seems to belong; the *Acer ampelophyllum*, as to the true nature of which there is still much doubt, especially in the absence of its fruit, would take its place, judging from its leaf, among the smallest species of the genus.

Thus there would remain only *Ulmus plurinervia*, the leaf of which is of tolerable size, and which, even without this indication, might have constituted an actual tree. For this, its probably distant station may sufficiently explain the rarity of its impressions.

To sum up,—in spite of obscurities which it is impossible entirely to elucidate, it is certain that nearly the whole of the organisms with deciduous leaves in the flora of Aix indicate limited dimensions, denoting mere shrubs; and if there were trees among them, this denomination could only be applied to the smallest number, and, so to speak, to a single species.

We terminate these considerations, which have been perhaps treated at rather too great a length, but in which the novelty of the subject necessitated more development than in ordinary cases, by formulating our conclusions as follows:—In accordance with all the indications, it is extremely probable that the plants with deciduous leaves of the flora of Aix only played in it a secondary part; and if their impressions are very rare in the beds formed at that epoch, their station at a little distance from the ancient shores, their distribution as isolated individuals, and the small size of most of them have concurred to produce that result. We affirm, lastly, that the periodical fall of the leaves in these species, far from implying the existence of a cold season, is a phenomenon very reconcilable with the high temperature which is indicated by the profusion of tropical forms in the flora of the Gypsum of Aix.

XXX.—*Remarks on the Rev. S. Haughton's Paper on the Bee's Cell, and on the Origin of Species.* By Alfred R. Wallace.

My attention has been called to the paper in the 'Annals' for June last on the above subjects, the author of which seems to me to have quite misunderstood and much misrepresented the facts and reasonings of Mr. Darwin on the question. As some of your readers may conclude, if it remains unanswered, that it is therefore unanswerable, I ask permission to make a few remarks on what seem to me its chief errors.

Mr. Haughton combats the views not only of those who believe that the regular structure of the Bee's comb can be accounted for through the agency of "natural selection" and variation, but also of the opposite school, who impute to the Bee a super-

natural or divinely inspired instinct, by which it is enabled to construct its cells on true mechanical and mathematical principles, so as to combine the requisite accommodation for rearing its brood and storing its honey, with the greatest amount of strength and the utmost economy of material. In his opinion of this last school I quite agree with him, but think he has not pointed out its weakest points. If we consider the cell as adapted to the size of the grub and young bee, and in its relations to the cells immediately surrounding it, there can be no doubt that the form of the cell itself, with its pyramidal base and arrangement in double tiers, gives the greatest economy of space and material possible. But if we look at the whole comb suspended vertically by its upper side only, we shall immediately perceive that the strain upon its uppermost rows of cells is many times greater than that upon its lower ones; so that, if economy of material was the main object of this beautiful structure, and the attainment of such economy was secured by unerring wisdom, the walls of the cells should regularly decrease in thickness from the upper to the lower part of the comb. The same mathematical knowledge that enables us to see the beauty and economy of the form of the individual cells, as surely points out the great waste of material in building the upper and lower portions of the comb of the same thickness and strength. We have here, I think, a conclusive argument against the notion that the bees are guided by any supernatural impulse to construct their cells on the best mathematical principles, so as to economize, in the highest degree, labour, space, and material.

When Mr. Haughton attempts to overthrow the theory of Mr. Darwin on this subject, we are compelled to demur to many of his statements, which, indeed, are often so deficient in clearness as to suggest the idea that 'The Origin of Species' has been but superficially studied by him. In his first paragraph, for example, he speaks of a class of writers by whom "the geometrical properties of the cells are alleged as a sufficient cause for the production of the insects that make them, from the advantage which these forms of cells are supposed to possess over other forms—advantages said to be so important as to decide the battle of life in favour of the insects that adopt the geometrical plan of making their cells." This is surely a most unfair statement of the doctrine that simultaneous favourable variations in structure and habits, accumulated by natural selection, may act and react on each other, and thus ultimately lead to such a modification of the insect as may better adapt it for constructing the most advantageous form of cell. Mr. Haughton's statement of the case is, that the cell made by the bee is a sufficient cause for the production of the bee; and he would

have his readers believe that this absurdity is maintained by the writers he alludes to.

The author then describes the following three forms of cells which he has observed, but does not always express his meaning with sufficient accuracy:—1. Hexagonal cells, somewhat pyramidal, with a rounded extremity. The British tree-wasp and the genus *Polistes* make cells of this form, in small groups, and often of a very fragile papery material. 2. "Hexagonal cells formed of adjoining prismatic figures, with rectilinear axes, terminated by a truncated plane, at right angles to the axes of the prisms." I have quoted this elaborate description literally, because I am quite unable to understand what the author means by a "truncated plane," which renders his meaning somewhat obscure. The cells of this form are said to occur in wasps' nests from the West Indies and South Africa. 3. The bee's hexagonal cell terminated by three faces of a rhombic dodecahedron, each of which forms one-third of the base of one of the cells of the opposite layer. It is not stated, but may be inferred, that the first two forms of cells are in a single layer only; and all these varieties of cells, it is said, may be accounted for "simply by the mechanical pressure of the insects against each other during the formation of the cell." Again, at page 428, "The true cause of the shape of the cell is the crowding together of the bees at work, as was first shown by Buffon. From this crowding together they cannot help making cells with the dihedral angles of 120° of the rhombic dodecahedron; and the economy of wax has nothing to do with the origin of the cell, but is a geometrical property of the figure named." There are, however, several important objections to this pressure-theory. Many exotic tree-wasps construct little groups of three or four hexagonal cells, only one or two insects working at them together. Here is no crowding, yet they are hexagonal. Again, a Mexican bee (*Melipona domestica*) makes a comb of cylindrical cells, only partially hexagonal; and in the Malay Islands there is a domesticated bee which makes oval cells, and though the insects are kept in hollow bamboos for hives, yet the crowding together does not make their cells hexagonal. The wild bee of Borneo, on the other hand, suspends its comb from the arms of lofty trees in the free air; and if crowding had *all* and economy *nothing* to do with it, one would think that here the cells should retain their normal cylindrical form; instead of which, they are as beautifully geometrical as those of our own hive-bee. But, what is still more important, Mr. Darwin states (Origin of Species, ed. 3, p. 251) that our bees build the cell-wall at first rough and ten times as thick as it is to remain when finished, it being afterwards gnawed down to the proper thinness. Here

is a complete proof of economy of wax rather than economy of labour, and a complete disproof of the theory of circular walls pressed into hexagons by the crowds of struggling bees, which is given us as a new theory of the formation of the bee's cell, unsupported by a single original observation.

To finish this subject of the bees, we will now pass to page 427, where Mr. Haughton produces his most crushing argument. He seems to suppose that it is necessary to the theory of Mr. Darwin that there should have been a number of species of bees, now extinct, filling up the gap between the single round cell of the humble-bee and the perfect geometrical structure of the hive-bee, each of them using a little less wax than the preceding one, and that, to effect this, it is necessary that there should have been a bee building a *triangular* cell, and after that, one building a *square* cell, before arriving at the *hexagonal* cell of the hive-bee. But in this view there is a misconception of the conditions of the problem. It is true that, to fill up a given space with cells of a given *area* and walls of equal thickness, the triangle will be more economical of material than the circle (with solid intervals), and the square more economical than the triangle. The primary use of the cell, however, is not the storing of honey—but the accommodation of the larva and pupa; for this it must have a certain *diameter*, and the triangular cell must therefore circumscribe the circular one, and will then be found to require more materials even than the circular cell with solid intervals, without taking into account the fact that the sides of the triangular cells, being without support in their whole length, would have to be thicker than those of any other form, if of equal strength. The same argument will apply in a less degree to the walls of a square cell.

A still more serious error exists, however, in supposing any such extravagantly shaped cells requisite to form the gradual passage from the circle to the hexagon, in order that every step of the process may give its proportionate saving of material. Let the reader draw a number of equal circles in contact, and he will at once perceive how very simple it is (considering that the bees build the cell-wall of a uniform thickness, and reduce it to the smallest serviceable dimensions by gnawing down the growing walls) to suppose them, when material was scanty, to gnaw out a little of the solid triangles left between the circles. The amount of intelligence perceptible in the habits of most insects renders such an act by no means beyond their capacities; and as every step in this direction would tend to the well-being of the community, what was at first done under the pressure of necessity would at length become a regular practice, and finally settle into that class of hereditary habits which we call instinct.

Some of these steps do actually occur in the *Melipona domestica* and other bees; and the immense quantity of honey consumed by the hive-bee to make a small quantity of wax, as well as its curious habit of cutting down the walls of its cells to a uniform thickness, are certainly very strong arguments in favour of this view.

Exactly the same arguments will apply to the origin, step by step, of the lozenge-formed planes forming the pyramidal base of the cell as to the hexagonal form of its wall; for these planes are the simple result of gnawing away the superfluous wax in the angles between the alternate spherical bases of the opposite layer of cells; and when this wax is so much gnawed away as to reduce all the walls of the cells to an equal thickness, the true geometrical figure which we see is the necessary result. (Origin of Species, p. 247.) Itis evident, therefore, that all the minute calculations of geometers respecting the amount of saving in this pyramidal base over a *flat* base to the cell is altogether beside the question, because a flat base could not arise out of spherical alternate bases in contact, by any such simple successive steps as are shown to result in the existing form.

On the question of the "origin of species" Mr. Haughton enlarges considerably; but his chief arguments are reduced to the setting-up of "three unwarrantable assumptions," which he imputes to the Lamarckians and Darwinians, and then, to use his own words, "brings to the ground like a child's house of cards." The first of these is "*the indefinite variation of species continuously in the one direction.*" Now this is certainly never assumed by Mr. Darwin, whose argument is mainly grounded on the fact that variations occur in *every direction.* This is so obvious that it hardly needs insisting on. In every large family there is almost always one child taller, one darker, one thinner than the rest; one will have a larger nose, another a larger eye: they vary morally as well; some are more poetical, others more morose; one has a genius for numbers, another for painting. It is the same in animals: the puppies, or kittens, or rabbits of one litter differ in many ways from each other—in colour, in size, in disposition; so that, though they do not "*vary continuously in one direction,*" they do vary continuously in many directions; and thus there is always material for natural selection to act upon in *some* direction that may be advantageous.

In his remarks upon this "unwarrantable assumption" (which is altogether his own), Mr. Haughton has the following passage:—"In the writings of Darwin there is this singular inconsistency, that, while he shows the utmost effects of human breeding on domestic animals to be capable of production in ten or twenty years, he denies the right of his adversaries to appeal

to the unaltered condition of the ass, the ostrich, and the cat for 3000 years," &c. The first part of this sentence is so completely out of the pale of grammatical construction, that I must conclude Mr. Haughton writes a very bad hand, and did not correct the proofs. But, so far from Mr. Darwin denying his opponents the use of the facts above alluded to, he himself offers them far stronger ones, in the many species of shells which have lived unchanged since the middle tertiary epochs, and of mammals whose remains are found in beds which testify that they have survived important changes of the earth's surface. No one who understands the theory of natural selection will imagine that these facts are in any way opposed to it.

The second supposed "unwarrantable assumption" is, "*That the causes of variation,* viz. natural advantage in the struggle for existence (Darwin), *are sufficient to account for the effects asserted to be produced.*" There certainly never was a more unwarrantable assertion made, than that Darwin assigned "natural advantage in the struggle for existence" as "the cause of variation." Darwin over and over again declares that the *cause* of variation is unknown (Origin of Species, pp. 8, 38), though the *fact* is certain and undeniable. Natural selection, acting through advantage in the struggle for existence, *accumulates* favourable variations, but in no sense *causes* them. This is the very foundation of Mr. Darwin's theory; yet even this is misunderstood or misrepresented by Mr. Haughton.

The third "unwarrantable assumption" charged upon Mr. Darwin is, "*That succession implies causation,*" "that the Palæozoic Cephalopoda produced the Red-Sandstone fishes," "that these in turn gave birth to the Liassic reptiles," &c. &c. Now those who have read the 'Origin of Species' know that such absurd doctrines as these are nowhere taught there; and I can only say to those who have not read it that I challenge them or Mr. Haughton to produce any passages which will bear such a meaning.

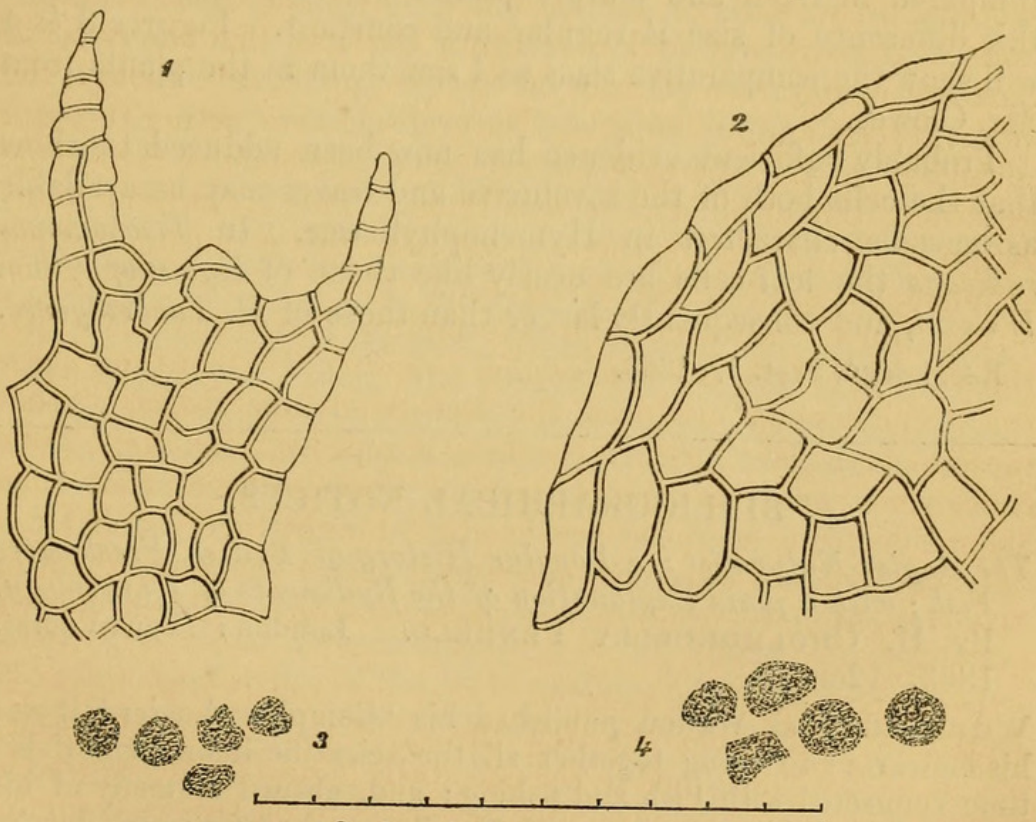
In conclusion, it is asserted "that naturalists who have accepted by multitudes the new theory of the origin of species are, as a class, untrained in the use of the logical faculties, which, however, they may be charitably supposed to possess in common with other men." This is the judgment of the Rev. S. Haughton on such men as Lyell, Hooker, Lubbock, Huxley, and Asa Gray. A perusal of his paper, with the remarks I have now made upon it, will enable any one to judge how far Mr. Haughton himself possesses those "logical faculties" which he is half inclined to deny to the mass of British naturalists. There are several other minor points in his paper which might be alluded to; but it has already occupied as much space as it deserves, and I will only,

in conclusion, quote from it a short paragraph which contains an important truth, but which may very fairly be applied in other quarters than those for which the author intended it:—"No progress in natural science is possible as long as men will take their rude guesses at truth for facts, and substitute the fancies of their imagination for the sober rules of reasoning."

XXXI.—*On the Tissue-cells of the Involucres of* Hymenophyllum.

By George Gulliver, F.R.S.

Since the publication, in the August Number of the 'Annals,' of my comparison of the leaf-cells of the British species of *Hymenophyllum*, which was done from poor specimens of these plants, Mr. F. Clowes has kindly given me some better-grown leaves of them; and, as he mentioned, I find that they will freshen in water like mosses. Accordingly, after these dried ferns had been put for an hour or two therein, the cells were



Scale, $\frac{1}{500}$ths of an inch.

Fig. 1. Tissue-cells of involucre of *Hymenophyllum Tunbridgense.*
Fig. 2. Ditto of *H. Wilsoni.*
Fig. 3. Spores of *Hymenophyllum Tunbridgense.*
Fig. 4. Ditto of *H. Wilsoni.*

found as perfect as in the growing plants; and many examinations confirmed the accuracy of the fact before stated, that the leaf-cells of *H. Wilsoni* are more elongated and larger than those