

A Transmutation of Lead (Second Method).

After having shown the transmutation of lead by the method mentioned above, we applied another very successful sparking method. Extra pure lead supplied by Kahlbaum was heated in a quartz tube up to about 800° for an hour, whilst a current of pure nitrogen was bubbled through. We thus obtained a lead preparation comparable with the lead in our lead lamp, in which it was heated to this temperature in a high vacuum. From the lead obtained in this way, two lead electrodes of 14 mm. diameter were made. Between these electrodes, mounted vertically at a distance of about 4 mm. apart in a vessel with a liquid dielectric, sparks at a voltage of 100,000 volt and 2 milliamperes were sent through the liquid for 12 hours.

The dispersed lead was partly at the bottom and partly in colloidal solution. The fine lead at the bottom of the vessel was collected, washed successively in pure benzene, alcohol, and ether, and finally dried in a desiccator. The metal in the colloidal solution was converted into nitrate by treating the liquid with pure nitric acid at 100°.

After evaporating the aqueous solution on a water bath, the nitrate was placed in a desiccator.

Both substances were examined. The method of analysis was substantially a method of Jannash (*Z. für Anorg. Chemie*, 12, 143 (1900)), as modified by us. Mercury eventually present is driven out by heating in a current of air and condensed in a glass capillary. Then iodine vapour is conducted over it, forming with mercury the red modification of the compound HgI₂. This extremely sensitive reaction, if applied to the electrodes as prepared by us from the extra pure Kahlbaum lead, showed that these electrodes were completely free from mercury, but the dispersed lead obtained by sparking in the circumstances mentioned showed a very distinct mercury reaction.

Consequently, also by this second method, the transmutation of lead into mercury could be realised.

Whether thallium is also formed is being examined.

Both methods mentioned above will be applied soon to bismuth, thallium, and other elements to be considered here.

In another letter we propose to give the results of the quantitative analyses and some theoretical considerations.

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November 29.

Selective Action of Polarised Light upon Starch Grains.

THE letter of Prof. E. C. C. Baly and Dr. E. S. Semmens in NATURE of December 5, p. 817, on the selective chemical action of polarised light upon starch grains seems to me to raise a question of fundamental importance for optics. Is it possible to reconcile such an action with received views about the kinematic relations of polarised light?

Let us begin by considering a case where difficulty does not arise. Baly and Semmens refer to Padoa's observation that "crystals of *o*-nitrobenzaldehyde are selectively decomposed by polarised light, provided that the crystals are correctly oriented to the plane of polarisation" (my italics). I have not had the opportunity of consulting Padoa's paper, and know nothing of the details; but there is no obvious *a priori* reason why such an action should not occur: for the molecules in the crystal are all similarly oriented, and may well be less able to withstand a displacement in one particular direction than in the perpendicular one.

It is easy, however, to pass to a case where the

difficulty does arise. Suppose the crystal pulverised, and the fragments shaken up so as to be oriented at random. They cannot now be all favourably oriented to the polarised beam. Some of them, on the other hand, will be more favourably oriented to a beam polarised at right angles to the first; and if both beams were present, they would presumably add their effect. We can get the two beams instead of one by simply removing the polariser, or, in other words, using unpolarised light. It seems clear that in this case polarising the light (with inevitable loss of half of the intensity) must diminish the chemical action. The same reasoning seems to apply with equal force to the case of the starch grains, assuming that these are oriented at random.

Great caution is required in weighing any *a priori* reasoning against what seems the result of direct observation. Both, however, involve the human element, and both are therefore liable to be wrong.

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December 14.

It was with much interest that I read the letter relating to the action of polarised light on starch hydrolysis contributed to NATURE of December 5 by Prof. Baly and Dr. Semmens. This letter, since the writers refer to a short "note" by myself in the *Annals of Botany* (July 1925), calls for a brief reply.

Prof. Baly and Dr. Semmens are, like myself, under misapprehension. I desired to call in question, not the possibility of the phenomenon occurring, but the trustworthiness of the evidence put forward by these authors in their paper (*Proc. Roy. Soc.*, 97 B, 250, 1924). The fact that a large amount of evidence in favour of the occurrence of this phenomenon may have been published by the authors or others elsewhere appears to me to be of great interest but quite irrelevant in so far as objection is taken to the "note" for which I was responsible. For example, Weigert's observations on the effect of polarised light on the photographic plate do not make me any readier to believe that potato starch grains in weak diastase solution, after four hours' exposure to polarised light, as a result of hydrolysis will present the appearance shown in Fig. 3, Plate 12, in the paper criticised. I am also dreadfully puzzled to understand why this figure, described in the text as showing "hydrolysis obtained with light polarised by a Nicol prism," was published, if it was not intended as evidence. The "complete disappearance of the grains" I have not observed with potato starch even after some days in strong diastase solution: in my experience a kind of "starch shell," which does not give a blue colour with iodine, persists obstinately.

Since an experiment of my own is cited (which was additional to a repetition of any described by the authors) in which a gelatine film was used, I may perhaps point out that the starch grains were resting on the surface of the gelatine, which was used merely as a convenient adhesive surface to prevent the grains from moving about, and that the polarised light reached the grains without passing previously through the gelatine film. But I should regard any such experiments that give a negative result as of little importance compared with one conclusive experiment of which the result was consistently positive: and no doubt Prof. Baly and Dr. Semmens will agree with me in this. The position remains unsatisfactory, however, until the reason for the negative result receives satisfactory explanation.

As one who has suffered from the painful effects of traversing in full sun snowfields in the Alps, I was