there been shown by Daubrée that the elements of the zeolites had been derived in part from the waters, and in part from the mortar, and even the clay of the bricks, which had been attacked, and had entered into combination with the soluble matters of the water to form chabazite. I, however, at the same time pointed out another source of silicated minerals, upon which I had insisted since 1857, viz., the reaction between silicious or argillaceous matters and earthy carbonates in the presence of alkaline solutions. Numerous experiments showed that when solutions of an alkaline carbonate were heated with a mixture of silica and carbonate of magnesia, the alkaline silicate formed acted upon the latter, yielding a silicate of magnesia, and regenerating the alkaline carbonate; which, without entering into permanent combination, was the medium through which the union of the silica and the magnesia was effected. In this way I endeavoured to explain the alteration, in the vicinity of a great intrusive mass of dolerite, of a gray Silurian limestone, which contained, besides a little carbonate of magnesia and iron-oxyd, a portion of very silicious matter, consisting apparently of comminuted orthoclase and quartz. In place of this, there had been developed in the limestone, near its contact with the dolerite, an amorphous greenish basic silicate, which had seemingly resulted from the union of the silica and alumina with the iron-oxyd, the magnesia, and a portion of lime. By the crystallisation of the products thus generated it was conceived that minerals like hornblende, garnet, and epidote might be developed in earthy sediments, and many cases of local alteration explained. Inasmuch as the reaction described required the intervention of alkaline solutions, rocks from which these were excluded would escape change, although the other conditions might not be wanting. The natural associations of minerals, moreover, led me to suggest that alkaline solutions might favour the crystallisation of aluminous silicates, and thus convert mechanical sediments into gneisses and mica-schists. The ingenious experiments of Daubrée on the part which solutions of alkaline silicates, at elevated temperatures, may play in the formation of crystallised minerals, such as feldspar and pyroxene, were posterior to my early publications on the subject, and fully justified the importance which, early in 1857, I attributed to the intervention of alkaline silicates in the formation of crystalline silicated minerals.*

While, however, there is good reason to believe that solutions of alkaline silicates or carbonates have been efficient agents in the crystallisation and molecular re-arrangement of ancient sediments, and have also played an important part in the local alteration of sedimentary strata which is often observed in the vicinity of intrusive rocks, it is clear to me that the agency of these solutions is less universal than was once supposed by Daubrée and myself, and will not account for the formation of various silicated rocks found among crystalline schists, such as serpentine, hornblende, steatite, and chlorite. When I commenced the study of these crystalline strata, I was led, in accordance with the almost universally received opinion of geologists, to regard them as resulting from a subsequent alteration of palæozoic sediments, which, according to different authorities, were of Cambrian, Silurian, or Devonian age. Thus in the Appalachian region, as we have already seen, they have, on supposed stratigraphical evidence, been successively placed at the base, at the summit, and in the middle of the Lower Silurian or Champlain division of the New York system. A careful chemical examination among the unaltered palæozoic sediments, which in Canada were looked upon as the stratigraphical equivalents of the bands of magnesian silicates in these crystalline schists, showed me, however, no magnesian rocks except certain silicious and ferruginous dolomites. From a consideration of reactions which I had observed to take place in such admixtures in presence of heated alkaline solutions, and from the composition of the basic silicates which I had found to be formed in silicious limestones near their contact with eruptive rocks, I was led to suppose that similar actions, on a grand scale, might transform these silicious dolomites of the unaltered strata into crystalline magnesian silicates.

Further researches, however, convinced me that this view was inapplicable to the crystalline schists of the Appalachians; since, apart from the geognostical considerations set forth in the previous part of this paper, I found that these same crystalline strata hold beds of quartzose dolomite and magnesian carbonate, associated in such intimate relations with beds of serpentine, diallage, and steatite, as to forbid the notion that these silicates could have been generated by any transformations or chemical re-arrangement of mixtures like the accompanying beds of quartzose magnesian carbonates. Hence it was that already, in 1860, as shown above, I announced my conclusion that serpentine, chlorite, and steatite had been derived from silicates like sepiolite, directly formed in waters at the earth's surface, and that the crystalline schists had resulted from the consolidation of previously formed sediments, partly chemical and partly mechanical in their origin. The latter being chiefly silico-aluminous, took, in part, the forms of gneiss and mica-schists, while from the more argillaceous strata, poorer in alkali, much of the aluminous silicate crystallised as andalusite, staurolite, cyanite, and garnet. These views were reiterated in 1863,* and further in 1864, in the following language, as regards the chemically-formed sediments: "steatite, serpentine, pyroxene, hornblende, and in many cases, garnet, epidote, and other silicated minerals are formed by a crystallisation and molecular re-arrangement of silicates generated by chemical processes in waters at the earth's surface."† Their alteration and crystallisation were compared to that of the mechanically formed feldspathic, silicious, and argillaceous sediments just mentioned.

(*To be continued.*)

* Proc Roy. Soc., May 7, 1857. Amer. Jour. Sci., II. xxiii. 438, and xxv. 289 and 435.

THE RELATIONS BETWEEN ZOOLOGY AND PALÆONTOLOGY‡

MY distinguished predecessor, the late Prof. E. Forbes, appears to have been the first who undertook the systematic study of marine zoology with reference to the distribution of marine animals in space and in time. After making himself well acquainted with the fauna of the British seas to the depth of about 200 fathoms by dredging, and by enlisting the active co-operation of many friends, among whom we find MacAndrew, Barlee, Gwyn Jeffreys, William Thompson, and many others, entering enthusiastically into the new field of natural history inquiry; in the year 1841, Forbes joined Captain Graves, who was at that time in command of the Mediterranean Survey as naturalist. During about eighteen months he studied with the utmost care the conditions of the Ægean and its shores, and conducted upwards of 100 dredging operations at depths varying from 1 to 130 fathoms. In 1843 he communicated to the Cork meeting of the British Association an elaborate report on the mollusca and radiata of the Ægean Sea, and on their distribution as bearing on geology. Three years later, in 1846, he published in the first volume of the "Memoirs of the Geological Survey of Great Britain," a most valuable memoir upon the connection between the existing Fauna and Flora of the British Isles and the geological changes which have affected their area, especially during the epoch of the northern drift. In the year 1859 appeared the "Natural History of the European Seas," by the late Prof. Edward Forbes, edited and continued by Robert Godwin-Austen. In the first hundred pages of this little book Forbes gives a general outline of some of the more important of his views with regard to the distribution of marine forms. The remainder of the book is a continuation by his friend Mr. Godwin-Austen, for before it was finished an early death had cut short the career of the most accomplished and original naturalist of his time. I will give a brief sketch of the general result to which Forbes was led by his labours, and I shall have to point out that, although we are now inclined to look somewhat differently on certain very fundamental points, and, although recent investigations with better appliances and more extended experience have invalidated many of his conclusions, to Forbes is due the credit of having been the first to treat these questions in a broad philosophical sense, and to point out that the only means of acquiring a true knowledge of the *rationale* of the distribution of our present fauna is to make ourselves acquainted with its history, to connect the present with the past. This is the direction which must be taken by future inquiry:—Forbes as a pioneer in this line of research was scarcely in a position to appreciate the full value of his work. Every year adds enormously to our stock of data, and every new fact indicates more and more clearly the brilliant results which are to be obtained by following his methods, and by emulating his enthusiasm and his indefatigable industry. Forbes believed implicitly, along with nearly all the leading naturalists of his time, in the immutability

* Geol. of Canada, pp. 577—581.

† Amer. Jour. Sci., II. xxxvii. 266, and xxxviii. 183.

‡ Abstract of Opening Lecture on Natural History delivered at the University of Edinburgh, Nov. 2, by Prof. Wyville Thomson, F.R.S.

of species. He says:—"Every true species presents in its individuals certain features, specific characters, which distinguish it from every other species: as if the Creator had set an exclusive mark or seal on each type." He likewise believed in specific centres of distribution. He held that all the individuals composing a species had descended from a single progenitor, or from two, according as the sexes might be united or distinct, and that, consequently, the idea of a species involved the idea of the relationship in all the individuals of common descent; and the converse, that there could by no possibility be community of descent except in living beings which possessed the same specific characters. He supposed that the original individual or pair was created at a particular spot where the conditions were suitable for its existence and propagation, and that the species extended and migrated from that spot on all sides, over an area of greater or less extent, until it met with some natural barrier in the shape of unsuitable conditions. No specific form could have more than a single centre of distribution. If its area appeared to be broken up, a patch not in connection with the original centre of distribution occurring in some distant locality, it was accounted for by the formation, through some geological change, after the first spread of the species, of a barrier which cut off part of its area, or by some accidental transport to a place where the conditions were sufficiently similar to those of its original habitat to enable it to become naturalised. No species once exterminated was ever re-created, so that in those few cases in which we find a species abundant at one period over an area, absent over the same area for a time, and recurring at a later period, it must be accounted for by a change in the conditions of the area which forced the emigration of the species, and a subsequent further change which permitted its return. Forbes defined and advocated what he called the law of "representation." He found that in all parts of the world, however far removed, and however completely separated by natural barriers, where the conditions of life are similar, species, and groups of species, occur, which, although not identical, resemble one another very closely; and he found that this similarity existed likewise between groups of fossil remains and between groups of fossils and groups of recent forms. Admitting the constancy of specific characters, these resemblances could not be accounted for by community of descent, and he thus arrived at the generalisation that in localities placed under similar circumstances, similar, though specifically distinct, specific forms were created. These he regarded as mutually representative species. Our acceptance of the doctrines of "specific centres" and of "representation," or at all events the form in which we may be inclined to accept them, depends greatly upon the acceptance or rejection of the fundamental dogma of the immutability of species, and on this point there has been a very great change of opinion within the last ten or twelve years—a change certainly due to the remarkable ability and candour with which the question has been discussed by Mr. Darwin and Mr. Wallace. I do not think that I am speaking too strongly when I say that there is now scarcely a single competent general naturalist who is not prepared to accept some form of the doctrine of evolution. There are no doubt very great difficulties in the minds of many of us in conceiving that, commencing from the simplest living being, the present state of things in the organic world has been produced solely by the combined action of "atavism," the tendency of offspring to resemble their parents closely, and "variation," the tendency of offspring to differ individually from their parents within very narrow limits; and many are inclined to believe that some law, as yet undiscovered, other than the "survival of the fittest" must regulate the existing marvellous system of extreme and yet harmonious modification. Still, it must be admitted that variation is a *vera causa*, probably capable, within a limited period, under favourable circumstances, of converting one species into what, according to our present ideas, we should be forced to recognise as a different species; and such being the case, it is perhaps conceivable that during the lapse of a period of time—still infinitely shorter than eternity—variation may have produced the entire result. The individuals composing a species have a definite range of variation strictly limited by the circumstances under which the group of individuals is placed. Except in man and in domesticated animals, in which it is artificially increased, this individual variation is usually so slight as to be inappreciable except to a practised eye; and any extreme variation which passes the natural limit in any direction clashes in some way with surrounding circumstances, and is dangerous to the life of the individual. The normal or graphic line, or "line of safety," of the species, lies midway between the extremes of variation. If at any period in the history of a species, the conditions of life of a group of individuals of the species are gradually altered; with the gradual change of circumstances the limit of variation is contracted in one direction and relaxed in another, it becomes more dangerous to diverge towards one side, and more desirable to diverge towards the other, and the position of the lines limiting variation is altered. The normal line, the line along which the specific characters are most strongly marked, is consequently slightly deflected, some characters being more strongly expressed at the expense of others. This deflection, carried on for ages in the same direction, must eventually carry the divergence of the varying race far beyond any limits within which we are in the habit of admitting identity of species. But the process must be, so to speak, infinitely slow. It is difficult to form any idea of ten, fifty, or a hundred millions of years; or of the relation which such periods bear to changes taking place in the organic world. We must remember, however, that the rocks of the Silurian system, overlaid by ten miles thickness of sediment, entombing a hundred successive faunæ, each as rich and varied as that of the present day, are themselves teeming with fossils fully representing all the existing classes of animals except the very highest. If it is possible to imagine that this marvellous manifestation of eternal power and wisdom involved in living nature can have been worked out through the law of "descent with modification" alone, we shall certainly require from the physicists the very longest row of cyphers which they can afford. Now, although the admission of a doctrine of evolution must affect greatly our conception of the origin and *rationale* of so-called specific centres, it does not practically affect the question of their existence, or of the laws regulating the distribution of species from these centres by migration, by transport, by ocean currents, by elevations or depressions of the land, or by any other causes at work under existing circumstances. So far as practical naturalists are concerned, species are permanent within their narrow limits of variation, and it would introduce an element of infinite confusion and error if we were to regard them in any other light. The origin of species by "descent with modification" is as yet only a hypothesis. During the whole period of recorded human observation, not one single instance of the change of one species into another has been detected, and, singular to say, in successive geological formations, although new species are constantly appearing, and there is abundant evidence of progressive change, no single case has as yet been observed of one species passing through a series of inappreciable modifications into another.

ON THE OBJECTS AND MANAGEMENT OF PROVINCIAL MUSEUMS *

ALTHOUGH every intelligent person knows more or less what these institutions are, and what they ought to be, there is probably no subject, connected with the modern means of education in natural science, concerning which so much misconception or ignorance is manifested and tolerated as in the Management and Objects of our Provincial Museums. The majority of them throughout England present such examples of helpless misdirection and incapacity as could not be paralleled elsewhere in Europe. Some noteworthy exceptions there are. But generally the managers or guardians of local museums are precisely of this unfit class, and seem to have no more notion of their charge than as mere curiosity-shops, and even display less intelligence than is shown in such shops, where the cupidity or shrewdness of the dealer induces him at least to take due care of, and give a local habitation and a name to, his wares. But in the provincial museums even this care and tittle of information is pertinaciously withheld, and the visitors are left to do the best they can amid the surrounding bewilderment. This is commonly made up of a most puzzling jumble of heterogeneous miscellanies, arranged, or rather scattered, with an equally sovereign contempt for the convenience or instruction of the public, and indeed all in such admired disorder as may most plainly show how Chaos is come again and Confusion can make his masterpiece, and how every specimen added to the heap only tends to increase or perpetuate the miserable derangement. It looks as

* Abstract of an Address to a Meeting of the East Kent Natural History Society, at Canterbury, Oct. 12, 1871, by its Vice-President and Honorary Secretary, George Gulliver, F.R.S.