do not support the conclusion that the activity of these salts is a *normal* atomic property of potassium, and that it is always directly proportional to the amount of that metal present in the salt.

In measuring and comparing the activities of the different salts, these were spread in turn in thin regular layers on a shallow tray, which was placed on the bottom of an ionising chamber 40 cm. long, 26 cm. wide, and 28 cm. deep. The saturation currents through the air in this chamber were measured with a sensitive quadrant electrometer, and were taken as measures of the activities of the different salts. Experiment showed that the saturation current increased with the thickness of the salt layer up to between 2 mm. and 3 mm., and for greater thicknesses remained constant.

The investigation included the examination of some thirty specimens of potassium salts, and thirteen samples of the salts of the other alkali metals. As a result of this examination it was found:—

(1) That samples of a selected potassium salt obtained from different sources exhibited widely differing degrees of activity. Two chlorides of potassium, for example, were found to differ by more than 40 per cent. in their activities, and two of hydroxides by an almost equal amount. In the case of cyanide of potassium, the variation in activity was especially marked, as two samples of this salt exhibited activities which were approximately only 5 per cent. and 20 per cent. respectively of that shown by a number of other samples of the same composition. A sample of potassium sulphite, too, was found to possess an extremely small activity.

With the majority of the salts, however, the variations were not so marked, but the differences observed, even when due allowance was made for the varying densities and states of division of the salts, were so extensive and of such magnitude as practically to preclude the view that the activity of potassium and its salts was connected with a *normal* atomic property of the metal.

(2) That while metallic sodium and several sodium salts did not exhibit the slightest trace of activity, some samples of sodium chloride, obtained in the form of rock salt, showed an activity comparable with that exhibited by a number of the potassium salts. This result, taken in conjunction with the low value obtained by Elster and Geitel in their measurements on the conductivity of air in a salt mine, would indicate that very probably some active impurity was present in the samples of rock salt examined.

(3) That with the exception of ammonium chloride, which exhibited a feeble activity, none of the lithium and ammonium salts examined showed the slightest trace of radio-activity; that a sample of rubidium alum was found on examination to exhibit an extremely small activity, and that a sample of caesium chloride exhibited one which was only just measurable.

J. C. McLENNAN.

University of Toronto, April 15.

Chemical Analyses of Water from Dew Ponds.

I HAVE been interested in the reviews of books and articles which have appeared in *NATURE* from time to time on the subject of dew ponds; and it occurred to me that the chemical analysis of the water of these ponds would help to settle the question of the origin of the water. This is a method used to some extent by sanitary authorities. I have had the opportunity of obtaining some specimens of water from different districts, and the specimens have been analysed by Mr. Claude Saville Grace, one of the students at this institute.

The first specimen (a) came from a dew pond on the southward down to the north of Ramsbury, Wiltshire. The pond is on the flat upland near the 693-foot mark on the 1-inch Ordnance map south of Aldbourne. The formation is chalk, so that analysis would immediately settle the point as to whether the water had come through the chalk or had been condensed from the air. The other two specimens, (b), (c), come from St. Boniface Down, north of Ventnor, Isle of Wight. The pond (b) is near the 787-foot mark, almost at the highest point of the down; the second pond (c) is on the neck between St. Boniface and Shanklin Downs. The downs are chalk masses lying on Greensand rock. I have added the analysis of the St. Boniface spring

water, a spring on the south side of the down facing Ventnor, about 450 feet above sea-level. It is locally known as a wishing well, and its chemical peculiarity is that it contains sulphuretted hydrogen in small quantity. It undoubtedly comes out of the chalk, and the sulphuretted hydrogen is due to the decomposition of pyrites which occurs in masses in the chalk.

	CaCO ₃ parts per 100,000	Cl parts per 100,000	
(a) Ramsbury pond	3.6	0.75	inland
(b) St. Boniface pond	4.3	3.5	near sea
(c) Shanklin pond	7.7	3.4	"
(d) St. Boniface Wishing Well.	23.9	7.3	traces H ₂ S

The quantities of CaCO₃ indicate to me that the waters are, in the cases (a), (b), (c), condensed waters which have been lying in the pond sufficiently long to take up a little CaCO₃. The ponds near the sea show increased chlorine, probably from salt spray blown up from the sea. The sulphuretted hydrogen shows the origin of the wishing-well water in the deep chalk.

To me there is very little doubt that all three ponds are simply water butts in which rain water is stored. Inspection of the ponds shows that they have much larger catchment areas than simply the water area, and the area is generally more than nine times that of the pond. We have to remember that for circular ponds and catchment areas of radii 1 and 3 respectively a rainfall of 20 inches would mean a depth of 180 inches (15 feet) when collected into the pond area, so we can easily understand the presence of water all the year round under these conditions.

It is interesting to note that a fresh-water pond is easily distinguished from the hard-water pond by the waterweed growing in the former. I have noticed the same carex in all the fresh-water ponds.

SIDNEY SKINNER.

South-Western Polytechnic Institute, May 8.

The Reflection of Distant Lights on the Clouds.

I DO not know whether observations have ever been made to determine how far the reflection of distant lights on the clouds may be seen. It may possibly, however, be of some interest to know that the lights of London may at times be seen in this way at a distance of at least fifty miles. At 11 p.m. on April 30 the reflections of the lights of several neighbouring towns were unusually bright as seen from here. The altitude of the Portsmouth glare was about 10°; the distance of the centre of Portsmouth is about 12.5 miles; the cloud height was therefore about 2.2 miles. Over Hindhead and Blackdown a bright band of light was visible. Circumstances prevented me from measuring its altitude, but I estimated it as one or two degrees. Now London lies exactly in this direction, and fifty miles would bring one to the well-lighted area of south London. If the cloud height were uniform, the altitude of the reflection at this distance should have been a little more than 2°. The only other large town in the same direction is Guildford; the altitude of its glare should have been 5°. I do not think I could have made so large an error in estimating the altitude, but apart from this the Guildford glare would not stretch along the horizon for more than 2°, while the observed band of light stretched for at least 10°, and possibly more, for trees bounded the view to the west and the downs to the east.

CHARLES J. P. CAVE.

Ditcham Park, Petersfield, May 9.

Jupiter's Eighth Satellite.

THE discovery at Greenwich Observatory of Jupiter's eighth satellite, its great distance from the planet, and its retrograde motion, have excited the interest of the astronomical world.

Until more extended observations have led to a more certain knowledge of the orbit, speculation is premature. But it is impossible to resist the conjecture that there is a bare possibility that the object is really the long lost Lexell's comet, which in 1770 was describing an elliptic orbit with an eccentricity of 0.7858, with a periodic time

of $5\frac{1}{2}$ years, in a plane inclined $1^{\circ} 34'$ to the ecliptic; the next return of which it was impossible to observe from the earth's position; which about 1779, August 23, approached Jupiter within 0.01 of the earth's mean distance from the sun; and which has not been again observed.

At that distance Jupiter's attraction exceeds that of the sun in the proportion of at least 200 to 1, and the distance from one of the satellites may have been very small. In this way it is not altogether impossible that the comet may have been diverted into an elliptic orbit round Jupiter, and a retrograde motion round the planet would be as likely as a direct motion to ensue. The intervention of a satellite is essential, and this, combined with the observed large angular distance from Jupiter in one part of its orbit, implies a large eccentricity. Should further observations reveal a moderate eccentricity, the impossibility of identity between this satellite and Lexell's comet will be proved.

The Athenæum, May 6.

GEORGE FORBES.

The Corrosion of Iron and Steel.

In a letter to NATURE of April 16, Dr. Frank Clowes states his belief that pure lead will dissolve to a slight extent in perfectly pure water "as iron did in the experiments made by Whitney and repeated by the American investigators, when they brought iron into contact with water under conditions similar to those which I had secured."

The American investigators here referred to are W. H. Walker, Cederholm, and Bent, who have recently studied the process of the rusting of iron (Journ. Amer. Chem. Soc., 1907, xxix., p. 1251), and agree with Whitney in regarding the whole subject as an electrochemical one.

With reference to the behaviour of lead in contact with water, I have nothing to say, for although the study of the corrosion of metals has been pursued by me for several months, my observations have not as yet been extended to lead. I wish to point out, however, that if this metal does dissolve to an appreciable extent in water as Dr. Clowes suggests, then the analogy adopted by this gentleman is open to criticism. I have made a very careful study of the process of the rusting of iron, and my results point conclusively to the fact that it is primarily the result of acid attack. These results will, I hope, shortly appear in print; it is unnecessary, therefore, to enter into any detail here. Nevertheless, one or two general remarks may prove of interest.

Walker and his co-workers found that if water in which iron has been boiled is concentrated to a few drops, the presence of iron can be detected by chemical means. It is clear that if the concentration of the iron in solution can be increased by the evaporation of the water, as these authors say, the same result should be obtained by protracted boiling of the water and iron, keeping the volume of the former constant by using a reflux condenser.

My experiments show, however, that such is not the case. The presence of traces of iron discovered by Walker, Cederholm, and Bent is not, therefore, to be ascribed to the action of pure water alone, but to that of traces of dissolved carbon dioxide.

In a very interesting series of investigations, Leduc (*Comptes rendus*, 1906, cxlii., p. 149) has recently shown that all the dissolved air cannot be expelled from water by the mere process of boiling. He further calculates that at least 1 c.c. of gas remains in a litre of water even after thorough boiling. Since carbon dioxide is not only much more soluble than oxygen and nitrogen, but also combines with the water to form carbonic acid, it is not unreasonable to suppose that a considerable percentage of this residual gas is carbon dioxide. This will effect the solution of a trace of iron. Although this quantity may be too minute to detect at once by chemical means, its concentration can be greatly increased by evaporating to small bulk, when the characteristic reactions for iron may be obtained. On the other hand, protracted boiling with reflux condenser cannot increase the concentration of the iron, since the amount of carbon dioxide remains the same. The extreme difficulty of removing all traces of this gas from water is not generally realised by chemists, and the precautions adopted by Walker and his co-workers were not sufficiently refined—hence their results. A single

molecule of carbonic acid is sufficient, theoretically, to cause the corrosion of iron (see my remarks, NATURE, September 27, 1906), and I have shown that in practice a very few molecules are operative.

There can be no reasonable doubt, therefore, that the electrolytic theory is wholly inadequate.

In a letter to NATURE of October 11, 1906, Mr. Richardson asks if the rusting of iron is not caused by bacterial agency. As no reply has been given, it may not be out of place to deal with the question here. The suggestion is not new. Schorler, Beythien, Adler, Raumer, and others have directed attention to a bacterium, *Gallionella ferruginea*, which obtains its life energy by oxidising ferrous carbonate or organic ferrous salts with the precipitation of rust. But no organism has been found capable of feeding on metallic iron. Indeed, if one such were found, it would have to effect the solution of the iron by some presumably acid secretion, and this brings us back to the acid theory of rusting.

J. NEWTON FRIEND.

Fault Lines in the Atlantic.

IN Prof. J. Milne's discourse at the Royal Institution which appeared in NATURE of April 23 is given an interesting map on p. 593 showing the folds and probable direction of fault lines in the Atlantic. In that map is shown the mid-Atlantic "rise" extending to about 40° S. The map, however, would have been more interesting had Prof. Milne included in it the recent bathymetrical researches of the *Scotia*, which were described in a preliminary paper in the *Scottish Geographical Magazine* in August, 1905. Here it is shown that the Scottish expedition sounded out this "rise" for a thousand miles further south than Prof. Milne has marked it, and also that that "rise" has probable connection with another running in a more or less E.N.E. and W.S.W. direction from the south of South America through South Georgia and the Sandwich Group, and also from Graham Land, Antarctica, through the South Orkneys and the Sandwich Group, thence passing eastward through Bouvet Island to Madagascar and the east coast of Africa. The extension of the mid-Atlantic "rise" continues the reflection of the South American continent, and again the transverse "rise" reflects the hypothetical and known portions of the coast-line of Antarctica.

WM. S. BRUCE.

The Pollination of the Olive.

As Knuth's great work on flower pollination, compiled from all available sources, says nothing whatever about the olive, I recently asked Prof. J. E. Coit, of Arizona, to look out for insects upon the flowers. He carefully examined many olive trees in flower at Tucson, and did not find a single insect at the flowers, with the exception of a thrips (apparently *Euthrips occidentalis*, Pergande), which occurred in great numbers. He kindly sent me some twigs with the Euthrips upon them, and I was able to observe that these insects were profusely dusted with pollen. No bees were seen at all. Prof. Coit adds:—"Olive pollen is formed and shed in such enormous quantities that I think the wind among the branches is the chief agent in pollination. If you jar a large branch of olive while it is in full bloom, a perfect cloud of green pollen will be seen to float away on the breeze."

Arizona, however, is not the original home of the olive, and the purpose of this note is to suggest that those who have an opportunity to see the plant in bloom in Mediterranean and other countries should make some observations.

T. D. A. COCKERELL.

University of Colorado, May 2.

The Coloration of Birds' Eggs.

I SHOULD be very grateful if anyone could tell me whether there is anything known with regard to the egg-colour of birds. Is there here, by any chance, a field for investigation which may throw light upon the so-called Mendelian phenomena? Or is there yet any explanation of how the egg becomes coloured, or why?

66 Hallam Street, W.

R. L. LESLIE.