make a catalogue of stars (epoch 1900) of the stars in Carrington's Catalogue and of the stars in the Astronomische Gesellschaft Catalogues from the pole to Declination 64° N.

For financial reasons the Director of the Royal Alfred Observatory, Mauritius, is no longer able to send photographs of the Sun to supplement the series taken at Greenwich and Debra Dun, but H.M. Astronomer has arranged to take daily pictures of the Sun at the Cape Observatory, and it is expected that in the future the series will consist mainly of photographs taken at Greenwich and the Cape, supplemented only occasionally from other sources.

The solar activity as shown in the numbers and areas of sun-spots during the year 1908 has declined a little below the level attained in 1907, but has been only slightly inferior to that of 1906. No marked and long-continued diminution of activity has set in. The tendency, so marked in 1907, for long processions of groups to form following each other at short intervals along a parallel of solar latitude has been less apparent, and certain meridians have become practically quiescent. But there have been a number of fine groups observed during the year, particularly in the months of August and September, when four groups were visible to the naked eye; and the groups of faculæ have been about as numerous, if not quite as extensive, as in any year of the present maximum, so that there is no sign as yet of the near approach of the next minimum.

The mean temperature for the year 1908 was 49°·8, or 0°·2 above the average for the 65 years 1841–1905. The lowest temperature of the air recorded in the year was 12·1 on December 30. During the winter there were 54 days on which the temperature fell below 32°·0, being two less than the average number.

The number of hours of bright sunshine recorded during the twelve months ending 1909 April 30 by the Campbell-Stokes instrument, was 1752 out of 4456 hours during which the Sun was above the horizon. As contributing to this exceptionally high proportion, in three of the months, October, November, and April, the amount registered was the highest for those months in any of the years since the register was begun. In April it reached a percentage of 60 for the first time in any month.

The rainfall for the year ending 1909 April 30 was 22·60 inches, being 1·52 inches less than the average of the 65 years 1841–1905. The number of rainy days was 155.

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## NOTES.

Comet Notes.—A new comet ($a$ 1909), of the 11th magnitude, was discovered by Mr. Daniel, of Princeton, N.J., on June 15. As it has passed perihelion and is rapidly receding from both Earth and Sun, it is of little interest.

Elements by Dr. Kobold.

| | | |
|---|---|---|
| T ........ | 1909 June 5·35 Berlin M.T. | |
| ω ........ | 5° 4′ | 1909·0. |
| ☊ ........ | 305 21 | 1909·0. |
| *i* ........ | 51 54 | 1909·0. |
| log *q* ...... | 9·9252 | |

Ephemeris for Berlin Midnight.

| | R.A. h m s | N. Dec. ° ′ | | R.A. h m s | N. Dec. ° ′ |
|---|---|---|---|---|---|
| July 4.... | 2 42 41 | 51 1 | July 16.... | 3 35 45 | 59 52 |
| 8.... | 2 59 22 | 54 23 | 20.... | 3 55 17 | 62 3 |
| 12.... | 3 17 5 | 57 19 | 24.... | 4 15 36 | 63 53 |

Brightness: 0·50, July 4; 0·24, July 20; that at discovery = 1.

In *Ast. Nach.* 4332 Dr. Ebell re-opens the question of the identity of Comet 1908 *a* with Encke's Comet, but decides in the negative, both from the motion and brightness of 1908 *a*. The Cape observations in 1908 May indicate that M in the elements used by him requires a correction of −3′, which would only imply a correction of 1′ in the geocentric place in 1908 January. The brightness of Encke's Comet diminished enormously between 1904 and 1908. In 1904 it was 8·0, reduced to distance 1 from Earth and Sun, while in 1908 it was about 12·0, which would make it about 16 in 1908 January. As Comet 1908 *a* was of mag. 12·5, it was about 3½ magnitudes too bright for Encke.

Further computations are being made at Pulkova to examine whether 1908 *a* might be a fragment of Encke, separated from it by some catastrophe which appreciably altered its orbit.

*Ast. Nach.* 4331 contains a continuation of the ephemeris for Berlin midnight of Comet Morehouse (1908 *c*), which is probably still within telescopic reach.

Brightness: 0·12, July 7; 0·05, Sept. 1; that at discovery = 1.

| | R.A. h m s | S. Dec. ° ′ | | R.A. h m s | S. Dec. ° ′ |
|---|---|---|---|---|---|
| July 7.... | 9 41 29 | 21 37 | Aug. 8.... | 10 3 39 | 20 23 |
| 15.... | 9 47 2 | 21 1 | 16.... | 10 8 43 | 20 28 |
| 23.... | 9 52 36 | 20 38 | 24.... | 10 13 46 | 20 40 |
| 31.... | 9 58 6 | 20 26 | Sept. 1.... | 10 18 33 | 20 58 |

It will be seen that the object is only visible in the southern hemisphere.

*Ast. Nach.* 4330 contains the following elements and ephemeris for Winnecke's Comet. The comet made a near approach to Jupiter in Dec. 1906 (distance 0·41), and suffered large perturbations:—

| | | |
|---|---|---|
| Epoch and Osculation.. | 1909 Oct. 4·0 Berlin M.T. | |
| M .................. | 359° 9′ 13″ | |
| π .................. | 271 36 54 | 1909·0. |
| ☊ .................. | 99 21 20 | 1909·0. |
| i .................. | 18 16 58 | 1909·0. |
| φ .................. | 44 34 46 | |
| μ .................. | 602″·157 | |

Ephemeris for Berlin Midnight.

| Date. | R.A. | N. Dec. | Date. | R.A. | N. Dec. |
|---|---|---|---|---|---|
| | h m s | ° ′ | | h m s | ° ′ |
| July 4.... | 10 40 24 | 23 54 | Aug. 1.... | 11 41 37 | 15 42 |
| 8.... | 10 48 19 | 22 54 | 5.... | 11 51 30 | 14 17 |
| 12.... | 10 56 30 | 21 50 | 9.... | 12 1 41 | 12 48 |
| 16.... | 11 4 58 | 20 43 | 13.... | 12 12 10 | 11 15 |
| 20.... | 11 13 43 | 19 33 | 17.... | 12 22 59 | 9 38 |
| 24.... | 11 22 44 | 18 20 | 21.... | 12 34 9 | 7 56 |
| 28.... | 11 32 2 | 17 2 | 25.... | 12 45 39 | 6 10 |

The brightness reaches a maximum in mid-October. It will be as bright early in August as when first seen at the last observed return in 1898. A. C. D. C.

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Atmospheric Absorption of Light by Night and by Day.—Nearly two years ago the published results of some experiments made by M. Nordmann, of the Paris Observatory, occasioned some surprise, notwithstanding the high authority responsible for the publication (for the account appeared in M. Lœwy's report for 1906) and the known skill of M. Nordmann. The fact said to be derived from the observational evidence was, in short, this:—In observing by a special arrangement the light of different colours emitted by stars, it was found that when the zenith-distance is not greater than about 75 the absorption of the red and orange rays in the spectra is notably greater than the absorption of the blue and violet rays.

It will be noted that this is diametrically opposite to the observed facts in the case of the Sun, for then the blue and violet rays are absorbed by transmission through the air more than the red, and the suggestion seemed to be that the atmospheric transmission by night is different to what it is by day. It should be added that below the limit of zenith-distance mentioned, the phenomenon shows a tendency to approximate to the diurnal type, though the blue still continued to be much less absorbed relatively to the red than it is in the daytime.

In the May number of the *Astrophysical Journal* this suggestion has been controverted by Mr. F. W. Very, who offers another explanation of the observations. He says the effects may be due to a physiological cause. By night and in feeble light the eye is much more sensitive to violet than by day, and is relatively much more sensitive to light of enfeebled intensity in the violet than to equally enfeebled green or yellow light, whenever the luminous intensities are equally dim. As the star sinks lower and becomes fainter, the eye becomes relatively more sensitive to violet light; but even this is insufficient to reverse the great absorption of violet rays at very low altitudes of the stars, since M. Nordmann finds that when the zenith-distance exceeds 70°, the ordinary daylight law is restored.

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Greenwich Weather in June.—The exceptionally sunny April and the still more abnormally sunny May of 1909 have been followed by a June whose total of bright sunshine has been not only far below that of any June since the Campbell-Stokes recorder was placed in its present commanding position on the roof of the Octagon Room at Greenwich Observatory, but actually less than that of any June since sunshine records were begun at Greenwich in 1877 with a less efficient instrument and with an inferior exposure. The number of hours registered was 106·9, which compares very badly with the 261·6 hours of June 1908, and the 325·9 hours of May 1909. But if this is simply the "swing of the pendulum," the same metaphor can hardly apply to the temperature, which has also been abnormal, that in May being quite normal. The Junes of 1852, 1860, 1862, 1879, and 1882 had lower absolute maximum readings, but the mean maximum for June 1909 is the lowest on record, and probably the reduction of the thermograph registers will show that the mean temperature is also the lowest on record. Until the end of the last century only one June (1888) in the Greenwich records from 1841 showed so many as six days with maximum temperature lower than 60° F.; but with the new century there is another tale to tell, for in 1902 there were seven such days, in 1903 nine, and in 1909 ten.

The rainfall for June 1909, exceeding $3\frac{1}{2}$ inches, though distinctly heavy, is not nearly a record, having been exceeded in 1903 and in 1905 during the new century. The rainfall for the first six months of 1909 is rather above the normal. W. W. B.

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New Observatories in America.—We have had brought to our notice a booklet called the 'Bulletin of Pomona College,' an institution established in California about twenty years ago, which has recently had added to it the Frank Brackett Observatory, the gift of a former student, equipped with a 6-inch equatorial, a 3-inch transit-instrument, together with a 6-inch horizontal telescope, a spectrograph, and a cœlostat, the gifts of various persons. At the dedication ceremony an address on modern views on sun-spots was delivered by Prof. Hale, which we will ask his permission to republish shortly.

By a coincidence we hear at the same time of the institution of an observatory in connection with the Nantucket Maria Mitchell Association, which appears to be a literary and scientific association whose members and officers are all ladies. The fact that Miss Annie J. Cannon is *Chairman* (*sic*) of the Observatory Committee is probably one of the reasons why the work of the astronomy class was largely relating to Variable Stars.

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Time in Holland.—We have information, on the authority of the Director of the Utrecht Observatory, that the exact Amsterdam time now adopted in Holland is Greenwich Time $+19^m\ 32^s{\cdot}13$. This has been found from a recent determination of the difference of longitude between Leiden and Amsterdam ($1^m\ 35^s{\cdot}98$), in connection with the longitude of Leiden as given in the *N. A.* ($17^m\ 56^s{\cdot}15$).

So far as we know, Greenwich Time was never made legal time in Holland by the legislature, but it has been in general use since 1892, when the time of our meridian was also adopted and was made standard in Belgium.

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J. G. F. von Bohnenberger.—Dr. Hammer, Professor of Geodesy at Stuttgart, writes to me to point out that in my notice of the above (p. 255) I omitted to refer to his great work on the triangulation and general survey of Württemberg, which is one of the principal reasons why he considers a memorial should be erected to him at Stuttgart.

Bohnenberger's grandson (mentioned on p. 255 of the June number) is still living, a pastor "emeritus" at Kirchheim-unter-Teck. W. T. L.

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Our sincere congratulations are offered to Prof. H. F. Newall, who has been formally appointed to the Chair of Astrophysics in the University of Cambridge, to Dr. W. H. Maw, who has received the degree of LL.D. from the University of Glasgow, both of whom are Past Presidents of the Royal Astronomical Society; and also to Sir Joseph Larmor, D.Sc., Lucasian Professor of Mathematics in the University of Cambridge, Secretary of the Royal Society, and a Member of the Board of Visitors of Greenwich Observatory, who received the Order of Knighthood on June 25 (King's Birthday Honours).

Mr. J. Lunt, Astrophysical Assistant in the Cape of Good Hope Observatory, has received the Honorary Degree of D.Sc. in the Victoria University of Manchester.

Attention may be called to the second edition of a small book by Mr. D. W. Horner, 'Observing and Forecasting the Weather.' Though it is hardly sufficient to make anyone a very reliable weather-prophet, it contains a good deal of interesting meteorology. Published by Messrs. Witherby & Co. Post-free 7*d*.

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## From an Oxford Note-Book.

At the Oxford Encoenia on June 23 Honorary Degrees were conferred on the Right Hon. Earl Grey, Governor-General of Canada, the Hon. Oliver Wendell Holmes, Associate Justice of the U.S. Supreme Court, and son of the great man of the same name who was similarly honoured 23 years ago; Thomas Brock, R.A., sculptor; and George Ellery Hale, astronomer. The following is the text of the speeches made in presenting Dr. Hale; it was rather hard on Prof. Love to have to translate "spectroheliograph" and "Astrophysical Journal" into Latin, and he came out tolerably well with "instrumentum illud" and "Ephemeridem." The Vice-Chancellor could scarcely expect to find a reference to the newly-discovered solar vortices in Greek literature, but he cleverly found a line containing the word ἕλικας:—

### *By* Dr. Love *presenting* Dr. Hale *for the D.Sc.*

Inter Astronomos qui ea quae in aethere solem circumfuso geruntur investigant nemini cedit Georgius Ellery Hale. Qui vir primus omnium fabricatus est instrumentum illud, ad lucis e solis puncto quovis emissae naturam cognoscendam aptissimum, quo hodie utuntur omnes fere solis observatores. Hoc subsidio fretus potuit flammas illas excurrentes, quae solis defectu plerumque cernuntur, sole pleno quasi in pictura exprimere: mox plagas lucidissimo candore fulgentes, quas faculas vocant, eodem modo repraesentare. Idem nuper docuit procellis hunc aethera vexantibus tenuissimas materiae particulas quasi turbine quodam agitatas vim magneticam miro modo gignere: quae omnia nemo demonstrare potuit nisi excogitandi peritissimus, in observando patientissimus, in causis cognoscendis sagacissimus. Neque ei satis erat Naturae arcana reserare, sed Observatoria duo in orbe terrae maxima fere et instructissima condidit atque ornavit: idem Ephemeridem, in qua recentissima de siderum natura ubique reperta pervulgantur, conscribendam curavit. Sodalicium denique maximum instituit quo omnes omnibus ex terris huius militiae caeléstis contubernales congregarentur.

### *The* Vice-Chancellor *to* Dr. G. E. Hale.

Vir subtilissime, signorum caelestium servator acutissime, qui quomodo se radiis, ut ait poeta, retexerit aetherius sol, a turri speculationis hyalis ac dioptris tuis accuratissime dignoscere calles, quique

ἄστρων
*μαστεύων πυκινὰς ἀμφιδρόμους ἕλικας,*
*οὐκέτ' ἐπιψαύεις γαίης ποσὶν ἀλλὰ παρ' αὐτῷ*
*Ζανὶ θεοτρεφέος πίμπλασαι ἀμβροσίης,*

Ego auctoritate mea et totius Universitatis admitto te ad gradum Doctoris in Scientia honoris causa.

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Mr. K. Hirayama announces in *A. N.* 4332 "the existence of a large error in the results of the observations made with zenith telescopes, which may be explained as the effect of a gradual change of the flexure." The announcement is provisional, and Mr. Hirayama is prepared to investigate further; but the importance of such a source of error, if substantially confirmed, is obvious. The possibility of a slow settling down of the instrument after it has been pointed and clamped was indicated by the R−D observations at Greenwich, and it was a note to this effect (*Observatory*, xxx. p. 431) which suggested the present enquiry to Mr. Hirayama, though the remark made in that note was not actually applicable to the case in hand. At the time (some fifteen or sixteen years ago) when this clue to the R−D discordance at Greenwich was obtained, and special observations devised to elucidate it, the discordance itself diminished nearly to vanishing point in a most tantalizing way, and the experiments were naturally defeated. But it has recently revived a little, and possibly the investigation might now be more successful. At any rate Mr. K. Hirayama has shown good reason for further research.

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Prof. W. H. Pickering's courageous attempt to predict the place of a planet outside Neptune must have interested all of us, and we should all rejoice in the success of such an enterprise. It is an ungracious task to point out reasons for mistrusting his result; but as the search for a faint object over a sensible area of the sky is a laborious one, the possibilities of waste labour and disappointment must be taken into account. In *Nature* for June 19 Mr. P. H. Cowell has already sounded a note of warning, but he has not pointed out specifically the objections to Prof. Pickering's reasoning, which is undoubtedly impressive on a first reading; and a word or two on the subject may be said here. The general idea is that there are recognizable "kinks" in the motion of the disturbed planet when it passes conjunction with the disturbing. If we have two such kinks, the interval between them will indicate the period of the perturbing planet, and therefore its distance, and the problem becomes very simple. But if (as in the case of Uranus) the perturbed planet has only been under observation long enough to give us one kink, so that the period of the perturbing planet is left doubtful, nevertheless there is one epoch when it is easy to indicate the place of the unknown planet, namely the epoch of conjunction, when the kink is being made. If this favourable moment is allowed to go by, then prediction of place becomes more and more difficult.

Now this attractive method is not applicable, as was pointed out by Prof. Sampson (Mem. R. A. S. vol. liv. p. 149), and some of the objections to it are as follows:—Prof. Pickering follows Sir John Herschel in using elementary reasoning, that is, no complex mathematics. He resolves the disturbing force into two, one an accelerating force along the tangent to the orbit, and the other a normal force; he makes diagrams of their intensities, and he calls attention to the notable kinks in the diagrams near conjunction, which, he contends, will produce similar kinks in the motion. If this were so, his method might be successful, though there are still some pitfalls; but it will suffice here to point out two causes which contribute to the blotting out of these kinks when we pass from the force-diagram to the displacement produced, and which Prof. Pickering appears to have overlooked. Firstly, there is no doubt, at an epoch preceding conjunction, a hurrying-up of the inner planet, and after conjunction a dragging back; and it might seem as though the latter would undo the work of the former. So it might if the planet were moving in a straight line so that the velocity did not vary in direction; then the velocity acquired under the acceleration might be removed by the retardation. The same might still happen if the planet were running in a smooth curved groove, so that velocity acquired in one direction was fully diverted into another. But it is not, and consequently the acceleration along the tangent at one point, being in a quite different direction from the retardation at another point, cannot be annulled by it. The two directions may easily be at right angles, so that their effects would be independent.

Secondly, suppose for a moment that the planet *were* moving in a straight line or a groove, there would be a notable smoothing or flattening of the kink as we passed from the force-diagram to the displacement-diagram. Let us represent the acceleration in longitude by

$$\frac{d^2l}{dt^2} = A_1 \sin nt + A_2 \sin 2nt + A_3 \sin 3nt + \&c.$$

Then the displacement in longitude is found by integrating twice, and we get

$$-n^2 . \Delta l = A_1 \sin nt + \frac{1}{4} A_2 \sin 2nt + \frac{1}{9} A_3 \sin 3nt + \&c.,$$

so that the coefficients of the higher harmonics rapidly diminish. But it is just these higher harmonics which make the sharp kink in the force-diagram; a rough analysis of Prof. Pickering's first force-diagram, where the kink is most marked, gave

$$A_1 = 2, \quad A_2 = 8, \quad A_3 = 6, \quad A_4 = 4,$$

showing how the suddenness of the kink depends on the large values of the higher terms. On integrating the ratios would be entirely altered, and we should have, corresponding to a first term 2, the coefficients

$$\frac{1}{4} A_2 = 2, \quad \frac{1}{9} A_3 = \frac{2}{3}, \quad \frac{1}{16} A_4 = \frac{1}{4},$$

so that the kinkiness would be a good deal smoothed out. It would be further reduced by the consideration previously mentioned, and the difficulties of separating it from other emendations to the orbit (due to wrong eccentricity, &c.) would be greatly increased. Indeed when one looks at the analytical expressions for the perturbations, or draws them diagrammatically and sees how smooth they are, it is difficult to believe that the influence of an unknown planet can be picked out by eye from a diagram. Mr. Cowell gives it as his opinion that it ought to be now possible to discuss the observations of Neptune for the detection of planet O, and we shall all hope that he is right and that the planet may be found; but he is probably not thinking of graphical methods. Sir John Herschel thought that a graphical method might have sufficed to find Neptune, but Prof. Sampson, when he so carefully went through Adams's work, concluded that Sir John Herschel had been mistaken, for reasons which he indicated (*loc. cit.*). The above remarks are suggested chiefly by a talk with Prof. Sampson on the whole subject, though he is not responsible for the form in which they are cast, as he had not, at the time of my visit, had an opportunity of reading Prof. Pickering's memoir.

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On the same visit to Prof. Sampson I was privileged to see the complete impression of the discussion of the eclipses of Jupiter's satellites. The observations were made, as we know, at the Harvard Observatory by careful photometric methods, and appear as Part I. of a volume of the 'Harvard Annals,' of which the discussion forms Part II. The whole volume is in type, and we may hope that copies will soon be distributed. It must be a great satisfaction to Prof. Sampson to reach the end of a very arduous investigation: the reduction of the numerous observations themselves to a form convenient for discussion, the overhauling of the general theory of the satellites, which was in a state that might be mildly described as most untidy, the comparison of observation and theory to form huge normal equations with thirteen unknowns, the colossal solutions of these equations on square yards of paper, repeated several times, and finally the scrutiny of the residuals—all this makes up a labour of Hercules which can scarcely be dreamt of by those who have not had experiences of the same kind. There still remain the tables incorporating the results of the discussion for the use of future observers, and on these Prof. Sampson is already engaged. But in this final labour he has the stimulus of the end in sight.

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At the end of such an investigation there are two main possibilities—one that the final residuals are accidental, so that known causes sufficiently satisfy the observations; the other the reverse, so that we are led to suspect some unknown interference. In either case we may find satisfaction or discontent, according to temperament. It is satisfactory to be able to say "Well, we know all about *that*!" and it is from a different standpoint attractive to realize that "There is something new to be found out here." Prof. Sampson's residuals lead him to the latter conclusion, and his interpretation of them must not be anticipated. But I will venture to call the attention of those who receive the volume to the neat way in which he establishes the existence of a disturbing cause. Assuming, in the first instance, that the residuals are accidental, he finds the probable error and constructs the probable-error curve for comparison with that given by the actual residuals. The two curves do not agree; there is an excess of observed small residuals. He then proceeds to find a new and smaller probable error which gives a new curve, fitting the observational curve near the origin, but leaving an excess of large residuals which are reasonably to be assigned to an unknown cause. The method is simple enough when once stated, but is quite new to me, and of such obvious value that it seems worth emphasizing.

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FROM a correspondent who has clearly been examining:—

What is aberration?

Aberration is an error on account of the principle of optics causing a difference between true and apparent longitude of the Sun?

Aberration is a distortion of the outline of an object caused by conditions of the atmosphere.

What is dip?

Dip is the circle that the pole of the Earth describes in 33 years, by it moving.

Dip is the elevation of the observer below the horizon.

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MANY of us get casual letters from paradoxers at times, but our poverty protects us from such appeals as the following, which are culled from a large collection in the possession of the administrators of a certain fund:—

(1) Sum required, £2000 if received within 2 weeks; "if not it will increase": for the purpose of confirmatory tests for complete corroboration of already known facts of an important mystery in which I have been forced to investigate, my having now arrived at a certain stage where it is necessary absolutely and urgently that I receive an amount of money sufficient to confirm this by personal and special scientific investigation that can only be conducted by myself.

(2) Required £5000 for distinguished services to my fellow-countrymen. I propose to make known as Universal Knowledge the cause of the Aurora Borealis, and to make known when and where and how I discovered this.

(3) I is a person (the first one of his kind) with a electric brain, with radiating electric waves from this myn brain all around into all directions—this very strong, that I has the ability to impress millions of inhabitants inside of a few minutes with my feelings and with my thoughts. In fact myn brain is this very extraordinary strong that when the wind is at a restitude, the with electricity stored clouds are attracted to my brain, and the vibration or induction from my head causes the discharge of the clouds, which are travelling through the space of my influenz.

In 7 years in U.S. "I has been the cause of fearfull electric storms," &c., 150 persons killed and wounded. Sum required, "Not less than £300, but £500 adviceable," to construct an electric radiator.

(4) Gentlemen,—Is this offer international? If so, I undersigned may say: I can demonstrate Air Navigation by useing the same principle our Globe Earth moves through Space or Ether. . . . I am after Capital to help me to demonstrate this problem.