n North America at about the same horizon. It is, however, at present impracticable for me to follow up this question for want of access to books of reference; but this is of comparatively minor importance, as my purpose has merely been to show that there have been glacial epochs comparable to that of the Post-Tertiary period; and having shown that such a glacial epoch did at one time affect a large portion of the Earth's surface, it becomes easy to acknowledge that similar periods of cold have occurred before and since, and that we must not attempt to ascribe every occurrence of Glacial beds of Tertiary or Pre-Tertiary age to some merely local cause. And having acknowledged this, we at once obtain what was wanted, a check on the palæontological timepiece, a time-signal on the chronograph of the world.

Note.—A month ago I would have appealed, as proof positive of the contentions stated above, to the discovery in the Salt Range of the Punjab of marine fossils identical with those of the Australian Carboniferous beds. These are derived from beds which exhibit ample proofs of glacial action, and were on that ground assumed by Dr. Waagen to be of the same age as the Talchirs, which he agreed with most of the members of the Geological Survey in regarding as of Palæozoic age. The pebbles in which the fossils were found might in hand-specimens be taken for concretionary nodules, and an imperfect description of their mode of occurrence would support this idea; moreover the coincidence of the fauna and physical conditions with those of the Australian beds is very striking. There was every temptation for me to accept Dr. Waagen's conclusions, but a careful examination of the beds, and of the mode of occurrence of the fossils, has convinced me that this is a mere coincidence, and that the fossils, which occur as transported pebbles, can consequently be of no use in determining the homotaxis of the beds from which they are derived. The stratigraphical relations of these beds are such as to associate them with the Nummulitics; and as boulder beds, presumably of glacial origin, have been recorded by Mr. Lydekker as conformably underlying the Nummulitics of Ladák, there is no difficulty in finding a horizon to which the beds can be referred.

III.—Essays on Speculative Geology.

2.—Probable Changes of Latitude.

By R. D. Oldham, A.R.S.M. etc.

Part I.—Glacial Periods in Low Latitudes.

In my last essay I had occasion to refer to the former existence of icebergs in localities which now lie in latitudes lower than those in which glacial action is known to have reached, even during the last Glacial period. But, surprising as it may be to find evidence of glacial action within a few degrees of, and, as in the case of the Bowen River Coal-field, a few degrees within, the tropics, this sinks into insignificance in the face of the evidences of repeated Glacial periods that may be found in India, and especially in the Himalayas.

In Kashmir Mr. Lydekker has described[1] a group of beds composed of a fine-grained matrix, through which are scattered boulders of crystalline rock; these were considered to be of glacial origin, and indeed it is difficult to conceive of any other satisfactory explanation. This group, the Punjal Conglomerates, has not yet been identified with certainty in the Simla region of the Lower Himalayas; but there is a group of beds whose position and appearance render it probable that they are of the same age.

Above this group, which may represent the Punjal Conglomerates of Kashmir, but separated from them by a considerable though undetermined thickness of beds and an unconformity, comes the Blaini group,[2] which is so unique in its character, and so constant over a large area, that it is most important in unravelling the structure of the hills. It consists of a band, seldom over 30 feet thick, of thin-bedded limestone resting on a "conglomerate," the matrix being usually a fine-grained slate, through which pebbles and boulders of slate and quartzite are scattered. The aspect of the rock is decidedly glacial, and my colleague Mr. C. S. Middlemiss has discovered a pebble scratched in a manner very suggestive of ice action.

Yet higher in the series there is the Mandháli group, which, though it has so far yielded no scratched pebble, is even more conspicuously glacial than the Blaini Conglomerate; and, yet newer, there are at the base of a quartzite series, provisionally known as the Báwars, some beds originally composed of fine sand, through which rounded fragments of quartzite sometimes over a foot in diameter are scattered; these beds are associated with a very coarse-grained arkose, itself indicative of a more severe climate than now prevails in these latitudes, even at an altitude of 15,000 feet. These last two groups have not yet been proved to be distinct; but there is no reason for doubting their distinctness, or suspecting their identity.

All these beds are conspicuously of subaqueous origin, and if we except the Báwar beds—which have so far been identified in one locality only—too widespread in their distribution and too constant in their characters to render it probable that they are of other than marine origin. There are, besides, very good reasons, which it is needless to enter on here, for supposing that all the sedimentary beds of the Lower Himalayas are of marine origin.

In the Lower Himalayas no pre-Tertiary glacial beds of later date than the Báwars have yet been determined; but in Ladak Mr. Lydekker has described a group of beds which he considers of glacial origin, as conformably underlying the Nummulitics.[3]

Leaving the Himalayas, we find in the Salt Range proofs of glacial action at more than one horizon. The newest of these is in the "Olive group," which was originally described as Cretaceous, and lately, on the strength of some *Conulariæ*, identical with species found in Australia, which were supposed to be derived from con-

[1] Memoirs Geological Survey of India, vol. xxii. p. 247.
[2] First described by Mr. H. B. Medlicott, Memoirs Geological Survey of India, vol. iii. pt. 2, p. 30.
[3] Memoirs Geological Survey of India, vol. xxii. p. 104.

cretionary nodules formed *in situ*, has been declared to be Carboniferous and contemporaneous with the Talchir group of the peninsula.[1] This last supposition may or may not be true; but, as I have already explained, there can be no manner of doubt that the fossils occur in transported pebbles, and are consequently valueless for determining the homotaxis of this group.[2]

Besides the Olive group, Mr. Wynne has described glacial boulder beds in the Speckled and Purple Sandstone groups of the Salt Range,[3] and in the Trans-Indus extension of that range glacial boulder beds[4] crop out from below a limestone of Upper Carboniferous age.

In the peninsula we know of but a single group of glacial beds, but it is not difficult to account for the difference; for, while the extra-peninsular area has yielded an extensive and fairly complete series of marine sedimentary beds, these are conspicuously absent in the peninsular area. The limestone of the Vindhyan series and the Talchir group of the Gondwanas may be of marine origin; but, apart from them, the rocks of the peninsula, where not of volcanic or metamorphic origin, are almost entirely river deposits; so that the absence of any trace of more than a single glacial period is more than possibly due to their records having been destroyed.

These facts are in themselves sufficiently striking and difficult to reconcile with some of the generally accepted hypotheses of geology, but they are emphasized by a detailed examination of two of the instances. To take the Olive group of the Salt Range, boulders and pebbles showing glacial striæ are abundant, and it is by no means unusual to find an irregular-shaped mass of hard crystalline rock with one, and occasionally more than one, of its surfaces ground into a flat facet, smoothed, polished, and striated with nearly parallel striæ.

In the case of the Talchir boulder bed of the peninsula, it has been usual to ascribe its origin to winter coast ice; but the flattened boulders of the Olive group indicate a more prolonged wearing, a greater pressure and a greater constancy of direction of motion than can be accounted for on this supposition. We are consequently driven to the hypothesis that they have been ground by a glacier which descended to the sea-level and gave off icebergs there.

Now the majority of these boulders consists of rocks of recognizably peninsular types, not a few are of a very highly siliceous felsite porphyry, which is at present only known in the Rajputana Desert, and not a single fragment has yet been found which can be declared to be derived from a Himalayan source. Besides this, the pebble band from which the *Conulariæ* referred to above were obtained exhibits certain peculiarities of distribution, which indicate that the source from which the pebbles were derived lay to the southwards.[5]

[1] Records Geological Survey of India, vol. xix. p. 22.
[2] Ibid. p. 127, *et sequel.*
[3] Memoirs Geological Survey of India, vol. xiv. pp. 87, 93, 214, etc.
[4] Ibid. vol. xvii. p. 239.
[5] Records Geological Survey of India, vol. xix. p. 129.

The boulder bed of the Olive group has not yet yielded any fossils of contemporaneous origin, but it appears to be perfectly conformable to beds of undeniably marine origin, and every argument from analogy is in favour of the supposition that it is itself either of marine or estuarine origin. But, as I have shown above, the land surface of whose waste it is composed lay to the south, so we arrive at the rather startling conclusion that when the beds of the Olive group of the Salt Range were being deposited, *there were glaciers which descended to the sea-level in a region which now lies within 34 degrees of the Equator.*

In that great and almost unknown tract lying between the Aravalli Mountains and the Indus, which is comprehensively entitled "Desert" on the maps, there may be found near the town of Pokran, in N. latitude 26° 55′, an old land-surface showing glacial groovings and striæ. These might be ascribed to the action of winter coast ice formed on the margin of a lake or sea; but, in the boulder beds which occur in the neighbourhood, and are without doubt of the same age as the glaciated land surface, there may be found facetted blocks which, like those of the Salt Range, could not be ascribed to anything but glacier action. Moreover, this land-surface is covered in places by a boulder bed with a hard intensely tough matrix, differing from the stratified boulder beds of the neighbourhood in much the same manner as the "till" of Scotland differs from the marine boulder-clays of the Midland Counties; if the hypothesis that the toughness of the former is due to its being a "Grundmoraine" be accepted, it follows that the same explanation will account for the toughness of the boulder beds of Pokran, and we have yet another proof of the existence of glaciers on this old land-surface.

The boulder beds in the Desert have been traced for sixty miles north-east of Pokran; in the vicinity of the old land-surface the boulders are almost exclusively of porphyry and syenite derived from it, but further north blocks of gneiss of the peninsular type become common; and in N. latitude 27° 30′, East longitude 72° 30′, there is a block of very coarse-grained granite, of which 10 feet × 7½ feet × 3 feet is exposed above ground. The nearest source from which this block could have been derived is in the Aravallis full 150 miles away. The age of these boulder beds appears to be the same as that of the Talchirs; the reasons for this conclusion are of a purely inferential nature, but their extent, combined with the distance from which some of the blocks have been transported, as well as their position on the western margin of the peninsular area, point to the conclusion that they are of marine origin; so that here again we have *evidence of glaciers having descended to the sea in a district now less than 27 degrees from the Equator.*

Part II.—General Considerations.

It has long been known that there were ample proofs of the former existence of mild, even subtropical climates within the Arctic circle; but the continuity of this climate, and the absence of any signs of

the extreme cold which now prevails in that region, was never fully understood till it was described and emphasized by Baron Nordenskiöld. In a lecture of his published in this MAGAZINE, after reviewing the evidence of the fossil flora and fauna, he remarks on the favourable nature of the country for geological investigation, on the completeness of the series extending one may say from the Silurian to the Tertiary, and emphasizes the fact that, in all the sections he had examined, he never saw a boulder "even as large as a child's head" in any rock of Tertiary age or older.[1] Various hypotheses have been propounded to account for these warm climates in the Arctic regions without involving a shifting of the earth's polar axis; the most ingenious and captivating of these is doubtless Mr. Wallace's modification of Dr. Croll's theory, according to which the mild climates of the polar area were due to the warming effects of currents of heated water, from the equatorial regions, which have been cut off by a gradual development of the continental areas. Looked at from the polar point of view, this hypothesis was legitimate and competent enough to account for the facts it was intended to explain; but an hypothesis is only acceptable as long as nothing directly incompatible with it is known, and however competent the hypothesis may be to account for the mild climates of what are now the Arctic regions, it is absolutely incompatible with the evidences of repeated glaciation in low latitudes which I have referred to above.

Mathematicians forbid us to explain the circumstances by a shifting of the axis of revolution of the earth. Whether in this they are right or wrong is immaterial, for it seems to me that there is an equally satisfactory hypothesis open to us. Mr. Fisher, in his "Physics of the Earth's Crust," has given good reasons for supposing that there is a fluid or semifluid layer intervening between the solid core and the solid crust of the earth,—in other words, that the latter has a power of shifting its position on the former; if this theory be accepted, it is quite conceivable that the portion of the earth's crust which now occupies the polar circle may once have lain under the Equator and *vice versa*; indeed I find in Mr. Fisher's[2] book an assertion of the probability of this shifting of the polar and equatorial areas based on reasons quite different from and independent of those I have given for the same conclusion.

The known facts of stratigraphical geology, more especially the existence of regions which can be proved to have undergone compression to the extent of two or more times their present dimensions, in immediate proximity to others in which the beds have suffered little or no compression, show that to some extent this shifting of the crust of the earth over its core must take place, and almost the only argument that can be produced against an extension of the same

[1] GEOL. MAG. 1875, p. 531, and 1876, p. 266. I cannot help contrasting this with my own experience in the Himalayas, where the series is well exposed in numerous deep valleys, where there is an extensive series of beds extending from even before the Silurian to the Tertiaries, and where evidences of pre-Tertiary glacial action met me, I might almost say at every turn.

[2] Physics of the Earth's Crust, p. 184. But earlier still see "On a Possible Cause of Climatal Changes," by Dr. John Evans, F.R.S., F.G.S., GEOL. MAG. 1866, p. 171.—EDIT.

reasoning would be derived from the doctrine of permanence of continents. It is, however, by no means inconceivable that the two hypotheses might be quite consistent; were the differences between the continental and oceanic areas entirely due to differences in the structure of the crust, the latter might shift its position relative to the core to any extent without interfering with the relative positions and forms of the continental and oceanic areas.

But is this doctrine so well established that it can be used as an argument against any hypothesis which is fairly supported by known facts? I think not. It is unnecessary to refer to the fact that the "Oceanic" island of South Georgia has been found to consist of clay-slate,[2] and not of volcanic rocks, as *ex hypothesi* it should, for there are certain peculiarities, in the palæontology of India and South Africa, which indicate the former existence of direct land communication between the two countries. This was first pointed out by Mr. H. F. Blanford, whose paper is somewhat contemptuously dismissed by Mr. Wallace with the remark that "the notion that a similarity of the productions of widely separated continents at any past epoch is only to be explained by the existence of a direct land connexion, is entirely opposed to all that we know of the wide and varying distribution of all types at different periods, and is no less opposed to what is now known of the general permanency of the great continental and oceanic areas."[2] This, however, implies a misconception of the nature of the evidence, which is far from being based merely on a "similarity of the productions" of the two countries. There are in Africa two distinct floras of different ages; one of these, that of the Beaufort beds, has a flora consisting of five distinct species, of which one is identical, two are closely allied to, forms found in the Damuda beds of India, and if we accept Dr. Feistmantel's[3] opinion, all belong to Damuda genera; associated with these plants is an extensive and peculiar reptilian fauna, of which the most prominent genus is *Dicynodon*, a genus at present unknown elsewhere except from the Panchet beds which overlie the Damudas, and another form, *Micropholis Stowei*, is a near ally of *Brachiops laticeps* from the Kamthi beds of Mangli.[4]

At a higher horizon in South Africa, in the Uitenhage formation, there is a flora consisting of 12 distinct forms, all generically different from any of the Beaufort species. Of these, one is identical with, four are closely allied to, species from the flora of the Rajmahal group in India. The difference between the flora of the Rajmahal group and of the Danudas is almost as great as in the case of the two corresponding African groups, for there are only three species in the Rajmahal flora which are in any way allied to any of the Damuda plants. We have then a close and continuous similarity between the fauna and flora of two countries lasting through a period long enough to allow of a complete specific, and almost complete generic change.

A similar but even more conclusive argument may be derived

[1] Nature, March 27, 1884, p. 509; GEOL. MAG. Dec. III. Vol. I. p. 225 (1884).
[2] Island Life, p. 398. [3] Palæontographica, 1878, p. 114.
[4] Manual of the Geology of India, vol. i. p. 123.

from the relations of the Marine Cretaceous rocks in India and other parts of the world. Both in Southern India and Southern Africa there are marine deposits of Cretaceous age, with regard to which Dr. W. T. Blanford writes in the Manual of the Geology of India as follows:—"Before quitting the subject of the Trichinopoly Cretaceous beds, it is necessary to notice the very remarkable resemblance between a portion of their fauna and the species found in certain strata in Southern Africa. In the description of the Gondwana system, and again in the account of the Upper Jurassic beds of Cutch, the remarkable affinities between Indian fossil plants and animals, and the forms found in South African beds, were repeatedly noticed, and there is a similar connexion between the Cretaceous formation in the two regions. In some deposits found resting upon Karoo beds on the coast of Natal, out of 35 species of *Mollusca* and *Echinodermata* collected and specifically identified, 22 are identical with forms found in the Cretaceous beds of Southern India, the majority being Trichinopoly species. . . . . The South African beds are clearly coast or shallow-water deposits, like those of India, and the great similarity of forms certainly suggests continuity of coast-line between the two regions, and thus supports the view that the land connection between South Africa and India, already shown to have probably existed in both the Lower and Upper Gondwana periods, and of which important indications are afforded by the Marine Jurassic beds, was continued into Cretaceous times. It is very surprising to compare the Middle Cretaceous fauna of Southern India with that of the distant beds of Natal, and then with the widely differing forms found in beds of the same age *in Central India and Southern Arabia*."[1]

Speaking of the latter he says, "Some of the species have a wide range in time among the Cretaceous rocks of Europe, but all occur in the Upper Greensand (Cenomanian), many being characteristic forms, and the Cretaceous rocks of the Narbadda valley must in consequence closely correspond to the Utatur group of Southern India. It is curious to note that, so far as is known, only one species, *Pecten* (*Vola*) *quinquecostatus*, is common to both, and even in this case the identification depends upon a question as to which palæontologists are not thoroughly agreed . . .

In strange contrast with the wide difference between the known fauna of the Bagh beds and that of the Southern India deposits is the similarity between the fossil remains of the Narbadda valley and those found in two localities on the south-east coast of Arabia. The collections examined from both localities are small, and were obtained in each case during a short visit; but although the united Arabian collections only comprise 13 species and the Bagh 12, three of these . . . . are common to the two countries. The Cretaceous beds of the lower Narbadda valley are about 750 miles distant from those of Southern India, and twice as far from the Arabian localities. The marked contrast between the fossil faunas in the one case, and the similarity in the other,

[1] *Loc. cit.* p 292.

tend to suggest the probability that a land barrier interposed in Middle Cretaceous times between Southern India with Assam and Arakan on the one side and the Western Narbada region with the south coast of Arabia on the other. We have thus another argument presented to us in favour of the Indian peninsula being portion of an ancient land-area; and taking into consideration the marked connexion between the faunas of the South Indian and South African Cretaceous deposits, and the circumstance that both appear to be of littoral origin, it is probable that this land-area extended to Africa."[1]

These facts indicate that the permanence of continents is a hobby which some of its admirers have ridden too hard, and at any rate prove that it cannot be used to stifle a plausible hypothesis.

Another group of facts which are in favour of the suggestion I have made above is the observational evidence in favour of a change of latitude in some of the principal European observatories. In the American Journal of Science for March, 1885, Professor Asaph Hall gives, on the authority of S. Fergola, the following table of latitudes of the principal observatories of Europe and America:—

Washington	1845	.....	38° 53′ 39″·25
do.	1863	.....	38″·78
Paris	1825	.....	48° 50′ 13″· 0
do.	1853	.....	11″· 2
Milan	1811	.....	45° 27′ 60″· 7
do.	1871	.....	59″·19
Rome	1810	.....	41° 53 54″·26
do.	1866	.....	54″·09
Naples	1820	.....	40° 51′ 46″·63
do.	1871	.....	45″·41
Königsberg	1820	.....	54° 42′ 50″·71
do.	1843	.....	50″·56
Greenwich	1838	.....	51° 28′ 38″·43
do.	1845	.....	38″·17
do.	1856	.....	37″·92

Besides this there are the Pulkowa observations which give the following results:—

Pulkowa	1843	.....	59° 46′ 18″·73 ± 0″·013
do.	1866	.....	18″·65 ± 0″·014
do.	1872	.....	18″·50 ± 0″·014

I am aware that the most recent investigations of the Greenwich observations by the present Astronomer-Royal have shown that there is no proof of continuous change of latitude; but it is surely something more than a coincidence that the change in every case is in the same direction; had the change been due merely to imperfect observation or the vagaries of refraction, it would hardly have exhibited these strange coincidences. There is, however, more than one way of explaining these slight variations of latitude, and, seeing that the observations extend over a comparatively short period of time, it would not do to attach too great importance to them in this connexion.

A more important argument is to be derived from the careful measurements of the pyramids of Gizeh which have been made by

[1] *Loc. cit.* p. 297.

Mr. Flinders Petrie. The orientations of the sides and passages of the great and second pyramids vary not more than 30″ from each other, but they both agree in varying rather than 5′ west of north;[1] it is inconceivable that this close approximation of the orientation of the sides and passages of these pyramids should be due to accident, nor is it conceivable that the builders would deliberately have introduced a variation of some 5′ west of north. A far more probable explanation is that owing to a shifting of the earth's crust on its core, or of the axis of revolution, there has been a variation of that amount in the direction true north since the pyramids were built.

Such, briefly stated, are the conditions of the problem. We have first a group of facts inexplicable, unless we grant the possibility of a shifting of the earth's crust on its core, or of the axis of revolution of the earth; secondly a group of facts inconsistent with the only hypothesis that could be urged against the first supposition, and thirdly a group of facts directly confirmatory of the latter.

In conclusion I must apologize for any injustice I may have—unintentionally—committed; an official geologist in India has to contend with many difficulties, not the least of which is the impossibility of keeping abreast of current literature, and a want of leisure for pursuing independent investigations. It had been my intention to work out this problem more thoroughly during the current year; but having been deputed to accompany an embassy to Tibet, the opportunity is gone, and not likely to recur for some years. I am consequently induced to put my notes on the subject together, somewhat hastily I confess, as it has been my good fortune to meet with a number of facts which cannot be ignored in any discussion of the problem of geological climates, many of them having either never been published at all, or only in a form not generally accessible to European geologists.