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