best for the present to class the Dyas, neither under Palæozoic nor Neozoic, but as a period of transition between these two great eras in the development of life.

I have kept the old divisions of the Trias, though I admit the desirability of eventually abandoning them, and of adopting the marine sequence of the Alps as a type. By that time, perhaps, a better name for the system may be found, and the Rhaetic may then be included in it. In Britain the Penarth Beds link themselves rather to the Lias than to the Trias, but this may be because our Trias is not marine.

In the Cretaceous system I have degraded *Neocomian* to its proper subsidiary rank as a stage of the Lower division. The use of the term for the whole of this lower series is an unwarrantable and unnecessary innovation, which can only result in confusion. The true Neocomian of Southern France seems to be the marine representative of our Wealden, and no part of it corresponds to our Lower Greensand. This last name and its correlative, Upper Greensand, are certainly doomed to abolition; the Lower Greensand can be studied so well in the Isle of Wight, where the labours of Edward Forbes added so largely to our knowledge of it, that no name can be more appropriate than *Vectian*; and I regard the introduction of a new name as preferable to the adoption of the French names *Aptien* and *Urgonien*. The Gault and Upper Greensand require further investigation, so I leave them for the present.

My reasons for admitting only two systems in the Tertiary, and for giving new names to these, have already been explained. Quaternary, Post-Tertiary, and Post-Pliocene I regard as unnecessary and unscientific terms.

III.—ON THE GEOLOGICAL HISTORY OF THE CORNISH SERPENTINOUS ROCKS.

By J. H. COLLINS, F.G.S.

1.—THE LIZARD SERPENTINES.

THE remarks by Professor Bonney on Dr. Sterry Hunt's recently published "Geological History of Serpentine," which appeared in the September Number of the GEOLOGICAL MAGAZINE, open up a very wide question—a question which broadly divides the non-chemical from the chemical geologists and petrologists. Dr. Sterry Hunt is well able to take care of himself in this discussion, and certainly I have no present desire to take part in it except so far as it relates to the Cornish areas, some of which are therein referred to. These I have especially studied, and with respect to them I feel bound to say that I find myself able entirely to agree with the conclusions of Dr. Sterry Hunt.

One of the regions in question, that of Porthalla, has also been referred to by Mr. Alexander Somervail, in a letter which appeared in the GEOLOGICAL MAGAZINE, 1884, Dec. III. Vol. I. p. 479. This letter, however, brings forward neither new facts nor new arguments—it merely sets my conclusions (see my paper read before the

Geological Society of London)¹ aside by a repetition of the dogmatic assertion that the serpentine here "has forced itself through and among the schists," and "actually infuses itself into their composition at and near their junctions, so as to form a kind of transition between the two rocks of opposite origin, which Mr. Collins has very evidently mistaken for serpentine in a less altered form, thus seeming to favour his mistaken conclusion of the serpentine being only an altered slate."

I may here remark that the expression "the serpentine being only an altered slate" does not quite accurately convey my meaning, although, of course, I find no fault with Mr. Somervail for using it. What I believe is, not that "the slate" has been altered into serpentine, but that, of an originally differing series of conformable bedded rocks, some beds have been converted into that peculiar variety of hornblende schist which characterizes the locality, others have been converted into true serpentine, and others again into a rock of intermediate character. In fact I consider that we have here the final results of a long series of processes of "selective metamorphism," the same agents acting for the same times on dissimilar masses, and so giving rise to dissimilar products. Moreover, as there were originally transition beds of the original strata, so now there are transition results in all stages, so affording complete series of passage specimens, not "apparent" merely, but veritable and real. In all these cases it seems to me likely that a hornblendic mineral has been first formed, and that the conversion of this into serpentine is the final change.

It is, moreover, obvious that such reactions might take place, under suitable circumstances, so as to change a rock mass into serpentine quite irrespective of its mode of origin, whether igneous or aqueous, and one has not to go outside the borders of Cornwall, perhaps not outside the borders of the Lizard district, to find examples of both classes of rocks which have undergone this change.

Speaking of the serpentine more especially of Coverack, Professor Bonney says, in his recent communication (*GEOL. MAG.* 1884, p. 409) : "If there be in Nature any indications from which we can assert the intrusive character of any igneous rock whatever, we have these indications in the case of the Lizard serpentine and the associated sedimentary series."

I by no means deny that this description is fairly enough applicable to the Coverack serpentine, and I do not dispute that this particular serpentine has resulted from the alteration of an igneous intrusive rock, as stated by the Professor. Still, I agree with Dr. Sterry Hunt in thinking that such appearances (of intrusion) may very often, if not usually, be "explained by subsequent movements of the strata in which the serpentines are included" (*op. cit. conclusion*).

Moreover, I assert that the serpentinous beds of Porthalla, Mullion, and the greater part of the Lizard district do not possess the unmistakeable indications alluded to, but, on the contrary, they have the closest possible inter-relations with the hornblende-schists of the same localities, both stratigraphically and chemically.

¹ Quart. Journ. Geol. Soc. 1884, vol. xl. pp. 458-471.

Prof. Bonney has expressed the opinion—and I understand Mr. Somervail to endorse this—that the existence of intrusive serpentine in other parts of the Lizard District would be *prima facie* ground for believing in a like origin for that of Porthalla; and this, in the absence of positive evidence to the contrary, I should be disposed to admit. The main object of my paper on the Porthalla rocks (which are entirely different from those of Coverack and Cadgwith) was to afford such positive evidence; and, speaking as a chemist, I must reiterate my belief that it is impossible for any one to explain satisfactorily the origin of the specimens I exhibited to the Geological Society in illustration of my recent paper, and especially the specimens marked C and D, without admitting that serpentine has been formed *within the substance of the hornblendic stratified rock*. Not only does this latter entirely enclose portions of chemically pure serpentine, but also there are fragments of intermediate composition of every degree visible, some of which I have isolated and analysed. It seems to me, therefore, we must either admit that these have been formed within the substance of the schist, or that they existed as serpentine previous to its formation, and now represent a kind of breccia. I have no doubt that the first hypothesis is the true one.

Prof. Bonney and others have appealed to the evidence of the microscope in disproof of my conclusions. I have not, however, arrived at these conclusions without much use of that instrument, and I still assert that the evidence—fairly interpreted—is in my favour. The massive serpentine of Porthalla does not present that reticulated structure which has been regarded as the result of the alteration of olivine rocks. But even if it did, this would be no proof of its igneous origin, inasmuch as the essentially Neptunian origin of many olivines has been quite conclusively shown by Dr. Sterry Hunt and others.

The transformation of one chemical substance into another by the withdrawal of some of its components and the substitution of others is a process which is continually going on in all rocks which are so situated that natural waters are able to percolate through them. It is well known too that all rocks are—some more, some less—permeable to water. To chemical geologists such changes are matters of every-day observation; yet, for expressing an opinion that such a transformation has taken place, in the case of the Cornish serpentes, my views have been characterized as something like the “transmutation” of the old alchemists (see discussion on my paper.¹)

Such changes have been fully described and traced in the case of serpentine by Dr. Heddle, Dr. Sterry Hunt, and many other writers of repute. Dr. Sterry Hunt states as the result of his own experiments that “whenever the comparatively soluble silicates of alkalies or of lime are brought into contact with solutions like sea-water—holding magnesia sulphate or chloride—double decomposition takes place with the separation of a very insoluble gelatinous silicate of magnesia, and further that precipitated silicate of lime is decomposed by digestion with such magnesian solutions, its lime becoming par-

¹ Abstract of Proceedings Geological Society of London, Q. J. G. S. p. 472, 1884.

tially or wholly replaced by magnesia" (Geol. Hist. of Serpentes, 1873, p. 169). These experiments date from 1860, their accuracy has never, I believe, been disputed, and the hypothesis, as regards the formation of serpentine, "has been adopted by Delesse, Gümbel, Credner, Favre, Dieulefait, and Stapf" (*ibid.* p. 206).

As regards the removal of alumina in solution, which has been thought to be a difficulty, Dr. Heddle says, "Silicate of alumina can be decomposed by chloride of magnesium or sulphate of magnesium, silicate of magnesia being formed" (quoted in Min. Mag. vol. iii. p. 139). I myself have found that sea-water, aided by heat and pressure dissolves out not only lime (when present) and alkalies, but also alumina and a little silica from all varieties of felspar.

The late David Forbes was a firm believer in the dolomitic origin of much of the Lizard serpentine. The following are two of his analyses of serpentinous rocks from a Cornish locality as given by Capt. Marshall-Hall (Min. Mag. vol. iii. p. 213):—

	Dolomitic.		Serpentinic.
Silica	3·43	...	39·48
Oxides of iron and alumina	1·15	...	1·65
Lime	13·37	...	24·14
Magnesia	20·12	...	18·16
Carbonic acid	49·04	...	13·83
Water	12·91	...	4·56
	100·02		101·82

Of course these substances are much farther removed from true serpentines than those ordinarily met with in the Lizard district. The changes indeed are evidently far from complete; they may nevertheless serve in some degree to indicate one mode in which such transformations have been brought about.

The origin of the Porthalla serpentine, as well as that of Mullion and the greater part of the Lizard district has been, I believe, brought about pretty much as follows:—

A series of beds of ancient stratified rocks—including siliceous limestones and dolomites of varying composition and texture, some therefore, more permeable than others—has been contorted and compressed during many ages. At some periods during the long series of changes, they have been subject to the action of magnesian chlorides in solution, at an elevated temperature, and under considerable pressure; submergence in the waters of the sea to a considerable depth would suffice to give all these conditions.

In this way from the more permeable beds silicates of alkalies and of lime together with alumina might be carried away in solution and silicates of magnesia deposited—the oxides of iron present remaining unchanged or nearly so. The less permeable beds perhaps have suffered internal changes of a less complete character—the ultimate chemical composition here may perhaps be pretty much as it was originally, but the components have been rearranged so as to give rise to the peculiar hornblendic and felspathic constituent minerals now present. Of course if there had been any veins of igneous rock previously injected before these chemical agencies had got to

work, these too if they were susceptible of conversion into serpentine might be so converted, and we should then have *intrusive* serpentine as well as interbedded serpentine, not merely in neighbouring localities, but actually in the same locality.

Assuming the intrusive nature of the Coverack rock, it may be that such has been its origin, and such has been certainly the origin of the serpentinous rocks of Clicker Tor, and other localities, to be hereafter referred to. I must here repeat, however, that I have been unable to find any evidence of the existence of intrusive serpentine rock at Porthalla, Mullion, or Kynance.

IV.—SOME SPECULATIONS ON THE PHENOMENA SUGGESTED BY A GEOLOGICAL STUDY OF VESUVIUS AND MONTE SOMMA.

By H. J. JOHNSTON-LAVIS, M.D., F.G.S., etc.

DISREGARDING for convenience the internal volcanic forces of the earth, let us take for granted that we have an unlimited supply of fused silicates at a very high temperature; what will be the changes and effects produced in the escape of such matters upon the earth's surface?

That, at the great sources of volcanic material the igneous mass is in a completely fused state there can hardly exist any doubt. By this is meant that the magma is an intimate mixture of various chemical elements and compounds that have not yet assumed any other than the fluid state, or, in other words, that the molecules move freely amongst each other and have not arranged themselves in any definite crystalline form.

One of the most important questions that first arises, and is not susceptible of complete response, is whether this fused magma contains aqueous matter in "solution"?¹

If we must really regard granite as a primary igneous rock, the presence of water cavities in the quartz might certainly seem to prove the existence of that compound in the original fused magma. We must nevertheless remember that such may have been absorbed from the aquiferous strata in the neighbourhood of the granite if intrusive; or supposing such a rock to be the original earth's crust, the question carries us beyond safe speculation. Even admitting that a small portion of aqueous matter is mixed in the fused magma, it is hardly probable that it is sufficient to account for the large amount of vapour escaping during explosive and even tranquil volcanic activity.

If we take 25 kilometers as the thickness of the earth's solid crust traversed by the igneous matter before its exit, we shall have made but a very moderate estimate. If we make a rough calculation of what amount of material can be contained within a volcanic chimney 25 kilometers long together with its ramifications, with a diameter equal to any of the cores seen in denuded examples of old volcanoes, we shall see that the quantity is very great.

Let us suppose the area of the section of the chimney equal the

¹ By "solution" is meant a condition similar to the solution of CO₂ in water.