THE
GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE II. VOL. IV.

No. III.—MARCH, 1877.

ORIGINAL ARTICLES.

I.—EVIDENCE AFFORDED BY THE PLANET MARS ON THE SUBJECT
OF GLACIAL PERIODS.

By EDWARD CARPENTER, M.A., late Fellow of Trinity Hall, Cambridge.

HAVING read with much interest Mr. Murphy's papers in the Journal of the Geological Society for 1869 (p. 350) and for 1876 (p. 400), in which he maintains that at the time of maximum excentricity of the Earth's orbit that hemisphere would be glaciated which had its *winter* in aphelion, as against Mr. Croll, whose theory is that the glaciated hemisphere would be that which had its *summer* in aphelion: it has occurred to me that I have never seen, in this discussion, any reference to the case of the planet Mars.

It may be worth while, therefore, in case no one has done so before, to point out how remarkable a parallel may be drawn between the state of affairs on that planet and on the earth, and how remarkable a confirmation the relative dimensions of the snow-caps on that planet appear to give of Mr. Murphy's view of the matter.

The excentricity of the orbit of the planet Mars is, as is well known, considerably greater than that of the earth's orbit. It is given by Sir John Herschel, in his *Outlines of Astronomy*, as 0·0931125. Leverrier's estimate of the maximum excentricity of the earth's orbit, as quoted by Mr. Croll, is 0·07075. The excentricity of Mars' orbit is therefore somewhat greater than the maximum excentricity of that of the earth. Again, the inclination of the axis of Mars to the perpendicular to the plane of its orbit is $28^{\circ} 51'$, which in a similar way is somewhat greater than the inclination of the earth's axis to the plane of the ecliptic, which is $23^{\circ} 27' 24''$. So that in both respects Mars offers a slight exaggeration of the conditions supposed to prevail in the case of the earth at the time under consideration. But these coincidences would be of no service to us, were they not supplemented by a third, most fortunate, coincidence. The axis of Mars, namely, is inclined at the present time towards one extremity of the axis of its orbit, and indeed towards the perihelion point; so that *as on the earth at the present time* the winter solstice of the Northern hemisphere coincides with perihelion, its summer solstice with aphelion, and the winter solstice of the Southern hemisphere coincides with aphelion, the summer solstice with perihelion. To show how nearly this is the case it will be sufficient to quote the lengths of the seasons. They are given as follows (from elements of the planet obtained by Maedler

and Sir William Herschel) by Mr. Breen, of the Cambridge Observatory. For the Northern hemisphere :—

Spring (vernal equinox to summer solstice)...	191½	Mars' Days.
Summer	181	" "
Harvest	149½	" "
Winter	147	" "

Thus we have on Mars at the present time all the conditions prevailing which the case under consideration requires—only exaggerated: the axial inclination is slightly greater, the excentricity is greater, and the year is longer, 687 of our days instead of 365. Yet the exaggeration, at any rate in the cases of the axial inclination and excentricity, is by no means so great as to destroy the comparison: on the contrary, it might be expected just to emphasize it. One more point before proceeding to actual observations of the polar snows of Mars; the distribution of land and sea on that planet is far more *equable* than on the earth, indeed there is a remarkable symmetry or likeness between the two hemispheres; at neither pole is there, apparently, an ocean of great extent, and, as is well known, land predominates largely over sea throughout, the two being in an estimated proportion of three or four to one. Thus, as far as we can at all judge, *geographical* causes of difference between the poles seem to be next to eliminated on Mars, and we are left to observe the results of the purely astronomical influences.

And they are remarkable. I am not, unfortunately, able at the present moment to lay my hand on the latest investigations into this subject; but I hope that the present paper may be the means of drawing more information from others. However, the main facts are clear enough. There is no great disproportion between the snow-caps of Mars; on the whole perhaps they are not far from equal. But there is a marked difference between their fluctuations: the Northern cap in fact changes slowly and little, the Southern fluctuates rapidly and much. The following facts will give an idea of their range and variation. In the year 1830 a favourable opportunity occurred for viewing the Southern cap; it was during the summer-time of that Pole, and the following measurements were taken, the times of year corresponding to our summer months being given (Breen's Planetary Worlds, p. 177) :—

June 16	Diameter of spot	12° 46'	July 7	Diameter of spot	6° 20'
— 23	"	11° 30'	— 9	"	5° 46'
— 26	"	7° 10'	— 19	"	8° 2'

Thus at a time of year corresponding to our July 9 the snow-cap fell to a minimum, and was only 5° 46' broad. It will be seen, too, that it fell off rapidly, having been more than twice as broad a month before. This would lead us to expect a large extent in winter-time: and we find that at the opposition of 1837 a good part of the Southern snow-cap was seen, although the South Pole was then turned away from the earth (and of course from the sun too); in fact it was estimated that it extended to a distance of 35° from the Pole, though this seems to have been an exceptional occasion.

In the year 1837 similar measurements were taken of the mini-

sum of the Northern snow-cap, with the following results, the times of year being again given in terms of our summer months:—

May 4	Diameter of spot	31° 24'	July 4	Diameter of spot	18° 24'
June 4	"	28° 0'	—12	"	15° 26'
—17	"	22° 54'	—20	"	18° 0'

Here we remark that the numbers concerned are much larger than—more than double, in fact—what they are for the South Pole; and we also remark a *slower* falling off towards the minimum. There is more difficulty about the maximum. In the winter season the edges of the spots, owing probably to the state of the atmosphere, are ill-defined; and, as already hinted, when Mars is in opposition, and therefore most favourably situated for observation, the wintry pole is turned *away* from us, and therefore at no time is there any opportunity of directly measuring the diameter of a snow-cap at its maximum. It is however certain, as mentioned above, that the maximum of the Northern cap is nothing extreme, and in all probability it is not far different from that of the Southern.

Certainly, to one who looks at the more general aspects of the question, nothing can be more obviously likely than that that should happen which does happen at the two poles of Mars: namely that that pole which endures the extremely hot summer and the extremely cold winter should present the extreme of fluctuation in its snow-cap, and that that pole which endures the moderately hot summer and the moderately cold winter should present a kind of moderation or mean in the fluctuation of its snow-cap. And I believe that the agreement of the behaviour of these caps with the more obvious theory of the behaviour of snow under such circumstances was one of the things which confirmed Maedler in the idea (for which confirmation is now no longer needed) that these white patches were indeed snow.

If Mars, then, gives us, in the variation of its polar snows, a true result of astronomical causes, it gives us a singular confirmation of Mr. Murphy's theory; for, though the results may be exaggerated on that planet, yet for that very reason they point out the more distinctly in what *direction* we should look for corresponding results on our own planet. They tell us, in fact, in unmistakable language, that, other things equal, our Northern hemisphere would now be the glaciated one. For though I have not spoken of *ice*-caps—since we have not such good reason for speaking of ice as of snow on Mars—yet no one would I suppose doubt that, as Mr. Murphy points out, the range of glaciation must lie within the summer range of snow, and that therefore the glaciated hemisphere must be that in which the summer range is largest, and the fluctuations (probably) least.

However, other things are not equal; and the able way in which Mr. Murphy points out that the fact that "the climate of the Southern hemisphere is on the whole maritime, and that of the Northern continental," counteracts the effects otherwise natural to the two hemispheres—that the geographical causes in fact overpower the astronomical ones—must command the assent of most readers.

There is one discrepancy, however, in Mr. Murphy's second paper; or an apparent one, probably due to a misapprehension on my part.

Admitting, in agreement with Mr. Croll, the existence of a Southern ice-cap on the earth at the present day, which he ascribes to geographical causes, while Mr. Croll ascribes it to astronomical, he still seems to concur with Mr. Croll in ascribing the great extent of the Southern oceans to the action of this ice-cap. For he says: "The ice-cap, as Mr. Croll has elaborately shown, will, by displacing the earth's centre of gravity, draw a greater share of ocean water to the glaciated hemisphere." Now it is obviously impossible to ascribe the oceanic character of our Southern hemisphere to the ice-cap, if the ice-cap is itself due to the ocean climate; so obviously indeed that it is impossible to believe Mr. Murphy intended to do so: yet it is difficult to read his words otherwise.

But there are difficulties every way. For if our Southern oceans are *not* mainly due to the ice-cap, it has to be considered that they *may* be due to a permanent internal displacement of the centre of gravity from the centre of figure of the earth—especially if Sir William Thomson is correct in supposing the interior of the earth to be solid. And if this is the case, then the tendency of water to collect in the Southern hemisphere will be a permanent one, and will always militate a good deal against the glaciation of the Northern hemisphere.

Certainly if the choice lay between supposing the great abundance of water in the Southern hemisphere to be due to a displacement of the earth's centre of gravity by a polar ice-cap, and supposing it due to a permanent internal displacement, I should prefer the latter supposition. And for this reason. The probability seems immensely strong that, in the process of formation of a body like the earth (whatever that process might be), the centre of gravity would *not* take the exact position of the centre of figure—immensely stronger than that it would. If the centre of gravity, however, were not in the centre of figure, it would necessarily lie in the axis of rotation: in fact, the axis of rotation would not be permanent unless it passed through the centre of gravity. Thus the centre of gravity would necessarily come to lie towards one or other of the poles; and thus would result a necessary, though indirect, connexion between the pole and the preponderance of water in its neighbourhood.¹

II.—NOTICE OF THE DISCOVERY OF UPPER DEVONIAN FOSSILS IN THE SHALES OF TORBAY.²

By JOHN EDWARD LEE, F.G.S., etc.

(PLATE V.)

IN the present state of our knowledge of the Devonian formation, it seems highly desirable, if possible, to correlate any of the beds in Devonshire with those of this wide-spread formation on the Continent.

¹ An excellent article on the planet Mars appeared in the Quarterly Journal of Science, vol. ii. for 1865, p. 369, from the pen of the late Prof. John Phillips, M.A., F.R.S., whose name is still dear to many of our geological readers.—EDIT. GEOL. MAG.

² The Editor expresses his regret that he has unwillingly detained this Notice without publication for several months, the plate not having been executed.—ED. GEOL. MAG.