

H. H. H. H.
M. H. H. H.

PEABODY ACADEMY OF SCIENCE.

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Principal J. W. Dawson

Dear Sir

In reading your late address to the Natural History Society of Canada I was very much pleased to find therein the opinions of a large and I hope increasing school of Naturalists so ably and clearly expressed.

My feelings at the time moved me to write you but I was prevented by a feeling of diffidence, which however I must at length disregard.

We were all, that is the officers of the Academy felt that you had given expression at last to the feelings with which we had viewed the

exceeding haste of the European world to swallow Darwinism without winking.

Under the shelter of your first protest, for such it really is when compared with the majority of papers on the Philosophy of Natural History, we may hope to hear from others of humbler rank on this side of the water.

However to proceed to the main object of my letter.

In mentioning Prof. Cope's paper you speak of him as the author of the law of Acceleration

I sent you soon after its publication in the Memoirs of the Boston Soc. of Nat. History a paper in which this same law was stated in very nearly the same words as those used by Prof. Cope. The same

class of facts also were used and applied to explain the growth and modifications of the structure of the Cephalopods in time.

I cannot of course and do not intend to accuse you of any injustice, since even Prof. Cope, whose originality I do not doubt in the least, did not comprehend the full meaning of the paper alluded to, and quotes my remarks as only approximations to the full statement of his law.

I must in fact blame rather the want of clearness in the style of the composition.

If you would be kind enough to look over it I think you will be able to penetrate this obscurity and unravel the thread by the help of the following.

Prof. Cope applies it only to
the origin of genera whereas I
consider it applicable to the origin
of all of that class of forms which
are sudden variations and direct
additions of higher characteristics
during the progress of the race or
group — and as applying to the
origin of degradational types
such as Scaphites etc during
the decadence of the group.

Prof. Cope's own ^{verbal} estimate of
my observations are that they depicted
the results of the action of acceleration
and his own gave the mode of
it's action.

I have no desire to dispute ^{this or} any decision
which secures the originality of these
remarks, but only to secure that point,
since otherwise all that I have
done for eight years and much that
I hope yet to do will be deprived
of it's claim to the attention of
investigators.

You will find that this is the real spirit and not a pretence if you will take the trouble to turn to p 331 of the Amer. Naturalist for Oct. 1869 and also to the review of Cope's paper p 146 Vol III of the same. The last is a review of Cope's Origin of Genera but in order to avoid all controversy I did not even mention my claims.

It has just struck me after looking up this reference that this might be used against me, but all my old companions know that eight years ago I advocated and supported the developmental theory in our debates while students with Agassiz.

Leaving this subject, which is not the most agreeable to me, far of all things begging for scientific recognition is next to

begging for money the most disagreeable, permit me to ask you a question.

Has Dr Stey Hunt been sick or is he absent. Both Prof Shaler and myself have mutually wondered at not hearing from him.

He is generally so prompt in his answers to correspondents, even those who like myself are not worthy of such attention, that we have become a trifle anxious to know. His promised visit to Salem and this vicinity is anticipated with great pleasure by his many friends in this neighborhood, and especially by me.

With the greatest respect and regard for yourself and for him I remain

Yours sincerely
A. Hyatt.