ourselves can testify, forms a thoroughly comfortable and secure bed or lounge. Mr. Stanley, we believe, was so favourably impressed with the hammock, that he has taken a supply with him in his present exploration; and for explorers in tropical countries, we should think it would prove useful in many ways, as it can not only be used as a bed, but, mounted on a pole, as a travelling litter or palanquin. For those of our readers engaged in explorations of any kind, geological, geographical, botanical, zoological, or even in doing an ordinary tour, in remote districts, we

ASHANTEE HAMMOCK ON SLINGING APPARATUS.

believe the hammock would be found of real service, as it would make them quite independent of sleeping accommodation, and would not increase the weight of their *impedimenta* by very many ounces. An idea of its construction and its adaptability to almost any circumstances may be obtained from the illustrations we give. We can honestly recommend the hammock as likely to answer all the purposes for which it has been designed.

THE SANITARY INSTITUTE

THE lecture by Dr. Richardson, published in our issue of last week, has called public attention to the Sanitary Institute of Great Britain, before which the lecture was delivered. The Institute was founded in July, 1876, at a public meeting held at St. James's Hall, and presided over by his Grace the Duke of Northumberland. The Institute has for its work a wide range of subjects. It has sprung, we may say, out of the necessities of the time, and in the first instance may be considered as a nucleus round which will cluster the many men of science who are now employed in carrying out the executive sanitary or health work of the kingdom. The various medical officers of health, the certifying surgeons under the Factory Acts, the engineers and sanitary surveyors of different localities, the mayors of municipalities, and the chairmen and presidents of local boards, all of these must needs take an interest in and in time form the body corporate of an institution framed for the purpose of becoming as it were a voluntary health parliament. In addition to these sections of the Institute there are many other sections of the community which will, we should think, earnestly join in the work. For reasons plainly stated by Dr. Richardson ladies are invited to take part in the proceedings and to help forward sanitary progress. We feel sure there will also be a large class of active men unconnected professionally with sanitary work who will be ambitious to take a part in the great practical scientific labour of the time, the only labour we may say in which science lends herself immediately to the aid and comfort of domestic life and felicity.

The detailed work of the Sanitary Institute has been in some measure projected by its founders; but it is more than probable that in the course of its natural development it will grow into something different from that which is now supposed. At the same time we are bound to say that the plan is sufficiently simple and practical to warrant the expectations of those who have mapped it out. The objects we have seen proposed are all directed to some useful and desirable end. To obtain a registration of the diseases of the kingdom; to establish communications with medical officers of health; to form local branches of the Institute throughout the kingdom; to examine and grant certificates of qualification to local surveyors and inspectors of nuisances, and to form a register of such certificated officers; to investigate the chemical aspects of the sewage question; to establish a sanitary exhibition, and to form a library of books on health subjects;—these objects, some of which must needs become a part of every sanitary organisation, are sufficiently comprehensive to cover any amount of work, and to tax any amount of industry that may be found in the best organised public body. So far the prospects of the Institute are brought beyond what is common to such undertakings in their earliest days. Members are daily being added, and an effective Council has been elected. Already one of the provincial towns, Scarborough, has invited the Institute to hold its first provincial Congress there, and in France a kindred society has been formed in sequence, and, it may be said without offence, in imitation of the one already founded in London. The visit of Dr. De Pietra Santa, of Marié Davy, and other savants from Paris to the meeting on Thursday last, is a significant sign of the good feeling with which the two rival societies have commenced their labours.

For our parts we welcome heartily both Institutes, and shall enjoy the privilege of watching their onward progress and recording their success.

ON THE SOURCE OF THE CARBON OF PLANTS

NEARLY half the dry substance of plants is carbon; and it is conclusively established that they derive, at any rate, the greater part of it, directly from the carbon-dioxide of the atmosphere, which the chlorophyll cells have the power of decomposing in sunlight, at the same time evolving oxygen. But this function of vegetation, which is so essential a complement to the processes of animal life, gives rise to many problems hitherto unsolved; and an important one is whether or not plants avail themselves of other obviously possible sources of carbon than that existing in such very small proportion, although in large actual amount, in the ambiant air.

Our knowledge bearing upon the subject as it exists in the present day, is the resultant of careful investigations by many observers. In the last century Bonnet discovered the gaseous exhalation; Priestley that the gas is oxygen; Ingenhouz that the oxygen is only evolved in sunlight; Sennebier that it is due to the decomposition of carbon-dioxide, but he believed that the carbon-dioxide is taken up in solution in water. Early in this century de Saussure carried out a long series of experiments on the relations between the carbon-dioxide decomposed, and the oxygen evolved, and on the amount of carbon-dioxide in the air compatible with the healthy development of plants. Since his time many eminent names have been added to the list of patient labourers in this field of inquiry.

Boussingault worked on the question whether the carbon-dioxide is absorbed by the leaves, or taken up by water through the roots; and by direct experiments proved that the leaves of plants do take up the carbon-dioxide, which is so sparingly, though so uniformly, diffused in the atmosphere. His researches led him to conclude that, by far the greater part, if not the whole, of the carbon which enters into the constitution of the organs of plants is derived from atmospheric carbon-dioxide; and while drawing attention to the fact that, for healthy and vigorous action, plants require large volumes of air to pass over them, and to the surprising rapidity with which they absorb the carbon-dioxide from it, he makes calculations as to the surface presented to the air by the leaves of different crops. Taking the average number of plants growing per hectare (about $2\frac{1}{2}$ English acres), he estimates that:—

Artichoke	gives a surface	of	142,410	square	metres.
Beetroot	,,	,,	49,921	,,	,,
Potato	,,	,,	39,641	,,	,,
Wheat	,,	,,	35,490	,,	,,

Boussingault also made experiments in regard to the absorption of carbon-dioxide by plants growing under different conditions as to soil and manures. He found that a Helianthus which in twenty-four hours would, without any manure, only decompose 2 c.c. of carbon-dioxide, decomposed 182 c.c. in the same time when supplied with manure containing nitrates and phosphates, 11 c.c. when with nitrates without phosphates, and only from 3 to 6 c.c. when manured with phosphates without nitrates.

That the carbon-dioxide contained in the atmosphere is sufficient for normal vegetation is proved by the abundant growth of heath and other wild plants on sandy hills; and the numerous experiments on water-culture conclusively show that a plant may grow luxuriantly, and store up an abundance of carbon, when supplied only with mineral salts, in a solution which contains little or no carbon-dioxide.

Sachs speaks of it as an unquestionable fact, "that most plants which contain chlorophyll (for instance, our cereal crops, beans, tobacco, sun-flower, &c.) obtain the entire quantity of their carbon by the decomposition of atmospheric carbon-dioxide, and require for their nutrition no other carbon-compound from without." He goes on to say: "The compound of carbon originally present on the earth is the dioxide, and the only abundantly active cause of its decomposition and of the combination of carbon with the elements of water is the cell containing chlorophyll. Hence all compounds of carbon of this kind, whether found in animals or in plants or in the products of their decomposition, are derived indirectly from the organs of plants which contain chlorophyll."

Dr. J. Boehm made direct experiments with seedlings of scarlet-runner, growing them under glass shades, luted with potass lye, in pots containing in some cases quartz sand moistened with a nutritive solution, and in others garden-soil rich in humus. The two sets were quite equal in development and duration of life; those in the garden soil formed quite as little starch as those in the sand; and from this he concluded that the carbon dioxide yielded by the garden soil had taken no share in the growth of the plants.

Liebig had, however, supposed that plants might owe some part of their carbon to the carbon-compounds in the soil, which were absorbed by their roots, and that young plants especially drew their supply from this source. He speaks of the effect of drought as checking the supply of carbon-dioxide by the roots, and throwing the plant exclusively upon that in the air.

But the tendency of more recent investigations points to the conclusion that the atmosphere and the parts of plants living in it are solely concerned in the storing up of the carbon of vegetation.

We may pause for a moment to consider the amount of the carbon so stored up.

Liebig estimated that more than 1,000 lbs. of carbon may be harvested annually from a Morgen of surface—somewhat less than two-thirds of an English acre.

According to the estimates of Lawes and Gilbert, with wheat for twenty years in succession on the same land there was an actual yield of 2,500 lbs. of carbon, per acre, per annum, where no organic carbon compounds were added to the soil, and where these were added (in the form of farm-yard manure) the actual yield in carbon was less. With barley, for twenty years in succession, the average annual yield was 2,088 lbs. of carbon per acre; and the indication is that some other crops, under similar conditions, acquire even more.

Estimates recently made of the forest growth in Germany give as much as 2,700 lbs. In tropical climates where vegetable growth is more luxuriant the amounts are far greater; and in the West India Islands as much as from $2\frac{1}{2}$ to 5 tons of carbon may be harvested per acre in the crop of sugar-cane.

With these large amounts of accumulation on the one hand, we have, on the other, an atmosphere containing carbon-dioxide in so small a proportion as 0·04 per cent.

Then we have to bear in mind the large supplies of carbon-dioxide within the pores especially of manured soils, as determined by Boussingault, and at the disposal of the roots of plants. Also the enormous quantity of water taken up from the soil and passing through plants during growth, probably at any rate more than 200 parts for every part of dry substance fixed, and the fact that carbon-dioxide is present in all natural waters would lead to the supposition that the roots would scarcely either take it up to no purpose, or act as a filter to that which constitutes so important a requirement of the plant.

Dr. Moll[1] has recently, by some interesting experiments, made a contribution to the evidence which is required to answer the question—Can leaves decompose the carbon-dioxide which is at the disposition of the roots? and argues that the proof that one part of the plant—the leaf—takes up and decomposes carbon-dioxide, is no proof that it is not taken up in another part—the root.

He quotes the experiments of Sennebier and de Saussure, but considers that they were not made quantitatively, or with sufficient exactness to solve this problem. For its elucidation he rests his methods upon Sach's theory, that the starch in the chlorophyll grains must be considered as the first visible product of the decomposition of carbon-dioxide, and that therefore, according to him, the presence or absence of starch in the leaves is the crucial test of the decomposition or non-decomposition of carbon-dioxide. In Dr. Moll's investigation of the starch contents he used Sach's modification of Boehm's method.

Five sets of experiments were made to meet the different aspects of the question.

In the first set glass shades were used, in one of which the air was kept free from carbon-dioxide by being luted with potass lye, while the other contained ordinary air, or air with an excess of carbon-dioxide, and was luted with water. The liquid lute was in porcelain dishes, made with a round hole in the middle; the central hole and outer edge being deeply rimmed. The shades, of less circumference than the dishes, were set in them, and were furnished with tubular necks, into which smaller tubes were fixed for the current of air to pass through, and for other requirements of the experiments. The exit tube of the shade in which the atmosphere was kept free from carbon-dioxide was conducted through a test-tube filled with pieces of pumice saturated with potass lye. Preliminary experiments with etiolated plants, with a watch glass containing baryta-water within the shade, satisfied the author that he secured having air absolutely free from carbon-dioxide under that luted with potass lye; and some early failures taught him how to regulate the supply of carbon-dioxide and air in the other shade, so as to grow plants as well-developed and healthy as those in the open air. With thick-leaved plants he found that it was necessary to add as much as 2 per cent. of carbon-dioxide to a volume of air supplied to them of about 2,500 c.c. daily, in order to satisfy their requirements for free growth.

Experiments were made with plants of French bean, nasturtium, gourd, and sugar-beet, growing in the open air in pots in good garden soil. From these was selected a leaf, or the upper part of a stem with several leaves, still organically united with the parent plant, which was passed through the hole in the porcelain dish, under the glass shade, and carefully secured air-tight, and from injury to itself, by cork and wadding. The plants for comparison were as nearly alike as possible in every respect, and a control plant grew in the open air between the shades. Both etiolated seedlings, which became green as quickly without carbon-

[1] "Ueber den Ursprung des Kohlenstoffs der Pflanzen." Von Dr. J. W. Moll (Utrecht)—*Landwirthschaftliche Jahrbücher*, Band vi. Heft 2.

dioxide as in common air, and well developed green gourd leaves, were tried. The gourd leaves, which contained starch at the beginning, entirely lost it within a day or two in the atmosphere deprived of carbon-dioxide, while those in the other shade remained still full of it. The shades, and the contents of the dishes, were then changed, so as to bring the starchless leaves into the shade containing carbon-dioxide. During the day these became again full of starch; while within twenty-four hours it had quite disappeared from the leaves in the other shade. In a similar experiment with sugar-beet the control plant in the open air was covered with a black paste-board box, and it was found that the leaves in the shade deprived of carbon-dioxide lost their starch at about the same rate as those in the dark. In no case was starch found in the leaves while they remained in an atmosphere without carbon-dioxide.

The second set of experiments was made with long leaves of bulrush and bur-reeds, which were etiolated, and then separated from the plants. With the same general precautions as before, the upper end of the leaf was inserted in the shade without carbon-dioxide, the lower in an atmosphere containing five per cent. of carbon-dioxide, whilst the space between was left free to the open air. This intermediate part was obscured by tin-foil, so that no starch could be formed in it at the expense of any carbon-dioxide passing through the tissues from the lower shade; and it was supposed that if such a phenomenon were possible, the spacious longitudinal air channels of these plants might be especially favourable to the transmission of the gas. These experiments usually lasted one day, and uniformly gave the same result; starch was formed abundantly where carbon-dioxide was at disposal in the air, while the excess of it in the lower shade had no effect upon the portion of leaf in the upper shade, which remained entirely free from starch.

The apparatus when arranged was always placed in a light window, shaded by gauze blinds if the sun were too hot; and in these latter experiments it was an interesting circumstance that, in the lower portions of these rather thick leaves, more starch was formed on the side next to the window; therefore, in two cases a piece of looking-glass was placed behind the shade, when, being equally illuminated, starch was formed in equal abundance on both sides of the leaf. This variation in the starch-formation, according to the amount of light, showed that that portion of leaf had not always used all the carbon-dioxide at its disposal, and that consequently there was an excess which might have passed upwards through the tissues.

The third set varied from these in having no part of the leaf exposed to free air, thus obviating the possibility of the carbon-dioxide being diffused into it in passing upwards through the plant. A glass vessel containing air without carbon-dioxide was placed within a large shade containing air with 5 per cent. of this gas; and a previously etiolated leaf, with its stem in water, was so fixed as to be partly in the one and partly in the other. After six or eight hours it was examined for starch. Without exception starch was formed abundantly in the parts in the large shade, whilst no trace of it was found in those in the inner vessel even quite close to the junction between the two.

The remaining two sets of experiments were made to ascertain whether starch formation in leaves, in the open air, is accelerated by giving an excess of carbon-dioxide, either to adjoining parts of the leaves themselves, or to the roots. In the first case leaves separated from the plant were divided lengthways. One half, with the stalk in water, was in a shade with air containing 5 per cent. of carbon-dioxide, its upper part projecting under the glass lid of the shade, which was luted with grease, into the open air. The other half of the same leaf was laid on the lid, on filter paper soaked with boiled water to keep it moist, and put as near as possible to the projecting piece of leaf. In the other cases etiolated leaves, organically united with plants whose roots were in rich humus soil, were divided lengthways; one half, quite cut off, was laid near to the other, and the two were examined and compared after some hours' exposure in sunlight. The results of both these sets of experiments were uniformly the same; careful examination showed that starch was formed as readily and plentifully in those portions of leaves excluded from any other source of carbon-dioxide than that in the air surrounding them, as in those having an excess of it at command.

From these experiments Dr. Moll concludes that starch is never formed in leaves in an atmosphere deprived of carbon-dioxide, however much of it may be at the disposal of the other, under- or above-ground, parts of the plant; nor can starch-formation be accelerated in one part of a leaf by an excess of carbon-dioxide being at the disposal of another part of it, either in the air, or through the roots.

The results of these elaborate experiments are doubtless in accordance with the direction of those of other modern inquirers on this subject. At the same time it will probably be felt, that, when long-accepted opinions, which many well-known facts seem to favour, are held to be called in question, we may still ask for further confirmation, before accepting as decisive, conclusions depending on the exact interpretation of experiments made with living organisms exposed to somewhat artificial conditions. It may be hoped, however, that this further instalment of evidence in a given sense will incite to further research.

OUR ASTRONOMICAL COLUMN

DE VICO'S COMET OF SHORT PERIOD.—It has been already remarked in this column that, according to Prof. Brünnow's last investigations relative to this comet, it appears necessary to admit a very material degree of uncertainty in the value of the mean motion determined from the observations of the year 1844, notwithstanding the comet was discovered on August 22, and followed till December 31, or for a period of more than four months, and, moreover, was observed with a degree of precision which has seldom been attained with these bodies. In Prof. Brünnow's masterly and elaborate discussion, "Mémoire sur la Comète elliptique de De Vico," which gained the prize offered by the Royal Institute of the Netherlands, in June, 1848, the planetary perturbations were calculated to the epoch of next return to perihelion in February, 1850, but in consequence of the computed positions showing that observation in that year would be quite hopeless, the calculation was continued with all possible precision to the ensuing perihelion passage early in August, 1855. The computed track in the heavens for this appearance was by no means an unfavourable one for observation; the comet would remain for a considerable period near the earth, being at its least distance on August 2, just before the perihelion passage, when it should have approached our globe, according to Prof. Brünnow's calculation, within 0·58 of the earth's mean distance from the sun. Nevertheless, it was not detected in this year—an object observed by M. Goldschmidt, not far from its track, in May, being certainly a distinct body, if the star of comparison was correctly identified. It was looked for repeatedly with the large refractors at Cambridge and Berlin. In 1860 again, ephemerides were prepared and a search was made, at least at the observatory of Harvard College, U.S., but ineffectually, indeed the chance of observing this comet when the perihelion passage falls in the winter must be but small.

The later results obtained by Prof. Brünnow, to which allusion is made above, will be found in No. 3 of his Ann Arbor *Astronomical Notices:* he there gives his reasons