states, to be regarded in the light of an abstract of a series of more extended papers published in the *Entomologist*. The papers in the latter publication from their title led us to suppose that Mr. Coste had made some contribution to our knowledge of the chemistry of insect pigments. I read them from month to month in the hope of getting new light on this subject, which is of such general interest to both chemists and biologists: I regret to say that I have been grievously disappointed. The experiments thus far described amount simply to the fact — not altogether astonishing — that strong chemical reagents modify the colours of Lepidopterous pigments or in some cases dissolve them out of the wings. The bearing of these observations on the chemistry of the pigments is so remote as to be practically useless until we know something of the chemical nature of these pigments. The methods adopted by Mr. Coste are not likely to advance our knowledge in this direction very much, and it is certainly remarkable that in treating of yellows he makes no reference [1] to the only real contribution to the chemistry of Lepidopterous pigments, viz. the experiments made by Mr. Hopkins, and published in the Proceedings of the Chemical Society in 1889. Mr. Coste is no doubt acquainted with those South American *Papilios* with a large red spot on the hind wing, which spot loses its red colour and becomes of a brilliant metallic bluish green when the wing is tilted so that the incident and reflected rays form a very wide angle. The colour is in this case doubtless a mixed result, partly due to pigment and partly to interference. Now, anyone who has observed this and other similar colour phenomena in insects might describe his observations as contributions to the physics of insect colours. If he thought proper to adopt this course, he would be misleading physicists. The observation of the bare facts is as much a contribution to the physics of insect colours as the statement that a rainbow can be seen in the sky is a contribution to the physics of illuminated water-drops. It seems to me that Mr. Coste's experiments bear the same relationship to the chemistry of insect colours that the mere observation of interference colours in insects bears to the physics of insect colours.

Quite independent of the facts recorded by Mr. Coste is the interpretation which he puts upon them. Here I must decidedly express dissent. It cannot be admitted, because by the action of certain reagents green is changed into yellow or red into yellow, that this indicates the evolution of green or red from yellow. There is no evidence that this result is a reversion effect at all. The analogy between the action of strong acids in modifying the colour of an animal pigment and the effect of true reversion is forced, and has no parallel in natural processes. Hot water is a chemical reagent; by its action on the brown pigment of the lobster the latter becomes red. If from this observation I drew the inference that the ancestral lobster was red, and that the hot water produced a reversion effect, I do not think that Mr. Coste would agree with me.

R. MELDOLA.

Oxford, April 24.

Eozoon.

MR. GREGORY has, I fear, slightly mistaken the meaning of my remarks, which were intended rather to excuse than to blame him. The specimen of Eozoon collected by the late Mr. Vennor at Tudor was figured in connection with my paper of 1867 as a type specimen, in so far as macroscopical characters are concerned; but it does not follow that slices from specimens less perfect in that respect, and now in my collection, may not be more instructive as showing minute structures. I may refer in this connection to the three specimens from Tudor and Madoc (Madoc being in the same formation with Tudor) figured by Dr. Carpenter in our original paper in the Journal of the Geological Society, vol. xxiii., pl. xii., Fig. 1. If anyone will take the trouble to compare these with the figures in Mr. Gregory's paper in the same Journal, vol. xvii., he will have a singular and impressive illustration of the different ways in which things supposed to be the same may appear to observers of different types.

Mr. Gregory is in error in supposing that he could see in the cases of the Peter Redpath Museum my specimens from Tudor and Madoc. I have not yet been able to place there any portion of my microscopic cabinet of Eozoon; but only a few hand specimens sufficient to show students the ordinary types of the fossil.

As to the Laurentian age attributed to the Tudor beds, I have already explained that this I subsequently regarded as an error, and so stated not long after the date of the paper of 1867. I now regard them as less ancient, though of pre-Cambrian age.

I shall be happy to show to anyone my little collection from Tudor and Madoc, including specimens in which Carpenter detected the canal system; but of these particular specimens I have unfortunately no duplicates for distribution, and would prefer to exhibit the slices in the modes I have found best suited for the development of the structures; as otherwise there might be some doubt whether the resulting impressions would more resemble Mr. Gregory's figures or Dr. Carpenter's.

Montreal, April 6. J. WILLIAM DAWSON.

[1] At least in NATURE: I have not the *Entomologist* at hand where I am writing.

The Theory of Solutions.

I AM glad to see that as to the main point, the character of the "gaseous laws" of solutions, there seem to exist no more differences between Mr. Rodger and me. For Mr. Rodger, in his letter on p. 487 of NATURE, limits his remarks to some dialectical expressions, to cover an honourable retreat. I wish not to follow him on this way, because it is an endless one.

As to the application of van der Waals's formula on solutions, Mr. Rodger is now forced to confess that this application is not so "meaningless" as he has formerly written; but he asserts that, shortly spoken, the form of application given in my book is so. To say the truth, if I have to choose, as in this case, between the agreement of a formula with Mr. Rodger's opinion, and the agreement of this same formula with experiment, I prefer the latter. W. OSTWALD.

Leipzig, April 12.

Physiological Action of Diminished Atmospheric Pressure.

WITH reference to the effect of diminished atmospheric pressure on the vital powers, alluded to in Prof. Bonney's review of Mr. Whymper's "Travels among the Great Andes of the Equator" (NATURE, April 14, p. 561), I do not know whether it is worth while recalling the well-known fact that numerous passes in the Himalayas, ranging from 17,000 to 19,000 feet, are habitually traversed by the hillmen, in the summer, with their flocks of sheep and goats carrying borax, &c. The highest pass is said to exceed 20,400 feet. In the same mountains Messrs. Schlagintweit reached an altitude of about 22,200 feet (Proc. As. Soc. Bengal, January 1866), while Mr. W. W. Graham ascended to 23,500 feet in 1883 (NATURE, September 11, 1884). I have myself, on several occasions, been to elevations of 17,000 to 19,000 feet, and beyond shortness of breath when climbing, never experienced any ill effects except once, when I, the four plainsmen with me, and three out of a considerable number of hillmen, felt severe headache during the evening after crossing a high pass. My companion on one trip, however, almost invariably suffered very severely from mountain sickness under similar circumstances. F. R. MALLET.

18, The Common, Ealing.

Sensitive Water Jets.

A FORM of this effect lately presented itself, which seemed in some ways new. A thin jet, 5 feet high and arched so as to be 3 feet at the base, was falling in a feathery spray. At 13 feet distance a small Wimshurst machine was set going: not instantly, but after two minutes, the spray gathered itself up almost into one clear line: although the jet was turned up and down and the machine was discharged the falling water would not resolve itself again into spray for fifteen or twenty minutes. It is difficult to imagine the medium for this action: it is too indefinite, perhaps, to suppose that an indicator is found for the trembling of a disturbed ether while it is dying down.

The well-known experiment is not known enough, for it is not often described in books. Take a glass rod, electrified ever so little, to a certain point; at once the jet collects itself; a slight move away brings back the old disorder, while an inch nearer makes things much worse. It is a striking illustration to help one to imagine what the electrical forces of the air may do We can perhaps understand those thick thundery rain-drops, that almost allow us to pass between them while they are giving friendly warning of what will come. W. B. CROFT.

Winchester College, April 14.