

discharge, but that spectroscopic data make it appear very probable that they may also be created outside the discharge by collisions of the second kind. I am well aware of Willey's work with an uncondensed discharge, where certain chemical actions occur in the absence of the afterglow. In this connexion it would be especially interesting to see whether there is sufficient energy in active nitrogen from an uncondensed discharge to bring out the band spectrum in cuprous iodide and the arc spectrum of copper.

As to the experiments with active hydrogen and ordinary nitrogen, my results are not extraordinary or new. The experiments are simple and were perfectly and easily reproducible. My negative results are, moreover, supported by several other investigators, including Bonhoeffer, who employed the same method (for literature see *J. Amer. Chem. Soc.*, *loc. cit.*). It still remains, I believe, for Dr. Willey to explain the kind of active hydrogen with which he was dealing, for (1) at 10 mm. pressure and 150 cm. (corrected in private communication) from the discharge no atomic hydrogen can exist (Kaplan, *Phys. Rev.*, **30**, 639; 1927); (2) electronically excited atoms or ionised species are out of the question; (3) there is no real evidence for the existence of H_2 , in fact, there is much evidence against it (see Urey and Smallwood's exhaustive attempts to prepare it with negative results, *J. Amer. Chem. Soc.*, **50**, 620; 1928; also Paneth and others, p. 29 of my paper).

The criticism is meaningless that since ammonia extinguishes the glow of active nitrogen (of which I am aware), that this constitutes a grave objection to my theory of ammonia formation—which by the way is not mine, since it has been expressed by Olsen (for reference see my paper, p. 33). My experimental procedure indicates that the pressures of nitrogen and hydrogen were low, down to 0.04 mm., and that a liquid air trap condensed out the ammonia immediately following the mixing chamber. It is certain that some of the ammonia formed, which would have otherwise reached the trap, was again decomposed. It is quite beyond the human eye to detect changes in the afterglow intensity at these low pressures, and it is rendered even more difficult due to stray light from the discharges. The results published in Table I. are only a few of the numerous runs made.

I feel that private communication in controversial matters, once experimental data have been presented, is a far more satisfactory means of discussion.

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The Mechanism of Formation of the Latent Photographic Image.¹

In a communication to NATURE of Sept. 24, 1927 (vol. 120, p. 441), the preliminary results were described of experiments made in an attempt to correlate the mechanism of the latent image formation with that responsible for producing changes of conductivity on illumination. It was shown that the apparent absence of the photo-conductivity effect in the ultra-violet was due to two things: (1) the small penetration of that light, and (2) the use of thick layers of the silver halide. With thinner layers, of the order of 70μ , the ultra-violet ($\lambda 3650$) effect in silver bromide was found to be about twice as great as that produced by the blue ($\lambda 4358$), thus supporting the original prediction that in very thin layers of the order of 1.5μ the effect at $\lambda 3650$ would rise to nearer ten times that

¹ Communication No. 67 from the British Photographic Research Association Laboratories.

at $\lambda 4358$, which is the ratio of photographic effects in very thin layers of slow, pure silver bromide emulsions. It was further predicted that in very thin layers the 'hump' of maximum sensitivity at $\lambda 4600$ in the photo-conductivity-wave-length curve would disappear. How completely these conclusions have now been verified can be seen from the accompanying graph (Fig. 1). The inference is that in very thin layers of silver bromide the three curves representing

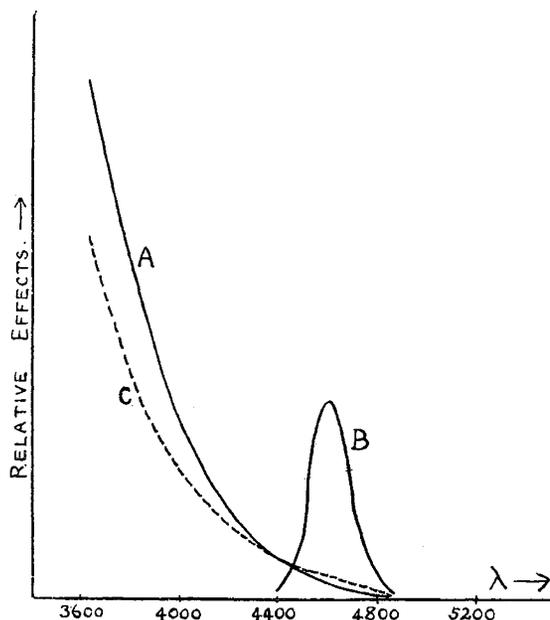


FIG. 1.—Curve A represents the relative photographic effects in 'single layer' emulsions of silver bromide. It also closely represents the absorption curve of this halide. The relative effects at $\lambda 3650$ and $\lambda 4358$ are as 10:1.

Curve B represents the relative photo-conductivity effects in layers of silver bromide of the order of 0.7 mm. and upwards. As the thickness is decreased, this curve gradually changes and approaches the form of Curve A. The closest approach yet obtained experimentally is given in Curve C, where the ratio of effects at $\lambda 3650$ and $\lambda 4358$ has risen to 7:1. The thickness of layer in this case was roughly of the order of 20μ .

(1) the relative photo-conductivity effects, (2) the relative photographic effects, and (3) the relative light absorptions, each plotted against the wave-length for equal incident intensity, are closely the same, indicating that in all probability the primary stage of the photographic mechanism is intimately connected with that which produces conductivity changes on illumination.

It is hoped that these results will be presented in detail and discussed at the forthcoming International Congress of Photography to be held in London on July 9-14.

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Forestry and Agriculture in Great Britain.

ALL forest economists will agree with the writer of the leading article in NATURE of May 5 that national poverty greatly increases the difficulties of a scheme of State afforestation, and that periods when money can be borrowed cheaply are most suitable for afforestation development. Only those, however, who take a superficial view of our national forest problem are likely to be greatly influenced by this consideration.

For many decades those who were alive to the facts of the case have advocated a policy of national