V. *Regio Mesirenica*, without true Seals (*Phocinæ*), but having *Otaria* and *Macrorhinus* from the south; no Sirenian known.

VI. *Regio Notopolagica*, characterized by four endemic genera of *Phocidæ*, and by the presence of many *Otariæ;* without Sirenians, but with two endemic forms of Cetaceans (*Neobalæna* and *Berardius*).

In conclusion, I will call attention to some of the more remarkable points in the general distribution of the marine Mammals, and to their apparent significance.

In the first place it is evident that the Pacific has much more in common with the Notopelagian region than the Atlantic. *Otaria* and *Macrorhinus*, quite unknown in the Atlantic, extend themselves to the northern extremity of the Pacific, the former pervading that ocean up to Behring's Straits, and the latter reaching to the Californian coast. It follows that in former ages there must have been some barrier in the Atlantic which did not exist in the Pacific to stop their progress northwards. The only barrier I can imagine that would have effected this must have been a land uniting South America and Africa, across which they could not travel. Adopting this hypothesis, we have, at the same time, an explanation of the presence of the Manatee on both the American and African coasts. The Manatee could hardly live to cross the Atlantic. It is only found close to the coast, where it browses on sea-weeds and other vegetable food in shallow water. How did it travel from America to Africa (or *vice versa*), unless there were a continuous shore-line between them? The same may be said of the Monk Seal (*Monachus*), of which one species lives in the Mediterranean and on the African coast and Islands, and another in the West Indies. We can hardly believe that these creatures could easily traverse the whole Atlantic. The hypothesis of a former barrier of land between Africa and America, which we know is supported by other facts of distribution,* would alone explain the difficulty.

On the other hand, in the Pacific we find no such break between the north and south. The aquatic Mammals of Notopelagia have evidently had free access to the whole Pacific for a long period and have well availed themselves of this facility.

Again, while the great Southern Ocean exhibits a considerable uniformity of marine Mammalian life, we see the Northern waters divided into two distinctly recognizable Regions by the interposed masses of land. All these facts, with the one exception of the supposed Atlantic Barrier, would tend in favor of the now generally accepted doctrine that the principal masses of land and water are not of modern origin, but have existed mainly in their present shapes throughout all ages.

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FORMER EXTENSION OF CORNELL GLACIER NEAR THE SOUTHERN END OF MELVILLE BAY.†

THE initial effort of Professor Tarr's paper is to controvert the opinions recently expressed by Chamberlin and Salisbury respecting the former extension of the general glaciation of Greenland, their view being that the coast was not universally and profoundly overwhelmed by the inland ice. The observations of Professor Tarr had a range in latitude of about 5° 30′, those of Mr. Chamberlin about 17° 15′, and those of Professor Salisbury about 12°. The joint observations of Chamberlin and Salisbury covered 18° 30′, the range of their landings being about 13° 40′. These landings embraced thirteen different localities, counting the numerous landings on the border of Inglefield Gulf as one. This statement has

* *Cf.* Wallace, Geogr. Distrib. Vol. I., p. 156.

† Bull. Geol. Soc. Amer., Vol. 8, pp. 251–268, Plates XXV.–XXIX., March, 1897.

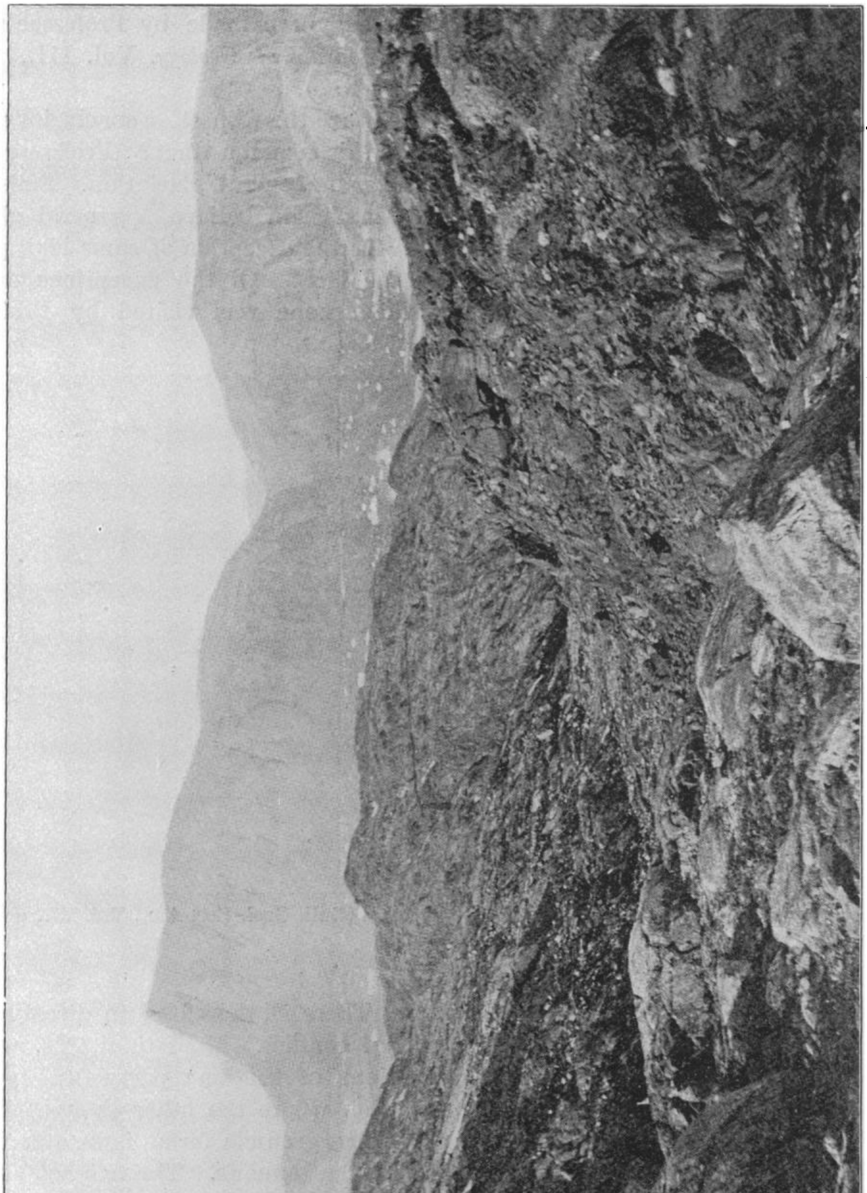

FIG. 1. "THE DEVIL'S THUMB. Elevation 2,650 feet; glaciated surface in foreground; rugged, angular topography in background on left. Transported pebbles obtained on crest of the Devil's Thumb. Photograph by J. O. Martin." [Author's legend.]

its occasion in the too insistent implication of the author of the paper under review that the observations whose validity he questions were limited to those made chiefly from a passing vessel. Of the latitude covered by the observations of Chamberlin and Salisbury about 8° 30′ lay south of the tract seen by Professor Tarr, and about 4° 30′ lay to the north of it. If the total distribution of observations be divided into three parts, 8° 30′, 5° 30′ and 4° 30′, in order from south to north, Professor Tarr's observations fall within the middle division and cover less than one-third of the whole.

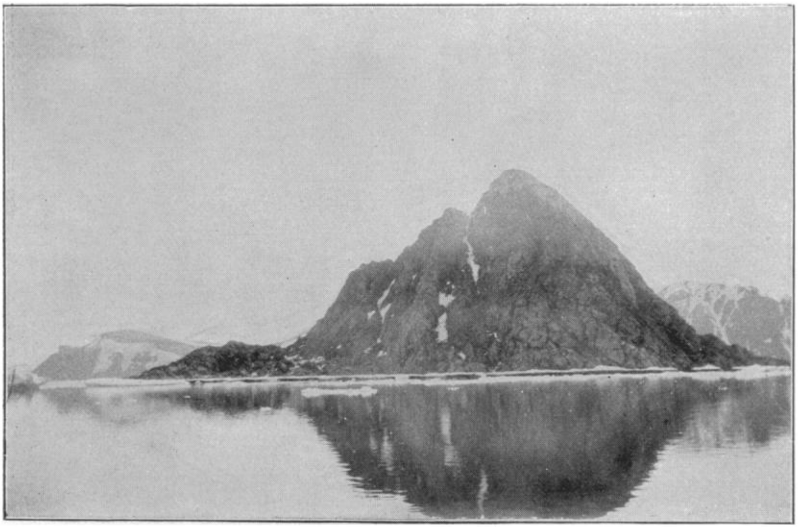

Fig. 2. Dalrymple Island—Type of unglaciated topography. (Bull. Geol. Soc. Am., Vol. VI., pp. 219; also Jour. Geol., Vol. II., 1894, p. 661.)

It appears, therefore, that the testimony of 5° 30′ is being urged to set right the testimony of 18° 30′ in a matter of *general* conclusions. In such an attempt it would seem altogether imperative that an author urging conclusions from the minor fraction should have ascertained, with scrupulous care, whether his own observations within that fraction confirmed or contradicted the *coincident* part of those made upon the much wider tract. Singularly enough, this vital prerequisite is neglected, although an explicit statement covering this common territory had been made by Professor Salisbury (*Journal of Geology*, Vol. III., 1895, pp. 876–877).

What are the respective conclusions relative to this common tract? Professor Tarr insists upon general glaciation. Salisbury and Chamberlin believe in general glaciation with the exception of some high peaks and lee faces. Of the exceptions named by them none was visited by Professor Tarr. There is, therefore, no direct observational conflict. More than this, no observations of the one party demonstrate glaciation where the other thought it absent. The grounds for an issue are, therefore, rather tenuous. The two sets of observations are in reality rather confirmatory than conflicting. The issue has arisen from an attempt to adjudicate the whole coast by a fraction which happens to be intermediate in type, having been neither strongly subjugated by glaciation nor left conspicuously intact.

The author recognizes that the conclusions of Chamberlin and Salisbury are based upon evidence of two kinds: "one, the presence of a driftless area in the Inglefield Gulf region, announced by Professor Chamberlin; and a second, the angular topography of the Greenland coast, described by both." The first evidence, although vital to a *general* conclusion, is passed without discussion, because it was not seen by the author. He remarks, however, in a footnote that he "cannot let this opportunity pass without raising the query whether the topography in the neighborhood of the Greenland driftless area is not such that an area of this sort would naturally be expected. Was not the movement of the ice outward and the main stream down the Inglefield Gulf? And is not the driftless area located in the place where the high Red Cliff peninsula would naturally have clogged the ice and hence prevented its action of erosion and notable transportation?" The driftless area is a part of the same ancient peneplain as the summit of the Red Cliff peninsula (*Journal of Geology*, p. 205–6, Vol. III., 1895). It lies on the *east side of Red Cliff peninsula* (see map, p. 668, Vol. II., *Jour. Geol.*). It lies *between it and the great ice cap*. It is separated from the peninsula by the valley of Bowdoin bay, about two miles wide and 2,000 feet deep. How an isolated part of a peneplain can protect from glaciation another part of the same plain lying between it and the source of the glacial motion and several miles distant is not easily understood. The suggestion apparently sprang from the same lack of circumspection that gave birth to the main issue without adequate grounds.

The evidence drawn from topography is the main subject of discussion in the initial portion of the paper. The essential point urged in the paper and reiterated in subsequent discussion is that angularity of topography is compatible with general glaciation, and that general observations on topographic contours have little or no value in deciding the prevalence of glaciation. The most important illustrative evidence in support of this is a photographic view of 'The Devil's Thumb.' The photograph is here reproduced (Pl. 26, Vol. VIII., 1886, *Bull. Geol. Soc. Am.*). (Fig. 1.) Nothing has recently so astonished the present writer or his colleague as the presentation of this photograph as an illustration of the *absence* of topographic signs of glaciation. It is a marvel to us how any glacialist could fail to read from these coutours—even from the summit contours— just that degree of glaciation which was found by the more rigorous lines of study. It is true that the lee side is angular, but this does not in any essential way confuse or obscure the dominant expression, which is that of moderate glaciation. That geologists may see how different are the contours that led Professor Salisbury and the writer to infer absence of glaciation in certain other regions, there is here reproduced the photograph of Dalrymple Island which was used as an illustration of the asperities of a typical angular topography (*Jour. of Geol.*, Vol. II., p. 661). (Fig. 2.) It must be evident to the critical observer that the two topographies belong to distinct types. The contours of Dalrymple Island clearly show the absence of any glacial softening. The contours of the so-called Devil's Thumb and of the adjacent region clearly portray a moderate measure of glacial softening. The normal asperities of a non-glacial arctic topography are gone. This topographic expression seems to the reviewer to be such as to be read with ease and accuracy even at a distance, especially after the general habit of the region has been determined by more rigorous lines of evidence. The photograph does not sustain the author in calling the topography angular in an unqualified sense; nor does it sustain him in insisting that it is impracticable for observers to detect such signs of glaciation.

An additional source of confusion is introduced by naming the promonotory of the photograph 'The Devil's Thumb.' It is stated in a foot-note that "This is the Devil's Thumb as given on the Danish and British Admiralty charts. The real Devil's Thumb of the Arctic explorers is some forty or fifty miles to the north of this" (p. 254). The true Devil's Thumb is, however, sketched on the British Admiralty chart. The sketch as there given is herewith photographically reproduced (Fig. 3). It will

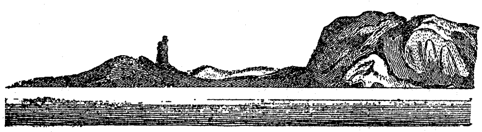

FIG. 3. The Devil's Thumb, as given on the British Admiralty Chart.

be seen that it does not bear even a remote resemblance to the promontory illustrated in the paper. It is to be assumed without question that the latter stands at the location designated on the charts as The Devil's Thumb, but this location is obviously an error. It does not seem to us that it justifies the transfer of the name Devil's Thumb to a new promontory to which the name has no fitness. It is especially unfortunate to introduce a second Devil's Thumb in this connection, because the true Devil's Thumb, by reason of its slenderness and angularity, has a significant bearing on the question of glacial extension, and together with Melville Monument a similar pinnacle somewhat further north has been so cited. Misapprehension has already arisen on account of this double use of the name. The slenderness and angularity of the Devil's Thumb are exaggerated in the sketch of the Admiralty chart—about as much as such objects are usually exaggerated by the impressions of the average observer. With some discount for this it gives a fair idea of this singular landmark.

If the true Devil's Thumb, Dalrymple Rock, and similar instances of angularity and asperity are taken as one type; if the contours illustrated by the paper under review be taken as a second type; if the contours of the Carey Islands (*Journal of Geology*, Vol. II., p. 662) be taken as a third type, and if the plainer topography north of Godhaab be taken as a fourth type, something of the degrees and gradations of topographical modification which the coast belt of Greenland suffered at the hands of glaciation will be indicated.

The view of the author that the peaks would remain more angular than the valleys seems perfectly valid so long as the conditions are *limited to the border of the ice cap*. Near the border the thickness of the ice in the valleys is much greater than upon the hills. The main flowage is through the valleys, and subjugation of the heights is relatively slight. On the other hand, this reasoning entirely falls to the ground when applied to profound glacial submersion, such as is implied by an advance of the 100 or 200 miles necessary to reach the heart of Baffin's Bay. Such an advance means a depth of at least 5,000 feet of ice on the peaks. In this case the difference between the depth on the heights and in the valleys is relatively much less, and three well established principles combine to emphasize the erosion of the peaks: (1) the upper portion of the ice moves faster than the lower; (2) erosion is correlated with rapidity of motion by some high power of the rate; (3) peaks yield to erosion more than embossments of the valley, because (a) the surface exposed to action is relatively greater, and (b) the attachment, and hence the resistance, of the exposed parts is relatively less. The erosion, therefore, proceeds on the crests with a facility superior to that in the valleys by the measure of some multiplier much greater than unity. When valleys are irregular the basal retardation is still further increased and the movement of the ice is correspondingly transferred to the upper horizons. The irregularities of the coast of the region in question give this fact special application. The interpretation of the author is, therefore, quite consistent with a *limited* extension of the ice border, but quite inconsistent with profound extension. The whole of the phenomena described in the paper are precisely concordant with moderate extension. They are as precisely discordant with great extension.

The remainder of the paper consists of a description of the Cornell glacier, of the evidences and amount of former invasion, of the recent advance and retreat of the ice, and of the evidences of present retreat of the Cornell glacier. This portion embraces much valuable data, unless it is vitiated, as it probably is not, by the lack of care which marks the controversial part. It is accompanied by excellent photographs, all of which, as the writer would interpret them, show evidences of greater or less glacial modification of contour.

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SUGGESTIONS FOR A NEW METHOD OF DISCRIMINATING BETWEEN SPECIES AND SUBSPECIES.

ACCORDING to present usage the rule which determines whether a particular animal or plant shall stand in our books as a species or subspecies may be stated as follows: *Forms known to intergrade, no matter how different, must be treated as subspecies and bear trinomial names; forms not known to intergrade, no matter how closely related, must be treated as full species and bear binomial names.* This principle was first distinctly formulated in the Code of Nomenclature of the American Ornithologists' Union, published in 1886. In the remarks that follow, the authors of the Code state: "The kind or quality, not the degree or quantity, of difference of one organism from another determines its fitness to be named trinomially rather than binomially. A difference, however little, that is reasonably constant, and therefore 'specific' in a proper sense, may be fully signalized by the binomial method. Another difference, however great in its extreme manifestation, that is found to lessen and disappear when specimens from large geographical areas or from contiguous faunal regions are compared is, therefore, not 'specific,' and, therefore, is to be provided for by some other method than that which formally recognizes 'species' as the ultimate factors in zoological classification. In a word, *intergradation* is the touchstone of trinomialism."

Eleven years have now elapsed since the publication af the A. O. U. Code of Nomenclature, in which the above canon and statement were first published. During this period the plan advocated has been very thoroughly tested, not only by ornithologists, but by systematists in many other departments of zoology, and also in botany. The time has come, therefore, when it should be possible to examine its practical workings, with a view to ascertaining whether or not the system is satisfactory.

In practice it has been found that only in a small percentage of cases does an author have at his command a sufficiently large series of specimens, from a sufficient number of well-selected localities, to enable him to say positively that related forms do or do not intergrade. The result of this obvious embarrassment is that authors usually exercise their individual judgment as to the *probable* existence or non-existence of intergradation, thus introducing the personal equation it was hoped to avoid. The natural result is a degree of inconsistency in the use of trinomials which has formed the subject