

## V.—THE RELATIVE AGE OF FLINTS.

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MR. G. ABBOTT is quite right in saying (GEOL. MAG. for June, p. 275) that much work has yet to be done before we understand the formation of flints, and I would add that the work required must include observations on other siliceous concretions such as chert and cherty nodules of all kinds. It is also unquestionably important that we should, if possible, ascertain whether such concretions were formed contemporaneously with the deposits in which they are now found, or whether they are a sort of separation product formed during the consolidation of the rock after upheaval above the sea.

To Mr. Abbott, as to many observers, the vertical veins of flint so often present in Upper Chalk, and termed by him "tabular flint," prove a stumbling-block. These veins do certainly appear to have been formed during or after the consolidation of the Chalk; but need we on this account proceed to assume that all other flints and cherts are of subsequent growth? Surely it is at least possible that some forms of flint are of contemporaneous origin, while others may be of subsequent date.

Before discussing this difficult question let us clear the ground a little by making a rough classification of flints. Mr. Abbott makes no mention of the horizontal or interbedded floors of flint which are sometimes continuous over many square miles (*e.g.* in Kent and Lincolnshire). These are much more correctly called "tabular flints" than the vein-courses which cut the beds at various oblique angles. No one who examines them could doubt that they are interstratified beds and not infilled cracks, though of course it is open to question whether they were always layers of flint.

Chalk flints may I think be classed under four heads:—

1. Nodular flints.
2. Tabular flints and flint floors.
3. Vein-courses.
4. Paramoudras.

An acquaintance with flints and their different modes of occurrence extending now over more than twenty years has only served to strengthen my belief that flint nodules and tabular flint-floors have been formed while the Chalk was being accumulated. I would not deny that the making of a flint may have been a long and slow process; there may have been a succession of phases and changes before an incipient nodule became a hard and glassy flint, but I think that the whole process must have been accomplished while the superincumbent chalk was still soft chalky ooze, and while it was still saturated with sea-water.

It is, perhaps, not generally known that the flints of the successive zones of the Upper Chalk have special characters of their own. The characters of the flints in a given zone are not exactly the same throughout England, but within certain areas they appear to be constant, and they have been used by Dr. Ch. Barrois and the late

Prof. Hébert as aids in dividing one zone from another. Further observations on this succession of forms of flint, with the view of determining how much dependence can be placed upon them, would be very useful; but the fact of there being differences is unquestionable and is evidence for the contemporaneous origin of the nodules.

The flints with hollow tubular cores and spaces to which Mr. Abbott refers are specially characteristic of the *Micraster corangium* zone, and have generally been formed round the bifurcating branches of *Doryderma ramosum* (*Spongia ramosa*, Mantell), not round *Siphonia* stems as he imagines. *Doryderma* was a siliceous (Lithistid) sponge and its subsequent solution has left the flints in the condition which Barrois calls *cariés*.

Now it is clear that in such nodules the flint must have been formed around and attached to the body of the siliceous sponge before its solution, and must have been as hard as it is now when the sponge tissue was dissolved out. The solution is not likely to have been accomplished until after the upheaval of the Chalk, *ergo*, the flint was there before. It is quite true that sometimes the cavities are partially or even wholly filled with subsequently-deposited siliceous matter, but this is usually chalcedony and is quite different from the external flint.

Mr. Abbott's suggestion to account for the supposed absence of flints in the Lower Chalk cannot possibly be accepted. He does not seem to know that what was formerly called "Lower Chalk" is now divided into *Middle* and *Lower* Chalk. Flints are common in the upper part of the Middle Chalk and though they do not occur in the Lower Chalk of the South-east or of the Midlands, they do occur in it and very low down in Devon, Dorset, and Yorkshire.

Mr. Abbott is quite right, however, in saying that flints can be found in every stage of development, and a study of these incomplete flints with the aid of a microscope ought to give us some insight into the manner of their construction. My friend Mr. W. Hill, F.G.S., has for some years been occupied in the study of such flints, and I understand that he intends to publish some of his observations at an early date. He agrees with me in regarding the imperfect flints as cases of arrested development apparently for want of a sufficient supply of silica.

The co-existence of flint and chert with a prevalence of sponge spicules is too frequent to be a mere coincidence. All who have recently studied siliceous nodules, notably Dr. Hinde, Prof. Sollas, and Mr. W. Hill, have connected the formation of the concretions with the occurrence of the siliceous sponges. But as to the details of the processes whereby the silica of the sponges has become concentrated in the siliceous nodules and layers we are at present very much in the dark. More observation and experiment is required before Nature yields up this long-treasured secret; but I can see no good reason why the initial formation of all the siliceous nodules, which occur in limestones, should not have been due to the same causes which have led to the production of calcareous and phosphatic nodules in sands and clays; and in the formation of these it is clear

that the decomposition of organic matter has played a prominent part.

Lastly, with regard to the vein-courses, I can confirm Mr. Abbott's observation as to the frequency of two layers, and I agree with his conclusion that they must have been formed in fissures, for I presume he means any kind of fissures, such as open joint-planes, and not necessarily faults. This being so, one would certainly have expected that the microscope would disclose some points of structural difference between the flint of veins and that of nodules. Mr. Hill however assures me that he cannot detect any material difference, and it would seem therefore as if we must admit that true flint can be formed in spaces which have been open cracks. It is quite possible however that the deposition of the silica in these cracks took place as soon as the cracks were opened, that is to say during the first upheaval of the mass and while the occluded sea-water was passing away from it. This water must have contained much silica in solution and the mere relief of pressure by upheaval may have been sufficient to cause precipitation of the dissolved silica, though why true flint and not chalcedony was deposited I am quite unable to explain. Similiar veins sometimes occur in the chert-beds of the Upper Greensand, but the silica in them is always chalcedony.

If the flint-veins were formed after complete upheaval and by deposition from water percolating down from the surface of the land, it is difficult to understand when and under what conditions such water could first of all take up silica and then deposit it as flint. Such water would be much more likely to leave veins of calcite behind it, and calcite crystals do not unfrequently occur in empty shells of *Ananchytes* and *Terebratula* and in the cracks which often occur in the beds of "Chalk Rock."

At the same time I admit that there are veins of flint which seem to have originated after the later disturbances of the Chalk, but the theory above suggested would still apply to such cases if we can assume that the areas in which they occur had not previously been raised above the sea level.

There are also chemical questions which probably a little experiment would answer: imagine a mass of chalk for the first time upheaved above the sea-level, the sea-water with which it was saturated and which probably retained much silica in solution would sink to the parts below sea-level, and rain-water, taking up carbonate of lime, would percolate downward. Now what would happen when the carbonated water came in contact with that containing silica, would not the silica be precipitated?

In this paper I have only dealt with the general aspect of the question; the manner in which flint was formed is undoubtedly a great puzzle, and it will not be solved without much patient investigation. So far as I am aware flints only occur in very pure *chalky*-limestones, such as Cretaceous Chalk and the Portland Limestone; but their occurrence in the latter shows that very deep water was not necessary for their formation.