DISCUSSION AND CORRESPONDENCE

BOTANICAL EVIDENCE OF COASTAL SUBSIDENCE

In a recent number of Science[1] Mr. H. H. Bartlett, writing under the above title, criticizes an earlier paper of my own concerning "The Supposed Recent Subsidence of the Massachusetts and New Jersey Coasts,"[2] on the ground that it represents hasty conclusions based on the examination of one locality where conditions are far from typical. Mr. Bartlett is of the opinion that the hypothesis there advanced to account for the appearances of recent subsidence along the coasts in question has "as a matter of fact . . . no bearing whatever on most of the evidence which has been offered."

Inasmuch as the brevity of my paper may be responsible for Mr. Bartlett's failure fully to understand it, perhaps a few words as to the development of the hypothesis presented in that paper may serve to make the hypothesis itself clearer. The writer's active interest in the problem of subsidence began a number of years ago when he was retained by counsel for the commonwealth of Massachusetts, in a case involving title to lands now below high tide level, to determine if possible the nature and extent of the recent subsidence along the Massachusetts coast. He entered upon this investigation with the belief that recent coastal subsidence in this district was a fact, well established by the studies of various students of shoreline phenomena; and his efforts were mainly directed toward ascertaining whether the subsidence was continuous in time and regular in rate, or whether it was intermittent and at varying rates. In connection with this investigation examination was made of numerous publications on recent changes of level in the United States and Canada, and many foreign reports on the same topic; and so fair familiarity with the literature of the subject was gained.

Even before undertaking the above investigation, certain geologically recent changes in the form of the shorelines about Boston had attracted the writer's attention; and in the succeeding years more or less consideration has been given to this subject. Several of his students have made special studies of selected areas along the coast, in each case giving attention to the problem of coastal subsidence. Their observations and his own, supplemented by studies of maps of certain areas not visited, led the writer to conclude that the coast had remained essentially stable for a long period of years; and that if any considerable subsidence had taken place, it must have occurred long ago.

The botanical evidence of recent subsidence was still a puzzle. Manifestly, the uppermost layers of the *Spartina* turf, and the stumps of cedars and other trees exposed in the marshes, could not be due to a remote subsidence, even if the lower layers of turf and the more deeply buried stumps had been depressed long ago. Careful attention has been given to this phase of the problem, and in this connection let me express my great indebtedness to Dr. Charles A. Davis, who gave me the opportunity to accompany him on several of his field excursions, and who took the time to visit with me two or three localities where I had studied the physiographic features of the shoreline. On these excursions I became fairly familiar with the botanical evidence of subsidence, and with the interesting methods of investigation which Dr. Davis has developed for the study of salt marsh deposits. By means of the ingenious peat sampler devised by Dr. Davis, the writer has endeavored to increase his knowledge of the structure of several of our marshes; and one of his students has made a detailed series of sections across the marshes at the mouth of the Neponset River, which will be referred to in a future publication.

As a result of these studies, it seemed to the writer that while the lower portions of the marsh deposits might indicate subsidence in times long past, the upper portions (the portions which furnish supposed evidence of *recent* subsidence) might be explained in either one of two ways: they might represent a resumption of the downward movement of the coastal region in recent times, after a long

[1] Science, N. S., XXXIII., 1911, 29–31.

[2] Science, N. S., XXXII., 1910, 721–723.

halt; or they might be explained as the result of local fluctuations of marsh level or of tide level, independent of a downward movement of the coastal region. The long halt in the supposed downward movement seemed to be demanded by the physiographic evidence.

Observations in the Scituate-Marshfield region revealed a cause of fluctuations in high tide level which appeared competent to explain most, if not all, of the evidences of recent subsidence. After a consideration of the tidal conditions along the Atlantic coast, and a study of the nature of the high tide surface about the coasts of England, as reported by Wheeler and others, the conclusion was reached that the conditions in the Scituate region had been repeated in a greater or less degree all along our coasts in recent geological times, as a necessary consequence of the nature of the high tide surface and the changes effected by wave action on all shores, particularly those composed of poorly consolidated materials. The cause seemed competent to explain a deposit of peat varying in thickness from a few inches to a possible maximum of 15 feet or more, according to the former range of the tides. As all of the supposed evidence of *recent* subsidence on the Massachusetts and New Jersey coasts are, so far as known to the writer, capable of explanation on the basis of a fluctuating high tide surface, and as the conditions on those coasts make such fluctuations in the past a seeming necessity; and as the physiographic evidence, on the Massachusetts coast at least, points to a long period of coastal stability in recent times, the conclusion seems reasonable that, while subsidence in the past may have occurred, the evidence of recent subsidence in these two areas is not decisive. Further study convinces me of the correctness of that conclusion. But whether it is correct or not, it was not reached with undue haste, nor was the botanical evidence of subsidence "lightly disregarded."

When a problem that has been discussed for many years, on the supposition that it involves but x factors, is found really to involve $x + 1$ factors, all of the earlier conclusions should be carefully reconsidered; not that they are necessarily wrong, but because it can not be known that they are right until the additional factor is fully considered along with the others. The problem of recent subsidence of the Atlantic coast has long been discussed; but the importance of a high tide surface which fluctuates with changes in the form of the shoreline, as a possible explanation of this apparent subsidence, does not appear to have been considered in the published discussions of the problem. It is an element of possible value in all cases of tidal shores which are irregular in outline or which are bordered by barrier beaches. Hence all conclusions which have been reached in regard to recent subsidence in such cases ought to be revised in order to take this element into account. What the result of revision in each case will be can not be foretold; and for this very reason the revision seems the more necessary. The writer is attempting such a revision for several localities, without prejudice in favor of any particular conclusion, and in most of the cases without any idea as to what the final result will be.

My statement as to the inconclusiveness of the evidence of recent subsidence on the Massachusetts and New Jersey coasts, was made after a careful reading of Mr. Bartlett's very interesting paper on the sub-marine *Chamæcyparis* bog at Woods Hole, Mass. Although this paper presented a most ingenious and interesting argument in favor of recent subsidence, the validity of the argument depended upon certain assumptions which seem to me untenable. It was not the object of my brief paper to discuss the voluminous evidence in favor of recent subsidence, but rather to make a short preliminary announcement of an hypothesis which appeared to be of considerable importance; for this reason I did not deal specifically with Mr. Bartlett's observations. Full attention will be given to all accessible evidence in favor of recent subsidence in a future publication.

A careful reading of Mr. Bartlett's criticism of my paper leads me to think that he has failed to discriminate sufficiently between low-lying peat deposits which may be of consid-

erable antiquity, and the uppermost deposits unquestionably of modern date which alone can be invoked as evidence of recent subsidence. It would appear, also, that he has not clearly understood my paper; for it is otherwise difficult to account for such statements as "the hypothesis of a fluctuating high-tide level has no possible application" to the botanical evidence of present subsidence as presented by Dr. Davis; or that the hypothesis "has no bearing whatever on most of the evidence which has been offered." Surely if the height of ordinary high tides gradually rises one or more feet as the result of changes in the form of the shoreline without change in the level of the land, this increase in tidal height will produce all of the phenomena which would be produced by actual subsidence of the land, "now going on." The task which confronts the student of shoreline changes involves a discrimination between different causes producing like results; and the best method of making such a discrimination is, in the opinion of the writer, to deduce the character and magnitude of the results which each hypothetical cause is theoretically capable of producing under the varying conditions which exist along an irregular coast, and then to compare the deductions with the actual phenomena as observed in the field. So far as I have carried such an analysis for the Massachusetts coast, the evidences of supposed recent subsidence are all capable of interpretation on the basis of a fluctuating high tide surface; whereas physiographic features of much importance seem incompatible with the theory of continued recent subsidence.

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MIASTOR AND EMBRYOLOGY

Reproduction by insect larvæ, a form of parthenogenesis known as pedogenesis, is extremely interesting. The writer was fortunate last fall in finding, in the partially decayed chestnut bark of a rail fence, numerous *Miastor* larvæ, forms not previously recognized in this country, though several species and representatives of allied genera have been studied by a few Europeans. *Miastor* larvæ and their allies should be of great service to teachers of zoology and biology, since they admit of the study at first hand of this interesting phenomenon. It is possible, with a no more elaborate outfit than an ordinary student's microscope equipped with a three-fourths objective, a microscopic slide and a few cover-glasses, to observe the vital activities of the young larva, to see the muscular, respiratory, digestive and nervous systems, to identify the ovaries and to watch the gradual development of the semi-transparent embryos within the body of the living mother larva. Furthermore, these forms are well adapted to more exact histological methods, being soft and therefore excellent subjects for serial sections and stains, particularly as it is comparatively easy to secure from one colony a series of individuals representing different stages of development.

Aside from the interest attached to their morphology and biology, there are other considerations which should appeal strongly to the teacher of zoology. These larvæ are widely distributed and, with an understanding of their habits, there should be little difficulty in finding them. They are small, and a piece of wood six inches long, three inches wide and half an inch thick, may contain or produce enough material for a fair-sized section or class. The larvæ are prolific and, under favorable conditions, would probably multiply at any season of the year. This is certainly true of the fall, the early winter and the spring. They can be kept alive for at least a month in microscopic cells, and with care a larval generation will develop in such restricted quarters. We have kept larvæ healthy and multiplying for nearly three months with nothing more elaborate than a moist piece of decaying wood clamped lightly to an ordinary microscopic slide and kept in a moist, dark box. Many of the larvæ were content with conditions on the surface of the wood, next the glass, and were therefore easily observed. These remarkable larvæ are very hardy; prolonged dryness simply results in a suspension of activities, while they are quite resistant to an abundance of moisture. Embryos will continue their development in mother larvæ even