

formation from the pre-Silurian downwards, irrespective of the exceedingly interesting eruptive series, which it is not the purpose of this preliminary notice to discuss.

X.—WAS THE DEPOSIT OF FLINT AND CHALK CONTEMPORANEOUS?

By G. ABBOTT, M.R.C.S.

MANY difficulties remain to be solved and much work to be done before we rest satisfied that we can understand how flint took the peculiar shapes and position in which we find it in the Cretaceous strata.

Most geologists will now agree with me that we must look to the action of segregation for the explanation. The older theory of colloid silica, as well as that of the chemical replacement of the organic matter in the sarcode of sponges by silica, long held sway, and even yet their influence is to be traced in the most recent literature on the subject.

To advance our knowledge and to obtain a working hypothesis at once accurate and comprehensive, ought we not to commence by trying to find out *when* the silica was deposited? Did it take place before or after the Chalk was raised above the sea-level?

Many writers imply, if they do not state, that the formation of flint took place contemporaneously with the deposit of the chalk strata. With this I disagree; anyway it seems of the utmost importance that this point should be inquired into, for manifestly the influences at work to effect the chemical changes would be quite different in the two cases.

The *tabular* flint of the Upper Chalk appears to give us some valuable data with regard to this point.

During the last twelve months I have examined all the specimens I could find, many *in situ*, and they all showed signs of having "grown" in faults.¹ Besides being found in the position where one would expect to find such fissures, they show innumerable traces of having been formed of two plates originally distinct, but which by the gradual addition of silica have approached each other and united into one thick tabular mass. Few specimens fail to show where this junction took place, whilst here and there irregular cavities remain, due, I presume, to the process in that portion being incomplete when segregation ceased.

The thickness of these tabular flints varies considerably—ranging from $\frac{1}{4}$ " to 6" or more, sometimes almost reaching these extremes within the space of a foot, but more often lying for long distances between fairly parallel walls. The external surfaces correspond to those of the broken chalk, having here and there excrescences which indicate the growth of flint in cavities on the walls of the fissure, the surfaces in some specimens being so angularly rugged that the Chalk must have been dry and hard when the fissure occurred.

I have looked for, but hitherto have failed to find, any other satisfactory explanation of these remarkable dyke-like deposits.

¹ This can hardly apply to the continuous *horizontal* layer of flint which occurs in the Chalk from Thanet to Dover.—EDIT. GEOL. MAG.

If, however, we may use this as a working hypothesis that tabular flints "grew" by gradual accretion and quite independently of any organic agency, can we any longer believe they were formed even partially when the Chalk was beneath the sea. The upper part of the Chalk would then have been in the condition of soft ooze, and even if cracks were formed, the spaces would have been quickly filled with cretaceous matter and the deposit of flint rendered impossible owing to the length of time it would have taken to "grow" on the walls of the cleft.

Very much of course depends on the settlement of this question. Will someone point out if it is unreasonable to look in this direction for a solution of our difficulties in reference to chalk flints?

If any of the old theories or even that of segregation *below sea-level* be applied to special forms of flint—paramoudra for instance—they fail to answer even the most obvious questions. The old idea that these potstones originated in cup sponges has been discredited because of the difficulty of understanding how sponges could grow out of one another and extend in great columns like paramoudra, through the height of the cliff. Unfortunately I have had no chance to examine them *in situ*, and therefore only hesitatingly suggest that their origin was entirely independent of animal organisms, they being merely broken pipes of silica which formed in the chalk subsequent to its elevation above sea-level. The silica might have been supplied by the percolation of water through the overlying arenaceous strata, while each section of the broken pipe after its slight displacement by the movement of the Chalk which led to its fracture, would probably still act as a centre for further deposits of flint, and thus all traces of *fracture* would soon be obliterated.

Both paramoudra and tabular flint I believe to have been caused by the deposit of amorphous and crystalline forms of flint in chalk cavities—each by gradual growth extending in the direction of least resistance, without depending upon the help of organic remains, but enveloping and matting together any substances close at hand.

Careful observation of an immense number of other specimens leads me to suggest that in spite of the bossy, irregular forms assumed by flint, it has increased after the same manner as other concretions. This condition is much obscured by the presence of colloidal silica, and has perhaps not been observed because it was not expected or looked for. Instances, however, can be well seen in the interior of hollow flints with cores, supposed to be the silicified casts of the cloacæ of *Siphonæ*. Although these cores correspond as a rule to the long axis of the cavity, yet there are frequent exceptions. Smaller secondary cores, too, are often present lying at various angles and blending into each other.

It has long been accepted as a fact that flint has often commenced to form in chalk by means of sponges, many of which were subsequently removed after acting as a sort of scaffolding for the flint. This explains the numerous hollow flints met with. To these organisms we must add many others, such as wood, shells, etc., which have acted as centres for the segregation of flint.

Owing to the fact that the addition of fresh silica often destroys all traces of the growth-stages of a flint, very little information can be obtained when the process is complete and the nodule a mere solid mass of flint. Fortunately there are innumerable cases to be found in every stage of development, and those who care to look can find them in most districts of the S.E. of England where the Upper Chalk exists. The absence of flint in our lower beds¹ would of course be explained by the assumption that only half the beds were above the sea-level when the process of segregation was going on.

I may, in conclusion, reduce my suggestions to the following propositions:—

1st. That Flint in the form now found in the Chalk was deposited subsequent to its upheaval above sea-level.

2nd. That whilst the large quantity of siliceous sponge spicules present must have had a considerable share in the formation of the nodular and perhaps the tabular flints, yet quite as frequently various other hard substances and even empty spaces assisted.

3rd. Chalk flints grew after the manner of crystals [or concretions?] and were regulated by similar laws.

TUNBRIDGE WELLS.

XI.—ANOTHER VIEW OF THE SUBMERGENCE OF THE BRITISH ISLES DURING THE GLACIAL PERIOD.

By JAMES D. HARDY.

I QUITE agree with the *submergence* of the British Isles during the last or any Glacial period; but I totally disagree with the theory that there has been any such *depression of the land* as is generally put forward by geologists. Where is the evidence of such depression? Certainly not since Pliocene times, unless the whole of Great Britain sunk and rose simultaneously like the parallel motion of a beam engine. Such a motion would imply not only a surface movement, but also a sinking of all underlying strata. How such a theory ever held for a time is a puzzle to me, excepting the absolute necessity of finding some reason for the sand and shell deposits up to 1500 feet O.D. That the whole of England and at least the greater part of Scotland was covered with water there is evidence enough, and that Britain was not covered with an Ice-cap there is also evidence, if one will look at it free of all old book theories. Geologists—like other specialists—follow their bell-wethers, first in one direction, then in another, and are rarely able to bring other knowledge to bear on their arguments, as I have found when arguing on this depression theory with them.

¹ Mr. David Forbes, F.R.S., pointed out, many years ago, that the Hard Chalk and Grey Chalk really contained an equal quantity of silica as the Upper Chalk with flints, but it had not segregated out into flints in the former as in the latter formation. An excellent account of the Chalk formation and of the Flints and their probable mode of origin will be found in the *Geology of England and Wales* by Horace B. Woodward, F.G.S. (1887) pp. 397–401. See also paper on Banded Flints by Dr. S. P. Woodward (*GEOL. MAG.* 1864, pp. 145–149, Plates VII. & VIII.), and the Isle of Thanet and its Continuity of the Flint Floorings by F. A. Bedwell, M.A., F.R.M.S. (*GEOL. MAG.* 1874, pp. 17–22, and *Proc. Geol. Assoc.* 1873, vol. iii. pp. 217–238, pl. iv.).—*EDIT. GEOL. MAG.*