

difficult. But this Prof. Birkeland says was likewise true of Toronto, yet Dr. Krogness retains this station; why he rejects Honolulu Dr. Krogness does not say. Again, he overlooks the fact that when he corrects Birkeland's scaling for San Fernando he has improved the easterly progression—Prof. Birkeland's value was nearly two minutes too high. In view of the uncertainties in Prof. Birkeland's scalings revealed by Dr. Krogness, and as Prof. Birkeland fails to specify the particular element considered, not full weight could be attached to this disturbance in the above table. It should also be stated here that Prof. Birkeland considered, in all, six characteristic points of the disturbance curve, and my result was based on *all* the scalings—seventy-two in number—and not merely the half-dozen taken by Dr. Krogness. Did I myself consider such limited data as Dr. Krogness uses adequate for the purpose, I might point out that his own figures show an easterly progression of the times on the order of what is to be expected, which would have been still further accentuated had he not rejected Honolulu.

Just as I am preparing this reply, I am in receipt of a letter from Dr. Chree, dated December 6, 1910, accompanying a copy of the proof-sheets, which he kindly let me see, of his paper before the Physical Society, November 11; he had also given a paper on the same subject at the British Association meeting. He is not in agreement with my general deductions or with those of Mr. Faris. His criticisms are in part covered by the foregoing reply to Dr. Krogness, and in part by my article in the December (1910) issue (*loc. cit.*). I can only say here that I cannot agree with Dr. Chree in several of his own deductions, and especially with regard to the possible inaccuracy of Mr. Faris's time scalings: I beg to refer him to pp. 213-4 (*loc. cit.*). Nor can I enter here into a discussion with regard to his criticisms of my hypothesis of ionic currents, for it would seem that he has unintentionally put into his discussion ideas which are new to me. I will only remark that nowhere in my papers have I supposed such a simple overhead electric current in the plane of the geographical equator as postulated by him; this is best shown by my mathematical analysis.

In conclusion, I would like to state my position once more, viz. *even our most sudden magnetic storms begin at measurably different times for various stations distributed over the globe. The data thus far available would show that the Greenwich mean times of beginning increase more often in an eastward direction than in a westward one.*

Our explanations as to the cause may differ, but I believe what I have just stated to be an actual fact.

L. A. BAUER.

Washington, D.C.,
December 19, 1910.

Tribo Luminescence of Uranium.

MOISSAN first directed attention to the pyrophoric properties of metallic uranium. The luminosity shown on shaking a bottle containing metallic uranium is due to the oxidation of small particles of the metal. Uranium is a hard but brittle metal; when pieces of it rub together small particles are knocked off, and if these are neither too small nor too large the friction may be sufficient to heat them above 170° C., at which temperature uranium inflames in air. The presence of smaller particles, which do not inflame visibly in air, is shown by their incandescence in a gas flame lit by the "spark" from the metal. The luminosity obtained by rubbing metallic uranium is not the same class of phenomenon as the luminescence produced by shaking a tube containing uranium nitrate; the latter has been described as tribo luminescence (Wiedemann). If the tube containing metallic uranium is filled with hydrogen no luminosity is obtained, whereas the luminescence of the uranium nitrate is unabated in such an atmosphere. The sparks obtained from uranium are hot enough to kindle a gas flame or explode a mixture of hydrogen and oxygen; in fact, I have been able to work a petrol engine by igniting the gas charger by means of such sparks. The luminescence

of the uranium nitrate crystals, on the other hand, is unaccompanied by any considerable rise in temperature. Pyrophoric properties similar to uranium are shown to a remarkable extent by Welsbach's alloys of rare earth metals and iron.

Tribo luminescence is shown by a large number of organic and inorganic compounds, e.g. arsenic trioxide, uranium nitrate, potassium sulphate, zinc sulphide, quinine valerate, aniline hydrochloride, benzoyl β -naphthylamine. Crystallo luminescence, or the luminosity produced during crystallisation, is practically the same phenomenon, being caused by the fracture of crystals after formation; it is well shown by mixtures of sodium and potassium sulphate. Tschugaeff found a connection between the optical activity and the tribo luminescence of organic substances, but Gernez has disputed the existence of any relation between them. Substances that phosphoresce readily under X-rays generally show tribo luminescence, and the connection between the two phenomena is accentuated by the observations of Karl, which show that quite pure inorganic substances do not show tribo luminescence. It is remarkable in view of the radio-activity of uranium that salts of this metal should show phosphorescence and tribo luminescence to such a degree; Karl has found, though, that quite "pure" uranyl acetate does not show tribo luminescence, while Tschugaeff mentions that the chloride and sulphate also do not exhibit this property, though they are all phosphorescent. The tribo luminescence of crystals may be likened—though analogies are dangerous guides to theories—to the bursting of an elastic band with a snap; when the cohesive forces between the molecules of the crystal are overcome the electrons are disturbed, and light waves result, while substances which easily phosphoresce or are radio-active would the more readily have their electrons disturbed.

Mr. Rudge mentions that the yellow oxide of uranium shows slight tribo luminescence; I could only obtain the effect by fairly vigorous rubbing in a mortar, and as the oxide changes to a dark colour with this treatment, the luminosity may be due to oxidation.

Mr. Rudge's letter directs attention to two interesting but distinct phenomena.

ALFRED C. G. EGERTON.

R.M.A., Woolwich.

The Clarification of Liquids by the Process of Tanking.

I SHALL be glad if any of your readers can give me information upon the following problem. In the clarification of liquids by the process of tanking, the settled clear liquid is drawn off from a tap situated on the side of the tank above the muddy layer. When the tap is turned on, does only the liquid above the tap run out or does some of the liquid below the tap run out also? In the special case of tanking oils, there is very little difference in specific gravity between the upper clear layer and the lower muddy layer. Further, how should the outlet be fitted so that on running out the upper layer the lower should remain least disturbed?

ROWLAND A. EARP.

Preston Brook, near Warrington,
December 22, 1910.

The Conduct and Song of Birds.

THIS morning, Thursday, is clear and frosty, but until now we have had constant rain. In spite of this the birds, for three or four days, have been singing as in early spring. The rooks have been visiting their old nests in the elms, and, our gardener assures me positively, have been carrying sticks and repairing their nests; this he has seen himself, and marked as exceptional. I suspect that this (unusual?) conduct and song herald a period of fine dry weather.

F. C. CONSTABLE.

Wick Court, near Bristol,
December 22, 1910.

P.S.—Fine weather here since December 22 until to-day, January 2!