The meteor of April 8 was directed from a radiant in the eastern limits of Virgo, and not far from Spica. A fireball was seen on March 16 last, which was probably from the same radiant, as the paths converge on the point 205° – 18°. This region is the centre from which many fireballs and ordinary shooting stars are directed in April and other months, as the following table will prove :—

Radiant point	Date.	Description	Observer or authority
210 – 6	January 5-11, 1870	Meteor shower	Tupman
205 – 8	January 20-22, 1877	,, ,,	Denning
200 – 5	January 20—Feb. 3, 1896	,, ,,	Herschel
204 – 10	January 21—Feb. 23, 1869	,, ,,	Denning from Tupman's obs.
202 – 9	February 13, 1869	,, ,,	Tupman
210 – 13	February 15-21, 1877	,, ,,	Denning
200 – 10	March 2-3, 1870	,, ,,	Tupman
205 – 18	March 16, 1896	Fireball	Denning
204 – 10	March 23, 1895	Meteor shower	Denning
204 – 8	March 31—April 12, 1872	,, ,,	Denning from Italian obs.
210 – 10	April 7-16, 1877	,, ,,	Denning
216 – 10	April 11, 1871	Fireball	Niessl
206 – 8	April 12-26, 1879	Meteor shower	Sawyer
210 – 7	April 1877	,, ,,	Corder
209 – 3	April 1874	,, ,,	Denning
209 – 9	April 1896	,, ,,	Herschel
198 – 8	April 18 ,1841	,, ,,	Forshey
214 – 13	April 21, 1887	Fireball	Niessl
218 – 5	April 21, 1889	Bright meteor	Denning
210 – 9	April 19-23	Radiant 10 fireballs	,,
207 – 7	April 22, 1876	Meteor shower	,,
206 – 9	April 27, 1851	Fireball	Niessl
209 – 8	May 3-15, 1872	Meteor shower	Denning from Italian obs.
214 – 7	May 12, 1878	Fireball	Herschel
214 – 7	May 29, 1889	,,	Denning
217 – 6	July 7, 1895	,,	,,

The mean of the twenty-six positions is 209° – 9°.

The fireball of April 12, 1896, came from a radiant in the N.W. sky at 50° + 42°. The large meteor of April 22, 1894, had a similar radiant (*Observatory*, June, 1894), and the same may be said of the fireball of March 9, 1875.

Bristol, April 27. W. F. DENNING.

Becquerel and Lippmann's Colour Photographs.

I WISH to raise a point in connection with the optics of photochromy, which was not touched upon at the recent discussion at the Royal Society. The photochromatic spectra produced by the earlier workers, and especially by E. Becquerel about 1850, have long been known and have always appeared to be very mysterious to those who have repeated the experiments. When Prof. Lippmann's success with the interferential method was made known some five or six years ago, and his first results exhibited in this country, many of those who were acquainted with the previous methods of producing coloured spectra by direct impression came to the conclusion that all the earlier workers had unconsciously been producing the Lippmann effect. This supposition was not unreasonable. In Becquerel's method, for instance, which gave the most brilliant effects, the sensitive film of violet chloride is produced on a surface of metallic silver, and is thus backed by the necessary reflecting surface. Even when the colour sensitive chloride is on paper, as in the still earlier experiments of Robert Hunt and Sir John Herschel, it is not unreasonable to suppose that the bounding surface of the paper and silver haloid reflects sufficiently well to produce the necessary interference. At the discussion following Prof. Lippmann's paper, Lord Rayleigh raised the question whether the earlier and later results were not due to the same cause, but there seemed to be an impression that the Becquerel and Lippmann effects were produced by different causes. For my own part, I am bound to confess that the reasons assigned for arriving at this decision still appear to be inconclusive. The main points which have been allowed to prevail are that the Becquerel photographs cannot be fixed, that they appear of the same colour at whatever angle they are viewed, and that they appear of the same colour by transmitted and by reflected light. The fact that these photographs cannot be fixed is easily explained if we bear in mind that the silver salt, is not embedded in a vehicle, as in Lippmann's process, and that there is consequently nothing to hold the laminæ apart at the correct intervals when the fixing solution has done its work. The other points are less easy to explain; but it may be suggested that the difference is here due to the earlier experimenters having used coarse-grained films, in which the silver haloid particles are sufficiently large to *scatter* the colours produced in the film by the laminated structure of the alternating planes of decomposition and no decomposition. The question is a purely physical one, and may be put into the following form :—If the Lippmann effect is produced in a coarse-grained instead of in a transparent film, would not the Becquerel results be obtained? If physicists can answer this in the affirmative, the difficulty of supposing that similar results can be obtained by totally different causes would disappear.

R. MELDOLA.

Aquatic Hymenoptera.

UNDER the title "On Two Aquatic Hymenoptera, one of which uses its Wings in Swimming," Sir John Lubbock, Bart., read a paper before the Linnean Society, May 7, 1863, therein describing two most extraordinary insects, which he named *Polynema natans* and *Prestwichia aquatica*.

Last year I had the good fortune to obtain a large number of both sexes of the first named, which, after most critical microscopic examination, I identified as belonging to Haliday's *Caraphractus cinctus*, the unique characteristic of the "*keeled* metathorax" placing the matter beyond a doubt. The late Prof. Riley, to whom I had the pleasure of showing specimens, fully confirmed my opinion, as also did Mr. Charles Waterhouse.

The life-history of any of these minute Hymenoptera is not worked out in one season—very far from it; and since last year I have steadily followed up the chain of facts, my efforts being again rewarded by finding this most exquisite Hymenopteron this season within twenty miles of London.

Encouraged by my success, I continued my search for some hours at a small pond, and at last captured two female specimens of the long-lost-sight-of *Prestwichia aquatica* (Lubbock), which has not been recorded since its first capture by Sir John Lubbock in 1862—thirty-four years ago!

The two specimens (and *I*) have scarcely taken any rest since their capture yesterday morning, May 4; but they have been constantly running or paddling *under* water, never once having been to the surface. When I first put them into the tank, they had the *greatest* difficulty in forcing their way through the film; but as soon as that was accomplished, they moved about with their legs, as propellers, far more rapidly than did *Caraphractus cinctus* with its wings.

I am looking forward to capturing the *male Prestwichia aquatica*, which has not yet been recorded by any entomologist.

21 Manor Gardens, Holloway, N. FRED. ENOCK.

Dalton's Atomic Theory.

IN the review of "A New View of the Origin of Dalton's Atomic Theory," published in your issue of April 16, your reviewer, in summing up the evidence as to the origin of the atomic theory, makes an omission of such importance that it cannot be allowed to pass unchallenged. He attaches great weight to Thomson's statement that in 1804 Dalton himself informed him "that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas." Now these researches, as pointed out by your reviewer, were begun in the summer of 1804, a date which is assigned to them by Dalton himself, and is confirmed by the entries in his laboratory note-books of the time; so that Thomson's statement amounts to saying that the atomic theory first occurred to Dalton in the *summer of 1804*. This conclusion appears to us to be entirely discredited by the fact that several detailed tables of atomic weights and lists of atomic symbols, which are dated *September 1803*, occur in Dalton's laboratory note-books, one of these tables being reproduced in facsimile at p. 28 of the work under review, but not referred to by your reviewer.

It must be remembered that Thomson's account of the origin of Dalton's theory was first published in his "History of Chemistry" (vol. ii. p. 291) in 1831, no less than twenty-seven years after his visit to Dalton had been paid. Moreover, in 1850, after the lapse of another nineteen years, he gave a second and totally different account of the origin of the same theory, saying it was founded on the analysis of protoxide and dentoxide of nitrogen (Henry, "Life of Dalton," p. 80).

THE AUTHORS.

THE question is whether Dalton was led to apply the Newtonian doctrine of atoms to the explanation of chemical