to be determinable; but in a future number of this journal I hope to be able accurately to name both the supposed milker and the supposed milk-cow.

Lippstadt HERMANN MÜLLER

Free-Standing Dolmens

MR. LUKIS, in a paper recently read before the Society of Antiquaries, nominally "On certain Erroneous Views respecting the Construction of French Chambered Barrows," but really a method of criticising severely Mr. Fergusson's work on the "Rude Stone Monuments," states that it is an "*error*" to suppose that the Dolmens of that country were ever free-standing; in other words, he lays down the "rule," "there were no free-standing dolmens in France." The announcement that, with regard to monuments of whose fashioners we know absolutely nothing, a universal negative of this kind can be safely laid down as a law, would be startling, did it not come from one who is backed by such extensive inductive evidence as is Mr. Lukis. His "rule" was "established by the extreme rarity of the instances." This being the case, he calls those "in error" who would, from these instances, form a small class, or species of dolmen. As, in an essay on the Cornish sepulchral monuments, which you recently most kindly reviewed at length, I am committed to this latter view—one, by the way, which I had struck out for myself before the appearance of the "Rude Stone Monuments,"—will you kindly permit me to call your attention to one structure which I have ventured to place, and shall still venture to place, in the discarded class? I do so as a protest against the dictum of Mr. Lukis being extended to our British examples, before a careful scrutiny has been made of *every* monument of the kind from one corner of our islands to the other. On this single instance, such as it is, it must be clearly understood that I build no theory; it will be for others to judge whether it does not afford some evidence of the difference in construction and use of the dolmen or table-stone proper, and the kist-vaen cromlech; one thing only I will add, that, limited as my experience is to the monuments of Britain, I shall not be exposed to the temptation of explaining away any observed fact in order to reconcile a doubtful comparison. Without feeling that I am guilty of "dabbling in archæology," or of setting forth "any dogmatic expositions of hypotheses" (!!), or of "establishing my proposition from second-hand information," or in short of being the victim of any very "erroneous view" (all which faults Mr. Lukis finds in those who differ from him), I consider that the following facts justify my statement that the monument I am about to describe always was, as it is now, a free-standing dolmen.

At Lanyon, in the parish of Madron, Cornwall, stands a tripod dolmen, or cromlech, consisting of three slim pillars of unhewn granite supporting on their summits a horizontal stone over 40 ft. in circumference and averaging 20 in. thick. In 1815 it fell; but previous to its fall a man on horseback could sit upright underneath the cap-stone. In 1824 it was again set up; but two drawings had been made of it in its pristine condition, one by Canon Rogers in 1797, and the other by no less accurate a draughtsman, half a century before, namely, by my ancestor, Dr. Borlase. Both these drawings agree in representing the extreme slimness of the pillars; their distance apart; and the great height of the monument; features which render it not unlike a gigantic three-legged milking stool. Then, as now, there was no mound about it, as there is in the case of each and all of the kist-vaen cromlechs. It stood on a low bank of earth, and the area had been often disturbed by treasure seekers. No houses are near it which could have received the stones of a denuded mound. Added to this, it is difficult to see how a kist-vaen, or *septum* of any kind, could have been formed beneath the cap-stone. Had a wall of *small* stones been built up from pillar to pillar the weight of the superincumbent mound must have forced them inwards, a catastrophe which the "dolmen-builders" were always most careful to avoid. Secondly, had *large* stones placed on edge formed the walls of the kist, how is it they are *all* removed, while every other cromlech in the district retains them? But, laying aside this evidence, my strongest proof is yet to come. The interment in this instance was *not in the kist at all.* A grave had received the body six feet under the natural surface of the surrounding soil, and within the area described by the structure. This being the case, of what use could an enclosed kist have been; or why should the cenotaph be covered in at all? Add to this again, that on the southern side of the structure, and so near it that a mound over the monument must inevitably have covered it up, stands a little circular ring cairn of the ordinary type, in the centre of which I found the remains of an inner ring, which, though now rifled, had doubtless contained an interment. Must I then explain away in deference to superior experience or received opinion each and all of the above facts, in order to reconcile this monument with those which seem to be totally different structures, viz., the kist-vaens?* Should I not by so doing be sacrificing a fact to an hypothesis, and is not that hypothesis of such a nature that even a single instance well established must shake it to its foundation? Should I not incur a charge of erroneousness equal to, if not greater, than that which Mr. Lukis brings to bear on all who differ from him?

No one can wish more than I do to see errors expunged, and the truth in these matters arrived at; but I must confess that I cannot see how this will be brought about by confronting one hypothesis with another equally dogmatic, and more universally inclusive. WILLIAM C. BORLASE

Castle Horneck, near Penzance

Fertilisation of the Pansy

I AM glad to be able to confirm, to some extent, from observation, Mr. Bennett's theory of the fertilisation of the Pansy, given in NATURE, vol. viii. p. 49. I watched a considerable number of specimens of *Viola tricolor* on a grassy hill-top where the smaller insects were very numerous and busy, and twice saw them entered by a minute fly. In the first case the insect was dusty with pollen when it arrived. It settled on the lower petal and walked up one of the black lines to the gap in the ring of anthers, through which it entered with some difficulty—leaving some of the foreign pollen on the stigma as it passed. When it came out it had still more pollen on it than when it went in, and again in passing the stigma it left some on it. It paused a moment on the lower petal to clean itself, and left a little ball of pollen on the hairs on one side of the stigma. In the second case, the insect alighted first on one of the upper unmasked petals, turned round and round as though seeking the guiding lines, and flew off to the lower petal, where, without hesitation, it followed the guiding lines as the other had done. After it had passed the stigma there was no pollen visible on its surface; but after it had come out, almost the whole of the lower half was covered. In each case the passage through the ring of anthers seemed rather a struggle. There were many bees about, but I did not see any of them visit the Violæ, although they were almost the only flower near. A. T. MYERS

Penrith, June 30

European Weeds and Insects in America

A CANADIAN friend writes to me:—"I have heard or seen it mentioned as a fact that European weeds and insects introduced into America flourish for a while, but after fifty or sixty years gradually disappear: for instance, that the Hessian fly (so called from having been brought over by the Hessian troops in their hay in the war of independence) has died out or ceased to give trouble, though at one time it totally destroyed the wheat crops of New England. I do not know how far the facts have been tested, or how far they are owing to improved agriculture."

This statement, if true, is obviously of great importance. Can any of your correspondents confirm or disprove it?

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, July 4

CHLOROPHYLL COLOURING-MATTERS †

IT would be impossible for me not to look upon the appearance of such a work as the one recently published by Dr. Gregor Kraus with much satisfaction, since the chief object of the author is to call the attention of his countrymen to the value of the spectrum-microscope in studying the colouring-matters of plants. He commences with a description of the instrument, and says that, though originally designed for the examination of microscopical objects, it is not only as useful as any

* The only other tripod dolmen in Cornwall, viz., that at Caerwynen, is also a free standing one (within the memory of man, at least), whereas the *kist-vaens* are *one and all* partially covered by their envelope.

† "On the Chlorophyll Colouring-Matters." ("Zur Kenntniss der Chlorophyllfarbstoffe und ihrer Verwandten"). By Dr. Gregor Kraus. (Stuttgart, 1872.)

larger spectroscope for the study of the absorption of sol-utions, but indeed in many cases preferable. He describes two different kinds of eye-piece, viz., a simple form made by Merz, and the far more complete Sorby-Browning, with the method of measurement proposed by Mr. Browning, and expresses his regret that the value of such instruments has been almost altogether overlooked by German botanists. In treating on the application of the apparatus, the author very justly points out the great advantage of having a bright illumination, without too much dispersion, and the importance of being able to examine the spectrum of a leaf or any other object in its natural state, in order to ascertain whether the colouring matters dissolved out from a plant by any solvent do really occur in it, or are products of decomposition. I would also myself add that in some cases the difference between the spectrum of a substance in a free state and when dissolved is so considerable that care must be taken not to conclude that there has been actual decomposition, until the character of the spectrum of the solid substance, in a free state, has been ascertained; and even when the spectra are very nearly the same, the position of the absorption-bands may differ sufficiently to make it possible to determine whether a colouring-matter naturally exists in a free state or dissolved in water, or in an oil, according as it is or is not soluble in water. The fact of being thus dissolved or not is in some cases, probably, a question of considerable physiological importance, since the existence of solid particles along with, or even actually surrounded by, a liquid capable of dissolving them, points to a very different origin and relation to structure to those of a substance merely dissolved in the juices of a plant or an animal. The solution of such a colouring-matter is sometimes one of the first changes that occur in decomposition, as if set free from minute cells.

Having explained the general methods employed, and given a list of the chief publications connected with the subject, the author proceeds to the consideration of various colouring-matters found in plants. If I had written this review immediately after the work was published, I should have expressed my agreement with the greater part of the author's conclusions; for they are those to which a most careful experimenter would be led by employing the methods generally known at that time; but during the last year I have devoted myself exclusively to this particular subject and have been led to employ almost entirely new methods of investigation, and the result is that I must now point out a number of particulars in which I think the author's conclusions are not altogether correct. These new methods consist chiefly in the more or less perfect separation of the different substances by means of bisulphide of carbon, alcohol, and water, used in varying proportions, and in a somewhat peculiar manner; in the employment of what I have named *photo-chemical analysis*, or the use of light as a reagent, so as to destroy some constituents, and leave others, which perhaps could not be separated by chemical methods; and in studying and comparing together all classes of plants, especially the lower cryptogamia, when growing in various conditions; and not only in examining them qualitatively but also in determining the relative amount of the different colouring-matters by a method of comparative quantitative analysis. I will not now enter into detail, but refer to a paper recently communicated to the Royal Society, on comparative vegetable chromatology, in which I have given a complete general description of the methods I have used, of the facts I have observed, and of the conclusions drawn from them, which have a very direct bearing on some of the most important questions in biology, and enable us to examine them from a new point of view.

One great value of the author's work consists in its giving a very complete account of the researches of previous investigators, which I have myself found extremely useful; since so much that has been written is difficult of access. At the same time, since the methods employed were often altogether unsuitable, and most of the experiments are now known to have been made with mixtures, many of the results are of very little more than historical interest. The work also contains three excellent lithographed plates of the spectra of the various colouring-matters in a natural or altered condition. The whole subject is treated in an admirable manner, and I trust that no one will think that I wish in any way to detract from the author's merit in taking this opportunity to illustrate the application of the methods which I think should be employed in such researches.

The coloured solutions obtained from leaves are very complicated mixtures. It is not at all unusual for them to contain as many as ten different coloured substances. The progress of our knowledge has to a great extent depended upon the application of improved methods, which have made it possible to distinguish the various constituents of these mixtures. The author has himself pointed this out, and shown that what was at one time called chlorophyll, and looked upon as a single substance, consists of a mixture of a bluer-green substance with a yellow substance. This kind of analysis had however previously been considerably extended. In a very short paper,* containing no description of the methods of experiment, or of the separate colouring-matters, Stokes said that his researches had led him to conclude that the chlorophyll of land plants is a mixture of four substances, two green and two yellow, and in my late paper I have shown that by the newer and improved methods it is easy to prove that there are not only these two green substances, one a blue-green and the other a yellow-green, having perfectly distinct and characteristic properties, though confounded together by nearly all other experimenters, but also four or even five perfectly distinct yellow substances. These various colouring-matters I have named *blue chlorophyll*, *yellow chlorophyll*, *orange xanthophyll*, *xanthophyll*, *yellow xanthophyll*, *orange lichnoxanthine*, and *lichnoxanthine*. They are all insoluble in water, and soluble in bisulphide of carbon, and besides one or two products of decomposition, they must all have been present in what has sometimes been called chlorophyll, and looked upon as a single compound. Now, almost the only points in which I feel compelled to differ from the author are those cases in which the new methods of examination prove that what he regarded as a single colouring matter is in reality a mixture of two or even more, which can be separated, and do occur separately in particular plants. Thus, for example, in Plate II. Fig. 1, he gives a drawing of the spectrum of the blue-green colouring matter of *Deutzia scabra*, showing six absorption-bands. Now, I feel persuaded that this colouring-matter must have been a mixture of three different substances, viz. my blue chlorophyll, my yellow chlorophyll, and the product of the action of acids on blue chlorophyll The bands numbered 1, 2, 3, and 6 are mainly due to blue chlorophyll. Part of No. 1 and No. 5 are due to yellow chlorophyll, and the band No. 4 clearly indicates the presence of a small quantity of the product of the action of acids on blue chlorophyll. This is almost always present when the preparation is made in the manner adopted by the author, but by neutralising the acid of the juice by carbonate of ammonia, or still better by employing a plant that has an almost perfectly neutral juice, chlorophyll may be obtained which gives a spectrum almost absolutely free from any such band.

In the spectrum shown by Plate III. Fig. 1 of the blue-green colouring-matter of an *Oscillatoria*, the bands of yellow chlorophyll are absent, for it does not exist in such *Algæ*, but the broad band shown at about 500 of the author's scale, not seen in the spectrum of the chlorophyll of *Deutzia*, must have been mainly due to orange xantho-

* Proceedings of the Royal Society, 1864, xiii. p. 144.

phyll, which occurs in considerable quantity in *Oscillatoriæ*, but is relatively almost absent in green leaves, and would not be separated by the method employed by the author in making the preparation. Comparatively pure blue chlorophyll, prepared from olive *Algæ* by the method described in my late paper, gives a spectrum free from absorption over the whole of the green and a considerable part of the adjoining blue. The close resemblance, and yet decided difference, between the spectra of the blue-green colouring matter obtained from the two above-named sources, did not escape the author's notice, but the methods employed were inadequate to prove that both contained the same principal blue-green substance, mixed in one case with one, and in the other case with another colouring matter. I may here say that the relative amount of blue and yellow chlorophyll differs very much in different classes of plants, and even in the same plant, when in different conditions, and the study of this variation leads to results of great interest in connection with vegetable physiology; since, amongst other things, it proves that leaves normally very yellow are quite unlike those that have turned yellow in autumn, but analogous to those which are abnormally yellow owing to absence of light, as though the deficiency of chlorophyll were in both cases due to weak constructive energy; and the comparative absence of yellow chlorophyll in such abnormally weak plants, belonging to the highest classes, causes their colouring to approximate much more closely to that of those of much lower organisation.

I must say that I object to the term chlorophyll being applied, as by the author, to a mixture of the various yellow substances belonging to the xanthophyll group. with one or both of the above-named green substances, The green colour of leaves is due to them, and they are both actually green, one a blue-green and the other a yellow-green, so that the terms blue chlorophyll and yellow chlorophyll appear to me very appropriate. It would be better and extremely convenient to adopt some such word as *endochrome*, to express any mixture of coloured substances contained in the cells of plants, which has no reference to any particular tint of colour.

The very materially different position of the chief absorption-band of chlorophyll when in the leaves of plants and when in solution has been noticed by the author, and likewise the difference in its position when the chlorophyll is dissolved in different liquids. He attributes this entirely to the difference in the density of the liquid, and concludes that in the leaves the chlorophyll may be combined with or dissolved in some dense substance. The difference in the position of the bands of chlorophyll is very small compared with the difference seen in the case of some other colouring-matters, and by carefully studying the question I have come to the conclusion that the position of the bands does not vary directly with the density of the solvent, or with any other general property, but is so independent that it is desirable to look upon it as a special property, and to call it the *absorption-band-raising* power. The extent to which the bands are raised varies much according to the substance; but, as an apparent rule, if the position is altered, they lie nearer to the blue end when the substance is dissolved than when in a free state. In accordance with this view of the subject, it appears as though in the living plants chlorophyll and various other colouring-matters exist in a free state, not combined with or dissolved in any wax, fat, or oil, with which, however, they often combine when the plant is boiled in water, and with which they are combined when a solution is evaporated to dryness, so that the spectrum of such a dried-up material may, and often does, differ most materially from that of the endochrome in the living plants. As an illustration of the opposite case, I may refer to the spectra of yellow flowers, which often show that the endochrome is combined with, or dissolved in, a fat or oil. When not thus combined, the spectra are so different that the colouring-matter might be, and sometimes has been, looked upon as distinct, before the true cause of the difference was known. The microscope alone could not decide this question, since visible granules might not be the free colouring-matter, and, on the contrary, it might be free, and the particles too small to be separately visible.

H. C. SORBY

(*To be continued.*)

RECENT RESEARCHES ON THE PHYSIOLOGICAL ACTION OF LIGHT

THE arrangements by which the mind is brought into relation with the outer world are—(1) a terminal organ, such as the retina, or the intricate structures of the internal ear, or the touch corpuscles of Wagner, for the reception of impressions from without; (2) a nerve, endowed with a special sensibility peculiar to the sense for the conveyance of influences from the terminal organ to the brain; and (3) a sensorium or brain in which, on receiving these influences, changes occur which give rise to the phenomena of consciousness.

Nerves act, therefore, as conductors from the terminal organs to the brain. These terminal organs are specially fitted for the reception of specific stimuli, such as the vibrations of the ether, which, when received by the retina, induce a change which is transmitted to the brain, and gives rise to the sensation of light, or the condensations and rarefactions of the air which cause sound. But though specially fitted for these stimuli, the terminal organs may be affected in other ways. For example, mechanical pressure on the retina produces a sensation of light, and many diseases affecting the auditory apparatus by compression, cause agonising sensations of sound. The nerves in connection with the sense organs are termed nerves of special sense, because they are supposed only to convey influences which are derived from the special terminal organs with which they are connected. These nerves are, however, themselves not affected only by the special stimulus which affects their respective terminal organ. As is well known, the optic nerve is not affected by light—a fact easily demonstrated by Marriot's experiment showing that the retina at the entrance of the optic nerve is insensible to light.

The nature of the specific change produced on the terminal organs by the action of external stimuli has not hitherto been experimentally examined. Let us take the case of the eye. Numerous hypotheses have been advanced. The action of light on the retina has been conjectured to be a mere communication of vibrations, an intermittent motion of portions of the optic nerve, an electrical effect, a heating effect, or a photographic effect like that produced by light on a sensitive surface, but up to this time there has been no experimental evidence in support of either of these views.

The result of investigations made by Mr. Dewar and Dr. McKendrick, of Edinburgh, communicated to the Royal Society of Edinburgh, has been to show that the specific effect of light on the retina and optic nerve is a change in the electro-motive force of these organs. They have been able to demonstrate this by the following arrangements:—The eye of a frog rapidly killed by pithing is dissected out of the orbit, so as to leave the sclerotic entirely free from muscle, and a portion of optic nerve intact. This preparation is placed on the cushions of the well-known arrangement of Du Bois-Raymond for collecting electric currents from animal structures, consisting of two zinc troughs, carefully amalgamated on the inner surface, and containing pads of Swedish filter-paper moistened with a solution of pure neutral sulphate of zinc. To protect the eye from the irritating action of the sulphate of zinc, thin films of sculptors' clay, mixed with a weak solution of chloride of