covered that those sudden deviations from the normal which turn up without assignable cause in most breeds of domestic animals and varieties of cultivated plants obey the Mendelian rules when crossed with the types. Johannsen's results proved the non-inheritability in the case of beans of those small random deviations from the normal in all directions on which Darwin had laid such stress, and these results were independently confirmed by Jennings, who worked on Protozoa, and by Agar, who studied small Crustacea (1912).

In consequence of these discoveries, biological opinion veered round in favour of regarding the conspicuous aberrations commonly known as sports as the raw material on which natural selection had worked. Darwin, it is true, had considered the question of whether sports might not be the starting-point of new species, and had decided that they could not be so on account of the rarity of their occurrence. He thought that the chance of such a sport mating with its like, even if highly successful in the struggle for existence, was infinitesimal, and that if it did not mate with its like its characters would be "swamped by intercrossing." But if the deviation were in a direction favoured by natural selection, it might be assumed that it would make itself felt in lesser degree even when its orginal possessor crossed with the type, and the first generation descendants might still survive on account of its lessened manifestation. Many have claimed that this argument is strongly reinforced by the discovery of the laws of Mendelism. For if the deviation behaved as a dominant when crossed with the type, all the first generation of the descendants of such a cross would show it in as strong a manner as its orginal possessor; and even if it were recessive it would appear in undiminished strength amongst one-fourth of the second filial generation. The real objection to regarding sports as the initiators of new species lies deeper. An animal does not survive on account of one organ. In its growth from the egg to the adult, it runs the gauntlet of many dangers, and a strong development of some one organ might determine its survival at one period of its existence, but if the deviation occurs very rarely the chances against that particular animal reaching the critical stage at all are enormous.

De Vries, the director of the botanical gardens at Amsterdam, carried out between the years 1886 and 1899 a series of cultures of the garden plant *Œnothera Lamarckiana*, commonly known as the evening primrose. He showed that every year a number of sports turned up, usually about half a dozen in 10,000 specimens, sometimes as many as three in a hundred, and that this sports usually, though not always, bred true when crossed with their like. De Vries called these sports "mutations," and imagined that he had surprised a species in a "fit of mutation," and that new species were not produced by a slow process of differentiation but by sudden jumps, so that they began their existence complete in all their details, as Minerva sprang from the head of Jove. The De Vriesian doctrine joined with Mendelism, and became the dominant doctrine of evolution and heredity for most of the first quarter of the twentieth century, and probably counts amongst its adherents a larger number of biologists than any other doctrine at the present time. It was first seriously put forward by Bateson in 1894 in a book entitled "Materials for the Study of Variation," in which he figured and recorded a large number of examples of monstrous deviations from the normal, and laid down two doctrines, one of which is undeniably true, whilst the second is really the De Vriesian theory. The first was "variation is evolution"; the second, the "discontinuity of species is due to the discontinuity of variation."

The De Vriesian view has reached its climax in a book termed "Age and Area" by Dr. Willis, a distinguished botanist. This book was published two years ago; in it Dr. Willis supports the idea that species originate in sudden inexplicable jumps which occur only rarely. This idea is difficult to distinguish from the pre-Darwinian doctrine of special creation. No wonder that Haeckel, who lived long enough to encounter this view in its early presentation by Bateson, said: "If views like this are to be accepted, it would be better to return to Moses at once."

(*To be continued.*)

## Biographical Byways.

By Sir ARTHUR SCHUSTER, F.R.S.

### INTRODUCTION.

THERE are things seldom referred to in obituary notices and sometimes omitted even in more ambitious biographies. They tell the tale of peculiarities or weaknesses, which the writer fears may detract from the merits of the man he has set out to praise. The biographer believes, with some show of justice, that his main object is to give a record of work accomplished and not a psychological analysis of character. But eccentricities, or even decided failings, form part of a man's personality. The extent to which his teaching carries conviction and affects the scientific outlook of his time, depend as much on his personal attributes as on the merits of his researches. We destroy the balance of a just valuation, if we ignore those shades of character or temperament which act as handicaps to the full fruition of his work.

It has been my good fortune to be acquainted personally with many of the men who laid the foundations of the science of the nineteenth century, and I have retained a vivid memory of such intercourse as I had with them. In writing down some of my recollections I have tried to outline personalities in a sympathetic spirit. If human frailties are sometimes exposed, I hope that the limits of allowable candour have never been transgressed, and that, apart from the personal factor, the incidents related may be found to contain some substantial contributions to the history of science during the middle period of last century.

### 1. URBAIN JEAN JOSEPH LEVERRIER (1811–1877).

Towards the end of December 1874, or nearly in the year 1875, I received an invitation from the Royal Society to take part in an expedition which was being

organised to observe the total solar eclipse of April 1875 in Siam. Norman Lockyer was expected to act as leader of the expedition, which was to start in February; the time for preparation was therefore short. Ultimately Lockyer, who was then acting as secretary to the Royal Commission on Science Teaching presided over by the Duke of Devonshire, did not obtain the leave of absence he had expected, and I was put in charge. I had never had any experience in mounting or dismounting astronomical instruments, or indeed in using them, and one of the appliances on which we depended—a large siderostat—was under construction and not expected to be ready before the eve of our departure. It was essential that I should get some knowledge of the instrument, and more especially of the process of silvering the mirror, which was to be a foot in diameter. A similar siderostat was in use at the Paris Observatory, where M. Adolphe Martin had found a simple and convenient method of silvering large surfaces of glass. I was therefore sent to Paris, the consent of Leverrier, the famous director of the Observatory, having been obtained.

I first called on Cornu to ask advice on some optical questions that had arisen and, needless to say, I met with a most friendly reception both from him and other scientific men, notably Jamin. When Cornu heard that I was to call on Leverrier, he shook his head and said: "Je ne sais pas si M. Leverrier est l'homme le plus détestable à Paris, mais je sais que c'est l'homme le plus détesté." This was not encouraging, and it was in fear and trembling that I entered the Observatory.

I was received by one of the assistant observers, C. Wolf, who remarked with a look full of sympathy: "You will find M. Leverrier in a very bad temper: he has just returned from a funeral." I was then barely twenty-three years old, and naturally looked upon Leverrier (who was then sixty-four) as one of the formidable veterans of science. I was ushered into the "Presence," received with a searching look and the abrupt question: "Qui êtes-vous et que voulez-vous ici?" I mildly answered that I understood Mr. Lockyer had written to explain the purpose of my visit. "So he has," said Leverrier, "but I want to hear it from you." After I had replied to the best of my ability, I was dismissed with the remark: "I have already given instructions that every assistance should be given you."

I spent an interesting and instructive week practising Martin's silvering process, which has the great advantage that the surface comes out polished, except for a thin veil which is easily removed without appreciable friction. When it was time to return home, I suggested to M. Wolf that it might be sufficient for me to write a letter of thanks to Leverrier without troubling him with a personal call. But Wolf would not hear of this, and I was shown again into the "lion's den." Leverrier was sitting at his desk, and by his side stood a trembling young assistant to whom he continued to speak, taking no notice of me. I listened to a conversation of which I remember the main points without pretending to literal accuracy.

LEVERRIER. And so you tell me, that after trying for a whole week you have not yet found the mistake in your calculations?

ASSISTANT. No.

LEVERRIER. You have, of course, applied the correction for . . . (I did not catch the details).

ASSISTANT. Yes.

LEVERRIER. Did you apply it with a plus or a minus sign?

ASSISTANT. Plus.

LEVERRIER. It ought to be minus. That is your mistake. Go and correct your calculations.

After the assistant had left the room, Leverrier chuckled. "I knew all along," he said to me, "what his mistake was, but I wanted to see whether he could find it out by himself"; and to my great surprise he continued, "come and have a walk round the garden." All traces of peevishness and severity had disappeared, and for half an hour or more he became a most interesting and encouraging talker. A new reflecting telescope was just being erected in the grounds of the Observatory, and he explained the uses to which it might be put, inviting me, whenever I felt inclined, to come and work with it.

He then began to speak on a subject on which he evidently felt very strongly. Great preparations had been made in the previous year, and much money spent, on fitting out expeditions to observe the transit of Venus, which had just taken place on December 8, 1874. The French Government had followed the example of other countries, but against Leverrier's advice. It was, of course, well known to astronomers that he preferred other methods of determining the solar parallax; but the Government would not listen to his advice. "Que voulez-vous?"—France had recently been defeated in war, and if she did not share in international work, the Government was afraid that its action might be misinterpreted and believed to be due to sulkiness or want of funds. But Leverrier strongly expressed his opinion that the money was all wasted, and that neither this nor the subsequent transit of 1882 would add anything of value to our knowledge. In this he proved to be perfectly right.

### 2. JOHN PRESCOTT JOULE (1818–1889).

I once asked Joule what he felt like when he heard that one of his papers was rejected by the Royal Society. "I was not surprised," he answered, "I could imagine these gentlemen in London sitting round a table and saying to each other: 'What good can come out of a town where they dine in the middle of the day.'"

There are some interesting and somewhat puzzling circumstances connected with the fate of that paper, which was communicated to the Royal Society by one of its secretaries, Peter Mark Roget, on October 16, 1840. Under the title "On the Production of Heat by Voltaic Electricity," it contained the account of an experimental investigation which had led Joule to formulate his all-important law, that the heat generated in a conductor by an electric current is proportional to the product of the resistance and the square of the current. The paper was read on December 17, and in due course a short abstract appeared in the Proceedings which gave the final result arrived at, and hence secured Joule's priority. It is, therefore, not quite correct to say that the paper was rejected. The difficulty arose in connexion with its publication *in extenso*. The paper was short—it would not have taken up more than four or five pages in the Proceedings—and it was perhaps

considered that such far-reaching results could not be proved by the comparatively few experiments conducted by Joule. Criticisms were also made on the ground that previous investigations on the same subject were not mentioned. On March 11, 1841, the communication was committed to the Archives.

A paper carrying the title "On the Heat evolved by Metallic Conductors of Electricity and in the Cells of a Battery during Electrolysis" shortly afterwards appeared in the *Philosophical Magazine*. It bears the date March 25, 1841, and its introductory paragraph concludes with the sentence:

> "I hope therefore that the results of my careful investigation on the heat produced by voltaic action are of sufficient interest to justify me in laying them before the Royal Society."

This remark has naturally led to the belief (definitely expressed by Osborne Reynolds in his extensive memoir on Joule published by the Manchester Literary and Philosophical Society) that the paper, as printed in the *Philosophical Magazine*, is the one declined by the Royal Society. This, however, is not the case. The difference in the title is significant. The Royal Society paper deals with solid conductors, covering only the ground which in the *Philosophical Magazine* appears as "Chapter 1," and is there followed by a second chapter, twice as long, dealing with electrolysis and adding considerably to the range and importance of the results. Even in the first part the two papers are not identical, only a few short paragraphs being unaltered—though it must be admitted that the alterations are not material. It is not at all certain whether the complete paper as it appeared in the *Philosophical Magazine* would have been declined by the Royal Society; but it is perplexing that the reference to the Royal Society has been left standing in the altered and extended paper.

We cannot suppose that Joule deliberately wished to convey a wrong impression, and only one explanation seems to me to offer itself. It may be surmised that some correspondence took place in the three months between the date at which the paper was read and that at which it was committed to the Archives. On being informed of the objections raised, Joule may have prepared a more complete account to be substituted for the paper originally submitted; but the Royal Society having finally declined to print the original paper *in extenso*, he was quite likely to forward the amplified version to the *Philosophical Magazine*, the reference to the Society in the opening paragraph being left standing by an oversight.

It is not my desire to acquit the Royal Society of all blame, but mitigating circumstances might be urged. Joule's experiments no doubt appear conclusive to us, but the very simplicity of his experimental arrangements, and the comparatively few numerical results given, may have raised doubts which were perhaps excusable. The heat generated was determined by the rise of temperature of a measured quantity of water in which a coil of uncovered wire was inserted, and no cooling correction was applied. Though Joule gave good reasons why these simplifications did not affect the result, such cavalier treatment of the minor sources of error may have shocked the academically-trained mind as showing want of respect for the dignity of the problem. It is seldom that referees can rise to the standard of Stokes, who, in reporting on a communication by an eminent man of science possessing great intellectual powers not always assisted by clearness of expression, gave his judgment as follows:—"The first part of the paper I can understand but do not agree with; the second part I cannot understand, but as the results arrived at may be important I recommend that the paper be published in the Philosophical Transactions."

Joule's later work is so intimately connected with the determination of the mechanical equivalent of heat, that the importance of his investigations in other domains is apt to be overlooked. The two volumes of his published researches show that he was by no means a specialist, but only those who knew him personally are aware of the extent of his knowledge and broadness of interests ranging over nearly all branches of physics. He was a pupil of Dalton, who had refused to instruct him in chemistry before he had learned the elements of mathematics. It was perhaps in recollection of his first teacher of science that Joule once remarked to Balfour Stewart: "If I were a young man I would concentrate my attention on atomic weights." When I first became acquainted with Joule, he was a little more than sixty years of age and in full vigour. The meetings of the Manchester Literary and Philosophical Society in those days will always remain in the memory of those who were fortunate enough to attend them. It was the custom then, and I believe is still, to devote the first half-hour to a discussion on any subject brought forward by some member, spontaneously or at the invitation of the president. A regular attendant, Joule was at his best on these occasions. He also made his presence felt at the council meetings as a confirmed conservative opposed to all changes. His health began to fail about 1882, but in November 1885 he dined at my house to meet the late Lord Rayleigh, who had come to Manchester on purpose to make his acquaintance. "I believe I have done a few little things but nothing to make a fuss about," he said, shortly before his mental powers began to fail.

After Joule's death, I was asked by his family to examine his apparatus and instruments—mostly constructed by his own hands—and I was fortunate to rescue his historical thermometers, which were lying covered with dust in an old stable attached to his residence. I was thus enabled to determine the difference between the scale value of the thermometer used by Joule and that of the standard of the Bureau international des Poids et Mesures. It appeared in the investigation that the glass of Joule's thermometers is more suitable to its purpose than the glass afterwards employed in England. The depression of the zero after being raised to a definite temperature is much smaller and more nearly approaches that of the hard glass used by French makers. These thermometers were presented by Joule's son to the Manchester Literary and Philosophical Society: most of the remainder of Joule's apparatus is preserved in the Physical Laboratories of the University of Manchester.