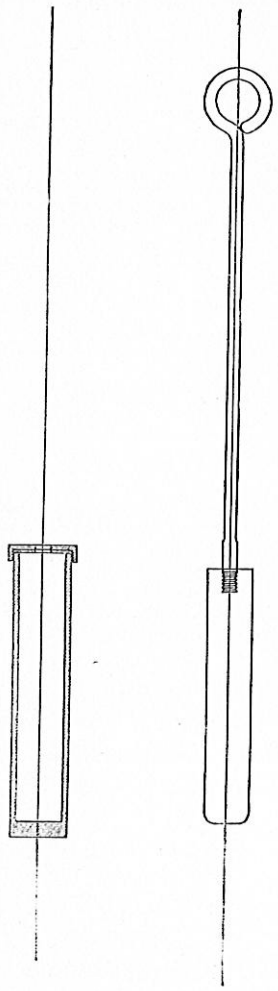
I accordingly had two sleeves prepared, one of turned and polished iron, the other left with a thick coating of oxide. Two sets of experiments, in each of which six observers took part, were made. In each set of experiments three observations were made with the polished, and three with the oxidized sleeve. In each case the observers were in the dark room for some minutes before the experiments began.

In the first set of experiments the observers gave their opinion, at the conclusion of the experiments, as a body, that the first appearance of colour was a greyish-white; as the sleeve became hotter the colour was yellow, and gradually changed into orange. There was little or no difference between the observers as to the instant of visibility; it was generally over a minute before the sleeve became visible, the light generally showing first on the generating line of the cylinder between the eye and the axis.



There was no difference in colour between the bright and the oxidized sleeves.

In the second set of experiments, the observers had no communication with one another, had no idea what colour they were expected to see, and their impressions were written down separately and independently. Their impressions were as follow, the observers being designated by α, β, &c. :—

(α) First colour visible, grey white, second colour white with a little mauve, third pale rose, fourth orange. The above was the first experiment (polished metal). The other experiments showed same colour, but no mauve seen. In the last experiment (a very low heat) the colour never passed beyond a pale yellow.

(β) For all experiments, first grey white, second yellow, third orange. Last experiment, no orange.

(γ) For all experiments except last, first white, second yellow, third orange.

(δ) For all experiments except last, first grey white, gradually becoming warmer till it reached orange.

(ϵ) First white like phosphorus in the dark, gradually getting to rose, and winding up with a reddish-orange not reached in the last experiment.

(ζ) First white with a dark shade, second yellow, third orange; no difference in any of the experiments except the last, where the temperature was lower, and the orange was not reached.

I may add that the temperature of the heating bar was a little reduced each experiment, the colours changed very slowly, and gave ample time for observation. A. NOBLE.

Poincaré's "Thermodynamics."

M. TAIT ne répond pas à mon objection sous prétexte qu'elle est sans importance. Je maintiens que nous n'avons aucun moyen non seulement d'assigner l'origine des forces électromotrices Thomson, mais encore d'en constater l'existence. Si M. Tait veut répondre, et s'il connait ce moyen, qu'il l'indique. Dans le cas contraire, s'il n'est pas en mesure de soutenir une quelconque de ses critiques, et s'il préfère un autre terrain de discussion, je suis prêt à l'y suivre.

Seulement je serai forcé d'être un peu plus long, car il me faudra passer en revue les trois reproches de M. Tait.

(1) La forme de mon ouvrage est trop mathématique.

C'est là une appréciation personnelle dont il n'y a pas à disputer. Je veux bien d'ailleurs d'une polémique sur une question de doctrine, mais non d'un procès de tendance où je jouerais le rôle d'accusé.

Toutefois il est certain que je consacre relativement peu de place à la description des expériences, et on aurait le droit de s'en étonner si je n'en donnais l'explication. Mon livre est la reproduction textuelle de mon cours; or mes auditeurs avaient tous suivi déjà un cours de physique expérimentale, où ces expériences leur étaient décrites en détail. Je n'avais donc qu'à leur en rappeler brièvement les résultats.

(2) J'ai mal parlé de la définition de la température absolue.

Autant que je puis comprendre, M. Tait ne trouve pas ma définition mauvaise, et n'en propose pas une autre; mais, dit-il, j'aurais dû parler des expériences de Joule et Thomson, qui permettent de mesurer la température absolue.

Or j'ai décrit ces expériences à la page 164, et j'ai montré à la page 169 comment elles permettent de déterminer la température absolue.

(3) J'ai laissé complètement de côté une explication mécanique du principe de Clausius que M. Tait appelle "the true (*i.e.* the statistical) basis of the second Law of Thermodynamics."

Je n'ai pas parlé de cette explication, qui me paraît d'ailleurs assez peu satisfaisante, parce que je désirais rester complètement en dehors de toutes les hypothèses moléculaires quelque ingénieuses qu'elles puissent être; et en particulier j'ai passé sous silence la théorie cinétique des gaz. H. POINCARÉ.

Ornithology of the Sandwich Islands.

HAVING just returned from an exploring expedition into the interior of Australia, on my way home I lingered in this group of islands, and was sorry to find that some species which have been obtained here are now no longer to be found.

My attention has been called to an interesting paper by Prof. Newton in your last issue (p. 465), on this subject, which seems to imply that the ornithological collection made by Sir Joseph Banks during his voyage in the *Endeavour* with Captain Cook no longer exists, which I beg to be allowed to make a few remarks upon. After the return of Sir Joseph Banks he had several cases of birds carefully mounted and arranged according to the localities in which they were collected. In one group of land birds from Owhyhee, another case contained a number of specimens from Botany Bay, conspicuous in the centre of which was a very fine specimen of the Black Swan, which was shot by Captain Cook himself.

These cases were in the custody of the Linnean Society of London until 1863, when they formed part of their Natural History sale.

These cases have been carefully preserved, and are now in the museum of my ancestor, Mr. John Calvert, together with a number of cases of birds which formed part of Sir Ashton Lever's collection, amongst which are a few from the Pacific Islands. These last cases were purchased from the executors of the late Mr. M. Armfield, of Catherine Street, Macclesfield,