the proper direction, and this may account for its great development in Trilobites. The small size of the masticatory appendages, as in Phyllopods, would also support this view.¹

The pygidium may have been of considerable assistance as a tail fin, and its increased size in post-Cambrian times would thus be accounted for.

This comparison does not claim, however, that the Trilobites have near affinities with the Phyllopoda, only that Trilobites had a Phyllopodan mode of life. The Trilobites are the most primitive Crustaceans known, the Phyllopoda are the most primitive Entomostraca. Nearer than this we cannot at present go.

IV.—SOME FURTHER REMARKS ON GRANITE: A REPLY.

By Lieut.-General C. A. McMAHON, F.R.S., F.G.S.

AFTER the expiration of twelve months, Mr. A. R. Hunt, who was present at the meeting of the British Association at Belfast, has published in your September number a criticism on my address. His paper deals with a variety of matters with which I need not concern myself. His remarks on tourmaline have reference to another author, and not to anything I have written on that subject. He also passes strictures on what he calls "incidental geology," but as he includes Lord Kelvin and Canon Bonney with me in his condemnation, I can only congratulate myself on being in such good company.

In the following observations I shall confine myself to the criticisms on my Belfast address, and as Mr. Hunt tells us (p. 403) that the whole object of his paper "is to discuss General McMahon's incidental remark—'the beryl is crowded with liquid and gas cavities, the former containing movable bubbles and deposited crystals as well as water,'" I shall begin by considering the author's remarks under this head.

Before proceeding further it will be as well to correct an inaccuracy into which the author has fallen. On p. 393 he quotes me as stating that "beryl was the first [mineral] to crystallise, at a temperature approaching 1,200° C." This is a mistake. I certainly said that beryl was the first mineral to crystallise out of the magma, but I nowhere ventured to give the temperature at which that or any other mineral of the granite began to crystallise. I do not think we possess at present sufficient data to enable us to give with any approach to accuracy the temperatures at which the several minerals contained in a granite crystallise, and I did not attempt this task. I was dealing in this portion of my address with contact metamorphism, and my object being to impress upon my hearers the potential energy of a granite in a molten or fluid condition, I thought it desirable to consider "the probable temperature reached" by the Satelej granite.

After considering the melting-points of various granitic minerals, and after stating that the case was complicated by the fact that

¹ Bernard, T.: "The Apodidae," 1892, Macmillan & Co. Quart. Journ. Geol. Soc., L (1894).

the presence of water at high temperature and alkali lowers, and pressure raises, the melting-point, I said that "if we consider the melting-points of the mineral constituents of granite we can hardly avoid the conclusion that for the magma to have attained perfect fluidity it must have reached a temperature of at least $1,200^{\circ}\text{C}.$ " I went on to consider, in order to strengthen the above view, Vernadsky's conclusions that kyanite is transformed into sillimanite, a well-known product of contact metamorphism, at a temperature of $1,320^{\circ}$ to $1,380^{\circ}\text{C}.$, and referred to Mr. George Barrow's observations in the Highlands of Scotland on the sillimanite zone between the kyanite and the granite. I also drew attention to his conclusions that the granite that had produced this contact metamorphism must have reached a higher temperature than that given by Vernadsky. $1,260^{\circ}\text{C}.$, it will be observed, was the estimate I gave of the temperature probably reached by the Satej granite when in a state of aqueo-igneous fusion. The question of the temperature to which the magma had fallen before the minerals began to crystallise out, is a totally different one. I did not venture to estimate this temperature in degrees of the Centigrade thermometer, but said generally that the magma must have been above the red-heat. There is a wide margin between $1,200^{\circ}\text{C}.$ and $550^{\circ}\text{C}.$ (red-heat). When I said that beryl was the first of the granitic minerals to crystallise, I did not imply that it began to crystallise at a temperature of $1,200^{\circ}\text{C}.$ All that I affirmed regarding it was that it began to crystallise before the mica, feldspar, and quartz which formed upon it, and whilst the magma was sufficiently fluid and mobile to allow the mineral to crystallise in its proper crystallographic form, without being "interfered with or molested by other solid minerals," "during the entire period of [its] crystallisation." Mr. Hunt apparently wishes to detract from the force of the evidence referred to above, namely, of the mica, feldspar, and quartz having crystallised upon the beryl, by remarking that muscovite and quartz are minerals that are found in schists, and that they are sometimes formed at low temperatures, and he cites Fouqué & Levy to show that quartz may be formed at a temperature of $200^{\circ}\text{C}.$ This argument, however, does not apply to the Satej granite described by me.

All petrologists are aware that some silvery micas and quartz can be formed in what Bischof terms the 'wet way,' at comparatively low temperature, but it is material to note that the Satej granite is not a metamorphic, but is an undoubtedly igneous eruptive rock. As described in my paper, it cuts boldly across the Tertiary gneissose granite of the Himalayas. The intruder, though probably also a rock of Tertiary age, is of considerably later date than the foliated rock invaded by it. It is not only the latest intrusive rock in that part of the Himalayas, but it must have been erupted after the strains and stresses of earth-movements had ceased to operate on the rocks in that locality, for it does not itself contain the slightest appearance of foliation.

In my address I stated that the temperature of the Satej granite "must have been above that of red heat," and I went on to remark

that "the potential energy of water held in a fluid state by pressure must have been great. When, therefore, in the course of the earth-movements which accompany or in some cases are caused by the intrusion of eruptive igneous masses, pressure was temporarily relieved by the rupture and faulting of rocks, the superheated water contained in the magma was ready to flash into steam with almost explosive violence."

Mr. Hunt calls attention to the expressions used in the above passage that water was held in a fluid state by pressure, and he also refers to expressions used in other passages in my address, where I speak of water at a red heat, and considers that the views indicated by these expressions are not only inconsistent with the old physical theory of granite, but entirely subversive of them. I will take the first remark objected to into consideration before proceeding to the other.

In his previous paper on vein-quartz (*GEOL. MAG.*, May, 1903, p. 212) Mr. Hunt reminds us that Dr. Sorby in 1876 estimated the critical temperature of water to be 412°C ., but that Professor Hartley in 1877 estimated it at 342°C ., which latter figure Mr. Hunt accepts as the true one. From Mr. Hunt's own showing, therefore, a margin of at least 242°C . exists between the boiling-point of water, 100°C ., and the critical temperature, 342°C . It follows, then, that throughout a range of 242° of Centigrade temperature water must be held in a fluid state in the molten magma by pressure, so that for this range of temperature, at all events, the expression used by me is strictly and literally true. As regards the phrase water "at or above a red heat," I was following so good an authority as Scrope, who, as quoted in my address, speaks of "red-hot water or steam in a state of extreme condensation and consequent tension." When I spoke of water being at or above a red heat (550°C .) I thought experts would understand that I had water in a gaseous state in my mind. For the purposes of my argument the clearer the fact was realized by my hearers that we were considering water in its gaseous state, the better for my purpose, which was to impress upon them the potential energy and the kinetic motion exercised by water at this heat.

At the commencement of my address I warned my hearers that, the time at my disposal being brief, I would use great simplicity of language, free from technicalities. I wished to avoid, therefore, a dissertation on such elementary topics as the critical temperature of water, and of water existing in three states as a solid, a liquid, and a gas, and to confine the attention of my audience to the consideration of the potency and energy of a molten granite intruded into other rocks, and thought that any digression to consider objects outside the central idea of my paper would have put it a little out of focus.

If an artist has painted a landscape with the intention of drawing attention to the beauty and poetry of Nature as exhibited by various forms of rocks and foliage with fleeting lights and shadows, it would ruin the broad effect of his picture to introduce into his

foreground, as I have sometimes seen, the figure of a sportsman firing at a duck or pheasant, and the bird falling to the ground with a broken wing and with feathers scattered in the air. The majority of those who examine such a picture would altogether overlook the beauty and poetry of the landscape, and would carry away the idea that the object of the artist was to illustrate the cruelty of sport. I think that some scientific writers labour under a want of the artistic sense. They leave their meaning obscure, not only by the want of lucidity of mind, but by scattering the attention of their readers, by diverting them to subjects beside the real point at issue. To prevent any further misapprehension I may mention that I abstained for a similar reason from entering into the question of what is implied by water being held in solution by granite.

Ostwald, in his remarkable work on solutions, has enlarged upon the theory that a substance in solution is split into its ions. This theory has, I think, been accepted as a good working hypothesis by the majority of chemists, as it clears up many points not otherwise capable of explanation. According to this theory water held in solution by a molten granite would be split into its ions, hydrogen and oxygen, and questions regarding the critical point of water would be held in abeyance during the time it was so held in solution. As, however, I did not, for the sake of simplicity, treat the subject in my address from the ion point of view, I will not attempt to do so now, but shall be content to regard the water in a granite above the critical temperature, as water in its gaseous state, though it must be remembered that if we regard it as a gas we must accede to it the kinetic energy and motion proper to a gas at a high temperature.

Mr. Hunt in his paper has come to the conclusion that "ordinary granites crystallised about the critical temperature of water," and in his paper on vein-quartz above referred to he records his conviction at p. 214 that "Fluid inclusions with deposited crystals are clear proof that the fluid was entangled in the quartz or other mineral, under 342° C." This conclusion or rule appears to me to be based on two assumptions, both of which I think erroneous. The first is that water enclosed in the minerals of an igneous rock was enclosed after it had assumed a fluid state. The second follows naturally from this, namely, that the mineral which enclosed the fluid water must have crystallised below the critical temperature. The main fallacy which underlies the above rule seems to me to have arisen from Mr. Hunt's failure to realise the possibility of molecules, on their coming together to form a crystal, bringing down entangled with them the molecules of a gas. My critic's conclusion is irreconcilable with the facts stated in my Belfast address, hence possibly his anxiety to discredit my facts under cover of an attack on the views expressed by me.

I have been led by the evidence supplied me by the study of thin slices to believe that the crystallisation of minerals in a plutonic rock, such as granite, was a slow and gradual process. I do not see how we can avoid this conclusion in the case of large

porphyritic crystals of felspar, which in some places on Dartmoor attain a length of from three to four inches. I also referred to zonal crystals as evidence of slow and gradual crystallisation, and I specially referred to zonal felspars which show a progressive change of species, from a more basic one at the heart of a crystal to a more acid one at its periphery. I explained this by referring to a law of general, though not of universal application, that the more basic minerals crystallised first, and that in a granite there is a progressive silicification of the magma as the basic minerals crystallise out, until at length nothing but free quartz remains. It is obvious that the gradual silicification of a plutonic rock must have been a process that extended over a considerable period of time, and in the case of a cooling granite there must have been a fall of temperature during this period. The crystallisation of a granitic mineral may therefore have begun at one temperature and have concluded at a much lower temperature. This is the more probable, as recent experiments on the melting-point of minerals appears to show that a period of plasticity precedes the development of rigidity in some minerals.

In the case of the beryl of the Satlej granite I certainly considered that its crystallisation had been a slow and gradual process. I showed that it was the first mineral to crystallise, and did so before the mica, the felspar, and the quartz had begun to form upon it. It seemed clear from the idiomorphic character of the beryl that the magma when it began to crystallise out was in a fluid condition, and I thought, as stated in my address, that a study of "thin slices of it under the microscope ought to give us a clue to the condition of the magma at the time the beryl was formed." The beryl contained, as I found by its microscopic study, not only inclusions of water, but inclusions of a gas. The evidence therefore seems to show that if the molecules of beryl entangled and brought down with them a gas or gases, there was no reason why they should not have brought down water in the gaseous state. That a molten magma at the heat supposed, rendered fluid by aqueo-igneous fusion, must have been in a perfectly mobile condition, permitting of the free motion of its constituents and an intermixture of its chemical elements, goes without saying.

The evidence submitted to the hearers of my address proves, I contend, that when the atoms of the chemical substances that constitute beryl came together to form molecules, and moved towards each other in the act of crystallisation, they entangled and brought down gases with them. There is nothing inherently improbable in this conclusion.

Those who have tried their hand at chemical analysis will understand how difficult it is to prevent a precipitate thrown down by chemical action from bringing down with it other substances which the chemist is anxious to retain in solution, repeated re-solution and re-precipitation being necessary in some cases where exact results are required. The power possessed by some metals at a high temperature of 'occluding' or absorbing hydrogen gas affords another highly suggestive illustration of molecular entanglement.

Thus Palladium, which possesses this power in a higher degree than any other metal, can absorb at a red-heat no less than 935 times its volume of hydrogen.¹ Doubtless hydrogen is not the only gas subject to occlusion. The discoveries in our own time regarding Helium, Argon, Uranium, and Radium have also shown us that chemical elements may long remain buried without their presence being suspected, as in the case of Argon, the presence of which in atmospheric air remained unsuspected until a few years ago.

The case of the gas Helium is also highly suggestive. The rare mineral Cleveite is the one with which Helium was at first associated, but astronomers² affirm that it is rather abundant in the sun and in many stars. The probability is, therefore, that it will hereafter be found in connection with other terrestrial minerals. The chemists of the future will no doubt discover buried in many rocks substances of which the ordinary chemical analysis has up to the present time taken little or no cognisance.

With these ideas running in my mind, I find no difficulty in believing that when beryl began to crystallise out of the magma of the Satlej granite its molecules should have entangled and brought down with them molecules of gas. The subsequent progress of the mineral seems to have been as follows:—The molecules came together above the critical temperature, bringing down entangled with them two or more gases. As cooling progressed and the temperature fell, the molecules of gas and the molecules of water in a gaseous state were enclosed in the mineral.

As the temperature still further fell the water passed from its gaseous condition to a fluid state, and as the rigidity of the beryl increased, the enclosures of water and gas were more and more compressed, until the dimensions of the cavities reached what we now see them to be. As the water thus shut up gradually lost its heat, the mineral matter, which had been held in solution by the heated water, was deposited. That this progressive development actually took place is, I think, proved by the evidence which shows that the beryl began to crystallise when the granite magma was still fluid, and that the beryl now contains inclusions of gas and of water with deposited minerals. That the granitic magma, when in a fluid condition, must have been above a red heat, is, I think, obvious.

When I visited Vesuvius in the Spring of 1885 I descended into the then existing crater, and approached its active centre as near as was safe. I then saw with my own eyes that the lava on which I stood was still red-hot immediately below its scoriaceous surface. If lava, after it has been poured out on the surface of the earth, still retains a red heat, we seem forced to the belief that an unconsolidated granitic magma buried at plutonic depths in the bowels of the earth must be in a still more heated condition.

Mr. Hunt, in his September paper, dwells at some length on volcanic conditions, and points to the case of Krakatoa as an illustration of sea-water having gained access to the roots of

¹ Roscoe & Schorlemmer's *Treatise on Chemistry*, new edition, 1894, p. 137.

² Miss Clerke's "Problems in Astrophysics," 1903.

a volcano. But the case of an active volcano in the immediate vicinity of the sea is very different from that of a heated magma of a plutonic rock like granite, buried at great depths in the bowels of the earth, far removed from the sea.

The possibility of the access of sea-water to the root of a volcano like Krakatoa may be readily conceded, as this would explain in a reasonable way the tremendous explosion which took place, but Mr. Hunt, in his theory of the crystallisation of rocks, goes much further. He contends that the access of water would liquefy a rock that had consolidated and already cooled considerably at a dry heat (p. 403); and he argues on the same page that the pressure of water at 3,000 fathoms being 9,000 pounds to the square inch, water may have gained access through fissures to highly heated though consolidated rocks, and must have resulted in "liquefaction, reconstruction, recrystallisation, and metamorphosis in almost every variety."

If by these remarks Mr. Hunt contemplates the case of a granitic magma buried at plutonic depths, I doubt if he will find many geologists able to go with him, and still fewer to believe that if sea-water could gain access through open fissures to a granitic magma, it would lead to the quiet liquefaction and recrystallisation of the minerals of the granite.

In my Belfast address I said that Sorby had proved "that the liquid contained in the inclusions in granite is water, and showed that it was caught up during the formation of the crystals, and was not introduced subsequent to the consolidation of the rock." I think, if my critic wishes to convert geologists to his theories about the formation of granite, he should begin by showing that Sorby's facts are erroneous and that his verdict based upon them is unsound.

I have only time, in conclusion, to notice very briefly another adverse criticism by Mr. Hunt. He expresses his disbelief in the permeability of rocks, and adduces as counter evidence the case of inclusions of liquid carbon-dioxide. He has specimens, he tells us, which have been imprisoned since Devonian times, and although they are now contained in thin microscopic sections they have not escaped, though the pressure exerted by liquid carbon-dioxide is 1,155 pounds to the square inch.

When considering the question of the porosity and the permeability of rocks, one adverse case is not sufficient in my opinion to dispose of a good many instances which show that chemical reagents have, as a matter of fact, penetrated into rocks and minerals.

The permeability of one substance by a liquid or gas depends, among other things, on the relative size of the molecules of the invader, as compared with the size of the interspaces between the molecules of the crystal invaded. To take a homely simile, a sieve of small meshes will allow sand to readily pass through them, but will effectually bar the passage of half-crowns. Some crystals, therefore, may be permeable to certain chemical reagents, but not to others. I have heard on good authority that the permeability

of agates is taken practical advantage of by German manufacturers in the preparation of agates for the market, beautiful internal dendritic markings, which add so much to their market value, being produced by soaking the agates in a succession of liquids. In my Belfast address, however, I did not consider the case of the permeability of cold crystals by cold solutions. On the contrary, my main object was to impress upon my hearers the fact "that heat increased the porosity of minerals, facilitated the passage of liquids laden with mineral matter through their pores, and increased the potency of chemical action."

In the course of my remarks I referred to numerous instances in which there was good evidence to show that crystals and rocks had been permeated by chemical reagents, and I need not go into these cases again. I do not see how the schillerisation of minerals or the formation of a pseudomorph of one mineral in the form of another can be accounted for if you do not believe in the permeability of heated rocks and crystals by heated chemical reagents. I foresaw that the idea of cracks and their subsequent obliteration would suggest itself to some minds, and in my Belfast address I said all that I considered necessary in reply to this suggestion.

V.—ON A NEW STEGOCEPHALIAN (*BATRACHOSUCHUS BROWNI*) FROM THE KARROO BEDS OF ARIWAL NORTH, SOUTH AFRICA.

By R. BROOM, M.D., B.Sc., C.M.Z.S.

IN the collection of Mr. Alfred Brown, of Ariwal North, there is a fairly complete skull of a moderate-sized Stegocephalian which differs very considerably from that of any form hitherto described. The specimen is in a sandstone matrix. Owing to a crack produced by weathering practically all the cranial bones adhered to the counter slab when an endeavour was made to display the remains. The sculpturing of the bones is thus hidden, but the sutures are for the most part distinctly seen. Fig. 1 represents a slightly restored view of the upper side of the skull. The most striking features are the great breadth of the skull and the relatively advanced position of the orbits. In these respects it makes a slight approach to the condition found in the American genus *Diplocaulus*. There is no distinct notch in the post-temporal region, as in most Stegocephalians, and there are no 'epiotic' cornua. There are two rudimentary cornua on the posterior cranial border, but they are formed by the so-called 'supra-occipital' bones. In the middle line between the frontals and nasals is a median bone, probably an ethmoid. I fail to detect a supra-temporal element as distinct from the quadrato-jugal, which is of large size.

The under surface of the skull has only been partly cleared of matrix. The occipital region is well ossified and extends far backwards. The condyles are well developed. The most noteworthy feature of the palatal surface is the large size of the pterygoids. These, with the 'parasphenoid,' form more than a third of the under surface of the skull. The pterygoids also form two large descending plates, which lie close to the inner sides of the