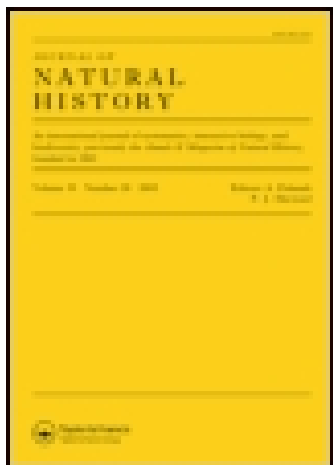


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### LVIII.—On the flint nodules of the Trimmingham chalk

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LVIII.—On the Flint Nodules of the Trimmingham Chalk.

By W. J. SOLLAS, M.A., F.R.S.E., F.G.S., Professor of Geology in University College, Bristol.

[Continued from p. 395.]

*The formation of the Flints.*—If it were possible to give a satisfactory explanation of the formation of the flints, a difficult and interesting problem would have been solved; and though a complete solution has not yet been attained, it is nevertheless certain that the observations of the past ten years have brought us remarkably near to it.

In this part of the paper (which was not read at Swansea) I propose to offer a brief general discussion of the subject, arranging our inquiries under the four following heads:—

(i) The source of the silica of which flints consist; (ii) its accumulation, chiefly as sponge-spicules, in the flint-bearing deposits; (iii) the solution of the accumulated silica; and (iv) its redeposition as flint and other forms of mineral silica.

(i) *The proximate source of the Silica.*—Two opposite opinions are held with regard to this:—one to the effect that the silica was introduced into the flint-bearing bed, subsequent to its formation, in solution from without; the other, that it was deposited contemporaneously, and subsequently dissolved *in situ*. According to the first view, which is advocated by Hull and Hardman in explanation of the Carboniferous chert of Ireland, and by Renard for the Carboniferous Phthanites of Belgium, a shallow sea became charged with an unusual amount of silica derived from the siliceous rocks of surrounding lands. The siliceous waters permeating the calcareous sediments below brought about a replacement by which they were converted into flint. The second was the opinion of Ehrenberg and Lyell, and is supported by W. Thomson, Wallich, and quite recently by Alexis A. Julien. According to it the silica has been derived from siliceous organisms, either collected into distinct layers or scattered through some other deposit like the siliceous remains now found dispersed in the Atlantic ooze.

For some years past I have regarded this latter proposition as an almost self-evident truth.

1. In a discussion on Dr. Wallich's paper on "the Physical History of the Cretaceous Flints," Dr. Sorby stated, in terms admirably terse, the general argument which has long been advanced in its favour. He says \*, "Though deep-

\* Quart. Journ. Geol. Soc. xxxvi. p. 91.

sea mud differs from chalk in many important particulars, yet still it is sufficiently related to warrant a comparison. Since the remains of siliceous organisms are absent from the chalk, but flints present, whilst in the deep-sea mud siliceous organisms are abundant and flints absent, probably the material of the flints had been to a greater or less extent derived from these organisms."

This argument depends on the analogy of some deep-sea mud with the chalk; and by this analogy the inference is drawn that siliceous organisms were at one time present in the chalk, just as they are now in the grey ooze. We shall now proceed to make this inference independent of analogy by showing that it is really nothing less than a statement of fact.

2. The deposits in which flints occur can be proved by direct observation to have originally contained abundant siliceous organisms, which have since, to a greater or less extent, disappeared from them.

The Trimmingham flints afford evidence straight to the point; for not only are sponge-spicules intimately associated with them and in great numbers, but these spicules afford us clear proof of the previous existence of a great mass of other spicules of which they are themselves but a miserable remnant. The small fragments of Hexactinellid and Lithistid network indicate the previous existence of whole skeletons of such network, and also of a great quantity of those minute spicules which in the living sponge are thickly strewn throughout its sarcodæ; of these flesh-spicules not a trace is now to be found. And finally, while a large part of the larger spicules and all the flesh-spicules of the sponges have entirely disappeared, those that remain present abundant signs of corrosion, and have evidently lost a considerable proportion of the silica they once contained.

We thus see, not only that certain spicules still exist in the flint-bearing chalk, but that, by the law of association, a vastly greater number of other spicules must have existed along with them. Somehow these other spicules cannot be found in the deposit now; somehow flint nodules, which are not associated with recent sponges, have made their appearance. And the inference is clear; as one says, the facts speak for themselves.

This argument holds not only in the case of the Trimmingham flints, but of nearly all flints which I have examined, and may be extended to many other kinds of siliceous deposits as well. In the Niagara chert-beds of the Silurian of North America remnants of sponge-skeletons abound. In the Car-

boniferous beds of Scotland we have the same association ; and in those of North Wales pseudomorphs of Radiolaria in calcite occur along with minute quartz crystals. The Lias of South Wales contains beds of chert literally crammed with sponge-spicules of large size ; and in some of the Lias limestones dispersed spicules are abundantly present along with minute quartz crystals and chalcedonized shells. In the Coraline Oolite of Yorkshire we find the calcitic pseudomorphs of *Geodites Sorbyanus*, to the abundance of which Sorby testifies ; and accompanying them are chalcedonized shells and numerous granules of silica with a radiate crystalline structure. The sponges of the Yorkshire oolite, often of large size, are known to have been siliceous solely by a study of their form and structure ; for they now consist of carbonate of lime, the silica which they once contained being, according to our view, chiefly collected in radiating crystalline patches or granules, which occur in association with them. In the freshwater Purbeck beds of Lulworth freshwater chert occurs, in which Mr. John S. Young, F.G.S.\*, has found numerous spicules of *Spongilla* (*S. purbeckensis*, Young). In the Cambridge Greensand we have a remarkable instance of the association we are illustrating in the fossil Renierid sponge *Pharetrosporgia Strahani*, the spicular fibres of which have exchanged a siliceous for a calcareous composition, while the chalk surrounding them in the interstices of the sponge has been converted into silex, with but slight alteration in morphological character.

I believe I may fairly claim to have substantiated the statement with which I set out, and will now only add the following passage, which I venture to extract from my paper on *Catagmat*† ; it indicates the same line of reasoning, though it was used in a quite different connexion :—"As regards siliceous sponges, many of these often exist now in a calcareous state ; but it may be as well to note that whenever a siliceous sponge becomes calcitized in fossilization the deposited silica is generally to be found somewhere not far off, either in patches in the sponge itself, or in granules or nodules such as flints in the surrounding matrix, or as chalcedony silicifying associated calcareous shells, *ex. gr.* in the Lias of the South-Welsh coast, or in minute dispersed crystals of quartz, *ex. gr.* in the Devonian and Carboniferous limestone. In compact strata, such as chalk or limestone, it may be taken as an almost invariable rule that the replacement of organic silica by calcite is always accompanied by a subsequent deposition

\* Geol. Mag. new ser. dec. ii. vol. v. p. 220 (1878).

† Ann. & Mag. Nat. Hist. 1878, ser. 5, vol. ii. p. 361.

of the silica in some form or other; and thus, if one finds flints, chalcedonized shells, or minute quartz crystals in such strata, one will naturally look for the remains of the siliceous organisms which supplied them, and one's search will seldom be unsuccessful."

As an objection to what we may call the theory of the intrinsic source of the siliceous in flint we may quote the following passage by Prof. Renard\*:—"The details of micro-structure which we have entered into prove also that it is impossible to admit, in order to explain the formation of Phthanites, as has been so often repeated in the case of flints, that these rocks are derived from an accumulation of organisms with siliceous envelopes. In the first place the examination of thin slices shows us but very seldom in these rocks sections of shells which one would refer to organisms with siliceous tests; and if in some cases we do meet with them, in flint for example, the siliceous envelopes are there so well preserved that, admitting the entire mass of the nodule to have been derived from the transformation of these remains of organisms into gelatinous silica, we cannot understand why some sections should have escaped this transformation and should have been preserved intact in the midst of the 'fusionment.'"

The Trimmingham flints appear to throw some light on this difficulty; for we find that as silicification proceeds and the nodule becomes more completely a flint, the sponge-spicules, which are abundant enough in the contiguous siliceous chalk, completely disappear in the flint itself; indeed one may even observe one half of a large spicule projecting out of a mass of siliceous, while the other half, which is certainly imbedded within it, is not to be distinguished from the surrounding flint. As regards the precision with which the form of some sponge-spicules is preserved in flint, my observations† show that this does not extend to their substance; for in such cases, though white, opaque, well defined, and apparently solid, they are really nothing more than empty hollow casts. When these casts become filled in with silica subsequently, as they sometimes do, they lose their solid appearance and become mere shadows of their former selves.

\* "Recherches lithologiques sur les Phthanites du Calcaire carbonifère de Belgique," par A. Renard, S.J. Extrait des Bulletins de l'Académie royale de Belgique, 2<sup>me</sup> sér. t. xlvii. nos. 9, 10 (1878).

† Quart. Journ. Geol. Soc. xxxiii. p. 817 (1877):—"Indeed, I may go so far as to state that whenever one sees a very white and opaque solid-looking spicule imbedded in clear transparent flint, one may expect to find it just the very reverse as regards solidity of what it seems" ("On the Genus *Siphonia*").

After having explained a difficulty with regard to the intrinsic view, one may *en revanche* suggest one to the extrinsic view. This is to be found in the restriction of the flints to definite layers in the chalk, the chalk above and below being free both from them and from sponge-spicules. It is difficult to see, in the first place, how a shallow sea came to consist of a strong solution of silica, and still more so to understand how it came to vary in a rhythmical fashion, sometimes being concentrated enough to lead to the formation of flints, and again pure enough to leave the intervening chalk almost absolutely devoid of silica.

(ii) *The accumulation of the Sponge-spicules.*—Since we have shown that the silica of the flints has in all probability been derived in many cases from accumulations of sponge-spicules, we have next to show how these accumulations were produced. Two different explanations naturally suggest themselves: either the spicules have been derived from successive generations of sponges which grew upon the same spot, or they have been separated from a large quantity of chalk and washed together by current-action.

The Trimmingham flints contain each a diversified collection of spicules derived from several different genera of several different families of sponges; and the assemblage of forms obtained from one flint does not differ in any distinct way from the assemblage obtained from another. This possession by a number of separate flints of a group of diverse spicules in common might lead us at first to suppose that the spicules had been drifted together by currents, except that such a supposition would not account for the characteristic form presented by many of the flints. Of this curious association of well-preserved external form with a mixture of spicules the chalk affords numerous striking examples, of which, perhaps, the best-known is that of *Cæloptychium*\*, which yielded to Zittel quite as many extraneous spicules as are figured here from the Trimmingham nodules, and which, at the same time, presents us with a much more characteristic external form. Another, less known, is that of the so-called Neptune's cup of the chalk: this is incontestably the Cretaceous representative of the existing *Poterion patera*, Hardwicke; so that it may well be named *Poterion cretaceum*. It is a suberite sponge with characteristic outer form, and when alive contained only pin-headed spicules; in its silicified state, however, it is crowded with other forms, which have been introduced into it from without. *Poterion cretaceum* has certainly not been drifted, it has been silicified where it stands; and so, we

\* Abhandl. der k. bayer. Akad. der W., h. Cl. xii. Bd. iii. 1876.

believe, have such of the Trimmingham flints as still retain a definite form. At the same time they have evidently received an addition to their proper spicular complement from sponges of other kinds; and we have still to consider whether these additional spicules were collected by current-action. That currents may have had some influence is clear enough. The flints were not formed at any abyssal depth; as we have seen, the associated spicules indicate limits of 100 to 400 fathoms; and Carter states that even at greater depths considerable drifting is produced by currents. Thus he says\* :—“The dredgings of H.M.S. ‘Porcupine’ indicate, through the specimens now with me, that about 100 miles north of the Butt of Lewis, in 632 fathoms (station no. 57), there must be a bed of sponge-spicules of many kinds, portions of which are rounded by the currents into pebble-like forms, which may one day become the nuclei of flints.”

The observation of Sorby that some specimens of chalk seem to show signs of a gentle washing-action, and the occurrence of a few small grains of quartz-sand in the Trimmingham chalk, are both evidences favouring the idea of current-action. Still I do not think that drifting has occurred to any great extent. The spicules are not sorted out and collected into a purely siliceous layer; but such of them as remain are intimately mixed with granules, coccoliths, and Foraminifera, which do not differ from ordinary chalk material, except in being partly siliceous; in other words, the separation which drifting might be called in to accomplish has not taken place. In like manner the different spicules themselves are confusedly mixed together, large and small alike, with no tendency for the small to occur in one place and the large in another. The once existing flesh-spicules, it is true, are absent—not because they have been washed away, however, but dissolved; for they are invariably absent in fossil sponges and stratified deposits. Neither Zittel nor I have seen a trace of them; and my observations on the comparative readiness with which they undergo solution in caustic potash serve to explain their absence. If drifting has taken place, it must have been to a very slight extent, sufficient to help in mixing the different sponge-spicules together, but not to sort them out into any distinct layer.

Our belief is that the area over which the Trimmingham spicules are now found was once a sponge-bed, where numerous sponges flourished, generation after generation, in a luxuriant meadow-like growth; many of them led a parasitic

\* Ann. & Mag. Nat. Hist. ser. 4, vol. xvi. p. 40 (1875).



or epizoid life upon others; several grew crowded together on the same object of support, just as at the present day one may find no less than seven different species of sponge growing together on one small fragment of *Lophohelia* not an inch square\*. With death and the dissolution of the organism the spicules were set free from the different adjacent sponges, and, falling into the same deposit, naturally mingled together; movements of the surrounding sea-water may very well have taken place, and would serve to render the mixture of the spicules more complete. In this way would be produced a layer of chalky ooze crammed with sponge-spicules of all sorts and sizes. Such sponges as possessed skeletons coherent enough to maintain their general form after death would be covered up and filled in with this mixture of ooze and spicules, and, undergoing silicification, would furnish us with instances of fossil sponges presenting a well-preserved form externally and a curious mixture of spicules within. For some suggestive observations on this subject Dr. Wallich's paper on "the Natural History of the Cretaceous Flints" may be consulted†.

Excepting that sponges do not periodically shed their spicules like leaves and spores, the explanation we have just suggested bears a striking resemblance to the "growth-in-place" theory of our coal beds. In the coal, as in the flints, the structure of the constituents has generally been almost entirely obliterated, yet some few of the leaves or spicules, as the case may be, are occasionally found in an admirable state of preservation; and just as a *Sigillaria* every now and again remains a solitary survivor of a whole forest, so now and then a whole sponge is to be found preserved out of a host of associates now vanished or turned to flint.

(iii) *The solution of the Spicules.*—From the preceding paragraphs it is clear that solution of sponge-spicules has been of very common occurrence. A summary of the evidence in proof of this, however, may not be out of place here. Thus:—1, fossil sponge-spicules are frequently eroded externally and their axial canals enlarged internally; 2, all flesh-spicules, necessarily once present, have entirely disappeared; 3, in many chalk-flints Ventriculite and Lithistid skeletons occur, perfectly preserved as to form, but not as solid network, merely as empty casts; 4, the skeletons of many fossil sponges have exchanged a siliceous for a calcareous composition.

As to the reality of the alleged solution there can be no

\* Carter, Ann. & Mag. Nat. Hist. ser. 4, vol. xii. pl. i. figs. 1, 2.

† Quart. Journ. Geol. Soc. vol. xxxvi. p. 68.

doubt; but as to the means by which it has been effected we have still much to learn. Alexis A. Julien, in a paper of the highest importance on the geologic action of the humic acids, suggests\* that albuminoid or glairy matters and acids akin to the azohumic of Thenard, produced during the submarine decomposition of organic matter, may have been the agents which accomplished the solution. This may very possibly have been the case, though possibly the water at the sea-bottom may, even without the assistance of these substances, have been a sufficiently powerful solvent; and this appears the more likely when we consider the considerable pressure under which such water exists, even at depths no greater than that under which the Trimmingham spicules were dissolved, the depth of water which we have indicated for them (100 to 400 fathoms) giving a pressure of from 20 to 80 atmospheres. An observation of Carter's tends to bear this opinion out; for some spicules which he examined, from depths not much greater than those under which ours were formed, were found to exhibit the usual signs of incipient solution, such as pitting of the surface and enlargement of the axial canals. Yet these spicules came from an area swept by a marine current, where organic matter was presumably not plentiful. The bottom-water of the sea is remarkably free from organic matter; and in this case we probably have to do with solution under pressure. Again, the rapid whitening of the black surfaces of freshly broken flints when exposed to the weather, as in the case of the flint walls in Cambridgeshire, seems to show that even pure rain-water is of itself capable, without any aid from pressure, of dissolving a form of silica much less soluble than that of sponge-spicules. It is true that the presence of a certain quantity of lime in the flints may have rendered them more liable to the action of slightly carbonated water such as rain-water, though, on the other hand, the exceedingly small proportion of lime present, as shown by analyses, may make us hesitate in attributing any great influence to it.

(iv) *The redeposition of Silica*.—After the silica of siliceous skeletons has passed into solution, it is again extricated in the solid state; and, since both the deposition and solution take place in the same deposit, a seeming difficulty presents itself, since one would have thought that the conditions which led to the one would have been incompatible with the occurrence of the other. An explanation is to be found in the fact that the one process is not merely a reversal of the other, and in the possibility that both did not take place at the same

\* Proc. Am. Assoc. Adv. Science, xxviii. p. 396, Saratoga Meeting, 1879.

time. For instance, the silica of the skeletons occurs in conjunction or probably in combination with an organic basis known as *spiculin*; on solution it is liberated from the spiculin and exists in the colloid state, whence it readily passes into the pectous condition, and subsequently becomes crystalline; it is, moreover, probable that, under conditions not yet investigated, a solution of colloid silica may give rise directly to silica in a crystalline form. If it be objected that in this expanded explanation, fact and conjecture are mixed together, I to some extent admit it, but at the same time remark that there is no conjecture in the statement that the silica which passes into solution is a very different thing from the silica which has passed out of solution. The one may be conveniently called organic, and the other mineral silica; the properties of the two are strikingly different; and the process which has really happened has been a solution of organic silica and a deposition of mineral silica, not a solution and deposition of the same kind of silica. In the next place, solution and deposition need not have proceeded *pari passu*; if one succeeded the other only after a considerable interval, there would be time for the conditions to change: an elevation of the sea-floor and a consequent shallowing of the sea might, for instance, have intervened; and if we suppose the silica to be held in solution through the influence of hydrostatic pressure, the diminution of this pressure would lead to its deposition. There are difficulties, however, in the way of this supposition which lead me not to lay great stress upon it.

We shall now proceed to consider the different modes in which the deposition of silica has been effected. Of these there appear to be three, viz. the simple deposition of silica, its deposition as a pseudomorph after carbonate of lime, and in combination with bases forming silicates.

(1) *Simple deposition of the dissolved Silica.*—(a) The simplest case of this is presented by the minute crystals of quartz which frequently occur dispersed through the substance of limestone beds; a figure of these is given in my paper on *Catagma* (*loc. cit.* p. 361). They are mentioned by Zittel (*Lehrbuch der Petrologie*, 1866) and fully described by Mr. T. Wardle of Leek, in his presidential address to the North-Staffordshire Field-Club in 1873. As the 'Proceedings' of this society may not be generally accessible, I venture to quote Mr. Wardle's description in full. He says:—

"My friend Mr. Woodcroft, who has made a careful examination of the Mountain Limestone of Caldon Low, gives the following as the result of his analysis:—carbonate of

lime, alumina, silica, carbonaceous matter, and traces of iron. Out of 30 lbs. of limestone dissolved in hydrochloric acid, there was left a residue which, when well washed with distilled water and dried, was found to contain 680 grains of mud (consisting of alumina for the most part and carbonaceous matter), and 1260 grains (or nearly 3 ounces) of silica, which, when we put it under the microscope, we were delighted to find consisted entirely of microscopic crystals, of six-sided prisms terminated by six-sided pyramids, the usual form of rock crystal. It may be accepted as a fact that in the Mountain Limestone these beautiful crystals are prevalent. Mr. Woodcroft has dissolved many pieces, and always found them. In the Buxton Limestone they occur in larger crystals and a little worn or corroded; but in that of this locality (except in the hydraulic Mountain Limestone of Waterhouse near Leek, in which the silica occurs in an amorphous form) they are always perfect in form, transparent, and very interesting 'objects,' averaging in measurement about the 400th of an inch in length, by the 1000th of an inch in breadth. The smallest are less than 1000th of an inch long. They are beautiful polariscopic objects. The encrinital slabs, which seem wholly composed of fossils, also contain these crystals. They do not appear to be present in the Liassic, Oolitic, or Silurian Limestones."

Prof. A. Renard also mentions precisely similar forms as occurring in the Carboniferous Limestone (assise V. *f*) of Belgium (*loc. cit.* p. 15, footnote). I have myself seen them in the Silurian limestone of Hamilton, Ontario, in the Devonian limestone of Newton Abbot, the Carboniferous of North Wales, and the Lias of Sutton, South Wales; and in all but the Devonian limestone they were obviously associated with the remains of siliceous organisms.

In these crystals we have an instance, disentangled from all complication, of the simple crystallization of quartz from a siliceous solution; and the notion that deposition of silica from diffused solutions could not take place without the presence of an organic nidus is thus completely disposed of.

(b) A similar case to the preceding occurs in some flints, where quartz crystals, with their apices directed inwards, line a cavity in the interior; but these crystals are macroscopic.

(c) The siliceous casts in the interior of some Foraminiferal shells appear to offer a case of the simple deposition of silica.

(d) The last case is that presented by various forms of chalcedonic incrustation. The fossil Lithistids of Blackdown and Haldon afford a good illustration of this. The reticulate skeletons of these sponges are now reduced to the condition

of hollow casts, while the interstices of the network are filled up by a chalcedonic deposit surrounding the exterior of the casts. The chalcedony has a fibrous structure, the fibres radiating from the incrustated surface; where one group of fibres meets another a sharp line of demarcation is produced; and intersecting lines of demarcation make with one another an angle of  $120^\circ$  ("Structure of *Siphonia*," *loc. cit.* p. 816). The chalcedony so constantly appears as a growth upon the siliceous skeleton that it looks very much as if the latter had exerted some special attraction upon the silica in solution, leading to its deposition. The idea finds support in an observation of Carter's, who asserts that in the Haldon Greensand a chalcedonic deposit frequently occurs on the imbedded sponge-spicules, but never on the elastic grains of quartz. On the other hand the deposition of the chalcedony on a siliceous skeleton may be explained without invoking the aid of any specific attraction; for if a solution of silica were to exert a solvent action on the siliceous skeletons bathed by it, it is quite possible that deposition might by the very act of solution be brought about, a molecule of mineral silica being deposited for every molecule of organic silica removed; and the process of crystallization over any surface once set up, would continue in the same place in preference to beginning afresh on some new one.

(2) *Deposition of Silica as a Pseudomorph after Carbonate of Lime.*—It is a curious fact that the action of siliceous solutions on carbonate of lime is not to displace the carbonic anhydride from the latter, but to replace the molecule of carbonate of lime as a whole; it is a fact, however, that has long been well known, though it is only lately that it has been shown to have been concerned in the formation of chert and flints. The valuable observations of Prof. Rupert Jones, the investigation of Hull and Hardman, and the elaborately careful study of Prof. Renard prove conclusively that flint and chert are to a certain extent pseudomorphs after carbonate of lime; and of this the Trimmingham flints furnish us with a fresh demonstration. Thus some of the nodules consist within of ordinary flint, black, translucent, and compact, but exteriorly simply of ordinary chalk with a few siliceous remains scattered through it. Between these two we find every intermediate stage of silicification. Passing from the chalk to the flint, one finds first the coccoliths, Foraminifera, and other calcareous constituents of the chalk converted into siliceous pseudomorphs retaining all the details of their original form, down to the delicate striæ on some of the foraminiferal tests; from the mixture of chalk material and its siliceous

pseudomorphs we proceed nearer the flint and reach a porous superficial layer, formed by the cementation of the siliceous pseudomorphs together into a siliceous network; the side of this network next the flint enters half immersed into it as it were; a step further and we reach the flint itself, the siliceous pseudomorphs being now completely involved and no more distinguishable from one another in the common "fusionment" than the separate snow-crystals of a mass of snow which has been frozen by infiltrating water into ice.

Instances of the deposition of silica after carbonate of lime are so plentiful and well known that it would be superfluous to mention them here; chalcedonized corals and shells are common in British deposits, and usually, so far as I know, in connexion with the remains of siliceous organisms.

Finally, the power of a dilute solution of colloid silica to replace carbonate of lime has been experimentally demonstrated by Prof. A. C. Church\*, who has actually converted a coral into silica by its means.

(3) *The deposition of Silica in combination as a Silicate.*—Since studying the structure of glauconitic grains from the Cambridge Greensand, I have taken a deep interest in the problem of their formation, but have never yet met with a satisfactory solution of it. The occurrence of glauconitic casts in the siliceous chalk of the Trimmingham flints, in the interstitial siliceous chalk of *Pharetrospongia*, and along with siliceous spicules on existing sea-floors is a very suggestive fact; and one sees no difficulty in the supposition that the dissolved silica derived from siliceous organisms should combine with the impurities present in the surrounding sediment, and so give rise to glauconitic deposits; thus, with such matters as iron oxide, alumina, and potash the silica is supposed to combine, while carbonate of lime it merely replaces. The case of the green grains of the Cambridge bed requires a little fuller consideration. In that deposit we meet with the fossil sponge *Pharetrospongia*, in which the structure of a coarse-fibred Renierid is perfectly preserved to us; no doubt it owes its preservation to the thickness of its fibres, the spicular components of which, however, no longer consist of silica, but of carbonate of lime. But if one Renierid sponge existed during the deposition of the Greensand, can we suppose that no other species was associated with it? Is it not infinitely more likely that a great number of others lived at the same time and have since disappeared? In the coprolites of the Greensand we have indeed evidence of the existence of

\* Chem. News, v. 95; Journ. Chem. Soc. xv. 107.

several other kinds of sponges, Lithistid, Choristid, and Hexactinellid, none of which now retains a siliceous composition; and considering that not a single instance is yet known to us of any tender small-spiculed sponge existing in the fossil state, although such must have been present in the ancient seas, it would appear certain that *Pharetrospongia* formed but an insignificant fragment of the sponge-fauna which existed both in the beds from which the Greensand was derived and in the Greensand proper itself. But if this sponge-fauna once existed and has disappeared, what has become of the silica which must have been produced by it? It certainly is not to be found as a deposit of free silica anywhere in the Greensand bed. On the other hand, what is the origin of the abundant glauconitic granules scattered through this bed? The answer that suggests itself is as follows:—The chalk marl which forms the greater part of the Cambridge Greensand still contains a good deal of argillaceous impurities, together with traces of ferruginous matter; and once it contained much more; the silica set free from decaying sponges combined with the alumina, iron, and alkalies present, to the entire exclusion of lime, and formed glauconite, which was deposited in green granules enclosing coccoliths and Foraminifera, some of which had probably been previously replaced by silica, since when examined in thin slices of the glauconite they are without a tinge of green and quite colourless. In the greensand of Devonshire a simple deposition of silica has followed the formation of the green grains, and cemented them and the other materials of certain beds into compact chert.

We appear to have travelled rather wide of the subject of the Trimmingham flints; but though we have not confined ourselves to this subject it has been steadily kept in view throughout, and we are now in a position to take up the scattered threads and to frame a consistent explanation of the flints, complete in all respects so far as it goes, save one, since it does not include an account of how they acquired their external form.

Briefly to sum up, a deposit of sponge-spicules accumulated in the chalk ooze, and in the presence of sea-water under pressure entered into solution. Replacement of the calcareous material of the ooze then ensued, small shells, and many large ones too, being converted into siliceous chalk, not flint, was the result. The chambers of the Foraminifera and the interstices of the chalk were now filled up by a simple deposition of silica, and the siliceous chalk became converted into black flint, an incompletely silicified layer of chalk remain-

ing as the white layer of its surface. Some of the silica combined with the iron, alumina, and alkalis present in the ooze, and so gave rise to the associated glauconitic grains.

The last question which remains for discussion is the origin of the various external forms assumed by flint.

A good deal of misconception appears to have arisen on this subject through a too exclusive attention to one particular form of flint arbitrarily selected as the type of all others. For this (generally the irregular nodular form) a theory is framed, which is then made to account for the rest. Thus, when Dr. Bowerbank attempted to show that flints are silicified horny sponges, he accounted for the flint-veins of the chalk by supposing them to be silicified horny sponges which had grown over the sides of an open fissure at the cretaceous sea-bottom; and Dr. Wallich, after giving an explanation of flint nodules and layers, speaks of the veins as formed by a "sluggish overflow" of silica-saturated protoplasm "into fissures in the chalk." There does not appear much to choose between these rival explanations of the veins: both are attempts to square a preconceived hypothesis with an obnoxious fact.

The forms of flint are chiefly four—those following the outline of some enclosed sponge-skeleton, irregular nodular masses, tabular sheets, and veins.

1. *The tabular sheets*, as offering the simplest case, may be taken first; they have in all probability been formed by the solution and redeposition *in situ* of an extensive bed of sponge-spicules. All the flint layers which I have examined exhibit abundant casts of various kinds of sponge-spicules confusedly mixed together. The chert beds of the Devonshire greensand, analogues of the chalk-flint layers, also contain numerous casts of spicules; and in the same formation deposits of loose spicules occur several feet in thickness.

2. *The Flint veins*.—Of these more than one explanation is possible; but we select the following as the most likely. We may fairly assume that the chalk traversed by the veins was permeated by a solution of silica derived from siliceous remains, and this at a time so far subsequent to its formation, that it had already become compact enough to be broken by fissures; whether organic matter, as Dr. Wallich understands it, would have endured so long as this, is uncertain but not probable. The solution of silica was bounded on one side by a free surface, that of the fissure; and free surfaces are eminently determinative of deposition, not only of silica but of calcite and many other minerals as well; we see this in geodes and in the mineral deposits formed within shells. It



is true that in most cases silica so deposited takes a crystalline form; but crystals of silica in the presence of chalk are of rare if not of unknown occurrence. We do certainly find them in the interior of some flints; but then they are seated on, and surrounded by, the siliceous matter, and are nowhere in actual contact with the chalk itself. It would indeed appear that the simple deposition of siliceous matter is impossible in the chalk; the first stage of deposition in this deposit is always that of replacement.

The deposition of the silica being determined as to place by the presence of the fissure, began, as we might expect, by a replacement of the chalk, silicifying the walls of the fissure; subsequently, as in the Trimmingham flints, a simple deposition of silica followed, cementing the siliceous chalk into compact structureless flint, and a flint vein was the result.

It will be seen that we merely make use of the fact that free surfaces are often surfaces of deposition, without explaining it; but to enter fully into this subject would be beyond the scope of the present paper.

3. *Flints formed about Sponges*.—The irregular nodules may be left to be dealt with after we have considered those forms in which the general outline of some known sponge, such as a *Ventriculite* or *Siphonia* is represented, or more generally those which have obviously been formed about some kind of sponge-skeleton. The characters of these are well known. One meets with, say, an egg-shaped mass of flint; and on breaking it open a conical *Ventriculite* is seen in the middle; sometimes the form of the *Ventriculite* is more closely represented, the vasiform skeleton being merely coated inside and out by a layer of flint, often about an inch thick; while occasionally a *Ventriculite* may be met with simply girdled by a ring of flint round the middle, the rest of the sponge remaining unenveloped. Sometimes the body of the sponge is enclosed, and not the roots, sometimes the roots and not the body. The sponges included are of various kinds—*Siphonia*, *Poterion*, and, in one instance known to me, a *Tethya*.

In attempting to find an explanation for the form of these flints we may consider the following suppositions:—(i) The form may have been determined by the presence of animal matter (protoplasm, *Wallich*), or (ii) of the products of its decomposition, or by the presence of the siliceous skeleton of the sponge, either (iii) through a special attraction exerted between it and silica in solution, or (iv) by its furnishing an extra supply of silica to the surrounding water, or (v) on account of its providing a free surface of deposition.

The first explanation may best be stated in Dr. Wallich's own words. Thus, speaking of the irregular nodules, he says:—  
 . . . . . “those characteristic amœbiform outlines which, according to my hypothesis, are dependent on the presence of, and the combination of the silica with, the accumulation of nearly pure protoplasm still sufficiently recent to have resisted admixture with calcareous or other matter” (*loc. cit.* p. 79).

As I have already shown in the earlier part of this paper that flints originate as silicified chalk, we need not spend time on a formal confutation of this hypothesis; but when Dr. Wallich remarks that “the various conditions that present themselves from the earliest elimination of the silica from the sea-water to the period when it becomes finally consolidated, have never, that I am aware, been consecutively followed out” (*loc. cit.* p. 89), I would take the liberty to refer him to a paper of my own, printed in abstract in the *Quart. Journ. Geol. Soc.* vol. xxix. p. 76 (1873), where the steps are perhaps almost as consecutively followed out as in Dr. Wallich's paper itself. As my paper has never been published in full, I shall make no apology for giving here a rather lengthy extract from it.

It is “necessary to inquire next how far there are any facts in chemistry or physics which throw any light on this singular and intimate connexion between animal matter and mineral substances. One fact noticed by Graham, and which any one may experimentally verify, is very noticeable, viz. that silicic acid has the property of actually combining with such substances as albumen and gelatin to form with them distinct chemical compounds, silicate of albumen and silicate of gelatin.

“If, then, such animals as sponges flourish in the bed of an ocean which contains a sensible amount of silicic acid, when these creatures die the consequence will be that the water, finding ready access to every corner of their organism, will yield its silicic acid to the greater attraction of the sponge-fibres, and will form with them a well-marked, definite chemical compound; and it is conceivable that in course of time this compound, like all other highly complex organic bodies, will decompose, its carbon, hydrogen, nitrogen, and oxygen will disappear, and the result will be a concentration of the silica in the form of flint, very much in the same way as carbon concentrates in coal.

“Other processes, of course, would proceed at the same time, aiding to the same end; any sodic silicate in the water would probably be decomposed by the carbonic anhydride escaping from the decomposing animal matter, and would form

sodic carbonate and silicic acid, which would combine at once with the organic matter of the sponge to form a silicate with it. Now sodic silicate is a crystalloid body, and would easily find its way into the interior of the fibres and sarcode of the sponge; but if decomposed there into silicic acid and sodic carbonate, the silicic acid would be entrapped in the organism, since it is colloidal and could not diffuse out. In the same way, when once a coating of colloidal silica had been formed round any body, while sodic silicate could easily pass through it, yet, when decomposed in the interior into silicic acid, as before it would be unable to return outwards, since the colloidal silica coating the organism would act as a dialyzer and would prevent it.

"Immediately the organism had grasped and extricated from the water a molecule of silicic acid, a difference of specific gravity would be set up between the spot where the silicic acid had disappeared from solution and the surrounding water. This difference would be rapidly equalized by diffusion, in which way the water which had yielded its silica to the sponge would be replaced by fresh supplies, the silica of which would again be removed and combined with the substance of the organism, throughout which this process would be actively going on, until in time it had combined with all the silicic acid which it had power to fix. In this way we produce that circulation of water which is absolutely necessary to any theory of fossilization, and explain how with merely molecular currents the sufficient supplies of silicic acid would be brought within reach of the organism undergoing fossilization. While the sponge was exposed to the direct action of the ocean-water, if it ever was so exposed, molecular currents might expedite the process; but when covered up by the fine sediment, in which it is afterwards found imbedded, it could only derive its mineralized water in this molecular way, by the well-known action of diffusion and without involving any of the transcendental mysteries of an undiscovered attraction. An observation of Petzholdt's shows that in certain cases the process of fossilization really has continued in an organism after it has been silted up. Petzholdt found that in dolomite occurring immediately around a flint, there existed but 2.31 per cent. of silica, while that a little further removed contained a little more than twice as much; the precise figures are 4.73 per cent. This may be explained on the hypothesis that animal matter entered into combination with the surrounding silicic acid and continued to do so after it had been overwhelmed in silt, and until its affinities were satisfied.

"Bischoff says that in the Infusorial beds of Rolt and Geishingham Ehrenberg found that no empty shells occur, since all the smaller species are filled with siliceous matter—a fact that meets with its explanation on this combination-and-concentration hypothesis. The same author mentions the occurrence of a Belemnite in which the calcareous rostrum was all replaced by barytic carbonate, whilst the more organic phragmocone was replaced by silica.

"It is generally assumed that the casts of Echinoderms in flint required for their formation the intermediate agency of sponges which inhabited their interior. No doubt sufficient evidence has been adduced to prove that this has certainly been the case with some of these casts; but one may just point out that it need not have been so with all of them; for it is possible that in a good many instances the animal matter of the decomposing Echinoderm itself may have sufficed to separate the silica from the surrounding medium without requiring invariably the assistance of indwelling sponges. So, too, in regard to the teeth of *Mosasaurus* found by Mr. Charlesworth to be injected with silica, we are not reduced to supposing with Dr. Bowerbank that the presence of this silica required for its explanation the preexistence of a sponge extending throughout the tubules of the tooth. This need by no means have been so, since the animal matter which we know once was present there is of itself sufficient explanation of the presence of the silica. In all these and similar cases the silica concentrated by the dissipation of the animal matter, which served in the first place to imprison it from solution, might remain in the crystalloid or the colloid state; at this distance of time we cannot determine. The silica of flint is generally found in a cryptocrystalline condition: no tendency to a crystalline appearance is seen in the general mass of the nodule; but, at the same time, it acts feebly on polarized light. This, however, proves nothing concerning its original condition, whether it was colloidal or crystalline; for I have lately succeeded in determining as a fact what has long been held as a hypothesis, viz. that as glass, when kept at a moderately high temperature for a long while, becomes devitrified, *i. e.* crystalline, so flint, in the course of ages, may have lost its originally colloidal properties and settled down into the static state of crystalline silica. The way in which I have determined this leads me to the subject of the well-known silicified shells of Blackdown. No one who has seen the silica filling these shells could for a moment assent to Dr. Bowerbank's extraordinary hypothesis of its spongy origin; it is evidently derived from the separation of silica from the siliceous waters

furnished by the action of carbonic acid on the sand of the formation, and in the following way :—Water holding silicic acid in solution, both in the crystalline and colloid condition, filters downwards through the beds of the Blackdown sand, and in its way meets a shell turned like a basin to receive it. Now whether the silica shall pass through the shell or not depends on two things, the permeability of the shell and the state, colloidal or crystalline, of the silica. The shell is generally permeable, and from its lamellæ of membrane acts precisely as a dialyzer : colloidal silica in solution on reaching the shell is stopped ; the water passes through, leaving the silica behind. This process goes on continuously till the silicic acid is so far concentrated that it sets and prevents any further action, or, as in some cases, actually overflows the shell. This concentration of silicic acid from weak solutions by the power of dialysis furnishes us with a very simple explanation of the condition of the Blackdown shells, without invoking the aid of hypothetical sponges. The *crystalline* silica, which the percolating water carries in solution, passes through the shell, and in some cases, under favourable conditions, crystallizes out in long fine prisms closely apposed to form a mamillary layer of chalcidonic appearance. Now let us see what happens to the calcic carbonate of the shell itself. The action here is one of loose chemical affinities. The water carries in solution as much silicic acid as it can hold. Calcic carbonate has a greater affinity for carbonated water than has silica ; consequently the calcic carbonate unites itself with the carbonated water, displacing some quantity of silicic acid, which takes its place in the shell and eventually entirely replaces it. We have on these grounds every reason, *à priori*, to believe that the silicic acid now found caught inside the shell was once colloidal, and that the silicic acid which has passed through the shell was crystalline. What do we find on examining sections of the silica in these two positions ? Not one colloidal and the other crystalline, as we should anticipate, but *both* crystalline. But there is this very important difference between them, viz. that in the one we should expect to be colloidal, crystallization has commenced from various centres in the mass, scattered mostly on both upper and lower surfaces, as if the whole had once been a jelly in which centres of crystallization were set up, from each of which crystallization radiated in all directions throughout the mass, till the crystals of different centres interfered with one another, encroached on one another, and the process was completed. But the one we might expect to be crystalline is in a very different condition : in this crystallization was evidently not an after-

thought, but at work from the very beginning; and the crystals commence all along the boundary of the shell, from which the silicated water oozed out. From this process of reasoning we conclude that colloidal silica has the power of changing, in course of time, into the static or crystalline condition. In the case of the Blackdown shells the colloidal silica probably remained for a long time in a jelly-like condition, which may, among other things, help to account for its perfect crystallization.

"Thus the crystalline state of flint nodules offers us no evidence for or against our theory of the formation of these fossils. This theory may be summed up under two heads:—(1) combination of silicic acid with animal matter of various kinds—a chemical *fact*; and (2) concentration of the silica from the silicate of animal matter thus formed, by the extrication of the organic part of the compound. This is a pure assumption, but one which agrees very well with other well-known facts in chemistry."

That organic matter has in certain cases been replaced by silex may be considered certain, the numerous observations made from the time of Von Buch and Bischoff down to the present day seem to leave no doubt on this point; the occurrence of silicified wood is an instance; and still more striking instances are known, as that of the nuts so often mentioned, in which the soft kernels have been converted into silex, while the shell remains unchanged—or, better still, that of the marvellous silicified *Trigonia* from the Portland beds of Tisbury, Wilts, described and figured by Mr. Charlesworth as still showing the structure of the animal, even to the separate filaments of its branchiæ; and if, in these cases, silicification of organic matter has occurred, it certainly might in that of the chalk-flints; but whether, as a matter of fact, it has so assisted in the formation of these bodies, is quite a different question; and what little evidence we can find bearing directly on the point seems to show that it has not. Some little light is thrown on the subject by the condition of the sponge enclosed in the flint. Very frequently it consists of a network, the interstices of which are empty and not filled with flint, the solid flint forming a complete enclosure to the sponge, but stopping short internally at the borders of the skeletal network, just where animal matter might be expected to have been most abundantly present. If we try to elude this difficulty by supposing the sarcode to have been already shed over the subjacent ooze, then all connexion is lost between the form of the sponge and that of the enveloping flint; or, again, if we suppose the sponge to have been completely covered by the

ooze, so that it might lose its sarcode by diffusion all round before silicification took place, then we are involved in the admission that a considerable time had elapsed between the death of the sponge and its silicification, since chalk accumulates slowly; and during this lapse of time the sarcode would have become decomposed.

(ii) We dismiss, then, the notion that protoplasm itself can have had any direct influence in determining deposition; but perhaps the products of its decomposition may have been more effectual, and we might attempt to substitute for Dr. Wallich's hypothesis a supposition of Alexis A. Julien, who says:—"I would therefore modify Sollas's theory by suggesting that *during the decomposition* of the sarcode of both animal and vegetable organisms, after death, gelatinous or colloid substances are generated, resembling *glairine*, which are soluble in sea-water, which combine with silica, and may therefore convey and concentrate it, dissolving its particles disseminated through submarine sediments, and which may in certain forms, produced by gradual oxidation, act also as acid solvents of lime, oxides of iron and manganese, &c. To this idea, in part, an early opinion of Bischoff approaches: 'Silicifications are nothing else than the result of combinations between the crenic acids (Quellsäuren) formed through decomposition of organic matter (*e.g.* of mussels and oysters) and silica, which in aqueous solution, *e.g.* as in the water of springs, comes into contact therewith'" (p. 364).

Julien, however, does not attribute the form of flints to the organic matter furnished by them, but speaks of the dissolved silica being deposited "... around the undissolved siliceous organisms or particles as nuclei."

We reach now the explanations based on the supposition that the skeleton of the sponge has had the chief part in determining the deposition of the surrounding flint; and (iii) with regard to the first notion, that silica has been attracted by the sponge-skeleton and so deposited, one may point out that, if true, silicification should have commenced from the surface of the fibres of the skeleton and proceeded outwards, while observation shows that this has not been the case, the skeleton remaining an empty porous network after being completely enclosed in flint. (iv) With regard to the suggestion that silica has been contributed by the sponge-skeleton to the pervading solution of silica, and so rendered the latter concentrated enough to bring about a replacement in the surrounding chalk ooze, there seems much more in it. The chief part of the silex enclosing the skeleton has clearly been gathered from without; but some silica has disappeared from

within, and this may have been just sufficient to lead to the deposition of the silix round the sponge in preference to some other place. The frequent absence of flint within the skeleton may be owing to the absence of chalk-ooze, which in these cases had failed to penetrate into the interior of the sponge. The mere zonal enclosure of the sponge by a ring of flint may be accounted for by supposing that the sponge-spicules from which the flint was derived formed a bed surrounding it at the level of the ring, but were not present in sufficient quantity to produce silicification above or below that level.

The complete enclosure of a sponge in a more or less spherical mass of flint may be accounted for by supposing that silicification once started at any place would continue there in preference to recommencing at a fresh centre.

Amongst some notes I made in 1873 I find a drawing which somewhat strengthens the notion that silica proceeding from the sponge-skeleton may have led to deposition. It shows an *Ostrea* seated on a *Ventriculite*, which has been everywhere coated with silica, except where the *Ostrea* is attached, the oyster lying below the general surface of the flint, which bulges out all round it. I feel some hesitation in placing entire dependence on a note made so long ago, when I was only just beginning the study of flints: but I well remember making the observation; and if the fact be as represented it would certainly seem as though the surrounding flint had been deposited through the influence of something proceeding outwards from the sponge, either silica in solution or, less likely, organic matter, and that the obstruction furnished by the oyster had prevented the accumulation of the silix immediately over it.

(v) Finally there is the supposition that the sponge skeleton may have led to deposition by furnishing a free surface to the siliceous solution. This is likely enough, but it is difficult to prove or disprove. In the case of other organisms, such as Echinoids, the tests of which have determined the deposition of silix, supposition iv. is excluded on chemical grounds, and the last supposition appears to be the only probable one. The characters of the flint urchins agree very well with it: frequently the silix is found only within the test, the siliceous solution having filtered through the walls, filling up the ambulacral pores; occasionally the test is only half filled with silix, as though it had rested half immersed in a bed of sponge-spicules; sometimes it is quite filled; sometimes the silix protrudes from the mouth and anus; and, lastly, the test is sometimes not only filled with silix but completely enclosed in it.



4. Lastly we have to consider the irregular nodules of flint. These, by their fantastic flowing outlines, are responsible for much of the theorizing which can only regard flint as a silicification of organic matter. Thus Dr. Wallich repeatedly lays stress on "the unique amœbiform nodulation of the flints,"—though one may remark that one of the characteristic features of an amœbiform outline is that it seldom remains the same two minutes together; and this cannot be said of flints, although, as Dr. Wallich speaks in another place of the flints showing "signs of the specific contractility of colloid silica," one might infer that he does not regard this character as absent. A flint moving by means of its pseudopodia would indeed be an interesting object; but perhaps the distinguished writer merely alludes to the excessive shrinking which colloid silica undergoes in passing from the pectous to the solid state; and certainly to one who has experimented with colloid silica, the wonder on Dr. Wallich's hypothesis would be, not that the flints show signs of shrinkage, but that they do not present them more markedly. The time for conclusions based on superficial resemblance is now gone by; we no longer regard "dendrites" as fossils on account of their moss-like form, nor profess to be "able to tell an honest man by the smell."

The direct action of organic matter seems to be excluded by the great lapse of time which would be required for the solution of sponge-spicules, and during which the organic matter would decompose and wholly disappear.

Many concretions exhibit an irregular form besides flints (the cornstones of the Old Red Sandstone for instance); only the irregularity is carried further in the nodular flints than in most cases.

The form of the nodules simply indicates the irregular distribution of siliceous solutions about an irregular bed of sponge-spicules, at the time they replaced the surrounding chalk and deposited silica in its interstices.

This paper has much exceeded the length I proposed on commencing it; and I will only remark in conclusion that at length, out of much that is uncertain, some few fixed points in the history of flints begin to appear. That the silica composing them has been derived from sponge-spicules is no longer a mere assertion, but a well-ascertained fact; that it has been deposited in the first place as a pseudomorph after carbonate of lime is also clear, and no less so that subsequently a simple deposition of silica converted the siliceous chalk into flint. Various causes have determined the external forms of flint, chiefly the distribution of the spicules which have furnished it, but partly the existence of open fissures

and cavities. And above and beyond the particular question of the formation of flint is the general fact that of the vast multitude of spicules which must have existed in nearly all stratified formations, only an insignificant remainder is now to be found; and in those which have disappeared we have the key to the great variety of silicifications which characterize ancient sediments.

## EXPLANATION OF THE PLATES.

## PLATE XIX.

- Figs. 1-3.* Spicules of *Discodermites cretaceus*, Soll.  
*Figs. 4, 5.* *Corallistes cretaceus*, Soll.  
*Fig. 6.* Curved acuate spicule, possibly from an Echinonematous sponge.  
*Fig. 7.* Cylindrical spicule, possibly from *Corallistes cretaceus*.  
*Fig. 8.* *Rhagadinia Zitteli*, Soll.: dermal spicule.  
*Fig. 9.* Acuate of *C. cretaceus*?  
*Fig. 10.* *Rhagadinia Zitteli*: body-spicule.  
*Fig. 11.* Lithistid spicule.  
*Fig. 12.* *C. cretaceus* (?): body-spicule.  
*Fig. 13.* *Nanodiscites parvus*, Soll.: dermal spicule.  
*Fig. 14.* *Eurydiscites irregularis*, Soll.: dermal spicule.  
*Fig. 15.* Small acerate (*R. Zitteli*?).  
*Fig. 16.* *Macandrewites Vicaryi*, Carter: body-spicule?  
*Fig. 17.* Tuberculated skeleton-corpuscle.  
*Fig. 18.* *Podapsis cretacea*, Soll.: body-spicule.  
*Fig. 19.* Forked spicule (*P. cretacea*?).  
*Fig. 20.* *Macandrewites Vicaryi*, Carter: dermal spicule.  
*Figs. 21, 22.* *Compsapsis cretacea*, Soll.: body-spicule.  
*Fig. 23.* *Podapsis cretacea*.  
*Fig. 24.* Disciform spicule of unknown nature.  
*Figs. 25, 26.* *Podapsis parva*, Soll.: body-spicules.  
*Fig. 27.* Dermal spicule (*Corallistes*?).  
 (All magnified 54 diameters.)

## PLATE XX.

- Figs. 28, 29.* *Pachastrellites fusifer*, Soll.  
*Fig. 30.* *Pachastrellites* (? *globiger*).  
*Figs. 31, 32.* *Tethylites cretaceus*, Soll.  
*Fig. 33.* Acuate spicule of *T. cretaceus* (?).  
*Figs. 34, 35.* *Geodites cretaceus*, Soll.  
*Fig. 36.* *Geodites*; small globate spicule.  
*Fig. 37.* *G. cretaceus*.  
*Figs. 38, 39.* *Pachastrellites globiger*, Soll.  
*Fig. 40.* *P. globiger* (?).  
*Fig. 41.* *Dercitites haldonensis*, Carter (?).  
*Fig. 42.* *Triphyllactis elegans*, Soll.  
*Fig. 43.* *Geodites cretaceus*.  
*Fig. 44.* *Pachæna Hindi*, Soll.  
*Fig. 45.* *Geodites cretaceus*.  
*Fig. 46.* *Rhopaloconus tuberculatus*, Soll.  
*Fig. 47.* *Dercitites haldonensis*?  
*Figs. 48, 49, 50.* Casts of Foraminifera.  
*Fig. 51.* Head of an acuate spicule (? *Tethylites*).  
*Fig. 52.* *Pachæna Hindi*.

- Fig. 53. Large sexradiate spicule.  
 Fig. 54. *Pachæna Hindi*.  
 Fig. 55. *Geodites cretaceus*.  
 Fig. 56. *Pachæna Hindi*.  
 Fig. 57. Small sexradiate spicule.  
 Fig. 58. Anchoring-spicule of a Hexactinellid.  
 Fig. 59. *Pachæna Hindi*.  
 Fig. 60. Anchoring-spicule of a Hexactinellid.  
 Figs. 61, 62, 63. Sexradiate spicules.  
 Fig. 64. *Pachæna Hindi*.  
 Fig. 65. Anchoring-spicule of a Hexactinellid.  
 Fig. 66. *Scoliorhaphis*?  
 Figs. 67, 67 a. Small sexradiate spicules.  
 Fig. 68. Anchoring-spicule.  
 Fig. 69. Spicule similar to a common form in *Euplectella*.  
 (All magnified 54 diameters, except figs. 67 and 67 a.)

LIX.—*Descriptions of two new Coleoptera from Madagascar.* By CHARLES O. WATERHOUSE.

AMONG some Coleoptera recently received from Madagascar, forwarded to the British Museum by Mrs. Toy, I find the two following species, which appear to be undescribed.

**Cetoniidæ.**

*Coptomia celata*, n. sp.

Flavo-viridis, nitida; elytris flavescens, ad apicem maculis duabus olivaceis; pygidio piceo, crebre transversim striolato.  
 Long. 9 lin.

Somewhat resembles *C. quadrimaculata*, Waterh., in colour, but is more elliptical in form, with less-projecting shoulders to the elytra. It is closely allied to *C. prasina*, Burm.; but, besides the difference of coloration, it differs in having the pygidium more strongly striolated. The sternal process is similar in form; but the portion which is formed by the mesosternum is not much longer than its greatest width; whereas in *prasina* it is at least twice as long as wide; the apex is rounded, shining brown. The elytra are sordid yellow, with an olivaceous spot just before the apex; the six dorsal striæ are very deep, the second and fourth interstices being much broader than the third and fifth.

*Hab.* Antananarivo.

**Lamiidæ.**

*Rhaphidopsis pulchra*, n. sp.

Nigra, dense furfurata; capite thoraceque læte fulvo-ochraceis, hoc tuberculo laterali punctisque duobus posticis nigris, elytris pallide flavo-albidis nigro maculatis.  
 Long. 10 lin.