

electrification; cf. *Amer. Jour. Sci.*, xv., 105) is rarely neutral as a whole. But it vanishes almost completely while the number of nuclei is relatively constant. In general, diminutions which are questions of seconds or minutes with the ions are more than questions of hours with the nuclei.

Just as in these cases there is no marked decrease of the number of nuclei while the ions all but go, so I have been unable to find any contemporaneous increase of number; and yet in my experiments with phosphorus and with water nuclei the activities of any generator for the simultaneous production of nuclei and of ions seem to increase and decrease together. I shall be able to state this more definitely at the conclusion of my present experiments on the efficiency of different types of water jets.

Finally, in my "Experiments with Ionised Air" (p. 12), I showed that in case of tests made with the steam jet, nuclei produced by the X-rays in atmospheric air were persistent in like degree with phosphorus and other nuclei. In fact, there was little difference in this respect among the nuclei examined. Nuclei produced in dust-free air, saturated either with water vapour or with hydrocarbon vapour, by the X-rays acting from without, retain the same order of persistence, whereas the ionisation is known to be fleeting. True, rubber stoppers and tubes made up a part of my condensation chamber, but in the case of water nuclei, at least, I can see no objection to this. The entire absence of electric field is always understood.

In all cases, therefore, the electrification vanishes and leaves a nucleus behind, sometimes larger, sometimes smaller. If, in any one of them, the nucleation and the ionisation vanished at the same rate, the case would be good presumptive evidence of their identity. But, to my knowledge, never does this occur. What justification is there, then, to call the phosphorus nucleus an "oxide," or if an oxide associated with ionised air, why does one not find the smaller air nuclei? I should answer that the phosphorus nucleus is the stable product of the initially ionised field. Again, why is phosphorus and dry air a more complicated system than air and water vapour under the action of the X-rays? Out of both systems eventually issues a stable nucleation. And why may one attribute to ionised air different condensational properties, according as positive or as negative ions are in question, without having first established that the corresponding air nuclei do not differ in size sufficiently to account for the condensational difference observed? Why may one condense on a nucleus from which the soul has fled and still be permitted to call it an ion? Why, indeed, does the nucleus persist after the ionisation has vanished; why does one not get back to dust-free air? My answer would be as in the case of phosphorus. As to water nuclei, I am much in doubt ever since I have been able to arrest the finest fog particles for examination whether the nucleus from shattered water is mere water dust. It seems to me, therefore, that electrification, if present simultaneously with nucleation, is an incidental accompaniment with no immediate bearing on the condensation produced, and for this reason I have in the above endeavoured to account for the nucleus at the outset chemically. CARL BARUS.

Brown University, Providence, U.S.A.

I do not think that any worker with ions or with condensation nuclei who may have read the papers on "Experiments with Ionised Air" and on "The Structure of the Nucleus" will consider my criticism unjust.

The latter part of the letter requires some reply. According to Prof. Barus, in all cases studied by him the nuclei were distinct from the ions, persisting long after the ionisation had disappeared. All that this proves is that he has not yet succeeded in observing condensation upon the ions, but only upon nuclei of another kind. According to my experiments (*Phil. Trans.*, vol. xciii. pp. 289-308, 1899), a fourfold supersaturation is required to cause condensation on the negative ions, a sixfold being required for the positive ions. To get such high supersaturations as these an exceedingly rapid expansion is required, and it is probable that the apparatus used by Prof. Barus is unsuitable for the purpose. In the presence of any considerable number of nuclei requiring inappreciable supersaturation

(as persistent nuclei always do) to cause water to condense upon them, it must be particularly difficult to reach the supersaturation necessary for condensation upon the ions. Such persistent nuclei always were present in Prof. Barus's experiments; his failure to get condensation upon the ions was thus to be expected. His results have no bearing, therefore, upon the interpretation of my experiments on the action of the ions produced by X-rays and similar agents on condensation (for in these experiments nuclei more persistent than the ions were absent), nor of the experiments upon the charge carried by the ions made by Prof. J. J. Thomson (*Phil. Mag.*, vol. xlv. p. 528, 1898, and vol. v. p. 346, 1903) and by Dr. H. A. Wilson (*Phil. Mag.*, vol. v. p. 429, 1903) with the same form of rapid-expansion apparatus as was used by me.

I have never been able to produce by the action of X-rays nuclei other than the ions, but possibly very intense radiation may do so, as ultra-violet light certainly does.

C. T. R. WILSON.

Cavendish Laboratory, November 23.

### Weather Changes and the Appearance of Scum on Ponds.

SOME experiments which I have been making during the last year seem to bear very directly upon the interesting phenomenon described by "Platanus orientalis" in your issue of November 5. These experiments show that numerous solid substances suspended or dissolved in water have, by virtue of their surface-tension relations, a marked tendency to accumulate at any surface separating water from gas (*vide Proc. Roy. Soc.*, August). Hence, by merely passing a stream of air-bubbles through solutions or suspensions of certain solids in water, it is possible to effect a considerable concentration of the dissolved or suspended solid in the upper layers of the liquid. Each bubble carries with it to the surface a load of solid particles, and leaves many of them floating there either as an ultra-microscopic "pellicle" or as a visible "scum." If a bubble is very minute, its load may be so great in relation to its volume that it may be entirely unable to rise, or may even sink. If, in these circumstances, the barometric pressure be diminished, the volume of the bubble increases in greater proportion than the surface-area, and therefore than the maximum load, with the result that numerous bubbles previously unable to ascend at once begin to rise towards the surface. If, during their ascent, the barometric pressure be sufficiently increased, at once they sink. If a vessel of water containing a sediment of sulphur or calcium soap, &c., be exposed to a sufficiently diminished air-pressure, the whole of the sediment will be seen to rise to the surface, the minute air-bubbles with their coating of solid acting like so many "Cartesian Divers."

In every ordinary pond gas-bubbles of various kinds are constantly being formed by the action of micro-organisms; in nearly every pond various solid substances, both organic and inorganic, possessing the required surface-tension relations, are present both in the mud and in suspension. The gas liberated will be constantly bringing scum-forming material to the surface, whether it rises in large masses or in small bubbles. Either a fall in the barometric pressure, or a rise in temperature, or an increase in the activity of the gas-producing organisms should therefore result in increase of the scum. It must, however, frequently happen that the scum is swept to one side by the wind or sunk by various mechanical disturbances.

It would be extremely interesting to learn whether by "decided change in the weather" your correspondent means a change attended by a falling barometer.

Pembroke College, Oxford.

W. RAMSDEN.

### The "Affenspalte" in Human Brains.

WILL you kindly allow me the privilege of using your columns for the following note? In a recent number of the *Anatomischer Anzeiger* Prof. Elliott Smith published a most interesting forecast of an extensive work which he has in hand, dealing particularly with the occurrence in human brains of an occipital operculum; this occurrence had been considered previously as very exceptional, but Prof. Elliott Smith is able to show that this is far from being the case.

The presence of such an occipital operculum implies the existence, in the cerebral hemisphere possessing it, of a sulcus, called by Prof. Elliott Smith the sulcus lunatus, which is strictly comparable to, if not absolutely identical with, the "Affenspalte" so typical of the brains of Simiidae and Cercopithecidae.

The examination of cerebral hemispheres of representatives of the lower human races is naturally suggested, and the aborigines of Australia, from several points of view, seem particularly appropriate in this connection. Following up Prof. Elliott Smith's suggestion, I have examined the brains of the aboriginal natives of Australia in the Cambridge Anatomical Museum. As a result, four out of eight hemispheres show plainly the sulcus lunatus and occipital operculum. In one case only is the condition symmetrical in the two hemispheres. The smallest brain of the four bears a sulcus lunatus and operculum on one hemisphere only. Where the sulcus lunatus is interrupted, compensation seems to be provided by deepening of the inferior occipital sulcus.

A Chinese brain in my possession has in each hemisphere a sulcus lunatus.

I shall be much obliged if you can kindly place these observations on record.

W. L. H. DUCKWORTH.

November 27.

#### The Rate of Nerve Impulses.

DR. ALCOCK, in his recent paper at the Royal Society, finds the rate of transmission of nerve impulses in man to be 65 metres per second. Sir Michael Foster, in his "Physiology" (1888, part i. p. 76), gives it as 33 metres per second. The difference is considerable, and places us in a dilemma:—(1) either Sir Michael Foster or Dr. Alcock is widely wrong; or (2) the rate of transmission has become greatly accelerated during the last fifteen years. Of the two, the latter seems to me the simpler explanation.

W. R. GOWERS.

#### The Leonids of 1903.

OBSERVATIONS were begun on November 15 at 17h. 57m. and continued until daylight rendered further watching useless. In the first five and a half minutes twenty meteors appeared, all but two of which were Leonids, so that the hourly rate of the latter was 200. This period seems to have been about the time of maximum, judging from the results of other observers. Shooting stars now began to diminish in frequency, as the sky was brightening as day approached, but in the half hour comprised between 17h. 57m. and 18h. 35m. (deducting time spent in recording) thirty-six were seen, thirty-four being Leonids. Beyond 18h. 35m. the twilight was too strong to expect to detect meteors, and though the look-out was continued until 18h. 57m. no more appeared.

The display was certainly very fine, Leonids shooting one after the other in various parts of the heavens, the effect being heightened by the crescent moon and Venus, shining resplendently side by side in the south-east. Most of them were bright, the average magnitude being 1 or a little greater. As is usual with the meteors of this shower, they moved swiftly and left streaks. The prevailing colours were blue and yellow.

The radiant point, as indicated by ten registered paths, was at  $148^{\circ} + 22^{\circ}$ .

The chief observed Leonids were:—

November 15.

Time h. m.	From	To	Mag.
18 7½	139½ - 6	138 - 9	> 1
18 10½	174 + 20½	179½ + 19½	= 2
18 20	110 + 9	106½ + 7	> Sirius
18 34½	139½ + 33½	132½ + 38½	= ♀

On the following night the sky was watched from 12h. 10m. to 14h., but though it was clear most of the time, only two Leonids were observed, and meteors generally were scarce. On November 18, from 18h. 5m. to 18h. 20m. no shooting stars appeared.

These two latter watches bring out an important fact, namely, that the shower very rapidly declined in strength after the maximum had been passed.

Sheffield, November 27.

ALPHONSO KING.

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#### ACCOMMODATION OF SCOTTISH SCIENTIFIC SOCIETIES.

IN response to a requisition signed by seven fellows, a special meeting of the Royal Society of Edinburgh was held on the afternoon of Thursday, November 26. There was a large attendance. The president, Lord Kelvin, occupied the chair; and Sir John Murray, seconded by Dr. John Horne, moved the following resolution, that.

"This meeting of the Fellows of the Royal Society resolves to instruct the council to enter into formal communication with the other scientific societies having their headquarters in Edinburgh with the view of concerting measures for obtaining the use of the Royal Institution building wholly and exclusively for Scottish scientific societies."

The resolution was also supported by Prof. Cossar Ewart, Prof. Chiene, Dr. Munro, Dr. Buchan, Prof. Hudson Beare, Sir James Russell, and Prof. Chrystal. The last named, in his official capacity as secretary, referred to the history of the relation between the society and the Board of Manufactures for Scotland; while most of the other speakers spoke from the point of view of the various other societies of which they were members, such as the Royal Scottish Geographical Society, the Royal Society of Arts, the Meteorological Society, the Royal Physical Society, the Geological Society, the Mathematical Society, &c.

All who spoke were unanimous in their opinion as to the importance of the scientific societies having their rooms and libraries in one building. The advantages of such a combination are evident to all interested in the progress of science, and need not be enlarged on in these pages. But there are features peculiar to the present movement which deserve to be widely known. These were touched upon and in many cases emphasised by Sir John Murray and those who supported him.

One of the most striking architectural ornaments in Princes Street is the Royal Institution, erected in 1828. The Royal Society has always occupied the west wing of the building, and the rest is at present mainly devoted to art in the form of a statue gallery and schools of art. Several rooms are used by the officials of the Board of Manufactures, the reorganisation of which forms the subject of an important report recently made by a departmental committee specially appointed. So far as this report has to do with the Royal Society, it is in practical agreement with the claims advanced by that body, as given in the evidence of the secretary, Prof. Chrystal. These were that the society should have increased accommodation for its growing library, and should sit rent free and in perpetuity. It was pointed out by witness after witness before the committee that the building is unsuitable for art, and the committee accordingly recommends the construction of a new building for national galleries and art schools. Should the people of Scotland carry this recommendation into effect, the representatives of art will evacuate the Royal Institution, and the question will arise as to the best use to be made of the rooms. The Royal Society cannot effectively occupy the whole building, and it is under these conditions that Sir John Murray brings forward his plan for the concentration of scientific effort in the capital of Scotland. Very little internal change in the building would make it suitable for the purpose, and there is a large central hall which would serve admirably for scientific meetings of wider scope or of a popular character.

An "equivalent grant" to Scotland of 2000l. dates from the Union of the Parliaments, and the Board of Manufactures was appointed shortly after that time to