

One case is where he starts upon his theory that two partially overlapping wings are present, one of which I had overlooked,—a theory he could not possibly have maintained with the St. John specimen beside him. "Its hind margin," he says, "is a little below the hind margin of the main wing." Only a mere fragment of the hind margin exists in the Boston specimen, and therefore the marking on the stone which he interprets as the hind margin of a second wing is recognizable with little doubt, and a glance at its relations to the other lines proves at once that it is simply one of the "numerous parallel and very close longitudinal" lines which he refers to a plant. An examination of the reverse shows no such mark at all, and the glazed texture of the stone, peculiar to it where any part of the wing is found, does not extend, on either stone, beyond the limits of the wing as I have defined them.

Dr. Hagen would, however, probably base his double wing theory more upon his next point: that the branches of what I have called the externomedian vein (found on the outer detached fragment) are elevated, while the "corresponding sectors of the main wing" (by which I suppose he refers particularly to the scapular and internomedian veins of the parts on the basal piece) "are depressed." That is, that the detached fragment represents one wing, the basal piece another. But in the St. John specimen, and indeed, though less clearly, in the Boston specimen, we have absolute proof of the inaccuracy of this view, since the externomedian vein, whether on basal piece or detached fragment is, *throughout its course*, elevated or depressed, according to whether obverse or reverse is examined; the same is the case in the reverse sense with the internomedian vein, which is elevated, on both pieces, where the externomedian vein is depressed, and *vice versa*. A little more familiarity with paleozoic wings would have taught Dr. Hagen to expect this very feature.

Dr. Hagen is equally unfortunate with the scratch of a tool on the costal margin of the Boston specimen, which he would look for in vain on the reverse stone. If it had been shown on my former plate (fig. 5) it would have lain 3 mm. to the right of the outermost cross-vein figured; his making the other existing cross-veins "*therefore* very doubtful" is accordingly unwarranted. The upper branch of the mediastinal fork is exactly as I have figured it, and not as Dr. Hagen represents, as the St. John specimen shows more plainly than its Boston reverse. Dr. Hagen's "cilia" near this point are again the longitudinal lines of his plant. There is no sort of reason for claiming the "fork, as it is called by the author," of the scapular vein, as belonging to an overlying wing, since it does not exist.

Dr. Hagen says he is "not able to classify the insect, except that it belongs to the Neuroptera (*sensu strictiori*)," though he adds, "what we see of the venation is more nearly allied to the Chauliodes type than to any other." He further asserts that "the venation has no similarity to Coniopteryx, Raphidia and Ephemera, and bridges in no way the gulf between the Neuroptera and Pseudoneuroptera, as stated by the author." These are bare assertions, unaccompanied by any proof whatever, and it would therefore be waste time to consider them; it will be enough to say with regard to the first, that, if true, the wing cannot belong to the Neuroptera, as he himself claims it does. My own claim, supported by direct evidence which can be examined into, was that it formed a distinct and extinct family of Neuroptera. My later study of a larger series of paleozoic as well as of existing types confirms my first conclusion.