125

JOURNAL

AND

PROCEEDINGS

OF THE

ROYAL SOCIETY

OF

NEW SOUTH WALES,

1882.

INCORPORATED 1881.

VOL. XVI.

EDITED BY

A. LIVERSIDGE, F.R.S.,

Professor of Chemistry and Mineralogy in the University of Sydney.

AGENTS FOR THE SOCIETY:

Messrs. Trübner & Co., 57, Ludgate Hill, London, E.C.

SYDNEY: THOMAS RICHARDS, GOVERNMENT PRINTER.

1883.

The Hawkesbury Sandstone.

By the Rev. J. E. Tenison-Woods, F.G.S., F.L.S., &c., &c.

[*Read before the Royal Society of N.S.W.*, 10 *May*, 1882.]

The Hawkesbury sandstone is that peculiar formation which constitutes much of the Blue Mountains west of Sydney, and which is also so conspicuous in Sydney Harbour, at the heads and on the banks of the Hawkesbury River. Its name was given by the late Rev. W. B. Clarke, who thus describes it:—

"*Hawkesbury Rocks.*—Over the uppermost workable coal measures, which are of considerable thickness, is deposited a series of beds of sandstone, shale, and conglomerate, oftentimes concretionary in structure and very thick bedded, varying in composition, with occasional false bedding deeply excavated, and so forming deep ravines with lofty escarpments, to the upper part of which series I have given the name of Hawkesbury rocks, owing to their great development along the course of the river basin of that name. These beds are not less than from 800 to 1,000 feet in thickness, containing patches of shale, occasionally with fishes, with fragments of fronds and stems of ferns, a few pebbles of porphyry, granite, mica, and other quartziferous slates, and assume in surface outline the appearance of granite, from the materials of which, and associated old deposits, they must in part have been derived. On the summit of the Blue Mountains and along the Grose River the thickness of the series is very much greater than near the sea. Patches of very small area contain bits of coal, carbonate of iron, and sometimes represent miniature coal measures. Towards the base bands of purple shales are frequent, and ferriferous veins with specular iron, hematite, ilmenite, graphite, and other minerals sometimes occur. In places, as about the 'Yellow Rock' near the upper Wollombi River, in Ben Bullen, and above the deep excavation of the Capertee amphitheatre, salt and alum are found in cavities formed by decomposition; and in other places, as at Bundanoon Creek in the Shoalhaven District, at Appin, and on the Bulli escarpment of the Illawarra, and at Pittwater north of Sydney, stalactites have been formed under similar circumstances. There is an enormous mass of brown iron ore, highly carbonized, partly worked, at Fitzroy, near Nattai; another on Brisbane Water, and a smaller patch on the coast, a few miles north of Sydney, and other similar patches in intermediate localities. These are in part associated with specular iron, which occasionally lines the joints of the sandstones close at hand with well formed crystals. The uppermost beds of this formation, especially where they become conglomerates, exhibit isolated summits, imitating ruined castles, and have thus been traced by me at intervals all along the escarpments to the westward of Sydney, from the latitude of the Clyde River to that of the Talbragar, and in certain localities within the longitude of that line and the coast. In the deep ravines of the Grose and Dargan's Creek, the one eastward, and the other westward of the Darling Causeway traversed by the Western Railway line, the slopes are studded with fantastic pillars sculptured by denudation and decay into imitative architectural forms. Similar forms cap the extension of the coast range to the head of the Goulburn River. The tints are poikilitic, darkening from exposure and exhibiting imitations of landscapes, sometimes of striking charater. The semi-crystalline fragments of quartz and the disposal of colours (suggesting the idea of the action of gases removing the ferruginous tint in places) have caused me to believe that some transmuting agency has affected large areas of the Hawkesbury rocks. The glistening of the crystalline quartz particles reminds one of the same character observable in the millstone grit in England. It is impossible to understand how considerable masses of the sandstones could have received such a present structure without the metamorphism suggested, for the crystalline faces are quite unabraded and belong to particles that have been collected originally by water holding silica in solution. By washing in acids the colouring matter of the particles may be entirely removed, and then it is seen that they are imperfect crystals. But the cementing matter is not always ferruginous, a felspathic cement holds them together with used mica, evidently derivative, and sometimes with granite. Another variation in character of the Hawkesbury rocks is their cohesion. In 1850 I was Chairman of the Artesian Well Board, and remember the difficulty we had in procuring tools hard enough to pierce the quartzose sandstone at the gaol in Sydney. The boring after a small depth was abandoned, one of the workmen precipitating the conclusion by blocking the bore-hole. But in parts of the railway lines there have been instances, as stated to me by the Engineer-in-Chief, when the largest blocks have been shivered to atoms by a not very heavy fall over an embankment."—*Sedimen. Form, N.S.W.*, 4th edit., pp. 70 *et seq.*

Extent.—The extent of this formation about Sydney is considerable. According to the geological map of New South Wales compiled by Mr. Wilkinson from Mr. Clarke's notes, it forms an oblong mass about 140 miles long, with a width of from 40 to 80 miles. With the exception of a north-western spur, which is a narrow range extending 50 miles from the mass, the whole area seems to be between two degrees of latitude. A line drawn westward from Newcastle on the north, and another westward of

Shoalhaven on the south, includes nearly the whole of its north and south extension. A similar line north and south between Sofala and Goulburn would be outside all its western boundaries, with one small exception near Sofala. These boundaries and the actual extent and thickness of the formation, can only be considered as approximate until an actual survey can be made. The geological map of the Rev. W. B. Clarke is only a sketch, in which in the course of time great modifications will have to be made. Thus, for instance, the same formation is found at Dubbo and along many places on the western plains, often of large extent, which are not indicated on the map. The Dubbo sandstone quarries have many plant remains, in which large and beautiful specimens of *Thinnfeldia odontopteroides* appear not to be scarce. There are also faint impressions of a leaf like *Glossopteris* with mid-rib and oblique venation too faint for determination.

Geological position.—In the whole of the area thus described the formation lies horizontally upon rocks of different age. Sometimes it is upon the coal measures, or again upon Devonian or Silurian rocks. It is overlaid in many places by the Wianamatta beds, and by basaltic, or at any rate volcanic products. But in no place is there any sign of upheaval. There are, however, at the first Zigzag very many signs of a downcast or fault. There the beds are for a very short distance highly inclined against the range, having the appearance of an immense landslip from the failure or subsidence of the ground. The rock which is inclined appears to be bent down from the main mass, which is quite horizontal.

Looking at the appearance which the area of this formation presents upon the map, one is struck by the great difference there is between its outline and that of any other. The granite lies in generally meridional lines. The Silurian and Devonian forms nearly the whole substratum of the eastern portion of the continent. The volcanic is abrupt and irregular, just as we should infer from its paroxysmal origin. The shape of the Hawkesbury sandstone formation may give a clue to its peculiar origin, and I shall refer to it again.

Stratification.—There is only one point in the description of the Rev. W. B. Clarke which requires amendment, and that is where he speaks of the false bedding as being only occasional. On the contrary, false bedding is almost universal—it is the characteristic of the formation in nearly every portion. There are two distinct forms of stratification. One which makes the main lines of subdivision. These are undulating lines which seem to divide the stone into massive layers of ever-varying thickness. These divisions are either mere partings or full of a very irregularly or finely stratified mass which looks very fissile, and composed of fine grains like dust. The layers are also separated occasionally by red bands of ironstone, of which more subsequently.

Between these then are finer lines of stratification which are mostly inclined to the horizon. They are irregular in thickness and in dip: sometimes composed of coarse sand and sometimes of the very finest dust. They are occasionally interrupted by belts of small pebbles, always abraded and often rounded, but through the whole mass of the sandstone of the Blue Mountains the stone is exceedingly fine-grained, and there are rarely any pebbles larger than a pea except in the very lowest conglomerates. None of these finer strata extend from one great subdivision into another. The irregularity of the dip of the false bedding is surprising. In a few feet the dip will vary in almost every direction and angle, though rarely at a greater one than 25 degrees. I have noticed also that often when the angle is small the sand is rather coarse. Sometimes a series of strata with a high angle will be cut off above by another series dipping on an exactly opposite direction. This gives rise to a herring-bone appearance which is often repeated three or four times in the same greater layer. In order to prevent confusion by the frequent use of the word strata, I shall call the greater divisions of the horizontal sandstone "layers," and the smaller stratification "laminæ." Between the layers there is often a shale or slate deposit, the laminæ of which are thin, horizontal, and rather difficult to trace.

The ferruginous bands do not always follow the lines of the layers or the laminæ—they frequently descend irregularly through the stone in undulating lines or circles, in fact, in almost every shape. They are in this case no more than stains, seldom forming stripes or bands of ironstone. By ironstone, I mean a compact hard rock of brown colour, probably the limonite of mineralogists, and containing silicate and hydrated ferric oxide; of the chemical peculiarities of these stains and bands I shall treat subsequently. When examined by the aid of the microscope the ironstone is seen to be a mass of grains of sand cemented together by a limonite paste, so that the actual amount of ferric oxide is small. But there are also broad bands of red stone in which no grains of sand can be detected. There is one exposed in the upper cuttings of the second Zigzag. It is about 3 feet thick in places, but very irregular and undulating, with sandstone partings like a bed of shale, which I make no doubt it has been. The destruction of the vegetable remains has been effected in oxydizing the ferrous salts, as I shall explain hereafter.

Fossils.—In the extract from Mr. Clarke's paper reference has been made to the plant impressions in this formation. I think they are mostly confined to the stems and roots of plants. *Sphenopteris alata*, Brogn., has been identified, but the portion of the formation whence derived is not stated. *Thinnfeldia odontopteroides* has been found near Mount Victoria. There are also small lenticular masses of coal, or rather jet, in some portions of which a coniferous structure could be made out—at least in some specimens from Double Bay which I examined. In the same sandstone fossil fishes have been found, ranging from 16 feet below the sea (at Biloela) to over 3,400 feet on the Blue Mountains. Two species have been determined, *Cleithrolepis granulatus*, Egert., and *Myriolepis Clarkei*, Egert. The former is not heterocercal; the latter is not fully determined.

At the base of the Hawkesbury sandstone there are thick beds of ironstone, often intermingled with what seems like a green silicate of iron. The whole formation lies conformably upon the coal measures.* The passage from one to another is almost imperceptible—it is difficult to draw any well-defined line. The false bedding disappears as well as the layers, and soon a very hard and coarse conglomerate succeeds, with beds of shale and impure coal.

Escarpments.—A peculiarity of the Hawkesbury sandstone is its precipitous character. Wherever it is met it is always cut into abrupt escarpments and presents precipitous faces; even where outliers are seen on the summits of hills they have the same character, and, when the formation abuts on the coast, cliffs and gorges such as are seen in Port Jackson are present. Mount Pigeon House, so named by Cook from the resemblance of the summit of this mountain to a dove-cote, is a case in point. The cliffs at Jervis Bay, belonging to the same formation, are much like those at Sydney Heads. Rocks very like the Hawkesbury sandstone are found in various places on the eastern side of Australia and in the interior. They are largely developed in the neighbourhood of the Endeavour River, and northwards in Northern Queensland. They also lie on a coal formation, but not conformably; but they are quite similar in character. They have undulating layers and laminæ of false bedding. They have also beds of ironstone, and are cut into precipitous faces, as in Sydney. A mountain called O'Connor's Nob is very similar to Mount Pigeon House; Mount Mulligan is a detached mountain of inclined paleozoic rocks, perfectly crowned with precipices of horizontal layers exactly like the Hawkesbury sandstone.

Main Range, Queensland.—Along the main range, between Brisbane and Toowoomba, numerous precipitous cliffs precisely like the Hawkesbury rocks are to be found. In some places they seem newer than the rocks of New South Wales, but in others there is no difference in appearance. Many of the cuttings and tunnels are just like those of the Blue Mountains. At the Little Liverpool Range, and again at Murphy's Creek, the formation is the same, and, though less picturesque than the Blue Mountains, is evidently of the same character. The undulating layers are on the whole horizontal. The highest parts of the main range west of Brisbane

* Except in the Blue Mountains the sandstone is not constantly conformable to the coal measures.

are built up by the overflow of lava on the summits of this sandstone, and there has been no upheaval. The beds have occasional fragments of wood converted into jet, in which the coniferous character can plainly be traced.

To the west of the main range in Queensland a similar formation is frequently seen—this is Mr. Daintree's desert sandstone. After an attentive examination of its structure, I cannot say that I could perceive any marked difference in external aspect. It looks perhaps a little less altered, but I had no opportunity of comparing both microscopically. The desert sandstone was never seen by me in so compact a state that it would bear grinding down into thin films for the microscope. But this is rarely the case with the Hawkesbury sandstone. Daintree's desert sandstones rest directly upon the cretaceous rocks, and they are therefore probably tertiary. It is a formation which is very extensively spread. It has layers and laminæ of exactly the same character as the Hawkesbury rock, with bands of ironstone, fossil wood, plant impressions, and conglomerates of small pebbles. The two formations probably differ widely in age, but no one can doubt that they have been accumulated under precisely similar conditions. The outliers and patches of desert sandstone are scattered all over the western interior, and wherever found the formation is horizontal, and the rock always presents precipitous and unequally weathered escarpments.

Composition of the Sandstone.—Before entering into any speculations as to the origin of this formation, I may mention that I never could succeed in getting a film of any of these rocks thin enough for the microscope. The rock is too loose and friable. But in trying experiments I arrived at what I think is a good idea of the structure. In some fragments of stone from Manly Beach (the outside beach rocks), and some others from Mount Victoria, I found that the stone consisted of very small rounded grains of sand cemented together by a siliceous infiltration, in which the facets of small crystals of quartz were traceable. The sand was not all siliceous. There were fine particles of opaque white, yellow, or brown felspar, and many scales of mica. When powdered, the round grains generally remained intact, while the glassy, transparent, and jagged fragments I took to be the hydrated silica or hyalite which formed the cement. Under the polariscope it always showed beautiful colours. Most of the quartz grains were dimmed on the surface, like ground glass. I infer from this partial examination of the stone, that the sandstone in the localities from whence my specimens were procured is derived from the decomposition of a granitoid rock. Water has subsequently dissolved a portion of the ingredients, and the hydrated silica has acted as a cement between the particles.

In trying to account for this widespread sandstone, however different in age the deposits may be, we may at least conclude that they all arose under the same conditions and have the same causes. Mr. Clarke has already pointed out that they are not marine; they contain too many plant remains for that to be the case. The same illustrious geologist has, like Mr. Daintree, suggested that they might be fresh-water, the remains of some immense fresh-water lake. This I think is also quite untenable. Lacustrine formations are not at all of this character. The beds are horizontal, marly or calcareous, and false bedding is rare; besides, we should expect to meet fresh-water shells, which we do not. This is the case with the Wianamatta beds, which may well have been fresh-water. Mr. Daintree has suggested that the whole interior of Australia may have been, in tertiary times, a vast fresh-water lake. But the formation is found on both sides of the dividing range. Besides, we must repeat again that the formation is not like a lacustrine one. Mr. Chas. Darwin, in a passage in the "Naturalist's Voyage," which I shall give subsequently, says that the rocks were formed in a sea with irregular floor, and were drifted into high banks round submarine rocks. But again we answer that no marine organism has ever been found in the stone, while plants have been. Messrs. Feistmantel and C. S. Wilkinson have suggested ice action in shallow seas to account for the boulders and conglomerates. But several other characters of ice action are wanting, and in face of the marine difficulty, if another explanation of the boulders can be found, the ice theory will find little support, especially as the formations extend nearly to the equator and India.

Absence of upheaval.—One fact seems to be lost sight of in all these theories, and that is that there has been no upheaval. The beds are horizontal in nearly every case, and there has been very little alteration of level since they were deposited. This is true wherever the formation is found: it is a most significant fact connected with our eastern mountain range. The highest portions are recent volcanic, granitic, or horizontal sandstones, which have not been upheaved from the sea. There has been evident depression about such places as Sydney Harbour, but no elevation anywhere.

Wide Bay sand dunes.—In looking for examples of this formation in the Colony of Queensland I have seen one or two phenomena which may furnish a clue to the history of these rocks. At a place called Double Island Point, about 100 miles north of Cape Moreton, there is a formation of sand which forms cliffs for some 3 or 4 miles on the south side of Wide Bay. The southern boundary of the bay is formed by two somewhat conical hills, separated by a long interval of low land from a low range of volcanic rock forming Double Island Point. This is covered with green vegetation and light timber. From the west end of the point the sand cliffs ascend. They are densely covered with a light brush (*Melaleuca genistifolia?*). The cliffs of sand are quite precipitous on the seaward side, and are from 100 to 200 feet high. On a close examination the cliffs present exactly the appearance of the Hawkesbury sandstone except in colour, and they are not consolidated. There are the same undulating "layers" of varying thickness, forming thick sinuous marks upon the cliffs, which can be seen at a great distance. The layers are entirely constructed of laminæ of sand with false bedding, which dips at every angle not outside 30°. The layers are of different colour, and they seem to preserve this colour throughout, giving the cliffs a curious ribbon-like structure. Some are white, others yellow, and some ochreous red. The formation is entirely one of blown sand. On the surface, where tea-tree brush does not grow, the sand forms the usual shifting dunes of rounded outline and great height. In places there are sand-slips on some of the dunes where the false bedding becomes revealed. The undulating lines which separate the various layers are found to consist of decaying vegetable matter, or rich loamy earth with roots, leaves, and landshells intermingled. They represent the former surface of the drifting sand, where its shifting has been stayed by the growth of a dense brush. Thus it has remained stationary for years, until a change of wind or perhaps a bush fire has brought the sand on to the surface again and overwhelmed it. In part of the brush there are swamps of water, at least so I was informed, but I had not time to examine them.

Burdekin River.—Before I point out the application of such formations to explain the Hawkesbury sandstone, it is better perhaps to give one or two more illustrations. In crossing the Burdekin River in 1879, at a place close beside the present railway, I noticed a hill of loose drift-sand, not far from the bed of the river. It would not have attracted my attention had it not been for the large quantities of loose sandy soil that I found on many parts of the banks of the Burdekin. There was no doubt that this hill was composed of blown sand and dust from the river, which is of a very sandy bottom, and so I supposed that all the other sand was derived in the same manner. My little sandhill remained unexamined then, but two years afterwards I came by the same spot and found that it had been largely cut away for the purposes of ballast, exposing a beautiful section. The appearances were precisely those of the Hawkesbury sandstone, but on a smaller scale. There were the layers and the laminæ, with false bedding. From the way in which the hill had been cut down I was able to see more of the structure than in the hills at Double Island Point. The laminæ not only dipped at every angle from horizontal to about 23°, but also in every direction according to the wind. Another very interesting and important fact met me here. There were large flat or rounded

pebbles in horizontal lines in this hill, generally near the layers. When I say large, I mean large considering the wind origin of the hill. None were over 2 inches square or thicker than ½ an inch, at least none that I saw, but they were numerous. The edges were rounded but this may have been because they came from the river-bed. Again, the laminæ were of various degrees of coarseness, some being very fine and others quite a coarse sand, just as if it had been sifted. I was able to account for this by what I witnessed. The wind was blowing in gusts from the east, as it usually does in the October mornings about 11 or 12 in the day. I noticed that where a strong gust came it carried all before it, even removing some of the small stones; but when the breeze was gentle only the fine sand would be removed, leaving a layer of coarser particles. So that in reality the coarse sand represented the sifting effect of light breezes rather than the heavier winds. I noticed here how the wind formed the laminæ. Strong breezes caused a steep dip in the deposition, and the length of the laminæ depended upon them, so that these laminæ rise and fall according to the velocity of the breeze somewhat in the manner of a wind-sail used for pumps. Other facts brought to light by this small eolian sand-hill I shall refer to by-and-by. I fully ascertained before leaving the spot that the hill had been formed by the wind, and not by the overflow of the river. In fact it was a moving sandhill, and had shifted its position considerably in the interval of short visits, during which there had been no flood in the river. Beds of river-borne sand have quite a different structure. It must be remarked that the bed of the Burdekin hereabouts is a flat channel about half a mile wide. In this there are two or three narrow and deep streams of water 40 or 50 yards wide. The rest of the bed is dry for the greater part of the year, and covered with rocks and loose drifting sand. A great many low eolian sand-hills consequently accumulate on its banks.

Pliocene aerial sands on the S. Coast.—Another instance can be seen on the south coast of Australia. It has been described in the "Geological Observations in South Australia," from localities where it is well exposed, that is at Cape Grant, near Portland, in Victoria, and at Guichen Bay, in South Australia. But it also appears in patches all along the south coast, from Port Phillip to the mouth of the river Murray, and always in connection with sand dunes. It was thus described in the work referred to. Round the coast a rock of dark brown colour is found to occur in patches of a rough and compact character; at times it forms sea cliffs of considerable height, and there it is seen to the best advantage. At a distance one would imagine the rock to be divided into large strata 14 or 16 feet thick; on a closer inspection another kind of stratification is discernible. In addition to the great divisions, which are so distinct that one would almost suppose that they were huge slabs of rock laid upon one another, the strata themselves are entirely made up of false bedding. This is a lamination which divides the beds into strata about 2 inches thick or even less. The laminæ are never horizontal, and never continuous across the great layers. The material of the rock appears like a sandstone, but under the microscope is seen to consist of fragments of shells and shore debris, with grains of fine siliceous sand and sponge spiculæ intermingled. There are no fossils, or at least they are rare. Professor R. Tate, who first asserted the formation to be eolian, found small shells in portions, and these were land shells—not marine, and of the kind now existing on the coast. When I first saw this deposit I imagined it to have been derived from marine currents; but a better knowledge of the floor of the ocean shows us that marine currents do not leave such stratification. Besides the land shells, and the fact that the strata show no signs of upheaval, I found in subsequent years, by various sections on the coast, that this deposit is only an indurated portion of the sand dunes with which it is always associated. It is an aerial rock, and is stratified by the wind alone. The only difference between this rock and the Hawkesbury sandstone is, that it contains a large quantity of lime, with brown coal occasionally.

Bermuda sandstones.—In the islands of Bermuda a similar formation is met with. Although generally very low, some parts of these islands rise to 250 feet above the sea-level, consisting of various kinds of limestone rock, sometimes soft and friable, but very often hard and even crystalline. It consists of beds which sometimes dip as much as 30°, and exhibit great contortions besides, with much false bedding. It has been put beyond a doubt, by a long continued series of observations, that the rocks are all due to the wind, which blows up the sand from the beach, and which itself is derived from coral and shells. The rain dissolves portions of the lime and consolidates it. In this limestone at Bermuda, as well as in the calcareous rocks of the south coast of Australia, we have those singular stalagmitic concretions which look like roots. In Bermuda the aerial rock contains a red earth some 2 feet thick under coral rock, and resting on a bed of calcareous sandstone, probably due to the decomposition of some minute organism.*

Aerial origin of Hawkesbury rocks.—We see from these illustrations what are the characters of aerial or wind-blown rocks. They are destitute of fossils, except land-shells or plant remains. They are not upheaved. They are most of all distinguished by

* Jones' *Guide to Bermuda.* More likely the red earth is due to the decomposition of vegetation which grew when this lower part was formerly the surface.

large irregular undulating layers, which are also subdivided by laminæ with every kind of dip and direction, rarely exceeding 23°. Now I am prepared to maintain that this structure only belongs to eolian rocks, and is never found in any other. The non-upheaval of the beds I take to be conclusive, without any other argument, yet still there are other facts quite as significant. We know of no marine formation which does not leave some marine fossils; that is to say, no matter how sandy the coast, or what the nature of the rock, marine remains are always found in the deposits. In all the records of deep-sea soundings, such a thing as an *azoic* formation has never been found. Even the very deepest sea had foraminifera, and it need scarcely be repeated that here there can be no question of a deep sea (of 2,000 or 3,000 fathoms for instance), as there has been no upheaval. The same objections lie against a river or estuarine deposit; the bedding is of a different character. The rocks in such cases are clayey and sandy, with frequent changes of mineral character, and last of all show alternations of marine, brackish, and fresh-water remains, with deposits of fine alluvial mud. And I might here point out the difficulties of an estuarine formation to account for a deposit which is found all over Australia. Where did the estuaries or rivers lead to or come from under such circumstances? Look again at the area of the formation upon the Blue Mountains. What kind of an estuary or river deposit would that be? Why, the Amazon or La Plata sink into insignificance beside it. Finally, take the known lacustrine formation, such as those of Auvergne, or what is better still, examine the sections exposed of any of the fresh-water lakes and swamps of the interior. This I have done in both places. I remember well the appearances presented by the lacustrine formations of Auvergne, and I have noted every section of the kind that I have seen in Australia. The appearances are totally different; impure limestone, with fresh-water fossils, beds of mud, all in fine horizontal strata. If by any chance a stream fed the lake, there is cross stratification, but quite of a different character. It is uniform in direction, and confined to a narrow area; it is the exception, not the rule.

Character of eolian sand.—But we may go further than this. The grains of sand themselves will give us some information as to their origin. The experiments of Daubrée and Phillips have shown, I think, very conclusively that water-borne sand breaks up after a certain distance into a certain fineness, and after this it does not break any more, while the small fragments always retain their angular character. Mons. Daubrée enclosed angular fragments of granite in a steel cylinder with water, and caused the whole to rotate at the rate of a progress of about 60 yards a minute. After the fragments had traversed a distance equal to about 20 miles the result was a formation in the tube of gravel, loam, and sand. The latter was never in larger grains than a quarter of a millimètre in diameter, but always in angular fragments. The felspar had disappeared, the sand was consequently entirely quartzose with a few scales of mica.

Messrs. Sorby and Phillips have both made sand particles the subject of special study. The latter has found that wind-blown sands have the grains nearly all rounded, especially if they have been exposed to the action of the wind for any time. The sands of the Egyptian deserts are all rounded. On the other hand, Mr. Phillips has found that fine sands taken from the beds of streams are always angular, and this even where there is good proof that they have been borne great distances by the water. The explanation of this fact seems easy to find. In the air there is nothing to prevent the friction of the particles on one another, and in water there is scarcely any impact or friction at all. At the end of this paper will be found all the observations which I have been able to make on this subject. As a rule I can confirm the conclusions of Mr. Phillips. I have microscopically examined all sands from all the rivers and creeks I have come across. The smaller particles are never entirely rounded unless the fragments are derived from a sandstone which was itself composed of rounded particles. On the other hand, some wind-blown sand, especially that composing sand dunes, is altogether abraded. This is well seen in the sand which forms the dunes at Moore Park, Waverley, and Bondi. Some yellow sand from the inner beach at Manly, which is no doubt derived from the sandstone cliffs near, is nearly all composed of abraded particles, but there are angular siliceous particles occasionally which I shall subsequently explain.

It will be remarked that I have said some wind-blown sands are abraded, because the grains composing the hillock already referred to on the Burdekin River were not at all abraded. The particles with very few exceptions were quite angular. They had been brought from the river-bed at no great distance, and had not been much blown about. I think it is only in the case of wind-blown sand, long exposed in loose drifting masses, that we can expect to find all the particles abraded. Then again, rocks composed of fine aerial dust, as some of the Hawkesbury sandstones have been, will show little of their origin except in their stratification.

Nature of the sand.—Now, in applying these principles to the Hawkesbury sandstone we find we are stopped by a difficulty which the age of the sandstones would lead us to expect. The rock has been completely altered by internal metamorphism. Reference is made by Mr. Clarke to this, and to the crystalline facets on portions of the rock. Any one passing across the Blue Mountains cannot fail to have noticed the sparkling of the sandstone rocks in the cuttings. A close examination will show that this is due to

minute quartz crystals whose facets stud the surface of the stone. The ferruginous stains on the rock and the bands of ironstone will also show that another kind of metamorphism has been going on. Water percolating through the stone has affected the felspar grains which largely entered into the composition of these sands, the iron they contained has been converted into the reddish-brown peroxide which forms the ferruginous bands and stains. Much of the excess of silica has crystallized in minute crystals, or formed a siliceous cement around other grains and made the rock harder and more compact. Thus, in some portions of the stone which I examined, partly rounded grains could be seen with minute crystals upon them, while other fragments could be seen to consist of two or three grains cemented together by a siliceous cement. I was never able to obtain a portion of this sandstone sufficiently hard to bear grinding down into a section for the microscope. On the other hand, the rock, even in the softest or most friable portions, can never be broken up so as to separate the constituent grains. But after having examined a very large number of specimens of stone from this formation taken from widely separated localities, I am of opinion that it has originally been formed of abraded grains of sand, or of fine dust, such as we might expect from an aerial deposit. In some cases the sands derived from the weathering of the rock bear this out, as, for instance, the marine sands about the Heads, which have clearly been derived from the Hawkesbury rocks. Let it be remarked that abrasion in this case cannot be from marine action, as the sands collected by me in other places, and derived from such rocks as granite, were not at all abraded, though they had evidently been long exposed to the action of the surf. Sands derived from the weathering of the same formation in other places, such as Mount Victoria, Lapstone Hill, and the second Zigzag, were not all abraded; but the grains were coarse, with large crystals of quartz in the midst; in fact some grains were more or less altered and partly crystalline, but there were evidences of the original constituents in many rounded grains. When seen under polarized light some of the larger fragments would manifest their compound character, and by watching the effect of the light as the Nichol prisms were revolved, the forms of the rounded grains embedded in transparent silica could be made out. I also imagined that in the rich play of colours under polarized light I could distinguish the fragments of hydrated or opalized silica which cemented the grains, but I write with hesitation on this point, as I had no certainty that the specimens thus distinguished were always hyalite; at least I had no other test than that they showed the same rich and varied colouring which was manifested by hyalite. The grains imbedded in ironstone are well preserved, and admit of being seen in thin films by the microscope. Where they have not been decomposed they are all rounded and abraded. By abrasion I mean that sort of opaque surface which is seen in ground glass. Even grains of transparent quartz are thus affected. With a high magnifying power the minute pits and scratches can be seen. This can only be due to aerial action. Water, as we have seen, does not produce this. In all the fine water-borne sands that I have examined the particles were angular and not abraded.

The reason why the sands derived from the Hawkesbury rocks are occasionally so little like the original constituents, is because they are the result of decomposition from a rock often composed of fine dust, which has now become compact.* When granite decomposes, the sand resulting does not consist of separate crystals of quartz, felspar, and mica, but rather the angular grains contain portions of each of these minerals. This I have seen from the microscopic examination of many specimens of granite sand which I gathered in various localities. In like manner the Hawkesbury sandstones do not decompose into their original constituents, but rather into the fragments, according as they are affected by weathering; that is of course when they have become metamorphosed into a hard compact siliceous rock. But there are very many portions of the Hawkesbury sandstone where the metamorphism is not complete and the eolian character of the grains is quite visible. This is everywhere the case in a very similar formation on the Main Range between Brisbane and the Darling Downs, and again at an intermediate range called the Little Liverpool Range, an isolated sandstone formation many hundred feet thick. I can assert nothing positively about the age of this range, except that it is older than the tertiary lavas, and younger than the coal formations upon which it rests. It has not been upheaved, but is simply horizontal layers of sandstone exactly like the Hawkesbury rocks. It was considered by Daintree as desert sandstone. The sand is coarse in places, and consists of light brown opaque particles, often loosely cemented together with opaque siliceous cement. The particles are nearly all abraded, and some quite rounded. The Main Range is composed of a similar rock, five or six hundred feet in thickness. It is quite horizontal and abrupt, but is made much more so by the outpouring of lava which has covered it in tertiary times.

Recent contemporary observations.—Mr. J. A. Phillips has shown in his paper on the History of Grits and Sandstones† how a large number of the carboniferous Permian and Triassic sandstones are composed almost entirely of quartz crystals which have

* In the examination of sands from the Blue Mountains care must be taken to scrape the material from the rock itself. Sands found in the gullies and water tables by the railway are derived for the most part from the surface and broken up by the transit into angular particles.

† Quart. Journ. Geol. Soc., vol. 37, p. 6 *et seq.*

been produced *in situ*. Numerous fine-grained sandstones, particularly among those of Triassic age, are composed of quartz grains so completely rounded as under the microscope to resemble water-worn pebbles. These grains are variously coloured red or brown by variously hydrated oxides of iron; in some cases, minute perfectly formed and beautifully transparent crystals of quartz have been developed on their surfaces. He further adds that, on examining a considerable number of modern sands, none of them except such as had been long subjected to the wearing effects of wind action, were found to resemble those of the "millet-seed" sandstones. Those which resembled them most were blown desert sands. In the discussion which ensued on this paper, Mr. Blandford said that some years ago he had examined the Indian desert, and found the grains of sand well rounded. They were mostly of quartz, with a few felspar grains, and occasionally of hornblende. The strongest wind there blows from the west. The sands had come from the coast and the river Indus. He further stated that the sands appeared to be unstratified, and this I can confirm in the appearance of all desert sands, but when a section is made the peculiar false bedding is immediately seen. Mr. Rutley on the same occasion called attention to the presence of felspar on many of the sandstones described, and suggested that it was quite possible for such sandstones to be changed into felstone. There was often much difficulty in distinguishing between the finer grained igneous and sedimentary rocks.

Summary.—To sum up these facts: I may state that observation has proved that wind-blown action seems alone competent to round grains of sand; angular fragments of quartz having a diameter of less than $\frac{1}{10}$ of an inch remain unrounded by the long continued action of currents, or by the continuous action of breakers after many years; yet the rounded character of the fragments of a wind-blown sandstone is often difficult of detection in a compact rock which has undergone internal metamorphism.

We now come to the inquiry as to the causes of those peculiar appearances in the eolian sandstones, such as the false bedding, the layers, and the ironstone bands and concretions.

False bedding.—It has been already noticed that the angle formed by the laminations never exceeds a certain value. This is due to the fact that rounded, or indeed small particles of sand of any kind, when perfectly dry, have a definite angle of repose. This angle is about 30°. With wet sand it is entirely different. It may lie at any angle as long as it is not completely saturated with moisture, so as to give the particles perfect freedom of movement one upon another, while mere dampness would increase the cohesive force and then the angle of repose would be increased. The pressure also of the grains of sand in dry masses is not direct but lateral. The angle at which the laminæ dip is therefore not so much an index of the force of the wind as of the quantity of sand conveyed by it. A slight steady breeze blowing for a day in one direction would tend to carry a good deal of sand, which as it heaped up in the places where it was deposited would slip down to the angle of repose just as we see happening in an hour-glass. But if the sand were very equally distributed by a strong wind which tended to smooth down rather than to accumulate heaps, then the angles of repose might be very low. I regard the laminæ as the result of periods of rest in the sand-drifts, and the thickness and direction as indications either of the duration or quarter of the wind.

Since my attention has been directed to this I have carefully examined every sand-heap that came under my observation, and also noted the effect of the wind upon them. I had a good opportunity for this at several of the coral islands inside the Barrier Reef. Most of them are formed of a fine-grained calcareous sandstone, partly cemented by the water and partly drifted by the wind. At Low Island I remained a week, and on my arrival noted the height and dimensions of a small heap of sand which was forming under the shelter of some drift-wood. By planting sticks at various places in the heap I was able, not only to measure each day's accumulation, but also the results of a change of wind or a calm. It was at the end of the month of October, when the trade winds blow generally from the south-east, but there are occasional calms and changes of direction, and these were in the morning. The sandhill was only a few feet high and a few hundred yards in superficial area, with a steep face on the leeward side. There were two long tongues of sand on the extremities which each day's accumulation brought further and further out. I found that the greatest accumulation on any one day was about 7 inches. This was during a light constant breeze. On cutting into that day's deposit it was found to be formed of four or five thin laminæ irregularly dipping at an angle of 30°. I could not account for the division into laminæ, but I supposed that they represented lulls in the wind. Again, on another day, when the wind was very unsteady both in force and direction, I was surprised to find how much had accumulated. On making a section through the day's work the laminæ were found to be extremely thin, almost in fact like the leaves of a book. They dipped in every direction and were inclined at various angles, and they differed in the degrees of coarseness. The coarser laminæ, as I have already observed, are not so much due to the stronger breeze as to the faint ones, which carry away the lighter particles from a layer of sand, leaving the coarser grains behind. I was surprised to find in this small sandhill rather heavy shells and fragments of coral, but I soon saw that what appears but a light wind easily carries such fragments along the sand. Some of the sand on these islands has become converted into a calcareous sandstone, in which both marine and land shells are embedded. These islands abound with *Helix Fosteriana*, Pfr., which is a good-sized but very light shell. It is to be remarked that though the sand on these islands is white yet the rocks derived from them are of a deep brown colour, which is the case with all rocks derived from coral that I have seen. It is also the case with calcareous aerial rocks generally.

Lamination.—It occurred to me that the cause of lamination might be explained by experiment. I had noticed, in watching the accumulation of heaps of sand in an hour-glass or in a common egg-boiler, that the sand formed a narrow pyramid on which the lighter particles gathered for a time into a little pinnacle of sand and then suddenly slipped down; thus the grains became distributed by a series of sandslips. Perhaps then a record of these slips could be preserved by using different-coloured sand. For this purpose I stained a quantity of fine sand with two or three different dyes. Using a very fine pipette glass fixed to a stand, I let the sand fall through on to a board. As soon as a sand-slip occurred I changed the colour. When a considerable heap had accumulated, I damped the centre and made a careful section with a piece of card. A beautiful series of laminations were exposed to view, the most of them having an angle of about 30° By covering the whole with red sand, and then varying the experiment so as to draw the glass gradually along and give rise to sand-slips, first in one direction and then in another, a section was produced which gave a tolerably fair illustration of a layer with false bedding at opposite angles, or as we frequently see, "herring-bone" lamination.

Wet sand, of course, may lie at a much higher angle. In those cases where estuarine deposits are found to be inclined, they have a constant angle which is often as high as 40°. Generally speaking, the layers of clay and shingle are perfectly horizontal. The mode of deposition of deltas from fluvial or estuarine remains is perfectly understood. They are composed of regular beds of rich alluvium and water-worn gravel. A section through the deposit at Lake Geneva at the mouth of the Arve shows occasionally inclined beds, but the angle is regular, and at the length of half a mile they become perfectly horizontal. There is no resemblance between the Hawkesbury sandstone lamination and that of an estuarine deposit, even if the area did not totally prevent such a supposition. In the Arve delta, of course, there are many fresh-water shells and alluvial remains of fresh-water plants and debris of all kinds. I do not think it possible to account for some of the irregular lamination in the Hawkesbury rocks except by supposing the materials to have accumulated as fine aerial dust. Water of any kind must have deposited it in a different manner.

On the other hand, at Bermuda, as already stated, the late Sir Wyville Thomson gives an account of a formation at those islands to which I have already referred. It is formed of very fine sand accumulated by the wind, and cemented by the slow infiltration of water. In a short time the whole of this will be a hardened rock, and if the observations of Sir Wyville Thomson happened to be lost, it might easily be regarded as a marine rock, were it not that the deposit is full of the trunks of cedar trees which the wind has blown down and mingled with the mass. I have very little doubt that much of the interior of Australia is composed of wind-blown sand to a considerable depth. In 1863 I was able to examine a section of a well, sunk in a sandy heath-like country about 300 feet above the level of the sea. There was 90 feet of laminated yellow, white, and red sands, resting upon fossiliferous miocene rock. The beds were in layers and laminations just like the Hawkesbury rock, except that they were quite loose, and not aggregated together. This was on the edge of the Murray Desert, where there are tracts of sand-hills 100 feet and more in height, covered with a light growth of heath-like vegetation, of which *Lepidospermum lanuginosum*, *Xanthorrea minor*, with various epacrids and sedges, form the principal plants. Every three years or so these are burnt off, and then the sand blows about quite loosely. The grains are quite rounded. Grassy level places are found in large tracts much below the level of the sand-hills, and then there is a stiff clay with swampy land. A sufficient accumulation of such deposits, hardening by lapse of time into stone, would give rise to a deposit exactly like the Hawkesbury rocks. The fine mud of the clay-pans in such country which retains the rains which fall in this desert in winter is often covered over with wind-drifted sands. This perfectly represents the curious stratified masses found between the layers in the Blue Mountains.

Ironstone.—The ironstone bands and markings must next occupy our attention. As already observed, these form a characteristic feature in the Hawkesbury sandstone. The rings of red or brown hydrated peroxide of iron and the thick bands of them in most of the formation cannot fail to arrest the attention; it is, moreover, not only a feature in the Hawkesbury rocks, but also in these laminated sandstones wherever they are found, as in the Darling Range, Little Liverpool Range, Dalrymple sandstones, desert sandstones, &c. Our inquiry here is limited to the sources from which these ores of iron are derived, and how they have come to be hydrated peroxides as we find them.

As to the sources, there can be no doubt that the sand of these rocks has not been entirely siliceous. It is often seen to be composed, even now, of fragments of mica, felspar, and fragments of hornblende and other derived minerals. Hornblende dykes are found to have penetrated many of the older rocks from which these wind-blown materials have been derived. In some hornblende rock there is sometimes as much as 14 per cent. of iron. According to Gustav

Bischof, there is no silicate in which silicate of iron does not enter. Proto-silicate of iron or green earth is found in drusy cavities of many basaltic and doleritic amygdaloids, in augite, augitic porphyry, and forming a coating upon chalcedony. Another source of iron is that much of the carboniferous rocks from which some of this sand has been derived is coloured green by proto-silicate of iron. It is important to observe that the iron in these cases is in the form of a protoxide, and either colourless, bluish, or greenish in tint. There is a powerful affinity between silica and protoxide of iron. The alkaline silicates, says Bischof,* convert carbonate of iron in water into protosilicate of iron. The green earth contains these silicates of iron and water, and gradually converts them into a persilicate. The reduction of persilicate into a protosilicate and its conversion into carbonate of iron has been proved by Gustav Bischof's experiments.† It followed from his investigations that decomposing organic substances in the presence of carbonic acid reduced hydrated peroxide of iron to protoxide, and also persilicate of iron into protosilicate and carbonate of iron. In most of the ironstone bands thin sections placed under microscope showed round cavities filled with red ferric oxide. These represented grains of some highly ferruginous mineral, entirely decomposed by water and carbonic acid.

These chemical relations will appear more significant by making use of Mr. Sterry Hunt's beautiful illustration.‡ The chemist knows that the iron as diffused in the rocks exists chiefly in combination with oxygen, with which it forms two principal compounds, the first or protoxide which is readily soluble in waters impregnated with carbonic acid and other feeble acids; and the second or peroxide, which is *insoluble* in the same liquids. I do not here speak of the magnetic oxide, which may be looked upon as a compound of the other two, neutral and indifferent to the most natural chemical agencies. The combinations of the first oxide are either colourless or bluish or greenish in tint, while the peroxide is reddish brown and is the substance known as iron-rust. Ordinary brick clays are bluish in colour, and contain combined iron in the state of *protoxide*, but when burnt in a kiln they become reddish, because this oxide absorbs from the air a further portion of oxygen, and is converted into peroxide. But there are clays which are white when burned, and are much prized for this reason. Many of these were once ferruginous clays, which have lost their iron by a process everywhere going on around us. If we dig a ditch in a moist soil which is covered with turf or with decaying vegetation, we may observe that the stagnant water which collects at the bottom soon

* Chem. Geol., vol. ii, p. 132.
† *Op. cit.*, vol. iii, p. 4.
‡ Chem. and Geol., Essays, p. 227.

becomes coated with a shining iridescent scum, which looks somewhat like oil, but is really a compound of peroxide of iron. The water as it oozes from the soil is colourless, but has an inky taste, from dissolved protoxide of iron. When exposed to the air, however, this absorbs oxygen, and the peroxide is formed, which is no longer soluble, but separates as a film on the surface of the water, and finally sinks to the bottom as a reddish ochre, or under somewhat different conditions becomes aggregated as a massive iron ore. A process identical in kind with this has been at work at the earth's surface, ever since there were decaying organic matters, dissolving the iron from the porous rocks, clays, and sands, and gathering it together in beds of iron ore or iron ochre. It is not necessary that these rocks and soils should contain the iron in the state of protoxide, since these organic products (which are themselves dissolved in water) are able to remove a portion of the oxygen from the insoluble peroxide, and convert it into the soluble protoxide of iron, being themselves in part oxydized and converted into carbonic acid in the process.

Thus we see that decomposing organic matter has the property of reducing the oxides of iron and rendering them soluble, and in this process the organic matter is consumed and converted into carbonic acid and water. In this way we may regard the beds of hydrated peroxide of iron in the Hawkesbury rocks as representing destroyed vegetable matters. Some of the carbon is however still preserved in the shales. Lenticular masses of coal and vegetable impressions are common. In some of the concretions of hydrated ferric oxides casts of the tougher fruits may still be found. Thus in a sandstone on the Burnett River I have found a cone beautifully preserved, and closely resembling some mesozoic Cycas. This will form the subject of a subsequent paper. At Dubbo the vegetable impressions are often composed of peroxide of iron.

Any one crossing the Blue Mountains must have noticed the capping of yellow soil on the sandstone. This yellow colour is due to iron, and represents the oxydization of the underlying rock materials, produced by water holding carbonic acid in solution. This carbonic acid is derived from the decaying vegetation of the surface. If we ask what becomes of the trees and grass which grow on the surface, the yellow soil gives the reply. This represents the surface vegetation of ages. We need not wonder that few or no fossil impressions remain of so abundant a vegetation. The oxide of iron consumes all.

Mixed origin of the strata.—We must not suppose that in an immense deposit like the Hawkesbury rocks one explanation will suffice for all the appearances met with. We may expect to find other besides wind-blown strata. This will be best illustrated by a description of what is at present going on in one of the eolian formations of Europe. There are few who have not heard of the

Landes (Heaths) in the south-west of France. In this extensive tract there are over a million of acres covered by shifting sand. About six million cubic yards are carried along each year by the wind. A great many of the sand dunes exceed 225 feet in height, and some are over 300, their height depending on the thickness of the current of air. Pools of water are found on all the sand dunes, and often completely hidden by a coating of sand which does not sink, thus becoming a dangerous pitfall. These pools are carried along with the shifting sands, caused by the filling up with sand on one side, and thus pushing the basin along. The formation of lakes and marshes in the French Landes is one of the most remarkable features in them. A row of ponds differing in shape and size but generally parallel with the coast, is prolonged over a space of 125 miles. Some of these were originally at the sea-level, and are now 66 feet above it. One covers an area of 15,000 acres. Much of this area has been reclaimed by planting pines. Sometimes the advance of the sand is arrested by circumstances which favoured the growth of vegetation, and at Arachon forests of gigantic pines have covered one sand tract, with oaks which were 46 feet in girth some years ago. On the other hand, there are plenty of places in the Landes where there are traces of former forests now covered with sand.

Thus we may expect to find in the Hawkesbury formation traces of lacustrine deposits, with former marshes and lagoons. In these fishes would become entombed, and the way in which they are found in this sandstone may be explained as follows:—In one of Stuart's expeditions he found in Lake Eyre, in the central desert, at a time when the waters had become very low, a number of small fish all dried and caked in salt. Now it is easy to see how in the shifting sands which form the shores of this lake, these fish might be covered by an advancing sand dune, and thus entombed as fossils. Here they formed a belt along the shore about 12 yards wide.

Shale.—There is not much difficulty in accounting for the shale in these sandstone areas. They are the remains of fresh and salt marshes or lagoons, such as are now found in the central deserts of Australia. The process may be seen in operation on the Queensland coast. Near Maryborough, or rather Pyalba, on the shores of Harvey's Bay, and again on Great Sandy Island, there is a deposit on the site of former marshes which is very like turf or dark brown coal. It is full of vegetable remains such as roots and stems, with grains of sand sufficiently numerous to give the mass a loose consistency. In spite of its carbonaceous aspect it will not burn. Except that it is a younger deposit, it is like the carbonaceous shales often found in the sandstone rocks.

Creeks.—We must also expect to find in the Hawkesbury rocks the remains of creeks and streams with their denuding effects, which of course would be very great on a loose sandy deposit. It is thus I explain all those appearances which have been attributed to ice action. Mr. Wilkinson, the Government Geologist, thus describes such appearances.* "In the sections exposed in the quarries at Fort Macquarie, Woolloomooloo, Flagstaff Hill, and other places, may be seen angular boulders of the shale, of all sizes, up to 20 feet in diameter, embedded in the sandstone in the most confused manner, some of them standing on end as regards their stratification, and others inclined at all angles. They contain the same fossil plants that are found in the beds of shale from which they have evidently been derived. These angular boulders occur nearly always immediately above the shale beds, and are mixed with very rounded pebbles of quartz. They are sometimes slightly curved, as though they had been bent whilst in a semi-plastic condition, and the shale beds occasionally terminate abruptly as though broken off. Had the boulders of soft shale been deposited in their present position by running water alone, their form would have been rounded instead of angular. It would appear that the shale beds must have been partly disturbed by some such agency as that of moving ice, the displaced fragments of shale becoming commingled with the sand and rolled pebbles carried along by the currents. Occasionally in the beds above those which contain the angular boulders occur a few rounded pebbles of shale, showing that the currents had swept along for some distance a few of the angular fragments until they had become rounded. These pebbles are usually oval in shape, and are embedded in such a manner that the longer axis of the pebble is nearly always inclined or dips towards the south-west,—thus indicating that the transported currents had chiefly come from that direction; whereas the angular boulders in the beds below are, as before mentioned, confusedly heaped together without regard to size." In another place Mr. Wilkinson says:—"From their lithological character the Hawkesbury rocks appear to have been formed in a comparatively shallow sea, which was subject to rapid and changing currents. This sea was bounded on the west by the mountains which extend in a northerly direction from the Shoalhaven River to the head of the Goulburn River. It is in the rocks near the ancient shore-line that we should more especially expect to find ice-grooved pebbles, but none have yet been discovered. Its northern margin, owing to great denudation, cannot so readily be determined, but it probably did not extend north of the Hunter River; and towards the east its extension is lost beneath the waters of the South Pacific Ocean." Mr. Wilkinson compares these rocks with the Bacchus Marsh sandstones, and cites the opinions of Messrs. Selwyn and Daintree as to those formations being formed under ice action.

* Jour. Roy. Soc. N.S.W., vol. XIII, p. 106.

The difficulties in the way of such an explanation are insurmountable, as I shall show at the end of this paper. The way I account for these boulders is that they are the results of the action of creeks or flows of water in the loose sandy hillocks. These would easily undermine the beds of shale and break them up, tossing large fragments on end, and mingling them with water-worn pebbles of the watercourse. A few days' dry wind would soon entomb these ruins in sand and turn the course of the creek. What a sudden downpour of rain and a flooded creek will do in breaking up loose beds must be familiar to any observer. Here are a few examples:—In August, 1829, a fragment of sandstone, 14 feet long, 3 feet wide, and 1 foot thick, was carried by the river Nairn, in Scotland, a distance of 200 yards On the same occasion the river Dee swept away a bridge of five arches, built of solid granite, which had stood uninjured for twenty years; the whole mass of masonry sunk into the bed of the stream and was seen no more. And the river Don, as we are assured on the authority of Mr. Farquharson, forced a mass of stone, *four* or *five hundred tons* in weight, up a steep inclined plane, leaving them in a great rectangular heap on the summit. A small rivulet called the College, in Northumberland, when swollen by a flood in August 1827, "tore away from the abutment of a mill-dam a large block of greenstone—porphyry—weighing nearly 2 tons, and transported it to the distance of a quarter of a mile."*

Glazed surfaces.—In these blocks of shale there is often a disposition to divide into small blocks of irregular form but curiously glazed surfaces. I have noticed the same in carbonaceous alluvium in other places. The creeks near Bathurst are, in the neighbourhood of the basaltic rocks, full of a dark brown shale much like what I have already described. When dry it breaks into irregular blocks with glazed surfaces. Again, the same curious appearance was noticed in a creek near Lytton on the Brisbane River, which also is close to basaltic rock. It is quite a recent formation, some of it having accumulated within the last few years. This must not be confounded with glazing from friction, as the shale is too soft to take a polish in that way.

Denudation.—There is nothing in connection with the Hawkesbury sandstone which has been matter for speculation more than the manner in which it has been denuded into such extraordinary precipices and gorges as are found in the Blue Mountains. It must be remembered that there is not the slightest evidence of upheaval or subsidence, except at the downcast already mentioned. As the beds were deposited, there they have remained. In the

* Lyell's *Principles of Geology*, 9th edit., p. 208, where similar instances are given in great number.

deepest gorges the horizontal beds on each side correspond in such a way as to make one believe they were once continuous. The difficulty is best expressed by the eminent Charles Darwin who, in the "Naturalist's Voyage," thus tells us how his visit to these mountains had puzzled him:—

Darwin's views.—"The first impression on seeing the correspondence of the horizontal strata on each side of the valleys and great amphitheatrical impressions is that they have been hollowed out by the action of water, but when one reflects on the enormous amount of stone which on this view must have been removed through mere gorges or chasms, one is led to ask whether these spaces may not have subsided. But considering the form of the irregularly branching valleys, and of the narrow promontories projecting into them from the platforms, we are compelled to abandon this notion. To attribute these hollows to the present alluvial action would be preposterous, nor does the drainage always, as I remarked near the Weatherboard, fall into the head of these valleys, but into one side of their bay-like masses. Some of the inhabitants remarked to me that they had never viewed one of those bay-like masses with headlands receding on both hands, without being struck with their resemblance to the bold sea-coast. This is certainly the case; moreover on the present coast of New South Wales, the numerous fine widely-branching harbours, which are generally connected with the sea by a narrow mouth worn through the sandstone cliffs, varying from one mile in width to a quarter of a mile, present likenesses, though on a miniature scale, to the great valleys of the interior. But then occurs the startling difficulty: Why has the sea worn out these great though circumscribed depressions on a wide platform, and left mere gorges in the openings through which the whole of the vast amount of triturated matter must have been carried away? The only light I can throw upon this enigma is by remarking that banks of the most irregular forms appear to be now forming in some seas, as in parts of the West Indies and the Red Sea, and that their sides are exceedingly steep. Such banks, I have been led to suppose, have been by sediment heaped by strong currents on an irregular sea-bottom. That in some cases the sea, instead of sowing sediment in a uniform sheet, heaps it round submarine rocks and islands, it is hardly possible to doubt after examining the charts of the West Indies; and that the waves have the power to form high precipitous cliffs, even in landlocked harbours, has been noticed in many parts of South America. To apply these ideas to the sandstone platform in New South Wales, I imagine that the strata were heaped by the action of strong currents and by the undulations of an open sea on an irregular bottom, and that the valley-like spurs thus left unfilled had their steeply sloping flanks worn into cliffs during a slow elevation of the land, the worn-down sandstone

being removed either at the time the narrow gorges were cut by the retreating sea or subsequently by alluvial action."

None of the difficulties suggested by Dr. Darwin are met by his theory, and the absence of upheaval or marine remains is fatal to it. On the other hand, the aerial origin of the rock exactly explains the facts. These immense sandhills may have been always detached from one another, or if united, could have been easily cut into the gorges previous to their consolidation. No doubt they have become precipitous to some extent by weathering and by the sweeping away of outlying masses of loose sand. It is the tendency of loose aggregations of sand to consolidate in the perpendicular direction, and this is best seen in the deserts of Africa and Arabia, where the consolidated sand has formed the most abrupt precipices and gorges. I do not think that the denudation has been very great, for most of these aerial hills were never united. It used to be the custom to refer the small horizontal caps and outliers on the tops of mountains to the remains of an enormous formation which had been denuded away. I myself thought this of O'Connor's Nob, near Cooktown, and Mount Pigeonhouse, near Jervis Bay. Such stupendous denudation on horizontal strata, without any upheaval or subsidence, baffles comprehension; but when the aerial origin of these outliers is understood the difficulty vanishes. There has been little or no denudation. The sandstone has been deposited just where it is found, and was never much larger than we see it now. But the very boldest escarpment show fragments of rock at their bases which have broken away from the undermining of looser friable portions.

Hardening of sand.—A difficulty with many will be the immense height of these sandstone cliffs, some of them being most certainly about 1,000 feet almost perpendicular. Probably the actual amount of blown sand is less than half this, and sandhills of that height are found in other places. At Cape Bogador and Cape Verde they are over 600 feet in height. Another difficulty may be the consolidation of loose drifting sand into stone. That blown sand hardens into stone is certain, for even the recently formed sand dunes of Cornwall yield a stone which is used for building purposes. The accounts of all observers confirm the same fact of the hardening of sandstone from drifting sand in Africa and Arabia. Mr. James Haswell gives an interesting account of a sandstone in course of formation in Fifeshire.* The sandstone in question, near a railway bridge at Ardross, was resting upon carboniferous strata, above which was a bed of tenacious clay containing recent shells. Above this was blown sand which was washed down by the rain over the clay, and deposited on ledges formed by the projecting beds of shale, while the siliceous particles of which

* The paper was read at the Edinburgh Geological Society, January 21, 1869.

the sand was composed were cemented together by carbonate of lime held in solution by rain-water. It was derived from the recent shells which occurred not only in the sand but in the clay. The cementing medium was also partly composed of hydrated peroxide. The result is a hard sandstone, not unlike one of much older date.

It is a remarkable fact that stone derived from the wind-blown sand hardens by exposure, probably from the greater facility thus afforded for the formation of the great cementing medium, silicate of iron. The initial cause of the consolidation would of course be the pressure, and this is why we find in these formations the cores or centres of the highest and heaviest sand-hills. Still it must be remembered that the strata of all these mountains are of a compound nature, portions of them containing shales, which proves them to have been at one part of their history lagoons or marshes. The fine aerial siliceous dust of which much of this rock is composed would also consolidate very easily by the mere dead weight and pressure of sand above. The hydrostatic pressure, which is used to consolidate graphite, would be nothing to the effect of thousands of tons of sand.

Fine red sands.—Some of the Hawkesbury sandstone is of a very fine texture, and of a peculiar salmon colour, which is plainly seen in some fresh broken masses. I was struck by the resemblance of its colour and grain to a thick deposit of sand which fell on the Mosquito Plains on October 8, 1865. The spring of that year was particularly dry, and the hot winds set in rather early. At daylight of that morning the sky had a most peculiar lurid appearance, very much like dull copper in colour. There were no clouds, but an unequal tint or turbid appearance in patches which showed a rapid movement southward. The thunder was incessant, and with a harsh metallic sound very different from the booming echoes of heavy rain clouds. The lightning used to shoot across the sky in forked streams. In the middle of the day a steady rain of fine dust began to fall, which soon covered everything with a yellow or salmon coloured crust. I gathered quantities of it, and found it to consist of very finely divided grains of rounded ferruginous sand. At that time I was interested in looking for Diatomaceæ. In referring to my notes on the subject, I do not find any reference to the presence of any angular particles, but the dust was so extremely fine that an inch objective did not suffice for its close inspection under the microscope. The wind was blowing strongly from the south, and the sand was moving in a contrary direction in an upper stratum of the atmosphere. The sand came from the edge of the desert about 500 miles to the north, as we learned from reports which reached us some ten days subsequently. There had been a tremendous northern hot wind in that locality, and the red sand had been blown away in large quantities, so as quite to expose the

roots of the porcupine grass. The deposit of red dust was fully 2 inches deep in a few places on the Mosquito Plains where the wind had drifted it along the ground. It easily hardened where it had been moistened, and would bear considerable pressure before crumbling again. I have no doubt that some of the Hawkesbury sandstone is composed of such a deposit, which probably was derived from a desert interior, where the moisture was less.

Eolian sandstone in China.—Baron Richthofen, in his large work on China, describes a formation which covers vast areas in that country. He mentions it as forming cliffs or bluffs on the Yellow River, which in some places rise to a height of 500 feet. In many places, he says, it reaches a thickness of 1,500 feet. It extends inland over all the high plains, from the alluvial flats of the Gulf of Tshili, over the Taihhang-shan Mountains up to plateaux 1,800 mètres high, and even to an elevation of 2,400 mètres (over 7,500 feet) above the sea in the Wer-tai-Shan Mountains, Northern Shansi. It stretches south of the hilly grounds beyond the valley of the Yangtze, and up that valley in a westerly direction for an unknown distance. It can be followed up the course of the Han, to the watershed of that river, and it is known to extend up the valley of the Yellow River without interruption, into the province of Kansuh. This enormous deposit, according to Richthofen, is solely the result of atmospheric waste and wind action, and he has brought forward a large body of interesting and important evidence to prove his theory. He insists also upon the fact that the organic contents of this deposit pertain exclusively to terrestrial remains, as in the Hawkesbury sandstone.

Dr. Geikie, in his *Pre-historic Europe*, from which the above is taken, adds (p. 167):—"It may be that we have hitherto underestimated the action of winds as geological agents in dry continental areas like those of Central Asia, and that aerial currents have played a much more important rôle in the past than has been generally supposed. 'No one * can realize the capacity of wind as a transporter of fine material who has not lived through one great storm on a desert. In such a simoom the atmosphere is filled with a driving mass of dust and sand which hides the country under a mantle of impenetrable darkness, and penetrates every fabric. It often destroys life by suffocation, and leaves in places a deposit several feet deep.' But such rapid accumulation occurs, I presume, only in the desiccated desert or its immediate neighbourhood. Deserts of shifting sand increase their bounds by a gradual encroachment of the dunes of the peripheral regions, continually advancing in the direction of the prevailing winds. The lighter dust which is carried on the wings of the wind and frequently transported for distances of several hundred miles,

* Pumpelly, Amer. Jou Science and Art, vol. 17, for 1879, p. 139.

leaves but a slight film upon the surface of the ground where it falls. And if this be so, one cannot but be amazed at the length of time required for the sub-aerial sifting of the material and for the transport from the dry central regions of Asia of that finest dust with which so large a portion of China eventually became covered, to a depth varying from 50 to 100 feet up to 2,000 feet."

Perhaps it is not entirely such a formation as this with which we have to deal in the Hawkesbury sandstone. Ours is a sand-dune area possibly not wholly like that of the Desert. It is no use at present encumbering ourselves with speculations as to whence this sand was derived. A very diligent and long continued examination of the constituents of the rock, taken from a very great number of places, and a better acquaintance with the physical characters of the older formations, will alone throw light on this question. We must not suppose either that the surface was wholly devoid of vegetation. If we remark how very little if any of the present vegetation is preserved in present soil we may be surprised to find so many impressions of ferns in the Hawkesbury sandstone. I should be inclined to think that the land around was a desert like Arabia, in which stand storms would be numerous and the accumulation of dust rapid. After the upheaval of the Permian strata the area may have been a desert region in which a few coal plants survived. A dry climate caused a rapid disintegration of strata and the accumulation of aerial sands. I do not pretend to assert that the upheaval took place immediately after the Permian period, but that it was not previous to that time, and may have been as late as the Cretaceous. The evidence of the plant remains is as yet insufficient to establish any period.

Stratified rocks not all aqueous.—At one time every formation not obviously fresh-water or intrusive was hastily concluded to have been derived from the sea, whether it contained marine remains or not. But we are no more justified in calling rocks marine without direct evidence than we are in calling them fresh-water. We are not acquainted with any existing sea-bottom utterly destitute of marine animal remains, no matter what the nature of the bottom. Foraminifera at least were always found, and these have existed from the earliest geological periods. But we have never heard of any marine area where the dredge brought up only plant remains and vegetable shale with sand destitute of lime and with small rounded pebbles. Mr. Selwyn, in his Notes on the Physical Geography, Geology, &c., of the Colony of Victoria,* refers to the absence of marine fossils from the lowest beds of the miocene rocks of Victoria, succeeding beds of evidently terrestrial character. This he calls a marine gravel. It is a wide-spread formation,

* One of the Intercolonial Exhibition Catalogues published in Melbourne in 1866.

being found over hill, plain, and valley. Much of the material composing it is rounded and waterworn, and Mr. Selwyn considered that there was evidence of too extensive and powerful an action to be ascribed to river floods. He adds that "very considerable areas now forming dry land in Victoria have been submerged in late tertiary times is unquestionable, and I believe that most if not all the older gold gravels, if not absolutely due to such cause, have at least been subjected to its influence, and in that case must be regarded as marine."* In answer to this, one might say that no marine remains are known to us of such a character. 2nd—That these gravel beds are very often found hundreds of feet above the level to which we know the tertiary submergence extended, viz. about 600 feet. 3rd—That their continuity is more apparent than real. 4th—That they often contain fossil wood, roots, and vegetable remains. 5th—That these drifts may well have been derived from the weathering of the carbonaceous conglomerates before the land was submerged at all. 6th—Finally, they may be the remains of a terrestrial formation such as I shall now describe.

Mexican eolian strata.—It has already been remarked that in many places in Europe there exists a recent formation at various altitudes which is more like a fresh-water deposit than any other, but yet found in situations which hardly admitted of any such explanation. Thus at Meudon, near Paris, there are deposits of an argillaceous fine gravel, resting unconformably on tertiary beds. It could not have come from the river Seine, which never reaches within 500 feet of the strata. A similar deposit is described in the Grecian Archipelago. It is a reddish loam, lying in horizontal beds at the highest altitudes. The explanation of these and similar facts has been given by Mons. Virlet-d'Aoust as the result of his observations in Mexico. †

Mons. Virlet-d'Aoust first remarked in Mexico a yellow deposit of clay or argillaceous marl, which not only completely enveloped certain isolated mountains, particularly volcanoes, but also constituted the sides and base of several chains of mountains such as those of Papocalepetl, Cetlatepetl, and Orizaba (17,370 feet). This formation is observed on the flanks of this giant of mountains, up to the height of about 12,000 feet above the sea-level, and it often attains especially in its lower part a thickness of from 150 to 300 feet. The deposit is somewhat of a miscellaneous composition, including angular fragments and rounded shingle derived from the underlying rocks. The enclosing cement or clay being of a very recent formation, has but a feeble consistency, so that where the torrents of tropical rain fall on it in the wet season enormous gullies or *Barrancas* are formed, into which the growing timber is precipitated and

* *Op. cit.* p. 25.

† See, for the full detail of these observations, *Bulletin de la Société de Géologie*, 2nd series, vol. XIII.

the whole debris carried as alluvium into the plains below. At first Mons. Virlet-d'Aoust imagined that this was an alluvial formation, derived from the washings from the mountain sides. But the deposit was found to cap the isolated mountains. It could not have been upheaved, for it was horizontal, and in any case included fragments of pottery and articles of human manufacture, besides wood and plant remains. As for its recent upheaval by volcanoes, none of them have reached the height at which this deposit is found. At last a sufficient cause was found in the dust-storms which are exceptionally violent and frequent in this region. The whole plateau is distinguished by immense whirlwinds of dust, or "*remolinos de polvo*," as they are here called, which are whirling along from various points on all fine days, carrying up in their course stones of very considerable size and other objects. These were thrown to heights of nearly 2,000 feet above the plain. Often the higher stratum of the air is rendered quite like a yellowish cloud from the quantity of dust remaining suspended.

I need not give all the arguments or the details of the facts which establish beyond a doubt that the formation is an aerial one. The able observer who thus explains the formation draws attention to the fact that there are many similar deposits. He also remarks that if the intermittent effect of whirlwinds would produce such a result, how much more vast and regular would be the effects in those places where the winds are constant in direction and strong, while passing over desert regions. Thus in China, the trade winds which cross the great desert of Gobi often bring a continuous rain of dust and fine sand over the southern regions. Dr. Macgowan describes one which lasted several hours, and was so dense as to hide the sun.

At Fontainebleau.—The sandstone of Fontainebleau is admitted by most geologists to be a formation derived from sand-dunes. It contains beds of shale and lignite, and a peculiar hydrocarbon called "*alios*," which is also found in the sand-dunes of the Landes.* The sandstone referred to occurs at Cernay, and both there and in many other places it is distinguished by the absence of level horizontal and regular stratification and the absence of any fossils. Mons. Stanislaus Meunier† gives the evidence in support of such an origin for these rocks and the sandstone of Rilly. He says that the suggestion of their eolian origin occurs to the mind at once, but this idea becomes a certainty when we find that they have the characteristics of the true sand-dunes of the Landes, and they enclose the same shales with the peculiar "*alios*" found there.

* *Comptes rendus*, vol. 59, p. 64. Also an analysis of the substance by Cloetz, p. 38.

† *Comptes rendus*, vol. 85, p. 1240.

Conglomerates.—The only question which still remains to be dealt with in dealing with these sands is the presence of pebbles and conglomerates. These latter cannot be attributed to wind. That the smaller lines of pebbles are aerial I make no doubt. Until I began to investigate the subject I had no idea how easily pebbles are borne along and up into the air by strong winds. On every windy day for the last two years, whenever I could, I have been out amongst the dust examining the heaps of stones formed by the wind, and watching or stopping the pebbles as they were swept by. All those of large size and shape I put aside for comparison, and it was surprising how much the dimensions of some exceeded my expectation. The conclusions I have come to are that a very small amount of wind action is sufficient to round the edges of very hard pebbles; that the abrasion is even more rapid than in water, and the result very similar. Some are completely rounded like water-worn pebbles. It is not necessary to suppose that these pebbles were more than rolled up the gradual slope of the dunes, a slope which always exists on the windward side. The broad and flat stones in the sandstone are of larger size, from their being more easily carried along. It is not at all an uncommon thing for a strong wind to lift up and carry along pebbles of an inch or a little more in length and half an inch thick. Let it be further borne in mind that except in the lowest strata, pebbles are rare, and those that are found are very small and only such as would be easily carried by the wind.

Whatever be the origin of the conglomerates, those of large size or great extent are only common at the base of the formation. They belong more to the coal measures than the sandstone. All the other instances can well be accounted for by (1) the action of creeks, of which there must have been many; (2) extraordinary storms or tornadoes; (3) concretionary action. That the latter is not an insignificant cause can easily be seen from the experiments of Stanislaus Meunier,* who found by the infiltration of chloride of lime and silicate of potash through heated loose sand, that concretions were rapidly formed. Accident revealed another illustration. During the siege of Paris sand was heaped upon the floor of the Geological Museum, to deaden the effect of the shells falling into the building. A heap of this sand lay at the foot of an immense block of iron pyrites from Portugal. The rain which came through the roof deeply corroded the mass, and the water flowing from the iron on to the stone deposited so much hydrated oxide of iron on to the sand that it became an irregular consolidated mass, portions of which were exceedingly hard, like iron.†

* *Presse scientifique des deux Mondes*, tome 2, de 1866.

† *Les causes actuels en Géologie*, p. 298. 8vo. Dunod, Paris, 1880.

Concretions.—Concretions or fragments of rock broken small and the edges abraded present pebbles of every variety of colour and apparent consistency. This can be easily seen by the examination of pebbles at the bottom of any stream. The river Medway in Central Queensland flows through a sandstone in every respect like the Hawkesbury sandstone, though it may be older, as it is full of impressions of *Lepidodendron nothum* and other plants. The pebbles at the bottom of the stream are of every colour, and differ much in mineral character. Some have come from a distance, but not many, as the river is rarely anything but a mere brook. The conclusion I draw from this fact is that it is not impossible to account for the conglomerates by even wind action. Supposing a wind-blown sand to become much altered and concreted. This always takes place by pressure, moisture, and other metamorphic processes which we are not able to estimate in every case, but whose action is evident. Let the sandstone be disintegrated by simple aerial weathering. This is no forced hypothesis. The thing is ever taking place in the arid deserts of the world. What is the result? The lighter portions are blown away; the concretions remain to strew the ground as a thick bed of shingle. Here follows an illustration of the process.

Stony Deserts.—In Central Australia, or rather south of the centre, between the 28th and 29th parallels of latitude, the country is distinguished by two features which entitle it to the names of the Stony and the Sandy Desert. The latter is a series of red sand ridges, whose glaring colour and whose aridity render them most striking objects. 20 or 30 miles of such sand is a common thing; it is quite loose, and is blown about by every wind. To these sandhills succeed immense plains, which are strewn as thickly as possible with rounded fragments of quartz and sandstone. A day's ride from the sandhills will place the traveller out of sight of all high land, and these stony deserts are like an immense seabeach with large fragments of rock scattered over the surface or buried in the ground by the force of waters. Such was Sturt's explanation of the stony desert, but he took an erroneous view of them from his limited knowledge of the country. In 1862 Mr. Howitt examined these deserts more thoroughly. He found they were a series of plains, some not over 17 miles wide, and others over 60. The stones are unequally distributed. They are very small in some localities, and form almost boulders in others. Many theories have been proposed to account for these stony deserts, the favourite of which is, that they are the remains left by some long-continued current of water running through the centre of the continent. In 1865 I wrote of this formation*:—"My own opinion is that these stones are the remains of a highly ferruginous

* Discovery and Exploration of Australia, vol. 2, p. 101.

sandstone which abounds in other parts of the continent. Where the strata contained a great deal of iron, there were formed siliceous concretions which resisted decomposition, while the rest of the rock fell away. * * * The red sand is certainly derived from a ferruginous sandstone. And if it be asked how the ridges should be so high and uneven, and the plains so low and flat, I answer that when the strata decomposed, the lighter portions drifted away into ridges, leaving the heavier remains scattered below on the plains." If the sand drifts again over these plains, and consolidates as it may easily do, we should have a wind-blown sandstone rock at the top, and a heavy rounded conglomerate at the bottom, and all this the result of aerial action alone. Such a deposit might be found over thousands of square miles, as it actually is so found in Central Australia, but no amount of ocean or river action that we know would produce such results. It is thus I offer to explain the wide-spread conglomerates which we find lying on the coal formation with very little change of character over thousands of square miles. Near Warwick and Stanthorpe, in Queensland, they are cemented together; at the Liverpool Range in New South Wales they are often found loosely aggregated. At the Endeavour River the same features manifest themselves, just as they do at the base of the Blue Mountains. A coast line might produce such a shingle, but there it would be of small width, and we should find marine remains, which here we do not. An ocean would not produce such results, and nothing of less extent than an ocean will meet the requirements of such an area; and then the presence of land plants, and the absence of marine remains, meet us again to destroy the ocean theory.

Ice action.—I have now a few words to say about the ice explanation for these rocks. It is true that there is a very scanty amount of fossils found in marine ice deposits, and also that they are quite wanting from some glacial beds; but, as a distinguished geologist has observed,—if we have not fossils, we have signs or marks, which are as clear indications of ice action as marine shells are of the presence of the sea. These indications are—(1) Till; (2) Moraines; (3) Glacial mud; (4) Boulder clay; (5) Ice grooves, scratches and polishing. *Till* is a deposit of excessively dense clay, stuck as full as it can hold of stones of all sizes, which are not arranged in any order, but look as if they had been forced and rammed in anyhow; big and little, angular and rounded together. Those fragments which are rounded, and in fact nearly all of them, show ice scratching and polishing. *Moraines* are confused masses of earth and stones jumbled together without regard to size, weight, or shape. The fragments are less grooved or scratched than in the till, because they have ridden on the top of the glacier; but they are always arranged in lines along a valley, or in a horseshoe-shaped heap across the end of it. *Glacial mud* is an extremely fine deposit of clay derived from streams issuing from the base of glaciers. It is formed by the impalpable mud which represents rocks ground down by glacier action. *Boulder clay* is a deposit formed partly by the drainage from glaciers, and partly of transported blocks of large size and various kinds of rocks. Boulder clay is stratified, but the stratification is often thrown into large folds and wrinkles, and ploughed up as it were on a gigantic scale by the former stranding of icebergs.

I do not think it necessary to go into detail in this matter any more than to say that we have none of these formations in the Hawkesbury rocks. The sand is utterly unlike any ice clay, and so are the included fragments. We have no such thing as ice scratches and grooves. Mr. Wilkinson mentions one instance of boulders which he attributes to ice action—this has been already referred to—and alludes to another which he does not describe. But if the ice interpretation were the correct one they should be the rule and not a rare exception in these vast deposits. I have every confidence in the wide experience and conscientious observations of my esteemed and learned friend the Government Geologist, and since I differ from him in the interpretation of these facts, I should like also to record here my high sense of the service he has rendered to geological science in New South Wales, the ready help he has given to me in these inquiries as well as on all other occasions.

But another difficulty is, that these glaciers must have come from an enormously high land to produce them on so grand a scale. We have no evidence that there has ever been such a mountain range. If there had been, it must have disappeared under a great and rapid subsidence. Yet it is upheaval, not subsidence, which we want, to account for the presence of the sandstone 3,000 feet and more above the sea. There is no parallel in all geology for the appearance and disappearance of mountain ranges in this manner. Moreover, we find this deposit far within the tropics, and where is the glacial system that would include such climatic changes? Finally, ice action is certainly unfavourable to the formation of coal and the luxuriant growth of ferns, yet these are the common remains in the sandstone. But really I do not think it is necessary to pursue this part of the subject any further.

Conclusion.—If these Hawkesbury rocks are the slow accumulation of aerial deposits since their upheaval from the sea, we have in them a monument which marks the extreme antiquity of this part of the earth's surface. We cannot fix precisely the age of the beds on which they rest. The advocates of the greatest age would rank them as Permian, while the most extreme opinions on the other side would not rank them as newer than the middle of

the Mesozoic period. The plants give us no great clue. They are those which belong to the Upper Coal basin, such as *Thinnfeldia odontopteroides.* Probably the beds went on accumulating long after this; or the plant may have a long range in its life history. We cannot fix any age for these beds. Similar deposits overlay marine beds with chalk fossils in Queensland. These cretaceous deposits are inclined at angles of between 20° and 30°. We have no such beds here in New South Wales. The beds with *Thinnfeldia* in Queensland are similarly disturbed, so that the Hawkesbury sandstone has been less disturbed than the formations of other parts of the continent. Here then we may suppose has survived that ancient fauna and flora which represents a long past epoch in the world's natural history and perhaps a link which connects us with the present time. Whatever disturbance there has been relates to the period of volcanic activity. This was shared by all the eastern half of the continent in tertiary times. Some of these lavas have burst through thick portions of the strata and now form the highest parts of the range. This outburst of volcanic matter on such a gigantic scale was no doubt attended with an alteration in the drainage as well as the watershed. We have daily increasing evidence of what the flora was, from the vegetable fossils which are being exhumed from beneath the beds of volcanic ash. It was quite different from what grows around us now, as far as the fossils will guide us. I may thus summarize the results of this essay :—

1. That the Hawkesbury sandstone is a wind-blown formation, interspersed with lagoons and morasses, with impure peat.

2. That there has been no upheaval, but rather a subsidence, which probably extends from the base of the range to the sea.

3. That the peculiar lamination of the beds is due to the angle at which dry sand slips and rests when blown by the wind.

4. The beds of ironstone represent vegetable matter destroyed in oxidizing the iron, and this is why so few plant remains are found.

5. The irregular layers of the sandstone formation probably represent what was a tranquil portion of the surface for a time, on which there may have been a vegetable growth now represented by ironstone bands.

6. The smaller gravel may be wind-blown; the larger may have been derived from creeks. This is also the origin of the fragments of shale. The creeks have undermined them and broken them up.

7. Conglomerates may have been derived from stony deserts, such as we have in the centre of Australia. They represent all the stones of a sandhill district from which the sand has been blown away.

8. The precipitous cliffs of the Blue Mountains are the hard central cores of sandhills, the loose portions of which have been easily blown or washed away.

9. That in all respects the sandstone is like many desert formations of the interior.

10. That a large arid or desert region has existed in Australia in mesozoic times, while to the north and north-west there was a cretaceous sea.

11. That this desert was terminated by the outpouring of vast quantities of volcanic rock, which altered the drainage and probably changed the climate.

12. We have no means of knowing the eastern limits of this ancient desert, as there has been subsidence on that side.

13. This formation differs but slightly from other and more extensive aerial ones in other countries, especially in Mexico, China, Arabia, &c.

14. There is no evidence of ice-action, and all the physical features are against such a supposition.

APPENDIX.

The following illustration of an eolian rock in the oft-cited case of Bermuda will be interesting:—

BERMUDA.—From "Notes by a Naturalist on the 'Challenger.'" By H. N. Moseley, M.A., F.R.S.; page 18.

The islands are almost entirely composed of brown calcareous sand, more or less consolidated into hard rock. In several places, and especially at Tuckerstown and Elbow Bay, there exist considerable tracts covered with modern sand-dunes, some of which are encroaching inland upon cultivated ground, and have overwhelmed at Elbow Bay a cottage, the chimney of which only is now to be seen above the sand. The constant encroachment of dunes is prevented by the growth upon them of several binding plants, amongst which a hard prickly grass (*Cenchrus*), with long, deeply-penetrating root-fibres, is the most efficient, assisted by the trailing *Ipomœa pes capræ*. When these binding plants are artificially removed the sand at once begins to shift, and the burying of the house, and the present encroachment at Elbow Bay, are said to have originated from the cutting through of some ancient sand hills for military purposes.

The sand is entirely calcareous, and dazzling white when seen in masses. When examined closely, in small quantities, it is seen to consist of various sized particles of broken shells. By gathering

samples from the shores where the material of which the sand is formed is first thrown up, and selecting portions where eddies of the wind have left the heavier particles together, a sand full of large fragments of shell, and containing even many whole shells of smaller species, may be obtained, and from the examination of these an accurate conclusion may be arrived at as to the main constituents of the finer and more comminuted sand, which is driven inland by the wind blown up into dunes, and from which the whole island above water has been formed.

The sand may be seen to be made up in by far its greater part of the shells of mollusca. Species of *Tellina*, *Cardium*, and *Arca*, contribute most largely to compose the mass, together with large quantities of pink-coloured fragments derived from a *Spondylus* which is common about the islands. A few gasteropodous shells contribute fragments, and a considerable number of foraminiferous shells occur in the sand, and no doubt careful examination would reveal the presence of fragments of tubes of *Serpulæ*, corals, calcareous algæ, *Bryozoa*, and Cirrhipede shells; but there can be no doubt that by far the greater mass is derived from the shells of mollusca.* Thus, although the foundations of Bermuda and its natural breakwaters and protections, without which it would not exist, are formed of corals; the part above water is mostly derived from another source, and even below the water the same is the case for some distance, for the same beds of sandstone were met with in an excavation carried to a depth of 50 feet.

The shells more or less broken are thrown up upon the beach and there pounded by the surf. As the tide recedes the resulting calcareous sand is rapidly dried by the sun, and the finer particles are borne off inland by the wind, to be heaped into the dome-shaped dunes. The rain charged with carbonic acid percolates through the dunes, and taking lime into solution re-deposits it as a cement, binding the sand grains together.† Successive showers of rain occurring at irregular intervals, some charged more, some less highly with carbonic acid, and forming each a crust on the surface of the dune of varying thickness, produce a series of very thin, hard layers in the mass of sand, alternating with seams of less consolidated sand which are to be observed commonly on the surfaces of fresh sand-dunes. These layers or strata of the hardened sand follow in form

* It would be of great interest to determine, by careful microscopic examination, what are relative percentages of the various calcareous structures composing the calcareous sands of coral islands in different parts of the world. I collected specimens of all the calcareous sands accessible during the voyage of the "Challenger," with that object. They vary much in composition, some being mainly foraminiferous.

† The process is described by Jukes, in his account of Raines Islet, "Voyage of the 'Fly,'" p. 339.

the contour of the dunes, and thus, where these have been perfect domes or mounds, dip outwards in all directions, with curved surfaces from a central vertical axis. Such an arrangement is constantly to be seen where sections of the older rocks are exposed. I saw especially good instances of it in a small island, near Castle Island in Harrington Sound. Where banks or long ridges of sand have been formed, strata following the surfaces of these in inclination are produced.

All kinds of curious irregularities in arrangement are to be found in the bedding of the strata, resulting evidently from the encroachment of one dune upon the edge of another, or the action of various eddies of wind, or the burying of a small dune in the edge of a larger one. In some cases an already hardened dune, after having suffered denudation by the action of the waves, has become buried in a more recent sand mound, and this process may have been repeated several times, as the accompanying diagram showing the arrangement of bedding in some rocks at Castle Harbour will show. I saw no rock in Bermuda with an inclination in its bedding of more than 35° 30′, which is not much more than the slope of some of the sand-hills.

Dana terms this calcareous sand rock "drift-sand rock."*

Nelson terms it "eolian formation," in his account of the geology of the Bermudas.†

Jukes observed that in Heron Island the main strata of calcareous rock composing the island dipped outwards from the longitudinal axis of the island towards the shore, north and south, with an inclination of from 8° to 10°, and Nelson observed similar dispositions of the strata at Bermuda.

The rock at Bermuda presents all degrees of consolidation, from beds of mere unagglutinated friable sand to extremely hard compact stone. The main component rock is a good deal softer than Bath stone. A much harder rock occurs at two places in the islands only, and is quarried for the construction of forts. The red fragments of *Spondylus* shell are especially well preserved in it. *A bed of lignite* was found at a depth of 40 feet *below the sea-level* in excavating for dockyard purposes, being evidently an ancient peat bed, such as those which now occur in the islands, overwhelmed with sand. Besides these primary sand rocks, a conglomerate is being formed on the shore in some places composed of beach fragments cemented together, as usually occurs in coral islands.

---

* Dana, *Corals and Coral Islands*, edition 1875, p. 182.

† Major-General Nelson, R. E., *On the Geology of the Bermudas*, Trans. Geog. Soc., London, vol. V, 1840.

Discussion.

Mr. Wilkinson, Government Geologist, read his paper in reply, as follows :—I feel it incumbent upon me to offer a few remarks upon the able and interesting paper which has been read, because in it Mr. Tenison-Woods has put before us a theory not only opposed to the views entertained by all previous observers as to the aqueous origin of the Hawkesbury sandstones, but also to the supposed evidence which I had the honor of bringing under the notice of this Society, of ice action having been concerned in the deposition of these rocks.

I am sure Mr. Tenison-Woods is desirous that we should freely express our views upon this very interesting question, for he kindly gave me a copy of his paper several days before it was read.

The paper, as you have heard, deals largely with the formation of blown sand deposits; and the description given of these is, in nearly all respects, very accurate. I may, perhaps, be justified in thus endorsing Mr. Tenison-Woods' description, seeing that I have made many examinations of blown sand deposits during the past twenty years. I shall not, therefore, dwell further upon this part of the paper, but I must take exception to the theory he now propounds. Were it not that I have made careful examinations of the Hawkesbury series in numerous localities, I might have had some reluctance in questioning the opinion of such an eminent scientific observer; and seeing that Mr. Tenison-Woods has happily given us a case in point, where Darwin's theory as to the formation of the Hawkesbury rocks has been proved faulty, I venture to be a little presumptuous, and say that my friend's theory may be at fault also.

Mr. Tenison-Woods thus summarizes the result of his essay :—"1. That the Hawkesbury sandstone is a wind-blown formation, interspersed with lagoons and morasses, with impure peat." I do not question the possibility of blown sand deposits occupying as extensive or even a larger area than that of the Hawkesbury formation. For instance, even in the Herbert and Diamantina district, beyond Cooper's Creek, there is a vast area, several hundred miles in extent, covered at intervals with blown sand ridges in the course of formation. My assistant, Mr. J. E. Carne, who has explored this tract of country, informs me that some of the sand ridges resemble huge railway embankments, and run for a distance of 12 miles without a break. They vary from 200 to 500 yards in width, and are about 60 feet high, and generally lie in the direction of the prevailing winds. Between them are mud flats, liable to inundation. In the dry weather the mud cracks, and numerous large and deep fissures open in all directions, so that in places you cannot ride across them, but have to take a circuitous route along the flanks of the sand ridges. The sand is of a red colour, and is evidently derived from the ferruginous quartzite rocks (probably Cretaceous), which, in a very fragmentary state, crop out at intervals, and form the Downs. I may mention another instance, though but a small one, with which you all are familiar. The valley lying between Sydney and Botany Bay is partly filled with blown sand deposits, while here and there occur small lagoons, in which carbonaceous sediment is accumulating. To a certain extent this illustration may serve to show sandstones and irregular shale beds in process of formation; but only in a small degree will they resemble the sandstones and shales of the Hawkesbury formation. The Hon. Francis Lord informs me that on the Coronulla beach, which is exposed to the easterly winds, the sand dunes have risen 10 feet during the last sixteen years, burying up as they advance on the lee side the trees and other vegetation. You cannot but be struck with the undulating and hilly surface of these blown sand areas. Now, if you look at the beautiful sections exposed in the cliffs along the Bondi coast, or in the smaller cliffs fringing the harbour, or better still, in those magnificent precipices in the Blue Mountains, the most prominent feature that you will notice is the horizontal stratification of the beds of sandstone; in fact, the parallelism of the main lines of stratification is a prevailing feature in the Hawkesbury formation, and this alone is evidence of the beds having been deposited under water. But in wind-blown formations such stratification is seldom seen to extend for more than a few yards.

Then, as regards the thinly-laminated shales, and fine-grained sandy ironstone shales, which sometimes occur between the main beds of sandstone, I do not see the necessity for ascribing their origin to dust-storms; for, in their laminated structure, they exactly resemble the aqueous rocks of the Coal Measures and other formations. Then, again, there are the carbonaceous shale beds, which Mr. Tenison-Woods believes to be of aqueous origin, and to have been formed in lagoons or morasses. But if you will closely examine these deposits—and there are many instances of them to be seen in the quarries and cliffs about Sydney—I think you will come to the conclusion that, after the sand had been laid down, strong local currents eroded channels and hollows in the sand beds; then quiet water succeeding, the fine earthy matter which could not settle in the currents which drifted the sand along, now settled down in the sheltered hollows and so far filled them up, until recurring currents either broke up the layers of mud, or overwhelmed them with fresh deposits of sand and pebbles. The evidence of such changes having taken place is very clear. I will adduce further proof of the aqueous origin of the Hawkesbury series, in replying to the other conclusions stated by Mr. Tenison-Woods.

The second conclusion is, "That there has been no upheaval, but rather a subsidence, which probably extends from the base of the range to the sea." This question does not bear directly upon the mode of deposition of the sandstones, excepting as tending to show that the rocks now below sea-level, if of wind-blown formation, must have subsided. It has always been the opinion of geologists that there has been a subsidence of the area between the sea and the base of the ranges near the Nepean, as stated by Mr. Tenison-Woods; but the arguments brought forward to show that there has been no upheaval I do not consider to be conclusive. It is said that there are no signs of upheaval, seeing that the beds lie in nearly a horizontal position. But then the same remark will apply to the Lower Coal Measures which occur near Wallerawang, at an elevation of 3,000 feet above sea-level, and are full of spirifers and other marine fossil remains. These beds exhibit no signs of disturbance, their bedding is nearly horizontal, like that of the Hawkesbury rocks overlying them, and yet the upheaval of them from the sea is unquestionable. In a similar manner they are found above sea-level in the Wollongong and Kiama districts. In fact, in most places where these marine beds occur in a horizontal position, we have the Upper Coal Measures and Hawkesbury sandstones overlying them; but where they have been locally disturbed and tilted up, as near Maitland, we find no Hawkesbury rocks, for it is clear that in this locality the overlying beds have been removed by denudation. At Mittagong, on the Great Southern Railway, the great mass of trachyte which forms the hill near the station has been upheaved through both the Hawkesbury beds and the more recent Wianamatta shales. We have evidence, therefore, on both sides of the Hawkesbury basin, of disturbance after the deposition of these beds; and I believe that the whole formation, and probably a great portion of the Dividing Range, was raised bodily from the sea without disturbing the horizontality of the strata, except, perhaps, in a few local instances. We see that the vast Mesozoic marine formation of Queensland, which extends into this Colony about the Darling and Mount Poole districts, has been upheaved in this manner.

The third conclusion states "that the peculiar lamination of the beds is due to the angle at which dry sand slips and rests when blown by the wind." This statement tends rather to weaken than support the theory of the eolian character of the Hawkesbury sandstones. The peculiar lamination, or "false-bedding" as it is usually called, referred to, is a structure not confined only to eolian rocks, but it is met with in almost all sedimentary formations, whether of marine or fresh-water origin, and is regarded as indicative of more or less strong currents in shallow water. I have seen it frequently in different aqueous formations. Mr. Selwyn, F.R.S., formerly Director of the Geological Survey of Victoria, and at present the Director of the Canadian Survey, whose long experience in geological surveying should entitle him to be considered one of the highest authorities upon the structure of rocks, speaking of the Mesozoic Carbonaceous formation in Victoria, says, "The character of the strata generally indicates that they have been formed in shallow water, under the influence of strong and constantly-varying currents, giving rise to much diagonal and wedge-shaped stratification or 'false-bedding.'" The formation referred to, which I have examined and surveyed in the Cape Otway, Geelong, and Gippsland districts, consists not only of sandstone and shales, but of beds of coarse pebble conglomerates. Mr. Selwyn also mentions that much "false-bedding" is observable in the sandstones of the older Grampian series. Jukes says, "That 'false-bedding' is a proof of frequent change in the direction and velocity of the currents which brought the sand and gravel into the water. * * Such appearances generally indicate shallow water, and are often seen in cutting through an old estuary or delta." Dana, in describing the structure of formations made from river and oceanic action combined, and referring particularly to sand-flats, such as that off the coast of New Jersey, which is 50 to 80 miles wide (nearly the area of our Hawkesbury formation), says, "The stratification or bedding is parallel to the general surface of the flat, because the successive additions are laid over this surface, consequently the bedding will be horizontal, or nearly so. The sand beds, where, in shallow water, and washed over by the tidal currents, have often the layers obliquely laminated. Where there are strong flows of the tides between islands and the mainland, or among groups of islands, the material may be in part pebbly, and oblique lamination may be a feature of the beds." Actual instances might also be quoted from Lyell, Rutley, and other writers, of the occurrence of false-bedding in fresh-water and marine deposits, as well as in blown sand-beds. With reference to the angle at which sand slips, and rests in air and in water, I have made numerous observations. The highest angle that I have found upon the slopes of blown sandhills is 36°. At Cape Otway, in Victoria, there are splendid examples of blowns and dunes. Some of them are about 50 feet high, and they are advancing inland at the rate of about 1 foot a year, covering up gum-trees in their course. The highest angle that I have observed by experiment of rolling sand in water is 31°. Now the greatest inclination that I have measured (and I have taken a great many measurements) of the false-bedding in the Hawkesbury sandstones is an angle of 26½°; the prevailing angle is about 20°. In blown sand-hills the angles of 30° to 33° are very frequent. Surely, then, were the Hawkesbury beds of eolian origin we should have found angles of inclination greater than 26½°. This is an important

point, and tends to show that this false-bedding is due to currents of water. The prevailing direction of the dip of the false-bedding is towards the north-east, showing that the currents came from the south-west; but there are often seen beds with the dip towards other directions.

Mr. Tenison-Woods' conclusions Nos. 4 and 5 refer to the origin of the ironstones. I do not consider that the irregular bands mentioned represent old land surfaces, for the bands not only curve in all positions but are sometimes vertical. They may be well seen in the cuttings along the Great Western Railway between Penrith and Lithgow. Most of them have been formed from the oxidation of iron in solution in the water, permeating the sandstones and shales and the joints traversing them. Professor Liversidge has made analyses of these ferruginous bands.

Conclusion 6 states that "The smaller gravel may be wind-blown, the larger may have been derived from creeks. This is also the origin of the fragments of shale. The creeks have undermined them, and broken them up." The smaller and larger gravels are included in such a manner in the sandstone beds that they have evidently been brought by the same currents that transported the sand, and which I have already alluded to. It is very unusual to find creeks traversing for any great distance blown sand beds for such a distance as these must have done, for the rounded pebbles of the larger gravel (which are sometimes over 6 inches in diameter) consist of quartzite, black slate, quartz, silicious slate, &c., which may have been derived from the Hartley ranges, some 60 miles distant, which are the nearest formations of the character of the pebbles. Amongst the everchanging surface of wind-blown sands it is very improbable that the courses of creeks would have been sufficiently constant to have enabled pebbles to have been conveyed by them for such a distance; whereas the transport of pebbles for a great distance by marine or estuarine currents, and their deposition in the manner in which we find these, is well known. As regards the shale fragments, if you closely examine their mode of occurrence you will see that they have not been undermined by creeks, for many of the fragments lie immediately above beds of shale that have not been disturbed. The sketch placed before you shows one of the fragments, 12 feet in length, tilted up and laid upon the unbroken part of the shale bed. But I shall again refer to these shale boulders when speaking of evidence of ice action.

Conclusion 7 states that "conglomerates may have been derived from stony deserts such as we have in the centre of Australia. They represent all the stones of a sand-hill district from which the sand has been blown away." It is impossible that the conglomerates in the Hawkesbury series could have been derived in the manner just mentioned. To satisfy this theory the conglomerates must occur at the base of the series; but they do not. Rounded pebbles are certainly found all through the series from bottom to top, but the conglomerates occur principally in the uppermost portions. Our former distinguished Vice-President (the late Rev. W. B. Clarke) mentions this fact in the passage quoted by Mr. Tenison-Woods at the commencement of his paper; and Darwin also states that the pebbles increase in number and size in the upper beds. I have examined many sections of the Hawkesbury formation and have not observed conglomerates at its base. Near Govett's Leap the conglomerates are cemented by iron and manganese oxides; the pebbles are generally small, but some of them are over 2 inches in diameter; a specimen taken from this locality containing a hard sandstone pebble is now on the table before you. I also exhibit specimens of the conglomerate from the Woolloomooloo quarries, as they well show the different kinds of the large pebbles, as well as the angular fragments of shale. As you approach the western margin of the Hawkesbury formation, near Marulan, you will find the upper sandstone beds gradually pass into massive pebble conglomerates of great thickness. This is just what we should expect to find on the margin of an old estuary or lake. In the cliffs at Bondi, and elsewhere about Sydney, you may see small beds, 3 feet thick, of pebble conglomerate. In their mode of arrangement the pebbles are plainly seen to have been deposited by aqueous agencies.

Conclusion 8. "The precipitous cliffs of the Blue Mountains are the hard central cores of sandhills, the loose portions of which have been easily blown or washed away." The horizontality of the beds and their structure, which I have already described, is obviously against such a supposition as that now stated; I need not therefore again refer to this. The precipitous cliffs of sandstone, and the sloping surfaces of the underlying coal measures where exposed, plainly indicate the nature of atmospheric agencies that have given them their picturesque shapes. But as this opens the subject of the denudation, or scooping out, of the great valleys which furrow both sides of the Main Dividing Range, and as my views have already been published in the "New South Wales Railway Guide-book," I will not enter upon the subject at the present time.

Conclusions 9 and 10.—"That in all respects the sandstone is like many desert formations of the interior," and "That a large and arid or desert region has existed in Australia in Mesozoic times, while to the north and north-west there was a Cretaceous sea." In reference to these conclusions I will only remark that if many of the "desert formations of the interior" do *in all respects* resemble the Hawkesbury formation, then, from what I have already seen of this formation, I very much question that they

were formed in "a large arid or desert region." And it is not improbable that the Cretaceous sea spoken of extended to Western Australia.

Regarding conclusion 11, we know that after the Cretaceous and during the Tertiary periods great volcanic activity was manifested along the elevated land which forms the Great Dividing Range; but with the exception of a few places along the crest of this range, I do not think that these eruptions altered the drainage, for we find that the old Tertiary drainage channels, or "leads," as the miners call them, took the same direction as the present ones; and the present range of Paleozoic rocks which extends through this Colony and Queensland, and the lateral range which branches off from it near Orange and stretches away to the Grey Range, must have existed in Mesozoic times, as they evidently formed the eastern and part of the southern margin of the Cretaceous sea.

Conclusions 12 and 13 I need not now refer to. No. 14 states that "there is no evidence of ice action, and all the physical features are against such a supposition." Now, the mode of occurrence of the angular boulders of shale, which I described in a paper which I had the honor of reading before the Society last year, I can only attribute to the action of ice. I do not see that the physical features of the period when the Hawkesbury rocks were deposited may have been against this supposition; on the contrary, I think they may have favoured it, for on the western margin of the Hawkesbury area, near Bowenfells, we find that at least a thickness of 10,000 feet of the Devonian formation has been removed by denudation. This is no supposition; it is fact, ascertained by actual measurement of the denuded strata, as shown upon my geological map of that district. What may not then have been the denudation of the remaining margin of the Hawkesbury area? Besides, we do not know the extent of the subsidence of the eastern margin of the Hawkesbury area, as Mr. Tenison-Woods has justly stated in his 12th conclusion. I do not look for any of the signs of ice action mentioned by Mr. Tenison-Woods, but I do for certain signs that he has not mentioned, and they are the signs of *ground ice*. At different levels in the series are thin beds of shale, and the sandstones immediately above these shale beds frequently enclose angular boulders of all sizes up to 20 feet or more in diameter. These boulders have been torn up from the underlying beds of shale and embedded in a very confused manner in the sand and rounded pebbles brought by the transporting currents. The angular form and mode of occurrence of these boulders of soft shale evidently show that the shale beds have been disturbed by moving ice. Professor Julius von Haast, Ph.D., F.R.S., Director of the Canterbury Museum, New Zealand, has also examined these boulder beds, and expressed to me his opinion that the underlying shales have been broken up by "ground-ice." In December, 1879, I contributed a paper on this subject to the Royal Society of New South Wales, and Professor W. J. Stephens, M.A., communicated to the Linnean Society of New South Wales the results of similar observations made by himself of the Hawkesbury rocks in the Upper Nepean district.

Professor STEPHENS said: When I heard last Wednesday a most ingenious account read by Mr. Woods on the geology of our sandstone, I felt I should like to have some opportunity of answering at least one or two details, but as the time was short on that evening, it was suggested and resolved that the discussion should be deferred to an adjourned meeting. On Saturday, accordingly, when the last instalment of his paper appeared in the *Sydney Morning Herald*, I set to work upon the subject with so much good-will that by yesterday morning I had completed something like forty octavo pages of matter. Fast writing is proverbially slow reading, and haste precludes brevity. I have therefore excised the greater portion of what I had prepared, and offer for your consideration only the following remarks.

Mr. Tenison-Woods states that "we are not acquainted with any existing sea-bottom utterly destitute of marine animal remains, no matter what was the nature of the bottom. Foraminifera, at least, were always found, and these have existed from the earliest geological periods." Now, so far as sea-bottoms of sand or calcareous matter are alone regarded, this proposition is in all probability correct; and thus, so far as the formation of the Hawkesbury rocks is alone under consideration, the argument which depends upon it is just. But I do not suppose that Mr. Tenison-Woods needs to be reminded that the same observer, the late Professor Sir C. Wyville Thomson, F.R.S., who was the first to demonstrate the existence of a previously unsuspected abundance and variety of animal life at almost abysmal depths, is also our authority for the occurrence of vast beds of an argillaceous ooze, which are perfectly destitute of any vestige of life or organism, and are now in process of deposition over large areas of still greater profundity. Moreover, whatever our present seas might indicate, we must not forget that there are many ancient marine formations, not metamorphic in structure, and composed of materials highly capable of preserving animal remains, which are, nevertheless, for thousands of feet in thickness together, absolutely devoid of any evidence of contemporaneous life, although isolated fossils found after long searching—one here and another there—show that a great variety of animals of highly complex organizations, and *a fortiori* more of a simpler structure, were in existence at the time of their deposition. The weight which Mr. Woods' authority naturally lends to every statement which he makes renders it necessary to notice this, lest an incautiously worded sentence which, after all, is generally true, might be accepted as one of absolute and universal application.

Under the head of Stratification, Mr. Tenison-Woods gives an account of the false-bedding or oblique lamination so characteristic of the Hawkesbury sandstones, and insists with perfect justice on the extreme prevalence of this phenomenon in their stratification. On one point, however, I must express my doubts. He states that "the irregularity of the dip of the false-bedding is surprising; in a few feet the dip will vary in almost every direction and angle, though rarely at a greater one than 25°." Now, I have for many years habitually examined every example of false-bedding that has fallen under my notice; but I have done this only in order to see in what direction these particular sands were moving when they formed these laminæ, or, in other words, according to my own view of the formation, to ascertain what was the general direction of the currents which drifted the said sand. Consequently I never took special note of the amount, but only of the direction of dip, or, which comes to the same thing, I disregarded dip, and regarded only strike. I can, therefore, only state my strong impression, without actual measurement, that the angle of dip is almost constant, varying very little indeed. I should in any case have paid little attention to sections, as in these the true obliquity of lamination is only seen when the plane of section happens to be at right angles to the strike of the laminæ, the angle gradually diminishing as this plane becomes more nearly parallel to the strike, until, when the section is along the line of strike, all obliquity disappears. In other words, the apparent inclination of the laminæ, as seen in section, varies from (say) 25° to 0°, as the angle which the section-plane makes with the strike varies from 90° to 0°. This consideration has always led me to examine the false-bedding only where its *planes* are thoroughly exposed, as in the water-tables of mountain roads, river beds, and in those flagstone quarries where this structure gives special value to the stone. And my impression—I am sorry that I cannot substitute a more positive term—after observing many hundreds of examples, is that the angle of oblique lamination is very nearly constant, and very near, but I should have thought less than 25°. I am quite alive to the absurdity of opposing mere impressions to exact observations, and do not expect to have my general recollections weighed, even for a moment, against the statements of Mr. Tenison-Woods; but this impression is so strong upon my mind, that I could not refrain from putting it before you as part of the defence of Water against Wind. I have not been able to leave Sydney since the original paper was read, and am therefore unable at present to verify my statements, as of course I am bound to do or surrender the point. On this head Darwin is clearly in error, first in stating that the dip of the laminæ is frequently as high as 45°, and, secondly, in referring them to disturbances of the sea during storms in which "the bed of the ocean is heaped up during gales into great ripple-like furrows and depressions, which are afterwards cut off by the currents during more tranquil weather, and again furrowed during gales." For my own part, I have no doubt that this lamination is in general caused by the flow of water carrying sandy or other detritus from one level to another. As the grains fall over the verge they arrange themselves in sloping beds, descending to the lower level. This slanting front is immediately covered by another thin course, dipping at the same angle. This process I have often watched in the sands of Morecambe Bay. It is, in fact, this drift of wind-and-water-shifted sands that changes with such rapidity the river courses in that estuary. The channels are continually but gradually altering at every tide, but the principal cause of their sudden and dangerous alterations is, I believe, the action of the wind on very thin sheets of water covering very quick sands. And the layers so deposited are, when solidified, the laminæ of the oblique or cross or false stratification of which we have such abundant illustration in the Hawkesbury beds.

The broad band of red stone exposed in the upper cutting of the second Zigzag deserves further investigation. I do not presume to question Mr. Tenison-Woods' views as to its formation, but only to indicate that it appears to form a distinct member of the upper beds, which may serve as a geological bench-mark to which other portions may be referred. For not only is it continous along the range of Hassan's Walls to Bowenfels, and back to Mount Victoria and the neighbouring cliffs to the east, but a similar rock appears at the top of the Bulli Pass, and similar beds of much greater thickness were pierced by the diamond drill at Sutherland. I think that Dr. Hector was inclined to suppose that in the latter case, at least, it marked the passage from the Carboniferous to the Hawkesbury beds; and though this step in the ascending scale would throw the lower portion of the western cliffs into the coal measures, yet the idea deserves consideration.

The undulating character of the true surfaces of stratification is frequently and emphatically referred to by Mr. Tenison-Woods. And if such irregularities of deposition were anywhere observed in the Hawkesbury rocks as are generally to be seen in sections of wind-blown sands, where the planes of stratification are never, or hardly ever, even approximately horizontal, but dip in any direction according as the wind might veer from time to time during their formation, it would be impossible to question the completeness of his demonstration; but the horizontality of these Hawkesbury beds is their principal feature. They do indeed thicken here and thin out there, and their surfaces are therefore

not truly horizontal; but they are horizontal enough to be called, as they are by Darwin, Clarke, and Mr. Tenison-Woods himself, horizontal rather than undulating, and to show that water was concerned somehow or other in their levelling. We may admit that where exposed sands are saturated at a certain depth with water, the friction of strong or continous winds will plane down all elevations above that horizon of saturation, and leave a surface as flat as those in question; but is it conceivable that wind, independently of water, should blow sands into horizontal beds?

Again, the materials of which the partings which separate these beds are composed are arranged in laminæ, which are "thin, horizontal, and rather difficult to trace." They are sometimes not to be traced at all; and, where they do exist, they may quite as well have been the result of the burying up of fresh-water algæ or other aquatic plants by successive layers of sand under water, as of a similar overlaying of a land vegetation, including, I suppose trees, stems, and branches, with hard woody tissue, or at least with nuts or the like, which might as well have been preserved in the partings, as casual fragments of the kind have been in the intermediate thickness of the beds. Now, Mr. Woods does, indeed, allow the action of water in swamps or pools, and in creeks, of which action the shales intercalated in the sandstone give frequent indications. They have been, he says, deposited in the swamps, and subsequently eroded by the creeks. And this would account for many, but not for all of the phenomena presented. I quote Darwin again: "In several parts of the sandstones I noticed patches of shale, which might at the first glance have been mistaken for extraneous fragments; their horizontal laminæ, however, being parallel with those of the sandstone, showed that they were the remains of thin continuous beds. One such fragment (probably the section of a long narrow strip) seen in the face of a cliff, was of greater vertical thickness than breadth, which proves that this bed of shale must have been in some degree consolidated after having been deposited, and before being worn away by the currents. Each patch of the shale shows also how slowly many of the successive layers of sandstone were deposited." Examples of similar erosion are common; an excellent one occurring in a quarry to the west of Rushcutter's Bay, which is duplicated by a nearly parallel section at the foot of the cliff to the east of Woolomooloo. The evidence in favour of the existence of strong currents of water is unmistakable. They have cut channels through sands and shales, and filled them up again, sometimes with stuff derived from the immediate neighbourhood, containing fragments of the already indurated beds, and sometimes with clean sand which may have been drifted some distance. Such rivers are not to be found in a system of sandhills, unless the dunes be formed by the river, or intervene between it and the sea.

Though the sandstone is generally fine-grained, yet it often becomes gritty and coarse-grained, sometimes so much so as to pass rather into a conglomerate. And the conglomerates of the Hawkesbury rocks are, so far as my observation extends, of two distinct characters. One, in which the pebbles are chiefly or entirely of quartz, very imperfectly rounded and almost always cemented by iron sandstone. Such rocks are common near Mount Victoria, Katoomba, and many other spots on the mountains and elsewhere. In the immediate neighbourhood of Sydney, however, there is a locality very easy of access and examination, in which this formation may be observed. I mean Clark Island, off Rose Bay. (Specimen produced.) Where the iron cement is absent I observe the quartz pebbles to be smaller, whiter, and very easily separated, as in many places about North Head, *e.g.* Clifton Heights. The larger pebbles are often, as may be seen, traversed by ferruginous veins, which, however, may not have been impregnated previously to their being imbedded in the composite mass. The second form of conglomerate is much rarer, is not very ferruginous, and contains larger and well-rounded pebbles of various rocks or minerals. I observed a good instance last year, in company with the Rev. Dr. Woolls, in the gorge of the Grose River, about a mile, as I should guess, from the junction of the Springwood Creek, or two miles below that of the Burralow. It is here intercalated with and passes into the sandstones, and has very much the appearance of a reconstruction of the materials of an older and more massive conglomerate, such as is found at Winburndale, near Kirkconnell. It may, indeed, be supposed that at the point which I have mentioned the coal measures have been cut into and exposed by the river. Perhaps this is the fact, as it certainly is a few miles higher up. I can only say that, with the strongest desire to find indications to that purport, I was at last obliged to acquiesce in their absence. Now, whatever may have been the origin of the first class, or quartzose conglomerates, the second are undoubtedly of aqueous, and in many cases probably of fluviatile origin. But if the first are, as in some localities they may appear to be, of aerial origin, owing their coarseness of grit to the sifting action of the wind, one is puzzled to account for the quartz being able to travel so far to the eastward, under constant attrition by blowing sand during the whole period of that protracted travel, and yet to remain rough and angular, instead of becoming perfectly rounded and polished. It may, indeed, be that origin of these fragments is to be sought much nearer their present abode, in some of the trap dykes which intersect the sandstones. If this be not the case, I cannot but think that their general character is adverse to the pneumatic theory. There is no difference of character that I can see between these two specimens (produced), one of which is from Katoomba, and the other from Clark

Island. If, indeed, any of our conglomerates were composed of concretionary lumps, I might accept Mr. Tenison-Woods' theory so far as they were in question.

Once more, as to the horizontality of the beds. The general slope of the original surface from the base of those eminences, such as Mount King George, Mount Tomah, and Mount Hay, which have been preserved from denudation by their caps of basalt, down to the eastern escarpment, is of course determined for us by the summit levels of the various ridges which remain as watersheds of general drainage. This slope, which is at the rate of 100 feet per mile, presents all the appearances which would lead one to suppose that it was a plane of marine denudation. I am confident that no one contemplating it for the first time could come to any other conclusion. But, as Mr. Tenison-Woods justly urges, marine action is out of court.* The "plane of marine denudation" must therefore have its name erased from the evidence, in spite of its extreme plausibility. And we shall be assured that no mistake has been made in this when we observe that the underlying coal measures slope in precisely the same manner towards the same quarter. Then the question occurs,—Were both formations constructed upon a pre-existing slope, to which their own bedding was accommodated; or has there been a general movement of elevation in the west, and depression in the east? I am certain from examination of the phenomena of the Hawkesbury valley that its bed must at one time—perhaps as far back as the period which we call Cretaceous—have been several hundred feet above the sea, and that within the Tertiary epoch it must have been at least 200 feet higher than at present. These considerations induce me to conclude that there has been a movement, though not perhaps to a very great extent, and certainly not such, either in direction or extent, as to raise any marine formations above the sea, but rather the contrary.

The portions of Mr. Woods' paper which deal with the shape of the sand-grains are exceedingly interesting and important. The recrystallization, or epicrystallization of the quartz had indeed been previously considered by the late Rev. W. B. Clarke; but no one, so far as I am aware, has previously attempted to apply to the Hawkesbury rocks the tests of roundness and angularity, as distinguishing water sands from wind sands. And although metamorphic action has obliterated, as the author laments, the characteristic form on which he relies, there are abundant instances where the grains have remained unaltered, and testify to their having at one time or other suffered attrition, or rather contrition, under long protracted periods of sand-drift. But though their

* I think it does not follow that all operations of water on the great scale are excluded also. There are other water-gods besides Neptune.

sphericity testifies to their having once been blown sands, yet that is a character so hard to destroy in such a material as quartz, excepting by molecular change or metamorphism, that it cannot be relied upon as a proof that all sandstones composed of such globules are of aerial origin. Nay, the same sand-grains may have been rounded and polished in times so remote as to have formed eolian beds as far back at least as the lowest Silurian; for the Potsdam sandstone, which is regarded as homotaxial, if not contemporaneous, with the Lingula beds, is largely composed of such materials, forming, evidently, an aerial deposit. Such rocks may have been—or rather perhaps have generally been—again reduced into their constituent grains, which have again been composed into a later sandstone, either aerial or aqueous, and that either marine or lacustrine. Out of the ruins of this another is built up, and out of this, in course of time, another, while nevertheless, in spite of their vicissitudes of association, the individual grains retain their sphericity, unimpaired from age to age. At the same time, it seems not improbable that the Hawkesbury sands were actually in the wind-blown condition immediately before their consolidation, and until they reached the water in which they were to be arranged; and I am ready to accept, in deference to the authority of Mr. Tenison-Woods, his explanation of certain phenomena in certain localities as a satisfactory account of their causes. It is very possible that, while certain portions of this huge sand-drift were being arranged and consolidated in water, on a large scale, other portions may have been similarly consolidated on land. It is only with reference to the relative importance of the two processes in these rocks that I venture to differ from the author*. No other hypothesis has ever been presented, I feel confident, which so satisfactorily accounts for the collection of the greater part if not all of the Blue Mountain sands, in their north-western range, as this which has now been proposed by Mr. Tenison-Woods. It may seem doubtful, however, whether the southern and coast ranges are of exactly similar origin, on account of the great height of the dividing range to the west of them. But if the basalt of the Crookwell and Grabbengullen ranges be, as it may very well be, of more recent origin than the sandstone, then the sands may have blown up along the present lines of the Lachlan and Abercrombie valleys, at that time probably nothing like so rugged as at present.

If I should venture to propose an hypothesis which should account for the geographical position or local fixture of, at least, the western portion of this great sandstone, I should point to the

*I suppose these sands to have been, in the main, derived from the weathering of the Carboniferous and older sandstones which appear to the westward, though I would not venture to say that a portion of the finer sand may not have been blown from the far interior; but I should think the nearer source the more probable for the formation as a whole.

unbroken line of igneous eruptions which marks the whole course of the dividing range, from the ridge between Cassilis and the Talbragar, itself volcanic, down to the culminating points of the Blue Mountain Range. The summits between the Cudgegong and Turon on one side, the Goulburn and Colo on the other, Mounts Wilson, Tomah, King George, and Hay, which are all understood to be capped with trap, lie nearly in this line, which is then unbroken by visible eruptions along the line of the upper Cox drainage, until similar outbursts again appear on (approximately) the same axis, west and east of the great Wollondilly hollow. Now these visible cappings of volcanic rocks are later than the sandstone, and have therefore had no influence on its deposition; but it is reasonable to suppose that they are only later outbreaks of the same energy which had previously formed a (possibly broken) range, penetrating the carboniferous beds in the position and direction above mentioned, just as the main Liverpool range does, running, as we see, nearly east from Casilis, and therefore almost at right angles to its southern extension towards the west. From the terminal point of this extension we see upon the map a most extraordinary streak of sandstone, forming the Main Dividing Range, but at right angles to the south-western extension of the range. It is like the handle of a frying-pan, when the pan itself represents the Hawkesbury basin, and, until I had the pleasure of hearing Mr. Tenison-Woods' paper, appeared to me quite inexplicable. Now, on the contrary, it appears the most natural thing in the world. Grant that a sand-drift existed from the westward, blowing for ages unchecked along the broad open of the Talbragar, and rising to the crest of the dividing range at this point—itself of then recent origin—then, it would assuredly form on the lee, if not also on the windward side of the elevation such a stripe of sands —ultimately to become sandstones—as we find there. If this volcanic ridge extended along the range to the southward—and what can be more probable?—similar sand-drifts up the valley of the Turon and Macquarie would, in like manner, accumulate about it, to be in time also converted into sandstones, and to be capped in particular points by subsequent eruptions along the same general line. The sands, or sandstones, not protected by this capping, have been subsequently removed, partly, I am ready to believe, by wind, but partly also by the action of water.

In illustration of Mr. Tenison-Woods' argument, I may refer to the wind-drifted sandstones of the Namoi. In the open valley between the Nandewar and Willela ranges, there are no sandstones above the general level, except those of carboniferous date, which have often been hoisted to a great height on masses of erupted felstone, as I should term it; but large hollows in the original valley have been filled by a fine white quicksand, evidently of eolian formation, in which water is abundant, as for example, at Killarney

Station, north of Narrabri. Similar formations, very little above the general surface level, and below the sometimes emerging sandstones and conglomerate of the Upper (?) Coal, are seen along the Bullawa Creek and elsewhere. The only fossils I could obtain in these are obscure remains of vegetable structure, with a few well-marked fragments of cycadaceous plants. Here, I repeat, in the broad open valley which runs without a break up to the head of Breeza Plains, or rather to the Gap, where there is a station on the Narrabri railway line, there are no sand-hills worth mentioning; but to the southward of Boggabri, where the ranges which separate the Terrabeile Creek from the Brigalow form a breakwind, we have a capping of undoubtedly eolian sandstone. This, the Willela range, has a very gradual slope to the west, but ends in a very steep escarpment upon the east, resting there upon beds of shale which are so ferruginous as to deserve the name of iron ore, and which overlie conformably the sandstone and conglomerates which there cover very thinly the actual coal. These rocks are something like some of the upper beds of the Hawkesbury rocks between Blue Mountain line and Mount Victoria; but they are not indurated like the greater part of the formation, and are much shallower. From their position, overlying as they do the Carboniferous beds, they may be really coeval with the Hawkesbury's, and the vegetation is very similar though richer.

At the same time, the general appearance of the stratification and composition of the rocks forming the Willela range is so very different from that of any portion of the Hawkesbury series that, while I admit that its geographical position and evidently eolian character serve to illustrate, and to a certain extent to corroborate the views which Mr. Tenison-Woods has so vigorously maintained, I am nevertheless bound to fix my attention upon the points of dissimilarity rather than on those of resemblance; and to infer that different modes of deposition have been followed in the two cases. If the Willela range is of entirely aerial origin, as seems certain, then it also seems probable that the Blue Mountain beds are not.

I regret that, for reasons already stated, I am obliged to defer the further statement of my own opinion upon this very interesting subject to a future opportunity. Meanwhile I should desire to express, in as emphatic a manner as possible, my sense of the great obligation under which Mr. Tenison-Woods has laid all our geologists, even though his views may not in all respects meet with unqualified approbation or assent. It is a great thing to have made the first sketch: details and corrections will be added from time to time.

Professor Liversidge said:—I am sorry to say I have not prepared any written criticism upon Mr. Tenison-Woods' paper, and that, in consequence, as compared with the previous speakers, I am

very much in the position of a guest unprovided with a wedding garment; but I made a few notes at the time Mr. Tenison-Woods was reading his paper, on the copy he kindly placed at my disposal, and I may now, perhaps, be allowed to refer to them. I think I was appealed to by Mr. Tenison-Woods in one or two cases, and perhaps it will be best for me to comment upon those matters in the first instance.

If I recollect aright, Mr. Tenison-Woods appealed to me in reference to the composition of the cementing material of the Hawkesbury sandstones. There is, I think, no doubt that for the most part this material is of a felspathic nature. Even on the most superficial examination the sandstone is at once seen to be made up of more or less rounded grains of sand, upon many of which a crystalline structure has been developed by metamorphic action, cemented together by a felspathic paste; in addition, scales of mica are usually visible, and smaller quantities of less common minerals. This sandstone has been probably derived from the disintegration of a granite or similar rock; the grains of sand represent the quartz, and the felspathic cement the felspar of the orignal rock; the mica scales, being light and more easily decomposed, have for the most part disappeared; some of the rarer minerals present in the sandstone were also derived from the original granitoid rock, but others have doubtless been formed in it subsequently.

Then there was a question as to the presence of hyalite—a hydrated form of silica. I am not quite satisfied that hyalite is present in quantity. The impossibility of obtaining a good section of the sandstone renders it very difficult in some cases to say whether the fragments are particles of crystallized quartz or particles of the non-crystallized hyalite. When you can prepare a good section of a rock for the microscope, the use of polarized light will generally enable you to distinguish between the two, but the sandstone is far too friable to permit of this; accordingly I am not satisfied that "hydrated silica has acted as a cement between the particles," as stated by the author of this paper.

The next question was as to the origin of the masses and layer of ironstone in the Hawkesbury rocks. There can, I think, be no doubt that the theory first put forth by Gustav Bischof, and now suggested by Mr. Tenison-Woods as an explanation of the presence of the oxide of iron in these rocks, sufficiently accounts both for the presence of, and for the peculiarities presented by, much of the ironstone, but not for all. Probably some of the larger horizontal bands or layers have been formed much as we see bog iron ore deposits accumulating at the present day. The larger veins have perhaps been formed by infiltration, but the smaller irregular veins, and the nodular concretionary masses, have probably been formed in a somewhat different way. In the first instance, it may be assumed that the oxide of iron was fairly uniformly diffused throughout the rock, but has since gradually segregated together until it has formed a compact mass, vein, or layer. It seems to be an order of nature for certain like particles to collect together. In many cases you will see a nucleus of brown hematite in this sandstone surrounded by concentric bands of a brown colour (also oxide of iron), which get fainter as the distance increases from the central nucleus; and I think that in most cases, certainly, there is no doubt that this oxide of iron was originally uniformly distributed throughout the mass of the rock. The calcareous and other concretions found in clays and shales often afford striking instances of the tendency of certain substances to separate out from the materials through which they are diffused and collect together to form nodules and veins.

I do not agree with Mr. Tenison-Woods that there is no evidence of upheaval; it is true that the beds have not been tilted to any extent, but if the rocks were deposited under water, then there must have been upheaval to account for their present great elevation in many parts. My own idea as to the origin of these Hawkesbury rocks has hitherto been that they had been deposited by water, and had since been elevated or upheaved; but now that I have heard Mr. Woods' paper I candidly confess that I should like to study them afresh, to turn aside all prejudices, and start *de novo*. I regarded them as of fresh-water origin. Of course, if they should prove to be of eolian origin, then there is the same necessity to assume that upheaval has taken place. Mr. Tenison-Woods states that the area occupied by these rocks is far too great for them to be of fresh-water origin; but I do not think that there is very much importance to be attached to this objection, since the area covered by them is not very many times greater than that now occupied by the Caspian Sea, the water of which is almost fresh, or by the Canadian and North American lakes, all of which are forming fresh water or lacustrine deposits of large extent.

It is stated, too, that aerial or wind-blown rocks are characterized by certain peculiarities, and that "they are most of all distinguished by large irregular undulating layers, which are also subdivided by laminæ, with every kind of dip and direction, rarely exceeding 23°. Now I am prepared to maintain that this structure only belongs to eolian rocks, and is never found in any other." I think this statement is open to discussion. The peculiar "herring-bone" lamination is not, I think, confined to eolian sandstones; a similar structure is not at all uncommon in the sandstones of the English Coal Measures and in the Greensand beds of the Cretaceous rocks. Nearly all writers upon geology refer to instances of false bedding of this particular kind in sedimentary rocks, and figures of such structure are given by De la Beche, Lyell, and others.

I agree with Mr. Tenison-Woods that the evidence as to ice action in the Hawkesbury rocks is not at present sufficient to warrant us in attributing the presence of the shale boulders to its agency. It is true that I have not made a special study of glacial deposits, but I have examined many of them of various kinds in the old country, and I have seen nothing here resembling them. The mere presence of angular masses and fragments of shale is in itself not sufficient evidence of either ground ice or other form of glacial action. I think it is probable that by some agency the beds of shale have been undermined, whether by running water or the action of the weather, and that the talus of broken-off angular fragments has become covered up with sand, and since consolidated. The screes or accumulations of rock fragments which form at the base of cliffs, both inland and along the coast, are either ground down into rounded pebbles, giving rise to conglomerates and sands, or they may be covered up without losing their angularity of form, and the latter appears to have been the case in this instance. Subsequent investigation may however bring to light indisputable signs of ice agency.

Mr. Tenison-Woods speaks of the consolidation of the loose sand into a solid rock by the mere dead weight and pressure of the sand above. Now I have no objection to take against this at all, for the effects of thousands of tons of pressure should have a very great deal to do in bringing about the consolidation of a mass of loose and porous sand, but I think that the cementing material has played a much more important part. It has long been a very interesting question to me, but one which I have not yet had an opportunity to tackle, whether there is any appreciable difference between the specific gravity of a rock taken from the surface and of another portion of the same rock taken from a depth, *i.e.*, whether the deep-seated portions of a mass of rock have undergone greater consolidation from pressure than the superficial layers. At first sight the question looks a simple one to settle, but I do not think that it would so prove, for many matters would have to be taken into consideration. I merely throw this out as a suggestion, with the hope that some one may take it up.

In speaking of the Stony Deserts, Mr. Tenison-Woods attributes their formation to the fact that certain portions of the loose sand have been consolidated, and that the lighter uncemented portions were drifted away by the wind, leaving a mass of stones behind; on this layer being again drifted over "we should have a wind-blown sandstone rock at top and a heavy rounded conglomerate at the bottom. It is thus I offer to explain the widespread conglomerates which we find lying on the coal formation, with very little change of character over thousands of square miles." But I do not think this would account for the very heterogeneous character of the pebbles composing these conglomerates; it would not account for the presence of pebbles of jasper, vein quartz, slate, and of numerous other materials such as we find in these conglomerates. If they had been derived in the way suggested, then, I think, they would be almost entirely composed of quartzite, or of ferruginous and sandstone pebbles, *i.e.*, they would practically be composed of the one material; in other words, of sand in a more or less hardened form mixed with ironstone; in fact, they would resemble in composition the masses of consolidated sand found lying on the surface of the ground in Wiltshire, and known locally as *greywethers*, from their fancied resemblance to sheep at a distance; or Sarcen—*i.e.*, Saracen stones—from the old idea that they had been brought over by the Saracens. The huge blocks of stone of which the Druidical temple of Stonehenge is built consist merely of masses of sand converted into quarzite and afterwards set free by the removal of the loose Eocene sand by which they were originally surrounded.

In summing up, Mr. Tenison-Woods states that "the precipitous cliffs of the Blue Mountains are the hard central cores of sand-hills, the loose portions of which have been easily blown or washed away." I think that there are a great many difficulties in the way of this explanation. The Hawkesbury sandstone seems to have been fairly uniformly deposited over its whole area, and I still think that the mountains are mountains, because the matter which once filled up the valleys and connected cliff with cliff has since been scooped out by the action of the weather and running water. But, as I have said before, I wish to again examine these rocks with the new light which Mr. Tenison-Woods has thrown upon the subject of their probable origin.

Before concluding, I should like to state how much gratification I have derived from Mr. Tenison-Woods' paper, and to express how deeply we are all indebted to him for having drawn attention to the subject in such an able way. It is a most valuable and suggestive paper, and I hope it may prove to be the commencement of a new era in geological work in connection with this Society. It is certainly one of the most interesting which has been brought before the Society upon geological matters for a long time, and bears the impress of hard continuous labour and investigation. There is nothing to equal a good, healthy discussion for elucidating questions of this kind.

The Rev. J. E. Tenison-Woods said:—In rising to reply to the objections which have been made to my paper, let me first congratulate the Society on the spirit in which the discussion has been carried on. As I am convinced of the truth of what I hold to be the origin of these rocks, I am sure that a full discussion of the question will serve to elucidate the facts, and render the explanation I have to make more apparent as the correct one. I shall take the objections *seriatim*. First, I do not consider that

all those of Mr. Wilkinson touch my argument. He says that "the parallelism of the main lines of stratification is a prevailing feature in the Hawkesbury formation, and this alone is evidence of their having been formed under water." I answer that the parallelism is neither more nor less than is seen in sand-blown formations. The real question is this: Do these sandstones correspond in every particular with exposed sections of aerial sands? This I have answered by showing from many actual instances that they do, and Professor Stephens has supplied other instances, equally convincing. "You cannot but be struck," says Mr. Wilkinson, "with the undulating and hilly character of these blown areas"; and he goes on to prove that nothing of the kind is seen in the Hawkesbury rocks. But I maintain that the undulating character is a conspicuous feature in the formation. Let any one look down into the valley from Piddington's Hill and see whether the whole contour of the ridges and ranges are not strongly suggestive of aerial sandhills. Why, what could be more undulating than the gorges and gullies of the whole mountain system? and though on the whole the greater layers are horizontal, as seen in large masses, they are clearly undulating when examined in detail. In fine, the external contour and the internal stratification is that exactly of all the aerial sandhills I have examined.

I think that I have not been quite understood about the absence of upheaval. I have stated that these beds are found just in the way they have been deposited by the wind, and that they have not been upheaved by the sea. Now, though horizontality may be no argument in small areas, yet when we trace the same thing over an immense territory, and see no tilting or inclination, then the evidence of non-upheaval is strong. In South Australia, for instance, we have the marine Miocene formation, which at about 90 miles from the sea is about 270 feet above the sea-level. Now, a fall of 3 feet in a mile is utterly inappreciable in a section, but can readily be traced over long distances. But here, at less than 50 miles from the sea, can we trace any tilting inclination, though the beds must have been raised at the very least some 4,000 or 5,000 feet? Observe, also, that it is not a question of the Blue Mountains merely. The same formation, or a very similar one, is found scattered over the whole continent. But whether we find it far away to the westward, on the summits of mountains, or on the east side of the divide and close to the sea, it is always the same, with no tilting or inclination, but just as it was deposited in its peculiar undulating layers and laminated false-bedding. When we add to this that the structure is that of wind-blown rocks the argument is very convincing. To say that the whole continent has been uplifted in one mass without any break or tilting is rather an extreme hypothesis. But these beds may have been fresh-water, it is objected. But the fossils are not fresh-water fossils. The ferns are land ferns. *Thinnfeldia* is a land fern, so is *Gleichenia*, and so are all the ferns I have met. The few water ferns that are known in existence are so peculiar that a very little experience would distinguish them. We have none of these in the sandstone. We ought also to have fluviatile shells or other fresh-water remains, but we find none except two species of fish, which are rarely found in what I readily admit may have been lagoons or creeks in this formation. Then again, if it be objected that land plants might easily be drifted down by rivers, I should admit such an explanation if we found them associated with other fluviatile remains, but there are no such things to be found. Again, if the ferns drifted long in the water they would soon rot and be broken up. How different is the state of those beautiful fronds found preserved in the sandstone at Mount Piddington and Dubbo. It seems to me easy to understand it if we attribute their preservation to an advancing drift of sand which covered them over and entombed them just as they grew.

Mr. Wilkinson objects to my interpretation of false-bedding, lamination, &c., because he thinks that the same structure is found in other rocks which are clearly aqueous. Some of the instances which he alleges are not cases in point, because it is not certain that the beds are aqueous. I am inclined to attribute to some of the Victorian sandstones an origin like our own. The use of Mr. Selwyn's name for opinions as to the nature of rocks would have great weight if we knew that the present question was ever fairly put before him. It is a comparatively new one in geology, and therefore the names of Dana, Jukes, and others must require the support of their reasons for any opinion which they give before they can command our assent. At present, I still maintain the proposition that the peculiar character of the stratification in the Hawkesbury rocks can only arise from eolian origin, and I must see strong reasons for abandoning this opinion. Mr. Wilkinson says, "Mr. Tenison-Woods' conclusions Nos. 4 and 5 refer to the origin of the ironstones. I do not consider that the irregular bands mentioned represent old land surfaces, for the bands not only curve in all positions but are sometimes vertical. They may be well seen in the cuttings along the Great Western Railway between Penrith and Lithgow. Most of them have been formed from the oxidation of water containing iron in solution permeating the sandstones and shales and the joints traversing them." In this Mr. Wilkinson overlooks the fact that I admitted the stains and vertical fissures to have been caused by infiltration. But I am sure he forgets how this oxidation is explained by all chemists. His words, "oxidation of water containing iron in solution," are obscure as an explanation. I repeat that water alone will not

dissolve peroxide of iron. It must have the aid of decomposing organic matter. The only way in which this can be explained is by the surface vegetation. In this matter the conclusions are not mine, but are received by all chemical geologists, from Bischof to Sterry Hunt. In reply to the objections against one of my explanations for conglomerates, I must repeat that these things are rare in the formation, and the pebbles are of small size for the most part. Mr. Wilkinson says that "it is impossible that the conglomerates in the Hawkesbury series could have been derived in the manner just mentioned. To satisfy this theory the conglomerates must occur at the base." But why? May not a part of a sandhill be blown away by small degrees, leaving all the heavier pebbles behind on the surface as a thick layer to be subsequently covered up by new layers of drift sand?

I thought I should have had the concurrence of my friend, Mr. Wilkinson, with regard to the change of drainage following the outpouring of tertiary volcanic lavas on the summits of the divide. He admits, however, that the old channels were often filled up by these igneous outpourings, and a new system formed. That a higher watershed was formed is not to be denied. That a change of climate was probably the result is not, I think, a far-fetched inference. The whole of the igneous table-lands in New England —sometimes 4,000 feet above the sea—have originated in the period I refer to, and any one must see what an important influence this has had in effecting climatial changes. It is one of the causes which I suggest may have given the desert area of the Blue Mountains a more humid climate, and thus encouraged a vegetation by which the shifting sands were permanently moored. To the rest of Mr. Wilkinson's objections, as they are more matters of opinion than facts in dispute between us, I shall not refer more particularly. The ice theory and the drift theory are now both before the world, with the observations by which they are supported, and they must now rest upon their own merits.

I think on the whole that I must thank Professor Stephens for the support he has given to my views in this matter. On one or two points he has misunderstood me. One is with regard to the "Challenger's" dredgings and the azoic regions of the deep. If I said that these were not destitute of signs of life, I meant life which was doubtless derived from the surface. Thus, in what is called the Globigerina ooze, there were abundant traces of foraminifera. Now, these organisms have existed in all seas from the earliest or nearly the earliest geological periods, and some of the species have come to us from very remote antiquity. They are found in all seas; they are also very easily preserved in rocks. I cannot imagine any marine remains destitute of such organisms, unless they belong to the early paleozoic rocks. There is no question that these are often azoic, as Professor Stephens states, but they are hardly cases in point. Even the Caspian Sea has its peculiar mollusca, and the Dead Sea, which supports but little marine life and no mollusca, has its shores strewn with fresh-water shells brought down by the waters of the Jordan. It is said that there may have been fossils, but they have been carried away by the infiltration of waters. Now, whenever such a thing takes place we have either the casts of the shells remaining empty or filled with other material, or we have a disturbance of the strata by the filling in of the spaces occupied by fossils. The latter case is almost unknown in geology. But would it not be a strange thing to find land plants and other delicate vegetable organisms perfectly preserved, and every other fossil so completely swept away as to leave not a trace behind? I agree with Professor Stephens, that a case may occur in which rounded grains of sand, in certain cases, may be found in a formation of aqueous origin, having been originally derived from the weathering of an aerial rock; yet the case would be an extreme one. Consolidated sandstones hardly ever weather into their original grains, as I have explained in my paper. But the evidence in this case is cumulative; the round grains and a wind-blown structure to the rocks are found together. The connected origin of both is the reasonable explanation.

In the matter of the dip of the laminations, I am not quite sure that I understand Professor Stephens; but if we mutually explained our views I think we should find our observations to agree in most particulars. Practically, the dip is not always at right angles to the strike, that is, when the dip is quaquaversal, as it is very commonly in rounded accumulations of sand. In this case, a section diagonal to the axis will give almost every angle to the laminations. That the variation is due to this in many instances I have no doubt; but I also think that another cause had been in operation, and that is the variation in the strength of the current, aerial or otherwise, by which these sands have been deposited. But the question does not seem to affect my theory to a great extent. What my learned friend says of the quartz pebbles is somewhat new to me, and, as he says, the formation must be rare in the Hawkesbury rocks. It has always been a puzzle to me why the pebbles of quartz are so little abraded. But this would be a greater objection to the aqueous theory; for, be it remembered, though water does not affect the outline of the finest particles of sand by abrasion, it is quite the contrary with large fragments of half an inch and upwards; these rapidly assume a rounded outline in running water. I have related Daubrée's experiments on this subject. There is another test in this matter which is familiar to everyone, and that is the generally ovoid shape of water-worn stones. We need not be surprised at the angular character of pebbles (especially of the

harder felspars or quartz), even though the surface has been polished by the action of blown sand. Through the kindness of Professor Stephens, I am enabled to place before the members of the Society to-night some specimens of granite and felspar from the first cataract of the Nile. These are continually exposed to the action of blown sand from the desert, and the members can judge of the effect of this from actual inspection. It can be seen that the constant impact of blown sand has given the stone a most brilliant polish, but at the same time not a single angle has been worn away. If the pebbles found in the Hawkesbury rocks do not bear a more evident polish, the cause must be looked for in their long entombment. But the facts remain that the surfaces of the pebbles are generally abraded, not rounded as they would be were the action that of running water. The exceptions may well be due to creeks, as they are so uncommon.

With regard to Professor Liversidge's remarks, I have first to thank him for the information he has afforded us on those matters about which I especially appealed to him. I have not expressed myself decidedly about the hyalite, though I threw it out as a suggestion that polarized light gives a good test for its detection. But we must not expect very great results from this method, because if the original grains were derived from granite, some of the quartz from that rock presents under the Nichol prisms the play of colours observed in colloid silica. In the beginning of my microscopic work in this matter, I was inclined to think the polarized light and the selenite plate would give me definite results; but when I varied the experiments, using rock crystal artificially pulverized, and various kinds of felspar, with true hyalite, the results were conflicting. I am going to try again at getting thin sections of the rock, and then I am in hopes that the grains in the cementing medium may be better seen. My friend, Mr. Wilkinson, does not believe that the grains of sand will afford any clue; yet I may state a fact which will be significant in the matter. Since my paper was read, Professor Liversidge wished to bring my theory to the test by the microscopic examination of sands. With this view he asked my opinion on about a dozen slides of dry sand, the origin or locality of which was entirely unknown to me. In every instance, except one, I was able to state, after a short examination, whether the sands were river, desert, or aerial sands.

I think that in the matter of conglomerates I went out of my way to explain a feature which, whether explained or not, does not affect my theory. Still, I differ from Professor Liversidge as to what concretions may do in forming pebbles in such a formation as this. It must be remembered that we have here silica, alumina, both oxides of iron, with smaller quantities of magnesia, potash, soda, and occasionally sulphur and manganese. The colours and forms of silicate of alumina and iron alone are endless, and those when worn down into pebbles would give almost every colour and appearance. Besides, before these beds were finally consolidated, the trap rocks began to have their influence. We must not forget how rapidly sandhills are cut down and form again by the wind. Thus a conglomerate in the middle of a sandstone may belong to a late portion of its history, and its pebbles belong to a trap rock now separated from it by many feet of sandstone. However, as Professor Liversidge says that he prefers to approach the subject cautiously and will carefully weigh my observations in a review of the formation, I can leave the facts to the painstaking and impartial examination which I know he and others will give them. To him and to Professor Stephens and Mr. Wilkinson my best thanks are due. What has been so kindly said as to the value of this paper I have heard with gratification, for the sake of this Society. The subject has been a labour of love to me, but if it has also been useful to the Society I am more than amply repaid.

Mr. Wilkinson exhibited a large number of maps, diagrams, and specimens in illustration of his views. Professor Liversidge showed a series of slides of eolian and water-formed sands and sandstones.

The President conveyed the thanks of the Society to the Rev. J. E. Tenison-Woods for his valuable paper.