

If a sabellid worm is put, under water, into a narrow glass tube, the rhythmic contractions of the blood vessels in its crown can be seen through the glass. Under these conditions the contractions continue for half an hour and then cease. But as soon as even the tip of the crown projects again beyond the end of the tube, the contractions recommence. Thus when worms in Nature have retired into their tubes for over half an hour their blood no longer circulates. Yet *Spirographis* can remain uninjured for eight hours inside its tube when the front end of the latter is artificially closed. After about eight hours the animal re-expands its crown through a new anterior lateral aperture which it forms by local digestion of the mucoid tube.

When a sabellid is put into sea water saturated with carbon dioxide, the pulsations of its blood vessels cease almost immediately. After the animal has been put back into pure sea water the pulsations recommence. Neither water lacking dissolved oxygen, nor water acidified with hydrochloric acid to the same pH as water saturated with carbon dioxide, stops the pulsations. The threshold pH for stoppage in water containing carbon dioxide is in the region of 6.0, varying slightly with species.

It has been concluded from a variety of experimental evidence that the natural inhibition of blood circulation when worms are in their tubes is due to an accumulation of excreted carbonic acid between the filaments of the closed crown, and that this carbonic acid does not act on the blood vessels through a nervous reflex, but it prevents the normal exit of carbonic acid produced in metabolism, which accumulates in the body and has a direct inhibitory action on the blood vessel walls.

A full account of this work will be published shortly.

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Photochemical Reaction of Hydrogen and Chlorine

I AM much surprised to read, in NATURE of December 17, that so fine an experimenter as Prof. A. J. Allmand has failed to inhibit the action of light on a mixture of hydrogen and chlorine, by drying. It is, with one exception, the easiest reaction with which to demonstrate the influence of traces of water on chemical action. So much so, that for many years it was one of my stock experiments in courses of elementary lectures.

Hydrogen and chlorine were generated by the electrolysis of hydrochloric acid, washed with water and conducted through a tube of phosphorus pentoxide for rough drying. The mixture then passed through a dozen thin bulbs blown in series on a tube of soft glass, in some of which had been placed phosphorus pentoxide. After about six hours, the wash water was saturated with chlorine, and half an hour later the bulbs were sealed off at their capillary joinings. The last bulb was exposed to the light of burning magnesium, and if it exploded after five seconds, the batch was assumed to be perfect. In lecture a pair of bulbs, one containing phosphorus pentoxide and the other not, were exposed to the same intensity of light and in no case did the dry gas ever explode, while the moist ones did so without exception. One of the dried

bulbs was exposed to bright sunlight for three days, and analysis of the gas afterwards showed that one third of the mixture was still in the uncombined state. Mellor and Russell¹, in repeating this experiment, found that two thirds of the gas had resisted the action of light.

In describing these experiments to the Chemical Society, I did the experiment successfully; in fact, on the principle that 'seeing is believing' I have, almost without exception, demonstrated my results experimentally at the meetings at which the papers were read. Prof. Bodenstein and Prof. Allmand owe, I think, the failure of their experiments to the complexity of their apparatus. It is almost impossible, on account of the slowness of diffusion, to dry an apparatus which has any dead ends. Mere 'baking out' at 200° is insufficient, as I have shown over and over again, to get rid of deep-seated moisture in glass. Long continued contact of a gas with pure phosphorus pentoxide is the only way in which success can be attained.

At the risk of seeming egotistical, I would ask all who contemplate working in this field to read my paper² on "Manipulation in Intensive Drying". If they do so, they would, I feel sure, save themselves much fruitless effort.

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¹ *J. Chem. Soc.*; 1902.

² *J. Chem. Soc.*; 1929.

Limiting Mobilities of some Monovalent Ions and the Dissociation Constant of Acetic Acid at 25°

MACINNES, Shedlovsky, and Longworth in reply¹ to our recent letter with the above heading² have asserted, without experimental evidence, that our conductivity technique was subject to errors and that the discrepancies between the results are to be attributed to this cause. We would, however, point out that by using the cell constant determined with 0.01 N potassium chloride by Parker and Parker's method³ we have been able to reproduce Kohlrausch and Maltby's results⁴ for KCl, NaCl, and KIO₃ at 18° over the range 0.0001–0.01 N to within 0.1 per cent,⁵ and this, it appears to us, gives definite support to the view that our experimental results are trustworthy to at least 0.1 per cent.

Two limiting cation transport numbers for KCl are given in our paper², namely, 0.497 the old classical figure of Kohlrausch and Maltby⁴, on which basis all limiting mobilities have hitherto been calculated, and 0.490 the most probable figure deduced from the results of MacInnes and Dole⁶. It was our original intention to redetermine this constant but owing to the transfer of the senior author to Woolwich, this project was abandoned. We now employ the figure 0.490, which appears to be the most probable value deduced from the determinations of MacInnes and Dole and of Longworth⁷. The American authors state in their letter that their value for the limiting conductivity of the chloride ion, based on conductivity and also transport number measurements of KCl, NaCl, LiCl, and HCl at 25°, is 76.32 "within a few hundredths of a per cent"; the value deduced previously by us was 76.4₅ (n_k for KCl = 0.490).

The use of the empirical formula of Shedlovsky⁸