as if there were no external wound, that is as if the fracture was a simple one. His appetite, sleep, etc., good, and the limb daily diminishing in size, while there is no appearance whatever of any matter forming. Thus a most dangerous accident seems to have been entirely deprived of its dangerous elements." *

The treatment was adopted in case after case, and the results were uniformly successful. Such results had never been obtained before by any surgeon, and we can well imagine the thankfulness that welled from the heart of the discoverer to God when he realised that he was in a fair way to banish the noxious effects of the germs that had devastated every hospital in Europe. "For many years afterwards," records Sir Rickman Godlee, "many who had not seen Lister's practice or obtained similar results themselves positively did not believe that his account of it could be accurate. They had been accustomed to see the opening of such an abscess followed in a day or two by a profuse and evil-smelling discharge, instead of which under the new treatment they were told that only a small quantity of inodorous clear serum, perhaps only a few drops, escaped at each changing of the dressing. They had been accustomed to see the patient pass at once into a hectic state, from which he was too often only relieved by death. Under the new treatment they heard instead of a gradual improvement in general health, while case after case was brought to a successful conclusion. It was perhaps naturally thought that Lister's enthusiasm had made him forget failures and exaggerate successes." † As Newton disclosed to us the new heavens and as Darwin disclosed to us the new earth, so Pasteur and Lister disclosed to us the new redemption of man.

In the memorable paper in the *Lancet*,‡ which appeared in a series of articles between March and July 1867, he records what had actually happened as well as giving a

* Sir R. Godlee, *Lord Lister*, p. 187.
† *Ibid.*, p. 190.
‡ Lister, *Collected Papers*, II.

balanced survey of the causes of infection. Out of eleven cases, two had suffered from hospital gangrene and one had died of hæmorrhage four months after the accident. This explains his saying, "I have had *some* rather sorrowful experience in bringing the method of treatment to a trustworthy state." Still, he had obtained nine successes out of eleven consecutive cases, and this constituted an event of the deepest significance.

Urged by his father-in-law, he prepared a paper on the antiseptic principle in surgery which he read before the British Medical Association meeting in Dublin in August 1867. Criticised as it was by men like Sir James Simpson, this paper on the whole was well received. Criticism, well-informed and ill-informed, soon made its appearance. The *Lancet* recognised what Lister had accomplished, and then proceeded to mix up the discovery of the antiseptic principle with the discovery of the use of carbolic acid. The *Medical Times and Gazette* wrote: "We cannot concede to him the credit of having introduced to the Medical public carbolic acid as a local application." *

Provoked by the attacks made upon him, Lister wrote an unguarded letter which characterised, without mentioning names, Simpson's assault on antiseptics as a feeble attempt to decry them as useless. Stung by this reply and fearing that the new system might threaten the place of his favourite invention of acupressure, Simpson wrote to Lister on June 16, 1865, on behalf of it. In turn, Simpson was forced to undergo the pains of mortification, for men hinted that his work on acupressure had been anticipated by an Italian surgeon, Giovanni de Vigo, in the sixteenth century. Besides, though acupressure had been employed in Aberdeen, it had not been much employed in either Edinburgh or Glasgow. Was not Syme in as leading a position in Edinburgh as Lister was in Glasgow? Did not father-in-law and son-in-law form a conspiracy to condemn a rival remedy?

In September 1867 there appeared an anonymous letter in the *Edinburgh Daily Review,* nominally signed by Chirurgicus but really signed by Simpson, and this letter accused Lister of appropriating credit for what had really been accomplished by Lemaire of Paris. According to Simpson, Lemaire

* *Medical Times and Gazette,* 1867, II, p. 355.

" points out very fully and elaborately its power of destroying microscopic living organisms, germs, or sporules—adduces the opinions of Pasteur, Helmholtz, Schultze, Schwann, etc., and shows its utility in arresting suppuration in surgery, and as a dressing to compound fractures and wounds. He dwells upon its use in many other diseases, medical and surgical." * The last sentence is true, but who would recognise Lemaire's *De l'acide phénique* in the first? Certainly not Lemaire himself. Richard Bentley was fond of saying that no man was ever written down save by himself, and his saying is most true. No antagonist ever wrote Simpson down so much as himself by his conduct in this controversy.

As Lister had never heard of Pasteur till a colleague informed him of his extensive labours, so he had never heard of Lemaire till a rival informed him.

Neither of Lemaire's works was to be found in the library of Glasgow University, and he had to send to Edinburgh for them. In the *Lancet* in October 1867, Simpson returned to the attack on antiseptics.† He accused Lister of gross ignorance of medical literature, and he accused him of being the appropriator of another man's ideas. In fact, Lister's theory was neither new nor true. What was new in it—if there was anything—was not true, and what was true in it undoubtedly belonged to Lemaire. In any case was not acupressure an infinitely better remedy than any antiseptic?

The big hound had barked disapproval, and the lesser hounds hastened to follow his example. Their barks appeared in the *Lancet*, and here are some of them:

" Mr. R. has occasionally sponged the wound, in the operating theatre, before applying the sutures, but not having found any advantage arise from it, he has discontinued the practice." ‡

" Mr. C. does not approve of Lister's method, which he considers meddlesome. Mr. C.'s experience is that wounds unite readily when left alone." §

" A considerable portion of cases have been attended with very satisfactory results; some have conformed in every

* Sir R. Godlee, *Lord Lister*, p. 202.
† *Lancet*, 1867, II, p. 546.
‡ *Ibid.*, 1868, II, p. 634.
§ *Ibid.*, 1868, II, p. 728.

respect with the theory of action promulgated by Professor Lister; but in a good number of instances, while the antiseptic action has been uniformly effective in utterly destroying putrefaction and fetor, yet in regard to its antipurulent properties such satisfactory results have not been obtained from the putty method." *

Lister's carbolic acid treatment was shortened to carbolic treatment and even to the putty method, a phrase that reflected the ignorance of those who used it. Lister felt grievously hurt by the controversy, and on October 13, 1867, he wrote to his father: "I think I have now said all that I am called upon to say, and if I feel sure of that I shall be willing to let people think and talk as they please, and devote myself with fresh ardour to the work that remains to be done in the way of perfecting the methods of the treatment. It is long since I gave up any idea of having any work I might do measured according to its deserts, whatever they might be: and I have always felt that for the editors of these medical journals to take no notice at all of any articles I might write was the best that could happen; so that the good, if there was any, in my work might quietly produce its effect in improving the knowledge and treatment of disease. 'Fame is no plant that grows on mortal soil' is a passage thee quoted to me in a letter many years ago. . . .

". . . I quite agree with what thee say about perfect candour in a discussion of this kind. But the truth is I never thought of such a thing as any merit attaching to happening to be the first to *apply* carbolic acid, whether to a sore, a wound, a fracture, or an abscess. Various antiseptic lotions have long been in use in surgical practice, and as soon as the antiseptic powers of carbolic acid became known, it could not be but that many surgeons would try it, as they had tried other antiseptics. Supposing that I had made the experiment with one of the antiseptics in ordinary use, say chloride of zinc, I really think it likely I should have got very much the same results, had I gone upon the same principles. And supposing I had afterwards learnt that some other surgeon had previously dabbled a preparation of chloride of zinc upon one or two compound fractures, but upon an entirely erroneous principle and so as to lead to no

* *Lancet*, 1868, II, p. 762.

practical result, this would not at all have interfered with my originality in the plan of treatment." *

Still, Lister must have felt cheered by a question asked by the *Lancet* on the results obtained in the Dowlais Iron Works. "Mr. Cresswell, whose surgical experience is very great, says that the use of carbolic acid in the treatment of wounds has revolutionised surgical practice at Dowlais. And yet Mr. Lister's treatment does not find much favour in London. Are the conditions of suppuration different here from those in Glasgow or Dowlais? Or is it that the antiseptic treatment is not tried with that care without which Mr. Lister has always pointed out it does not succeed?" †

In 1866 the chair of Systematic Surgery at University College, London, fell vacant. It was Lister's old college, and if one looked at his standing in Glasgow University combined with the outcome of his researches it seemed as if he possessed a strong claim to the vacancy. On the other hand, John Marshall, a capable if not an outstanding surgeon, was also a candidate, and he had already served as assistant surgeon for eighteen years. In the issue Marshall was elected, though in the light of after events such an appointment makes curious reading. Lister's father-in-law consoled him: "I am glad you take this conduct on the part of U.C. in the right way, not as a discouragement but as an inducement to exertion. In the end you may not improbably have reason to feel grateful for not being allowed to quit your present position. It is a great field, much greater for hospital practice than you could possibly have had in London—and also much more favourable for the acquisition—or, as I should rather say, the extension of professional character. London has its advantages no doubt, but, when these are compared with its disadvantages, your present position, I sincerely believe, is more fruitful of rational happiness. In order to maintain a good metropolitan place it is necessary to do a great deal of dirty work—which I am quite sure you would decline—and therefore have the discomfort of all sorts of worthless persons puffed up as your equals, or superiors, while at present you are perfectly secure from such consequences. It was such considerations that led me to return

* Sir R. Godlee, *Lord Lister,* p. 205.
† *Ibid.,* p. 207.

from London, and they should, I think, reconcile you to not going there." *

On the resignation of Syme in 1869 Sir James Simpson was bold enough to propose that the chair of surgery Syme held in Edinburgh University should be abolished! Seemingly he was afraid that the son-in-law of his detested rival should be appointed, though it does seem amazing that the holder of the chair of midwifery should be anxious on any pretext whatever to put an end to the chair of surgery. It was the misfortune of many to see in his nature ever the malevolent Mr. Hyde, while to others he possessed the benignant figure of Dr. Jekyll. Though Sir James Simpson was prepared to go to such lengths, Lister was appointed. The Professor of Materia Medica at Anderson's University, Dr. James Morton, had been Surgeon to the Glasgow Royal Infirmary. How little he cared to keep in touch with progress in his profession is evident from the circumstance that he never set foot in one of Lister's wards in order to observe for himself what sort of treatment the antiseptic was. This did not in the least prevent his stating that "Pasteur's theory in regard to the existence of certain spores or germs in the air" had not been proved—to his satisfaction.† Even if there were germs, they were neither injurious nor did they cause "suppuration of a bad kind." The astounding point is that a colleague of Lister's should stoop to make such an attack without knowing a single thing practically of antiseptic treatment. There are none so blind as those who won't see, and Morton belonged to this terrible variety of scientific man.

At the meeting of the British Medical Association at Leeds in 1869, Nunneley, a prominent surgeon in the city, attacked Listerism and Pasteurism. The arguments of Pouchet and Hughes Bennett (1812—1875), the Professor of Physiology in Edinburgh, who opposed the germ theory,‡ were put forward. Careful physician and physiologist, Bennett was a careless controversialist, who did not fail to stir up much antagonism. Nunneley admitted that he had never tried

* Sir R. Godlee, *Lord Lister,* p. 211.

† *Ibid.,* p. 250.

‡ J. H. Bennett, "The Atmospheric Germ Theory" (*Edin. Med. Jour.,* 1868, XIII, p. 816). Cf. *Brit. Med. Jour.,* 1869, II, p. 256.

the antiseptic method, but his colleagues had! Their results were unsatisfactory, and its employment in Leeds was exceptional. Writers in the local press, even from Glasgow, supported Nunneley.

Simpson passed away in May 1870, and Syme in the following June. "The two most outstanding personalities, Syme and Simpson, had both received unmistakable warnings that their warfare was accomplished, . . ." writes Sir Rickman Godlee. "The removal of these two men, as it were, cleared the atmosphere of the medical world in Edinburgh, and in very different ways affected Lister's position there. Syme and Simpson had for the most part been in conflict, and each had so many devoted followers that it was not easy to avoid being a partisan of one or the other. This led to a cleavage in the ranks of the profession and some bitter feelings, which only gradually declined after the disappearance of the protagonists.

"This is not the place to form an estimate of Simpson's character. Few men who could claim so many friends had so many detractors. For the former he was the embodiment of all the virtues; the latter were unable to speak of him with moderation. We have already seen the position which he took up with regard to Lister and his work, and the way in which he conducted his opposition. If he had lived longer and they had been constantly thrown together in the University and civic affairs, it is hardly to be supposed that further causes of friction could have been avoided. Simpson was one who entered into such contests *con amore*; to Lister they were repugnant and distressing to the highest degree.

"The death of Syme was an unmixed sorrow. Coming as it did, soon after that of his own father, it emphasised the fact that the last link with the preceding generation was severed. It was one of Lister's characteristics to admire some men with a whole-hearted devotion that hardly admitted the possibility of faults, and Syme was conspicuously one of these. In the *Scotsman* of June 28, 1870, there was an obituary notice the authorship of which was attributed to Lister, and was never disowned by him. . . . It ends with the following words: * 'The hostility which he excited in a few was greatly outweighed by the friendship he inspired in the

* Sir R. Godlee, *Lord Lister*, p. 253.

many. Rarely is it granted to any one to attach to himself the enduring love and admiration of so large a number of his fellow-men.' " *

Lister, on his father-in-law's death, stepped into his position as the first surgeon in Scotland. At Edinburgh University he numbered among his medical and surgical colleagues men like Patrick Heron Watson and James Spence, Matthews Duncan and Thomas Keith. The physicians included men of rank equal to that of the surgeons, for they were Laycock and Fraser, and Grainger Stewart. "It was," in the competent opinion of Sir Rickman Godlee, "a self-contained, highly intellectual University circle—somewhat cliquish, no doubt, but becoming less so. The conflicts of the past were not completely forgotten, but a calmer and more charitable spirit was descending upon Edinburgh, as well as upon the rest of the scientific world. Doctors, at all events, were becoming accustomed to the idea"—in the year of grace 1870—"that it was not only possible, but far best, to conduct their discussions in moderate language unblemished by personalities." †

The master requires disciples if his teaching is to prevail. If the pupils possess the graceful style of a Huxley or the attractive eloquence of a Tyndall, so much the better for the diffusion of his views. Darwin was supremely fortunate in owning such a master of English as Huxley as his disciple, for Huxley constituted himself "Darwin's bull-dog." Just as Darwin's motto was peace at any price, so Huxley's was war whatever its cost. Though Lister himself wrote many articles, he never wrote a book. Darwin wrote *inter alia* the *Origin of Species*, and Sir J. D. Hooker and T. H. Huxley are not the only readers who found it to be a tough morsel to digest. ‡ John Tyndall (1820—1893) came to the rescue of Lister, as Huxley came to the rescue of Darwin, and rendered Lister undoubted assistance. Attractive as a writer, Tyndall was even more attractive as a speaker. While engaged upon a series of striking experiments on the decomposition of vapours by light, he observed that a luminous beam, passing through the motionless air of his experimental

* R. Paterson, *Memorials of the Life of J. Syme*, p. 327.
† Sir R. Godlee, *Lord Lister*, p. 254.
‡ L. Huxley, *Life and Letters of T. H. Huxley*, II, p. 192.

tube, was invisible. The happy thought occurred to him that such a beam might be utilised to detect the presence of germs in the atmosphere. For air incompetent to scatter light, through the absence of all floating particles, must be free from bacteria and their germs. Experiment after experiment yielded the conclusion that "optically pure" air is incapable of developing bacterial life. In properly protected vessels infusions of fish, flesh, and vegetables, freely exposed after boiling to air rendered moteless by subsidence, and declared to be so by the invisible passage of a powerful electric beam, remained permanently pure and unaltered. On the other hand, when the identical liquids were exposed afterwards to ordinary dust-laden air, they soon swarmed with bacteria. In spite of these experiments and in spite of the experiments of Pasteur and Lister, Dr. Bastian could still persist in the belief in spontaneous generation.

As Huxley thought that a cobbler like Kelvin ought to stick to his last in the shape of physics, and not intermeddle with geology, some scientists thought that a cobbler like Tyndall ought to stick to his last in the shape of physics, and not intermeddle with bacteriology. The *Lancet* furiously denounced Tyndall for stepping outside his province. Even Lister wrote to his brother: "It is almost a pity that Tyndall should have meddled with things beyond his beat." * Tyndall himself pertinently pointed out: "My own interference with this great question, while sanctioned by many eminent names, has been the object of varied and ingenious attack. On this point I will only say that when angry feeling escapes from behind the intellect, where it may be useful as an urging force, and places itself athwart the intellect, it is liable to produce all manner of delusions. Thus my censors, for the most part, have levelled their attacks against positions which were never assumed, and against claims which never were made." † To this position we can guess how sympathetically Jenner and Simpson, Lyell and Helmholtz, Darwin and Pasteur would have listened, for had they not all suffered

* Sir R Godlee, *Lord Lister*, p. 280.

† J. Tyndall, *Essays on the Floating Matter in the Air in Relation to Putrefaction and Infection*. p. 27.

grievously "when angry feeling escapes from behind the intellect"*?

It is amazing to note how the specialist scientist seems to imagine that no one, save the man working in his own narrow field, possesses the slightest right to examine—or, worse still, criticise—his results and the conceptions underlying them. Darwin used to contend that intelligent men were those who most readily gave adhesion to his views, while those whose studies qualified them to grasp particular portions of his investigation, for the most part, stood aloof. The curse of science is the narrow-minded specialist who cannot see a single inch beyond his nose. Such a man is absolutely of the mind of Cardinal Newman, who did not ask to see the distant scene. His aim is:

> Lead, kindly Light, amid the encircling gloom,
> Lead thou me on;
> The night is dark and I am far from home,
> Lead thou me on.
> Keep thou my feet; I do not ask to see
> The distant scene; *one step enough for me.*

The man who is going to leave his mark in science must be a man capable of watching the distant scene as well as watching the ground under his feet. Tyndall did see the distant scene, and therefore he wrote with strength: "I am dealing with a question on which minds accustomed to weigh the value of experimental evidence are alone competent to decide, and regarding which, in its present condition, minds so trained are as capable of forming an opinion as regarding the phenomena of magnetism or radiant heat. 'The germ theory of disease,' it has been said, 'appertains to the biologist and the physician.' Where, I would ask in reply, is the biologist or physician, whose researches, in connection with this subject, could for one instant be compared to those of the chemist Pasteur? It is not the philosophic members of the medical profession who are dull to the reception of the truth not originated within the pale of the profession itself." * Herein Tyndall was seriously mistaken. For do we not find such philosophic members of the profession as Sir James Simpson and James Morton, and Hughes Bennett, not to speak of such men as Mr. R. and Mr. C., all stoutly opposed

* J. Tyndall, *Essays on the Floating Matter in the Air in Relation to Putrefaction and Infection*, p. 42.

to the reception of truth not originated within the profession?

Tyndall intervened in the dispute about germs possibly because he was an Alpine climber and probably because he cared to seek a general view of truth. "Il faut cultiver notre jardin" is the motto of some scientists who only cultivate the merest patch. "Il faut cultiver notre jardin" is the motto of some scientists who, though they do not cultivate the whole of their garden, insist in taking a comprehensive survey of it. The Tyndall type is surely the finer type of the two. Such a type seeks truth, but he seeks truth that enables him to glance all around him. He seeks truth, but he seeks it with beauty. In fact, the scientist is at bottom a creative artist who takes a Terentian view of his domain. "Le savant," wrote the wonderful Henri Poincaré, "n'étudie pas la nature parce que cela est utile; il l'étudie parce qu'il y prend plaisir, et il y prend plaisir qu'elle est belle. Si la nature n'était pas belle, elle ne vaudrait pas la peine d'être connue, la vie ne vaudrait la peine d'être vécue." There is, of course, the bread-and-butter view of science, but does man either in science or—for the matter of that—in anything else live by bread alone?

A colleague of Lister, Thomas Keith, had done remarkable pioneer work in ovariotomy. Keith and Spencer Wells had skilfully reduced the mortality rate in this operation to 30 per cent. It is a measure of the progress of surgery to say that the present rate is 5 per cent. Yet there are sceptics who tell us that the main task of the surgeon has been to devise complex operations for the fresh diseases he has discovered, darkly hinting that he himself is, in some mysterious fashion, the cause of these diseases. Keith's success was partly due to his manual dexterity and partly due to his amazing attention to all manner of details, especially in the matter of cleanliness. By 1870 he had reduced his mortality after ovariotomy to 16 per cent., which was a startling result. Should he, in order to diminish even this rate, adopt the antiseptic treatment? Should he adopt the spray which created an antiseptic atmosphere? Lister himself felt doubtful in this particular instance, for the peritoneal cavity possesses natural protections against the attacks of micro-organisms. Keith, however, decided to adopt the plan of his colleague. According to Lister, "on at length adopting strict antiseptic measures with an improved spray, for a while he surpassed himself by an unbroken series of eighty successful cases. Yet, wonderful as this achievement was, it was only a difference in degree from his former experience, and assuredly no absolute proof of superiority of the new means employed." * Keith afterwards abandoned the spray, partly, it is believed, because he thought its effect injurious to the patients, partly because it was certainly injurious in the highest degree to himself. He continued, however, to use the other antiseptic precautions, and to this he attributed the superiority of his later results.

As time went on, for we must now look some distance ahead, the question was discussed with bitterness, almost with ferocity, amongst the ovariotomists. Some, whilst disowning the antiseptic principle root and branch, apparently unconsciously adopted much of its practice; others adopted both principle and practice with the exception of the spray, and there were yet others who omitted no jot or tittle of Lister's methods. Meanwhile it was obvious to candid observers, through all this strife of tongues, that, although the statistics of the antiseptic surgeons were on the whole the best, some at least of their opponents could show very excellent results indeed; and this provided a stumbling-block to many, though not to Lister.†

Lister was the very man who insisted in taking a comprehensive view of the garden he so assiduously cultivated. Among the many advantages of such a plan, it preserved that openness of mind that is so essential in the task of discovery. Even the man with the open mind welcomes something in accordance with its main bent more cordially than something that tells against that main bent. For in the course of time an inquiring mind takes up a certain attitude to some truths which means, at bottom, that he does not care for other truths not quite on the same lines as those he adopts. In 1883 Metchnikoff announced his discovery of phagocytosis, a discovery that supplemented

* Lister, *Collected Papers*, II, p. 276.

† Sir R. Godlee, *Lord Lister*, p. 288. Cf. *Brit. Med. Jour.*, 1869, II, p. 335.

the work that Pasteur had begun.* If a proof of the internationalism of science is required, here it is. A French chemist does part of the task, and a Russian pathologist complements it. Pasteur had discovered the existence of the micro-organism, Metchnikoff had observed the actual contest between the invading micro-organisms and the phagocytes or defending living cells. If the phagocytes win, they devour the micro-organisms. If they lose, the micro-organisms devour them. Eat or be eaten—such is the law of their life. The Russian discoverer afforded Lister another proof of the correctness of what he was doing, and we can imagine the glee with which he welcomed such a result. The use of the spray was causing Lister searchings of heart, but the use of the spray was not fundamental to his antiseptic method. If, to take a present-day example, Metchnikoff had altered his ideas on germs as much as Einstein has altered Newton's ideas, what would have happened? Would even Lister's openness of mind have stood the test of anything so unwelcome?

Lister kept his treatment before the members of his profession. He delivered, for example, an address before the 1871 meeting of the British Medical Association at Plymouth.† In 1875 he wrote a paper extending over no fewer than seven numbers of the *Lancet*.‡ The same year he gave two impressive demonstrations of antiseptic surgery before the 1875 meeting of the members of the British Medical Association at Edinburgh.§ The heads of the profession listened, and not a few of them went on in their accustomed ways. The young graduates came to Glasgow to see for themselves, and they were not sufficiently hardened by the customs of their profession to keep their eyes blinded and their ears shut.

Sir Rickman Godlee surveys the situation in the seventies, and holds: "And yet it is not too much to say that in London it [i.e. Listerism] made but little way until these younger men had reached a sufficient degree of seniority to

* *Biol. Centrabl.*, 1883, III, p. 560.

† *Brit. Med. Jour.*, 1871, II, p. 225; Lister, *Collected Papers*, II, p. 172.

‡ *Lancet*, 1875, I, pp. 365, 401, 434, 468, 603, 717, 787; Lister, *Collected Papers*, p. 206.

§ *Edin. Med. Jour.*, 1875-6, XXI, pp. 193, 481; Lister, *Collected Papers*, II, p. 256.

have the charge of wards, and that, even in Edinburgh and Glasgow, some of the old spirit and much of the old practice prevailed well on into the seventies, when Lister's old pupils began to occupy positions on the full staffs of their hospitals.

"This may seem strange to those who were not taught by, and have not served under, the surgeons of the old school, or who do not appreciate the limitations imposed upon assistant surgeons in those days. There was not, so to say, more than enough of either hospital or private practice, at that time, to go round, as there is now; and the seniors stuck with much tenacity to their posts, thus allowing but little opportunity to the juniors to expand themselves. Night and day, Sundays and week days, they would turn out to do their 'casualty' work. Visits to continental schools, week-ends in the country, and long summer holidays were almost unknown, and the orthodox point of view was that of a well-known surgeon who said, 'My moors are in Old Burlington Street.'

"It is only fair to make allowances for these men and to add that the description applies only to a class, amongst whom there were striking exceptions. They were following in the steps of their predecessors. They had been brought up to think much of the art and comparatively little of the scientific side of surgery. Their minds were not trained for the reception of such an abstract idea as that of the germ theory, or to weigh the arguments of its supporters against the louder and more incessant replies of its opponents. For the most part indeed they were convinced that it was the dream of cranks and enthusiasts. It was therefore incredible to them that the true light had been revealed to such men, that their own time-honoured creed about inflammation was a delusion, and that a complete change in their methods of treatment was essential.

"Still, for decency's sake, the thing had to be given a trial. But it was done in a perfunctory manner, with a scornful half-faith or no-faith; and as the new method involved great difficulties, even for enthusiastic disciples working in the old surroundings, it is no wonder that *the seniors, almost without exception, failed to obtain Lister's results. Their failures caused them but little sorrow, but rather the satisfactory feeling that, after all, this new doctrine* *had nothing in it, and that they had not all their lives been following cunningly devised fables.**

"When I was a student full of the confidence of youth, and doing my best to educate my superior officer—no less distinguished a man than Sir John Erichsen—I lamentably failed with a compound fracture, for reasons now easy to recognise, and received from him the stern injunction, 'No more antiseptics.' Erichsen was by no means a bigoted man, and he was always very friendly to Lister. He was simply old-fashioned, and would have paid but little attention to, and perhaps hardly have understood, Lister's explanation of my failure." †

There is no reason to think, in spite of Sir Rickman Godlee's reasoning, that the medical profession is really a much more grievous offender in its rejection of the new ideas that occasionally come into contact with its members. Lyell found the same want of an open mind in geology, and the precursors of Darwin found the same want of an open mind in evolution as Darwin himself experienced. In some respects the case of Joule and Helmholtz is even more significant. For the doctrine of the conservation of energy did not touch any of the ordinary prejudices of scientists, yet Joule and Helmholtz found the same want of an open mind as Lyell and Darwin.

No form of literature is more attractive than autobiography when it is thoroughly sincere, as Sir James Paget's is. It is this quality combined with its simple style that gives this book its literary charm. Sir James takes the reader into his confidence and lifts the veil from his early life. As he was a man of as rare nobility of character as Lister himself, the book in which his character is enshrined is extraordinarily well worth reading. His son, Mr. Stephen Paget, tells the story of the later life of the great surgeon, and the outcome is a volume of surpassing interest. Why is it that such typical men of action as soldiers and statesmen, travellers and adventurers, seldom have great biographies? Why is it that the literary and scientific men have such great biographies? The lives of Johnson and Macaulay stand as high in the class of literary men as Sir

* I italicise these words.
† Sir R. Godlee, *Lord Lister*, p. 315.

Rickman Godlee's *Lord Lister* and Mr. Stephen Paget's *Memoirs and Letters of Sir James Paget* stand in the class of scientific men.

We have spent our scanty leisure in reading hundreds of the biographies of men and women of all ages, and we do not know a single man we respect more than Sir James Paget. To us, therefore, it is a personal sorrow to find that even he made the following comment on a case of compound fracture of the leg treated by Lister's method:

"Collodion was put on at once, and then carbolic acid applied. You know we are trying the effects of carbolic acid for compound fractures and some other forms of injury, after the manner which has been so strongly recommended by Professor Lister. In this case I would say that the carbolic acid was applied, if not with all the skill that Professor Lister would employ it, yet with more than is ever likely to be generally used in the treatment of fractures; and yet it certainly did no good. I will not say that it did harm; if it did harm, it was rather through my fault in leaving it too long when the wound should have been left open to discharge itself. But, at any rate, carbolic acid, applied here with a considerable amount of care and skill, failed altogether to attain its end; for, three days after the fracture, we observed that the limb was becoming the seat of inflammation of the acutest kind." * So in 1869 Paget informed his students when he gave them a clinical lecture at St. Bartholomew's. When the leading surgeon in a leading London hospital gave such qualified condemnation to Listerism, we can imagine what other surgeons were saying at their clinical lectures. As Paget was warmly respected by the members of his profession and as his language was studiously moderate, the harm he did was incalculable. In a similar fashion Tyndall did injury to Lister, for his lectures tended to concentrate the attention of surgeons upon the air as the prime source of infection. With dejection of spirit Lister replied to the lecture of Paget. He pointed out imperfections in Paget's treatment of technical interest. The fact, however, was that assistant surgeons

* Sir J. Paget, Clinical Lecture on the Treatment of Fractures of the Leg in the *Lancet*, 1869, I, p. 317. Cf. *ibid.*, 1869, I, p. 380; 1870, I, p. 91; 1873, II, p. 182.

had to be trained in the antiseptic treatment, and occasionally they came for a single day to Glasgow, and such a flying visit was worse than useless. For it made surgeons confident that they understood the details to be employed at operations, whereas they did nothing of the kind. In any revision of the Prayer-Book, there ought to be such a clause as "Save me from my friends." The scientist feels inclined to vary this clause, "Save me from my ill-instructed friends" (who, of course, fondly believe that they are well-instructed).

We are afraid that the influence of Sir James Paget dominated London medical opinion. At any rate, there was little progress effected by the antiseptic methods. At the 1873 meeting of the British Medical Association in London, John Wood, of King's College Hospital, spoke unfavourably of it.* We are so anxious to represent the contemporary attitude of hostility correctly that we venture upon another quotation from Sir Rickman Godlee's biography:

"On the whole, as time went on, there was a tendency for apathy to pass into opposition. To mention antiseptic surgery was to cause irritation, or at least to elicit a scoff or a sneer; and Lister's name became to London surgeons like that of Aristides the Just to the Athenians.

"If any one should think that *the case against the majority of London surgeons* † has been overstated they should read the report of a confused debate at the Clinical Society of London which took place in 1875.‡ The antiseptic system was introduced incidentally on the report of certain cases supposed to have been treated antiseptically, but in none of which the treatment was properly carried out. It is almost incredible that the leading London surgeons could unblushingly discuss such a vital question in so loose a manner. It can only be explained by the assumption that they had no conception of its importance; and yet the meeting took place very shortly after Lister's striking demonstrations to the British Medical Association at Edinburgh.§ The comments of the *Lancet* are instructive:

"'Mr. Lister and his disciples are themselves to blame

* Sir R. Godlee, *Lord Lister*, p. 319.
† I italicise these words.
‡ *Lancet*, 1875, II, pp. 562, 628, 737.
§ *Ibid.*, 1875, II, p. 565.

for much of the obscurity that overshadows this question, inasmuch as they have never yet openly and fairly met the challenges that have been thrown out to them to produce the statistical results of their practices, say, for five or six years past. . . . Notwithstanding the very able papers on the recent improvement in the details of antiseptic surgery which Mr. Lister lately published in these columns, followed as they were by demonstrations at Edinburgh at the meetings of the British Medical Association, there is less antiseptic surgery practised in the metropolitan hospitals than ever there was.'

"The article goes on to refer to other rival systems, such as the 'open method' advocated by Humphry of Cambridge, that of Spence who used warm water and tincture of iodine, or that of Callender who trusted to cleanliness alone, for all of which brilliant results had been claimed. Further on it is said, 'Oddly enough, there is at least one metropolitan hospital where one surgeon follows, strictly and exclusively, Lister's plan, and the other surgeons as persistently reject it. Here, one would think, there is some ground on which to stand and view impartially the merits of the two modes of practice. What is the result? After the experience of several years, not to put too fine a point on the matter, it is found that the success of the antiseptic system is certainly not greater than that of the ordinary methods, and it is stated to be actually less.'

"The following week the *Lancet* returned to the charge: 'Happily, it is no part of the business of the clinical surgeon to bolster up theories, be they good or bad, or to make facts rigidly conform to them. The germ theory may be perfectly well founded; but nine surgeons out of ten do not care much whether it is or not, so long as they cure their cases and reduce their mortality to the lowest possible degree.' * In these words, 'such is the measured judgment of Sir Rickman Godlee,' the mental attitude of the average London surgeon in 1875 was accurately described." † Nor can we forget that one of the leading London surgeons, Sir James Paget, and one of the leading Edinburgh surgeons, Sir James Simpson, had pronounced similar verdicts.

* *Lancet,* 1875, II, p. 597.
† Sir R. Godlee, *Lord Lister,* p. 320 ff.

Now we can well understand that in the case of rare diseases like Vincent's angina and bulbar paralysis, elephantiasis and exfoliative dermatitis, a surgeon should suspend his judgment. But the diseases Lister set out to combat were of everyday occurrence, and were levying a frightful toll of death throughout the whole of Europe. Of course it is difficult to obtain exact figures, but the following rough-and-ready ones are sufficient to enable us to grasp the gravity of the situation facing every surgeon, wherever he was practising. The result was that a mortality of 24 to 26 per cent. of cases of major amputations of limbs was considered very satisfactory in London. In Edinburgh the death-rate after such operations was 43 per cent.; in Glasgow, 39 per cent.; in Paris, 60 per cent.; in Zurich, 46 per cent.; and in Vienna, 43 per cent. In the Army, the death-rate sometimes reached such appalling proportions as 75 to 90 per cent. Nobody, save the antiseptic people, quite knew why. Surely it constitutes a grave indictment of the medical profession during the seventies, when Listerism was well under way, to note how callously the average surgeon behaved when what promised to be a remedy was put before him. One would have thought that he was prepared to try anything whatever—if it gave the least promise of a cure for such terrible rates of mortality. It was not enough to read about Listerism. We should have thought that surgeons would have emigrated on the spot to Glasgow or to Edinburgh in order to learn at first-hand the proper fashion to employ the antiseptic apparatus. One thinks of the patients who suffered from erysipelas and gangrene, septicæmia and purulent infection, and one sorrowfully reflects on all the lives that might have been saved had surgeons preserved somewhat of an open mind. One patient lingered on in pain and discomfort. Another counted the days of sickness, not by pain and weariness, but by the sufferings of those who were left at home without a guide, and, perhaps, starving. Suffering there is and there, one supposes, must be, but what are we to say to avoidable suffering? If, to use a pregnant remark of King Edward, preventable, why not prevent it?

There is the man who can criticise and there is the man who can construct, and we must never forget that there is a legitimate place for criticism. For often before a reform can be effected old obstructions must be swept away, and in this task of destruction the work of the critic is essential. To take an historical example, the destruction wrought by the men of the first French Revolution was necessary if any true conception of liberty was to become a working reality. For that work men like Diderot paved the way, though Voltaire did much less than people imagine. For though he attacked much that deserved attack, at heart he was a dogmatist of dogmatists, a fact that seems to escape Lord Morley in his penetrating study of the French satirist. Still, though there is the critic who is wanted, is there not a critic who is not wanted, the critic who simply spends his own time—and wastes that of others—in criticising? Lister, like all pioneers, met this class of critic. Sir William Scovell Savory (1826—1895) did not quite belong to this class. In that wonderful year, 1859, he succeeded Sir James Paget as lecturer on general anatomy and physiology at St. Bartholomew's Hospital. Though his lectures differed from those of his great predecessor, they were no less admired by those who attended them. The emolument he received for his clinical duties and lectures in 1881-2 exceeded £2,000, probably the largest income ever received for surgical teaching in London. The mediæval fashion of putting down awkward questioning was to invoke the phrase, "Ita scriptum est." The fashion of Savory was no less effective, for he spoke as a great authority, delivering final judgment on the problems of surgery. We are irresistibly reminded of Fustel de Coulanges, the French historian. "Do not applaud me," he said one day to an enthusiastic audience, "it is not I that speak to you but history that speaks by my mouth." He regarded his results as independent of himself, and criticism as something like blasphemy. The dissent of competent scholars never led him to modify his conviction not only that he had reached truth but that truth is easy to reach.

Savory possessed the solid conviction that he had reached truth and that Lister had reached error. He attended the 1879 meeting of the British Medical Association at Cork, and there pronounced a verdict against Listerism and its works, and he fondly believed that his verdict was the verdict of science. For fourteen years antiseptics had been in the possession of the medical profession—if its members cared to employ them. With his dignified features and with his distinct voice, he pronounced the doom of the new ideas, and he pronounced it with all the confidence that sprang from his prominent position in London surgical circles. He set his points in so strong a light, and placed his contention on so solid a basis, that it seemed impossible that any answer could be found to his weighty manner and his no less weighty—so it seemed to him—conclusions. One felt, as his periods rolled forth, as if the voice of pure reason was speaking through his lips. Lister had attempted to prevent suppuration. Were his attempts a whit more successful than his own? Were they even as successful? He considered his annual average of about 6 cases of pyæmia, 20 of erysipelas, and 26 of blood-poisoning following operations represented as good a result as it was reasonably possible to expect.* He concluded by making the usual demand for statistics.

The demand for statistics was easy to make and difficult to supply. Ridiculous people prefer the charge that figures can prove anything. They can do nothing of the kind in the hands of skilled statisticians who are seeking the truth. Figures can be *made* to prove anything in the hands of the partisan trying to establish a case. Savory was such a partisan. Lister undertook operations that Savory would not dream of undertaking. How could anyone compare Lister's operations with Savory's? What was required were statistics showing the average of cases in which suppuration occurred and the incidence of septic diseases among compound fractures and after operations when no previous wound was present. Unless such considerations could be taken into account, what use were the ordinary bald statistics of mortality after amputations? Such considerations were rightly present to the mind of Lister when he refused to embark on a contest where the odds would have been heavily weighted against him, a fact of which Sir William Savory was perfectly aware.

From 1859 Pasteur had been investigating the origin and the reality of microbes and from 1865 Lister had been

* *Brit. Med. Jour.*, 1879, II, p. 212.

putting these investigations to practical account. That is, in 1879 Savory had had twenty years to meditate over the labours of Pasteur and fourteen years to meditate over those of Lister. The amazing outcome of his meditations was the worthlessness of the antiseptic method! "Savory's address," records Sir Rickman Godlee, "is still spoken of as the swan-song of the already dwindling race of pre-antiseptic surgeons. It expressed, however, the views of a considerable proportion of the senior members of the staffs of the London and provincial hospitals at the time. Like a poultice, it warmed and comforted the soul of many a middle-aged man, who had begun to feel the discomforts of an undermined faith; though it almost made some of the younger men, to whom time passes but slowly, despair of the future." *

At an influential meeting in December 1879 of the South London Division of the Metropolitan Counties Branch of the British Medical Association Lister dealt with Savory's Cork address. He met the statistical charge preferred against him, and he emphasised the new facts he had proved, saying, "If these matters have not attracted attention, it cannot be because they are not worthy of it; I presume it is because I have not the capacity to bring them before my professional brethren with sufficient force to impress them upon them. It is not, I say, that these things are unimportant; but that they are not believed." † His peroration should have moved any surgeon, even Sir William Savory: "I feel I owe an apology to the meeting for having detained it so long, and I return you my sincere thanks for having listened to me so patiently. In such a gathering of medical men as I see before me I cannot avoid speaking warmly on a matter so near to my heart. I have been charged with enthusiasm; but I regard enthusiasm with reference to the avoidance of death, pain, and calamity to our fellow-creatures as a thing not at all to be ashamed of; for I feel this to be a matter of which I may say in the words of Horace:

> Aeque pauperibus prodest, locupletibus aeque,
> Aeque neglectum pueris senibusque nocebit." †

* Sir R. Godlee, *Lord Lister,* p. 323. † *Ibid.,* p. 324.

There were practically only two London surgeons who were moved to carry out what this speech eloquently and reasonably suggested, and they were (afterwards Sir H. G.) Howse of Guy's and Marcus Beck of University College. Sir William Savory was in no wise moved: such men never are. Even at the Seventh International Medical Congress, held at London in 1881, he referred to his Cork address as still the epitome of his creed. Nor did he stand alone at this Congress, for Sampson Gamgee (1828—1886), an old fellow-student of Lister's, eloquently supported Savory.

The debate of 1879, according to Sir Rickman Godlee, "also clearly showed how the often unconscious adoption of portions of Lister's practice interfered with the acceptance of the doctrine as a whole. From the very first the antiseptic leaven began to work and led to a general diminution of hospital diseases, which was forthwith attributed entirely to improvements in hospital hygiene. And yet, though this was the favourite view to hold and to express, one surgeon after another made attempts to obtain results equal to Lister's by simpler, or what were thought to be simpler, means. One satisfied his conscience by merely substituting oakum for lint as a dressing, another by supplying weak solutions of iodine to his wounds. Callender of St. Bartholomew's had a system of his own which Lister described as 'a thoroughly antiseptic treatment,' and Jonathan Hutchinson of the London Hospital kept his dressings constantly soaked with spirit of wine and lead lotion. So did Croft and MacCormac, who followed Lister's practice implicitly, though not with complete success, in the fine new St. Thomas's Hospital—the first up-to-date pavilion in London." * It is enough to startle one to ascertain that "long after Lister came to London [in 1877] it was hard to find a dozen surgeons in the metropolis who were really competent to carry out the antiseptic treatment of a serious case." †

The Irish surgeons in Dublin and Belfast, with a few notable exceptions, ignored the innovation of Lister or laughed at it. Even while he was at Edinburgh, Lister received no support from his colleagues on the senior staff,

* Sir R. Godlee, *Lord Lister,* p. 324.
† *Ibid.,* p. 326.

though he secured adherents among the juniors. The surgeons of the Royal Infirmary, Glasgow, pretended to give the new treatment a trial, but the clear evidence is that "the thing was a sham." * Nor is it true to hold that antiseptics were more readily accepted abroad. The prophet, save in Germany, Switzerland, and Scandinavia, had no more honour abroad than he possessed at home.

In Germany Ernst von Bergmann of Berlin, Tillmans of Leipzig, Thiersch of Leipzig, and his intimate friend, Richard von Volkmann of Halle, Esmarch of Kiel, König of Göttingen, Trendelenburg of Bonn, Nussbaum of Munich, and Stromeyer of Hanover, adopted the antiseptic treatment. Von Langenbeck of Berlin preserved an attitude of neutrality. Thamayn of Halle, and a man in such an outstanding position as Billroth of Vienna, were frankly hostile. In 1880 Volkmann maintained at a clinical lecture that in medico-legal cases a surgeon could be called to account if he completely ignored antiseptics and lost a patient from pyæmia. "For this," he writes, "I was reproached in the most violent manner both verbally and in print. A distinguished medical-jurist (*Gerichtsarzt*) wrote a letter about me in which he called me a fanatic, and said that no medical jurist alive would reproach a practical surgeon who had acted faithfully according to the teaching of the text-books recommended at the University, because a practical surgeon could neither buy all the new books, nor ought he to allow his principles to be shaken by every new discovery." † These sentiments stand for those of a class, and are therefore important. But it is easy to note the stages by which such principles harden into a scientific creed which nothing and no one can shake.

A. W. Schultze, who was a convinced Listerian, points out that by 1875 the early enthusiasm in Germany had cooled because surgeons could not succeed in obtaining Lister's results, so that in some places actual opposition had set in. Schultze had visited our schools as well as those on the Continent, and he records that "in London Lister has few adherents. The principal surgeons [in 1875] have nothing to do with it, because they say they do not obtain

* Sir R. Godlee, *Lord Lister*, p. 328.
† *Ibid.*, p. 340.

from it any better results, and, speaking generally, the whole affair is too complicated for them. Precise objections you do not hear; the details of the practice are usually unknown to them."

Not less instructive was his round of the Continental schools. It was the same story in Holland, Belgium, South Germany, and Vienna. He never saw the treatment properly carried out. It had been tried and given up. No rational objections were offered. For the most part surgeons were content to clap carbolic acid dressings on to wounds and suppurating surfaces, and looked upon Lister's publications with distrust.*

Billroth stood in the same rank as Bergmann. Great as a man, he proved equally great as a teacher, training such men destined to make their mark as Czerny, Mikulicz, and Wolfler. They diffused his ideas and his inspiration to Heidelberg, Breslau, Prague, and other Universities not merely in Austria and Germany, but in Universities so far away from Vienna as those of Belgium and Holland. The intimacy between Billroth and his pupils was as intimate as that between Paget and his. Billroth looked up to Liebig, and expressed the same disbelief as Liebig in micro-organisms as a cause of decomposition. Twice in October 1875 he confided his opinion to Volkmann. "If you were not," he wrote on October 27, "so energetic a supporter of this method I should say the whole thing was a swindle; but still Lister's personality charms me." † Again he wrote to Volkmann: "I find the failures in Lister's treatment very instructive. I would on no account miss them. Absolute perfection has no interest for me. I am curious what will come after Lister; as a rule such things do not last more than five years." † To Neudorfer, an Austrian Army surgeon who vehemently opposed Lister in November 1876, Billroth wrote: "I share your opinion that Lister's theory still has a hole somewhere; most investigators probably hold this opinion." †

In the Franco-German War of 1870-1, in spite of the lead Pasteur had given to the French surgeons, the French did very little to extend Listerism to the wounded. The Germans

* Sir R. Godlee, *Lord Lister*, p. 343.

‡ *Ibid.*, p. 349. Cf. Briefe of Theodor Billroth.

did much more. Lister devised a specially simple method of his treatment readily applicable to the wounded.* Surgeons like Berger and Perier, Terrier and, above all, Championnière bestirred themselves on behalf of the success of antiseptics, and the last published in 1876 the first complete account of antiseptic surgery. Vilmos Manninger thus accounts for the French failure to seize the new ideas: "France, the land of fashions, was occupied with the discussion of so many other methods which their inventors advocated in preference to this new doctrine from abroad, that it advanced with tardy steps. One particular method, the 'pansement ouaté of Alphonse Guérin,' was set up in opposition to Lister's treatment and held its own until 1880. It was because France in mere method of dressing was looked upon as the hinge upon which antiseptics turned, that the essence of Lister's teaching was long in obtaining a secure foothold." † One remedy held the field, and therefore no other remedy was worth trial. Championnière, a highly competent witness, agrees with Manninger that the French surgeons insisted in regarding Lister's system as a kind of dressing, not as a method of treatment. In 1909 Championnière told his class that in the war of 1870 his chief prevented him from bringing carbolic acid to the field hospital where patients were dying by the thousand of septic disease. His chief objected to such new-fangled ideas, and the carbolic acid was taken back to Paris unopened. Many bodies of Frenchmen, thanks to this prejudice, were left on the battlefield, and were also not taken back to Paris. "I only remind you," Championnière remarked to his students, "as a matter of history, that for many years I was the subject of a sort of persecution on the part of those whose scientific repose I was violently upsetting, a persecution which only our contemporaries can remember." ‡

In Italy the adoption of Listerism was even slower than in France. Writing in 1878, Dr. Giuseppe Ruggi mentions seven Italian surgeons as the rare exceptions who had at least given it a trial. One of these was Bottini of Pavia,

* *Brit. Med. Jour.*, 1870, II, p. 243; Lister, *Collected Papers*, II, p. 161.

† V. Manninger, *Der Entwickelungsgang der Antiseptik und Aseptik*, p. 100.

‡ Sir R. Godlee, *Lord Lister*, p. 355; Championnière, *Brit. Med. Jour.*, 1902, pp. 1819-21.

and he speaks in 1878 of the acceptance of it in other countries, though in his own it "has been suffocated, up to that time, by the terrible and insidious weapon, apathy." * Ruggi testifies: "Italy is the most indifferent of all nations and seems as if she neither interested herself nor wished to interest herself in this method of treatment, which has been estimated so highly by the great surgical leaders of Germany." *

If Europe was slow to adopt antiseptics, America—and that is saying a good deal—was slower. This is all the more remarkable when we bear in mind how often Americans visit Germany, and how lively the admiration for German opinion used to be—and, for that matter, still is. In November 1877, Dr. Robert F. Weir, Surgeon to the New York and Roosevelt Hospitals, said: "It is only lately that, in America, attention has been given practically to the teachings of Lister in respect to the treatment of wounds. In fact, aside from an article by Schuppert in the *New Orleans Medical and Surgical Journal,* little or nothing has appeared in our medical journals relative to the results of the so-called antiseptic method. . . . The reason why American surgeons—who justly have the reputation of being eager to seize upon any improvement in their art—have been tardy in testing the success of this mode of treatment, may, perhaps, be stated as follows: 1. That the treatment, as enunciated by Mr. Lister, has been repeatedly changed in its details; 2. That it was too complicated, and demanded the supervision of the surgeon himself, or, in a hospital, of a carefully-trained staff of assistants; 3. That many who had tried it had been unsuccessful in the cases where the essay had been made. But the most weighty objection which was asserted or entertained, was the positiveness of the enunciation of the germ-theory in explanation of the process of decomposition in the secretions of a wound." †

In 1877 Lister left Edinburgh University for the chair of Clinical Surgery at King's College, London, and there is little doubt that he came to the metropolis as a man with a mission. This mission was to convert London to the

* Sir R. Godlee, *Lord Lister,* p. 356.
† *Ibid.,* p. 357.

antiseptic doctrine, where, after the lapse of twelve years since it was first announced, it had made but little progress. What the state of surgical opinion was like is evident from the fact that John Wood, a colleague of Lister's at King's College, never in his heart thought that there was much in antiseptics. In 1877 he openly declared that his new colleague "owed his fame to Germany," drawing the conclusion that "the Germans were a dirty people," and that accordingly the new treatment "was not really necessary in England." As Wood was a good anatomist, and a better surgeon and a still better operator, his opinion possessed weight.

There was growth, there was development, with Listerism, and this is what we should naturally expect. Men stood on the shoulders of Lister, and gazed ahead. Aseptic surgeons like Ernst von Bergmann, known to some of us as one of the surgeons who attended Frederick the Noble and also known to fewer of us as one of the commanding scientific men of his age, held that organisms falling on a wound after an operation might be washed away by an unirritating fluid like salt-solution. Bergmann had been destined first for the ministry and then turned to philology. During the Franco-German War he had the foresight to divine the enormous extension of surgery to which antiseptics would give rise. Conscious of his great powers, this supremely able man abandoned pure science and turned to applied science in the shape of surgery. His pupils sought to get rid of chemical antiseptics even from their dressings, applying only materials, such as cotton wool or gauze, which had been rendered sterile by super-heating. Did Lister ever understand how aseptics arose out of antiseptics? Did Virchow ever understand how the young men *would* pour the new wine of bacteriology into the old bottle of cellular pathology? Did Ehrlich ever understand how the new generation altered his conceptions of immunity?

The outcome of the extension of aseptic surgery was antagonism to antiseptic surgery. Lister was honest enough to admit that in the light of newer knowledge a certain number of the beliefs upon which his system was founded had to be modified or even discarded. "But it is to be regretted," observes Sir Rickman Godlee, "that so much should have been made of this, and that in the feud that has arisen between the advocates of antiseptics and aseptics intemperate language should have been used. Such was never employed by von Bergmann or by Lister. There ought not to have been a contest at all; for the two systems are not really opposed to one another. . . . The pity of it is that, for the moment, surgeons appear to feel bound to range themselves either on one side or the other." * In the nineties, and even in 1925, there is the spirit of attack that Lister had to endure so far back as the sixties. The idea underlying the modern practice is sound, but it is not new. "Nothing," Sir Rickman Godlee judiciously points out, "is gained by strong language either on one side or the other. It is painful to hear the modern school spoken of as heretics, or the old school as effete. Lister himself seldom referred to the matter in public, and then it was in words not of anger but of grief." †

The old opposition gradually died away, though occasionally men like Lawson Tait and Bantock, the gynæcologists, joined the ranks of the aseptics in their abuse.‡ Men came to acknowledge Lister's creative share in the movement for the prevention and the relief of human suffering, which Sir William Osler termed man's redemption of man. In 1902 Lister received the Copley Medal of the Royal Society. At the anniversary dinner he returned thanks on behalf of the Medallists, and ended his speech by a reference which showed how the man of seventy-five felt his old wounds. "He had often thought that if he did deserve any credit, it was at the time when, perfectly convinced of the truth of the principle on which he acted, and persuaded also of the enormous importance to mankind of being able to carry out that principle in practice, he worked for years with exceedingly little encouragement from his professional brethren. There were, however, two great exceptions, his father-in-law and his students." §

* Sir R. Godlee, *Lord Lister,* p. 458.

† *Ibid.*, p. 466.

‡ L. Tait, "The Antiseptic Theory tested by the Statistics of One Hundred Cases of successful Ovariotomy," Medico-Chirurg. Trans, 1880, LXIII, p. 161; Trans. Internat. Med. Congress, London, 1881, II, p. 228.

§ Sir R. Godlee, *Lord Lister,* p. 576.

IX

FORGOTTEN SCIENTISTS

HOSTILITY to new ideas is as marked a feature of the ancient world as it is of the modern. The search for natural law is expressed in Cato's reply to Scipio: "My wisdom consists in the fact that I follow nature, the best of guides, as I would a God and am loyal to her commands." Reuchlin could say: "I reverence St. Jerome as an angel, I respect De Lyra as a master, but I adore truth as a God." A Cato might follow nature as a God or a Reuchlin might adore truth as a God, but in classical times such a pursuit was dangerous. The Athenians banished Anaxagoras on the ground of impiety. They also banished Protagoras and burned his books publicly. When Carneades of the New Academy visited Rome on political business, and took occasion to deliver some of his sceptical discourses, the Romans promptly expelled him. Aristophanes attacked Socrates for impiety and materialism in teaching that the clouds were mechanical emanations and not divine persons.

Socrates, in Plato's *Phædo,* attacks the physiological studies of Anaxagoras on the same ground. He is explaining to his disciples why he refused to take the hint conveyed to him by the Athenian Government that his escape would be connived at. "All my splendid hopes were dashed to the ground," Socrates holds, "my friend, for as I went on reading I found that the writer [i.e. Anaxagoras] made no use of Mind at all, and that he assigned no causes for the order of things. His causes were air, and ether, and water, and many other strange things. I thought that he was exactly like a man who should begin by saying that Socrates does all that he does by Mind, and who, when he tried to give a reason for each of my actions, should say, first, I am sitting here now, because my body is composed of bones and muscles, and that the bones are hard and separated by joints, while the muscles can be tightened and loosened, and, together with the flesh, and the skin which holds them together, cover the bones; and that therefore, when the bones are raised in their sockets, the relaxation and contraction of the muscles makes it possible for me now to bend my limbs, and that that is the cause of my sitting here with my legs bent. And in the same way he would go on to explain why I am talking to you: he would assign voice, and air, and hearing, and a thousand other things as causes; but he would quite forget to mention the real cause, which is that since the Athenians thought it right to condemn me, I have thought it right and just to sit here and to submit to whatever sentence they may think fit to impose. For, by the dog of Egypt, I think that these muscles and bones would long ago have been in Megara or Boetia, prompted by their opinion of what is best, if I had not thought it better and more honourable to submit to whatever penalty the state inflicts, rather than escape by flight. But to call these things causes is absurd."* So the age-long conflict of idealism with materialism proceeds in the fifth century B.C., as it proceeds in the nineteenth.

As Cato endeavoured to follow nature, so St. Augustine pleaded for freedom for all in her pursuit. He accepted the fact of organic evolution as readily as a naturalist of our day, but a study of its factors was a matter of extreme uncertainty. "It very often happens," he points out, "that there is some question as to the earth or the sky, or the other elements of this world . . . respecting which one who is not a Christian has knowledge derived from most certain reasoning or observation (by a scientific man), and it is very disgraceful and mischievous and of all things to be carefully avoided, that a Christian speaking of such matters as being according to the Christian Scriptures, should be heard by an unbeliever talking such nonsense that the unbeliever perceiving him to be as wide from the mark as east from west, can hardly restrain himself from laughing."† Such was the liberal view he laid down in the fifth century of our era, and such was the view on which he long con-

* Plato, *Phædo*, p. 179.

† *De Genesi ad Litteram*, bk. I. sect. 39 (xix).

tinued to act—till the days of the Donatist controversy. Fourteen hundred years after the death of St. Augustine we still breathed the air of hostility to new forms of thought. From the days of Jenner to those of Lister, who passed away in 1912, we experience its all-pervading presence.

Portions of the careers of Jenner, Simpson, Lyell, Helmholtz, Joule, Darwin, Pasteur, and Lister we have surveyed. These portions stop, for the most part, with the publication of the great conception with which the particular genius is associated. We stop our analysis of the career of Jenner with his publication of his book, *An Inquiry into the Cause and Effects of the Variolæ Vaccinæ,* in 1798 and the criticism thereof; of the career of Simpson with his discovery of chloroform in 1847 and the criticism thereof; of the career of Lyell with his publication of his *Principles of Geology* in 1830-3 and the criticism thereof; of the career of Helmholtz to his discovery of the conservation of energy in 1847 and the criticism thereof; of the career of Joule to his discovery of the mechanical equivalent of heat in 1843 and the criticism thereof; of the career of Darwin to the publication of the *Origin of Species* in 1859 and the criticism thereof; of the career of Pasteur to his discovery of micro-organisms in 1859 and the criticism thereof; and of the career of Lister to his discovery of antiseptics in 1865 and the criticism thereof. Inevitably the task of analysing the criticism takes us, in some cases, beyond the date we have fixed for the end of our labours. There is no reason to think that the strictures levelled at any of these men ceased with the dates we have taken. If this book were not to grow to a portentous length, there must be limits set to it, and we have chosen limits that may well seem to some to be arbitrary. These limits are dictated by considerations of space, and by nothing else.

Helmholtz himself tells us how a Professor of Physiology replied testily when invited by a physicist, during a discussion on the images of the eye, to accompany him home to see the experiment. With annoyance, he retorted that "a physiologist had nothing to do with experiments, though they might be well enough for physicists." * Another Pro-

* L. Koenigsberger, *Hermann von Helmholtz,* p. 49.

fessor of Pharmacology, and an academic reformer as well, taking the intellectual part himself, and leaving the lower experimental side to a colleague, gave up all hopes of him when he explained he regarded experiment as the true basis of science. Towards the end of 1850 Helmholtz discovered the ophthalmoscope. One distinguished surgical colleague told the discoverer that he should never use the instrument. For it would be too dangerous to admit naked light into the diseased eye! Another was of opinion that the mirror might be of service to oculists with defective eyesight. For his part he had good eyes and wanted none of it. *

No one thinks that hostility to Darwin ceased soon after 1859. It certainly had not ceased in 1871 when he published the *Descent of Man.* St. Augustine rejected the doctrine of special creation, and the Church rejected it till the days of John Milton, whose *Paradise Lost* was the cause of its reception. Huxley fondly believed that Moses wrote "Trespassers will be prosecuted" across the path of the naturalist. He was wrong. It was John Milton who affixed this notice. Will the pleasure his poems have given atone for the pain of such a conception as special creation? For hundreds of years the Church entertained no such idea, and men of the stature of St. Augustine emphatically repudiated it. The genius of one man was enough to plant it in the heart of Europe.

In passing, we may note that simultaneous discoveries such as that of Wallace and Darwin are by no means so uncommon as some suppose. Bichat and Lamarck in France, and Treviranus† made simultaneously three independent attempts to treat the phenomena of organic life as a whole and in connection. Karl Friedrich Gauss, whose discoveries were forestalled as often as Newton's, and Legendre simultaneously developed the so-called method of least squares. Gauss followed out the theories of Laplace and Legendre at the same time as George Green, whose fundamental *Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism,* published in 1828, was so long ignored. H. C. Schumacher and Gauss also simultaneously arrived at the same conclusions on

* L. Koenigsberger, *Hermann von Helmholtz,* p. 40.
† E. Haeckel, *Natürliche Schöpfungs Geschichte,* Band I, Vorlesung 4.

Abel's *Memoir on Elliptic Functions*.* Saccheri performed at the close of the seventeenth century, the task of developing a logically consistent geometry which accepted the other Euclidean axioms, though denying the parallel axiom. Gauss, with his penetrating genius, pondered on this question, doubting the absolute necessity of the Euclidean geometry. Once again he was anticipated, for Johann Boylai (1802—1860), a Hungarian, and Lobatschewski, a Russian, also doubted the absolute necessity of traditional geometry. It was quite in keeping with the extraordinary genius of Gauss, which so resembled that of Henry Cavendish, that in the cases of Schumacher and Boylai and Lobatschewski, he should simply have experienced relief that there was now no necessity for him to publish his own results, results that affected the very foundations of the whole of geometry! There is a right spirit of detachment, but surely the limits of it are reached in the cases of Gauss and Cavendish. Think only of some of the consequences of the ideas of Boylai and Lobatschewski. In the hands of Riemann they assumed new possibilities when he contemplated the effect of denying the infinity of the straight line. For it meant the amazing extension of geometry in our days—the extension to space of four, five, or any number of dimensions. Some trace the Einstein principle of relativity back to Descartes. There can be no manner of doubt that it goes back in its pedigree to the simultaneous work of a Russian and a Hungarian, which had all been done by Gauss.

The tale of simultaneous discovery can readily be lengthened. In fact, if we had at our disposal the *n* dimensions in space—on paper, at least—we should require it if we were to give all the examples. Young and Laplace independently developed the theory of capillary action. Avogadro and Ampère practically suggested at the same time that equal volumes of different gases contain an equal number of smallest independent particles of matter, a far-reaching hypothesis. Sir Norman Lockyer and the French astronomer,

* K. F. Gauss, *Werke*, III, p. 395. We may here say that all Gauss's letters are worth reading, and fortunately there are many of them. There are: "Briefwechsel zwischen C. F. Gauss und H. C. Schumacher"; "Briefwechsel zwischen Gauss und Bessel"; "Briefwechsel zwischen C. F. Gauss and W. Boylai"; and "Briefe zwischen Humboldt und Gauss."

Janssen, announced together the existence of three bright lines in the solar spectrum. Jacobi and Abel developed, independently of Gauss, what Gauss called the "new transcendent functions." Jacobi and Abel, following out the investigations of Legendre, came to Gaussian conceptions entertained a generation before them. Hermann Grassmann (1809—1877) quite independently worked at similar extensions of our arithmetical and geometrical conceptions, leading him on to quaternions as presenting a special form of the extended algebra and geometry elaborated from different beginnings. Möbius and Plücker at the same time threw their strength into purely geometrical researches as contrasted with the dominant French school of analysis. Independently and quite unknown to Michael Faraday, or to each other, Sir William Rowan Hamilton, the discoverer of quaternions, at Dublin and Grassmann at Zürich were elaborating, between 1835 and 1845, the geometrical conceptions and vocabulary required in the representation of "directed" quantities. To-day we know these quantities as vector analysis, and we also know what a large share vector analysis takes in the scientific labours of our time. As Helmholtz at Potsdam and Joule in Manchester were working at different sides of the conservation of energy, so were Clausius at Zürich and Kelvin at Glasgow working at the same side of this problem simultaneously. Clausius and Kelvin simultaneously sought to reconcile Sadi Carnot's conceptions with Joule's experiments. Kelvin, characteristically enough, developed his views in a generalised attitude, and Clausius, just as characteristically, developed his from a particularised angle and gave his treatment a more purely mathematical turn. Kelvin, Clausius, and Macquorn Rankine at the same time applied the Carnot doctrine that heat and work are convertible to the discovery of new relations among the properties of bodies. In 1869, Mendeleef, the Russian, and Lothar Meyer, the German, published almost together their classification of the periodic arrangements of the elements, according to their atomic weights. In 1900, De Vries from Holland, Correns from Germany, and Tchermak from Austria simultaneously rediscovered and repeated Mendel's experiments recorded in 1865.

People say that ideas are in the air. They are nothing of the kind. They are in the minds of men and women who have the genius to perceive them. That they should perceive them simultaneously every now and then is no matter of surprise. Rather, the matter of surprise is that such simultaneous discoveries should not occur more frequently than they do. We can scarcely avoid raising the question, Why should not two men of genius in science collaborate? Supposing Darwin and Wallace had collaborated? Supposing Adams and Leverrier had collaborated? It does not seem to have occurred to the co-discoverers of the evolution principle that they should have joined forces, and we almost doubt the possibility. A couple of cases may be given. Liebig (1802—1875) and Wöhler (1800—1882) co-operated successfully, in spite of the fact that they pursued different lines of thought and were trained in different schools. Of course many scientific memoirs bear two names, but the status of the joint authors is generally known to be unequal. One of them is either paying a kindly compliment to the younger worker, or (alas! for poor human nature) he is assuming credit for work done in his laboratory with a minimum of exertion to himself. One of the rare exceptions is the association of G. D. Liveing, so long the Grand Old Man of Cambridge University, and Sir James Dewar, and this association undoubtedly carried no suggestion of inequality. Real scientific partnership is much rarer than successful literary collaboration.

No scientist has carried out a long series of experiments, leading him to formulate conclusions, without wishing that he had anyone with whom he could discuss their worth. Pasteur ached for Chappuis to take up science in order to afford him that scientific partnership for which he longed. Chappuis, however, turned aside to philosophy, and Pasteur was obliged to tread his solitary path. Long before him, Milton had remarked, "I have chosen the lonely way." It was as lonely for Pasteur as it had been for Newton, journeying in strange seas of thought unaccompanied. The motto prefixed to Pasteur's "*Etudes sur la Bière*" is: "The greatest distortion of the intellect is to believe things because one wishes them to exist." At one of the meetings of the Académie Française, while the interminable Dictionary was being discussed on January 29, 1885, it flashed across his mind, bursting for a Chappuis in whom to confide: "I do not know how to hide my ideas from those who work with me; still, I wish I could have kept those I am going to express a little longer to myself. The experiments have already begun which will decide them.

"It concerns rabies, but the results might be general.

"I am inclined to think that the virus which is considered rabic may be accompanied by a substance which, by impregnating the nervous system, would make it unsuitable for the culture of the microbe. Thence vaccinal immunity. If that be so, the theory might be a general one: it would be a stupendous discovery.

"I have just met Chamberland in the Rue Gay-Lussac, and explained to him this view and my experiments. He was much struck, and asked my permission to make at once on anthrax the experiment I am about to make on rabies as soon as the dog and the culture rabbits are dead. Roux, the day before yesterday, was equally struck." *

Working inductively, Jenner had discovered his vaccination. Working scientifically, Pasteur was, to the permanent benefit of mankind, to develop his remedy. The day came when the remedy was to be tested on Joseph Meister, the little Alsatian boy, who had been bitten by a mad dog at Meissengott near Schlestadt. M. Vallery-Radot reveals how tenderly—and how anxiously—Pasteur watched the cure of the boy, for to him as to Lister a patient was never an item in a ward. He was "this poor lad" who has the misfortune to be inoculated with hydrophobia. Meister was cured. Did the doctors believe in the cure? Not at all. At the time when Dr. Grancher first accepted the responsibility of conducting inoculations, one of Pasteur's most determined opponents, M. Peter, reiterated his requests to the Academy of Sciences almost weekly, that they should order the laboratory in the Rue d'Ulm to be closed. Passionately Peter declared that, instead of curing rabies, they were in reality communicating the disease. One morning Dr. Grancher met Pasteur listening to a physician who was putting forth his objections to the doctrine of microbes in general and of the treatment of hydrophobia in particular. Wearied out with the objections at last, Pasteur replied:

* R. Vallery-Radot, *Life of Pasteur,* p. 413.

"Sir, your language is not very intelligible to me. I am not a physician and do not desire to be one. Never speak to me of your dogma of morbid spontaneity. I am a chemist; I carry out experiments and I try to understand what they teach me." * Opponents discussed in April 1886 the three deaths of Russians who came at an advanced stage of the disease. The failures they could see: the successes, such as Meister, they were unable constitutionally to see.

In 1886 Pasteur was a man of forty-four, one of the most outstanding scientists in the length and breadth of France, yet even he was subject to the hostility of men like Peter. When in August 1886 the discoverer read some articles of fierce criticism, he exclaimed: "How difficult it is to obtain the triumph of truth! Opposition is a useful stimulant, but bad faith is such a pitiable thing. How is it that they are not struck with the results as shown by statistics? From 1880 to 1885 sixty persons are stated to have died of hydrophobia in the Paris hospitals; well, since November 1, 1885, when the prophylactic method was started in my laboratory, only three deaths have occurred in those hospitals, two of which were cases which had not been treated. It is evident that very few people who had been bitten did not come to be treated. In France, out of that unknown but very restricted number, seventeen cases of death have been noted, whilst out of the 1,726 French and Algerians who came to the laboratory only ten died after the treatment." †

There was open hostility on the part of medical men, and there was concealed or anonymous hostility on the part of some members of the public. The discoverer suffered from that pestilential person, the writer of the anonymous letter. Envy, malice, and hatred employed their trumpet tones against him. In 1886, with a career of scientific brilliance behind him, newspaper after newspaper wrote insulting and scurrilous articles. Colleagues, knowing his sensitive nature, endeavoured to console him. "I did not know I had so many enemies," he mournfully thought. "You know," said M. Grancher, "that M. Pasteur is an innovator, and that his creative imagination, kept in check by rigorous ob-

* R. Vallery-Radot, *Life of Pasteur*, p. 425.
† *Ibid.*, p. 433.

servation of facts, has overturned many errors and built up in their place an entirely new science. His discoveries on ferments, on the generation of the infinitesimally small, on microbes, the cause of contagious diseases, and on the vaccination of those diseases, have been for biological chemistry, for the veterinary art and for medicine, not a regular process, but a complete revolution. Now, revolutions, even those imposed by a scientific demonstration, ever leave behind them vanquished ones who do not easily forgive. M. Pasteur has therefore many adversaries in the world, without counting those Athenian French who do not like to see one man always right or always fortunate." * The Greek did not admire Alcibiades because he was invariably called Alcibiades the Just, and apparently this Greek owned successors in the France of the eighties. In spite of M. Grancher, we entertain a shrewd suspicion that the obstacles Pasteur encountered in the medical world can be traced back to the motto of his *Etudes sur la Bière*. For medical man after medical man believed things because they wished them to exist, or perhaps we may say medical man after medical man believed things because they had existed while they were young men, and therefore they were resolved that they should go on persisting in their existence. The fact that Pasteur—or, for the matter of that, any other scientific man—had taken them to a stage beyond what they had known in their college days was absolutely immaterial. Their motto was, " The thing that hath been is that which shall be, and there is no new thing under the sun." At least, if there was a brand new thing under the sun, ostrich-like they were determined to hide their heads in the sands and not see it. Nelson was not deliberately more blind at Copenhagen than these medical men were resolute in being. They applied the telescope to their blind eye. Of course they saw nothing in Pasteurian research. But they might have been asked another question, How could they with eyes wilfully shut see anything in it? They might also have been asked a second question, How are they going to account for his results? Of course they could say —what stupid folk are always saying—when any one of them was buttonholed, Results? My dear sir, there really are

* R. Vallery-Radot, *Life of Pasteur,* p. 441.

no results. You can invariably juggle with figures. Besides, have you heard of the deaths? With a shake of the professional head, it could be implied that if discretion permitted—it could be whispered perhaps—the doctor could effectively remark, THREE deaths! Think of that. If I only could tell you all—and then he could break off, implying far more deaths than three. Pasteur the discoverer! But we all know the rigorous implacability of his reasoning! We all know the absolute form he gives to his thought! In theory, he MAY be right. True, he has made lucky guesses before. But this time his guess has been unlucky! There are those deaths! Those he cannot explain away! So the wiseacres shook their heads, so they shrugged their shoulders. Time proved them wrong. That time proved them wrong does not prevent the successors of those wiseacres shaking their heads and shrugging their shoulders, even in the year of grace 1925, when a new idea is placed before them.

It is a law of mechanics that to every action there is an equal and opposite reaction. The mightier the wave, the greater is the stretch of sand ultimately left exposed. The example of Francis of Assisi raises his followers to a pinnacle beyond the reach of mankind; but the work of the satirist and the record of the annalist agree in their evidence that the friars of the sixteenth century were as much below the level of good men as their predecessors were above it. Through the mouth of Pericles, Thucydides praises the Athenians for the exact qualities which, in the eyes of Demosthenes, they utterly lack. The energy of the Athenians of one century was as much above the normal level as that of their descendants of the next century was below it. There are many swings of the pendulum backwards and forwards before the repose of the mean is reached. In the world of science there is the inevitable reaction against a man who has stood on a height. The discovery of any great truth is always followed by an over-valuation, from which there is certain to be a reaction. There is such a reaction, for example, against the theory of natural selection which will inevitably come into its own. All the criticism, however, we have recorded has not been of this class, which is, after all, an incident of human nature, though it ought not to be so. The criticism we have recorded has generally been of the type which insists and persists that the new view is not true, it cannot be true, and anyhow, we should not dream of accepting it. It is of the class of the Scots who announced, "I am open to conviction, but I am a dour deevil to convince." The creed of the scientist is that he stands open to every form of truth that the laboratory could bring him. No creed could be more correct. Creed and practice in science, however, are by no means precisely the same thing. Not a few scientists resembled the Scots. They were open to conviction, but the examples of Jenner and Simpson, Lyell and Helmholtz, Joule and Darwin, Pasteur and Lister proved that they were "dour deevils" to convince, and many of them permanently remained unconvinced. Take an instance. Sir Arthur Shipley informs us that Alfred Newton (1829—1907) was in some respects old-fashioned and with fixed ideas, he was like Mr. Crisparkle's mother, "always open to discussion, but he invariably looked, as the China shepherdess looked, as though he would like to see the discussion that would change *his* mind."

Long before there was a Christian Church the Greeks employed their *autos da fe.* Anaxagoras and Protagoras, Carneades and Socrates knew by practical experience what they meant in real life. To-day science can proclaim—if she chooses—that she employs no *autos da fe.* In fact, she can proudly claim that she has never done so. We, however, are not so sure. Is there any punishment equal to that which, in the name of Science and with the august authority of Science, has been inflicted upon Jenner and Simpson, Lyell and Helmholtz, Joule and Darwin, Pasteur and Lister, in some cases by ignoring their epoch-making ideas, and in other cases by fighting them to the death? Did any set of men so torture the body as scientists tortured the minds of these discoverers by bitterly criticising them? One would have thought that the thanks of the whole scientific world would at once have been their rightful due. In not a single case was this so. We may be told that such things do not happen nowadays. Of this we are by no means sure. Our scientific *autos da fe* take a new form. If your conceptions are revolutionary, you are not elected, say, quite so soon a Fellow of the Royal Society. You are not elected to committees to which other men, with not a tithe of your abilities, are elected. If you are a young lecturer with a big idea in your mind, you had better be careful. For if your Professor does not report well of you, you may remain an assistant Professor or a mere lecturer all your days. Men able to practise concealment of views like Cavendish and Gauss—though with none of their genius—are men likely to beat you in the days when the selection committee chooses the new Professor. Nor are these dangers by any means imaginary. What hope is there for a man whose papers are systematically declined by the Royal Society of England, or the Royal Society of Edinburgh, or the Royal Irish Academy of Ireland? The same question exactly applies to such foreign bodies as the Académie Française des Sciences or the Preussische Akademie d. Wissenschaften. The old motto for the aspiring scientist can be revised, for it is, " Abandon all hope ye who do *not* enter here."

The want of recognition, we do not doubt, has brought the work of many a promising man to an end. We might as well say that the harm of the Index Expurgatorius may be measured entirely by the books on its list. Of course, this is utterly out of the question. We measure the harm done by the Index Expurgatorius not merely by the books put on it, but also by those never written because their authors were afraid. Authors, even scientific ones, are not all brave men physically. Many a man is not afraid for his own sake. But he is afraid for the sake of truth and—for he is a human being—for the sake of the girl to whom he is engaged. Sir Isaac Newton was not the only man in the seventeenth century who shrank from publishing his results because he feared the hostility his ideas might excite. Henry Cavendish was not the only man in the eighteenth century who shrank from publishing his results because he preferred to attack new questions. Nor was Karl Friedrich Gauss * the only man in the nineteenth century who shrank from publishing his results because he feared the hostility his ideas might excite. In Shakespeare's *Henry IV* there is a description that applies: "I am not yet of Percy's mind, the Hotspur of the north; he that kills some

* On Gauss, cf. Hanselmann, *K. F. Gauss*; W. von Sartorius, *Gauss zum Gedächtniss*; and E. Schering, *C. F. Gauss*.

six or seven dozen of Scots at a breakfast, washes his hands, and says to his wife, 'Fie upon this quiet life! I want work!'" There is a class of scientist who aches for strife. Thomas Henry Huxley cared for truth. No one who has read the fine biography of him his son, Mr. Leonard Huxley, has written can doubt that for a single moment. Did he never care for controversy for controversy's sake? He spawned hypotheses, though he never, so far as we know, originated a single discovery save the one he made as the undergraduate of nineteen. He went astray over his Bathybius hypothesis, and he went astray over the phylogeny of the horse. Now before both these mistakes were discovered, would it, we imagine, have gone well with a young man who pointed out the errors in either? A pure lover of truth, like that supreme genius, Faraday, would probably have admitted the error. We are not altogether so sure in the case of a man who loved controversy as Huxley did, for it was the very breath of his nostrils. Can you always chase truth with a logical forceps?

Nothing came amiss to the destructive powers of Huxley. It might be the views of General Booth on social reform or it might be the folks who talk about the natural rights of man. He lashed them all, and right vigorously he laid on the lash. To use present-day phraseology, his controversial complexes suffered no repression. No doubt the subconscious is as much overworked as x, y, and z in algebra, and we hesitate to employ this term. Still, subconsciously this attitude of Huxley towards controversy, psychologically speaking, affected his attitude towards the pursuit of truth. Instead of constructive work, his is destructive work, and it is seldom possible for the same brain to undertake these two types of creative energy. Even the *élan vital* of Bergson is not sufficient for such a task. Huxley's coinage of the word Agnosticism has always seemed to us to be a parable of the whole man in science as well as in political economy and in political philosophy. Gladstone wrote foolish articles on matters with which he was not conversant, like his account of Creation as revealed in Genesis and the order of evolution as shown by modern biology. Huxley confided in Mr. H. F. Osborn: "When this article reached me, I read it through and it made me so angry that I believe it must have acted on my liver. At all events, when I finished my reply to Gladstone I felt better than I had for months past."* The question, however, is, How did some young men of science feel as they read such slashing attacks? Was truth to be clutched by the hair—in the pages of the *Nineteenth Century*—as a constable might capture a ferocious prisoner?

Because Huxley is one of the most typical of the Victorian scientists we spend some more space on him. On September 29, 1890, he informs Sir J. D. Hooker: "I wish quietude of mind were possible to me. But without something to do that amuses me and does not involve too much labour, I become quite unendurable—to myself and to everybody else.

"Providence has, I believe, specially devolved on Gladstone, Gore, and Co. the function of keeping 'home 'appy' for me.

"I really can't give up tormenting *ces drôles*." †

Did it ever occur to him that just as there is a scientific atmosphere so there is a theological one, to be lived and not merely to be crammed up in Suarez or in any other authority? We verily think that such an idea never crossed the mind of Huxley. As well might one learn anatomy from book-work only!

On January 10, 1891, he confides in Sir Michael Foster: "I knew the saints were not bad hands at lying before; but these Booth people beat Banagher.

"Then there is —— awaits skinning, and I believe the G.O.M. is to be on me! *Oh, for a quiet life*." ‡

We return to Shakespeare's *Henry IV*, for the parallelism is marked. "I am not yet of Percy's mind, the Hotspur of the north; he that kills some six or seven dozen of Scots at a breakfast, washes his hands, and says to his wife, '*Fie upon this quiet life!*§ I want work!'" Huxley never secured the deeply reflective mind, simply because he did not care for it. He could not say, with Alfred de Vigny, "J'ai porté dans une vie toute active, une nature toute contemplative." The brooding spirit of truth never dwelt among

* *Impressions of Great Naturalists*, p. 93.
† L. Huxley, *Life and Letters of T. H. Huxley*, II, p. 269.
‡ *Ibid.*, II, p. 275. I italicise the words.
§ I italicise these words.

his manifold activities, spreading peace and the atmosphere of fruitful ideas over everything. Gladstone had written articles which laid them open to his facile pen. Very well, then. Pen must be put to paper, and the articles demolished as a contractor demolishes an empty house. Lord Randolph Churchill had set the fashion. Did it strike the mind of Huxley that in attacking Gladstone, he, in spite of Mr. Winston Churchill's brilliant biography of his father, lowered himself to the rank of Lord Randolph Churchill?

The case of Sir Richard Owen is far more serious, for he had won for himself a serious scientific position. We read in 1851 that Huxley writes: "It is astonishing with what an intense feeling of hatred Owen is regarded by the majority of his contemporaries, with Mantell as arch-hater. The truth is, he is the superior of most, and does not conceal that he knows it, and it must be confessed that he does some very ill-natured tricks now and then. A striking specimen of one is to be found in his article on Lyell in the last *Quarterly,* where he pillories poor Quekett—a most inoffensive man and his own immediate subordinate—in a manner not more remarkable for its severity than for its bad taste. That review has done him much harm in the estimation of thinking men—and, curiously enough, since it was written, reptiles have been found in the old red sandstone, and insectivorous insects in the Trias! Owen is an able man, but to my mind not so great as he thinks himself. He can only work in the concrete from bone to bone, in abstract reasoning he becomes lost—witness *Parthenogenesis,* which he told me he considered one of the best things he had done." * Huxley was then but twenty-six when he pronounced this cocksure judgment. Then—and always—his was an *esprit positif.*

He tells his sister Lizzie, on March 27, 1858, that "I have a high standard of excellence and am no respecter of persons, and I am afraid I show the latter peculiarity rather too much. An internecine feud rages between Owen and myself (more's the pity) partly on this account, partly from other causes." †

In *Punch* for May 15, 1862, under a picture of a gorilla,

* L. Huxley, *Life and Letters of T. H. Huxley,* I, p. 93.
† *Ibid.,* I, p. 158.

bearing the sign, " Am I a Man and a Brother? ", appeared a squib, and we give the concluding verses:

Next HUXLEY replies
That OWEN he lies
And garbles his Latin quotation;
That his facts are not new,
His mistakes not a few,
Detrimental to his reputation.

"To twice slay the slain"
By dint of the Brain
(Thus HUXLEY concludes his review),
Is but labour in vain,
Unproductive of gain,
And so I shall bid you " Adieu."

Did Huxley bid Owen adieu? Of course he did nothing of the kind. On May, 13, 1871, he writes to John Tyndall: " You know Mrs. Carlyle said that Owen's sweetness reminded her of sugar of lead. Granville's was that plus butter of antimony! " *

When Sir Richard Owen died, his son had the temerity to ask Huxley to write an appreciation of the labours of his father. Putting aside his private feelings, Huxley, with a magnanimity that does him credit, did the task in 1894 in a way that proves the standing of Owen to all who care for what a big man undoubtedly did. Huxley pronounced, after the death of Owen, that " Owen's time . . . might have been fully occupied by the famous *Memoir on the Pearly Nautilus,* which was published in 1832 and placed its author, at a bound, in the front rank of anatomical monographers. . . . During more than half a century, Owen's industry remained unabated; and whether we consider the quantity, or the quality, of the work done, or the wide range of his labours, I doubt, if, in the long annals of anatomy, more is to be placed to the credit of any single worker. . . . It is a splendid record; enough, and more than enough, to justify the high place in the scientific world which Owen so long occupied. If I mistake not, the historian of comparative anatomy and of palæontology will always assign to Owen a place next to, and hardly lower than, that of Cuvier, who was practically the creator of those sciences in their modern shape; and whose works must

* L. Huxley, *Life and Letters of T. H. Huxley,* II, p. 167.

always remain models of excellence in their kind. It was not uncommon to hear our countrymen called "the British Cuvier," and so far, in my judgment, the collocation was justified, high as the praise implies.

"But when we consider Owen's contributions to 'philosophic anatomy,' I think the epithet ceases to be appropriate. For there can be no question that he was deeply influenced by, and inclined towards, those speculations of Oken and Geoffroy Saint-Hilaire, of which Cuvier was the declared antagonist and often the bitter critic. . . . When Owen passes from matters of anatomical fact and their immediate interpretation to morphological speculation, it is not surprising that he also passes from the camp of Cuvier into that of his adversaries. . . .*

"But it will cease to be so remarkable to those who reflect that the ablest of us is a child of his time, profiting by one set of influences, limited by another. It was Owen's limitation that he occupied himself with speculations about the 'Archetype' some time before the work of the embryologists began to be appreciated in this country. It had not yet come to be understood that, after the publication of the investigations of Rathke, Reichert, Remak, Vogt, and others, the venue of the great cause of the morphology of the skeleton was removed from the court of comparative anatomy to that of embryology." †

Many of the details of this ‡ long and this generous eulogy we have left out, though the student of the growth of science will care to read the whole of it. Readers of the first volume of Mr. Leonard Huxley's biography of his father cannot help noticing the bitterness of the feeling existing between Huxley and Owen, and this is evident in the biography Mr. Huxley has also written of Sir J. D. Hooker, a couple of extremely important pieces of work. They can hardly help thinking that if some of this generosity of feeling had been manifested by T. H. Huxley towards Sir Richard Owen during the lifetime of the great anatomist, what a happier place the world of science would have been! Psychology teaches us that if complexes are repressed, they

* R. Owen, *The Life of Professor Owen,* II, p. 306.

† *Ibid.,* II, p. 309.

‡ It occupies pp. 273 to 332 in vol. II of R. Owen, *The Life of Professor Owen.*

nevertheless make their influence felt. Huxley, like all the rest of us, was under the sway of these complexes, and they undoubtedly altered his attitude to Owen. Lately a friend of mine was asked her opinion of a lady who possessed a pair of beautiful eyes and a sharp tongue. Reflectively she answered, "Mrs. ——. Oh, she has the most beautiful pair of eyes I have ever seen in a woman's head." There was a pause in the conversation, and then came another question, "But what about her tongue?" "I leave that to speak for itself." Huxley perceived the beauty of the anatomy and the palæontology of Owen, and it is a thousand pities that he allowed his private grudges to sharpen his tongue on one who, in spite of mistakes, is the Cuvier of England.

Of all the melancholy reading we know, there is nothing quite so melancholy in the annals of the nineteenth century as the scientific controversies that disfigure it. Men who ought to have co-operated together are to be found, privately and publicly, slaying one another in reputation. Goethe, who was *inter alia* a scientist, wrote:

> Es bildet ein Talent sich in der Stille,
> Sich ein Charakter in dem Strom der Welt.

Ein Talent certainly comes to its own in *der Stille,* but does it come to its own in *der Streit?* There is that attitude of waiting, that passiveness, Wordsworth attractively sets forth as part of our position to the world of nature:

> Nor less I deem that there are powers
>   Which of themselves our minds impress;
> That we can feed this mind of ours
>   In a wise passiveness.
>
> Think you mid all this mighty sum
>   Of things for ever speaking,
> That nothing of itself will come,
>   But we must still be seeking?

Disagreement in the mathematical sciences might seem at first sight incredible. Yet who does not know the controversies connected with the theory of parallel lines, the meaning of infinitesimals, the correct measurement of force, the conservation of energy, and the like? There are fashions in the methods employed in the solution of equations, and men of one set of fashions in this matter denounce men of another set of fashions. Despite the labours of Gauss and Sir William Hamilton, P. G. Tait could never convince Lord Kelvin that the method of quaternions was a fruitful one. Tait himself used this method, but Lord Kelvin would never countenance it. Tait in turn disputed the discovery and the enunciation of the second law of thermodynamics with Clausius. Hirn and Zeuner fought hotly as to the cause of the serious discrepancy between the theoretical and practical figures referring to the work in the steam-cylinder. This was known as the "Water or Iron" Controversy.* The three brothers Weber,† freeing themselves from the metaphysical assumptions of their day, occupied themselves with the method of exact measurement applied to physical, physiological, and mental processes. A subject, however, so remote from human passions as Wilhelm Weber's law of electro-dynamics or Ernst Heinrich Weber's law of psycho-physics aroused long controversies. We travel on to another grave question, What were the relations between mathematical and experimental physics? During the first half of the nineteenth century men like Gustav Magnus (1802—1870) and Hermann von Helmholtz perceived a danger existing. Just as "Natur-philosophie" had enticed many from nature and observation, so mathematical theories, involving prolonged calculations, they feared, might similarly entice them from nature and observation.

Helmholtz used to lay stress on the transcendent genius of Thomas Young (1773—1829) and competent thinkers of our time cordially agree in this opinion. The average scientist thought, however, that a man who was physician, physicist, and Egyptologist was probably Jack of all trades and master of none. For Newton alone Gauss reserves the adjective "summus," and we feel tempted to extend it to Young. Tscherning terms Young the founder of physiological optics. The emission theory of light held the day till he made his investigations, setting forth the wave theory of light. Radiant light, he concluded, consists of undula-

* Prof. Unwin's Forrest Lecture, *The Electrician,* XXXV, p. 46 ff., p. 77 ff.

† Ernst Heinrich, 1795—1878; Wilhelm, 1804—1891; and Eduard, 1806—1871.

tions of the luminiferous ether. The far-reaching nature of his investigations was not grasped at the time, and Lord Brougham had an easy task in criticising one so unknown to fame as Young then was. Sydney Smith remarked that Brougham had made two great discoveries in the *Edinburgh Review*. The first was that Byron was no poet, the second that Young was no philosopher.* Young published a masterly reply to Brougham, who had asserted that he could find in the papers, containing the investigations, "nothing which deserves the name either of experiment or discovery," deemed them "destitute of every species of merit," and admonished the Royal Society for printing such "paltry and unsubstantial papers." Thoughtful people read the *Edinburgh Review*: they did not read the pamphlet of Young, which remained unknown. He accounted for the first time for the constancy of the angle of contact of a solid and of a liquid. He was the first to formulate the term "energy." He introduced absolute measurements in elasticity by defining the modulus as the weight which would double the length of a rod of unit cross-section to which it was hung. He quite saw the impossibility of any material theory of heat, holding that it consisted of vibrations of the particles of bodies, "larger and stronger than those of light." With interests as wide as those of Leonardo da Vinci, he turned his attention to the hieroglyphic inscriptions found on Egyptian remains. He provided the beginnings of a hieroglyphic alphabet and he provided a hieroglyphic vocabulary of about two hundred signs, most of which have been confirmed by recent research. The thoughtful public, however, was persuaded that the enormous labours of Young were worthy of little respect, and, thanks to controversialists like Brougham, little was heard of Young's contributions till they were adopted abroad.

Sir Humphry Davy opposed the atomic theory of Dalton, and Dalton in turn opposed the law of volumes of Gay-Lussac. Hermann Kolbe broke down the formalism of the older chemical type theory, but he stood out virulently against the representatives of modern chemistry. Fierce battles raged between rival optical theories of emission and undulation, and they also raged between rival theories as to the

* Horner, *Life of Sir C. Lyell,* I, p. 470.

origin and maintenance of the power of the Voltaic Pile.* The lively contests of the Wernerians and the Huttonians occupy a large chapter in the troubled history of geology. Nor has the controversial aspect of science disappeared in our own day. Take an example. The labours of Gregor Mendel have been a long time coming into their own. After prolonged experiments he wrote a paper, giving a clear and concise account of his results, and sent it to the able botanist Nägeli. This met with no response. Undaunted at first, he sent it to a local scientific journal in 1865, where it languished in obscurity. Meeting with no recognition, he died a disappointed and embittered man. Despite all appearances to the contrary, he frequently remarked, "Meine Zeit wird schon kommen." The time came, but it came to him as it came to Semmelweis, too late, for his body lay in the grave. Mr. Bateson and Mr. Punnett of Cambridge are the prominent exponents of his ideas, which of course they have developed. The leaders of the Biometric School of Inheritance are the late W. F. R. Weldon and Mr. Karl Pearson.† Is it unfair to hold that this School of Inheritance viewed with marked disfavour the Mendelian School?

The spirit of stillness, not the spirit of strife, is the spirit in which illuminating ideas fill the mind of the investigator. His brilliancy and his daring may be spoilt by his combativeness. Brilliant he must be, daring he must be. The qualities of patience and perseverance, of cautiousness and conservatism, are qualities also demanded in due proportion. When the discoverer broods over his conceptions, there often flashes upon him the big notion that will co-ordinate the lesser ones. The shores of the world of science are strewn with the wrecks of prophecies that remained unheeded because many who might have recognised their value were either preoccupied with conceptions with which they clashed or were thick in the fray with other scientists. How many, except de Candolle,‡ cared for Goethe's divination of the nature of vegetable organism or for his anticipations of colour-theory? How many cared for the prophecy of Marcus Antoninus Plenicz of the germ theory of disease,

* J. Tyndall, *Faraday as a Discoverer*, p. 73 ff.

† Cf., for instance, M. Onslow, *Huia Onslow*, p. 78.

‡ *Organographie*, I, pp. 243, 551. Cf. Goethe, *Werke*, Abth. I, Bd. VII (Weimer ed.).

anticipating Pasteur by almost a hundred years? How many cared when in 1846 Rasori announced that parasites produced fevers? * How many cared for the prophecy of Faraday of the electric telegraph or the similar prophecy of Gauss † in 1835? How many cared for the anticipation of the principle of spectrum analysis discerned by Bolzano of Prague in 1842? How many cared for the experiments of Joseph Henry, the American, who so far back as 1842 carried out experiments similar to those carried out by the greatest of all the pupils of Helmholtz, Heinrich Hertz, and Henry prophesied that the discharge was oscillatory? ‡ How many cared for the mathematical demonstration of the oscillatory nature of the discharge given by Lord Kelvin in 1853? How many cared when James Thomson predicted in 1850 that when you knew the mechanical equivalent of a degree of temperature and the work of the expansion of ice, you could calculate how much the freezing point of water must be lowered by pressure? How many cared for the anticipation of spectrum analysis in 1845 § or the prophetic work on the nature of fluorescence announced by Sir Gabriel Stokes in 1860? How many to the year 1900 cared for the outcome of Gregor Mendel's experiments?

A list of predictions like this sets one in a frame of mind to raise again the old question, Can a man do his work too soon? Kant is reported to have said to Stägemann in 1797: "I have come too soon; after a hundred years people will begin to understand me rightly, and will then study my books anew and appreciate them." ‖ Did Thomas Young, as Helmholtz maintained,¶ come a generation too soon? Did James Hutton, as Huxley maintained,** come

* Sir R. Ross, *Memoirs*, p. 119.

† Gauss wrote to Schumacher: "With a budget of 150 thalers [£22 10*s*.] annually for Observatory and Magnetic Institute together, really extensive trials cannot of course be made. But could thousands of thalers be bestowed thereon, I think that, for instance, electro-magnetic telegraphy might be carried to a perfection and to dimensions at which imagination almost starts back." Cf. *Briefwechsel zwischen Gauss und Schumacher*, II, p. 411 ff.

‡ M. Pupin, *From Immigrant to Inventor*, p. 266.

§ Kirchhoff, *Gesammelte Abhandlungen*, p. 625; Kelvin, *Baltimore Lectures*, p. 100.

‖ *Tagebücher, von Varnhagen von Ense*, I, p. 46.

¶ H. L. F. von Helmholtz, *Vorträge und Reden*, I, p. 279.

** T. H. Huxley, *Geological Reform*, 1869.

before his time? Kant, Young, and Hutton of course stand in the front rank. We may, however, raise the question about much smaller men. Did Jeremias Benjamin Richter anticipate the labours of Dalton too soon? * His mind was filled with the idea of applying mathematics to chemistry in general and with ascertaining the atomic weight of the different elements, and we must not forget that Cuvier dates the revolution in chemistry from the introduction of the mathematical spirit.

While it is possible that some men arrive too soon for the absolute appreciation of their services, it is certain that many have been denied this appreciation because scientists were engrossed with the suppositions they entertained or were too busily employed in fighting other discoverers to afford time to note the worth of their views. William Smith (1769—1839) is reckoned the father of British geology to-day, but in his own day William Whewell notes that Smith "had long pursued his own thoughts without aid and without sympathy." † "No literary cultivation of his youth awoke in him the speculative love of symmetry and system; but a singular clearness and precision of the classifying power, which he possessed as a native talent, was exercised and developed by exactly those geological facts among which his philosophical task lay. Some of the advances which he made had been entered upon by others who preceded him; but of all this he was ignorant. and perhaps went on more steadily and eagerly to work out his own ideas from the persuasion that they were entirely his own." ‡ He belonged to that race of amateurs who have assisted so greatly by their discoveries, a race that used to flourish more in Great Britain than in any other country. But "Stratum Smith," though known at home, was unknown abroad. The Continent took as little notice of his geological conceptions as he himself took of Continental conceptions.

If anyone wishes to disbelieve in progress, we commend to his attention the able book F. Rosenberger has written on the *Geschichte der Physik*. He gives a long list of references to theories of forgotten scientists whose labours

* H. Kopp, *Geschichte der Chemie*, II, p. 350; A. Wurtz, *Histoire des Doctrines Chemiques*, pp. 9, 13.

† W. Whewell, *History of the Inductive Sciences*, III, p. 427.

‡ *Ibid.*, p. 423.

before and after 1850 lay buried in unkindly oblivion till an historian like himself resurrected them. It is a saddening and maddening piece of work. It is saddening when we reflect that men have toiled and their toil has been ignored. It is maddening when we also reflect that because their toil has been ignored their work has had to be done all over again. The proportion of first-class brains in the world is always small. The Pearson curves show that out of a hundred candidates in a mathematical examination only six of them will gain out of a total of 100 over 80 marks, while in a literary examination only one out of a hundred candidates will gain over 80 marks. In blunt English, this means that on the literary side of the activities of a college out of a hundred undergraduates there is only one of them likely to prove his worth by gaining a first-class in the Tripos examination. Oddly enough, the Pearson curves allow of a higher proportion of mathematical candidates turning out well. The examination success does not perhaps prove very much, for there is a whole world between the powers of absorbing the contents of a book and the powers that originate ideas. When Robertson Smith was at Aberdeen University he took for his degree mental and moral science as well as mathematics. Bain was his Professor in the former subject. Temperamentally, Robertson Smith, who was a most pugnacious individual, did not care for Bain's school of thought. At his degree examination he answered Bain's questions as Bain would have them answered, and then he characteristically appended his own refutations of the answers he had written. Bain was too fair-minded not to give Robertson Smith his first-class. Then, to Bain's astonishment, Robertson Smith asked his Professor for a testimonial. He received it, and here it is: "Mr. Robertson Smith has shown unequalled capacity in absorbing knowledge: whether he can reproduce it remains to be seen." This testimonial puts the point precisely. There are many who can absorb knowledge. How many can reproduce it? Few indeed are the men capable of this high task, and, as we turn over the leaves of Rosenberger's volumes, we sigh as we witness the striking results of investigator after investigator washed in the waters of Lethe.

The fruits of the labours of Berthollet in chemistry remained ungathered till Professor Ostwald put his hands to this tree of knowledge. Berthollet and Bergmann gave rise to the view of the "manifold play of forces acting to and fro, the result being that every one gets its due. The more powerful substance gets more, the weaker less. Only in cases where one of the possible compounds in consequence of its properties entirely leaves the field of contest, either by falling down as insoluble or escaping as gas, can that complete decomposition take place which Bergmann held to be the normal result." * These ideas were neglected till 1867 when two Norwegian chemists, Guldberg and Waage, "put the ideas of Berthollet into precise mathematical form and subjected the resulting equations to the test of observation and verification." † Another chemist, Berthelot, revived Bergmann's theory in his famous third law derived from thermo-chemistry, and this in turn was corrected by Willard Gibbs,‡ who also endured neglect for a generation. Bergmann anticipated some of the work of Berzelius. Berthollet favoured the view that heat was a material substance, for did he not belong to the range of ideas favoured by the French physicists brought up in the school of Newton and Laplace? In chemistry Laplace dominated Berthollet, who held himself aloof from the teaching of a founder of chemistry, Lavoisier. Presuppositions were present to his mind. Influenced by the mathematical theory of attraction and by the mechanical laws of equilibrium, on which Laplace and his school laid so much stress, Berthollet sought to coordinate chemical affinity with what he called astronomical attraction. There was a germ of truth in Berthollet's ideas, but Proust and Richter came along with their theory of fixed proportions, and the work of Berthollet sank into oblivion.§

The atomic theory of Dalton was as ill received in England on its discovery in 1803 as it was well received on the Continent. His English contemporaries paid as scanty

* W. Ostwald, *Die Energie und ihre Wandlungen*, p. 20.

† *Ibid.*, p. 21.

‡ W. Ostwald, *Lehrbuch der allgemeinen Chemie*, II, p. 163 (pt. II); Berthelot, *Comptes Rendus*, p. 118.

§ W. Ostwald, *Lehrbuch der allgemeinen Chemie*, II, p. 557 (1st ed.); *Die Energie und ihre Wandlungen*, p. 20; H. Kopp, *Entwickelung der Chemie*, p. 271 ff.

attention to it as Bacon paid to Harvey's discovery of the circulation of the blood or to Napier's invention of logarithms. Yet the facts on which Dalton based his theory are incontrovertible. Sir Humphry Davy propounded objection after objection: that was his contribution to Dalton's mighty achievement.

By his celebrated hypothesis on the behaviour of equal volumes of different gases towards pressure, temperature, and chemical combination, Pietro Avogadro took in 1811 the first step towards the establishment of the atomic theory of matter. Forced by difficulties in his hypothesis, he conceived the idea of a compound atom or particle which he called the molecule. His conception commended itself to Ampère. It was, however, 1840 before a chemist of the standing of Laurent would consider it. It is curious to observe that there was a "radicle" or German way of looking at the atomic theory and a "type" or French way of looking at it. Avogadro and Ampère were in a position to explain, for instance, how a certain number of molecules of hydrogen were able to combine with an equal number of molecules of chlorine.* Avogadro put forward this significant fact, and it suffered neglect. Ampère also put it forward, and it similarly suffered neglect.

In 1849 Foucault showed the direct reversal of the sodium line in the spectrum of the electric arc. His anticipation of a fruitful line of activity was lost sight of till Gustav Kirchhoff took it up. He wrote in 1859: "I conclude that coloured flames in the spectra of which bright lines present themselves, so weaken rays of the colour of these lines, when such rays pass through them, that in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in which these lines are otherwise wanting." † Kirchhoff "at once gave birth to two great applications of this principle—the search, through the study of the spectra of distant stellar sources of light, after the ingredients which are present in those distant luminaries, and the search, through the study of the flames of terrestrial substances,

* A. Wurtz, *Théorie Atomique*, p. 64; A. Rau, *Die Theorien der modernen Chemie*, II, p. 107 ff.

† Sir G. Stokes, *Philosophical Magazine*, March 1860, p. 194 ff. Cf. J. Scheiner, *Astronomical Spectroscopy*, p. 148.

for new spectral lines announcing yet undiscovered elements."

Geology, chemistry, and physico-chemistry all alike witness to the indifference of the followers of these subjects. Discoveries are made, and are made to be ignored or to be forgotten. There is a book supposed to be written on the snakes of Iceland, and when you open it you turn over page after page till in the middle of it you meet the sentence, "There are no snakes in Iceland." There are at times when we feel sorely tempted to think that not a few scientific men have come to the conclusion, There are no discoveries in science, or rather, There are no discoveries in science except those that fit in with our preconceived ideas. Save on some such hypothesis as this, how are we to account for so MANY cases of neglect? A case here and a case there we might understand, but we encounter many cases. What are we to conclude? All the names we have given are those of men admittedly to-day in the foremost position. They are not the names of second-rate and third-rate observers.

The indifference and the neglect we behold in geology, chemistry, and physico-chemistry we behold in the biological sciences, and, above all, as we shall see, in mathematics and mathematical physics. Christian Conrad Sprengler was a naturalist filled with enthusiasm who "after being ejected from the rectorate of Spandau for neglecting his flock in favour of flowers, settled down to a frugal life in Berlin, and gave lessons in language and botany. The commonest plant became new by what he had to say about it; a hair, a spot, gave him opportunities for questions, ideas, investigations."* He pointed out that many flowers are "dichogamous"—that is, that though the organs for self-fertilisation exist in the same flower, nevertheless because of a want of time-keeping or for other reasons, pollination is carried out by crossing, wherein the visits of the insects are, through elaborate existing arrangements, instrumental. In 1793 he published his astonishing book, *The Secret of Nature discovered in the Structure and Fertilisation of Flowers.* Johann von Sachs, in his *Geschichte der Botanik,* considers Sprengel's little work to contain "the first attempt to explain the genesis of organic forms out of definite relations to their

* J. A. Thomson, *The Science of Life*, p. 192.

environment." * To-day it is a classic, but from 1793 to 1859 it remained on the topmost shelves of the library, covered with the dust of sixty years.

In 1837 Darwin had written in his note-book, "Do not plants which have male and female organs together, yet receive influence from other plants?"† The answer to such a question seemed to him "full of truth." The answer had already been given by Sprengel forty-four years before. In other words, Darwin was doing work that had been done once for all. In 1841 he heard of *The Secret of Nature* through Robert Brown, who, "in common with the rest of the world, looked on Sprengel's ideas as fantastic." ‡

Ah, yes, remarks the objector, this happened during the first half of the nineteenth century. To-day scientific periodicals are taken in every laboratory, and such scandalous neglect is utterly out of the question. Is this really the case? We shall come to Fabre in a moment, but now we must glance at the career of that astonishing Russian, Karl Ernst von Baer. § As we all know, the ancestors of many Russians like von Baer have gone to Russia just as the ancestors of the Huguenots like Emil Du Bois Reymond have gone to Germany. Von Baer (1792—1876) broke the spell that Cuvier had cast on the natural sciences. He stands midway between Cuvier and Darwin. With his geographical and anthropological studies, he combined his outstanding morphological labours, and all of it is characterised by a rare breadth of mind and an even rarer sense of balance. T. H. Huxley shows that von Baer recognised development as the "sole basis of zoological classification; while in France Cuvier and Geoffroy Saint-Hilaire were embittering each other's lives with endless merely anatomical discussions and replications, and while in Germany the cautious study of nature was given up for the spinning of Natur-philosophies and other hypothetical cobwebs."|| Von Baer's work was negative as well as positive. His negative work was to get

* J. von Sachs, *Geschichte der Botanik.*

† F. Darwin, *Life of Darwin,* I, p. 90; III, p. 257.

‡ *Nature,* 1874, p. 80.

§ His *Autobiography* was published in 1865, and Stieda published his *Life* in 1877. Professor R. Stolze gives many documents in his *K. E. von Baer und seine Weltanschauung.*

|| T. H. Huxley in Taylor, *Scientific Memoirs,* New Series, p. 176.

rid of the metaphysical speculations that were hampering the rising school of biology. Some men competent to judge assign to him the position of being the greatest embryologist of his age, if not indeed of all time. By his positive work he augmented our knowledge of the early development of the germs of animals by discovering the ovum in the body of mammals before fructification.* Nor is this by any means his sole contribution to embryology. He pursued his researches in order to perceive what light the facts of classification threw on the facts of development. In effect, he raised the question, Does the changing embryo of the higher animal gradually pass through the permanent forms of the lower animals? † Do animals recapitulate in their own development the ancestry of the race? As Arthur Milnes Marshall put it (1852—1893), "They climb up their genealogical tree."

Opposed as he was to extreme Darwinianism, von Baer recognised that "the higher and lower development of the animal coincides perfectly with that histological and morphological differentiation which gradually arises in the course of the development of the individual." ‡ Development is in truth the establishment of differences. He sums up his conclusions when he states that the "development of an individual of a certain animal form is determined by two conditions: first, by a progressive development of the animal by increasing histological and morphological differentiation; secondly, by the metamorphosis of a more general form into a more special one." § It is melancholy to have to add that the greatest embryologist of all time, though he was known in Russia and Germany, was completely unknown elsewhere. In England, W. B. Carpenter and T. H. Huxley drew attention to his manifold activities. The latter gave extracts from von Baer's principal writings thirty years after the Russian had begun his researches. The reaction against natural selection is now in full swing, and there is

* Cf. J. A. Thomson, *The Science of Life*, p. 123. Cf. also *Lebensgeschichte Cuvier's von H. E. Baer*, ed. Stieda, 1897, p. 72.

† K. E. von. Baer, *Ueber Entwickelungsgeschichte der Thiere Beobachtung und Reflexion*, fifth scholion. Cf. T. H. Huxley in Taylor, *Scientific Memoirs*, pp. 186, 189.

‡ T. H. Huxley in Taylor, *Scientific Memoirs*, p. 219.

§ *Ibid.*, p. 220.

every reason to expect that at last the writings of von Baer, who passed away in 1876, will at last come into their own. But look at the time that day has been deferred! It took Huxley thirty years, and it is going to take us—if von Baer's writings really are read—close on a hundred.

The life of Jean Henri Casimir Fabre extends to almost a hundred years, for he was born in 1823 and lived to 1915. He was always that incomparable observer Darwin deemed him to be. The English scientist read the first volume of Fabre's *Souvenirs,* dying before the second volume of them appeared in 1883. In spite of the recognition of men of the rank of Darwin, the neglect of Fabre persisted to the year 1910. By then he had reached the mature age of eighty-seven, and surely that was a late stage in his life for recognition at last to be flung to him. True, this recognition continued to increase from 1910 to 1915, so that Fabre had gleams of sunshine to brighten the declining days of one who suffered the bitter winter of neglect. It is, we fear, perfectly possible even in our day for a man to do research of the highest class, and for this work to fail in securing recognition.

On November 30, 1897, Lord Rayleigh, the President of the Royal Society, delivered the address at the anniversary meeting, and the burden of it was the neglect which so often fell to the scientific pioneer of new paths. Think of the President of the Royal Society facing the assembled Fellows of that august body, and the chief matter in his address is the ignorance and neglect of work done—by the very scientists who were present! For the Fellows of the Royal Society number among them—or ought to number among them—all the ablest scientists of the day. " For the advancement of science," Lord Rayleigh announced, " the main requirement is, of course, original work of a high standard, adequately explained and published. But this is not enough. The advances so made must be secured, and this can hardly be, unless they are appreciated by the scientific public. In some branches of Pure Mathematics it is said that readers are scarcer than writers. At any rate the history of science shows that important original work is liable to be overlooked, and is perhaps the more liable the higher the degree of originality. The names of T. Young, Mayer, Carnot, Waterston, and B. Stewart will suggest themselves to the physicist; and in other branches, doubtless, similar lists might be made of workers whose labours remained neglected for a shorter or a longer time. In looking into the more recent progress of Geometrical Optics, I have been astonished to find how little correlation there has been between the more important writings. That Coddington should have remained unknown in Germany and von Seidel in England need not greatly surprise us; but in this subject it would appear that a man cannot succeed in making even his countrymen to attend to him. Coddington seems to have heard nothing of Cotes and Smith, and Hamilton nothing of Airy and Coddington.

It is true that no two writers on theoretical subjects could differ more in taste and style than do Hamilton and Coddington. The latter addressed himself to special problems, the solution of which seemed to have practical importance. Among his achievements was the rule relating to the curvature of images, generally known as Petzval's, although Petzval's work was of much later date. Hamilton, on the other hand, allowed his love of generality and of analytical developments to run away with him. In his *Memoir on Systems of Rays*, with its elaborate and rambling supplements, there is little to interest the practical optician, though the mark of genius is throughout apparent. It was only in two or three pages of a later paper that he applied his powerful methods to the real problem of Optics. As Finsterwalder has remarked, his "six radical constants of aberration," expressing the general properties of a symmetrical instrument, are at once an anticipation and a generalisation of von Seidel's theorems. But the published work is the barest possible summary. If Hamilton had been endowed with any instinct for Optics proper, he could have developed these results into a treatise of first-class importance. In more recent times Hamilton's footsteps have been followed by Maxwell as well as by Thiesen and Bruns, of whom the two latter do not seem to have realised that Hamilton (or even Maxwell) had concerned himself with the subject at all. The natural development of Hamilton's ideas will be found in an able memoir by Schwarzschild (1905).

I have spoken of English work that lay neglected, but a scarcely less notable instance is the splendid discovery of the microscopic limit by Fraunhofer, a man who combined in the highest degree practical skill with scientific insight. Thanks to the researches of Abbe and Helmholtz, it is now well known that there is a world that lies for ever hidden from our vision, however optically aided; but neither of these eminent men realised that the discovery had been anticipated by Fraunhofer. Some, perhaps, may doubt whether Fraunhofer's argument, founded upon the disappearance of spectra from gratings of extreme fineness, is of adequate cogency. To this I may reply that I was myself convinced by it in 1870, before either Abbe or Helmholtz had written a word upon the subject." *

The annals of the nineteenth century bear testimony to many examples of neglect besides those mentioned by Lord Rayleigh. Among its most outstanding names is that of Laplace (1749—1827). His attitude to the universe was a purely impersonal one. There are forces at work, and his object was to understand the nature of those forces. There was therefore no sceptical tendency in his mind when he told Napoleon his reason why in the volumes of his masterpiece, the *Mécanique Céleste,* the name of God did not appear. His answer meant that personality did not come within the scope of his labours: " Sire, je n'ai pas besoin de cette hypothèse." In his book he gave, between 1799 and 1825, an admirable analysis of the outcome of Newton's work. True, Newton's body had lain in the grave—if we take the latter date—for ninety-eight years, but what of that? If nature never makes a leap, why should scientists be in a hurry? Of course the higher the originality of the work, the less danger of any scientist being in an undue hurry. Many investigators had been labouring since Newton's death in 1727, and Laplace had popularised their labours in his *Exposition du Système du Monde,* which appeared in 1797. Behind the *Mécanique Céleste* and the *Exposition du Système du Monde,* we perceive the whole time the *Principia* is looming in the background of Laplace's thought.† It is not a little remarkable to note that much as Laplace busied himself with the

* *Royal Society Proceedings,* Series A, LXXX, 1908, p. 239 ff.

† On the influence of Newton, cf. R. Wolf's informing *Handbuch der Astronomie, ihrer Geschichte und Litteratur.*

calculation of the combined effects of gravitational forces at various points in space, he entirely ignored the question how such effects come about. Kant thought that he had come too soon, and the writings of Laplace bear their witness to the pathos of this fact. Forty years before Kant put forth his nebular hypothesis,* Laplace, in complete ignorance of the work of the Königsberg philosopher, performed the task once again. There is a certain nemesis in the circumstance that just as Laplace neglected to read Kant, so some scientists in the opening decades of the nineteenth century placed the *Mécanique Céleste* in the dusty recesses of their library.

We know no one we can quite range alongside Laplace, except Lagrange, who is one of the greatest mathematical geniuses of the nineteenth century. At a not too respectful distance, however, we think Evariste Galois (1811—1831) claims a place. Cut off in his twenty-first year, he did work of rare suggestiveness. During his brief span of days it was beginning to be recognised that geometrical transformation had its counterpart in the transformation of algebraical forms by the processes of substitution. These processes of substitution received some attention at the hands of young Galois. "Le mérite de Galois," points out Professor Sylow, "ne consiste pas essentiellement dans ses propositions, mais dans la généralité de la méthode qu'il appliqua. C'est son admirable théorème fondamental qui a donné à la théorie des équations sa forme définitive, et d'où est sortie, en outre, la théorie des groupes généralisée, qui est d'une si grande importance, on peut le dire, pour toutes les branches des mathématiques, et qui déjà, entre les mains de Jordan, de Klein, de Lie, de Poincaré et d'autres, a enrichi la science d'une longue suite de découvertes importantes." † From 1831 the fragments of Galois remained unpublished, and of course unrecognised. Then Liouville published them in 1846.‡ When submitted to the Académie Française des Sciences, Lacroix and Poisson had reported on them as almost unintelligible. In 1866 Serret made some attempt to render the ideas of Galois accessible to the general public interested

* Lord Kelvin, *Popular Lectures and Addresses,* II, p. 65; G. F. Becker, *American Journal of Science,* V, 4th series, 1898.

† Professor Sylow's paper on Abel's work is in the "Memorial Volume," and our quotation is from p. 24.

‡ Liouville, *Journal,* II; cf. Picard's reprint.

in science.* The first really valuable step to make them generally known was taken by Camille Jourdain when he dealt with them in his *Théorie des Substitutions,* published in 1870. Forty years after his premature death, one scientist perceived in his short papers the germs of the quite novel and comprehensive "Theory of Groups." There is no more competent judge than Arthur Cayley (1821—1895), and those who require to know his opinion of Galois can note it in the *Encyclopædia Britannica.* It is a tribute paid by one master-mathematician to another master-mathematician.†

The importance of Evariste Galois's "Theory of Groups" is evident in all that Marie Sophus Lie (1842—1899) did to 1877. This outstanding Norwegian thinker believed that this theory was destined to fill a central position in the mathematical science of the future. "The conception of Group and Invariant was for him [i.e. Lie]," writes M. Nöther,‡ "not only a methodical aspect from which he intended to review the entire older region of mathematics, but also the element which was destined to unify the whole of mathematical science." For many a weary day after he set forth his wonderful conception, his numerous writings remained without any sign that the scientific world had ever heard of it. "Like my teaching or dislike it," said one of our teachers at school, "but don't say you don't care."

On the principle that, as Willard Gibbs found to his cost, no good thing could come out of Connecticut, so Lie found that scientists clearly thought that no good thing could come out of Oslo. If it did, they did not care for it. In process of time M. Picard § and Henri Poincaré—but what was there that Poincaré had not the intuition to see—came to perceive the worth of Lie's ideas.|| The man, however, who brought Lie's conception before the world of science was undoubtedly F. Klein, whose "Erlangen Programme" of 1872, entitled *Vergleichende Betrachtungen über neurere geometrische Forschungen,* forced its members to see,

* Serret, *Algèbre Supérieure.*

† Cf. Cayley's article on "Equation," p. 32.

‡ *Mathematische Annalen,* XLIII.

§ *Traité d'Analyse,* 1896, III.

|| Cf. *Ency. Math. Wiss.,* II, p. 402; Nöther, *Mathematische Annalen,* XLIII, p. 22; F. Meyer, *Bericht,* p. 231; *Ency. Brit.* art. on the "Theory of Groups."

whether they were willing or not. Cayley and James Joseph Sylvester (1814—1897) had been working on lines similar to Lie's. The form in which our mathematicians accept the invariant theory is the form in which the great Sylvester presented it. The terminology of it which they accept is the terminology he introduced. Still, it was not till 1897, when Professor Burnside published his *Theory of Groups of Finite Order,* that Lie's points came before the average F.R.S. Lie had begun his work in 1873, and he threw himself so whole-heartedly into it for the next three years that he once spoke of himself as having, during that period, lived only among his groups of transformations. In 1877 he published some results in a number of memoirs in a new journal in Oslo, edited by Sars, Müller, and himself, and some of them in the *Mathematische Annalen,* a paper that surely ought to have rendered them accessible. He suffered severely from the entire lack of interest bestowed upon his papers, for the mathematicians practically said, despite men like my old teacher, "We do say we don't care." Mr. A. R. Forsyth writes that Lie's "story at this time reads like the occasional experience of the investigator who lives, remote from his fellow-workers and unstimulated by eager pupils, voyaging through his sea of thought alone, at the end finding himself weary, isolated, unacknowledged, perhaps therefore discouraged, and certainly left uncheered by any confident satisfaction that others are following him." * He went through his Gethsemane alone, and his *cri du cœur* moves more than the morbidly sensitive.

Like Gauss, who suffered in the same fashion for a while, Sophus Lie ceased to look after invariants, and busied himself for the rest of his life with differential geometry. In 1886 he left Oslo for the chair of mathematics at Göttingen. "Unhappily," speculates Mr. A. R. Forsyth, "recognition appears to have been, not merely slow in coming, but almost too late when it came. There is no doubt that his ceaseless activity in thought and work had undermined his strength, and his spirit had brooded in lone-

* *Proceedings, Royal Society,* LXXV, 1905, p. 64. It is greatly to be desired that this Society should gather the obituaries of its Fellows into single volumes occasionally. Vol. LXXV is a notable saving of time to him who wants to grasp the personalities of scientists. We are not all Laplaces in our indifference to personality.

liness." * At Göttingen honours came to him. He received the honorary or foreign membership of societies and academies in great numbers. Four years before his death, he became a Foreign Member of our Royal Society. At the back of his mind lay the thought that from 1877 to 1899—he was only forty-seven when he died—he might have been working at the discoveries we now know to be so brilliant, and thanks to the absence of recognition he had not been able to execute the task he had meant to execute. Other tasks had come, but they were not—this is the sting of the whole matter—the tasks he had himself chosen when he was a man in the heyday of life. He was barely twenty-one when he set to work on his great contribution, and in his case it was certainly true that the thoughts of youth are long, long thoughts. As for his belated honours, he felt precisely as Thackeray felt. "When I was a lad," the great novelist remarked, "I wanted toffee but I hadn't a shilling. Now I'm a man and I have the shilling, but I don't want the toffee." What Sophus Lie wanted was recognition for his ideas in 1877: what he did not want was honours for himself. For he experienced the feeling that every true worker experiences, and that is that his conceptions are dearer, far dearer, to him than any membership of any Academy or any Society—when it comes far too late. Then he has the shilling, but what does he care for the "toffee"?

The fame of Henry Cavendish (1731—1810), son of Lord Charles Cavendish, has only come to be recognised in any widespread form during the last century. True, Clerk-Maxwell knew Cavendish to be that supreme genius he is now recognised to be. Was it that his interest in science was as passionless as that of Machiavelli was passionless in politics? He made discoveries, without giving a single sign that they were of signal value. Of enormous wealth, he was, as Biot expressed it, *le plus riche de tous les savans et le plus savant de tous les riches.* In secret and in solitude he began his work, and in secret and in solitude he continued to the end. As Darwin was content simply to watch when Chambers published a hasty book on evolution, so Cavendish was content simply to watch when men advo-

* *Proceedings, Royal Society,* LXXV, 1905, p. 67.

cated theories that he had demonstrated to be wholly erroneous. Everything came to him, quantitatively, and his family motto, "Cavendo tutus," marked the care he took with his experiments.

Tennyson sang:

> Yet all experience is an arch, wherethro'
> Gleams that untravelled world, whose margin fades
> For ever and for ever when I move.
> How dull it is to pause, to make an end,
> To rest unburnish'd, not to shine is use!
> As tho' to breathe were life. Life piled on life
> Were all too little, and of one to me
> Little remains; but every hour is saved
> From that eternal silence, something more,
> A bringer of new things; and vile it were
> For some three suns to store and hoard myself,
> And this grey spirit yearning in desire
> To follow knowledge like a sinking star
> Beyond the utmost bound of human thought.

Of all the poets of the Victorian Age, Tennyson was the most scientific in spirit, the one to whom the workings of the mind of the scientist were most openly revealed. Sentiments like these were far more foreign to Cavendish's nature than the pregnant remark of Pascal: "On se persuade mieux, pour l'ordinaire, par les raisons qu'on a soi-même trouvées, que par celles qui sont venues dans l'esprit des autres." No man was ever more single in his desire to know the causes of things than Cavendish, and we may be sure that he felt far more than three suns the happiness Virgil promised to those who understood those causes. Passionless as he undoubtedly was, he must have experienced some of the joys of the spirit as well as those of the mind. His lucid and subtle brain was content to record his observations in papers that have only seen the light really in 1921 when the two large volumes containing them were published by Sir Joseph Larmor, who edited incomparably the electrical and dynamical papers, and by Sir Edward Thorpe, who edited the chemical papers.* To the non-specialist reader the electrical volume is more absorbing than the chemical one. In the latter it is obvious that Cavendish was never able to free himself from the presupposition of the existence of phlogiston. He did, however, free himself from the almost

* H. Cavendish, *Scientific Papers,* 2 vols., Cambridge, 1921.

prehistoric belief, from the classical belief, in the four elements, earth, air, fire, and water, and he took a man's share in freeing others from the burden of this belief. As Sir Edward Thorpe shows, it was one of his papers which "gave a final and decisive blow to the conception of a universal air, elementary and primordial. That its true significance was everywhere clearly perceived is abundantly proved by the literature of the period. The Royal Society showed its appreciation of its merit by awarding its author a Copley medal." Trying to ascertain the true nature of air and fire, he furnished the first clear and incontestable proof of the compound nature of water, and of the nature and relative proportion of its constituents. He established that common air was sensibly uniform in character, and this for the first time.

Thomas Young was a scientist with cosmopolitan tastes who knew Cavendish well, and yet, to quote Sir Joseph Larmor's words, his "account of his [i.e. Cavendish's] electrical researches shows a complete ignorance of Cavendish's unpublished work, and this ignorance must have been shared by the whole scientific world." * Cavendish was aware of the leading nature of the "degree of electrification" or, in our phraseology, potential. He had no galvanometer, and he made none. Why indeed should he? For he himself was a natural one, able to compare in accurate terms numerically the intensity of the currents he caused to pass through his body. Then he compared the sensations he felt in his wrist and elbows, estimating which of the two shocks he felt the more powerful. Clerk-Maxwell remarks: "The accuracy which Cavendish attained in the discrimination of the intensity of the shocks is truly marvellous, whether we judge by the consistency of his results with each other, or whether we compare them with the latest results obtained with the aid of the galvanometer, and with all the precautions which experience has shown to be necessary in measuring the resistance of electrolytes." † Such an accurate human galvanometer almost staggers belief!

The thrill which Lord Rayleigh's announcement of the discovery of argon caused to the scientific world has

* H. Cavendish, *Scientific Papers,* I, p. 17.
† *Ibid.,* I, p. 25.

scarcely been damped out by the lapse of time. By patient whittling away of known constituents of the air—constituents which of course had long been believed to constitute it completely—he had found a small residuum which was thereto unknown. Lord Rayleigh himself had drawn attention to the fact that Cavendish had, a century earlier, also noticed a residual "small bubble." More than this, he himself repeated this early Cavendish experiment and showed that this "small bubble" must have consisted of argon and the other gases which were found later as a corollary. This anticipation of the most modern work astounds one, and it is by no means the only instance of the kind. What is the most recent development in meteorology? Undoubtedly it is the study of the constitution of the upper atmosphere. Cavendish began that study twenty years before anyone else, by arranging with a balloonist to collect samples of air at different heights. What is almost the last discovery in astronomy? Is it not that a ray of light is bent on passing near the sun? Cavendish computed the amount of bending more than a century ago. We cannot, of course, regard him as having handed down the succession of Einstein, for he undoubtedly held Newton's view that light consisted of material corpuscles. In between came a whole dynasty committed to the immateriality of light and the existence of ether on which gravity could not act. Lord Kelvin, for instance, "gave strong reason to feel certain that aether was outside the law of gravitation." It is notable that Cavendish obtained a result which is only half Einstein's, and is now known not to correspond with the facts. The remarkable point, however, is that he should in any wise have anticipated these essentially modern developments. In passing, we may remark that among the pecuniarily independent devotees of science there are such outstanding names as those of Cavendish, Herschel, Joule, Murchison, Spottiswoode, Lyell, and Darwin.

A man who could foresee in 1842 the principle of spectrum analysis was no common man. Accordingly, Bernhard Bolzano (1781—1848) had long given proof of his powers by his brilliant setting forth of the first rigid deduction of the various algebraical series.* His notions, in

* Cf. his tract on the Binomial Theorem.

the opinion of Hermann Hankel, as to convergency of series are "eminently clear and correct, and no fault can be found with his development of those series for a *real* argument (which he everywhere presupposes); in the preface he gives a pertinent criticism of earlier developments of the Binomial Theorem,* and of the unrestricted use of infinite series, which was then common. In fact, he has everything that can place him in this respect on the same level with Cauchy, only not the art peculiar to the French of refining their ideas and communicating them in the most appropriate and taking manner. So it came about that Bolzano remained unknown and was soon forgotten; Cauchy was the happy one who was praised as a reformer of the science, and whose elegant writings were soon widely circulated." † Still, Hankel has the merit to detect value in the forgotten conceptions of Bernhard Bolzano just as he had precisely the same merit to detect value in the forgotten conceptions of Hermann Grassmann. Indeed the line of reasoning started by Bolzano has been carried on and developed by Weierstrass with unexpected and important results.‡

In his *Puissance Motrice du Feu*, Sadi Carnot attempted in 1824 to determine mathematically the power of a steam-engine. His original memoir lays down that "the production of motion in steam-engines is always accompanied by a circumstance on which we must fix our attention. This circumstance is there—establishment of equilibrium, or level, in the caloric—that is to say, its passage from one body where the temperature is more or less elevated, to another where it is lower. . . . The production of moving force is therefore due in steam-engines, not to a real consumption of caloric, but to a transference from a hot body to a cold body." § Implicitly he had introduced into mathematical and physical science the question of the availability of the forces of nature. Following in the steps of Laplace, Black, and Fourier, he held that heat was an imponderable substance which might hide itself—might become latent—but could be neither created nor destroyed. Such a view was far

* On this point cf. O. Stolz, *Mathematische Annalen*, XVIII, pp. 255, 257.

† H. Hankel in his article on "Limit," p. 210.

‡ H. A. Schwarz, *Journal für Mathematik*, LXXIV, 1872, p. 22.

§ S. Carnot, *Puissance Motrice*, pp. 5-6 (1878 ed.).

from the mind of such a sagacious observer as Henry Cavendish, and it was also far from the mind of Davy and Rumford. There are specks on the sun, and we can afford to admit that there are specks on the reasoning of Carnot. Yet his speculations form the starting-point for the modern theory of thermo-dynamics. They were unknown in 1824 and they remained unknown long after 1824. Benoit Pierre Emile Clapeyron, an engineer, came across them in 1834, and he published in the *Journal de l'Ecole Polytechnique* * his "Mémoire sur la Puissance Motrice de la Chaleur." Through a translation of this paper in Taylor's *Scientific Memoirs,* Lord Kelvin heard of Carnot's early work, and he never rested content till he had found it.† Through a translation in Poggendorf's *Annalen,* in 1843, Helmholtz became acquainted with the great tract published in 1824.

Lord Rayleigh did more than read a Presidential Address to the Royal Society in 1897, for he took as lively an interest in the papers of other scientific men as Sir William Rowan Hamilton himself. The majority of scientists simply care for the plot they cultivate, and seldom trouble with the broad expanse before them. It was a sacred writer who pointed out that "where there is no vision the people perish," and science perishes with them. In the Archives of the Royal Society Lord Rayleigh found an abstract of a paper written by J. J. Waterston, who published books that are but little known. Interested by the brief reference, Lord Rayleigh perused the paper, finding, to his astonishment, that in 1845 Waterston had for the first time enunciated the conception that the temperature of a gas is to be measured by the *vis viva* or kinetic energy of the colliding molecules. In addition to this, his paper contained the first calculation of molecular velocity, and all this had been done ten to fifteen years in advance of his time, anticipating much of the work of Clausius, Joule, and Clerk-Maxwell. Waterston enunciates the principle that "in mixed media the mean square molecular velocity is proportional to the specific weight of the molecules." ‡

* 14th Cahier.

† Cf. S. P. Thompson, *Life of Lord Kelvin,* I, pp. 132, 210, 252, 256-8, 273, 279; II, p. 949.

‡ Lord Rayleigh, *Life of Lord Rayleigh,* p. 45.

As Samuel Johnson stood as a man in the market-place to atone for an act of disobedience as a boy, moved by a kindred feeling of compunction Lord Rayleigh took steps to have the paper published.

Waterston was Scots, and Lord Rayleigh wrote to Tait at Edinburgh, and Tait replied on February 13, 1891: "This promises to be a somewhat sensational inquiry—worthy of a detective! I have reached this point, that J. J. W. was a civil engineer, a near relative of people living here now, and that he was in the employment of the East India Company, but disappeared about seven years ago, and not a trace of him has been discovered in spite of the most anxious search." * One of the referees of the Royal Society reported on Waterston's work thus: "The paper is nothing but nonsense, unfit even for reading before the Society." Lord Rayleigh, persisting in his search, came into communication with George Waterston, a nephew of the writer of the paper. George Waterston wrote: "He [i.e. his uncle] talked however in a manner that seemed to me strangely contemptuous of scientific men with but few exceptions. He had not a word of complaint, nor did he speak of being neglected or ill-used, but I distinctly remember the Royal Society was characterised in very strong terms useless now to repeat. We have it on record what they thought of his paper. . . . He returned the compliment in no measured terms. He would not attend the meetings of the Royal Society of Edinburgh, though some friends sent him billets, and rather avoided the society of scientific men. He was of a most social, kind disposition, enjoying the society of young people. He never married, and besides his mathematical work he was fond of music, chess, and billiards.

"When in India he published a book, *Thoughts on Mental Phenomena*. It had no sale and he continued his physical and mathematical studies, contributing papers to the *Philosophical Magazine*. . . .

"His friends share your surprise that he should not have put forward more definite claims, but he was of a very retiring disposition. To me he appeared to put forth his papers like some mathematical question for others to tackle, and not being a scientific man I was never sure whether

* Lord Rayleigh, *Life of Lord Rayleigh*, p. 45.

all his contemptuous words of other scientific men were not the fruit of some exaggerated views of the importance of his own work, though he was always so simple and straightforward that I put it down to his not stating his views in a sufficiently practical manner. I remember at one time in a popular magazine seeing his name coupled with that of Mayer in regard to the heat of the sun, and when I spoke to him about it he simply made a grimace." *

The seminal mind of Karl Friedrich Gauss (1777—1855) required a generation or more before it penetrated the mind of the average able scientist. It was more than a quarter of a century before the spirit of exact research he introduced leavened in any wise the German Universities. He lived a life as lonely as that of Newton, the man he placed on a pedestal. For twenty years his explorations on the theory of numbers remained as unknown as Sophus Lie's, and when it at last came within the ken of mathematicians, it proved as inexhaustible as Newton's *Principia* had been a century earlier. The parallel between the English genius and the German genius might be drawn out far, and the neglect of their respective labours fills certainly one item in it, and the intuition, common to them both, fills another. We can —in part, at least—understand the feelings of Gauss when as an old man he heard Riemann touch a string, in a great dissertation, that had been vibrating in the master's mind for fifty years, unheard and unheeded by any other mathematician.† The reluctance to publish was common to Newton,‡ Gauss, and Cavendish, for Cavendish takes an illustrious place in the history of thought. It was twenty years before Newton's *Principia* or Gauss's *Disquisitiones Arithmeticæ* attained any adequate meed of recognition. The Académie Française des Sciences, after the manner of learned Academies, received both books with dignified but decided disapproval. Such a devoted member of the University of Cambridge as William Whewell, the Master of Trinity, undoubtedly is slow to admit that there was the least reluctance on the part of the University to accept Newtonian

* Lord Rayleigh, *Life of Lord Rayleigh*, p. 45. The *Dictionary of National Biography* contains no notice of Waterston.

† The Preface in Riemann, *Mathematische Werke*, ed. Weber, p. 517.

‡ K. F. Gauss, *Werke*, III, pp. 401-6; V, p. 627, an important passage.

philosophy.* It is, however, a well-established fact that the University of Edinburgh taught such philosophy thirty-five years before the University of its discoverer.†

Möbius and Plücker in Germany (1801—1868), recognising that too much attention had been bestowed by the French on mathematical analysis, turned their talents to refined geometrical researches, and their simultaneous labours long remained unknown and unrecognised. In our day, no doubt, their labours in the development of algebraic and geometrical methods bear notably on modern algebra and modern geometry. Plücker himself, curiously enough, was ignorant of the researches of his fellow-German, Möbius, and he was also ignorant of the *Traité* of Poncelet, published in 1822. Above all, he was ignorant of the mathematical theories of Poisson and Lord Kelvin as well as those of Gauss and Weber. For a long time even Gauss did not know the arithmetical discoveries of Fermat and the proofs of Euler, Lagrange, and Legendre.‡ Obviously, the President of the Royal Society might in the decade after 1867, with perfect propriety, have given an address of a nature similar to that of Lord Rayleigh's of 1897.

George Green (1793—1841) published his extremely striking essay on the application of mathematical analysis to the theories of electricity and magnetism in 1828, and for all the mark it left on scientific opinion it might as well have remained unpublished. The latest edition of the *Dictionary of National Biography* contains an article on Green, and in the course of it Mr. G. J. Gray, the writer of it, never once mentions the most important piece of work Green ever did.§ To-day it is universally recognised as containing some of the most illuminating conceptions of the nineteenth century. Not till 1845 did that ardent student, Lord Kelvin, contrive to catch sight of it,‖ and the vast majority of

* W. Whewell, *History of the Inductive Sciences,* II, p. 149 ff.

† Sir A. Grant, *The Story of the University of Edinburgh,* II, p. 296.

‡ K. F. Gauss, *Werke,* ed. Schering, I, p. 6; II, p. 444.

§ *Dictionary of National Biography,* VIII, pp. 485-6. We ought to say at the same time that from a scientific point of view, as well as of course from an historical point of view, this *Dictionary* is infinitely above the *Biographie Universelle and the Allgemeine Deutsche,* useful as both these series are.

‖ S. P. Thompson, *Life of Lord Kelvin,* I, pp. 45, 84, 99, 108, 113, 115, 117, 141; II, pp. 686, 822, 824, 827, 829, 831, 872-3.

scientists neither knew of it nor—what is infinitely sadder—wanted to know of it. He took it off to Paris when he succeeded in securing a copy from his tutor, Hopkins, and published it in *Crelle's Journal,* thus making it known—if anyone cared—as the fundamental treatment of the potential theory. Nor is Green the only case of such gross neglect, for McCullagh, the Dublin mathematician, and Sir Gabriel Stokes, that remarkable son of a Sligo rectory, suffered from it, though not quite so conspicuously as Green.

Jean Baptiste Joseph Fourier (1768—1830) published in 1822 his famous *Théorie Analytique de la Chaleur,* which, in the hands of Ohm and Lord Kelvin,* has been applied to physical science, and in the hands of Dirichlet and Riemann † to subtle mathematical conceptions.‡ For fourteen years the manuscript of it lay hidden among the archives of the French Institut.

Augustine Fresnel (1788—1827) with Thomas Young contributed in establishing the undulatory theory of light. The Académie Française des Sciences received his first memoir on the diffraction of light, and did not print it till 1826.§ Despite neglect and despite the opposition of the leading authorities, except Arago, he pursued his own course, though not altogether undauntedly. Other papers of his the Académie Française either lost or mislaid. There is reason to believe that the cavalier treatment of his papers was due to the opposition of Laplace and his party at the Institut, and this opposition even Arago failed to overcome. Réaumur also experienced the tyranny of the Académie Française.||

Neither Hermann Grassmann ¶ (1809—1877) nor Jakob Steiner attained the positions at Stettin and Berlin respectively worthy of their powers. In his *Ausdehnungslehre,* published in 1843, Grassmann introduced a novel

* H. L. E. von Helmholtz, *Vorträge und Reden,* I, p. 101 ff.; Lord Kelvin, *Mathematical and Physical Papers,* passim, and II, p. 41 ff. especially.

† B. Riemann, *Mathematische Werke,* p. 218; G. A. Gibson, *Proceedings of the Edinburgh Mathematical Society,* XI and II.

‡ E. Mach, *Principien der Würmlehre,* p. 78 ff., 116 ff.

§ Sir J. Herschel in his article on "Light" in the old *Ency. Metrop.* Cf. W. Whewell, *History of the Inductive Sciences,* II.

|| A. Maury, *Les Académies d'Autrefois,* I, pp. 123, 280; T. H. Huxley, *Critiques and Addresses,* p. 112.

¶ Cf. Schlegel, *Hermann Grassmann and his Grassmann'sche Ausdehnungslehre.*

fashion of considering geometrical relations. His is a science of pure extension, the application of which to empirical space is geometrical. He began in 1844 what Riemann continued in 1854. For his investigations he considered space of three dimensions to be simply a particular case of pure extension in any number of dimensions, which are not necessarily determined by the same propositions as our empirical space. Lacking appreciation, Grassmann translated the *Rig-Veda* in 1876 and 1877, and composed a dictionary for it in 1872 to 1875. Like Young, he was a mathematico-physical student who was also a philological student. Alas! there is another resemblance, for both had to endure the prospect of no audience for their ideas. Alas! the admirer of Young, Helmholtz, came close to the researches of Gauss and Plücker, and passed them by. The writings of Young, like those of Faraday, lay buried in print with none to erect a monument over the grave. Comte praised Gall the phrenologist, censured Cuvier, condemned Young's undulatory theory of light, and spoke derisively of the "abuse of microscopic investigations."

Michael Faraday (1791—1867) was an experimentalist of unsurpassed and perhaps unsurpassable genius and divined the nature of magnetic and electrical action, bringing his divination invariably to the test of the laboratory.* Though not a trained mathematician, yet in spite of this serious hindrance † Clerk-Maxwell was able to translate his far-reaching conceptions into mathematical language. In process of time both Clerk-Maxwell and Helmholtz ‡ came to recognise the translation of Faraday's lines of force in the magnetic and electrical phenomena he taught them to gaze at through his eyes. While his experiments attracted attention, his theories attracted but little till Clerk-Maxwell, who performed for him the task of popularising that Voltaire performed for Newton, and Helmholtz compelled scientists to perceive the enormous worth of his theories. France and

* For Faraday's life there are Bence Jones's two volumes and Tyndall's graceful sketch. There is need of a fresh biography which will take into account recent developments of Faraday's views. No such book exists, and we earnestly hope it will soon exist.

† For other hindrances cf. H. Bence Jones, *Life and Letters of Faraday*, II, p. 344, and his *The Royal Institution*, p. 311.

‡ H. L. F. von Helmholtz, *Vorträge und Reden*, II, p. 277.

Italy became aware of the importance of Faraday's views before his own countrymen, who bestowed too little attention on the conceptions of his transcendent genius. His electrolytic law exercised hardly any influence on the development of chemistry.* The labours of Sir J. J. Thomson and Sir William Crookes in our day and of Lord Kelvin in his day on the discharge of electricity in rarefied gases bring us back to the "dark discharge" Faraday witnessed in 1838 in the days before there was either spectrum analysis or vacuum tubes.† It is startling to find that the researches of Faraday could remain unknown in Germany just as those of Cavendish were unknown in France or even as those of Coulomb were unheard-of in England. The labours of Gauss, save by Sabine, were also ignored by our countrymen. Of course this means that the same problems were attacked in the different countries, and sometimes attempted by men who, blissfully unaware of this circumstance, were once more attempting their solutions. There is conservation of energy in the world of matter. There is none in the world of mind. One point in writing this book is to draw the attention of scientists to this circumstance, and to show that while there has not been conservation of energy in the world of mind, is that any sufficient reason why there never should be any such mental conservation? This mental conservation is one of the most urgent needs of this moment.

John Tyndall wrote with all his wonted power in his spirited sketch of *Faraday as a Discoverer.* He points out that "the objects of scientific thought being the passionless laws and phenomena of external nature, one might suppose that their investigation and discussion would be completely withdrawn from the region of feelings, and pursued by the cold dry light of the intellect alone. This, however, is not always the case. Man carries his heart with him into all his works. You cannot separate the moral and the emotional from the intellectual; and thus it is that the discussion of a point of science may rise to the heat of the battlefield.

* H. L. F. von Helmholtz, *Wissenschaftliche Abhandlungen,* III, the Faraday Lecture, II, W. Ostwald, Der Allgemeine Chemie, II, Part I, p. 530.

† Lord Kelvin, Presidential Address before the Royal Society, November 1893.

The fight between rival optical theories of Emission and Undulation was of this fierce character; and scarcely less fierce for many years was the contest as to the origin and maintenance of the power of the voltaic pile. Volta himself supposed it to reside in the contact of different metals. . . . Volta's theory of metallic contact was so clear, so beautiful, and apparently so complete, that the best intellects of Europe accepted it as the expression of natural law.

"Volta himself knew nothing of the chemical phenomena of the pile; but as soon as these became known, suggestions and intimations appeared that chemical action, and not metallic contact, might be the real source of voltaic electricity. This idea was expressed by Fabroni in Italy, and by Wollaston in England. It was developed and maintained by those 'admirable electricians,' Becquerel of Paris and De La Rive of Geneva. The Contact Theory, on the other hand, received its chief development and illustration in Germany. It was long the scientific creed of the great chemists and natural philosophers of that country, and to the present hour there may be some of them unable to liberate themselves from the fascination of their first-love.

"After the researches which I have endeavoured to place before you, it was impossible for Faraday to avoid taking a side in this controversy. He did so in a paper, 'On the Electricity of the Voltaic Pile,' received by the Royal Society on April 7, 1834. His position in the controversy might be predicted. He saw chemical effects going hand-in-hand with electrical effects, the one being proportional to the other; and, in the paper now before us, he proved that when the former was excluded, the latter were sought for in vain. He produced a current without metallic contact; he discovered liquids which, though competent to transmit the feeblest currents—competent therefore to allow the electricity of contact to flow through them if it were able to form a current—were absolutely powerless when chemically inactive. . . .

"The memoir of the *Electricity of the Voltaic Pile,* published in 1834, appears to have produced but little impression upon the supporters of the contact theory. These indeed were men of too great intellectual weight and insight lightly to take up or lightly to abandon a theory. Faraday therefore resumed the attack in a paper communicated to the Royal Society on February 6, 1840. In this paper he hampered his antagonists by a crowd of adverse experiments. He hung difficulty about the neck of the contact theory, until in its efforts to escape from his assaults it so changed its character as to become a thing totally different from the theory propounded by Volta. The more persistently it was defended, however, the more clearly did it show itself to be a congeries of devices, bearing the stamp of dialectic skill rather than that of natural truth.

"In conclusion, Faraday brought to bear upon it an argument which, had its full weight and purport been understood at the time, would have instantly decided the controversy. 'The contact theory,' he urged, 'assumes that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, *can arise out of nothing* *: that without any change in the acting matter, or the consumption of any generating force, a current shall be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power,* and is like no other force in nature. We have many processes by which the *form* of the power may be changed, that an apparent *conversion* of one into the other takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the conversion of electricity into magnetism. *But in no case, not even in those of the Gymnotus and Torpedo, is there a pure creation or a production of power without a corresponding exhaustion or something to supply it.*'

"These words were published more than two years before either Mayer printed his brief but celebrated essay on the Forces of Inorganic Nature, or Mr. Joule published his first famous experiments on the Mechanical Value of Heat. They illustrate the fact that before any great scientific

* All the italicised portions are so done by Tyndall.

principle receives distinct enunciation by individuals, it dwells more or less clearly in the general scientific mind. The intellectual plateau is already high, and our discoverers are those who, like peaks above the plateau, rise a little above the general level of thought at the time." *

Long as our quotation has been, its length has been required in order to reinforce our conclusion that a man who soared so high in the intellectual heavens was eclipsed during and after his day.

Faraday and Georg Simon Ohm did work on similar lines, though Ohm occasionally anticipated his great contemporary. This he notably did in 1827,† when he established the proportionality of the quantity of electricity passing through a circuit with the same electro-motive force in the same conductor.‡ Ohm also introduced the conception of electric resistance, and showed how this varies as the length and inversely as the thickness of the same conductor, and is different in different conductors. Fechner and Pouillet tested Ohm's law, finding it true. In spite of this confirmation of his results, Frenchmen remained sceptical of its validity and Englishmen scarcely heard of it. With us the labours of Ohm shared the same fate as those of Laplace, whose astronomical attitude to nature remained unknown except to a few specialists. Weber followed a train of ideas similar to those of Laplace, with the outcome that our text-books passed it by. Clerk-Maxwell announced that Faraday and himself were opposed to Weber's theory.

In 1831, in his *Krystallometrie,* Hessel introduced a strictly geometrical treatment of problems in planes of symmetry, taking the lead in deducing the different possible forms of symmetry and in showing that in all thirty-two different forms of symmetry or groups are geometrically possible.§ These thirty-two fundamental groups of crystals fall into six classes, according to the different systems of

* J. Tyndall, *Faraday as a Discoverer,* p. 73 ff.

† Cf. his *Die galvanische Kette, mathematisch bearbeitet.*

‡ Cf. Lommel's introduction to Ohm's collected papers, *Gesammelte Abhandlungen,* VII; Sir C. Wheatstone, Bakerian Lecture, 1843, in *Philosophical Transactions,* 1843, p. 303 ff.; Lord Kelvin, *Popular Lectures and Addresses,* I, p. 76.

§ Liebisch, *Physikalische Krystallographie,* pp. 3-50; Groth, *Physikalische Krystallographie,* p. 324 ff.

crystallographic axes or the number of planes of symmetry belonging to them. His *Krystallometrie* was forgotten for a generation, and then Bravais rediscovered it in his *Etudes Crystallographiques,* which appeared in 1851.

In the course of his short career James McCullagh (1809—1847) did geometrical work sufficient almost to place him beside Chasles and Poncelet. He disagreed with Fresnel in holding that the vibrations of plane-polarised light are parallel to the plane of polarisation, and Fresnel proved right. This does not, however, take from the high degree of value possessed by McCullagh's geometrical papers and by his able attempts to construct a dynamical theory of the luminiferous ether. Sir Joseph Larmor illuminatingly traces the modern vortex theory beyond Macquorn Rankine to McCullagh. Sir Joseph Larmor thinks that in his *Essay towards a Dynamical Theory of Crystalline Reflexion and Refraction,** McCullagh "arrived at a type of elasticity (of the ether) which was wholly rotational, . . . somewhat after the manner that a spinning flywheel resists any angular deflection of its axis." † Sir Joseph Larmor proceeds to point out that "Rankine, never timid in his speculations, expounded McCullagh's analytical scheme soundly and clearly, in full contrast with the elastic properties of matter, as representing a uniform medium or plenum endowed with ordinary inertia, but with elasticity of purely rotational type." ‡ However, "up to the period of Lord Kelvin's vortex atoms . . . the earlier theories . . . could only have been hypothetical speculations." § McCullagh's extant papers remained but very imperfectly known. He died by his own hand in a fit of temporary insanity, induced by his devotion to science. Careful search was made of his manuscripts for his physical and geometrical investigations, which it is well known he had ready for publication, but no trace of them could be found.

William James Macquorn Rankine (1820—1872) was a civil engineer who spent his leisure in a series of researches on molecular physics. Rankine the Scots, Clausius the

* *Transactions of the Royal Irish Academy,* 1839.
† Sir J. Larmor, *Aether and Matter,* 1900, p. 26.
‡ *Ibid.,* p. 77. Cf. p. 73.
§ *Ibid.,* p. 25, note.

German, and Lord Kelvin the Ulsterman were the three who realised profoundly the implications of Carnot's *Puissance Motrice du Feu*, a tract that Lord Kelvin had sought for as ardently as Green's tract. Rankine, Clausius, and Kelvin are the three founders of theoretical thermodynamics. From 1850 onwards Rankine put forward his theory of molecular vortices, "which assumes that each atom of matter consists of a nucleus or central point enveloped by an elastic atmosphere." * Clerk-Maxwell thought in 1878 that "whatever he [i.e. Rankine] imagined about molecular vortices was so clearly imaged in his mind's eye that he, as a practical engineer, could see how it would work. However intricate, therefore, the machinery might be which he imagined to exist in the minute parts of bodies, there was no danger of his going on to explain natural phenomena by any mode of action of this machinery which was not consistent with the general laws of mechanism. Hence, though the construction and distribution of his vortices may seem to us as complicated and arbitrary as the Cartesian system, his final deductions are simple, necessary, and consistent with facts. Certain phenomena were to be explained. Rankine set himself to imagine the mechanism by which they might be produced. Being an accomplished engineer, he succeeded in specifying a particular arrangement of mechanism competent to do the work." This procedure was exactly in keeping with that of Lord Kelvin, who never felt happy till he had reduced his conceptions to a working model. Then—but not till then—he realised that he was making progress. With perfect truth, therefore, Clerk-Maxwell informs us that "as long as the training of a naturalist enables him to trace the action only of particular material systems, without giving him the power of dealing with the general properties of all such systems, he must proceed by the method so often described in histories of science—he must imagine model after model of hypothetical apparatus, till he finds one which will do the required work. . . . The theory of molecular vortices was distinguished from other theories which attribute motion to bodies apparently at rest, by the further assumption that this motion

* W. J. Rankine, *Scientific Papers*, p. 17.

is like that of very small vortices, each whirling about its own axis." *

The practical profession to which Macquorn Rankine belonged is enough to show folks that they are not dealing with a mere theorist—whatever that term of opprobrium may mean. Influential he has been at home, but uninfluential he has been abroad, especially in Germany. The reason of this lack of European fame arises from the circumstance that the theories of Clausius occupied so dominating a position that they left no room for the ideas of Macquorn Rankine.

Macquorn Rankine, James Thomson, and William Thomson (Lord Kelvin) gradually came to realise that there was a general doctrine of energy, breaking with the older physical theories in so doing. Chemistry had become an exact science since mathematics had been applied to it. These three came to hold that there ought to be a broader interpretation embracing more than certain restricted groups of natural phenomena. Such an interpretation led to the Phase Rule of Willard Gibbs of Yale, who proceeded in 1874 logically from a few of the general notions we possess on the subject of matter and energy. Contemplating actual aggregate of bodies, he found them in the condition approaching thermodynamic equilibrium on which the reduction of the Phase Rule depends. By the way of abstract theory, he introduced into physics the so-called semi-permeable membrane and the osmotic pressure against it, which now plays so fundamental a part in the study, and is regarded as a mode of expression of actual reversible energy-relations between solutions in nature. He marked out the channels of chemistry within which a scheme of reactions can proceed by aid of a discussion of the relations of co-existing states or phases of the material. With insight he considered the phenomena of interfacial films in relation to their physical and chemical combination. Willard Gibbs published his views in the *Transactions of the Connecticut Academy,* in a memoir of over three hundred pages, and they were as much buried there as those of Sophus Lie were in his Oslo journal. Clothed in mathematical form, European chemists were not quite prepared

* J. Clerk-Maxwell, *Nature,* 1878; his *Scientific Papers,* II, p. 662; P. G. Tait's memoir of Rankine in the *Collected Papers,* XXIX.

for them. Horstmann had done kindred work which remained long unrecognised.* True, Clerk-Maxwell † divined their value, but, like Henri Poincaré, of what did he not divine the value? The papers of Willard Gibbs, with this outstanding exception, were left severely alone. Helmholtz barely heard of them later. Gibbs's colleagues at Yale do not seem to have manifested any concern in the conception of " Heterogeneous Equilibrium," a neglect that always puzzles us. The Dutch chemist Roozeboom, after the lapse of years, perceived the importance of the Phase Rule. He brought the abstract relations of Gibbs to the test of the laboratory, and subjected to this test they emerged from it triumphantly. Willard Gibbs made deductions in the seclusion of his study at Yale, and Roozeboom actually found that these deductions were true in fact as well as in theory. Wilhelm Ostwald collected and translated the memoirs of Gibbs in 1892,‡ and to-day a whole literature has gathered around them. Carnot and Clausius, Joule and Kelvin are outstanding names, but the name of Willard Gibbs stands alongside theirs.

Riemann worked in the same abstract manner as Gibbs. He constructs a certain mathematical space, and in the process we see that, so far as mathematical activity itself is concerned, there is no reason why this space should be dowered with three dimensions, and with three dimensions only. But we see more than that. We see that the primary notions have nothing peculiarly " spatial " about them. We can deduce from them not only certain characteristics of space, but certain properties of linear algebraic equations, of mixtures of gases—of a large number of things, in fact, that are not spaces. As for what differentiates space from these non-spatial things, our equation tells us nothing. But, in this simple region, we find that all the mathematical relations we derive admit of obvious geometrical interpretations.

* W. Ostwald, *Allgemeine Chemie,* II, Part II, p. 111 ff. ; G. Helm, *Energetik,* p. 141 ; P. Duhem, *Traité de Mécanique Chimique,* I, p. 84 ff.

† In his *Theory of Heat.* Cf. his paper in the *Transactions of the Cambridge Philosophical Society,* 1876. Cf. A. D. Ritchie, *Scientific Method,* p. 80.

‡ *Thermodynamische Studien,* von Willard Gibbs. Cf. W. Ostwald, *Allgemeine Chemie,* II, Part II, p. 114; G. Helm, *Grundzüge der mathematischen Chemie,* and his *Energetik,* passim.

And by defining congruence in a suitable way we can bestow metric relations upon this space; we build up an Euclidean space. Have we now formulated the properties of "real" space? For about two thousand years it was supposed that this particular mathematical development had its precise analogue in the relations of bodies in the external world, that it was a perfectly correct account of the geometrical properties of real space. Even the greater assumption was made that this particular geometrical development was the only one of which man's mind was capable. It is true that this assumption had its critics. From Proclus to Gauss there were always doubts about the entire necessity of Euclidean geometry, even though its descriptive validity might be admitted. But with the erection by Boylai and Lobatchewski of a self-consistent, non-Euclidean geometry, the mind became aware of its own powers. The mind, having won this freedom, proceeded to exercise it, and with the work of Riemann, a mathematician of almost unequalled insight and profundity, a vast new region was opened up to it. This new mental adventure, profound, subtle, and vigorous as it was, seemed for long to be a mere efflorescence of the free intelligence. Like the world of music, it seemed a region exhibiting the free activity of the mind when no longer bound by the arbitrary conditions of experience. That Riemann himself saw a possibility that these researches might throw light on the properties of actual space is evident from a prophetic remark in his famous "Probevorlesung," *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen,* when, after remarking that, if space is a continuous manifold, its metrical relations must be sought outside it, in "darauf wirkenden bindenden Kräften," and concludes by referring this problem to the science of physics. This remark excited no response at the time it was made; most probably, indeed, it appeared completely unintelligible, for a genius as great as Riemann's own was necessary to perceive its full significance. The genius at last appeared in the person of Einstein,* who has succeeded in identifying Riemann's "binding forces" with gravitation.

* Did Johann Soldner in 1801 anticipate Einstein? If so, he is a remarkable instance of a forgotten scientist.

CHAPTER X

LIMITATIONS OF SCIENTISTS

HUMAN life is the old in the new, the old being in a new aspect. History exhibits that union of two opposites, permanence and progress, which is so baffling to the mind. It has a permanent identity and sameness because it exhibits the same species of being and the same eternal truth in all its sections. It also presents a constant variety and change, because it shows this same human nature and this same common variety in new forms. This co-inherence and this co-working of the two factors, of the old and the new, of the conservatism and the progress, is the very essence of history. It is difficult to seize and hold both conceptions at one and the same time, as the constant controversies of scientists show. It is easy—and it is very natural—to separate past discoveries from present, and to make a choice of the one or the other as the key to all new ideas and the foundation of their application to practical life and action. It is simpler to say that the scientist is concerned wholly with the present or that he is wholly concerned with the past. The extremists on both sides have a much easier task than one who occupies the central position between them. For a simple idea is much easier to define and manage than a complex one; but it is neither so fertile nor so completely true.

Turn to Bacon's *New Atlantis,* and there we find its author protesting against the mistaken use of imagination and authority in science, root causes of the limitations of scientists. "There is not," we read in Bacon's grave language, "and there never will be an end or limit to this; one catches at one thing, another at another; each has his favourite fancy; pure and open light there is none; every one philosophies out of the cells of his own imagination, as out of Plato's cave; the higher wits with more acuteness and felicity, the duller, less happily but with equal pertinacity." Are these words one whit less true in 1925 than they were in 1625? Yes, they are not nearly so true, and for that we are grateful. If there, however, be any truth in the preceding nine chapters we have written, notably the ninth one, then it is still lamentably obvious that presuppositions stand in the way of that greater scientific progress we all desire. Aristotle is always in Bacon's mind when he protests against the easy assent to authority and against the willingness of most men to receive, without discussion, symmetrical and agreeable—yet fictitious—theories. These he brands with the name of the *Idols of the Theatre.*

In his *Novum Organum,* Bacon investigates the internal impediments to knowledge, those inherent in the human mind itself. Thence he proceeds to analyse them closely. The mind, so it seems to him, instead of being a perfect mirror to reflect the truth, distorts everything that it reflects by its unevenness. "I do find," we see, "therefore in this enchanted glass four Idols, or false appearances of several and distinct sorts, every sort comprehending many subdivisions: the first sort I call Idols of the Nation or Tribe; the second, Idols of the Palace (or Market-place); the third, Idols of the Cave; and the fourth, Idols of the Theatre."

"Plus ça change, plus c'est la même chose," such is the sad saying that comes into our head. Baconian phraseology we no longer employ, but the idea underlying it is still present with us. Shelley sings splendidly:

> Happiness
> And science dawn though late upon the earth;
> Peace cheers the mind, health renovates the frame;
> Disease and pleasure cease to mingle here,
> Reason and passion cease to combat there,
> Whilst mind unfettered o'er the earth extends
> Its all-subduing energies, and wields
> The sceptre of a vast dominion there.

The serious factor, disturbing the truth of these lines, is that the mind of the scientist is very far from an unfettered condition. In his *Souvenirs d'Enfance et de Jeunesse,* one of the most fascinating of all his books, Rénan tells us how he felt drawn to Tréguier, that sombre old town, "écrasée par sa cathédrale," which gave him his "indestructible pli." "On y nageait en plein rêve, dans un atmosphère aussi mythologique au moins que celle de Bénarès ou de Jagatnata. . . . Je n'étais à l'aise que dans la compagnie des morts, près de ces chevaliers, de ces nobles dames, dormant d'un sommeil calme avec leur levrette à leurs pieds et un grand flambeau de pierre à la main." How many scientists have their Tréguier in the shape of some hypothesis, some Idol of the Cave, giving them an "indestructible pli"? This "indestructible pli" has been the curse of science in the past, and the present has not altogether shaken it off its shoulders. Is it possible, for example, that Mr. Bateson is so preoccupied with the study of Mendelism as to be blind to other laws? Is it possible, to take another example, that De Vries is so pre-occupied with Mutationism as to be blind to other laws? The obsession of a hypothesis turns to the possession of a mind, taking it into complete captivity. Each of us has his Tréguier, and unfortunately we are not always aware of the hold that such a sombre mental dwelling-place has over us.

Sir William Osler once upon a time hinted that a man was too old, mentally speaking, at forty. After that age he seemed to contemplate the lethal chamber for the scientist. There is considerable truth in his contention. For after forty or (say) fifty the scientific mind loses much of its elasticity, becoming too cautious and too conservative and too seldom brilliant and too seldom daring. In history a Lord Bryce and a Sir Adolphus Ward become younger men as their weight of years increases. How many Lord Bryces or Sir Adolphus Wards are there in science? These two historians proved receptive of new opinions to the very last, and this is the desideratum for the man of science who is going to grasp fresh truth so long as he lives. The serious difficulty for him is that so much of his labours is concerned with details. Immersed in these details, he tends to become like Bunyan's man with the muck-rake. He was busy with the filth, and never raised his eyes to gaze at the vision over his head. Similarly the scientist is often so busied with petty facts that he cannot see the wood for the trees. The myopic offender has little to say for himself. If a man is content with the view "one step enough for me," he is lost. He must of course take one step at a time, but he must also be prepared to gaze at the distant scene. Must he labour intensively? Of course he must. Must he labour extensively? Of course he must. The long view and the short view are equally essential, but in combination, not in an abstract—and false—antithesis. It is well to lift our eyes to the sunlit range; but we shall reach it only by taking heed to our footsteps in the shadowy plain. The command to see things *sub specie alternitatis* is no command to close the eyes to things temporal. On the contrary, it is a command to see them steadily and whole in their right perspective. The long view proves indeed to be no separate act of distant vision, but a lamp to our scientific path in a workaday world.

One of Mr. Brooke's good sayings in *Daniel Deronda* is, "I want that sort of thing—not ideas, you know, but a way of putting them." The way of putting them is vital to the man who wants to make a seminal discovery. With Candide "cultivons notre jardin," but at all costs let us occasionally look around all of it. In our plot we may, if we please, say with Marshal MacMahon, "J'y suis et j'y reste." If we are content so to say, we shall in science meet our Sedan. Sir Richard Owen was uncrowned king of one department of the scientific world in England to the year 1858. About then he ceased to grow as he had been growing, with the outcome that where he was he remained, and in so remaining he lost his crown. Similarly Sir Roderick Murchison lost his crown which he had worn with so much honour for so many years. Sir Richard Owen and Sir Roderick Murchison are gone from our midst. Have they left no successors? Are there scientists at this moment who are cast in a mental mould which they are incapable of bursting? Tréguier maintained its fascination over Rénan to the close of his life. Tréguierism, if we may coin a word, is not confined to an extraordinarily graceful French writer. There are scientists who profess the faith of Tréguierism quite unconsciously. For there are departments in their world afflicted with crude heresy and with withered orthodoxy. As the years of life advance very few of the members of these departments escape that conservatism and distrust of new ideas that mark the veteran thinker.

If anyone takes the trouble—and the exquisite pleasure he will reap will be his reward—to read the biography of Henry Sidgwick, he will find a man close to our ideal thinker. Every sentence of his writing illustrates the familiar slowness and cautiousness in method combined with a sincerity of spirit so constant and so intense that the heart of it may almost be heard throbbing as the words are read. Characteristic of Sidgwick, it is also, we like to think, the very quality which made him so typically English a thinker. Our love of truth is not intellectual, but moral; the virtue of the gentleman rather than that of the man of science. The typical Englishman among our men of letters is Samuel Johnson. But Johnson is not the typical English thinker, because he was not primarily a thinker at all. For the thinker all questions were open. For Johnson, however, many questions—the wisdom of the English constitution, for instance, the wickedness of the Whigs—were closed. "Quieta non movere," the policy of not questioning the actual system of things but making it somehow workable, may be the wisest for the majority of men. But it cannot be the method of the thinker or inquirer. And it was not the method of those who are most typical of English thought, especially Joseph Butler, the most typical of all. No one was so like Butler in the nineteenth century as Henry Sidgwick. There is of course more play of the intellect in Sidgwick—a play which sometimes goes so far as real humour—and there is far less of that sorrowful earnestness as of a prophet calling to a perverse generation which is so frequent a note in Butler. But there is the same visible and audible sincerity, the same resolute and persistent will to give its fair weight to every objection and never to say one syllable more than the truth allows. Such a spirit in science would change the tone and temper of the twentieth century, turning it into a brotherhood of men striving for the whole truth and turning it aside from making this brotherhood into the sheerest of rivalry. The common aims and the common interests of all scientists, whatever their nationality, ought to bind them into a unity. The tale of the nineteenth century from the days of Edward Jenner onwards is that it has given them diversity instead of this vaunted unity.

T. H. Huxley was not wont to speak evil of science, and his witness is not accordingly prejudiced against it. Sometimes he is afraid of the specialists and sometimes he is afraid of their jealousies. In memorable words he dwells on the former danger: "We are in the case of Tarpeia, who opened the gates of the Roman citadel to the Sabines and was crushed by the weight of the reward bestowed upon her. It has become impossible for any man to keep pace with the progress of the whole of any important branch of science. It looks as if the scientific, like other revolutions, meant to devour its own children; as if the growth of science tended to overwhelm its votaries; as if the man of science of the future were condemned to diminish into a narrow specialist as time goes on. It appears to me that the only defence against this tendency to the degeneration of scientific workers lies in the organisation and extension of scientific education in such a manner as to secure breadth of culture without superficiality; and, on the other hand, depth and precision of knowledge without narrowness." The dangers he feared in his day are of course enormously greater in ours, and his remedy is one that we cannot apply. The reason is obvious. As science advances, it grows increasingly complex. The words employed in any particular department acquire a specialised meaning. Moreover, fresh words have to be coined in order to express brand-new conceptions. Can the discoveries of Riemann be put into plain English? Can those of Mendel? We feel convinced that if Huxley himself were alive to-day, and were to undertake to do for Mr. Bateson what he did for Darwin, he would miserably fail in the attempt. This does not mean for a single second that Mr. Bateson's writing is so much below the level of Darwin's. It does mean that Mr. Bateson is working with more complex material. In point of fact, he has to face all the accumulations of knowledge since 1859. The plan, then, Huxley suggested is plainly an unmanageable one.

Huxley stood in dread of the rivalries existing among men of science. Obviously this comes out more clearly in the early part of his career than in the later part. For in the early part he was making his way, and therefore felt the weight of opposition on the part of his rivals. In the later part he had made his way, and therefore could bear opposition down. On March 5, 1852, he had finished a piece of work on the morphology of the cephalous mollusca, and here is what he writes:

"I told you I was very busy, and I must tell you what I am about and you will believe me. I have just finished a Memoir for the Royal Society, which has taken me a world of time, thought, and reading, and is, perhaps, the best thing I have done yet. It will not be read till May, and I do not know whether they will print it or not afterwards; that will require care and a little manœuvring on my part. You have no notion of the intrigues that go on in this blessed world of science. Science is, I fear, no purer than any other region of human activity; though it should be. Merit alone is very little good; it must be backed by tact and knowledge of the world to do very much.

"For instance, I know that the paper I have just sent in is very original and of some importance, and I am equally sure that if it is referred to the judgment of my 'particular' friend—that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty.

"You will ask with some wonderment, Why? Because for the last twenty years —— has been regarded as the great authority on these matters, and has had no one to tread on his heels, until at last, I think, he has come to look upon the Natural World as his special preserve, and 'no poachers allowed.' So I must manœuvre a little to get my poor memoir kept out of his hands.

"The necessity for these little stratagems utterly disgusts me. I would so willingly reverence and trust any man of high standing and ability. I am so utterly unable to comprehend this petty greediness. And yet withal you will smile at my perversity. I have a certain pleasure in overcoming these obstacles, and fighting these folks with their own weapons. I do so long to be able to trust men implicitly. I have such a horror of all this literary pettifogging. I could be so content myself, if the necessity of making a position would allow it, to work on anonymously, but —— I see is determined not to let either me or any one else rise if he can help it. Let him beware. On my own subjects I am his master, and am quite ready to fight half a dozen dragons. And although he has a bitter pen, I flatter myself that on occasions I can match him in that department also." * Huxley was only twenty-seven when he wrote such a searching criticism of the section of the scientific world with which he came into contact. The gravity of the last paragraph can be best estimated by the fact that there always have been—and there always will be—young men anxious to carve out their careers. Were such stratagems necessary in 1852? Are they necessary in 1925? In spite of the World War, human nature has not been perceptibly modified during the last three score years and ten.

In August 1876 Lister gave the graduation address when the new graduates were capped. In homely verse Sir Douglas Maclagan wrote:

I'm passed, I'm passed,
And capped at last;
I'm qualified and free now,
On pasteboard neat,
Or brass door-plate,
To write myself M.B. now.

Lister seized the occasion to hold out what he considered to be Religio Medici. "In investigating nature," he warned the graduates, "you will do well to bear in mind that in every question there is the truth, whatever our notions may be. This seems, perhaps, a very simple consideration, yet it is strange how often it seems to be disregarded. I remember at an early period of my own life showing to a man of high reputation as a teacher some matters which I happen to have observed. And I was very much struck and grieved to find that, while all the facts lay equally clear before him, those only which squared with his previous theories seemed to affect his organs of vision. Now this, gentlemen, is a most pernicious, though too prevalent, frame of mind. When I was a little boy I used to imagine that prejudice was a thing peculiar to some individuals. But, alas! I have since learned that we all are under its influence, and that it is only a question of degree. But let us ever contend against it; and remembering that the glorious truth is always present, let us strive patiently and humbly to discover it. And considering the weakness of our nature makes

* L. Huxley, *Life and Letters of T. H. Huxley*, I, p. 97.

it often hard for men to recant an error to which they have once committed themselves, you will see an additional reason against such rash and premature publication." * In 1852 Huxley found Tréguierism in London and in 1876 Lister found it in Edinburgh, and it is of course present to-day throughout the world of science. English law presumes in the world that a man is innocent until found guilty. Tréguierism presumes in the scientific world that a man is found guilty of error until proved innocent. Rudolf Virchow (1821—1902) was not unknown in the arena of controversy. Moved by the harm it had done, he wrote in the preface to his collected writings in 1861: "No doubt science cannot admit of compromises, and can only bring out the complete truth. Hence there must be controversy, and the strife may be, and sometimes must be, sharp. But must it even then be personal? Does it help science to attack the man as well as the statement? On the contrary, has not science the noble privilege of carrying on its controversies without personal quarrels?" Such a privilege ought to be used, but has it?

The attitude of a Joseph Butler or of a Henry Sidgwick is the ideal of the scientist just as much as it is the ideal of the moral philosopher. Creed and practice, even in the domain of science, do not always coincide.

In an ideal world of science all the members are moved by a pure love of truth and there are no such things as envy, hatred, and malice. In the actual world there is much love of truth. Is it pure? Of course it is not, as the preceding chapters amply testify? Are scientists beings who move in a world of thought where such human failings as envy, hatred, and malice are not so much as mentioned? Of course they are not. There is, as Huxley's letter of 1852 reveals, much envy on the part of the investigator towards the young graduate who brings to him a piece of original work which trespasses on his chosen domain. There is, as Lister's address in 1876 reveals, deep-seated prejudice. Now of course in the world of reason two blacks do not make one white. In the world of ordinary life they sometimes do. Did not Jowett darkly hint that logic is neither a science nor

* Sir R. Godlee, *Lord Lister,* p. 389.

an art, but is simply a dodge? Not a few graduates practically—shocking as it is to relate—hold a similar view? Convince such a man that there have been conflicts in science, that there are conflicts in science, and it is not nearly so hard to open his eyes to the circumstance that theology is not the only domain where such a warfare has been waged. Quite frankly, one object in writing this book has been to show the scientist from the annals of the past that his domain has been—and is—infected with precisely the same virus that has been at work in the world of religion. It is for the reader to judge whether we have proved this or not. It is part of our thesis, and by it, for the most part, this book stands or falls.

In the world of science new ideas have not in the past met with that welcome that a priori we should have thought possible. Nor has this lack of friendly greeting been altogether unmixed with professional jealousies. If we take the case of Huxley again, it is simply because his biography is one of the fullest with which we are acquainted, and the biographer writes it with a frankness which is wholly admirable. In 1879 there was a proposal to remove the School of Mines from such a crowded part of London as Jermyn Street to the dignified seclusion of South Kensington. It was a matter that commended itself warmly to Huxley. Naturally the alumni of the School by no means saw eye to eye with him in this matter. They met at a public dinner to which Huxley had been invited. The chairman, stirred by the presence of so many friends of the School, spoke enthusiastically in favour of the present position of the School. The applause was vigorous when, to the surprise of everyone, Huxley stood up, and signified his protest by walking out of the dining-room. Of course he was entitled to his opinion, but so too were the alumni.

In England and Scotland professional jealousies are mitigated by the circumstance that the Crown has the right to appoint to some chairs. Is there any subject that gives rise to so much feeling as the appointment to a Professorship? In different ways we have been connected with the three older Universities of Oxford, Cambridge, and Dublin, and one of the sorriest spectacles we know is to observe occasionally how a School of Science has been seriously hampered because one colleague will not work harmoniously with another. If we may speak of our own profession for a moment—for in it such feeling is not altogether unknown—it is pleasant to note how a newcomer in a diocese is so much more warmly received in England than in Ireland. No doubt part of this difference arises from the fact that the Crown in England, though not in Ireland since 1869, possesses the power of nominating men to all the Bishoprics, all the Deaneries, and not a few Canonries. It is impossible to feel jealousy of the Crown in England. It is possible to feel jealousy of the Board making an appointment in Ireland. Of course in England, as in Ireland, Tréguierism is not wholly unknown.

The examples we have given of the conflict between scientists and science stop with the days of Lister, and the reason of this is obvious. It is not that fresh cases of conflict do not exist: it is simply that such evidence is not available simply because the scientists are still alive. On the ground of taste as well as on the ground of lack of evidence we do not proceed to the present moment. Recent biographies—and biographies are essential for our purpose—reveal the melancholy fact that this conflict persists. In 1921 the English edition of the *Life of Elie Metchnikoff* appeared and in 1923 Sir Ronald Ross published his *Memoirs*. In 1924 Lord Rayleigh's biography of his great father also appeared.

Madame Metchnikoff wrote a revealing study of her husband in which we catch almost as many glimpses of the man as we do of the scientist. A devoted lover of music, he possessed much knowledge of art and had many friends in the art world of Paris. Like so many of the greatest discoverers, he was attracted to the field of his life's work by a delight in its beauty. His æsthetic sense was gratified by the observance and discovery of the phenomena of structure and function. Metchnikoff (1845—1916) was a Russian zoologist who breathed a serene atmosphere which altered the second he changed to pathology. Feeling his way to a startling discovery in pathology, he tells us that while he was Scientific Director of the bacteriological station at Odessa, medical society met with hostility every work which issued from the laboratory. Tentatively he issued his phagocyte theory, by which he proved that natural history could be applied to medicine. This theory he had seen in a flash of prophetic insight. But this was an innovation, retorted some. Metchnikoff pursued his researches, which were to show that recovery and immunity depended on the absorption and digestion of living, virulent microbes by phagocytes. Emmerich in 1887 attacked the new view violently, and even though the discoverer travelled to Munich in order to afford a personal explanation, it was unavailing. There was peace neither at Odessa nor at Munich. Koch lived in Berlin, and he was a discoverer. Surely he would listen to his evidence. Accordingly, to Berlin he hastened in 1887. Koch "received him coldly. For a long time, while examining specimens of the spleen in relapsing fever, he refused to recognise in them an example of phagocytosis. Though he was obliged to bow to the evidence, he yet remained unfavourable to the phagocyte theory, and all his assistants followed his example. Metchnikoff was much surprised and grieved by this hostility towards his ideas, notwithstanding that they were based on well-established facts." *

Madame Metchnikoff is of course the only person really to know the effects of such marked opposition towards her husband's ideas. The outcome of Bacon's *Idola* could not be more marked. "Here," she tells us, "was the realm of secular traditions, deeply rooted, and of theories generally admitted but resting on no biological basis. Attacks and objections against his theories came following each other with a rush, only to be compared with the racing clouds of a stormy sky or the hurrying waves of a tempestuous sea. An epic struggle began for Metchnikoff which was to last for twenty-five years, until the moment when the phagocyte theory, his child now grown up, was to emerge victoriously. To each attack, to each objection, he answered with fresh experiments, fresh observations annihilating objections; his theory was assuming wider and wider scope, becoming more solid and convincing. . . . But only his intimates knew how much the struggle cost him in vital force, what sleepless nights, due to continuous cerebral tension, and to the

* O. Metchnikoff, *Life of Elie Metchnikoff*, p. 133.

effort to conceive some new and irrefragable experiment, what alternations of hope and depression. . . ." *

Now if Metchnikoff had been the very first to perform such experiments, if there had been no Pasteur, no Lister, we could better understand such determined opposition. But he was by no means a pathological Columbus. The objection of the average man to a proposal is, Why, I never heard the like of that before! To him such an objection is fatal. There were even scientists who replied—of course in proper terminological exactitude—Why, I never heard the like of that before. The tragedy is that in the eighties a genius like Metchnikoff was forced to spend twenty-five years of his precious time in meeting their attacks. The years that might have been devoted to the perfecting of his toil had to be given to meeting attack after attack. By experiments on the rouget of pigs he met the objections of Emmerich. By experiments on the anthrax of pigeons he met the objections of Baumgarten and his pupils. By experiments on the anthrax of rats he met the objections of Behring, who affirmed that immunity was purchased by the bacteriological power of the serum. His attacks were serious, for he discovered antitoxins, and this seemed to favour the chemical or humoral theory of immunity. According to the latter, microbes and their poisons were rendered harmless by the chemical properties of the blood serum, properties similar to those of disinfecting substances. A series of fresh researches was imperative. What part was played by phagocytes? What part was played by antitoxins? The investigator was at last enabled to draw the required conclusion. He ascertained the nature of the narrow link between immunity and the function of the phagocytes which probably elaborate the antitoxins as a product of their digestion of vaccinal toxins. His *Leçons sur la pathologie,* published in 1892, contained conclusions, with the evidence for them, that ought to have silenced all opposition. Of course it did nothing of the kind.

Madame Metchnikoff records that "the persistent and bitter opposition of physicians to the phagocyte theory made a great impression on Metchnikoff, and, while stimulating his energy in defence of his ideas, it maintained

* O. Metchnikoff, *Life of Elie Metchnikoff,* p. 146.

him in a state of nervous excitement and even depressed him.

"He asked himself why this obstinate opposition to a doctrine based on well-established facts, easily tested and observed throughout the whole animal kingdom? To him, a naturalist, it seemed clear and simple and all the more admissible that it was confirmed by the generality of its application to all living beings.

"But, he thought, perhaps the real cause of the attitude of the contradictors lies in the very fact that medical science only concerns itself with the pathological phenomena of higher animals, leaving their evolution out of account, as well as their starting-point in lower animals—whilst it is the very simplicity of the latter which allows us to penetrate to the origin of the phenomena.

"Perhaps a general plan of the whole, in the shape of a comparative study, embracing the whole animal scale, would throw light over the generality of phagocytic phenomena and would make their continuity understood through normal and pathological biology. He determined to make this effort." *

His *Leçons sur la pathologie* covered the whole ground, and, so far as the objectors were concerned, covered the whole ground in vain. The attacks made upon Edward Jenner at the beginning of the nineteenth century were made upon Elie Metchnikoff at the end of the nineteenth century. By a curious coincidence the problems at which Jenner and Metchnikoff were working were kindred in nature, for Jenner was working at the beginnings of Immunology. Pasteur and Lister, Koch and Metchnikoff—and indeed the whole modern therapeutical movement—trace their descent logically from what a country doctor in Gloucestershire initiated. Immunology has made progress, distinct progress. In spite of this, however, in 1925 the Medical Research Council assures us that not one of the causal organisms of the common communicable diseases has been discovered in any of our University laboratories. Yet when Elie Metchnikoff presented results, the reception of them was so cold that it daunted even his warm heart. With his keen imagination, keen vitality, keen persistence, he saw far down the corridors of time, new vistas alluring him ever onward.

* O. Metchnikoff, *Life of Elie Metchnikoff*, p. 150.

His gaze ever was fixed on the far-off hills and on the giant peaks of the far-distant and unknown country beyond. Some of that unknown country he was happy enough to win, but some was hidden by mists raised by men, and

> This high man, with great things to pursue,
> Dies ere he knows it.

The history of scientific discovery attests the need there is for a clearing house for men who are engaged in research. For one's labours may be either superfluous or assistance for them may be provided. The labours may be superfluous, for one may ascertain through such a clearing house that one's idea has been exploited to its limits. Assistance for them may be provided when this is not so, for one may meet another worker engaged in a kindred task. Then there are all the advantages gained through discussion with some one who is qualified to afford light and leading. The clearing house is not always possible, for the simple reason that there may not be another man in a position to give help of any kind. We must also remember that the discovery may be a welcome one or an unwelcome one. It may be a welcome one for which many are clamouring, for it will co-ordinate scattered tiny generalisations into the whole to which they belong. It may be an unwelcome one for which nobody is clamouring, for it may be the very first of a set of generalisations which will not be sufficiently ripe for the large generalisation for another generation. An example will explain what we mean. Dr. Banting discovered insulin, and everyone wanted it. Dr. Edridge-Green discovered a new theory of colour vision, and nobody wanted it. Dr. Banting's discovery of insulin was a sort of keyword that completed a cross-word puzzle. It completed, co-ordinated, and explained the researches of nearly a generation of laboratory workers. Dr. Edridge-Green's discovery set out with the destruction of the current colour-theory, and the outcome was that he saw his theory neglected for twenty years. The supporters of the current theory were horrified at the unorthodoxy of the views of the discoverer. We never heard the like of that before! The United States and Germany paid some heed to his view, but the body most difficult to convince was our own Royal Society.

Discoveries appear in learned papers, but there are no less than 24,000 of them throughout the world, and more than half of them are devoted to the biological sciences. Very few libraries in the world contain more than a fraction of these publications. In our country an effort is being made to group the libraries of provincial universities, and to pool borrowing and consulting powers. Geographical distances in America put such co-operation out of the question. The proposal the Americans make is to start a journal of abstracts of the researches that are being actually pursued throughout the world. The National Research Council of the United States and many of the leading American scientific institutions have given a general support to the proposal. The Rockefeller Foundation is reported to have promised an annual contribution of fifty thousand dollars a year for ten years for the editorial expenses. On a small scale this plan has been tried. Our Physiological Society, for instance, issues a valuable series of abstracts of papers dealing with physiological research. The Imperial Bureau of Entomology circulates abstracts and bibliographies dealing exhaustively with the economic side of insect life. Since 1864 the annual volumes of the *Zoological Record* have provided classified lists of zoological memoirs, with detailed indexes to their contents. The proposal, then, is one that has been tried on a national scale, and ought to be tried on a cosmopolitan one. When a scientific society has its annual meeting, why, in addition, should there not be a list with its secretaries, specifying what pieces of original work members are pursuing? We feel convinced that such a list would have saved, for example, some of the heart-sickness from which a man like Metchnikoff suffered so bitterly. If the clearing house method is not always feasible, probably the list is not always feasible. Still, for the sake of a Dr. Edridge-Green or a Metchnikoff we should risk it.

Less than a year ago Sir Ronald Ross * published his delightful "Memoirs," and they reveal his many-sided nature. Mathematicians deplore him as a worker wasted in alien researches. Poets have recognised his gifts by electing him President of the Poetry Society. His musical talent is

* He was born in 1857, and happily he is still with us.

evident when we remember that on the marriage of his daughter it was one of his compositions that was selected for the wedding march. As child, youth, and man he felt obsessed with the idea of undertaking research in mathematics, of writing great verse, and of composing transcendental music. Entering the Indian Medical Service he became immersed in mathematics, poetry, music—and polo. "Homo sum; humani nihil a me alienum puto"—this could evidently have been taken for his motto. The interest in the malaria problem was bound to come, and it did come in 1880.

Malarial fevers proved the main obstacle in the way of the conquest of the Papacy by the Holy Roman Empire. Had a Frederick I or a Frederick II possessed a remedy for these fevers, how the course of the world's history would have been deflected! They have swept away millions until in 1640 the Countess d'El Chinchon, wife of the Governor of Peru, introduced Chinchona Bark into Europe. In 1700 this remedy was in widespread use. A century later men ascertained that a parasite inhabiting the body of a water-fowl was also found in the bodies of fish on which that type of fowl habitually feeds. May a parasite own two sets of hosts in the animal kingdom? This possibility became a probability when Sir Patrick Manson discovered that the small worm producing that rare disease, elephantiasis, is carried by a mosquito in whose body it has passed a portion of its existence. In 1878 Laveran, a French army surgeon, working at Bône in Algeria with a microscope as inadequate as any instrument in Pasteur's early laboratory, discovered and described the malarial parasite as it appears in the human blood.* He watched the small, wriggling bodies, each ensconced in its blood corpuscle. Golgi, who continued his work, noted fully the budding process in the blood—the so-called asexual cycle of the malaria parasite.† Ross thought in terms of malaria, and wrote in terms of it:

In this, O Nature, yield, I pray, to me,
  I pace and pace, and think and think, and take
The fever'd hands, and note down all I see,
  That some dim distant light may haply break.

. . . . .

* Sir R. Ross, *Memoirs*, pp. 40, 92, 101, 119-22, 125-6, 128-9, 194.
† *Ibid.*, pp. 121, 134, 194, 339, 396-7, 480.

The painful faces ask, Can we not cure?
  We answer, No, not yet; we seek the laws.
O God, reveal thro' all this thing obscure
  The unseen, small, but million-murdering cause.

On leave in London in March 1894, Ross met Dr. (as he then was) Patrick Manson.* Curiously enough, Ross had at that time formed the conclusion that Laveran's result was inaccurate, for he had been unable to repeat it.† Manson convinced him that in forming this conclusion he was wrong. On a November afternoon in Oxford Street, Manson declared, "Do you know, I have formed the theory that mosquitoes carry malaria just as they carry filariæ?"

There had been precursors of Darwin from time immemorial, and there had been precursors of Ross, but it was reserved for him, just as it was reserved for Darwin, to furnish the complete proofs of the accuracy of the view propounded by Manson. Any able man can enunciate views. It takes a Darwin or a Ross to demonstrate their truth, and to spend, if necessary, a lifetime of hard work in the task of demonstration. Thanks to Ross the Panama Canal has been built and the tropics have become habitable by the white race. Millions of lives have been saved, and their saviour is Sir Ronald Ross.

The story of the discovery made by Ross on the afternoon of August 20, 1885, is as pathetic as any in the career of Elie Metchnikoff. Unsupported by his profession, subject to constant interruption, raised to the heavens as he seemed to be on the brink of success, dashed down to the depths as he seemed to be on the brink of failure, yet he persisted grimly in his self-chosen labours. As he notes, "I was up against a very difficult problem indeed—an equation containing two unknown quantities." There is romance in the way the essential discovery was at last made. Mosquito after mosquito had been examined that day with negative results, and about one o'clock Ross started the dissection of an anopheles mosquito. Nothing was found on examination, and only the stomach tissue remained to be looked through. His eyesight felt already strained, and it seemed hardly worth while to continue the search.

* Sir R. Ross, *Memoirs*, pp. 122, 124, 127-9, 131, 134-5, 155-6, 187-98, 207, 233-4, 238, 245, 287-8, 305-8, 318, 331-4.

† *Ibid.*, p. 129.

Methodically, however, he persisted, and, to his delight, discovered the malaria parasite living and growing in the mosquito. If admirers of Jenner deemed May 14, 1796, a red-letter day in the annals of mankind, we, as admirers of Ross, deem August 20, 1885, another red-letter day.* Ross records his awe:

> This day relenting God
>   Hath placed within my hand
> A wondrous thing; and God
>   Be praised. At His command
>
> Seeking his secret deeds
>   With tears and toiling breath,
> I find thy cunning seeds,
>   O million-murdering Death!
>
> I know this little thing
>   A myriad men will save.
> O Death, where is thy sting?
>   Thy victory, O grave?

During all the time Ross had been pursuing his researches we find that the authorities of the Indian Medical Service, the India Office, and the Colonial Office hampered him negatively and positively.† In all these he found Tréguierism absolutely rampant. Negatively, these bodies offered him no encouragement during an arduous piece of work of the last possible importance to all three departments. Positively, they actually—and it seems deliberately—placed many obstacles in the path of one who found sufficient natural obstacles in tracking the devious ways of the anopheles mosquito without requiring any artificial obstacles.‡ In disgust in 1899 he resigned his commission in the Indian Medical Service. In fact, there was more assistance tendered to Jenner at the end of the eighteenth century than was tendered to Ross at the end of the nineteenth. His *Memoirs* describe with justifiable bitterness and scorn the scandalous attempts made to pirate his work by such Italians as Bastianelli,§ Bignami,|| and Grassi,¶ and others, and the

* Sir R. Ross, *Memoirs*, pp. 223-6.

† *Ibid.*, pp. 201-2, 240-3, 315, 317, 355-6, and indeed passim. Cf. also pp. 367, 391, 412-13.

‡ *Ibid.*, pp. 93, 211-12, etc.

§ *Ibid.*, pp. 335, 348-52, 392, 398, 400-10, 480-1.

|| *Ibid.*, pp. 121, 127, 194-6, 207, 288, 335-54, 366, 392, 398, 400-10, 480-1, 485.

¶ *Ibid.*, pp. 122, 194, 263, 287-8, 335-52, 366, 382, 398-412, 414, 440.

scientific scepticism he met with in various quarters. In connection with these grievous injustices he was asked on one occasion which he preferred, the thieves who stole his pearls or the swine that trod them in the mire. His retort was that at least the thieves knew their value, for it required some audacity to be a pirate, whereas it required none to be a pseudo-scientific sceptic.*

Lord Kelvin was a genius who might have found the clearing-house system or the list useful. For it is perfectly plain, if we take one section of his vast work, that Macquorn Rankine and Clausius, Joule and Helmholtz—not to mention others—were all working pretty simultaneously at aspects of the problem of the conservation of energy. Instead of a clearing house or a list, Lord Kelvin continued all his life to consult Sir Gabriel Stokes, probably the man whose mind was in closest sympathy with his own. Be that as it may, the moment a matter seized hold of him, his very first thought was what Stokes would think of it. "I must consult Stokes," was a remark often to be heard on his lips. Examples of genuine scientific co-operation are few. There is a couple of such well-known cases as Liebig and Wöhler, of Liveing and Sir J. Dewar. Examples of genuine scientific consultation are many, and among the most conspicuous of these is that of Kelvin and Stokes. When the latter died, Kelvin attended the funeral at Cambridge. When he was leaving the grave, Kelvin remarked to a friend, "Now that Stokes is gone, I shall never return to Cambridge."

The gigantic genius of Lord Kelvin was generally recognised in his own day. That singularly competent authority, Sir Joseph Larmor, attended the funeral of this genius in Westminster Abbey. As he walked away from the Abbey in company with Sir Archibald Geikie to the rooms of the Royal Society, Sir Joseph made a remark which graphically brings before us the greatness of Lord Kelvin. "Conceive," he held, "a perfectly level line drawn from the summit of Newton's genius across all the intervening generations; probably the only man who has reached it in these two centuries has been Kelvin." †

* Sir R. Ross, *Memoirs*, pp. 430, 468, 475.

†Sir A. Geikie, *A Long Life's Work*, p. 350. Sir Joseph Larmor tells me that this remark is apocryphal.

In pure mathematics Lord Kelvin was excelled by others, but as Helmholtz said of him, the power of translating real facts into mathematical equations, and vice versa, is far more rare than that of finding a solution of a given mathematical problem, and it was in this direction that he displayed his striking qualities. Another aspect of the same quality was the extraordinary combination he showed of the theoretical with the practical. He was not an Ulsterman for nothing. He once remarked that there could be no greater mistake than to look superciliously on the practical applications which were the life and soul of science. So it was that he did not think it beneath him to turn from the most abstruse inquiries into the constitution of matter or the doctrine of energy to the invention of a common water tap.

The most fruitful period of his life was the first ten years he was Professor at Glasgow University. His fertility from 1845 to 1856 remains unparalleled. Then, *inter alia*, he conducted his investigations in thermodynamics with its reconciliation of the ideas of Carnot with the experimental results of Joule. Then he enunciated the principle that in the material world there is a universal tendency to the dissipation of mechanical energy, and that any restoration of mechanical energy, without more than an equivalent of dissipation, is impossible in inanimate material processes and is probably never effected by means of organised matter, whether animal or vegetable. Of this principle Sir Joseph Larmor has remarked that the advance brought about by its mere enunciation is to be measured by its very inevitableness to our present modes of thought, and that it is more difficult now to recognise the limitations that must have belonged to the time when its formulation gave rise to such surprise and wonder. Lord Kelvin proceeded to the deduction that within a finite period of time past the earth must have been unfit for the habitation of man as at present constituted, and within a finite period of time to come must again become so, unless operations have been or are to be performed that are impossible under the laws to which the known operations going on at present in the material world are subject.

On what subject has there been more nonsense written and spoken than on the end of the world? From classical times, from the days of the millenarians to those of the World War—is it not always appearing under one guise or another? Geologists intervened in the discussion, and some talked sense and some nonsense. Even the biologists—why it does not quite transpire—entered into it. Huxley taught men political philosophy and he taught them political economy. In 1869 he spoke out. "The critical examination," he held, "of the grounds upon which the grave charge of opposition to the principles of Natural Philosophy has been brought against us rather shows that we have exercised a wise discrimination in declining to meddle with our foundations at the bidding of the first passer-by who fancies our house is not so well built as it might be." * Is this a scarcely veiled hint that outsiders should not meddle in a matter that was no concern of theirs? Is it, in fact, a form of Tréguierism? So Lord Kelvin took it, for a few weeks later at Glasgow he pertinently inquired: "I cannot pass from Professor Huxley's last sentence without asking, 'Who are the occupants of "our house," and who is the "passer-by"?' Is not geology a branch of physical science? Are investigations, experimental and mathematical, of underground temperature not to be regarded as an integral part of geology? Are suggestions from astronomy and thermodynamics, when adverse to a tendency in geological speculation recently become extensively popular through the brilliance and the eloquence of its chief promoters, to be treated by geologists as an invitation to meddle with their foundations which a 'wise discrimination' declines?" †

An eminent scientist, Silvanus P. Thompson, wrote an able biography of Lord Kelvin which is curiously supplemented by the biography Lord Rayleigh published last year of his father. Thompson reveals the limitlessness of Lord Kelvin's intellect, though occasionally he hints at the limitations, a matter on which Lord Rayleigh has just told us a great deal. Kelvin neither altogether accepted Clerk-Maxwell's electro-magnetic theory of light nor the notion of "displacement currents" on which that theory is based.‡

* *Quarterly Journal of the Geological Survey of London,* 1869, XXV, part I, pp. xxxviii to iii.

† S. P. Thompson, *Life of Lord Kelvin,* I, p. 549.

‡ *Ibid.,* II, p. 879.

Of course we all know the triumphant investigations that have been conducted by such pupils and disciples of Clerk-Maxwell as Lord Rayleigh and Sir J. J. Thomson, Sir Oliver Lodge and Sir Richard T. Glazebrook, G. F. Fitz-Gerald and John Hopkinson, J. H. Poynting and Oliver Heaviside. In 1896 Lord Kelvin wrote to Colonel (as he then was) Baden-Powell, "I have not the smallest molecule of faith in aerial navigation other than ballooning, or of the expectation of good results from any of the trials we hear of." * Of metaphysics he entertained as hearty a hatred as Boltzmann himself.† In 1871 he regarded war —here he was in keeping with true Victorian spirit—as a relic of barbarism probably destined to become as obsolete as duelling.‡

Like Cayley, he would never allow the use in physics of the method of quaternions.§ Nor was this a matter of old age. For in 1845, when only in his twenty-first year, he met Sir William Hamilton and point-blank refused to entertain this ingenious method of symbolic analysis. His collaborator, P. G. Tait, was in this respect more open-minded. Lord Kelvin, despite the arguments of Tait, refused to employ quaternion notation or quaternion methods. To his dying day he would have none of these things, and in process of time grew to hate the name of vector. He waged a thirty-eight years' warfare with Tait on this matter, refusing to admit quaternions into the *Natural Philosophy* they both wrote. "When confronted with a new factor in discovery, Thomson's attitude of mind," points out his biographer,|| "varied according to the circumstances of the case. Thus when Kerr in 1876 announced his discovery of electro-optic stress, Thomson was instantly and almost explosively excited; he had predicted this very effect thirty years before, and had written of it to Faraday, who had himself looked for it in vain at a still earlier date. When Röntgen's discovery of the X-rays was announced at the end of 1895, Thomson was entirely sceptical, and regarded the announcement as a hoax. On the other hand, when

* S. P. Thompson, *Life of Lord Kelvin,* II, p. 1122.
† *Ibid.,* II, pp. 1122, 1124.
‡ *Ibid.,* II, p. 1128.
§ *Ibid.,* I, pp. 450, 452; II. pp. 1137-8.
|| *Ibid.,* II, p. 1125.

Crookes first showed him the radiometer, one evening in 1874, he sat down watching it in perfect silence for nearly an hour, gazing at it, shading the light from it at intervals with his hand, or moving it towards the lamp or from it, and thinking—thinking. Not even in 1906 was he satisfied that the true theory of the radiometer had ever been given."

Tréguierism afflicted Lord Kelvin to a marked extent, as Lord Rayleigh reveals in a number of instances. He was anxious to secure recognition for the grossly neglected work of Willard Gibbs of Yale. His Phase Rule, with all its implications, concerned many of the ways in which the different sciences intersect, e.g. biology and chemistry. The chemical members of the Council of the Royal Society deemed it "not chemistry." * Rayleigh insisted that the title of Gibbs's great paper, *The Equilibrium of Heterogeneous Substances,* would serve as a first-rate definition of that subject. On September 13, 1891, Lord Kelvin objected: "I feel very doubtful as to the merits of Willard Gibbs's applications of the *Second Law of Thermodynamics* referred to by J. J. Thomson. Do you attribute merit to them?" † On February 9, 1892, he wrote, "I find no light or leading for either chemistry or thermodynamics in Willard Gibbs." † To-day Willard Gibbs, his mark, is written over chemical lectures and even chemical text-books.

In 1895 the labours of van't Hoff and Arrhenius had brought before the scientific world their theory of electrolytic dissociation which, in spite of opposition, has now won general assent. Lord Kelvin had learnt something of it from friends, and was fierce in his denunciation of it. Lord Rayleigh lent him a book in which the views of van't Hoff and Arrhenius were duly set forth. Lord Kelvin proved as dour as any Scots to convince. Meeting with an inconclusive argument, he felt exultant. "I remember meeting Kelvin," writes the present Lord Rayleigh,‡ "in the conservatory as he was leaving the book-room, and I was going to it. He waved the book in triumph as he crucified the fallacy. 'He is *not* equal to *pdv,*' he said triumphantly, repeating the words several times with emphatic relish.

* Lord Rayleigh, *Life of Lord Rayleigh,* p. 172.
† *Ibid.,* p. 172.
‡ *Ibid.,* p. 238.

'It is Meyer's old mistake of 1842, and here we have it again in 1893.'

"However, his indignation abated somewhat as he read further. 'He will think before long that he discovered it himself,' Rayleigh [the father] remarked, after his visit was over."

W. Wien developed his "displacement law" of black-body radiation in a fashion that has won approval. Lord Rayleigh was convinced, but Lord Kelvin was not. When he met with the conception of work done by a piston moving against radiation pressure, he stigmatised it as "Thermodynamics gone mad!" * At first he proved every whit as antagonistic to the new views of gaseous conduction developed by Sir J. J. Thomson, Sir E. Rutherford, and Sir A. Schuster. Lord Rayleigh happened to mention something about the carriers of the electric charge, a term employed by one of these writers. "Why," Lord Kelvin burst out, "do you call them carriers?" The new name signified the new idea and it was abhorrent to him.*

"Kelvin," such is the conclusion of Lord Rayleigh, "was equally eager in discussion of theoretical views, but in his mature years at any rate he was by no means laudatory about the theories of other workers as he was about their experimental results. Indeed to those who did not realise the tremendous record of achievement that stood to his name, his way of discussing new views might well have seemed not a little perverse. As Rayleigh often said, 'He is a most interesting personality, not only for his powers, but also for his limitations.' † . . . Kelvin's scientific discussions or arguments with my father were often on abstruse questions, and I cannot now attempt to reproduce any of them from recollection. But it was as good as a play to hear them. 'I cannot see the shadow of an argument in that,' Kelvin would say. 'Well,' Rayleigh would reply, 'I regard it as rigorously proved: and I think you will be convinced if you will only read it as I have set it out here in half a page of print.' But it was not easy to get him to do this. He would take it up, but the first line or two

* S. P. Thompson, *Life of Lord Kelvin*, p. 238.
† Lord Rayleigh, *Life of Lord Rayleigh*, p. 238.

would send him off on some train of thought of his own, and his eye would wander from the printed page." *

The idola of Bacon and the Tréguierism of Rénan are plainly written on the course of their thought. Rénan realised it, but did Kelvin? Rayleigh realised it in the case of Kelvin, but apparently Kelvin did not recognise it in his own case. The limitations of Kelvin are only tolerably plain in the biography Silvanus P. Thompson wrote. It was reserved for Lord Rayleigh to dot the i's and stroke the t's of Silvanus P. Thompson. And we cannot help shrewdly suspecting that in not a few other cases, did we but possess another writer to dot the i's and stroke the t's of the biographer, we should have before us the limitations of the man depicted in the *Life and Letters of* ——, be the scientist whom he may.

Now the causes of the idola of Bacon and the Tréguierism of Rénan actively at work in the mind of Lord Kelvin are by no means due to his rigid conceptions of the many meanings—how elusive some of them are!—given to that phrase, the reign of law. He well knew—no one better—in what a wide sense one must use that other phrase—how elusive some of its meanings are!—natural law. To listen to some scientists a law of nature is as rigid as the command of General Martinet of the French Army used to be. "Go," said the centurion to his soldier servant—and he went. "Come"—and he came. Such used to be the idea of a natural law. Lord Kelvin knew infinitely better than all this. In the controversy with Huxley on the right of the physicist to criticise the conclusions of the geologist, he takes occasion to remark, "I have not presented definite results. I have amply indicated how 'loose' my data are; and I have taken care to make my results looser." † There is a haze around all seminal thought, and there is a haze around all scientific law. C. J. Vaughan (1816—1897), the Master of the Temple, was once asked the question, What is the main difference between the conceptions of Westcott and Lightfoot? He pondered, and then gave his answer. The difference he found to lie in the circumstance that when Lightfoot got hold of an idea he wanted to make

* Lord Rayleigh, *Life of Lord Rayleigh*, p. 243.
† S. P. Thompson, *Life of Lord Kelvin*, p. 549.

it as definite as ever he could, whereas when Westcott got hold of an idea he wanted to make it as indefinite as ever he could. Lord Kelvin amply realised the wisdom lying behind this remark of Vaughan's. Illuminating conceptions cannot be tied up into neat parcels carefully corded. What at bottom is a law of science but such a conception generalised as skilfully as its discoverer can make it? * An American scientist to-day of the standing of Mr. H. F. Osborn can seriously ask the question, Are the biological laws of life, like the ultimate laws of physics, beyond analysis? When we have traced all vital functions to the primordial living ovum, and " Nature's self untwisted lies into its first consistencies," the untwisted knot is as hard to be explained as ever. Is it as Du Bois Reymond held, *Ignoramus et Ignorabimus*? Still, as Poincaré remarked, thought is the lightning flash between two infinities of blackness. But it is the lightning which matters.

A scientific theory exists to assert a correspondence between the laws of our mind and the happenings of the external world. Man can recognise no other order except the order to which he himself is obedient; a universe which did not obey this order would be an irrational universe and for ever inaccessible to the methods of science. The extraordinary success of scientific explanations in accounting for observed phenomena sometimes seems very surprising; that mathematics, in particular, should be applicable to material happenings seems little short of the incredible. For mathematics is the fruit of a free activity of the mind; the mind is here conditioned only by its own laws. The empirical origins of mathematics were mere points of departure; henceforth the mind proceeds under its own momentum, with no further reference to experience; the exciting cause is really as insignificant as were the notes of the yellowhammer which gave birth to Beethoven's C minor Symphony.

Thinkers like the great German physicist Boltzmann feel the difficulty in bringing the apparently dissociated activity of the department of mathematics into contact with the happenings of the external world, and in his absorbing piece of work on *Space—Time—Matter,* H. Weyl unquestion-

* Cf. the remarkable paper of Dr. F. R. Tennant on "The Reign of Law," *The Modern Churchman,* September 1924, pp. 305-22.

ably experiences the same trouble. Boltzmann exhibits distrust of many of the workings of the mind. Science, in his opinion, though very often abstract, possessed a certain validity, since it issues in the prediction of events which are accessible to sense perception—that is all. A profound thinker like Weyl forces us to adopt the conclusion that the only thing that is behind everything is mind. It is a conclusion as old as Berkeley and as new as Weyl. Does the mind create space? Does the mind create time? To both questions Weyl returns an unhesitating affirmative. Does the mind create matter? Here his conclusion is not so unhesitating, but he leans to the affirmative view. At any rate, mind has put space and time within the framework of matter. The old jest against Berkeley ran, What is matter? Never mind. What is mind? Never matter. Nowadays the whole emphasis is shifting to the dominant position increasingly being held by mind. We have moved so far from the materialistic attitude put forward by John Tyndall in his Belfast address that this once famous counterblast is to-day almost wholly unintelligible.

Clerk-Maxwell in his day was implicitly feeling after views like those of Weyl and Boltzmann, and what he used to feel implicitly scientists of the rank of Einstein, Eddington, and Weyl are feeling explicitly. Professor Eddington suggests that the fundamental entities towards which the theory of relativity leads us may be "the very stuff of our consciousness." Weyl insists towards the close of his *magnum opus, Raum—Zeit—Materie* that accordingly "it must be emphatically stated that the present state of physics lends no support whatever to the belief that there is a casualty of physical nature which is founded on rigorously exact laws."

The nineteenth century lived on the idea of law, the sense of continuity, the theory of evolution. And suddenly with the discovery of radium combined with the novel doctrines of Clerk-Maxwell and Lorentz, Monsieur and Madame Curie, Poincaré and Minkowski, Niels Bohr and Einstein, the very principles and foundations of our scientific world crash about our ears. Are there any principles? Does the earth move, after all? Is there any ether? What do we mean exactly by the conservation of energy? Are all mechanical forces merely phases of electro-magnetism? Do laws evolve and change like living things? Is it a case where there is as well as a living chess-player also living chess-pieces? Do laws advance disconcertingly by leaps and bounds and brusque mutations? Is their simplicity a mask which we set on the complex anarchy of nature? Is science a mere convention, a set in fact of cunningly devised fables? Are the laws of science just the rules of the game? Is there anything of which we can be sure that it will be true in another thousand years?

Questions like these can be found in all the fructifying essays of Henri Poincaré, the genius of France but yesterday, and to-day he is with Newton and Kelvin, with Laplace and Lagrange. He is not read among us to anything like the extent his delightful writings entitle him. His was an intelligence as universal as Leonardo da Vinci's. He began with mathematics, and only the mathematician can appreciate his discoveries in mathematical analysis and in differential equations. He is the creator of the *fonctions fuschiennes* which gave a fresh impetus to the non-Euclidean conceptions of Boylai, Lobatchweski, and Riemann. Poincaré was an inventor in geometry, he was an inventor in physics, he was a discoverer in astronomy; and it is not amazing to us to find that he occupied, one after another, the chairs of these sciences at the Sorbonne. One day the Dean exclaimed, "Unfortunately the Faculty possesses no Chair of Scientific Philosophy, or we could ask Poincaré to fill it!"

What this universal scientific genius discovered, others popularised, for he had—and deserved to have—many pupils. Seldom was a master more worthy of the homage his students unstintedly rendered to him. In astronomy he could write: "Les méthodes de Lagrange et de Laplace ne sont plus valables que pour quelques siècles et non, comme on le croyait, pour des milliers et des milliers d'années; les fondements sur lesquels s'appuient les astronomes pour faire leurs merveilleuses prédictions sont, en réalité, ruineux." Is there a page of his many volumes—we could wish that there were many more volumes of his essays—in which there does not appear how "ruineux" is the conception of laws eternally true? Is there a page where we do not hear of the contingency, the transitoriness, the approximateness, the imperfection, without exception, of the laws of science? What, then, is there? Is there any such thing as absolute truth? Not at all. There are scientific hypotheses which sensible men employ because they must, despite the Newtonian idea of Non Fingo Hypotheses, employ them. Is it very surprising that Boutroux and Bergson hived his honey in their nests? Is it startling to meet Boutroux teaching us *La Contingence des Lois de la Nature?* Is it surprising to meet Bergson teaching us how to grasp *L'Evolution Créatice*?

In 1914 Professor A. Aliotta gave us his book on the idealistic reaction against science. Throughout his pages the influence of Poincaré, as well as that of Boutroux and Bergson, is plainly traceable. So far indeed has this reaction proceeded that instead of the reign of law, we hear much more of the total absence of law. Activism and Agnosticism (in the scientific sense, a position enough to make the body of Huxley turn in its grave), Bergsonianism and Voluntaryism, Anglo-American Pragmatism and the Primacy of Practical Reason—such were some of the forms of thought to which the last couple of decades have given birth. The swing of the pendulum against reason, in all its forms, has come, and come with a vengeance.

Bubble after bubble like Bergsonianism and all the rest of them arose in France and other countries, and the subtle brain of Henri Poincaré enjoyed himself in pricking them. Did Boutroux reproduce his teaching in one form? Did Bergson reproduce it in another? Did Brunetière proclaim the bankruptcy of science? Did Le Roy announce that it was a means of manipulating matter? Did Le Roy say, "Le savant crée le fait"? Then it was for Poincaré to show how far the answers to such questions were or were not warranted. "The experimenter," he maintained, "does not create the fact; he only creates the language in which he describes the fact; Euclidean and non-Euclidean geometry, for instance, speak different languages but express the same truth, as you may translate the same fact into French or German. Our formulas are fragmentary, our hypotheses approximative and sometimes contradictory. Yet a mind, of our own sort and quality, but vaster, could merge their variety into a coherent hypothesis. And, from the point of view of such a mind, the laws of science would show no imperfection, only men of science would appear at different moments more or less well informed."

The volumes of the extremely lucid French which Henri Poincaré has written are the best place to note all that he can tell us in his matchless style of how contingent are all the laws of Nature. This matter, however, is so important that we spend somewhat more of our space in quoting from a clear book that Mr. Bertrand Russell, who has given so much thought to scientific method—not to speak of other matters—has given us. In his *A B C of Atoms,* published less than a couple of years ago, we have his views on the transitoriness of the laws of science:

"It is necessary, however, to utter a word of warning, in case readers should accept as a dogmatic ultimate truth the atomic structure of the world which we have been describing, and which at present seems probable. It should not be forgotten that there is another order of ideas, temporarily out of fashion, which may at any moment come back into favour if it is found to afford the best explanation of the phenomena. The charge on the electron, the equal and opposite charge on a hydrogen atom, the mass of an electron, the mass of a hydrogen nucleus, and Planck's quantum,* all appear to modern physics as absolute constants, which are just brute facts for which no reason can be imagined. The aether, which used to play a great part in physics, has sunk into the background, and has become as shadowy as Mrs. Harris. It may be found, however, as a result of further research, that the aether is after all what is really fundamental, and that electrons and hydrogen nuclei are merely states of strain in the aether, or something of that sort. If so, the two 'elements' with which modern physics operates may be reduced to one, and the atomic character of matter may turn out to be not the ultimate truth." †

* Is it sufficient to say that this quantum is a certain fundamental constant h, such that when a body is vibrating v times per second the energy of this body is, because of this periodic motion, $h\,v$ or some exact multiple of $h\,v$? Is it enough to say that, concretely, a billion billion times h (taking a billion to mean a million million) is a quantity barely appreciable by our senses?

† B. A. W. Russell, *The A B C of Atoms,* p. 152.

In a striking passage Mr. Russell comes to his own conclusions, and it is not difficult to read between the lines of what he writes: " But even if the size of an electron should ultimately prove . . . to be related to the size of the universe, that would leave a number of unexplained brute facts, notably the quantum itself, which has so far defied all attempts to make it seem anything but accidental. It is *possible* that the desire for rational explanation may be carried too far. This is suggested by some remarks of Eddington. The theory of relativity has shown that most of the traditional dynamics, which was supposed to contain scientific laws, really consisted of conventions as to measurement, and was strictly analogous to the 'great law' that there are always three feet to a yard. In particular, this applies to the conservation of energy. This makes it plausible to suppose that every apparent law of nature which strikes us as reasonable is not really a law of nature, but a concealed convention, plastered on to nature by our love of what we, in our arrogance, choose to consider rational. Eddington hints that a real law of nature is likely to stand out by the fact that it appears to be irrational, since in that case it is less likely that we have invented it to satisfy our intellectual taste.* And from this point of view he inclined to the belief that the quantum principle is the first real law of nature that has been discovered in physics.

" This raises a somewhat important question: Is the world 'rational,' i.e. such as to conform to our intellectual habits? Or is it 'irrational,' i.e. not such as we should have made if we had been in the position of the Creator? I do not propose to suggest an answer to this question." †

Mysticism has come back to life. The contemptuous attitude of R. A. Vaughan (1823—1857) is no longer possible to anyone who cares to grasp the whole round of the experiences of life, and what is true of life is true of science. " Mathematics," writes Mr. Bertrand Russell in his *Mysticism and Logic,* " may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true." So Clerk-Maxwell found it, and so many of his scientific descendants are finding out.

* S. Eddington, *Space, Time, and Gravitation,* p. 200.
† B. A. W. Russell, *The A B C of Atoms,* p. 169.

Words like the following sound like an echo of Boltzmann or Brunetière, of Eddington or Einstein, yet they were penned by Clerk-Maxwell long before their day or even before the day of Henri Poincaré:

"If, therefore, those cultivators of physical science from whom the intelligent public deduce their conception of the physicist, and whose style is recognised as marking with a scientific stamp the doctrines they promulgate, are led in the pursuit of the arcana of science to the study of the singularities and instabilities, rather than the continuities and stabilities of things, the promotion of natural knowledge may tend to remove that prejudice in favour of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of that of the past."

Unexpectedness has marked the course of science during the last two decades. Now here the necessarily dogmatic nature of the text-book has gravely tended to confirm the limitations of not a few scientific men. The idola of Bacon and the Tréguierism of Rénan have laid their marked impression on them. How could it in some respects, as we reflect on the character of the ordinary text-book, be much otherwise? For the author who writes a book of science for use in schools and even in colleges writes with an air of authority. Any man of thought in any subject, even far removed from the domain of science, realises the haze that surrounds thought every whit as well as C. J. Vaughan (1816—1897) when he set forth the difference between the fluid form of Westcott's thought compared with the hardness of outline presented by that of Lightfoot. The text-book provides one clear-cut explanation—that is all. So far as the boy or the average undergraduate can tell, this is the only explanation. For sheer dogma, commend us to the book in the hand of a boy or a young man. How can either of them realise that there are other explanations, that the one offered to them is but one of them and may not even be more than in part correct? In any case, it is only a short statement of a highly condensed nature.

From the text-book turn to any scientific masterpiece, say, to Newton's *Principia*. There we read:

"But they, that like not this, may suppose light any other corporal emanation, or any impulse or motion of any other medium or aethereal spirit diffused through the main body of aether, or what else they can imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy; only whatever light be, I suppose it consists of rays differing from one another in contingent circumstance, as bigness, form, or vigour."

In Newton, as in any other genius, there is nothing of that cocksure air in the scientific manual that, we feel convinced, so injures the spread and the advance of scientific knowledge. Another object in writing our book will have been gained if we can persuade more of the rank and file in science, the average lecturer and the like, to peruse not merely *le dernier cri* but also the *magnum opus* of the genius of the past. Many matters were found in Newton's *Principia* by P. G. Tait and Lord Kelvin even after the lapse of almost two centuries, and we are certain that there are many other matters still hidden in it. We urge, then, the perusal of the *magnum opus* of the past, and we urge whole-heartedly the perusal of the biography of the writer of the *magnum opus.* Is there any form of reading more likely to dispel that air of certainty which in our day inflicts so much injury on science? A reading of memoirs is sure to lead one to see more in old conceptions than one has imagined, and in many an instance to be set on the track of an idea long hidden from the ken of men.

The perusal of biographies is our own favourite amusement, but of course it is more than this. No one can read hundreds of biographies without reflecting on the nature of the scientific man. The illusion that he is a being actuated by pure reason has long been shed, partly because he is found by his controversies to own a heart as well as a head; partly because by his heart, moving on the lines of Pascal, he has arrived at discoveries; and partly because he is at bottom an artist. Truth is as many-sided as man's nature, and it takes the whole of his nature to grasp it. It is not for nothing that Leonardo da Vinci was so many professions in turn. So he enlarged his experience of the *tout ensemble* of existence, and therefore he made his endless discoveries in widely-differing branches of knowledge. He was a great military engineer, he was a great civil engineer, he was a discoverer in many fields of knowledge, and he was a great artist. True, he never attained the assured craftsmanship of Titian or Paul Veronese, the free facility of Velasquez or the amazing audacity of Rubens. Still, he painted pictures that the world will not willingly forget. Leonardo da Vinci has left many a descendant among men of science in their artistic temperament. The artist possesses intuition as well as reason, and we are inclined to think that many of the past controversies arose because of this very matter. The intuition of one investigator combined with his set of presupposition led him to regard truth so exclusively from his own angle that he could not allow for the circumstance that another investigator was regarding it every whit as exclusively from his own angle. If we assume that in science there is no such thing as intuition, no such thing as the artistic temperament, and if we also assume that men are guided entirely by pure reason, then we utterly fail to understand the scientific quarrels we have chronicled. On the other hand, if we assume that in science there is intuition, there is such a thing as the artistic temperament, and if we also assume that men are not entirely guided by pure reason, then we understand these scientific quarrels. Men of science have been led, and are being led, along certain lines of research by a feeling of intuitive probability.* For intuition, like conscience, stands in need of guidance. Lotze gave intuition a place in thought to which it had been a stranger, but, like all who did a piece of pioneer work, he claimed far too much for intuition. Feeling † and intuition have, all the same, their due place, even though it is a subordinate one, in the discovery of all truth in general and of scientific truth in particular.

Science is poetry in the profoundest sense of the term. It is, of course, not true, save in a case like Tennyson's, that poetry is science. "The wind bloweth where it listeth, and thou canst not tell whence it cometh nor whither it goeth." So spoke Jesus Christ of old, and as we peruse the history of scientific discovery we realise afresh

* Lord Balfour expounds this admirably in his Gifford Lectures.

† The dedication of J. H. Muirhead's *The Service of the State* is to "M. T. M., who taught me to feel what Green taught me to think."

the truth of this saying. There are such surprises as a Newton from a Lincolnshire farm—or a Tennyson from a Lincolnshire rectory—or a Kelvin from the heart of busy Belfast. The greatest personalities in science have not obviously been the product of their environment. Nor is it a whit more true of literature or war or statesmanship. Literature has its surprise in a Shakespeare from Stratford-on-Avon, war its surprise in a Napoleon from Ajaccio, and statesmanship its surprise in a Lincoln from the backwoods of America. Environment, no doubt, in skilful hands will explain much, but will it explain the origin of a Newton and a Kelvin, of a Shakespeare and a Tennyson, of a Napoleon and a Lincoln?

A Kelvin, a Weismann, an Einstein, care passionately for music, a Hooker and a Cayley care passionately for art, thereby revealing the affinity of the scientist with the artist. Such men can say with Landor:

> Nature I loved, and, after Nature, Art;
> I warmed both hands before the fire of life;
> It sinks and I am ready to depart.

Nor do we wonder at such love of music when we remember that Leibniz held that "music is the pleasure the human soul experiences from counting without being aware that it is counting." We recall that music and mathematics originated together in the discovery of Pythagoras. The connection between the two dates back to classical times, and in our own day Spengler exerts himself to trace the connection over again, holding that the development of music throughout its various stages in our European culture has been intimately related with the stages of the development of mathematics.* Thought for the ordinary mortal requires complete consciousness. Thought for the extraordinary mortal does not require it. The artistic temperament can carry on a process of thought for a long period with almost complete unconsciousness. Wagner describes, for instance, how after a sleepless night followed by a dull walk: "I stretched myself dead tired on a hard couch awaiting the long-desired hour of sleep. It did not come; but I fell into a kind of somnolent state in which I

* *Der Untergang des Abendlandes*, I, p. 576.

suddenly felt as though I were sinking in swiftly flowing water. The rushing sound formed itself in my brain into a musical sound, the chord of E flat major, which continually re-echoed in broken forms; these broken chords seemed to be melodic passages of increasing motion, yet the pure triad of E flat major never changed, but seemed by its continuance to impart infinite significance to the element in which I was sinking. I awoke in sudden terror from my doze, feeling as though the waves were rushing high above my head. I at once recognised that the orchestral overture to the *Rheingold*, which must long have lain latent within me, though I had been unable to find definite form, had at last been revealed to me." *

What happened to Wagner in music happened to Poincaré in mathematics. He had been working for a considerable time at a complicated problem about Fuschian functions. "One night," he confesses, "I took some black coffee, contrary to my custom, and was unable to sleep. A host of ideas kept surging in my head; I could almost feel them jostling one another, until two of them coalesced, so to speak, to form a stable combination. When morning came, I had established the existence of one class of Fuschian functions, those that are derived from the hyper-geometric series. . . ." †

The next step was when he endeavoured to represent these functions by the quotient of two series. Consciously he thought this out, and then he had to leave his home at Caen in order to take part in a geological conference arranged by the School of Mines. The incidents of the journey drove his mathematical work out of his brain. "When we arrived at Coutances, we got into a brake to go for a drive, and, just as I put my foot on the step, the idea came to me, though nothing in my former thoughts seemed to have prepared me for it, that the transformations I had used to define Fuschian functions were identical with non-Euclidean geometry." ‡ A set-back for some days occurred, and he set out for the seaside, turning his attention completely away from his unsatisfactory mathematics. "One

* *My Life*, II, p. 603.
† H. Poincaré, *Science and Method*, p. 53.
‡ *Ibid.*, p. 53.

day, as I was walking on the cliff, the idea came to me, again with the same characteristics of conciseness, suddenness, and immediate certainty, that arithmetical transformations of indefinite ternary quadratic forms are identical with those of non-Euclidean geometry." *

On his return to Caen, Poincaré reflected on his result which carried him a stage further. The example of quadratic forms demonstrated to him that there are Fuschian groups other than those which correspond with the hypergeometric series. Could he apply to them the theory of the Theta-Fuschian series? He saw he could. Could he then deduce that there are Fuschian functions other than those derived from the hyper-geometric series? He also saw he could. Then came the crucial step of forming all these functions. Systematically he set to work on the problem, and solved the whole of it save one part. This he could not bring in, and it defied all his conscious efforts. Leaving Caen in order to serve as a conscript, he naturally had his mind preoccupied with drill and the duties of the barrack-room. "One day, as I was crossing the street, the solution of the difficulty which had brought me to a standstill came to me all at once. I did not try to fathom it immediately, and it was only after my service was finished that I returned to the question. I had all the elements, and had only to assemble and arrange them. Accordingly I composed my definitive treatise at a sitting and without difficulty." † Someone ‡ has laid down that the coming upon the natural scene of the musician's soul reveals a new range of meaning and beauty which before were dormant in the physical structure of the natural world; and reality as a whole assumes through him a new way of being. Such was essentially the nature of the experience of Henri Poincaré.

That the experience of Poincaré is by no means unique, the lives of Sir William Rowan Hamilton and of Kekulé attest. The former tells us that "quaternions started into life, or light, full grown, on Monday, October 16, 1843, as I was walking with Lady Hamilton to Dublin, and came up to the

* H. Poincaré, *Science and Method,* p. 54.

† *Ibid.*, p. 55.

‡ I cannot trace the source of this. When the Sinn Feiners stole my books and manuscript, they also stole all my note-books.

Brougham Bridge, which my boys have since called the Quaternion Bridge. That is to say, I then and there felt the galvanic circuit of thought *close,* and the sparks which fell from it were the *fundamental equations between i, j, k; exactly such* as I have used them ever since. I pulled out on the spot a pocket-book, which still exists, and made an entry on which, *at the very moment,* I felt that it might be worth my while to expend the labour of at least ten (or it might be fifteen) years to come. But then it is fair to say that this was because I felt a *problem* to have been at that moment *solved*—an intellectual want relieved—which had *haunted me* for at least *fifteen years before. Less than an hour* elapsed before I had asked and obtained leave of the Council of the Royal Irish Academy—of which Society I was, at that time, the President—to *read* at the *next General Meeting a Paper* on Quaternions; which I accordingly *did,* on November 13, 1843."

The German chemist Kekulé informs us of how he came in 1865 to hit upon the ring formula for the benzene molecule. When twenty-eight he was living in London, and used to discuss chemistry with a friend. He says: "One fine summer evening I was returning by the last omnibus, outside as usual, through the deserted streets of the metropolis, which are at other times so full of life. I fell into a reverie, and lo, the atoms were dancing before my eyes. Whenever, hitherto, these diminutive creatures had appeared to me, they had always been in motion, but up to that time I had never been able to discern the nature of their motion. Now, however, I saw how, frequently, two smaller atoms united to form a pair; how a larger one embraced the two smaller ones; how still larger ones kept hold of three or even four of the smaller; whilst the whole kept whirling in a giddy dance. I saw how the larger ones formed a chain, dragging the smaller ones after them but only at the ends of the chain. I saw what our past master, Kopp, my highly honoured teacher and friend, has depicted with such charm in his *Molecular-Welt*; but I saw it long before him. The cry of the conductor, 'Clapham Road,' awakened me from my dreaming, but I spent a part of the night in putting on paper at least sketches of these dream forms. This was the origin of the structure theory."

This provided him with the conception of the chain formula for the ordinary hydrocarbons of the paraffin series, but benzene still remained a mystery till he had another fit of inspiration. "I was sitting," we read, "writing at my text-book, but the work did not progress. My thoughts were elsewhere. I turned my chair to the fire and dozed. This time the atoms were gambolling before my eyes. My mental eye, rendered more acute by repeated visions of this kind, could now distinguish larger structures of manifold conformation, long rows, sometimes closely fitted together, all twining and twisting in snake-like motion. But look! What was that? One of the snakes had seized hold of its own tail and the form whirled mockingly before my eyes. As if by a flash of lightning I awoke, and this time also I spent the rest of the night in working out the consequences of the hypothesis." Such is the account Kekulé gave in 1890 when men celebrated the anniversary of this discovery.

If the scientist is not attracted by music, he may be by architecture, which is the art Goethe called "frozen music." The laws of statics and dynamics are at work in the structure of the building. If the building is worthy of the name of architecture, the laws of statics and dynamics are translated into a thing of beauty which is a joy for ever. If neither music nor architecture exercises magnetic force over him, painting may. If none of these three can weave its spells, literature may succeed where it fails?. Here of course biographies ought to help us, but very often they do not reveal a single artistic taste of the subject. It does not follow from this omission that the scientist has had no such tastes, for it sometimes means that the biographer has a false impression of the dignity of his labour, deeming such matters as love of music or of architecture as beneath his notice. There is no adequate life of Newton, and what Sir David Brewster has done for us is to depict the mathematician and to leave the man to the one side. We may be sure that Newton, different in most matters from ourselves, was composed of the familiar flesh and blood. We want new biographies of Newton and Clerk-Maxwell, drawing the men and setting their results against the background of the newer knowledge gained since their time. We also require an elaborate biography of Henri Poincaré, taking account of the man as well as of his philosophy. The men who write these biographies must be of imagination all compact, as Sir William Osler has put it. For scientific subjects are by no means aliens in the land of imagination. Our work-a-day world is bounded by the three dimensions of the space in which we live and move. The mathematician like Weyl or Einstein has long transcended these three dimensions, forming a conception of space of 4 or 5 or *n* dimensions. A Sir J. J. Thomson or a Sir Ernest Rutherford, engaged on the mass of an electron or the mass of a hydrogen nucleus, piercing the secrets of the smallest entities, brooding over the dance of vortices imagined by a Kelvin, with his magic wand summons elemental forces to reveal the nature of their powers to his scientific gaze. From one aspect we behold the disciplined brain of the man of science. From another aspect we behold the imaginative inspiration of the poet. Newton's transition from a falling apple to a falling moon was, at the outset, a leap of the imagination.

Instead of the processes of mathematics being the most inhuman, they are, rightly regarded, the most human. For if the brain of the scientist is present, the inspiration of the poet must also be present—if any great discovery is to be revealed to the sight of mankind. "The mathematician's best work is art," holds Mittag-Leffler, "a high and perfect art, as daring as the most secret dreams of the imagination, clear and limpid. Mathematical genius and artistic genius touch each other." In a past generation James Joseph Sylvester (1814—1897) was deemed one of the greatest mathematicians of his day, taking rank with his friend Cayley. We learn that he was wont to write in language enriched with poetical imagination, and by illustrations drawn from themes far afield from pure science. Men reproached him for so doing. But he could proudly point to the fact that he not only made mathematics but he also made mathematicians who were attracted by his graphic methods. He had a genuine love of literature and cared intensely for the structure of English verse, publishing in 1870 *The Laws of Verse,* an attempt to illustrate from his own verses and those of others the principles of what he called phonetic syzygy. His own verses show ingenuity and invention, and there is the authentic note of true poetry in his translations from German. As an undergraduate member of St. John's College, Cambridge, he cared for music as fervently as Kelvin or Helmholtz or Einstein. Indeed he had taken lessons in singing from Gounod. It is intelligible, therefore, that in his *Theory of Reciprocals* Sylvester should ask, "Does it not seem as if Algebra had attained to the dignity of a fine art, in which the workman has a free hand to develop his conceptions, as in a musical theme or a subject for painting? It has reached a point in which every properly-developed algebraical composition, like a skilful landscape, is expected to suggest the notion of an infinite distance lying beyond the limits of the canvas." Nor is he singular in his opinion. "Mathematics," maintains Mr. Bertrand Russell in our generation, "possesses not only truth, but supreme beauty—a beauty cold and austere, like that of sculpture. The true spirit of delight, the exaltation, the sense of being more than man, which is the touchstone of the highest excellence, is to be found in mathematics as surely as in poetry." Sir Christopher Wren may have built St. Paul's Cathedral out of stone, but we can say of him that "he was really an artist using the stuff of science as his material." The same remark applies to every scientist worthy of his high calling. "There is no such thing as an unimaginative scientific man," we have heard G. F. Fitz-Gerald declare. He was in this matter wildly astray. There are indeed unimaginative scientific men. As an undergraduate in two universities we heard them lecture, and since graduation we every now and then meet them.

Arthur Cayley (1821—1895) ranks with the greatest of mathematicians. Nor were painting and architecture aloof from him. The works of such painters as Masaccio, Giovanni Bellini, Perugino, and Luini proved a special source of delight to him. Architecture attracted him as much as painting. With all this range of knowledge, he was a mathematician of as catholic tastes as Euler himself. Singularly learned in the labour of other men, he owned a width in his range of reading that was enviable. Clerk-Maxwell * wrote lines based on a profound admiration of Cayley.

* His Life, p. 636.

O wretched race of men, to space confined!
What honour can ye pay to him, whose mind
To that which lies beyond hath penetrated?
The symbols he hath formed shall sound his praise,
And lead him on through unimagined ways
To conquests new, in worlds not yet created.

First, ye Determinants! in ordered row
And massive column ranged, before him go,
To form a phalanx for his safe protection.
Ye powers of the *n*th roots of minus one!
Around his head in ceaseless cycles run,
As unembodied spirits of direction.

And you, ye undevelopable scrolls!
Above the host wave your emblazoned rolls,
Ruled for the record of his bright inventions.
Ye cubic surfaces! by threes and nines
Draw round his camp your seven-and-twenty lines—
The seal of Solomon in three dimensions.

March on, symbolic host! with step sublime,
Up to the flaming bounds of Space and Time!
There pause, until by Dickinson * depicted,
In two dimensions, we the form may trace
Of him whose soul, too large for vulgar space,
In "*n*" dimensions flourished unrestricted.

Cayley's favourite authors were Sir Walter Scott and Jane Austen. *Guy Mannering* and *The Heart of Midlothian* among Scott's, and *Persuasion* among Jane Austen's, were the books he liked best. He was fond of George Eliot's novels, particularly of *Romola.* Ian Maclaren's *Beside the Bonnie Brier Bush* met with words of warm praise. On the other hand, he did not like Thackeray, and would not read Dickens. He had a keen liking for many of Shakespeare's plays, notably *Much Ado about Nothing,* and some of the historical dramas. He liked Milton's shorter poems as much as he disliked *Paradise Lost.* Did this dislike spring from a subconscious feeling of all the harm Milton inflicted upon both science and religion by his special creation theory? Scott's poems Cayley often read, and he displayed a lively appreciation of Coleridge's *Ancient Mariner.* A good linguist, he read French, German, Italian, and Greek, entertaining profound regard for Plato. Grote's *History of Greece* and Macaulay's *History of England* were favourites, and he never seemed to tire of Lockhart's *Life of Scott.*

* Lowes Dickinson painted his portrait.

In our day Einstein brings to whatever he deals with a breadth of outlook, a wide generality of conception, that remind us of Cayley and Poincaré. The piano to Einstein forms, to use his own words, "a necessity of life." He is a good violinist, an accomplished musician. His face, we learn, is illumined when he listens to music. His favourites are Bach, Haydn, and Mozart. He likes much less Beethoven and Wagner, while to such romantics of music as Chopin and Schumann he is as frankly indifferent as he is to painting. Architecture and literature both attract him. While not attracted by Ibsen, he is warmly attracted by Cervantes, Keller, and Strindberg. Goethe he reads, but Shakespeare he adores. Above all, he admires Dostoevsky, notably his masterpiece, *The Brothers Karamazov*. Einstein confesses that "Dostoevsky gives me more than any scientist, more than Gauss." All literary analysis or æsthetic subtlety, it seems to Einstein, fails to penetrate to the heart of a work like *The Brothers Karamazov*: it can only be grasped by the feelings. His face lights up when he speaks of it, and he can find no word for it but "ethical satisfaction." Men say that the keynote of this thinker's emotional existence is the cry of Sophocles's *Antigone*: "I am not here to hate with you, but to love with you."

Insight combined with intuitive probability marks the labours of Blaise Pascal. Walter Pater thus depicts his powers: "Hidden under the apparent exactions of his favourite studies, imagination even in them played a large part. Physics, mathematics, were with him largely matters of intuition, anticipation, precocious discovery, short cuts, superb guessing. It was the inventive element in his work, and his way of painting things that surprised most of those most able to judge. He might have discovered the mathematical sciences for himself, it is alleged, had his father, as he once had a mind to, withheld him from instruction in them." At the end of his days Rousseau realised that the great labour of his life, which had been to express intuitive certainty in words which would carry intellectual conviction, had been in vain, and his last words were: "It is true as soon as it is felt."

Helmholtz was as well aware as Tyndall of the large share taken by intuitive probability in the striking results of Faraday. He guessed by hypothesis, but he was always careful to subject his hypothesis to the test of experiment. He provided himself with a guess in order to guide him in the laboratory. The series of experiments suggest that this guess will not cover the results at which he has arrived. Very well, then. The next step is to form another hypothesis more adapted to cover all the fresh facts. Theoretic divination formed the prelude to all his experiments, but he never for a single second hesitated to throw away any preconceived notion the moment facts stood in its way.

Last year was the centenary of the birth of Lord Kelvin, and accordingly the anniversary was honoured by speeches testifying to the nature of his labours. By a striking coincidence in the speeches of Lord Balfour at the centenary banquet and Sir J. J. Thomson at the Institute of Civil Engineers, stress was laid on Lord Kelvin's intuitive qualities. "What was characteristic of Kelvin above all others," observed Lord Balfour, "with the exception of Archimedes, was that he almost instinctively applied the knowledge which the study of natural laws gave him to the needs and happiness of mankind." Sir J. J. Thomson observed, "To the intuition of the engineer Kelvin allied the genius of the mathematician, and he could so apply mathematics to the solution of physical problems that he justified Bacon's statement that 'research begins with physics and ends with mathematics.'" Kelvin's friend Rayleigh informed John Aitken in 1917: "I recommend you not to be too modest! a good instinct and a little mathematics is often better than a lot of calculations."

In theoretical investigations intuitive probability has succeeded beyond all expectation, and in practical ones it has similarly succeeded. This was proved during the World War, when, for example, Lord Moulton was diverted from his duties as a judge in order to attend to the production of nitrogen and other matters indispensable for our success. Before he had been elevated to the bench, he had had a large practice in patent-cases. There he used to say that his first impression of the validity of a disputed patent was in all probability the right one. Men might argue him out of his impression, but the verdict generally confirmed what he had thought at first. He entertained a lively sense of what he termed "back-of-the-brain" working. His scientific knowledge was wide, and it was backed by a real scientific instinct. Whether a new scientific suggestion was practicable or impracticable, he divined. We give an example. During the war the question of our devoting our energies to the fixation of atmospheric nitrogen arose. It was perfectly possible, and Moulton knew that it was actually being tried in Germany. Of course were he successful, here was a source of nitrogen which neither foreign interference nor U-boats could affect. He divined that with the limited stores of goods and labour available, the plan would very likely not prove of any benefit to us before the end of 1918. He accordingly rejected the project of the fixation of atmospheric nitrogen. As a matter of fact, experiments conducted since the signing of the Peace of Versailles demonstrate the correctness of his decision.* This peculiar power of feeling for new truths is a prime requisite for an original investigator: it is the *Forscherblick* quality of genius.

The scientist shares many of the artistic gifts in no scanty degree if he is conducting researches of the highest class. Such gifts, by their very nature, lead occasionally to misunderstanding that sometimes develops into bitter controversies. Of Max Planck, the deviser of the quantum theory, Einstein entertains a warm admiration, saying of him that "the emotional condition which fits him for his task is akin to that of a devotee or a lover." The quarrels of lovers in the past have not invariably led to the renewing of love. But what if the scientists recognise that they are artists swayed by the artistic temperament? Such a recognition may lead them to make more allowances for those who differ from them. At a celebration given in honour of Planck in 1918 Einstein gave a glowing picture of the ideal physicist. "I agree with Schopenhauer," he said, "that one of the most powerful motives that attract people to science and art is the longing to escape from everyday life with its coarseness and desolating barrenness, and to break the fetters of their ever-changing desires. It impels those of keener sensibility out of their personal existence into the world of objective perception and understanding. It is a motive force of like kind to that which drives the

* H. F. Moulton, *Life of Lord Moulton*, pp. 49, 205.

dweller in noisy, confused cities to restful Alpine heights whence he seems to have an outlook on eternity. Associated with this negative motive is the positive motive which impels men to seek a simplified synoptic view of the world conformable to their own nature, overcoming the world by replacing it with this picture. The painter, the poet, the philosopher, the scientist, all do this, each in his own way." Each of them can attain towards this goal—if the idola of Bacon and the Tréguierism of Rénan do not interpose obstacles.

The man of science lives in relation to a world infinitely greater than ourselves, offering a spectacle perpetually renewed, incomparably vast, and behind the splendid harmony of natural laws he derives something vaster, brighter still. Such a vision will enable him to forget the prepossessions that have blinded him in the past. For our chapters contain record after record of his errors of omission and of commission. Long ago Hegel pointed out that tragedy is not the conflict between right and wrong, but the conflict between right and right. When we regard the scientist as an artist, this renders the causes of conflict all the more intelligible. The vision of all we might be and the contrast of how much we have fallen short of this vision in the annals of the nineteenth century are enough to make us yearn to get rid of the idols that block the path. The man of science has learnt the beauty of exactness, the horror of tampering with the result, and the difficulty in the interpretation of it. Knowledge in general and science in particular render us yet another service. In her fields, in her courts, no one of us labours alone. To her monuments each of us can bring but a stone, nor hope to add more than a fragment. The man of science works for humanity, and we all love that for which we labour and offer ourselves up in daily sacrifice. He will respond to the sense of solidarity. As he responds to this sense, the feeling of awe and wonder steals over him. Truth is his quest. Truth is the quest of other scientists. They and he are co-workers in the creative process which eternally proceeds. They all realise that love of truth, that care in its pursuit, and that humility of mind which make the possibility of error always present. Where is there place for cocksureness of attitude, for infallibility in pronouncement? Where is there room for that dreadful trinity, envy, hatred, and malice, when all ought to have rid themselves of the idola of Bacon and the Tréguierism of Rénan? As the feeling of awe and wonder steals over the scientist, and as he rids himself of his prepossessions, the sense of strife dies away, to be replaced by the sense of the services of other labourers in the same field.

Tennyson sings:

> Let knowledge grow from more to more,
> But more of reverence in us dwell,
> That mind and soul, according well,
> May make *one* music as before
> But vaster.

And to this prayer with all my heart and head, I say, "Amen."

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Printed in Great Britain by Hazell, Watson & Viney, Ld., London and Aylesbury.