

by its own fibres connected. Yet the consumers state it will not spin—a customary objection to anything new. More recently a similar import (about half a dozen bags of 70 lbs. each) came from the River Plate *via* Pernambuco. Any quantity can be had from the east side of the Andes and the plains of the Amazon. As to the staple of the cotton, it is very silky and short; but by grafting, or superior technical cultivation known to naturalists, it might no doubt be improved. Large quantities must be brought to market, and then machinery will be altered to suit its working, as was the case with alpaca, which has a silky fibre. He sold one bag of the Barrugudo cotton at 3*d.* per lb.; but, as the Yorkshire buyer did not accept delivery, the whole of the last lot was taken by the importer for stuffing sofa cushions and mixing in feather beds, instead of purchasing swandown at 12*s.* 6*d.* per lb. Here is a large field for the use of such fibres; and if brought to this country in large quantities, it must be mixed with cotton, like Mingo or devil's dust, or be spun up with sheep's wool. Through the kindness of Mr. M. J. Whitty, of the 'Liverpool Daily Post,' the writer was authorized to exhibit a sample of new fibre from the wild flax of North America. Millions of bales, he states, can be obtained at a cost of less than 4*d.* per lb., so profusely does the wild flax exist. These new fields ought to command attention when there is so much anxiety to increase the supply of cotton. The author contends that six million acres of land in Ireland can be had at a nominal rent, on which good cotton can be grown, the land never having been grazed, scratched, or nibbled by cattle.

On the Functions discharged by the Roots of Plants; and on a Violet peculiar to the Calamine Rocks in the neighbourhood of Aix-la-Chapelle. By PROFESSOR DAUBENY, LL.D., M.D., F.R.S.

This violet, although its petals are of a uniformly yellow colour so long as the roots are in contact with the zinc, seems to be a mere variety of the common *Viola lutea*, which has purple petals when it grows on ordinary soil; and accordingly, on the confines of the two strata, the petals of the plant are partly yellow and partly purple. The author made some further remarks upon the absorption of mineral bodies by the roots of plants, and in conclusion gave it as his opinion, that the selective power possessed by them indicated a force independent of any physical cause, and which he therefore regarded as of vital origin.

On the Influence exerted by Light on the Function of Plants.
By PROFESSOR DAUBENY, LL.D., M.D., F.R.S.

The author referred to certain principles established by him in a paper published in the 'Philosophical Transactions' for the year 1836, in which it was laid down, first, that the decomposition of carbonic acid and the consequent disengagement of oxygen was influenced by the luminous rays of the spectrum, and not by the calorific or actinic ones; secondly, that under particular circumstances nitrogen is emitted during sunlight from the leaves of plants; and thirdly, that other functions of plants, such as the greenness which the leaves assume, the peculiar property which belongs to certain ones, as to the sensitive plant, of collapsing on the application of stimuli, the exhalation of water from the leaves and its absorption by the roots, are probably dependent upon the same influence.

On the Method of Mr. Darwin in his Treatise on the Origin of Species.
By H. FAWCETT, M.A.

He said that, as he could not conform to what he believed was the rule, that communications should be read (Mr. Fawcett being blind), he would promise to keep as close to his subject as though he had written his paper. The title which he originally fixed upon was, "That the method of investigation pursued by Mr. Darwin, in his Treatise on the Origin of Species, is in strict accordance with the principles of logic." He feared that he might be charged with presumption in attempting to say anything on Mr. Darwin's great work, which had already engaged the attention of the most accomplished naturalists of the day. He had been

assured that the discussion on the subject at the last Meeting of the Association had never been surpassed in the interest it excited or in the talent which it called forth. Indeed, the work had divided the scientific world into two great sections; Darwinite and anti-Darwinite were almost the badges of opposite parties. Professor Owen, Professor Sedgwick, and Mr. Hopkins had given to the new theory a decided opposition; Sir Charles Lyell, Professor Huxley, and Dr. Hooker had given to it a support more or less decided. All who took an interest in the subject had a right to inquire whether the theory—whatever might be thought of its details—had been logically brought forward. The province of logic was not to discover new facts, but to decide whether facts were legitimately used to establish that which it was pretended they proved. It was constantly alleged that Mr. Darwin was illogical; that he had not followed the Baconian method. The 'Quarterly Review' assured us that Mr. Darwin had not followed in the steps of Newton and of Kepler; but nothing was more easy than to make such charges, which often only concealed pretentious assumptions of scientific knowledge. It was more pertinent to inquire—What is the method of solution of which such a problem admits? He insisted that if ever solved it could only be by a method analogous to that attempted by Mr. Darwin. It could only be solved in this way:—An hypothesis, resting upon more or less perfect induction, must be started; from that hypothesis certain deductions must be drawn; these deductions must be tried, by seeing whether they would explain the phenomena of nature, and they must be verified by seeing whether they agreed with what can be observed in nature. If this explanation and verification was complete, the hypothesis was advanced from an unproved to the position of a proved and established theory. The Bishop of Oxford last year said that the theory was so absurd that no scientific man could for a moment think that it was in any degree worth considering. But Dr. Hooker, than whom a more eminent authority could not be quoted, at once disposed of the Bishop by saying, that as he believed the theory worth considering, he ought to "apologise for addressing the meeting as a man of no scientific authority." Dr. Hooker added that he knew of the theory five years before; that, at first, no one more opposed it; but five years' devotion to natural history had convinced him that the theory was worthy of the most careful consideration and examination. Mr. Darwin, with the most perfect candour, explained in his work that his theory did not yet explain all the facts of nature; but it must not be supposed that his twenty years' labour had done nothing to advance the ends of science. Mr. Darwin had strictly followed the rules of the deductive method as laid down by John Stewart Mill. When Kepler inferred his law of the connexion between the major axis of the planets and the times of their revolution, he so inferred from observation, which he could strictly verify by mathematical calculation. The origin of species does not admit of such a verification. In chemistry there was much more power of proof or verification by experiment than was possible in physiology; so with other sciences. When laws of nature cannot be discovered by experiment, we are obliged to go to deductive reasoning. Newton had only an hypothesis, and not a theory, as to the law of gravitation; the law he first tried was an incorrect one. He tried again; and then, as Professor Whewell said, by a tentative process he discovered the correct law. Mr. Darwin had told him (the speaker) that his hypothesis was not at once suggested to him. He found in his studies that there was something wanted to explain many of the observed phenomena; years passed, and at length his hypothesis was very indirectly suggested—for he said that it came from reading Malthus's 'Essay on Population.' Twenty years of unremitting labour he had devoted to the endeavour to verify the conclusions which might be deduced from this hypothesis by the facts observable in nature. He believed that Mr. Darwin's second work, for which the author had accumulated a great mass of knowledge, would prove beyond doubt that no one could have been a more conscientious or laborious observer than he had been. Newton could verify his hypothesis by the simplest experiment—he had but to drop a stone from a tower and to note the time occupied in its descent. But the problem of the origin of species is concerned with an epoch of time associated with geological epochs; therefore experiment could only be made during so short a time, that nothing more could be obtained than an argument resting on a, comparatively speaking, unsatisfactory analogy. Darwin had been able to show that by a system of artificial natural selec-

tion two organisms, originally descending from the same form, could be made to differ so much, that if they were found as fossils they would undoubtedly be classed as distinct species; and, therefore, how a morphological species could be produced. But his experiments had failed to show how a physiological species could be produced; for no one could show that two varieties from the same form could be made to differ so much that they would possess the quality of infertility. This was too often forgotten by objectors. The Egyptian sculptures were pointed to to prove that during 3000 years the causes looked to by Darwin had done nothing to alter the form of animals. But what would be said to him who, by discovering that 3000 years ago Mont Blanc was of the same altitude as now, should think that he had thus disposed of the theories of modern geology, that the stupendous peaks of Switzerland were lifted from their ocean bed, and that every change on the surface of the earth had been produced by an indefinite continuation of physical causes which are in ceaseless operation? Mr. Darwin admitted that geology did not show that in animal life there had been those transitional links that ought to exist according to his theory, and according to any other of gradual transmutation. He (the author) could not see that this theory detracted one iota from any of the attributes of the Creator. If we suppose that the introduction of every new species required a distinctive act of creative will, then, of course, the Creator must have interposed every time a new species was introduced. But, if we supposed that every living organism has descended from those forms in which life was first placed upon this planet, it does not in the slightest degree dispense with the necessity of supposing that life could only first be so placed by the act of Omnipotent Creative Will. It was a favourite illustration in religious works, the discovery of Newton which explains how planetary motions are produced; and he (Mr. Fawcett) believed that if ever the day came when the origin of species should be explained in fulness and simplicity, he who so explained it would be considered not only to have advanced science, but to have conferred a benefit upon religion. The attackers of Darwin forget that he has not attempted to displace a theory received as right, but merely to throw some light where all before was dark. We should, therefore, be all the more ready to welcome the conscientious labours of one who like Mr. Darwin had unremittingly devoted himself to explain to some extent what had been aptly termed the "mystery of mysteries."

On the Arrest of Pupal Metamorphosis of Vanessa Antiopa or Camberwell Beauty. By GEORGE D. GIBB, M.D., M.A., F.G.S.

After making a few remarks upon deformities and arrest of development amongst the insect tribe, the author proceeded to describe some examples occurring in the *Vanessa Antiopa*, which were exhibited to the Section. Of twenty-eight specimens which he had obtained in the month of July, all underwent complete metamorphosis, with three exceptions. These to some extent illustrated the progress of the process of emersion of the imago from the pupa-case.

In the first specimen, the first stage of emersion was accomplished, *i. e.* a part of the wings had protruded from each lateral fissure throughout its whole length to the extent of $\frac{3}{16}$ ths of an inch, permitting a view of the anterior part of the thorax. Metamorphosis then became arrested, and existence terminated.

In the second example emersion was more advanced; the left wings had emerged a $\frac{1}{4}$ of an inch only, whilst the right almost wholly protruded, but remained in contact with one another. The puparial case is on the point of freedom, and the lower part of its abdominal segment is empty. Here further metamorphosis became arrested, and life ceased.

In the third, emersion was complete; metamorphosis, however, was not so, and it was associated with malformation. The right anterior wing was fully expanded, whilst the posterior was crumpled up. The left anterior wing was almost wholly wanting; a mere rudimentary appendage existed two lines long. The left posterior wing was only partly expanded posteriorly, the remainder being crumpled up.

The author entered into the probable causes of these arrests of change and development, and believed that they did not depend upon injury, from the care taken when the chrysalides were first collected.