roof, facing north; at Truro they are placed on the roof of the Royal Institution, about forty feet above the ground, in a wooden shed through which the air passes freely; at Falmouth they are eleven feet above the ground, close to a wall, and in a confined situation; at Helston we are not informed how they are placed; and at the Scilly station we are only told that they "are well placed"—a statement which the observations themselves render very doubtful.

The times of observation are hourly at Falmouth, 9 A.M. and 3 and 9 P.M. at Helston, and as respects the other three stations we have no information. In reducing the observations, "corrections for diurnal range" are used in some cases, though the observations themselves show that the range corrections adopted are plainly not even approximately correct for the place.

A system of meteorological observation which would furnish the data for an inquiry into the important question of a comparison of the local climates of Cornwall requires yet to be instituted. Such a system must secure at each of the stations included within it, uniformity in exposure of instruments, uniformity in hours of observation, and uniformity in methods of reducing the observations. Till this be done, such climatic anomalies, as we have pointed out in the case of Bodmin, will continue to be published, certainly misleading some, and probably leading others to dispute the usefulness of meteorological observations.

We have much pleasure in referring to the additional meteorological information given in the tables, which is often of considerable value, particularly that supplied for Helston by Mr. Moyle, whose tables have the merit of giving the results for the individual hours of observation, as well as deductions from these.

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.*]

Vibrations of a Liquid in a Cylindrical Vessel

IN NATURE for July 15, there is a short notice of a paper read before the Physical Society by Prof. Guthrie on the period of vibration of water in cylindrical vessels. It may be of interest to point out that the results arrived at by Prof. Guthrie experimentally, and many others of a like nature, may also be obtained from theory.

In the first place the fact, that the period of a given mode of vibration of liquid in a cylindrical vessel of infinite depth and of section always similar to itself (*e.g.* always circular) is proportional to the square root of the linear dimension of the section, follows from the theory of dimensions without any calculation. For the only quantities on which the period τ could depend are (1) ρ the density of the liquid, (2) g the acceleration of gravity, and (3) the linear dimension d. Now as in the case of a common pendulum it is evident that τ cannot depend upon ρ. If the density of the liquid be doubled, the force which act upon it is also doubled, and therefore the motion is the same as before the change. Thus τ, a time, is a function of d, a length, and g. Since g is — 2 dimensions in time, $\tau \propto g^{-\frac{1}{2}}$, and therefore in order to be independent of the unit of length, it must vary as $d^{\frac{1}{2}}$ inasmuch as g is of one dimension in length. Hence $\tau \propto d^{\frac{1}{2}} g^{-\frac{1}{2}}$. This reasoning, it will be observed, only applies when the depth may be treated as infinite.

The actual calculation of τ for any given form of vessel involves, of course, high mathematics, the case of a circular section depending on Bessel's functions. But there is an interesting connection between the problem of the vibration of heavy liquid in a cylindrical vessel of any section and of finite or infinite depth, and that of the vibration of gas in the same vessel, when the motion is in two dimensions only, that is everywhere perpendicular to the generating lines of the cylinder. If λ be the wavelength of the vibration in the latter case,* which is a quantity independent of the nature of the gas, and $k = 2\pi \div \lambda$, the period τ of the similar vibrations in the liquid problem is given by

$$\tau = 2\pi \div \sqrt{\frac{gk(\epsilon^{kl} - \epsilon^{-kl})}{\epsilon^{kl} + \epsilon^{-kl}}},$$

l being the depth. The formula shows that in accordance with Prof. Guthrie's observation τ diminishes as l increases, and that when l is sufficiently great

$$\tau = 2\pi \div \sqrt{gk}.$$

If x be the value of k, viz. $2\pi \div \lambda$, for a circular vessel of radius unity, then the values of x for the various modes of vibration are given in the following table extracted from a paper on Bessel's functions in the *Philosophical Magazine* for November 1872.

Number of Internal Spherical Nodes.	Order of Harmonic.			
	0	1	2	3
0	3·832	1·841	3·054	4·201
1	7·015	5·332	6·705	8·015
2	10·174	8.536	9·965	11·344

Thus if d be the diameter of the vessel, the period τ of the liquid vibrations is given by

$$\tau = 2\pi \sqrt{\frac{d}{2gx}};$$

so that if d be measured in inches, the number of vibrations per minute, n, is given by

$$n\sqrt{d} = \frac{30}{\pi}\sqrt{24 \times 32{\cdot}19 \times x}.$$

For the symmetrical mode of vibration considered by Prof. Guthrie, $x = 3{\cdot}832$, giving

$$n\sqrt{d} = 519{\cdot}4$$

agreeing closely with the experimental value, viz. 517·5. Even the small difference which exists may perhaps be attributed to the insufficient depth of the vessels employed.

This mode of vibration is not, however, the gravest of which the liquid is capable. That corresponds to $x = 1{\cdot}841$, giving

$$n\sqrt{d} = 360{\cdot}1,$$

and belonging to a vibration in which the liquid is most raised at one end of a certain diameter, and most depressed at the other end. The latter mode of vibration is more easily excited than that experimented on by Prof. Guthrie, but inasmuch as it involves a lateral a motion of the centre of inertia, it is necessary that the vessel be held tight.

The next gravest mode gives $x = 3.054$, and corresponds to a vibration in which the liquid is simultaneously raised at *both* ends of one diameter, and depressed at both ends of the perpendicular diameter. In this case the value of n is given by

$$n\sqrt{d} = 462{\cdot}7$$

Terling Place, Witham, July 15 RAYLEIGH

* Namely, the length of plane waves of the same period.

Insectivorous Plants

IF further confirmation be needed of Mr. Darwin's discovery of absorption by the leaves of the *Drosera rotundifolia*, it is afforded amply by the following experiments which I have just concluded:—

Having deprived a quantity of silver sand of all organic matter, I placed it in three pots, which I shall call A, B, and C. In each of these pots I placed a number of plants of the *D. rotundifolia* under the following conditions:—(1) Perfectly uninjured, but washed all over repeatedly in distilled water. (2) Similarly washed, but with all the roots pinched off close to the rosette, and with the leaves all buried, only the budding flower stalk appearing above the sand. (3) similarly washed, with the roots and the flower stalk left on, but all the leaves pinched off, the roots being buried in the sand. (4) Similarly washed, roots left on, four leaves buried in the sand, two leaves flower stalk, and roots left above the sand and the roots protected against the possibility of their absorbing anything from the sand. All the plants were carefully watched, so that no flies were caught.

roof, facing north; at Truro they are placed on the roof of the Royal Institution, about forty feet above the ground, in a wooden shed through which the air passes freely; at Falmouth they are eleven feet above the ground, close to a wall, and in a confined situation; at Helston we are not informed how they are placed; and at the Scilly station we are only told that they "are well placed"—a statement which the observations themselves render very doubtful.

The times of observation are hourly at Falmouth, 9 A.M. and 3 and 9 P.M. at Helston, and as respects the other three stations we have no information. In reducing the observations, "corrections for diurnal range" are used in some cases, though the observations themselves show that the range corrections adopted are plainly not even approximately correct for the place.

A system of meteorological observation which would furnish the data for an inquiry into the important question of a comparison of the local climates of Cornwall requires yet to be instituted. Such a system must secure at each of the stations included within it, uniformity in exposure of instruments, uniformity in hours of observation, and uniformity in methods of reducing the observations. Till this be done, such climatic anomalies, as we have pointed out in the case of Bodmin, will continue to be published, certainly misleading some, and probably leading others to dispute the usefulness of meteorological observations.

We have much pleasure in referring to the additional meteorological information given in the tables, which is often of considerable value, particularly that supplied for Helston by Mr. Moyle, whose tables have the merit of giving the results for the individual hours of observation, as well as deductions from these.

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.*]

Vibrations of a Liquid in a Cylindrical Vessel

IN NATURE for July 15, there is a short notice of a paper read before the Physical Society by Prof. Guthrie on the period of vibration of water in cylindrical vessels. It may be of interest to point out that the results arrived at by Prof. Guthrie experimentally, and many others of a like nature, may also be obtained from theory.

In the first place the fact, that the period of a given mode of vibration of liquid in a cylindrical vessel of infinite depth and of section always similar to itself (*e.g.* always circular) is proportional to the square root of the linear dimension of the section, follows from the theory of dimensions without any calculation. For the only quantities on which the period τ could depend are (1) ρ the density of the liquid, (2) g the acceleration of gravity, and (3) the linear dimension d. Now as in the case of a common pendulum it is evident that τ cannot depend upon ρ. If the density of the liquid be doubled, the force which act upon it is also doubled, and therefore the motion is the same as before the change. Thus τ, a time, is a function of d, a length, and g. Since g is -2 dimensions in time, $\tau \propto g^{-\frac{1}{2}}$, and therefore in order to be independent of the unit of length, it must vary as $d^{\frac{1}{2}}$ inasmuch as g is of one dimension in length. Hence $\tau \propto d^{\frac{1}{2}} g^{-\frac{1}{2}}$. This reasoning, it will be observed, only applies when the depth may be treated as infinite.

The actual calculation of τ for any given form of vessel involves, of course, high mathematics, the case of a circular section depending on Bessel's functions. But there is an interesting connection between the problem of the vibration of heavy liquid in a cylindrical vessel of any section and of finite or infinite depth, and that of the vibration of gas in the same vessel, when the motion is in two dimensions only, that is everywhere perpendicular to the generating lines of the cylinder. If λ be the wave-length of the vibration in the latter case,* which is a quantity independent of the nature of the gas, and $\kappa = 2\pi \div \lambda$, the period τ of the similar vibrations in the liquid problem is given by

$$\tau = 2\pi \div \sqrt{\frac{gk\,(\epsilon^{kl} - \epsilon^{-kl})}{\epsilon^{kl} + \epsilon^{-kl}}},$$

l being the depth. The formula shows that in accordance with Prof. Guthrie's observation τ diminishes as l increases, and that when l is sufficiently great

$$\tau = 2\pi \div \sqrt{gk}.$$

If x be the value of k, viz. $2\pi \div \lambda$, for a circular vessel of radius unity, then the values of x for the various modes of vibration are given in the following table extracted from a paper on Bessel's functions in the *Philosophical Magazine* for November 1872.

Number of Internal Spherical Nodes.	Order of Harmonic.			
	0	1	2	3
0	3·832	1·841	3·054	4·201
1	7·015	5·332	6·705	8·015
2	10·174	8.536	9·965	11·344

Thus if d be the diameter of the vessel, the period τ of the liquid vibrations is given by

$$\tau = 2\pi \sqrt{\frac{d}{2gx}};$$

so that if d be measured in inches, the number of vibrations per minute, n, is given by

$$n\sqrt{d} = \frac{30}{\pi}\sqrt{24 \times 32{\cdot}19 \times x}.$$

For the symmetrical mode of vibration considered by Prof. Guthrie, $x = 3{\cdot}832$, giving

$$n\sqrt{d} = 519{\cdot}4$$

agreeing closely with the experimental value, viz. 517·5. Even the small difference which exists may perhaps be attributed to the insufficient depth of the vessels employed.

This mode of vibration is not, however, the gravest of which the liquid is capable. That corresponds to $x = 1{\cdot}841$, giving

$$n\sqrt{d} = 360{\cdot}1,$$

and belonging to a vibration in which the liquid is most raised at one end of a certain diameter, and most depressed at the other end. The latter mode of vibration is more easily excited than that experimented on by Prof. Guthrie, but inasmuch as it involves a lateral a motion of the centre of inertia, it is necessary that the vessel be held tight.

The next gravest mode gives $x = 3.054$, and corresponds to a vibration in which the liquid is simultaneously raised at *both* ends of one diameter, and depressed at both ends of the perpendicular diameter. In this case the value of n is given by

$$n\sqrt{d} = 462{\cdot}7$$

Terling Place, Witham, July 15 RAYLEIGH

* Namely, the length of plane waves of the same period.

Insectivorous Plants

IF further confirmation be needed of Mr. Darwin's discovery of absorption by the leaves of the *Drosera rotundifolia*, it is afforded amply by the following experiments which I have just concluded:—

Having deprived a quantity of silver sand of all organic matter, I placed it in three pots, which I shall call A, B, and C. In each of these pots I placed a number of plants of the *D. rotundifolia* under the following conditions:—(1) Perfectly uninjured, but washed all over repeatedly in distilled water. (2) Similarly washed, but with all the roots pinched off close to the rosette, and with the leaves all buried, only the budding flower stalk appearing above the sand. (3) similarly washed, with the roots and the flower stalk left on, but all the leaves pinched off, the roots being buried in the sand. (4) Similarly washed, roots left on, four leaves buried in the sand, two leaves flower stalk, and roots left above the sand and the roots protected against the possibility of their absorbing anything from the sand. All the plants were carefully watched, so that no flies were caught.

I fed pot A with pure distilled water, B with strong decoction of beef, and C with ·0026 per cent. solution of phosphate of ammonia.

The results are briefly these, after seventeen days' experimentation: In A all the plants are growing and looking perfectly healthy, though those with four leaves buried and the roots exposed, looked sickly for a few days. Now, however, they are putting forth new leaves; so are those with all the leaves pinched off and the roots buried.

Those with the roots pinched off and all the leaves buried are bursting into flower.

In B all the plants are greatly damaged, those with the leaves only, and those with the roots only are quite dead. Those with the roots off and the leaves buried have their leaf stalks much blackened, as described by Mr. Darwin as the result of over-feeding. The pot smells strongly of ammonia.

In C the condition is very much as in A, but the growth has been much more active, for some of the plants with the roots off and leaves buried have pushed new leaves up through the sand, and those with only four leaves buried have put out numerous new leaves, and their roots are quite dry. In one of these latter I amputated the roots five days after it had been in the pot, and it is as vigorous as the rest. About ·03 of a grain of phosphate of ammonia has been supplied to this pot during twelve days for twelve plants.

It is, therefore, perfectly certain that the sun-dew can not only absorb nutriment by its leaves, but that it can actually live by their aid alone, and that it thrives better if supplied with nitrogenous material in small quantities.

The nitrogenous matter is more readily absorbed by the leaves than by the roots, for over-feeding kills the plant sooner by the leaves alone than by the roots alone. But it is also certain that the roots absorb nitrogenous matter.

On June 17 I read a paper to the Birmingham Natural History Society, in which I announced that I had been able to separate a substance closely resembling pepsine from the secretion of the *Drosera dichotoma.* Since then I have also separated it from the fluid taken from the pitchers of various nepenthes.

The secretion from the *Dichotoma* was gathered on a feather which was washed in pure distilled water. It made the water very viscid, although probably the whole amount gathered from the only available plant was not more than six or eight minims, and an ounce of water was used. One cubic centimetre of this solution to five cubic centimetres of fresh milk separated a thick viscid mass, with a very small quantity of whey, in about twelve hours, at the ordinary temperature of the atmosphere. This mixture was kept in an open test glass three weeks, but it never became putrid.

The remainder of the solution was accidulated with dilute phosphoric acid, and then a thin mixture of chalk and water was added drop by drop till effervescence ceased. The mixture was allowed to stand for twenty-four hours and the clear fluid removed.

The precipitate was treated with very dilute hydrochloric acid, and the result treated with a saturated solution of pure cholestearine made by Beneke's method, in a mixture of absolute alcohol and absolute ether. The mass which separated was then dissolved in absolute ether, and in the resulting water was suspended a greyish flocculent matter which, on examination was found to be perfectly amorphous. It was dried at a temperature of 42°, and weighed, roughly, a third of a grain. It was partially soluble in distilled water, not at all in boiling water, greatly soluble in glycerine, and it produced the characteristic viscid change on a small quantity of fresh milk.

Fluid was taken from three nepenthe pitchers which had not opened their valves, to the amount of 2·3 cubic centimetres. It was treated in the same way as described above, and yielded a trace of the flocculent matter. Seven cubic centimetres of fluid from pitchers which had been long open and contained abundant insect *débris*, yielded the same flocculent substance. It has a specific gravity fractionally greater than water, and has reactions quite similar to the substance separated from the *D. dichotoma*, and which I propose to call droserine.

At Mr. Darwin's suggestion I have tried the action of the fluid of four virgin pitchers of the *Nepenthe phyllamphora* on cubes of albumen one millimetre in measurement. After twenty-eight hours immersion there was no indication of change by any one of the four fluids. Yet the chemical differences in all four were very marked. One only was viscid, yet it contained not a trace of the grey flocculent matter which I regard as the ferment. One only was at all acid, the other three being absolutely neutral. One contained quite a large quantity of the ferment, whilst the fourth had no reaction in silver lactate, so that I imagine it was only pure water. On the contrary, fluid taken from pitchers into which flies have previously found their way is always very acid, has a large quantity of the ferment, and acts in a few hours on cubes of albumen, making them first yellow, then transparent, and finally completely dissolving them.

The quantities obtained were too small to submit to analysis, and I am not sufficiently an adept in chemical manipulation to give a better account of this interesting substance.

When studying the nepenthes, I was puzzled to see the use of the channel which exists on the back of the pitchers, and which is formed by two ridges furnished with spikes in most of the nepenthes, but not in all, which run up to the margin of the lip of the pitcher.

I found that one plant under observation was infested by a small red ant-like insect, numbers of which had found their way into one particular pitcher. I observed two or three on the leaf of this pitcher, and I carefully observed their movements. They occasionally approached the edge of the leaf, but always turned back when they encountered the spikes which run down the margin, and which are the same as are seen on the ridges. In all the mature pitchers the stalk hangs in contact with the pitcher just between those two ridges, about half way between the attachment of the stalk and the lip of the pitcher.

At this point of contact the insects marched on to the pitcher, and then, of course, found themselves on the pathway between the ridges. Here they again always turned back when they encountered the spikes, so that they soon found their way to the lip.

Here they paused, and seemed to enjoy some secretion which seems to be poured out on the glazed surface of the lip. Then they travelled onwards, and met the fate of their companions. I found about thirty of these insects in this pitcher, and as they were in various stages of digestion, I presume they were entrapped at different times. I could see no reason why they all went to this pitcher, though no doubt there was one. The secretion in which they were being digested was very viscid and very acid. In the unopened pitcher the secretion is only faintly acid and not at all viscid. The secretion is increased, therefore, as Mr. Darwin has shown to be the case in *Drosera*, in quality after food has been taken in.

The footpath extending from the petiole to the lip of the pitcher, armed on each side with a *chevaux-de-frise*, to prevent the prey wandering off, is a contrivance which is manifestly for the advantage of the plant; so also, is the umbrella which is extended over the orifices of the pitchers in many of the nepenthes. Its obvious use is to prevent dilution of their gastric juice. In some the lid does not cover the orifice; probably there is something special in their habits.

The glands which line the pitchers differ considerably from the *Dionæa*, and they are placed in curious little pockets of epithelial cells, the meaning of which is not evident.

LAWSON TAIT

Curious Phenomenon in the Eclipse of 1927

ON the morning of June 29, 1927, there will be the next solar eclipse in England in which anything in the shape of totality can be seen. In an examination of eclipses I made two or three years ago, I considered this one would be total for a brief period in the north of England, as mentioned in NATURE, vol. xii. p. 213. But the curious point worthy of notice is the following:—As the moon's disc only just overlaps that of the sun, we may expect to see the red flames visible, not as prominences, but as a line of red light encircling the sun for a few moments. The probable appearance of such a phenomenon in a slightly total eclipse of the sun was pointed out by Prof. Grant in a paper in the December Notices of the R.A.S., 1871 (q.v.) The eclipse of June 29, 1927, seems to afford such an opportunity as the Professor wished to find out. Although this eclipse, therefore, is but an apology for a total one, it may acquire an interest of its own for posterity. See my little work, "Eclipses Past and Future" (Parkers) on this subject. SAMUEL J. JOHNSON

Upton Helions Rectory, Crediton, Devon

Spectroscopic *prévision* of Rain with a High Barometer

MY letter of last Monday (in last week's NATURE, p. 231) having been sent off when we (in Edinburgh) were still in the