smattering of the more recent development of the science. Being thus completely furnished, he has entered the arena. The result is altogether what might have been expected. Inaccuracy of statement, meaningless definitions, sins of omission and of commission, abound. If anyone wishes to learn the rudiments of chemistry, let him eschew this catechism as he would poison.

M. M. P. M.

Summer Holidays in Brittany. By Thomas J. Hutchinson. With Map and Illustrations. (London: Sampson Low and Co., 1876.)

MR. HUTCHINSON is well known, among other things, for his researches among Peruvian antiquities, and therefore, to his tour in Brittany he brought a trained observation. He has managed to write a very pleasant book on rather a worn subject, a book which is likely to give its readers a desire to follow the author's example. Indeed it might form a very useful guide-book for the district traversed by Mr. Hutchinson, and would have the advantage of being much more pleasant reading than guide-books generally are. Mr. Hutchinson evidently made good use of his time when in Brittany, and to those who have not read much on the subject the book will furnish a great deal of information on the nature of the country, the characteristics and manners and customs of the people, the antiquities, historic and prehistoric, the ecclesiastical and political history, and many other interesting points. A very good map and some fair illustrations add to the value of the book.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Colour of Flowers Grown in the Dark

IN NATURE, vol. xiii, p. 348, Mr. Thiselton Dyer gives an extract from Sachs's "Text-book of Botany" to the effect that no change is produced in the colour of flowers by growing them in the dark. This led to a letter from Mr. J. C. Costerus (vol. xiii, p. 427), calling attention to the results obtained by Askenasy, published in 1876, who found that some kinds were changed in colour, and some not changed. In the autumn of 1873 I made a number of experiments on this subject, and published a short account of them in the same year in the *Quarterly Journal of Science*, vol. iii, p. 474. I came to the same conclusion as Askenasy has come to, and was also able to establish some important generalisations. I will only mention a few special instances. I was unable to cause any change in the colour of the common Orange Lily (*Lilium aurantiacum*), whereas I found that a very considerable change was produced in the case of *Erysimum Peroffskianum* by only a moderate degree of darkness. This may perhaps be owing to the fact that the orange tint of these two kinds of flowers is due to entirely different substances. That of the lily is due to what I have called orange xanthophyll, whereas that of the other flower is due to a much less stable compound, giving an entirely different spectrum, met with also in the orange marigold, and therefore named by me *Calendula xanthine*. Comparing together the mixed colouring matters found in an equal weight of the petals, I found that the amount of the *Calendula xanthine* was only half as great in the petals grown in the dark, whereas the more yellow constituents were reduced only to three-quarters, so that the general colour was more yellow. I found that a similar change could be produced in the case of the marigold. If shaded when the flowers are somewhat grown, the total colour may be very considerably reduced without there being any material change in the ratios of the different colouring matters, whereas when grown in the dark from a very small bud, the ratios are changed, as in the case of *Erysimum*. Growing flowers in the dark seems to stop the normal development to a greater or less extent according to the nature of the colouring-matters, the effect being the greatest in the case of those substances which are the most easily decomposed. We thus find

what appears at first sight to be a very unlikely result, viz., that those constituents which, when dissolved out from the petals, are the most easily discoloured by exposure to light, are formed in relatively greater amount when the flowers are grown in the light, which is easily explained if we assume that a higher vital power, depending on the presence of light, is necessary to overcome the more powerful chemical affinities of the less stable compounds.

H. C. SORBY

The Ash Seed Screw

MR. STEPHEN WILSON remarks, in his note on this subject (NATURE, vol. xiii, p. 428), "Why the seed generally becomes twisted as it dries is a very interesting question. But what seems to me the most remarkable fact about this phenomenon is, that in every case, and in all trees alike, the thread of the screw is in one direction." He also alludes to the uniformity in the direction of torsion in the awns of two species of oat. Torsion of all kinds occurring in plants is usually assumed to be due to unequal longitudinal tension (see Sachs's "Handbook," p. 770). In a paper read before the Linnean Society, March 16, I pointed out that the uniformity in the direction of the torsion cannot be thus accounted for; and a totally different explanation was given of the twisting and untwisting of the awns of certain fruits (*Avena elatior* among the number) when they are dried and moistened. It was shown that the power of torsion resides in the individual cells of which the awn is constructed, and that it is by their combined action that the awn, as a whole, becomes twisted in drying. It appeared to me extremely probable that the same explanation would hold good for the twisted wing of the ash fruit. I therefore boiled one in nitric acid and chlorate of potassium, by which means the woody tissue is separated into its constituent cells. These were then teased out on a slip of glass and thoroughly dried over a lamp, and it was found that many of them had become twisted on their axes; and, which is important, that they were all twisted in the same direction as the fruit itself. This artificial drying represents the natural drying process which occurs during the ripening of the fruit. In both cases contraction and consequent torsion result from the loss of water, but in the natural process the cells not being free to twist independently, are compelled to combine in producing that torsion of the whole fruit which we are considering. It is interesting to find the same principle holding good in the case of the ash screw as in that of the awns of various Gramineæ and Geraniaceæ, and the twisted tails of the achenes in *Anemone montana*. Moreover, I strongly suspect that the principle of the torsion of an organ being dependent on the twisting of its constituent cells is capable of wider extension, so as to embrace the torsion of the stems of twining plants, &c. This subject I hope immediately to investigate.

FRANCIS DARWIN

P.S.—The samara of the sycamore is a more efficient parachute than that of the ash, but the wing has no appreciable twist, and there is no uniformity in the direction of rotation assumed as the fruits fall to the ground.

Down, Beckenham, April 4

The Animal of Millepora

It is a remarkable fact that during all the discussions on the late L. Agassiz's statements regarding the animal of Millepora some very careful drawings of it have been in the possession of Major-General Nelson, R.E. They were done by himself during his residence at Bermuda at the time when he was writing that communication to the Geological Society on the reefs and general structure of the islands which has made *Lieut. Nelson, R.E.*, a name of mark.

In common with most naturalists, I had expected that soon after the *Challenger* reached Bermuda, we should have had a satisfactory description of this very interesting polyp, so that the truth, or the contrary, of Agassiz's description could be tested. But it was not until July in last year that any communication relating to the subject was sent off from the *Challenger*, the paper being read on Nov. 25, 1875, at the Royal Society. Mr. Moseley noticed therein that the examination of Millepora is beset with serious difficulties, he, however, states that there are large and small polyps, and that both kinds have tentacles, and "they appear to be four in number, and compound." He observes: "they are simply retracted by means of muscular fibres which are arranged round the base of the cylindrical stomach

radially, and that no mesenteries have been seen." Mr. Moseley was naturally dissatisfied with these poor results, and hoped to do better things at Hawaii. In the meantime I became aware of the value of the drawings I have already alluded to, and as I am at work on several subjects with Gen. Nelson, I sent a communication and the drawings to the "Annals and Magazine of Natural History" before the evening of April 6, when Mr. Moseley's paper on Millepora was heard by me at the Royal Society. It is a satisfaction for me to be able to state that Gen. Nelson's drawings prove that Agassiz saw a part of a polype, and that Mr. Moseley's beautiful delineations, far in advance of all, testify to the correctness of my fellow-worker. I do not credit the hydroid nature of the polyp now, any more than I did when writing the reports on the British fossil corals, and I believe Millepora to be an Actinozoan.

P. MARTIN DUNCAN

The Use of the Words "Weight" and "Mass"

THE relations between weight and mass, gravity and acceleration, are so well defined in all good treatises on dynamics, that it appears superfluous to dwell on these questions. But as it has been stated by Prof. Barrett, vol. xiii., p. 385, that the C. G. S. system of units has been introduced into the course of Mechanics in this College, I may be permitted to say that the system actually employed is not that referred to by your correspondent. I generally employ the kilogramme, metre, and second, and sometimes the foot, pound, and second, to measure a dynam or unit of force. The dynamometers alluded to as about to be exhibited at the Loan Exhibition of Scientific Instruments at South Kensington are suitable to the former system, and I use them for the measurement of dynams in kilogrammetres. One of these dynamometers is graduated for every 200 grammes up to 100 kilogrammes, the other for every 100 grammes up to 10 kilogrammes, and they cannot be depended on for results within the tenth of a kilogramme. Spring dynamometers, though suitable for the large units employed in mechanics, are totally unfit for measuring units on the C. G. S. system. I concur with Prof. Everett, in his book on this system, when he says:—"A spring balance, it is true, gives a direct measurement of force, but its indications are too rough for purposes of accuracy" (p. 8). Spring dynamometers are therefore unsuited to a system where the units are measured by $\frac{1}{1000}$ of a gramme, or about $\frac{1}{7000}$ of a grain, as in the C. G. S. system.

HENRY HENNESSY

Royal College of Science for Ireland, Dublin

The Physical Constitution of Steam

I BELIEVE the following remarks on the physical constitution of steam are in some degree original, in form at least, though perhaps not in substance.

Dr. Andrews has shown by his experimental researches on carbonic acid, that at a temperature above 31° C. or 88° F. the difference between the gaseous and the liquid states no longer exists. I quote the following brief statement from an admirable paper on the subject, by Prof. James Thomson, in the "Proceedings of the Belfast Natural History and Philosophical Society," 1872;—he is speaking of the case in which a given quantity of carbonic acid, of which part is in the gaseous and part in the liquid state, is kept at constant volume, while the temperature, and consequently the pressure, are gradually increased:—

"As the temperature and pressure are augmented, the gaseous part is always increasing in density, and the liquid part is diminishing in density, till at last the two come to have the same density with one another, and then they are perfectly alike in every respect, all distinction between them having vanished. At this stage the temperature is 31° C., and the pressure is about seventy-five atmospheres. Above this temperature of 31° no change of pressure can cause gasification or liquefaction; and above this pressure of about seventy-five atmospheres, no change of temperature can cause gasification or liquefaction."

This temperature of 31° C. is called by Dr. Andrews the critical temperature for carbonic acid. Above its critical temperature, although carbonic acid may have the density either of a gas or of a liquid, the two states are not sharply separated from each other as a liquid is from its vapour, but graduate into each other insensibly. It is believed that every gas and vapour has its own critical temperature. Those of the permanent gases are believed to be so low as to be unattainable by any known process. That of water or steam, on the contrary, is probably too high to be observed in a glass tube, and consequently too high to be directly

observed at all: for the only known test of the critical temperature being attained, consists in the disappearance of the visible boundary surface between the liquid and the vapour or gas. My purpose is to show how the critical temperature for steam may be approximately estimated with a great degree of probability.

The fact that the latent heat of steam diminishes as the temperature increases, formerly seemed to me one of the strangest of all facts; but the above-mentioned properties of carbonic acid, and no doubt of all gases and vapours, make it quite intelligible.

The *latent heat* is defined as the heat given out when steam is condensed into water of its own temperature. The *total heat* is defined as the heat given out when steam is condensed into water at zero Centigrade, and is the sum of the latent heat and the temperature. According to Regnault, the relation between temperature and total heat is expressed by the formula—

$$\lambda = 606.5 + .705 t.$$

λ being the total heat and t the temperature. This has been ascertained to be true from 0° to 230° , and if it is true for all temperatures, at a temperature of 872.7 the total heat and the temperature would be the same, and the latent heat would vanish: 872.7 is consequently the critical temperature which is deducible from the above formula for water.

Old Forge, Dunmurry,
Co. Antrim

JOSEPH JOHN MURPHY

Coloured Solar Halos

IN the interesting letters of Drs. Schuster and Frankland I note some remarks on the rarity of the apparition of complete halos about the sun in this country. Had I read these remarks some six years ago I should have passed them by without surprise. It so happened, however, that my attention was drawn to the subject of halos, coronas, &c., by Kämtz's "Lehrbuch der Meteorologie" about the year 1869, and I at once began to examine the sky near the sun every fine day, and note down any appearance of halos, fringes, &c. When I began, my impression was that I should rarely see the solar halo seeing that it had escaped me for several years. However, I soon found out my mistake, and the subjoined list, compiled from the observatory note-books, gives the number seen each month in 1874 and 1875. No doubt, several escaped my vigilance in some months and a few in others. The figures, at any rate, show that the phenomenon is by no means rare.

Solar Halos in 1874 and 1875.

	1874.		1875.		1874.		1875.
Jan. ...	0	...	2	July ...	3	...	4
Feb. ...	1	...	2	Aug. ¹ ...	1	...	—
March ...	1	...	2	Sept. ...	2	...	4
April ...	11	...	4	Oct. ...	0	...	2
May ...	8	...	1	Nov. ...	3	...	0
June ...	11	...	2	Dec. ...	1	...	0
				Jan. ...	1	...	8
				Feb. ...	0	...	—
				March ...	—	...	—

Had I been quite sure that Dr. Schuster's remarks referred to the ordinary solar halo, this letter would have reached you a week ago or more. I will only add that the halos I have observed are nearly always complete (when the sun is high enough), that they are often very bright and most striking phenomena, and that the radius is usually about 22° .

Bermerside, Halifax, April 6

JOSEPH GLEDHILL

"The Effect of the Sun's Rotation and the Moon's Revolution on the Earth's Magnetism"

THE above was the title of an article by J. Allan Brown, published in NATURE, vol. xiii. p. 328.

The establishment of these facts, if they have not already been published, will aid amateur investigators very much in arriving at satisfactory conclusions in regard to the phenomenon above referred to.

First, from the revolutions of the sun, do the positively defined edges of sunspots always reappear at the same moment, and are their relative positions with regard to each other ever the same through a series of years, giving them a fixed and positive character, both as to position and time of revolution?

Second: At what times, with regard to the position and area of the largest and most numerous sunspots, and whether they are hidden by the revolution of the sun, or face the earth, is the

* I was from home most of this month in both years.