Biographical Byways.[1]

By Sir ARTHUR SCHUSTER, F.R.S.

3. BALFOUR STEWART (1828–1887).

IF I were asked to name Balfour Stewart's outstanding quality as a scientific investigator, I should designate his absolute freedom from preconceived ideas both in the selection of his subjects and the manner in which he treated them. He was fond of arguing by analogy or familiar illustration. According to the writer of his obituary notice in the Proceedings of the Royal Society, who knew him intimately, he was "full of the most weird and grotesque ideas." I cannot say that I ever became conscious of this in my own intercourse with him, but I only came into contact with him after his slow recovery from the injuries sustained in the Harrow railway accident of 1870. He was not a good lecturer and had difficulty in keeping order in the lecture-room—perhaps it would be more correct to say that he did not take the trouble to keep order, being too sympathetic with youthful exuberance. In the laboratory he was an inspiring teacher, and it would not be an exaggeration to say that he was the godfather of much of our modern science, both Poynting and J. J. Thomson having received their first lessons in physics from him.

Balfour Stewart's family intended him for a mercantile profession, and at the conclusion of his university studies he spent some time in Australia. But science had laid its spell on him, and he soon returned to Edinburgh, where he became assistant to J. D. Forbes, who had considerable influence in shaping his mental outlook. It was during the six years he spent at Edinburgh that the work on the equilibrium of temperature radiation was begun and, in its essential features, completed. In 1859, Balfour Stewart was appointed superintendent of the Kew Observatory, which was then managed by a committee of the British Association under the presidency of P. Gassiot. All went well until the organisation of the meteorological service of the country was transferred from the Board of Trade to a committee of the Royal Society, consisting of eight Fellows, with General Sabine as chairman. The expenses were covered by a Treasury grant of 10,000*l*.

Trouble soon arose, and, I think, both for their historical interest and in justice to Balfour Stewart's memory, an account of the incidents which ultimately led to his retirement from the directorship of Kew Observatory should be given. I am enabled to do so on the evidence of the relevant documents, which came into my keeping after Balfour Stewart's death. When the Board of Trade had agreed to the request of the Meteorological Committee for the assistance of a scientific secretary, Balfour Stewart was appointed to that office, understanding that he was to be the scientific adviser of the Committee; but when afterwards he was designated simply as "Secretary to the Committee" he disliked the omission of the qualifying word "scientific," but acquiesced. "Nevertheless," he declared in a printed statement from which I quote, "I continued to understand that it was my special duty, in case I might see anything defective in the scientific position of the Committee, to urge them to amend it."

Differences of opinion soon arose between Sabine and Stewart with regard to the method of reducing meteorological observations, and his repeated requests for clerical assistance were declined by the Committee. The crisis came when Balfour Stewart directed General Sabine's attention to what he considered to be an error in an unconfirmed minute of one of the meetings of the Meteorological Committee. Stewart's account of the interview which took place concludes with the following statement: "He [General Sabine] assured me there was no mistake and added in answer to a question that he, *on his own responsibility*, had authorised the preparation of such of those results at the central office as had not been authorised by the Committee." To use a familiar term, Sabine admitted having cooked the minutes. At the same time, Balfour Stewart was privately told that the chairman was much opposed to his scheme of reducing observations, and that there was not much chance of its being adopted. With regard to the merits of the proposed scheme there can be little doubt. Stewart had submitted it to a few independent men of science and the reply of the Astronomer-Royal, Sir George Airy, may be given *in extenso*.

> "I have read with much satisfaction the paper of your Remarks on Meteorological Reductions, etc., especially with reference to Vapour. I do hope that by going on thus you may make Meteorology a science of causation, and raise it from its present contemptible state.
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> "I have often thought that much may be gained by ascertaining at what rate aqueous vapour disseminates itself through air, and should long ago have made experiments, but that I want a hygrometer of sufficient delicacy. I then thought of suggesting it to the Kew Committee. Your paper restores the interest in my old intention, and I think I shall write to Mr. Gassiot."

Lord Kelvin (then Sir William Thomson) also gave his full approval, writing:

> "I believe the plan you propose is adapted to bring out information of the most valuable kind, from observations which, until reduced on some such plan, might be accumulated indefinitely without any practical benefit."

Stewart was naturally distressed by the manner in which his advice was set aside, no scientific grounds being given. Fearing that the anxieties of his office might affect his health, he wrote a letter to the chairman of the Committee resigning the secretaryship. He also tendered to Mr. Gassiot his resignation as superintendent of Kew on the ground that the two bodies were closely bound together, but declared at a meeting of the Meteorological Committee that he gave up this office with extreme reluctance. He was asked, in an interview with Mr. Gassiot, whether there was anything that would induce him to withdraw his resignation, and was given to understand that Sabine would wait to hear the condition under which he would continue office before taking further steps. Stewart then wrote a letter explaining the difficulties

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in which he was placed owing to insufficient help in the numerical work and stating that, if some assistance were given him in the preparation of the preliminary reductions of the observations, he desired to withdraw his letters of resignation. The reply was as follows:

"I regret that you were so determined to send in your resignation. It appears Sir Edward Sabine has written to Bombay, where Colonel Smythe is, and nothing can be done until the reply comes."

Sabine's letter referred to, offering the appointment to another man, was posted on the day of the interview with Gassiot, and—as Stewart points out—before his resignation had been formally accepted by the respective committees.

The obstinacy with which Sabine pressed his own opinion is perhaps intelligible in a man who was then eighty-one years old, but there is nothing to say in extenuation of the want of generosity exhibited in the following letter to Stewart:

"My not having responded to your request more promptly and more fully, has not arisen from want of solicitous thought, and wish to serve you.

"I feel assured that if your work at Kew had ere now been crowned by the looked-for completion of the account of the results of the long and invaluable series of magnetic observations, the later and longer portion of which were under your own superintendence, you might, and I might, have appealed triumphantly to such an evidence, not only of what you were capable of doing, but of what you had done, as placing you in a pre-eminent position."

The letter is dated May 31, 1870, and was presumably written in answer to a request for a testimonial in view of Stewart's candidature for the chair of physics at Owens College, Manchester. With regard to the implied complaint, I have before me the copy of a letter written by Balfour Stewart, from which it appears that he was waiting, previous to 1865, for the details of the observations which were in Sabine's possession. In spite of his repeated requests they were never sent to him. He could scarcely be expected to start on an extensive work of reductions before he had the whole material before him.

Balfour Stewart's greatest scientific success was achieved in his researches on the equilibrium of radiation in an enclosure of uniform temperature, which led to the enunciation of the connexion between radiation and absorption. His omission to drive home convincingly the application of his results to the explanation of the dark Fraunhofer lines was, in his own later opinion, due to a want of chemical knowledge. Looking at a flame coloured with common salt, and believing that the yellow colour of the flame was due to luminous sodium chloride, he was disappointed to find that a plate of rock salt did not sensibly absorb the emitted light.

Stewart had the faculty of recognising the importance of problems, even when he had not the power theoretically or experimentally to make much headway in their solution. He saw, for example, the need of studying the temperature equilibrium in an enclosure which contained moving bodies, both radiation and absorption being affected by the Doppler effect. But instead of looking for the solution of the difficulty—as was subsequently done by Wien—in an adjustment of the law of radiation as depending on temperature and wave-lengths, he imagined that the equilibrium of radiation was actually destroyed, the second law of thermodynamics being satisfied by the mechanical forces necessary to maintain the motion. In conjunction with Tait he designed an experiment in which a disc was kept rotating *in vacuo*, and believed he had actually discovered an increase of the temperature of the disc. The success of the experiment depended of course on the perfection of his vacuum, and Stewart shared the erroneous belief of the time that a perfect so-called chemical vacuum could be obtained by filling a vessel with carbonic acid, exhausting with an ordinary air pump and absorbing the remnant of the gas with caustic potash.

I have remarked that Stewart's mind worked a good deal by analogies. He was fond of one particular illustration. Imagining a moving train and a body of men cutting across by jumping into it from one side and out of it at the other, it is clear that the train will gradually lose speed. The idea was applied to special cases and suggested several experiments to him. I joined him in one of these, in which an electric current was passed through water and an electromotive force applied at right angles to the current. Stewart hoped to detect some interference of the currents with each other. The same type of reasoning was in his mind in contemplating possible mechanical effects of radiation. I believe that at the bottom of these speculations was some prophetic glimmering that a propagation of energy always implies a propagation of momentum. The weak feature of his work was, that he often designed and tried experiments of a refined nature with appliances which were insufficient, and even at that time might have been improved upon—such were his attempts to discover a screening effect of metals on gravitation, or a change of mass by chemical combination. In the latter experiments, in which the combining bodies were mercury and iodine sealed up in a glass bottle, J. J. Thomson, who assisted him, nearly lost his eyesight through an explosion.

Stewart was indefatigable in his work. While the days were spent in the laboratory, he pursued his statistical investigations on magnetic and solar phenomena in the evenings. Some of these researches are published under the joint names of himself, De la Rue and Loewy. The latter gentleman—though I believe he had some claim to scientific knowledge—was chiefly employed as an assistant, paid for carrying on the numerical work, which was often heavy. I believe that De la Rue's share consisted in supplying the funds. One morning Stewart arrived at the laboratory in a great state of distress. In looking over the proofs of a paper accepted for the Philosophical Transactions, he had found that the numerical work was all wrong. Loewy had, in fact, saved himself trouble, and evolved the results out of his inner consciousness. The paper had to be withdrawn, and De la Rue paid a substantial sum for the expenses already incurred in printing. Neumayer, who was at the time director of the "Sternwarte" at Hamburg and on whose recommendation Loewy had been engaged, told me afterwards that he had sent for Loewy and charged him with manipulating the results. Loewy admitted doing

this, but excused himself on the ground that, while he had originally worked honestly, Stewart had never checked his results, so that the blame must be his.

Some reference should be made to a little volume, "The Unseen Universe," published in 1875, and intended to reconcile science with revealed religion. It appeared anonymously at first and, though probably forgotten now, it created a sensation at the time, running rapidly through many editions, in the later ones of which the authors' names—Balfour Stewart and P. G. Tait—were given. Referring to Tait's contribution, Stewart told me that when he first approached him, suggesting a joint publication, his consent was subject to the condition that Stewart should write the book while he would make himself responsible for the preface. When this was agreed to and the manuscript of the preface arrived, Stewart was amused to find that it was almost entirely taken up with an attack on John Tyndall, who was Tait's *bête noire*. It had to be re-written, and to judge from internal evidence I should surmise that not much more than the first paragraph was Tait's work. I am under the impression, nevertheless, that Tait's share in the book was not negligible, and that though he acted mainly in an advisory capacity at first, he made substantial additions in the later editions.

Towards the close of his life Stewart became much interested in so-called spiritualistic phenomena, but he always insisted—sometimes with great vigour—on his disbelief in messages from the dead, which were contrary to his religious convictions. With regard to unexplained phenomena, in which fraud may possibly have a share, it must be said that Stewart's confiding nature rendered him quite unfit to act as a judge. He was like a child in these matters. A certain personage near Buxton—so far as my recollection goes, a clergyman—wrote to Stewart about his powers of second sight, which enabled him to find a hidden object or name a card drawn at random out of a pack. Stewart went to see him several times and was impressed. "What is most remarkable," he told me after the second or third visit, "is that the power can be transferred to others. There is a servant girl in the house who, after a stay of a few weeks, has acquired it and can now name an unseen card just as well as her master." Not a shadow of suspicion had crossed his mind.

Stewart's conversation was always suggestive and sometimes witty. The Principal of Owens College had a habit of writing letters to the professors when he had any fault to find. These always began with some complimentary remarks, the sting being reserved for the concluding sentence, or frequently a postscript. "Every billet has its bullet" was Stewart's comment after receiving one of these communications.

The Theory of Evolution since Darwin.[1]

By Prof. E. W. MACBRIDE, F.R.S.

THE most recent development of the doctrine of evolution is the revival of Lamarckism—that is, the belief in the inheritable nature of the effects of use and disuse. Just as Bateson in 1894 enunciated the doctrine of the origin of species by sports long before this view was consecrated by the experimental labours of De Vries and given the name of the "mutation theory," so Eimer (1887) and Cope (1888) rebelled against the Weismannian conception of an unalterable germ plasm totally independent of the effects of the experiences of the body. Eimer put forward the doctrine of orthogenesis. This theory states that variations are the results of the effects of the environment on the complex constitution of the living organism, but that this constitution determines the character of these variations; they are not indefinite, but take place in a few definite directions. Eimer, who chose for his special subject of observation the wall-lizard *Lacerta muralis*, and later the swallow-tailed butterflies, pointed out that new variations make their first appearance in the later stages of growth and become inherited earlier in life as the generations succeed one another. A beautiful example, he explains, is afforded by the Ammonites, in which new features are first distinguishable in the outer coil of the shell, which is, of course, the youngest and latest to appear, whereas in succeeding strata the new feature is found affecting the more central coils. Thus it will be observed that Eimer draws the most decisive support for his theory from palæontology. Eimer seems to suppose that he is an opponent of Lamarck, but the only difference between them that I can discover is that Eimer seems to regard external conditions as altering the hereditary tendencies by direct action as sulphuric acid acts on metal, whereas Lamarck considers that external conditions stimulate an organism to make a response, and that it is this tendency to response that is inherited.

Cope, in his book "The Origin of the Fittest," likewise advocates the inheritance of the effects of use and disuse, and relies on palæontological evidence to support his view. He points out that if the development of the Ungulates during the Tertiary period followed, we find evidence that the shocks and strains to which the leg bones were subjected, and which in moderation create enlargement and strengthening of those bones during the lifetime of the individual, gave rise, as generation succeeded to generation, to permanent thickenings, fusions, and elongations of these bones; and that the modifications in teeth can likewise be explained as reactions to the changing character of the food by which the Ungulata were supported. Cope's views have become increasingly prevalent amongst North American palæontologists, and are almost universally accepted by them to-day.

The first great blow to Weismannism was delivered by the cytologists and experimental embryologists. The foundation stone of the "germ-plasm" theory was the fundamental distinction between body cells and germ cells, and the theory that, as development proceeded, the body cells were specialised so that each could only give rise to its special part of the body. But Driesch showed (1900) that if certain segmenting eggs were fragmented, a piece so small as one-eighth of the whole could give rise to a complete embryo, and Hertwig and Driesch further proved that the arrangement of these cells could

[1] Continued from p. 55.