For examples of that spirit which makes controversy ennobling alike to head and heart, we must turn to the great leaders of thought in the present age. The following paragraph from a notice of Mivart's "Genesis of Species" in the *Liberal Christian* is the best thing we have seen in that paper for many a long month :—

"Meanwhile, what an example to theologians and historians and literary critics does not the spirit of the great writers in science of our day set! In respect of courtesy, candor, the single love of truth, the exercise of magnanimity toward competitors, the grateful sense of others' services, we know nothing in professedly Christian writers superior—might we not say equal?—to what is exhibited uniformly in Lyell, Huxley, Darwin, Wallace, Mivart. Indeed, the moral graces have rarely been so beautifully exhibited in the heat of honest rivalry as by the whole class of English physicists of this generation. Darwin is the very Bayard of chivalrous honor and deference in his scientific writings. Wallace is a Sydney, and Mivart a knight *sans peur et sans reproche.* These men, differing greatly, earnestly, manfully, never stoop to injustice or any arguments *ad invidiam.* They deal in no side looks at the public, like bad actors coquetting with the pit. They write on conscience, in the love of truth, in the fear only of doing each other wrong. Let alone Mr. Darwin's ethics or religion; make them theoretically what you will, he practises the highest religious principles and exhibits the most difficult Christian graces in his ever tempting and exciting position as the head of a school which owes its importance to the sustained originality of his genius and the fortification of his cardinal doctrine. But he would evidently die sooner than willingly deceive as to a fact or deny another man's rights in discovery. When have theologians exhibited as much candor and love of truth? How will the *odium theologicum* bear comparison with the loves of these scientists, from whose honest researches clergymen commonly shrink as though 'their craft were in danger?' Such a temper can have in it no possible fruits of evil or danger to true religion."

June 16 1875

THE BIRMINGHAM DAILY POST

INSECTIVOROUS PLANTS.

A paper on "Insectivorous Plants," was read to the members of the Natural History and Microscopical Society, Midland Institute, last evening, by Mr. Lawson Tait, vice-president of the society. There was a good attendance.

Mr. TAIT said: The fact that certain plants had mechanical contrivances for the capture of insects had long been known, but its real bearing on comparative physiology was not recognised until Mr. Darwin brought to bear on it the effect of his marvellous powers of patient investigation and wide generalisation. The subject which they had to deal with that evening was another field, which promised another scientific victory, the testing of another of those missing links which were constantly presenting themselves, and which were always found to fit in somewhere so well. Mr. Darwin had made them slightly acquainted with some of his results, but they would not appear in full for some weeks. What he (Mr. Tait) had to say on the present occasion was chiefly the result of his own observation and experiment, and he ventured to communicate it at the risk, perhaps, of finding himself anticipated by Mr. Darwin, or of being subject to his correction. There were five classes of plants which possessed, or had been supposed to possess, arrangements for the capture of insects, of which they might accept four; but it was in the interesting class of the *droseraceæ* that the most undoubted examples were found. As in all other physiological matters there were great differences in degree, and it must not be supposed that every fly-trap was a fly digester; still less must it be taken for granted, as it has been too readily in the case of the *sarracenia*, that fly digestion must necessarily mean absorption of the products. In fact, direct absorption of the products by the leaves was so hypothetical that he was inclined to disregard it altogether. He knew Mr. Darwin was inclined to accept it, but he did not know his grounds. The *droseraceæ* were a widely scattered family, inhabiting all climates, especially frequent in Australia, equatorial America, and South Africa. They knew only three varieties in this country, the *drosera rotundifolia*, *drosera Anglica*, and *drosera intermedia*, with perhaps a doubtful fourth, named *drosera obovata*, which was probably an hybrid between the *rotund-folia* and *Anglica*. The British *droseraceæ* were small and rather fragile herbs, often stemless, having the leaves on a rosette, or with elongated stems, having alternate or whorled leaves. These variations seemed due to the condition of growth. The flowers were regular on a raceme, and the leaves were covered with hairs tipped with glands. The English name "sundew," as well as the Greek appellative "*drosera*," were descriptive, as under certain conditions the leaves appeared as if wet with dew, though the statement that they were always wet, even in the hottest sunshine, was not quite accurate. The habitat of the plant was always wet and associated with the growth of peat, and it seemed to prefer a spot where an edge of peat overhung it, so that it was not exposed to the sun's rays directly; and it certainly was under such conditions that its peculiar physiological functions were most active. Withering told them that Mr. Whateley, a surgeon, of London, first noticed that the folded leaves of the D. rotundifolia all contained dead insects so long ago as August, 1780, but he thought the merit of the first accurate observation on the matter must be given to a Bremen naturalist, named Roth, who published in 1782 a paper, "Von der Reizbarkeit der Blätter des sogenannten sonnenthaues." Withering's reference to Whateley was not published for eight or nine years after, and the way in which it was done bore evidence that it was an attempt to claim credit for an observation made and previously published by another. Further, Mr. Whateley's own letter settled the matter, for he stated that he noticed contraction of the leaves when irritated with a pin. If that were so the D. rotundifolia must have altered its character and conduct considerably in 95 years, for no such contraction was to be seen now. His (Mr. Tait's) own observations completely confirmed those of Mr. Bennett on the matter, for nothing short of the natural food of the plant, or at least something closely resembling it, would induce the leaf to close. He preferred to think that Mr. Whateley was mistaken, and that his observation was accidental and purposeless, while Roth's was scientific and discriminate. The word which Roth used, "irritability," was that which, to cover their ignorance of the exact nature of the process, they were compelled still to employ, even though it must be admitted that mere irritability would not account for the whole of the phenomena. The leaves of the sundew had no difference in structure from that of other leaves, save that there was a great abundance of regularly distributed spiral vascular tissue, extending from the leaf stalk to the leaf and thence into each of the glandular hairs. Indeed, the latter fact was the great peculiarity of the plant, for in that particular the hairs differed so far as he knew from the hairs seen in the leaves of plants belonging to any other natural orders. In fact it removed them entirely from the category of hairs, and placed them apart by themselves, and he preferred to call them "gland-tipped fingers." In the saxifrages and in pinguicula the glands were set on simple epithelial hairs, into which no vascular tissue entered; in fact, they were composed of single epithelial cells placed end to end. In the leaf of the drosera the bundle of spiral vessels could readily be seen leaving the tissue of the body of the leaf, and could be traced up to the very centre of the gland. Throughout its course in the finger it was covered with ordinary cellular tissue, the cells elongated and mingled with air spaces and covered with chlorophyll containing epithilium, on which prominent stomata were scattered. In the more mature condition of the plant, especially if it were exposed to the sun, the epithilium of the fingers formed erythrophyll, and then the leaf seemed less active. The glands were composed of epithilium set into some cellulose on the fibro-vascular bundle, and between the cells were inter-cellular spaces, which might be absorbent canals. The glands and fingers on the leaves of the D. rotundifolia seemed to be divided into two sets, one of which he termed central and the other fringe. The central fingers were shorter than the fringe fingers, and they very rarely contained erythrophyll; but there seemed to be an important difference between the functions of the two. Thus the presence of moisture round the central glands was much more apparent than in the fringe glands, the former alone seeming to act as the [illegible] for the [illegible]. He had not often seen an insect caught in the fringe glands. The central fingers did not bend much, while it would be seen that the fringe fingers closed completely over the victim, and that after they had done so, and then only, certain very interesting foldings of the leaf took place. If under favourable circumstances an insect were placed living in the central glands of a mature leaf, it would adhere to those with which it came in contact, probably drawing some more to it by its struggles. If the leaf were watched from time to time through a lens, it would be found that the amount of viscous secretion in the central glands would increase in quantity in a few hours, and that many of the fingers in the immediate neighbourhood of the fly would bend towards it. As the glands approached one another the secretions touched, and three or four little drops would be seen to unite into one larger, and by the time they reached the fly the quantity became quite considerable. In about twelve hours the fly would be found to be dead, and completely saturated with the moisture from the glands. The death of the insect was due to this secretion, because it would remain living for an almost indefinite time if the glands were amputated, and it was secured in its place by a little gum. In fact, amputation of these glands, without any injury to the body of the leaf or to the fringe glands, completely stopped the whole process, while it was very materially hastened if the fly were killed before it was placed in the leaf. On the contrary, if the fringe glands were amputated and the central glands left intact the insect was wetted and killed, though not so rapidly as when the leaf remained uninjured. The next action consisted in the gradual bending inwards first of a few and then the whole of the fringe glands if the object was large enough to require it. For it was one of the most interesting facts of the whole enquiry that if the insect was very small, and was caught in the glands near the margin of the leaf, only those fringe fingers in its immediate neighbourhood closed over it. Nothing short of some rudimentary form of nervous system seemed to him enough to account for this action, and though he could recognise no structure in the leaf which he could identify as such, he thought it was not impossible that the fibro-vascular bundle might have such a function. The closing of the fingers generally took from twelve to thirty-six hours. After it was accomplished a set of movements took place affecting the body of the leaf. The upper and outer margin curved forward and inwards over the fly, so that the latter was embraced within the leaf, and evaporation was prevented. If the leaf was very active, still another movement took place, consisting of the bending of the leaf forward and downward, so that the back of the leaf was turned upwards, and the incurved face downwards. This condition lasted for two or three days, and then the folds gradually widened, and the dried remains of the insect dropped off. He also described the action in the D. binata, and in the leaves of the saxifraga tridactylites, the D. intermedia, the dionæa muscipula. The latter beautiful fly-traps had recently been the subject of investigation by Dr. J. B. Sanderson. In describing this Mr. Tait said that when the leaf was hungry the lobes were widely separated, and the surface was seen to be speckled with red dots. This was due to the erythrophyll of numerous glands exactly the same in construction as those already described in the D. rotundifolia, but without fingers. They were not wet while the leaf was open, and the fluid they secreted when in action was not viscid. On these points he knew that he was not in harmony with other observers, but he was very positive about them. Besides these glands three fibres almost colourless might be seen regularly placed on the inner surface of the lobe. They consisted of substance of the leaf, and were the sensitive fibres. They were not hairs. Stimulation of the fibres induced closure of the trap in a few minutes, and when a large blue bottle fly was put into the trap it closed almost instantaneously, and with each struggle of the fly spasmodic efforts on the part of the leaf to close more tightly might be seen. The closing of the lobes took place quite independently of the temperature or nature of the substance used to stimulate, so that it was not due to any communicated current. The method of closure was curious, for it did not depend merely upon an influence exerted at the base of the lobes where they joined, for it began by the bending downwards of the marginal spikes, and the incurvation of the whole margin of the lobe, so that its inner surface, previously almost plane, became in a great measure convex. He had found that too large a meal was detrimental to the leaf, and of course for a very small one. But after some hours the contracting force of the leaf seemed to be re-exerted very slowly but very powerfully, for the spikes were straightened, the incurvation of the leaf greatly diminished, and the lips of the trap brought accurately together, so that evaporation of the contents was impossible. This force was unquestionably exercised at the hinge. By this time secretion had begun and the insect was killed. There was a large number of most important questions in connection with these wonderful movements, which required most careful working out. Some of these Mr. Darwin was engaged upon, and some he (Mr. Tait) had taken up, but none as yet were completely answered, save the one undertaken by Dr. Sanderson—namely, "What is the electrical condition of the leaf in relation to these movements?" He quoted from the results of Dr. Sanderson's investigations as published in the proceedings of the Royal Society. Dr. Sanderson's observations showed completely that the currents in the dionæa were exactly similar in the laws to which they were subject to the currents in animal muscle and nerve; for when the leaf-stalk was placed in the electrodes the galvanometer indicated the existence of a current opposed in direction to that of the leaf, showing that the electrical conditions in opposite sides of the joint between stalk and leaf were antagonistic to each other. Consequently, so long as leaf and stalk were united each prevented or diminished the manifestation of the electro-motive force by the other. This was completely in accordance with what was observed with reference to nerve. He was enabled by accident to make observation which was singularly suggestive of the absoluteness of the analogy. He was engaged one day repeating Dr. Sanderson's experiments on a dionæa, when he was summoned away to a distance, and on his return he found the plant attached to the electrodes in a hot drawing room, exposed to the full glare of the sun, and the plant seemed in a dying state. To try if it were dead he touched the sensitive fibres of the leaves that were open, and found that irritation of the fibres of one lobe produced no effect on it, but caused incurvation of the margin of the opposite lobe, which finally advanced to the median line. This was precisely the same thing as reflex action, and the case of the damaged dionæa leaf was identical with that of a man who had had his back broken, and the spinal cord severed in the injury. Looking at the phenomena, he was not at all clear that they were in a position to deny consciousness to the dionæa and drosera. When he related these curious results to Mr. Darwin, that gentleman replied to the effect that he had noticed similar results. It was singular that Mr. Darwin should have noticed the same fact as he (Mr. Tait) had done, and it was natural that he should attribute it to the injuries he had inflicted; but as in his case the injury was universal, his explanation was not applicable, while on the other hand his (Mr. Tait's) explanation of a true reflex action, and therefore the existence of a definite nervous system, seemed to be so. That conclusion, however, still wanted a great deal to make it substantial. The insect coming in contact with the hairs, the leaf closed, and the process of digestion began. The first step was the closing of the lips of the trap, as he had already described; the next was the pouring forth by the glands of an abundant secretion, which ultimately filled the cavity of the trap. There did not seem to be more than one system of glands in the dionæa for the reason that the closure was enough to kill the animal. He therefore regarded the glands in this case as equivalent to the fringed glands of the sundew. With regard to the acidity of the fluid secreted he thought the question required further investigation. In the secretions, however, there was one of those peculiar ferments which had such strange actions on organic substances. He had not been able to procure the secretion of the dionæa in sufficient quantity to analyse it, but from the viscid secretion of the drosera binata or dichotoma he had been able to separate a substance which he proposed to call *droserin*, and which had some most remarkable properties resembling pepsin, but differing very materially from it. That this droserin had a solvent action on insects might be seen by anyone who tried the experiment. It did not completely dissolve the insect, but neither did the human gastric juice completely dissolve the food, but they both took the "goodness" out of what they are applied to. Droserin was specially applicable to insects, and was not intended for general diet, for raw mutton, cooked beef, fish, potato, bread, and a host of other things which he tried were all swallowed by the leaves, but they were not digested, and seemed to do the plants harm. Cheese positively killed them. They were most completely taken in with dry sponge, and tried hard to get it over, but it only served him (Mr. Tait) as it served Dr. Beaumont in the case of Alexis St. Martin, it enabled him to gather the gastric juice. What became of the products of digestion was one of the problems still unsolved, and on this point Mr. Darwin and he differed. Mr. Darwin was of opinion that the leaves absorbed the products of digestion, and he (Mr. Tait) thought so at first, but he had failed to find any evidence of absorption by the surface of the leaves. On the other hand, his experiments tended to show that the products of digestion ran down the leaf stalk to the roots, and were there absorbed as manure was. The physiological interest of the whole matter was that there they had a number of plants possessing a function, that of gastric digestion, which had been supposed to be exclusively the property of the animal kingdom, and some of them had further something very like a rudimentary nervous system. In fact, it must be concluded that plants were nearer relatives of our own than we had ever yet believed.

There were exhibited fine specimens of *Cristatella plumosa*, which has lately been plentifully distributed by the Waterworks Company through their mains. The newly-discovered animal *Bucephalus polymorphus* was also exhibited. A specimen of a young pup, perfectly formed in all but the head, having no eyes, no nose, no mouth, was exhibited by the secretary.

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