in the ridges above. The blocks are angular, like the fragments in a breccia, and they rest irregularly one upon the other, supported in all positions by the angles and edges of those beneath.

They are not weathered to any extent, though the edges and points are in most cases slightly rounded; and the surface, also perceptibly worn, but only by the action of the atmosphere, is smooth and polished; and a very thin, extremely hard, white lichen, which spreads over nearly the whole of them, gives them the effect of being covered with a thin layer of ice.

Far down below, under the stones, one can hear the stream of water gurgling which occupies the axis of the valley; and here and there, where a space between the blocks is unusually large and clear, a quivering reflection is sent back from a stray sunbeam.

At the mouth of the valley the section of the "stone river" exposed by the sea is like that of a stone drain on a huge scale, the stream running in a channel arched over by loose stone blocks, or finding its way through the spaces among them. There is scarcely any higher vegetation on the "stone river;" the surface of every block is slippery and clear, except where here and there a little peaty soil has lodged in a cranny, and you find a few trailing spikes of *Nassauvia serpens*, or a few heads of the graceful drooping chrysanthemum-like *Chabræa suaveolens*.

These "stone-rivers" are looked upon with great wonder by the shifting population of the Falklands, and they are shown to visitors with many strange speculations as to their mode of formation. Their origin seems, however, to be obvious and simple enough, and on that account their study is all the more instructive; for they form an extreme case of a phenomenon which is of wide occurrence, and whose consequences are, I believe, very much underrated.

There can be no doubt that the blocks of quartzite in the valleys are derived from the bands of quartzite in the ridges above, for they correspond with them in every respect; the difficulty is to account for their flowing down the valley, for the slope from the ridge to the valley is often not more than six to eight degrees, and the slope of the valley itself only two or three, in either case much too low to cause blocks of that form either to slide or to roll down.

The process appears to be this. The beds of quartzite are of very different hardness; some are soft, passing into a crumbling sandstone; while others are so hard as to yield but little to ordinary weathering. The softer bands are worn away in process of time, and the compact quartzites are left as long projecting ridges along the crests and flanks of the hill-ranges. When the process of the disintegration of the softer layers has gone on for some time the support of their adjacent beds is taken away from the denuded quartzites, and they give way in the direction of the joints, and the fragments fall over upon the gentle slope of the hillside. The vegetation soon covers the fallen fragments and usually near the sloping outcross of the hard quartz, a slight inequality only in the surface of the turf indicates that the loose blocks are embedded beneath it. Once embedded in the vegetable soil a number of causes tend to make the whole soil-cap, heavy blocks included, creep down even the least slope. I will only mention one or two of these. There is constant contraction and expansion of the spongy vegetable mass going on, as it is saturated with water or comparatively dry; and while with the expansion the blocks slip infinitesimally down, the subsequent contraction cannot pull them up against their weight; the rain-water trickling down the slope is removing every movable particle from before them; the vegetable matter on which they are immediately resting is undergoing a perpetual process of interstitial decay and removal. In this way the blocks are gradually borne down the slope in the soil-cap and piled in the valley below. The only other question is how the soil is afterwards removed and the blocks left bare. This, I have no doubt, is effected by the stream in the valley altering its course from time to time, and washing away the soil from beneath.

This is a process which, in some of the great "stone-rivers" in the Falkland Islands, must have taken an enormous length of time. I fear that the extreme glacialists will see in it a danger to the universal application of their beloved theory to all cases of scratching and grooving. I have known too much of the action of ice to have the slightest doubt of its power; but I say that ice had no hand whatever in the production of these grand "moraines" in the Falkland Islands.

In the West Highlands of Scotland, and in many other parts of the world, I have often noticed that when a hill of such a rock as clay-slate comes down with a gentle slope, the outcrop of the vertical or highly-inclined slates covered with a thick layer of vegetable soil or drift containing imbedded blocks and boulders derived from higher levels, the slates are frequently first slightly bent downwards, then abruptly curved and broken, and frequently the lines of the fragments of the fractured beds of slate can be traced for a yard or two in the soil-cap, gradually becoming parallel with its surface, and passing down in the direction of its line of descent. These movements are probably extremely slow. I well remember many years ago observing a case, somewhere in the west of Scotland, where a stream had exposed a fine section of the soil-cap with the lines of broken-down and crushed slate-beds carried far down the slope. The whole effect was so graphically one of vigorous and irresistible movement that I examined carefully some cottages and old trees in hope of finding some evidence of twisting or other irregular dislocation, but there appeared to be none such. The movement, if it were sufficiently rapid to make a sign during the life-time of a cottage or a tree, evidently pervaded the whole mass uniformly.

It seems to me almost self-evident that wherever there is a slope, be it ever so gentle, the soil-cap must be in motion, be the motion ever so slow; and that it is dragging over the surface of the rock beneath the blocks and boulders which may be embedded in it, and frequently piling these in moraine-like masses, where the progress of the earth-glacier is partially arrested, as at the contracted mouth of a valley, when the water percolating through among them in time removes the intervening soil. As the avalanche is the catastrophe of ice-movement, so the land-slip is the catastrophe of the movement of the soil-cap.

As I have already said, I should be the last to undervalue the action of ice, or to doubt the abundant evidences of glacial action; but of this I feel convinced, that too little attention has been hitherto given to this parallel series of phenomena, which in many cases it will be found very difficult to discriminate; and that these phenomena must be carefully distinguished and eliminated before we can fully accept the grooving of rocks and the accumulation of moraines as complete evidence of a former existence of glacial conditions.

C. WYVILLE THOMSON

ON THE INFLUENCE OF GEOLOGICAL CHANGES ON THE EARTH'S AXIS OF ROTATION[1]

THE subject of the fixity or mobility of the earth's axis of rotation in that body, and the possibility of variations in the obliquity of the ecliptic has of late been attracting much attention. Sir W. Thomson referred shortly to it in his address at Glasgow last September,

[1] An account of a paper by G. H. Darwin, M.A., read before the Royal Society on November 23, 1876.

and Dr. Jules Carret has just published an ingenious book on the subject.[1]

The paper, of which the following is an abstract, is an attempt to investigate the results of the supposition that the earth is slowly changing its shape from internal causes. The first part is devoted to the mathematical consideration of the precession and nutations of a spheroid slowly undergoing such a change. It is shown that the obliquity of the ecliptic must have remained sensibly constant throughout geological history, and that even gigantic polar icecaps cannot have altered the position of the Arctic circle by so much as three inches; and this would be the most favourable redistribution of matter for producing that effect.

But a slow distortion of the earth would displace the principal axis of figure of the earth, and the axis of rotation would always sensibly follow the axis of figure. Thus the result would be a change in the geographical position of the poles, without any alteration of the diameters of the arctic circles, or in the width of the tropics.

For reasons, which cannot be given here, it is maintained that the earth would not be rigid enough to resist the effects of considerable departures from the figure of equilibrium, such as would arise from a wandering of the pole of figure from its initial position; and that readjustments to an approximate form of equilibrium would probably take place, at considerable intervals of time, impulsively by means of earthquakes. Such periodical adjustments would not sensibly modify the geographical path of the principal axis as due to terrestrial deformation.

The rest of the paper is given to the consideration of the kinematical question of the change in the geographical position of the pole, due to any distortion of the earth. It is assumed, in the first place, that the deformation is such that there is no change in the strata of equal density; and accordingly all suppositions as to the nature of the internal changes accompanying geological upheaval and subsidence are set aside. The forms of continents and depressions are investigated, which would cause the maximum deflection of the pole for elevations and depressions of given amounts.

In order to make numerical application to the case of the earth, some estimate is required of the extent to which it may have become distorted during any one geological period. From the consideration of certain facts, the author believes that from $\frac{1}{10}$ to $\frac{1}{20}$ of the whole earth's surface may have, from time to time, undergone a consentaneous rise or fall; and that the vertical rise or fall may be about 10,000 feet, or rather equivalent to about 10,000 feet, when allowance is made for the influx of the sea into depressed areas.

The first application given in this paper is to continents and seas of the most favourable shapes and positions. It may be here stated that if $\frac{1}{200}$ of the earth's surface is elevated by 10,000 feet, the deflection of the pole is $11\frac{1}{3}'$; if $\frac{1}{20}$ of the whole surface, $1° 46\frac{1}{2}'$; if $\frac{1}{10}$, $3° 17'$; and if $\frac{1}{2}$, $8° 4\frac{1}{2}'$.[2] In each case an equal area is supposed to fall simultaneously.

Other examples are also given for continents and seas which do not satisfy the maximum condition; in some the boundaries are abrupt cliffs, in others shelving.

The conclusion is arrived at that a single large geological change, such as those which obtain on the earth, is competent to produce an alteration in the position of the pole of *from one to three degrees of latitude*, on the hypothesis that there is no change in the law of internal density.

Various other hypotheses as to the nature of the internal changes accompanying the deformation of the earth are discussed.

1. It is shown that if upheaval and subsidence are due to a shrinking of the earth as a whole, but to the shrinking being quicker than the mean in some regions and slower in others, the results are the same as those previously attained.

2. The increase of surface-matter due to the deposit of marine strata also gives the same results.

3. The hypothesis that upheaval and subsidence are due to intumescence or contraction immediately under the regions in question is considered. Under certain special assumptions, too long to recapitulate, it is shown that the previous results must be largely reduced. It appears that if the swelling or contracting stratum is tolerably thin and at all near the surface, the deflection of the pole is reduced to quite an insignificant amount. Even if the intumescence extends right down to the centre of the earth in a cone bounded by the elevated region, the results would be only about $\frac{2}{3}$ of the former ones. Hence it appears that the earlier results can only be stated as the greatest possible for given superficial changes.

In conclusion it is pointed out that if the earth be quite rigid, no redistribution of matter in new continents could ever cause the deviation of the pole from its primitive position to exceed the limit of about 3°. But if the previously maintained view is correct, that the earth readjusts itself periodically to a new form of equilibrium, then there is a possibility of a cumulative effect; and the pole may have wandered some 10° or 15° from its primitive position, or have made a smaller excursion and returned to near its old place. No such cumulation is possible, however, with respect to the obliquity of the ecliptic.

It is suggested that possibly the glacial period may not have been really one of great cold, but that Europe and North America may have been then in a much higher latitude, and that on the pole retreating they were brought back again to the warmth. There seem to be, however, certain geological objections to this view.

[1] "Le Déplacement Polaire." Savy, Paris, 1877.

[2] The area of Africa is about ·059, and of South America about ·033 of the earth's surface.

THE NEW STAR IN CYGNUS[1]

ON January 9 the sky was unusually clear and the spectrum of Dr. Schmidt's Nova came out with amazing sharpness and brilliancy. In addition to the five bright lines seen on the 2nd, two others were detected, viz.:—

		Mill.mm.	
No. 1a	W. L.	594	Very narrow line.
,, 7	,,	414±	Excessively faint, but still certainly and repeatedly seen.

Between wave-lengths 655 and 594 the spectrum was certainly banded, and, most probably, there were two additional faint maxima of brilliancy in that interval. The continuous spectrum attains a maximum in the region about W. L. 525, and extends, though possibly not without interruption, as far as the faint line No. 7. The star was estimated of seventh magnitude, and was of a red colour with a decided tinge of purple, reminding me forcibly of the varieties of red produced by the quartz-plate in Zöllner's photometer. RALPH COPELAND

Lord Lindsay's Observatory, Dunecht, January 13

OUR ASTRONOMICAL COLUMN

THE NEW COMET.—The comet discovered by M. Borrelly, at Marseilles, on the morning of February 9, appears to have been found independently by Herr Pechüle at the Observatory of Copenhagen on the following morning. During the past week it has been making a pretty near approach to the earth, and had the weather been more favourable in Europe, it would probably have been very generally observed.

The following elements of the orbit have been calculated by Mr. Hind from the first observation by M. Borrelly, one at

[1] *Astronomische Nachrichten*, No. 2,117.