MIDDLETON'S

IMPEACHMENT OF MODERN

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THIS publication exposes the falsity of the book "Popular Astronomy," written by the Astronomer Royal, and it challenges the further use of that work as a school book. It disproves by Euclid, the possibility of the earth being either a sphere, or a spheroid! It expresses gravitation in geometrical form and shape, and thus with far more than the power of words only, it proves gravitation to be utterly impossible, as claimed by astronomers and others, and as part of either sphere or spheroid. This expression of gravitation in geometrical form and shape is beyond question as a true and correct expression of the Astronomer Royal's own sentences and of what must be on sphere or spheroid, and it claims to be the *first* genuine geometrical expression of gravitation in *form* and *shape*, and as a part of the geometry of sphere or spheroid.

This publication disproves the possibility of the Earth being either a sphere or spheroid, and this fact is made to rest on various and conclusive proofs.

THE
DEPOSITION OF E. E. MIDDLETON ESQ.

[My readers are advised that I have devoted the last seven years exclusively to this great question—that I have no profession or calling whatever, but am late of H.M.'s Regular Army (not Volunteers), being *late Lieutenant of H.M.'s 51st King's Own Light Infantry*.—I am, THE AUTHOR.]

THIS is to depose that the Book "Popular Astronomy" written by Sir George Biddulph Airy, K.C.B., Astronomer Royal, is a book which is not fairly representative of the science it pretends to teach and illustrate, but is of the nature of a gross cruelty published to the British public, and has been issued to the extent of nine editions, and in contempt of constant remonstrance urged through a long course of years, and which remonstrance has been lectured throughout the country, and has been published in book form, and as the subject of lectures has been additionally published by the press. The book "Popular Astronomy" is issued as a *school book*, and as authorized by an author holding the appointment of Astronomer Royal, is a book of the very greatest danger to the education of the youth of England; and the book is of the very greatest danger, as being insidiously destructive of the great fundamental truths of geometry, and out of harmony with the standard arts of painting and sketching, and being directly in parts and indirectly in other parts subversive of and destructive of the fundamental sanity of the English language itself; in that by the argumentative dogmas of this pernicious and most mischievous so-called science the very meanings of some of the best assured words of the English language are being construed in a manner dangerous to the sanity of rising generations.

The book "Popular Astronomy" contains printed matter as illustrations, and statements which are not the fair representations that they are advanced as being, and which as statements do not fairly admit of the important arguments that have been deduced from them, and which as illustrations do not follow in their construction the fundamental agreements recognised in the standard

arts of sketching and painting, and parts of the Earth, as invariably represented by those arts, and are not the true exposition and fair representation of certain observations of the heavenly bodies, which observations the Astronomer Royal has taken from time to time and has recorded in his book at page 122; but the diagram representing these observations is delineated at page 55 and is constructed with a *total disregard to scale*, so much so that the reader is led to believe by the division of the circumference between two stations, that those stations are distant apart according to their relative geographical positions as degrees of latitude, and to the extent of about ten degrees of latitude: but when the figure is critically examined, it is found that the space in between the stations equals some 32—34 degrees, and this excess is proved by producing the plumb-lines until they meet. The illustration as published bears that appearance which tends to support the false science of modern astronomy; but which appearance is in direct contradiction to the context of description given of those very observations at pages 58, 83, 84, and 122: for instance, the context places 12 degrees between the two stations: the diagram represents in one sense about ten degrees, but in reality places Shanklin in about latitude 41 degrees, as if in Spain, and also places Balta in about 73 degrees north, instead of 60 degrees and odd minutes.

Page 137 of the book also contains other observations which the Astronomer Royal has either in part observed, or the knowledge of which the Astronomer Royal has attained to in the natural course as accruing to his official authority, and which he has also, and in a most marked and most illegitimate manner, unfairly delineated and unfairly constructed, by the same device, of a shift of the latitudes: in this second instance Greenwich being posted in the South of France, and he has thus unfairly constructed this diagram to the great *educational detriment* of the *British Nation*, but at the same time to the emolument of the Astronomer Royal himself, though to the great personal injury and hardship of those able critics who have devoted the best years of their literary career, and in some cases have grown grey as ruined men, *ruined by their devotion* as the determined opponents of the irreligious falsity taught and inculcated in the Book of Popular Astronomy, and as the determined opponents of the idle unproved and unprovable theory, contained in all that which is taught and inculcated as the *unholy* and most *blasphemous* and CREATOR-INSULTING doctrine of a round globular world, rotating on an axis, and rushing forward through space around the sun.

THE EVIDENCE IN SUPPORT OF THE DEPOSITION.

The book "Popular Astronomy" is a work of paucity of language, confliction in statement, assumption of unnatural premises, general misstatement, incongruous, involved, and disconnected passages, together with illustrations, totally illegitimate and without excuse in their delineation and construction, and illustrations of that nature which (understanding them as I do, after having imitated them, as they are, and constructed them as they should be) a very strong sense of moderation leads me to term, gross cruelties published to the British public. It is hardly possible to open the book at any advanced page, without finding the critical faculty startled by those unmistakable traces which invariably betray the professor of the impossible in nature.

There is sufficient to find fault with in the work to enable even a superficial critic to put together without much trouble and as merely superficial error, a volume of very biting criticisms, and I may say that such a work has already been published.

This superficiality in error is so abundant that were I to introduce it into this deposition, I would be obliged to compile a large volume of superficial criticisms, which though very destructive of the author's reputation do not of necessity include that order of evidence needful to support the purpose of this deposition, which purpose I most moderately advance as the exposure of gross cruelties heedlessly persisted in, though in contempt of remonstrance, and destructive of the mental culture of the youth of England.

Beneath this superficiality in error there lies a vast quantity of most serious misrepresentation; a quantity in illustration alone so great that it is not my intention to introduce the whole even of this kind of evidence into this deposition, for were I to do so it would be unnecessarily lengthy; but it is my intention to introduce only certain portions, such as are at once sufficient and also the most unanswerable by the author, and further I have chosen as the chief support of my case, two especial illustrations which demand the very highest order of talent in order to permit the critic to pierce below their outer pretentious seeming, and to point the finger of exposure to their utterly wanton and totally inexcusable construction.

As regards that which is sufficient, I shall hope that any one of the diagrams, out of the number exposed in this deposition may be deemed sufficient justification for an application to stop the sale of the book "Popular Astronomy."

The first illustration that I advance in this deposition is that of the boy's hoop at page 70 of "Popular Astronomy!"
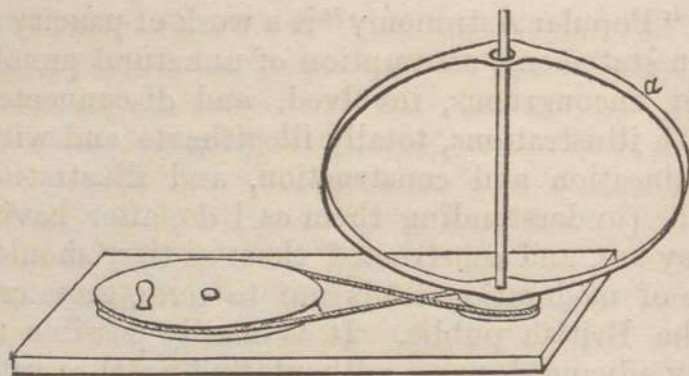
The Astronomer Royal advances this boy's hoop as the foundation for an argument that the Earth is flattened at the poles; and from this flattening he then proceeds to argue that it is oval in its shape, and that it revolves on an axis.
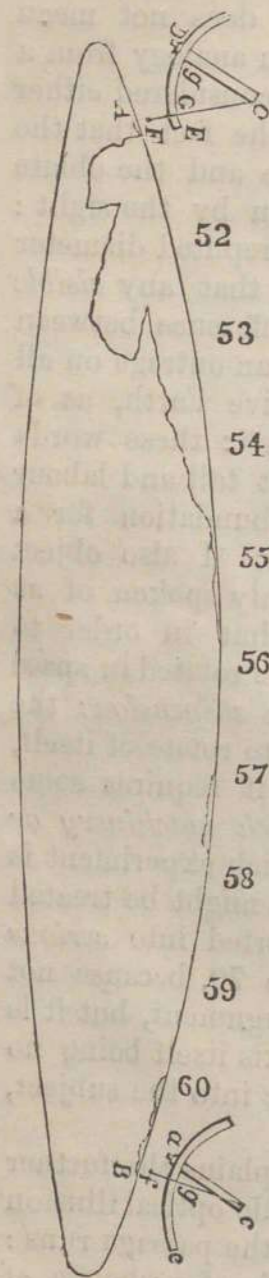
I wish it to be distinctly understood, before I enter on any criticism or refutation of this illustration, that *even if it were correct in itself*, I should still regard it as utterly inapplicable as affording any argument towards the shape of the Earth. The argument for the Earth's rotation is introduced in these words at page 70 of "Popular Astronomy." "If we take anything circular which admits of a change of form; if, for instance we mount a hoop as in Fig. 23, in such a manner that we can make it revolve rapidly, and whirl it round; then as soon as the motion of rotation takes place, the hoop becomes flattened."

I object to this statement, that the Astronomer Royal has taken an unfair advantage of an ordinary optical illusion, which is tolerably sure to attend the rapid rotation of circular and discular bodies, *such as a plate;* for such bodies in that they are only circular and not spherical, do present different optical illusions to the sight, according to the rate and manner of their rotation. It is very difficult, if not impossible to make a hoop or a plate rotate rapidly in space, and free from all support or connection that may disturb the judgment of the sight, without there being some oscillation in the object rotated, and that oscillation will, according to the amount of it, cause such an optical illusion, that a plate suspended by a cord and rapidly rotated will appear to flatten both at top and bottom to such extent as to resemble a barrel; but no man fit to hold an important office under Her Majesty's warrant would permit himself to be the dupe of such an

evident illusion, and one so easily proved to be such, for in order to prove the falsity of the absurd idea that the object rotated suffered a distortion, it is only necessary to stop the plate and examine it, on which it will be seen that no distortion whatever has taken place: no *flattening whatever*. Again I object to the illustration as unfair, in that the word circular does not mean spherical, and it is irrational to attempt to form an analogy from a flimsy hollow hoop to the massive solid Earth, considered either as a sphere or a spheroid, and I also advance the fact that the difference said to exist between the perfect sphere and the oblate spheroid is so slight as not to allow of detection by the sight: that difference being only some 27 miles, out of a reputed diameter of nearly 8,000 miles, and it therefore follows that any *visible flattening* of the hoop would *far overshoot* the difference between the sphere and the oblate spheroid. Again it is an outrage on all honest minded people to speak of the great massive Earth, as of something "which admits of a change of form": these words are unfair and inapplicable, considering the great toil and labour which is required in order to dig out a foundation for a mansion, and only to the depth of a few feet. I also object to the illustration, in that the Earth is commonly spoken of as a spherical body rotating freely in space, so that in order to afford any sound argument, the hoop should also be rotated in space *perfectly free* from any support, pressure, or *even suspension:* the fact being that an ordinary boy's hoop is too light to rotate of itself, when suspended from a twisted cord, and that it requires some machinery or pressure to make it spin; any *such machinery or pressure* at once *invalidating* the experiment; which experiment is in itself a gross palpable absurdity, and one which might be treated as childish if it were not so mischievously imported into *serious* matter. Further, I object to this figure at page 70, because not only is the hoop *improperly* used as a means of argument, but it is actually represented as rotated *on an axis*, the axis itself being no part of the hoop, and being an *unfair introduction* into the subject, in order to *influence* the observer.

Page 87 of "Popular Astronomy" at once explains the further unfair advantage which is taken not only of the idle optical illusion of the so-called flattening, but of the axis itself; the passage runs: "If we take anything circular that is susceptible of a change of shape, and whirl it round an axis, it will change from a circle into an oval; we think therefore that even supposing we had nothing else to guide us, there is good reason to infer, from the oval shape of the Earth, that it does turn on an axis." In this passage *the axis* is

introduced, whereas in the former passage it was *not even mentioned*. The whole sentence is a true specimen of the false, pretentious and illegitimate style of argument, invariably used throughout the whole book. The Astronomer Royal commences by being the dupe of a *feeble optical illusion*. Secondly, he argues from that optical illusion that the Earth is flattened at the pole; thirdly, having thus assumed the oval shape of the Earth, he argues *back again* from that secondary assumption to *the axis* which we see most improperly imported into the figure, and with the double purpose of *spinning the hoop*, and then impressing the audience and reader of the book that there is an axis, and that the Earth turns on it : to sum up, the Astronomer Royal argues *from* the feeble optical illusion, or falsely contrived experiment, *to* the flattening and consequent oval shape of the Earth, and then he argues back from the oval shape to the rotating on an assumed axis. I urge that the whole argument is a standing disgrace to the author of it, and that it is highly detrimental to the mental culture of the youth of this country.

The next illustration to which I call attention is that at page 55. This illustration, together with all the reasoning founded upon it, is false and of the nature of a gross cruelty published to the British public and is destructive of the healthy intellect of the youth of England.

The illustration is to represent observations taken with an instrument called the Zenith Sector, from two stations, the one marked A representing Shanklin in the Isle of Wight, and the other marked B representing Balta in the Shetland Islands : the object of the observations was to determine the curvature of the Earth. The Zenith Sector is a portable instrument, and is thus described at page 55, with a sketch of the instrument at page 56 :—" Fig. 19—a telescope swinging upon pivots A B, and having attached to it an arc CDE graduated into degrees and minutes. There is a plumb-line CF connected with the upper end of the telescope or with one of the pivots."

It must be distinctly understood that these observations were taken with this instrument, for the set purpose of determining the shape of the Earth ; and it must also be understood, that in order that the Earth should appear to be curved, it was necessary that the plumb-lines should hang at an angle to each other, at the different stations, a *method of hanging impossible in nature ;* but in order to bear out *another part* of the Astronomical theory to the effect that lines of sight to a fixed star *are parallel,—* it was necessary that the telescopes should be represented as parallel. Both these most grave and most serious misrepresentations are set forth in his figure by the author of " Popular Astronomy," and the

Fig. 19.

latitudes of Balta and Shanklin have been shifted, and thus aid to enforce these misrepresentations which are insisted upon in pages 56—57, and that in direct contradiction to his own admission of the facts of the case as they actually occurred, and as printed and published at page 122 of his work " Popular Astronomy" : further, he has drawn the diagram in neglect of the fact that the position of the plumb-line in nature is invariably parallel to a former position, and that in some measure it (in this experiment) bore a relation to the telescope, and that relation would have depended upon that of the telescope, and that as the telescope was parallel to its former position, so the plumb-line must also have been parallel to its former position : in fact the placing of the telescope together with the fall of the plumb-line, would have been a mere repetition at Balta to what it was at Shanklin, had the following printed statement been the truth as regards the parallel of the telescope : such printed statement is however flatly contradicted by another printed statement at page 122, and the known facts of observation prove it false.

At pages 56 and 57 the Astronomer Royal states : " The plumb-line at A would hang in the direction CGF, and that at B in the direction *c y f.* The place of the star however which I observe is unaltered. The telescope is to be pointed in the same direction whether we use it at Shanklin or Balta : or the line CD is parallel to *c d.*" In this statement the Astronomer Royal has advanced just that state of the case which would be necessary in order to support his preconceived views of a round globular world, but also just that state of the observations which was exactly contrary to the facts of the case as stated by himself at page 122 in these words, " that the direction of the telescope when pointed to
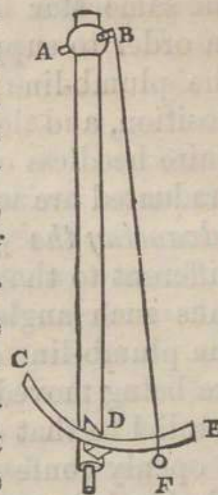
the same star is apparently different." It is absolutely necessary in order to support the popular delusion of a globular world, that the plumb-line should appear to hang at an angle to its former position, and the Astronomer Royal has stated that such was the case, quite heedless of the fact, that any difference in the reading on the graduated arc would be caused by the movement of the telescope *advancing the graduated arc past the plumb-line*, and to an angle different to that in which it was placed at the first station, and that thus such angle was not caused by any impossible inclination of the plumb-line out of the true plumb-fall, but by the graduated arc being moved past the plumb-line, which latter could only be parallel to that of its former position. This true state of the case is openly confessed to at page 122, but no sooner has the author admitted that the plumb-line was *"the same"* (parallel), whereas the telescope shifted, than he immediately and frivolously suppresses the truth of the case, and bases his ultimate decision on his former erroneous statements.

At page 122 he says, "The direction of the telescope is really the same at the two places, but the direction of the plumb-line is different at the two places." So far the author bears out his former remarks, but he *continues*, and now comes the genuine truth of the position: he adds, "But if we consider it only as a matter of observation at each of the two places, then we fancy that the direction of the plumb-line is *apparently the same*, at the two places, and that the direction of the telescope is apparently different. Thus the direction of the telescope when pointed to the same star is apparently different at Shanklin from what it is at Balta."

Here then at last we have a plain straightforward admission that the plumb-line was the same (parallel), and that the telescope was different, as must have been the case, in that it would be quite absurd, and against reason to expect to see the same fixed star from two places 830 miles apart, and through parallel telescopes, but the directions of the telescope would of necessity be inclined at an angle towards each other.

The Astronomer Royal having made the above admission of the facts as they really appeared, instantly suppressed them in the following words : "But in point of fact the direction of the telescope is the same at both, and the direction of the plumb-line or the direction in which a stone would fall is different at the two places." I object to the term "point of fact" and demand to know what "fact" is alluded to ? This latter decision of the author is clearly opposed to the facts of the case as admitted and printed, as also published in his own book, "Popular Astronomy," at

page 122. I will now point out his words : "But if we consider it only as a matter of observation at each of the places." I ask in profound astonishment, in what other light than as only a matter of observation, has the Astronomer Royal to regard the actual process, and the illustration explanatory of the same? Plainly, most plainly, none whatever ! Its observations were the primary as legitimate object sought for, and for which the telescope was placed in position, and those observations were to be considered solely as observations ; the directions of the telescope as the genuine directions of the telescope ; and the directions of the plumb-line as the genuine directions of the plumb-line ; and had this been adhered to, a truthful as natural and consequent decision would have been arrived at, and an illustration in harmony with such decision might have been and should have been constructed ; and it would then have been seen from a correct illustration, that the telescope *was inclined to its former* position, and that the *plumb-line was parallel to its former position*, and that the Earth is *an extended surface* and neither the sphere nor the spheroid it has been heedlessly delineated, *in spite of the plainest facts to the contrary* and professedly gathered by the Astronomer Royal in person and on the spot. To turn to the Astronomer Royal's illustration at page 55, the latitudes of Balta and Shanklin have been shifted from the proper latitudes, and this shift of the latitudes shows that this important demonstration has been constructed with a most unwarrantable and extraordinary license. It is not my intention to pursue the unwarrantable and extraordinary license of these false latitudes, beyond plain exposition of their immediate effects, because were I to bestow a deeper criticism on these latitudes, such criticism (in this figure) would lead to perplexing niceties in geometricial construction ; therefore, as a license of an exactly similar kind, has been taken in a further illustration, and in connection with which illustration the keenest criticism is happily free from any possible perplexity as geometrical criticism is happily free from any possible perplexity as geometrical niceties of construction, I shall leave such criticism until that further illustration is treated of. I hold that geometrical demonstration should be in keeping with the context of description, and should be consistent in itself ; and I hold that correctness is especially binding in a work of this authority and importance. The Astronomer Royal mentions the angle found by the observations as 12 degrees, but he has constructed his figure in utter contempt of that angle, and has used one between 33—34 degrees, and this angle necessitates 33—34 degrees of latitude between Balta and Shanklin ; for the approximated angle between the tele-

scopes, from their respective horizons, is (in the figure) the same as the number of degrees of latitude between the two stations: it therefore follows that the printed figure represents Shanklin as in Spain, as in latitude about 41 degrees and Balta as in latitude about 73 degrees: I have prepared a demonstration which shows the falsity of the latitudes very clearly. This diagram is a demonstration of the Astronomer Royal's figure, and contains parallel lines of observation, as directions of the telescope, and I may just mention here, that (to the best of my careful research) all lines of observation to luminaries for latitude, form parallel lines, when represented on a sphere, and this fact alone forms a conclusive proof, that the Earth is not a sphere: those lines on the oblate spheroid diverge outwards like the fingers from the palm of the hand, when not in contact. These geometrical facts should have taught the astronomers long ago, that the Earth must be an extended surface. The Astronomer Royal should have constructed his figure placing Balta and Shanklin in their true latitudes, and by adjusting his scale he might have had as much room between the stations as he chose: those latitudes are nearly if not exactly ten degrees apart, so that the distance between the stations should be nearer 600 miles than 830 as stated by the Astronomer Royal, and the angle between those stations, as read from the assumed different horizons, ought to be nearly 10 degrees, and not 12 degrees: moreover on the theory of the oblate spheroid, the degrees of latitude would perforce decrease towards the pole, so that the distance between Balta and Shanklin by latitude, would be even less than 600 miles, and this number divided by 12 (as stated by the Astronomer Royal) would not give a result as 69 miles for a degree of latitude; nor will 12 degrees give the desired result, even if the 600 miles, are treated as nautical miles and reduced to statute miles, with the slight deduction for polar depression and then read as 688 miles: so that in every sense the Astronomer Royal's figure, and the deductions formed from it, with the measure of the degree, and of the Earth's diameter, and general size, as found from that degree, together with its said globular shape, and all other arguments based on the figure are false and misleading.

I now challenge a comparison of one of my diagrams marked A with that printed at page 55 of "Popular Astronomy." In the figure, per book, I found that the angles made by the telescope with the horizon of each place was for Balta about 73 degrees and that for Shanklin about 39—41 degrees which gives a difference of 32—34 degrees as the angular difference between the two stations, and not the professed 12 degrees, as stated in the context of the book

"Popular Astronomy." This angle of 32—34 degrees is to be found not only from the direction of the telescopes, but from the convergence of the plumb-lines,—though even as read from these latter, it is necessary to leave some margin of expression and to read the angle as 32—34 degrees. The angle of 32 degrees is ample to show the extraordinary license which has been adopted in altering the angle of 12 degrees quoted in the context, to such an enormous angle of even 32 degrees. The angle of 32 degrees is sufficient for all present purposes; for if the angle of 33 degrees be taken, then another degree would be found on the circumference as marking the different reading between the telescopes at their respective horizons, and that extra degree would show those telescopes parallel as now at 32 degrees. A glance at the printed figure will show the telescope of A in quite an impossible position, as removed some 50 miles or more up in the air, and as remote from the position A, and this position of the telescope renders the measurement of the angle at A a little doubtful, but as between 39—41 degrees: nevertheless there are other ways of determining the angle should an extreme nicety of expression be aimed at, and should such suit the advancing. Again the plumb-lines are represented as not parallel to each other, but as hanging in *strained impossible* positions *relatively to that general level or datum line* which is invariably to be found in *any genuine representation* of any portion of Earth's surface, as recognised by the standard arts of painting and sketching: either the standard arts of painting and sketching are false, and totally false, so as to render worthless all the landscape painting in the kingdom, or else the Astronomer Royal has falsely represented his diagram. No sane person whatever would question the general correctness of the standard arts as painting and sketching in their delineation of a general level or datum line, and that whether the picture represent a lake or the full distance of the Suez Canal: it therefore follows that the printed and published diagram is a false and mischievous construction. Moreover, the fact that the Astronomer Royal delineates his telescopes as parallel *shows that he is treating of a sphere* and not of an oblate spheroid, though his whole book is an *assertion* of the *oblate spheroid*.

There still remains one point to be cleared up in connection with this figure at page 55, and that is that the angle of 12 degrees between the two stations could only be found by the difference of the readings of the telescopes; and here I also draw attention to the Zenith Sector itself, and I challenge the legality of its use, for I fail to see in it anything but an instrument especially calculated

by its construction to introduce doubt and uncertainty into observations which might have been and should have been taken either by the mural circle or the Navigator's sextant. The Zenith Sector, with its plumb-line appears to me an instrument which should never have been used at all. I also fail to receive the Astronomer Royal's method of finding that angle by what he *calls the direction of the vertical* at page 56, in one part of that page, but at the top of the same page, calls the direction of the perpendicular to the horizon, and thus shows that he means the position of the plumb-line. It is easy to follow him, and see that he starts from the assumption of a round world, and argues that the plumb-line will fall at an angle to its former position; but such argument is *not based on his observations, and the use of his faculties*, and could only be excusable in a very ignorant weak minded Astronomer *who had never heard of Euclid*. He wishes us to believe that it is the shift of the plumb-line that causes the angle of 12 degrees, but such could not be the case, for though the plumb-line may be shifted as the telescope is moved, it will only be shifted on to *another parallel*, as in my diagram marked B, and by no possibility could the shift of the plumb-line include an angle or *afford any angular measure whatever*: the angular measure found could be produced only by the shift of the telescope, or the inclined angle of the telescope, and that angle is read on the graduated arc, which arc is moved past the plumb-line by the inclination of the telescope; *and by this process alone* is the angle of 12 or other angle to be found. Had a proper instrument been used instead of the Zenith Sector with its plumb-line, there would have been no ground whatever for the introduction of the plumb-line into the experiment. Further, had the telescope of the Zenith Sector used at Shanklin been then and there fixed by a screw, and the plumb-line also pinned in its then position, and had the instrument, then as fixed, been set up at Balta, it follows that both the telescope and plumb-line would in nature have been parallel to the positions at Shanklin, and it would at once have been found necessary to unscrew the telescope and unfasten the plumb-line, or the reading on the graduated arc would have been just the same as that read at Shanklin, and moreover the star could not have been seen through the telescope at Balta! The assertion that the plumb-line hangs at an angle with its former position is thus shown to be absurd, *and is not based upon observation*, but is based upon the *idle assumption* of a spherical world, which would necessitate angular plumb-lines instead of parallel ones, and which may be better understood as the spokes of a

carriage wheel, or radii from the centre of the figure which I am now going to discuss as at page 57 of "Popular Astronomy."

TO PROVE THE CONVERGENCE OF THE PLUMB-LINE IMPOSSIBLE.

By this figure I am going to prove that the plumb-lines from two stations on the Earth as A and B cannot possibly include any angle whatever, and that when produced, they will not meet as the Astronomer Royal has supposed and has most improperly treated of.

I have just pointed out that the Astronomer Royal is in error in his statement that the plumb-line at the two stations gives an angle as between those plumb-lines, and that he has thus acquired the angle 12 degrees. *Nothing of the sort is possible.* The position of the plumb-line *is the vertical itself*, and when that plumb-line is moved on to a second position, it merely marks *a second position* for the vertical, or perpendicular position, which it

Fig. 20.

(the plumb-line) occupies, and which position is perforce parallel to its former one *and cannot include any angle whatever*.* What the plumb-line does is to mark the reading on the graduated arc, and thus shows how much the telescope has been inclined from its former position, which is quite a different matter: *it shows the angle between the telescopes*. The truth of this statement that the plumb-lines are parallel, and cannot include any angle whatever, *is very easily proved*, and I am now going to prove it, and had I the smallest doubt whatever of the very great ease by which this point can be settled I would most assuredly not have advanced it at all. It is however a *most vital point* and one in itself *quite conclusive* of the shape of the Earth as proving *the impossibility* of the Earth being spherical or spheroidal as is supposed. The Astronomer explains the use to which he intends to put this figure, and his words are: "Now then I have arrived at something which I can use for taking the dimensions of the Earth." In these few words, the Astronomer Royal refers to the angle of 12 degrees. He continues, "In the way that I have described I have the inclination between the line which is perpendicular to the surface at A Figure 20, and the line
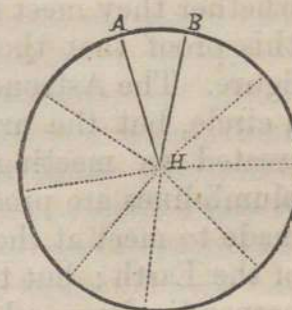
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* See my Diagram, marked B, in which the are plumb-lines shown in their proper positions as parallel and the telescope is inclined.

which is perpendicular to the surface at B." This remark is in error, as he has not attained the angle, in the way that he has described, but he attained the angle or some angle, by the inclination of the telescopes, which in turn moved the graduated arc across the plumb-line, which plumb-line was hanging plumb up and down and parallel to all its former positions, *and nothing else is possible*. The author's next words are : "If I continue these two lines downwards until they meet at a great distance below as at H, I shall get a centre from which I may," &c. It is sufficient for my purpose that the plumb-lines are produced till they meet, and it is not a matter of any importance as to whether they meet exactly at the centre or whether they meet slightly out of the centre : it is sufficient for this proof that those lines meet, when produced as in the printed figure. The Astronomer's figure is a circle, and thus I shall treat of a circle, but the argument would be just the same were the lines treated as meeting in a spheroid. In the published figure the plumb-lines are produced towards the centre of the Earth, and are made to meet at the point H, in a plane passing through the centre of the Earth : but these plumb-lines must be and are considered as perpendiculars, and they are thus produced in accordance *with the practice in geometry* of producing lines at pleasure, and when so produced they meet at the same point H in the plane of H, *and on the same side of it*, and which plane passes through the centre of the Earth ; but this is directly contrary to Euclid, *Book XI, proposition 13, in which it is distinctly proved that there *cannot be two perpendiculars* from the *same* point in a given plane, *and on the same side of it ;* and there can be but *one* perpendicular *to a plane* from a point without that plane. It therefore follows that the position of the plumb-lines as in the diagram, page 55, and also as produced and meeting at the point H in the figure at page 57 are false to geometry as the positions of perpendiculars, and are proved to be thus false by the Book of Euclid XI, proposition 13 ; and it is also certain that the position of the plumb-line in nature, and as constantly demonstrated in the various trades of ship building, of house building, and practical engineering *is the position of the perpendicular*, and therefore it follows that the diagram at page 55 of " Popular Astronomy" is both *false* and *impossible*, and that its further demonstration in Figure 20, page 57, with those line *produced* and *meeting* in the point H is *false to geometry* in its result, *false to Euclid* in its result, and *false* as *impossible* of *practical*

* See the Diagram of Euclid, Book XI, proposition 13,—marked.

exposition as part of the Earth. It should be remembered that with regard to matters which fall *within* the domain of practical mechanical exposition, that which is *distinctly impossible* of such mechanical exposition *has no standing ground as theory on paper*. A diagram of Euclid, Book XI., proposition 13, is attached to this deposition, and from that diagram it can be seen that Euclid's figure allows of an exact representation of the Astronomer Royal's printed figure, and permits of a circle being drawn from the central point through the two radii BC just as in the Astronomer Royal's figure, and as contended for at page 57 in these words : "I shall get a centre from which I may make a sweep to describe the curvature of this part." When these words of the Astronomer Royal are applied to the figure of Euclid, Book XI., proposition 13, a *practical reproduction* of the Astronomer Royal's figure is *instantly brought about*, and as may be seen from the diagram which I annex in illustration of this particular feature. Again we know from every-day experience, *and Euclid lays it down* also, that * parallel lines if produced will not meet. The Professor has *confessed* at page 122 that these plumb-lines are parallel, or in his words: "*that the direction of the plumb-line is apparently the same ;*" consequently the lines would not meet as he has represented them at page 57 and in the point H, and therefore both his diagrams are false, not only false, but without any just excuse for their existence ; and all the arguments based upon them are false, as is the value of the degree, and the measure of the Earth, and all other matters depending upon the construction of the figures at pages 55 and 57. There are other matters in connection with these figures, and with the distance of 830 miles and important matters also, but I have determined to dispense with any mention of them here, and at present, as criticism which might lead to needless, and possibly misunderstood discussion. I am at this point satisfied that the errors of these figures are such that I am able to advance most crucial criticism which admits of *no possible as sound truthful refutation*. I am therefore more than contented to waive all those numerous points which, though in error, might have led to the perplexity of those whose duty it is to adjudicate in this matter : *this most serious and most grave matter*. I now advance to the consideration of the next illustration.

* This clause is appended in order to bring out into strong light the confliction between the diagram and the context of description, and also to add further point to the conflictions of the context itself.

THE THIRD ILLUSTRATION.

This illustration at page 137 of "Popular Astronomy," ninth edition, is utterly false and quite impossible of *legitimate* construction, as showing a result in illustration resembling the printed figure; and as permitting a figure to be constructed such that the height of the moon could be found by the method of parallax, and as stated by the Astronomer Royal, or even as permitting the height of the moon to be found by any convergence of the observation from Greenwich with the luminary. This illustration is not only false, but is most *gravely in error*, and shows a more determined and more deliberate license in construction than the former delineation at page 55 : so much is this the case that I am indeed being very gentle in speech (from a comparative point of view) when I term this illustration a most exceedingly cruel publication, and one which cannot be in any sense excused as accidentally produced and sold to the British public. This illustration is also constructed in the most supreme contempt of, and contradiction of, the context of the book as stated in pages 136—142 inclusive. The following illustration is one by which the author of "Popular Astronomy" attempts to bear out geometrically the distance of the moon as found from the supposed shadow of the Earth and as stated in an earlier part of his work.
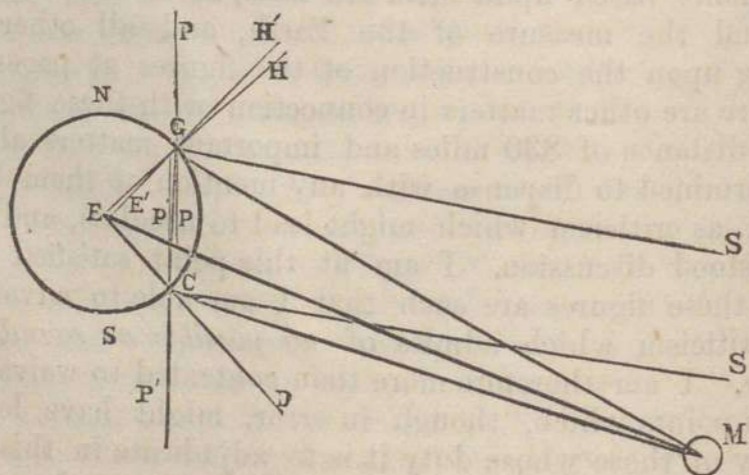

Fig. 40.

Pages 136—142 inclusive are descriptive of this figure and the author's remarks on it, and at page 136, he opens this explanation by saying, "Now in Figure 40 let GC be the earth, M the moon. I wish to measure the distance of the moon from the earth. I have two observatories from which I view the

moon. One of them, G, we will suppose to be at Greenwich or at Cambridge ; the other, C, to be at the Cape of Good Hope." So far the Astronomer's language may be allowed to pass unchallenged, but his next words comprise an astounding fallacy and an astounding jump into matter, which is entirely disconnected with the subject of the moon's height. I will just add, that *this wild license of disconnection* is not at all singular as respects the book "Popular Astronomy." His words are :—"In remarking on the grounds of a person's judgment of the distance of an object, D, Figure 38, as observed with the two eyes, I said that it depends on this : that the object is seen by the two eyes in two different directions." On this remark of the astronomers I have at once to point out that he overlooks the fact that the moon, though seen in two different directions, is so seen by two *different pairs* of eyes and *not by two eyes only*, or by one person only. Moreover the moon's distance is found by calculation, but not in any way by the *judgment of distance* by the eye, as the opening words of his last sentence would seem to imply ; and as if a man were *judging distance* on a target for rifle-shooting. His figure at page 115 is quite *superfluous*, as we do not require to be told that the eyes focus to a point when we choose to make them, or when we leave Nature to control our sight. The Astronomer Royal next inquires
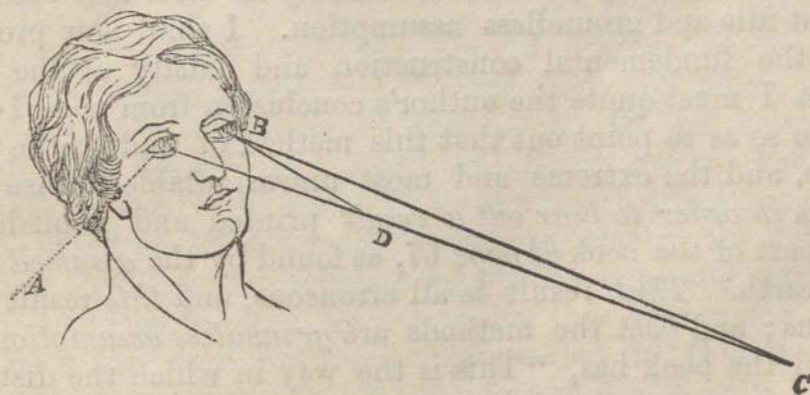

Fig. 38.

—"How can that difference be ascertained ? It can be ascertained by observing at each of these observatories the Polar distance of the moon." Of this method of measurement I have to observe that it is utterly and totally false, and it is most unjustifiably conducted by the Astronomer Royal on assumptions, and especially as regards the South Pole, of the very existence of which there is every reasonable doubt ; and on unjustifiable assertion as regards the value of the angle M at the moon—an angle which he asserts

is the difference between two angles with which it has *no connection* whatever; and also as an angle which he asserts is the measure between two Polar distances exceeding 180 degrees: the fact being that the South Polar distances are in themselves assumption as regards the South Pole; but secondly, that when calculated and borrowed as an assumption and properly set up to what the figure would be if true, the Polar distances do not exceed 180 degrees. When the construction is true to a flat Earth I do not find any excess whatever over 180 degrees. When the construction is correctly drawn as on a sphere the lines *will not meet* at the moon's declination. When the construction is correct as on an oblate spheroid the lines *diverge* from the moon, and when the figure is drawn with the extraordinary license of the book then the Polar distances largely exceed 180 degrees, as set off from the true positions of the observers on the spot and spots where they must appear as true *latitudes* of Greenwich and of the Cape of Good Hope, and on the *surface* of the Earth, and not as Polar distances improperly set off from the assumed *centre* of an assumed spherical or spheroidal figure; and such excess is greatly in excess of the $1\frac{1}{2}$ degrees claimed as the excess, and the parallactic measure of the angle at M. So far, then, I have pointed out that the Astronomer Royal's method of finding the height by parallax is entirely in error and based upon the most idle and groundless assumption. I shall now proceed to expose the fundamental construction and falsity of the figure; but first I must quote the author's conclusion from page 141, and this I do so as to point out that this method of finding the moon's distance, and the extreme and most unwarrantable license of this figure, is *in order to bear out a result* printed and published in *a former* part of the book *at page* 67, as found by the *assumed shadow* of the earth. *That* result is all erroneous, and *this* result is also erroneous; and *both* the methods are *groundless assumptions.* At page 141 the book has, "This is the way in which the distance of the moon is measured; and we may say, as a general result, that the distance of the moon from the earth is about thirty times the breadth of the earth." I shall prove that this measure of 30 times the "breadth of the earth" should have been 30 times the width of the shadow at no less speed than 68,091 miles an hour, a width of shadow of 272,364 miles instead of 8,000 miles, and here I must repeat, this figure is one of *unwarrantable license,* by means of which the Astronomer Royal has deliberately endeavoured to bear witness to his former result of 30 times 8,000, or 240,000 : *that* distance being in *error* to the extent of 30 times 272,364 miles.

or 8,170,920 *eight millions one hundred and seventy thousand nine hundred and twenty miles;* and it follows that the result of 30 times 8,000 miles which the Astronomer Royal *pretends* to extract from this figure at page 137, and as bearing out his *former* result, is also in error and *hopelessly* so.

The Astronomer Royal's result is very justly in most serious error, as a result from a figure every published line of which is utterly and *irretrievably at variance* with the published context of description; and which when constructed so as to demonstrate the context of description *will not even admit of the problem* by which he professes to prove the correctness of the distance as stated in the earlier part of his work. In the figure the letters G and C signify G the latitude of Greenwich and C that of the Cape of Good Hope. The latitude of Greenwich is 51 degrees 28 minutes 38 seconds North, but this is represented by the Astronomer Royal's figure as about 43 degrees North and the observatory of the Cape of Good Hope in latitude 33 degrees 56 minutes 3 seconds South is represented as at C about 44 degrees South, and in consequence the whole figure is one of the most unwarrantable license, and this most extraordinary license renders the whole figure worthless; and this most unwarrantable and most extraordinary license is rendered very palpable to the sight by *comparing* the Astronomer's figure with the *true construction* appended to this declaration and marked D.

In the Astronomer's figure the lines of observation *meet* at the moon; in my figure those lines *diverge* from the moon, like the open fingers from the palm of the hand. In the Astronomer's figure also the directions of North and South are marked by the letters GP for the North and CP′ for the South direction, and the positions of those lines to each other are exactly the reverse of what they should be, as represented by the true and correct latitudes, and as may be seen by comparing the Astronomer Royal's figure with the true construction appended to this deposition. Taking the central line through the figure as N E S, it will be seen that in the Astronomer's figure the North direction G P lies *farthest* from the central line NES or from simply the central E, whereas in the correct construction it lies *nearest* to the central NES, or to the centre E, because latitude 51 North is *nearer* to the Pole than latitude 33 degrees South. The same error is to be noticed as regards the line CP′, which the Astronomer represents as *nearer* to the central line NES, or to the centre E than GP, whereas it is *further remote,* because latitude 33 degrees South is less than latitude 51 degrees North and is therefore *more remote* from the Pole. I draw the very

gravest attention to the *monstrous* confliction exhibited by the positions of these directions GP and CP': they display in the *broadest light* and in the *simplest* manner the extraordinary license of this figure at page 137 of "Popular Astronomy." To turn to the author's method, he speaks of the moon as distant from the Pole 108 degrees of Polar distance ; but on his figure that angle measures about 118 degrees, and the Pole instead of being 38 degrees from Greenwich (as stated in page 137) is about 47 degrees, and when the figure is truly constructed as on an oblate spheroid, it is proved that the angle of 38 degrees should be slightly in excess of 38 degrees, and that the angle of 108 degrees should be slightly in excess of 108 degrees, and that either as 108 degrees or as slightly in excess of 108 degrees the line of observation *will not converge* to the moon, but *diverges* from it, showing the figure to be a *gross palpable absurdity*, and also raising the just conclusion that the figure is constructed as in the book, simply because it was found *impossible* to construct even the semblance of a representative figure, without adopting the above *extraordinary license of construction ;* and this may be gathered by comparing my figure with that of the book. Again, the result in distance is made to turn on the angle at the moon, which angle is stated to be $1\frac{1}{2}$ degrees in page 139, whereas when the diagram is imitated as per book, that angle is found to be about 14 degrees, and the distance to the moon instead of being 240,000 miles is something under 22,000 ; and further the author's method of procedure by parallax is in error, and it is in error as regards the angle of $1\frac{1}{2}$ degrees, both as found by the parallax of the Polar distances and also as found by the second process of observation between the star and the moon. At page 139, when speaking of the Polar distances, he mentions 108 as the Polar distance from Greenwich, and $73\frac{1}{2}$ as the Polar distance from the Cape, and he then adds these together and finds an excess over 180 degrees of $1\frac{1}{2}$ degrees. Here again I must point out that the Astronomer Royal has not been sufficiently direct to quote the altitudes, as strictly found at Greenwich and the Cape of Good Hope. He has however given (and what I gather he did not intend to do) just sufficient clue to enable me to calculate the whole number of possible angles, from first to last, as for both stations. I have thus calculated the angles and I cannot find the least excess over 180 degrees for the Polar distances. The Astronomer Royal infers that such excess of $1\frac{1}{2}$ degrees was found by observation. If I were to go to the Cape myself and were to find such excess I would not attach the importance to it which the Astronomer Royal has done. I would not expect to find it

at all were the observations taken from the moon down to the horizon, and by the legitimate altitudes, for if such excess really existed in connection with the legitimate altitudes, then latitude by the moon would be out to the extent of such excess. On the other hand, if I found such excess by measurement between the moon and a supposed South Polar star, but failed to find it by the legitimate altitudes, I would be suspicious of the star, as a true point of reference, and if I found the star true relatively to Polar distances from all the other planets, I would regard the excess as due to some irregularity of the moon's path. I however do not receive this excess, as I find an astronomer, the late Mr. Johannes Von Gumpach, mentions the Polar distances of heavenly bodies *as all alike* from the Cape of Good Hope. His work published in 1862, at page 94, reads "Then the sum of the North and South Polar distances of *any given heavenly body.*" It is plain that he makes no distinction for the moon as differing from the other luminaries. Page 96 contains a table of Polar distances between Greenwich and the Cape of Good Hope, for no less than 16 luminaries, and in no case do the Polar distances differ from 180 degrees by as much as six seconds. Such trifling errors as are mentioned are probably due to the state of the tide, if found by the altitudes ; and if found direct, are simply just the natural errors of observations, and they prove that the Polar distances at the Cape and Greenwich are equal to 180 degrees, and Mr. Gumpach fails to make an exception of the moon, for though the moon is not one of the 16 luminaries, still at page 87 he repeats his *inclusive judgment* from page 94 to the effect that all the heavenly bodies alike are under one rule with regard to the reduction of their apparent places; his words are at page 87, "with reference to all heavenly bodies without exception." He especially mentions the moon and includes even comets.

I have less to do with the actual existence of this excess than with the haphazard illustration published by the Astronomer Royal ! At the same time I point out that this excess of $1\frac{1}{2}$ degrees cannot exist on a globe unless a semi-circle can contain more than 180 degrees—a fact we know to be impossible. On a non-rotating stationary Earth, an ellipticity of the moon's path, might cause an overlap of the moon's declinations from Greenwich, and the Cape, and thus lead to some slight excess in Polar distance, but such excess could not possibly represent the angle at the moon. I call both the illustrations at pages 55 and 137 *haphazard*, because I find that they only admit of imitation, and are so utterly at variance with true figures

that they do not permit of construction in keeping with the context of description. As regards the excess of $1\frac{1}{2}$ degrees, it is certain that I do not find any excess whatever over and above 180 degrees, as arising out of calculations; and I find that when set up correctly as on a plane flat Earth the calculated angles agree exactly with the exigencies of construction as set to scale.

To turn to the second method advanced by the Astronomer Royal for ascertaining the measure of the angle M at the moon, by means of measurement from a star, the Professor counts the angular distance between the moon and the star, measured from the two observatories, and sets to work in the most foolish manner to treat those angular distances *as if both from one observatory*, and thus *deducts* the one from the other, and then most improperly bears out his former assertion that the angle at M, measures $1\frac{1}{2}$ degrees, and he even ventures on the words, "The angle GMC is the difference between the two angles SGM, SCM." The fact is that as those angles are *from two different* observatories, it is worse than idle to talk of deducting the one from the other, and in truth, as they *have no possible connection*, so it may be certified that they have no *possible difference*, and again the Astronomer Royal's system *falls to the ground*. In addition, the parallels of observation on the sphere would not include any angle, and the reading of the star would be identical at each station: on the spheroid the reading would differ by a few minutes only, but the difference is *useless*.

I have now shown conclusive evidence that this figure at page 137 is false, and I point to that evidence as extraordinary in its nature, and as being quite *impossible as accidental*; and I also enumerate that evidence as the great fundamental license in the *shift* of the latitudes, and which fundamental license is in this case a gross cruelty of a very treacherous order. Secondly, I enumerate that evidence as the falsity of the Polar distances as in diagram and as published at page 137 of "Popular Astronomy;" thirdly, as the fact that the lines of observation from Greenwich and the Cape *do not meet at the moon*, and do not even converge towards the moon, *but diverge* from it; fourthly, as the most *unwarrantable delineation* of the directions GP and CP' which are the North and South directions; fifthly, that the angle at the moon instead of being $1\frac{1}{2}$ degrees as stated, is in the printed figure *nearly 14 degrees*; sixthly, that the distance as found from the printed figure is *nearer* 22,000 *miles* than 240,000; seventhly as regards the star, that the angular measurement from such star to the moon has been taken *from* (2) two observatories, and *not from one and the same observa-*

tory, and that therefore, there is *no connection* whatever between the angles, and it follows that there cannot be *any difference* in the sense of a difference to be added or deducted; lastly that the figure *professedly bears out a former* result which former result is *enormously in error*, to the extent of being only one *thirtieth* of what *it should be*. I therefore conclude that the Astronomer Royal's system of parallactic measurement *falls to the ground*.

FURTHER *in re* SECOND ILLUSTRATION.

I now return to the second illustration, in order to prove that the author of "Popular Astronomy" has taught and inculcated in his printed book "Popular Astronomy" the theory of the Earth's motions in space, and yet that the said author, the Astronomer Royal, has for his own purposes, and in his calculations of the moon's distance, treated the Earth as a body not moving forward in an orbit through space. In connection with this second illustration the Astronomer has accounted the distance between the two stations A and B as 830 miles, and by means of observations taken at the two stations the Astronomer Royal has decided that the two stations are 12 degrees apart, and he then proceeds to find the measure of a degree by the simple process of dividing the 830 miles by 12, and he thus values the degree at 69 miles, as may be seen from his own printed remarks in pages 84 and 58: at page 84 the book reads "and therefore we should have to travel 69 miles to make the inclination of the plumb-lines one degree." At the bottom of the page and in connection with the same measurement of degrees the book reads, "So that we come to the conclusion, so far as our measures go, that the form of the Earth is somewhat turnip-shaped, or is what we call an oblate spheroid." These printed remarks stamp this measurement of degrees and all argument based on it *as of the first importance*. I shall now show that in this determination of the degree the Professor has *entirely ignored the movement of the Earth in orbit through space*. The distance between the stations is said to be 830 miles, and that distance has been divided by 12, because 12 is said to be the angle found between the two stations; but this distance of 830 miles must be considered as a measure on a *stationary* Earth. It is very clear that no *allowance* has been made for the *Earth's motion in orbit* through space *during the days of transit* from Shanklin to Balta. The Astronomer Royal does not state what time elapsed between the two observations, but his

remarks afford sufficient evidence that the observations were made at *different dates*; and the fact of different dates alone could justify the Astronomer Royal's words at page 122, in as far as those words imply that *he himself saw* and adjudged the position of the telescope and the fall of the plumb-line *at each station*, and which latter observation on his part would require *different* dates. I conclude that convenient times were chosen, and most likely and naturally that the observations at Shanklin and Balta were made when the officers of the survey of Great Britain were at those places in connection with the survey; Shanklin being towards the most Southern point of that survey, and Balta being nearly the most Northern station.*

One week, however, will serve my purpose as an interval between those observations, and I will count the Earth's forward motion in orbit at 68,091 miles an hour, which is the rate assigned to it at page 224. In one week the Earth would have travelled 11,439,288 miles, to which must be added the 830 miles between Shanklin and Balta, and thus the distance to be divided by 12 instead of 830 miles would be 11,440,118 miles, and the measure of the degree, instead of being 69 miles, would be 953,343 miles, which reduces this measurement of a degree to a *palpable absurdity*, and exposes the fact that the Astronomer Royal has entirely ignored the forward motion of the Earth in orbit through space around the sun, although *he forces it* as part of his theory upon the British public; and indeed without it his unholy and blasphemous theory would be impossible even as fiction. Additional evidence to the fact that the Astronomer Royal has treated the Earth as not moving forward in orbit through space is to be found in "Popular Astronomy" at page 67 in connection with the distance of the moon, as found from the so-called shadow of the Earth during the period of a lunar eclipse. At page 67 the Astronomer Royal when dealing with the distance of the moon as found from the so-called shadow of the Earth, writes as follows: "The time which the moon occupies in passing through this shadow is roughly speaking four hours. The moon then is at such a distance that in passing through the shadow of an object as big as the Earth she is occupied only four hours. The moon therefore in her course describes the breadth of the Earth in four hours." From this statement of the

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* It would have been hardly possible to travel from the one station to the other under a week, remembering that these observations refer to some years ago, when travel was much more tedious than it is now, and even now a week would probably only just suffice.

Astronomer Royal's it is plain that he is treating the Earth *as not in motion* in orbit through space, and that he is counting the shadow at 8,000 *miles* instead of 272,364 *miles*, which latter would be the representation of the Earth's *motion in orbit* at 68,091 miles an hour for *four* hours, and he sums up by the following: "Therefore the moon is distant from us by about 30 times the Earth's breadth." The distance thus stated would be on a stationary earth and as per the Astronomer Royal 30 times 8,000 miles, or 240,000 miles, as is stated at page 166 in the following words: "Its mean value may be roughly stated at 240,000 miles, or about thirty times the breadth of the Earth." With regard to this last quotation, I have to point out that this term of "30 times" is permissible, but the term "breadth of the Earth" is *entirely in error* and should be the time and duration of the shadow or *breadth of the shadow* at no *less speed* than 68,091 miles an hour; though even this rate fails of what the *true speed would be*,—and thus the moon's distance would be nothing less than 30 times 272,364 miles, or 8,170,920 miles. It is then again proved that the Astronomer Royal *has neglected* the said forward motion of the Earth in orbit through space in this calculation of the moon's distance, and this will be better understood when I state that the *mere width* of the shadow is *not reckoned* at all in the 272,364 miles, but that the *speed of that width* is reckoned as that of the speed of the Earth itself, and though the actual speed at the shadow would be greater, it *is impossible to say how much greater*, and therefore the less speed is adopted as sufficient to expose the fact that the speed itself has been overlooked. Again, of the shadow, it is of no importance what that width would be in that the 272,364 miles is the displacement *from centre* to *centre* of the shadow during the four hours; and whatever width might be assigned to the shadow, such width would be a quantity to be added to the 272,364 miles; but in that it is absurd and impossible to state what such width of shadow might be, *I have not considered it at all as a numerical entity*.* The Astronomer Royal teaches that the Earth moves forward in orbit around the sun, and at page 224 he specifies the speed of the Earth's motion in one hour as 68,091 miles in the following words: "And therefore the line E*e*, which is the Earth's motion in one hour, is 68,091 miles."

This sentence shows what the Astronomer Royal teaches in one part of his book; and his valuation of the shadow at 8,000 miles

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* I deal with nothing but the central hair-line of the supposed shadow, which hair-line would be displaced by nothing less than 272,364 miles.

shows him to be convicted for the second time out of his own work of *neglecting* to pay the least attention to the Earth's motion in this most important matter of the moon's distance. I have referred to a distance E*e* measuring 68,091 miles, and I therefore add the illustration from which E*e* is taken.
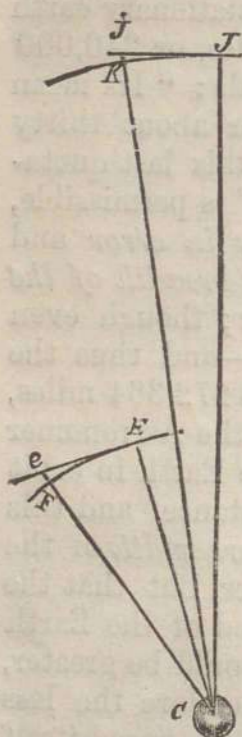
This illustration also affords further proof that the Astronomer Royal does account the Earth as moving forward in orbit through space in some of his calculations, but that at other times, as in this distance of the moon, he utterly and entirely ignores it, and in consequence the distance of the moon is false and his whole system of Astronomy is in error. This illustration is to demonstrate the calculation for the sun's attraction *e*F, but the Astronomer Royal commences his calculation by assuming CE, which would be the distance to the sun. He assumes CE as the distance of 95,000,000 miles, and with that as a starting point, but which in itself is only an assumption, and one which is challenged and denied, yet with only this assumption to start from, he deduces the distance E*e*, a distance as he says of 68,091 miles, as the distance that the Earth would travel in one hour, and then adding the square of the assumed CE and of E*e*, the hourly distance deduced from the primary assumption CE, he extracts the square root to represent the greater side C*e* of the triangle C*e*E, and which square root is nothing but the result which must follow from the prior assumed side CE of 95,000,000 miles, together with the distance E*e*; this latter being entirely a resultant from the assumed radius CE, and results in the distance of the longer side as a little over the initial assumption of 95,000,000 miles, and the sun is then said to have attracted the Earth through space 24·402 miles in one hour as shown by the distance *e*F.

FIG. 57.

I have now shown for the second time that the Astronomer Royal *does treat the Earth as moving forward in orbit through space*, and I have also shown that *he neglects* that movement in several of his calculations, and especially in his system of measurement as *applied to the moon's distance*, and consequently his system *falls to the ground*.

THE SECOND ILLUSTRATION.

I now again return to the Second Illustration.

If there can be said to be one point in connection with these two illustrations of more importance than another, such point is the one which I am now about to consider. I will also state that I have had considerable trouble in dealing with the succession of these features in evidence, and the difficulty has been caused by the fact that the features of this evidence have been arranged in order, so as to conform to the charges of my brief; this evidence being an *undertoned* repetition of the evidence in support of my brief, and differing from my brief only in the fact that the language of the brief is of necessity much more forcible, and is *more especially suited to the exigencies of a prosecution*. The point which I shall now especially refer to in connection with the Second Illustration is nothing less than the Astronomer Royal's main argument in connection with the use of the Zenith Sector, and its application as demonstrated at page 55 of "Popular Astronomy," and as quoted in the context of his book, in pages 55, 56 and 83, 84. I will premise my remarks with a few lines on the Zenith Sector itself.

THE ZENITH SECTOR!

I have already remarked that the Zenith Sector, with its mischievous plumb-line, is an instrument which should not have been imported into this question at all. It is not a *necessary* instrument in any way. The Astronomer Royal himself admits that the instrument is not necessary, and that the Mural Circle might have been used in its place. At page 56 the Astronomer Royal attempts to excuse the use of this improper instrument in the following words. He says, "and the peculiar advantages of using this instrument instead of the Mural Circle are these."

In this short sentence the Astronomer Royal has *confessed* and *admitted* that the Mural Circle *could have been used* in place of the Zenith Sector. The mural circle being an instrument, as a telescope mounted on a circular arc of degrees, and totally without the attachment of any *mischievous plumb-line*.

Why then was the Zenith Sector introduced into this question?

The Astronomer Royal says at page 56: "First, it is easier to carry about from one situation to another." Of this I must remark that considering the carriage was all at the Government's

expense, the preference for the one instrument over the other, as far as carriage was concerned, could only have been a preference to suit the highly-paid conveniences of those who handled the instrument, and where the public service is concerned and results of the first importance are to be hoped for from the expense incurred, the above is no just excuse for the use of an inferior instrument in the public service, and where the superior instrument might have been employed. Secondly, the Astronomer Royal adds, as another reason: "and next, the observations made by it are confined to that part of the heavens where the refraction is scarcely sensible." To this I reply that the Astronomer Royal *should have been more direct*. If he wished his hearers and his future readers to understand that with this instrument, the Zenith Sector, he measured the co-altitudes from the Zenith down to the star, he should have said so in plain words. If on the other hand he aimed at puzzling his audience, then he most successfully made use of language calculated to bring about that result. The *genuine* and leading feature in either instrument, as in the Zenith Sector or the Mural Circle, is identically the same, namely the *telescope*, and even if it was purposed to measure the co-altitudes down to the star, instead of measuring the altitudes up from the horizon, still the telescope of the Mural Circle could have been as certainly used as that of the Zenith Sector, and though there may have been some especial reason for measuring the co-altitudes down to the star, still, the altitudes should most assuredly have been measured up from the sea horizon, because it is on the altitudes in observations that the rules for observations are based; and although in this particular instance it would be just as easy to find the altitudes from the co-altitudes, and the difference of the one equals the difference of the other, yet if the Zenith Sector could not measure the altitudes, then I contend that it was unwise to use that instrument exclusively, or even as the chief instrument in this experiment: in addition the altitudes should have been quoted, as the experiment was conducted in the public service. I must also plead that no thorough-going controller and director, in any honest calling, would attempt to save himself or his subordinates that trouble which might be needful and necessary in order to execute orders, in the one best way, nor would he for a moment discard such a comparatively first-class instrument as the Mural Circle for such a dubious innovation as the Zenith Sector.

If it must remain for the present unadjudicated matter whether the Zenith Sector has been imported into this question to answer the purposes of controversy, there is at any rate no

difficulty in pointing out the very gross misuse which has been made of it. It is beyond question that the use of the Zenith Sector in connection with these and other experiments has permitted attention to be diverted from the *legitimate issues* of the observations, and has also favoured the introduction of *new phrases* into these lectures, and which new phrases have served the purpose of diverting attention from the *recognised terms*, legitimately and habitually used in connection with observations : further, the Zenith Sector has permitted the easily-misunderstood feature of the plumb-line to be imported into these observations, and into the question of the shape of the Earth generally. In short, this instrument, the Zenith Sector, has been utilised in connection with this general question of the shape of the Earth with the unjust and always dishonourable purposes of the mere controversialist, and I point out that the Mural Circle could not have furnished the illegitimate issues which have been published as the results of the Zenith Sector. I understand from this work "Popular Astronomy" that the Zenith Sector was in use before the date of the experiments especially mentioned at page 55, and I am not aware who was the inventor of the instrument, nor do I know for what particular service it was constructed in the first instance, but I cannot conceive that any *leading* astronomer *could possibly be the dupe of this instrument*, and I am thoroughly satisfied by a *minute criticism* of astronomical pretensions, and which criticism has now been pursued by myself for *seven or eight years*, that *every effort* has been made by professional astronomers to *dupe* the British public *to the utmost*, and to *prolong* and conduct the publication of astronomy simply and purely in the one-sided spirit of the *worst form of controversialist*. I therefore do not see myself called upon to attempt to make any excuses for these men, nor to place upon their publications other than those strictures, which they so clearly and legitimately bear.

I must remind those who in some humility of character may consider a faith which is allied to a state of being duped to be preferentially honourable as the outcome of their humilities, that no such faith, and no such wavering on the border-land of a *necessitous failure* of insight, comes to my rescue, nor offers me any *honourable hesitation !* I see matters very clearly, and as I see them, so my duty to the truth, to generations now in youth, to generations to come, and to the youth and age, as also the unborn of the world at large, compels me, without hesitation, or what might be termed in others, an honourable demur, to advance what I see plainly and distinctly.

THE RESULTS OF THE ZENITH SECTOR!

Before examining the phrases made use of by the Astronomer Royal, in connection with the use of the Zenith Sector, I will just state the terms which as designations of observations are in common use, and thoroughly understood by engineers, surveyors, and sailors, and by all who are conversant with observations of luminaries. With regard to these especial observations, we have to deal with three especial parts of observations, and I will therefore confine my expression to the three terms which are in common use as denoting those parts of observations! The three terms are, the zenith of the place, the altitude of the observation, and the co-altitude of the observation. Of these terms the zenith is the direction plumb overhead; the altitude is the elevation of the luminary above the horizon; and the co-altitude is the remainder when the altitude has been deducted from 90 degrees. I will now point out that of these parts of observations the Astronomer Royal has dealt especially with the zenith of the place and the co-altitudes of the observations. I will now examine how the Astronomer Royal expresses himself in connection with these well-known terms. When speaking of the results of the Zenith Sector, he says at page 55, "It gives us the direction of the vertical there, or the direction of the perpendicular from the horizon;" again at page 56 he adds, as the result of the instrument, "I have got a measure in degrees, minutes, and seconds of how far the star is from the vertical;" again at page 57 he says, "Thus we obtain the difference of the direction of the vertical at the two places." The above phrases are singularly indefinite and puzzling; so much so, that I doubt whether any person of ordinary scientific culture in connection with observations would be able to follow a lecturer who shall at a lecture make use of such out-of-the-way terms when speaking of observations of luminaries; and the latter phrase, "Thus we obtain the difference of the direction of the vertical at the two places" would, I am confident, be difficult of reception to even an ordinary intelligent person, well versed in observations, and with the book in front of him, and time at his disposal. The Astronomer Royal has in these few phrases dealt with observations in a manner just the reverse of that which is customary. It is customary, and it is the intelligent habit, to take the observation by directing the instrument to the star, and then to measure the altitude down to the sea horizon, or to use an artificial horizon, and further to quote the altitude, and should the co-altitude be required then it is customary to deduct the

altitude from 90 degrees, and to speak of the remainder as the co-altitude, but the co-altitude might be measured singly, and as a check on the altitude, and if it were further wanted to explain this co-altitude, it would be customary to speak of it, as so many degrees and parts of degrees, between the direction of the telescope to the luminary, and the Zenith of the place. Instead of this customary and lucid explanation, dividing the observation into two well-known parts, as altitudes and co-altitudes, and further giving a clear and definite valuation of the latter, —the Astronomer Royal has described the result as "the difference of the directions of the verticals at the two places," thus failing to attach due attention to the valuations of the altitudes and co-altitudes which form the especially important numerical features of the observations, and upon which the whole result in calculation depends,—failing to attach attention to these features, but fixing attention on the plumb-line, and thus when the observation was repeated at a second station diverting attention from the *all-important shift of the telescope* to the improperly asserted *shift of the plumb-line*. As judged from the gross misuse of this instrument in connection with these particular observations, the chief aim and purport of its use appears to have been to divert attention from the turn of the *telescope* to the divergence of the *plumb-line;* and from the legitimate altitudes and from the all-important numerical reference of the co-altitudes in triangle at the luminary, and to place this latter as the divergence of the plumb-lines. Coupled with the telescope is the graduated arc of degrees, and attention has been diverted from the telescope, and the shifting graduated arc, which is moved by the telescope past the plumb-line, and to a greater extent than the plumb-line is moved parallel by parallel, past the graduated arc;—attention has been diverted, from telescope and graduated arc, and *mischievously concentrated* upon the *plumb-line*, and the object in view is to impress the listener or reader with the absurd fallacy that the plumb-line has some mysterious power of movement of its own, independent of the telescope: in short, we are called upon to believe that the plumb-line is *attracted towards the centre of the Earth*, and therefore, at each new station, *shifts of itself*, and hangs at an angle to its former position; the telescope with its graduated arc being as in the Astronomer Royal's diagram, the same as before, and being exactly on a parallel at Balta to what it was at Shanklin: whereas any sane honest person can see quite clearly that the *movement of the telescope* as turned towards the star is what moves both the plumb-line and the graduated

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arc, and that *unless the telescope is moved* neither the plumb-line nor the graduated arc will show any movement whatever, but that, should the instrument be set up, the plumb-line would hang from *year's end to year's end* in exactly the same position, and would never show the least alteration unless disturbed by the wind or some accidental cause, and were the instrument shifted as from Shanklin to Balta, and the telescope with its graduated arc fixed by a screw, and then set up at the new station, and to a level as before, the reading on the graduated arc would be identically the same in the two places, showing that the plumb-line was hanging perfectly unmoved relatively to its former connection with the graduated arc; and, in order to see the same star,—the star being truly a fixed star,—it would be found necessary to unscrew the instrument, in order that the telescope might be shifted as turned, so as to admit of the star being seen. Prior to the unscrewing of the telescope, it would in nature and as in diagram G with similar altitudes be quite impossible to see the same star through it, as the telescope's direction would then be other than convergent to its former position as at Shanklin, and the unshifted telescope being other than convergent, the plumb-line would perforce be parallel to its former position; and not only parallel, but in exactly the same upright superficies as drawn through the initial position of the plumb-line at the one station to its initial position at the other station. I draw particular attention to the upright superficies of the plumb-line, for when the telescope is altered, being unscrewed and pointed to the star, the shift of the telescope of course moves the plumb-line, which, hanging still parallel, is then moved from its former superficies, to a second upright superficies, and *this is the sole and only legitimate manner* in which the plumb-line can be altered in its parallel position at each and any station. The Astronomer Royal's diagram leads us to believe the very reverse of this, namely that the telescope could be set up at Balta parallel to its position as at Shanklin, and that the telescope would not require to be altered to a *convergence* in order that the star might be seen, and its altitude be found; but, that acted on by the mysterious power of attraction, the *plumb-line would move*, and hang at an angle, and thus show a different reading on the unmoved graduated arc. Had this been told us by a person who had just been liberated from a lunatic asylum, and in which he had been justly detained for positive insanity, we might afford to pay no attention to it; but when it is advanced at a lecture, published, and *insisted* upon as part of the *necessary culture* of the world, and that by the first scientific

authority, and an authority supported to the *utmost* by a *salary*, *title*, and the *highest official position* possible relatively to the professional calling of the individual, then it is impossible for sane, as honest and honourable people, to either condone or fail to stigmatize in the most deserving terms a doctrine so pernicious and destructive of the sanities of both present and future generations.

Now the fact is that the Astronomer Royal has quite failed in his description of these observations, not only as regards the genuine facts, but even as describing his own view of the matter and that which he thinks due to the assumed spherical Earth. He has not even understood the result of the matter, from his own stand-point, and this I will make exceedingly plain; but, first of all, in order to show that I am not doing him any injustice, I will make a considerable quotation from his own book, page 83. He says: "If then we plant the Zenith Sector at A, figure 18 or 20, the plumb-line will hang in a direction perpendicular to the surface at A. But if at B, the plumb-line must hang in the direction perpendicular at B; therefore if at A we observe a star nearly overhead, then the plumb-line will fall over the point G of the arc; but if we carry the Zenith Sector to B, and *turn the telescope* to the same star, the plumb-line will fall on the point G of the arc." I have now to draw special attention to the Astronomer's words "turn the telescope." These words, "turn the telescope" firstly show us that *after all* the telescope is to be turned, and this I feel sure is a conclusion which the majority of just readers have formed for themselves, but secondly, these words conflict, both with what immediately follows and also with pages 57 and 122. At page 57 he thus speaks of the telescope: "the telescope is to be pointed in the same direction, whether we use it at Shanklin or at Balta." Now, had we been left to our own just translation of the term "*same direction*," most of us would, and I should, have translated it as meaning *converging lines* relatively to the same star, and there the difficulty would have ended: but no, says the Astronomer Royal, nothing of the sort; and he proceeds to translate the term "same direction" for us, and adds: "or the line C D is parallel to c d." Here then is our translation, and it is insisted upon over and over again; nevertheless, it is also departed from at page 122, and there in successive sentences it is both insisted upon and departed from with the flattest of flat contradictions.

At present I have to deal with the insistance, and this insistance is again supported at page 83, and follows close on the words "*turn the telescope.*" The quotation proceeds: "Inasmuch, there-

fore, as the telescope, from being directed to the same star, which is excessively distant, takes the *same direction in different places,* and inasmuch as the plumb-line takes different directions in different places, by means of these we get the *variable* positions of the plumb-line referred to the *invariable position* of the telescope." Now in this sentence, when speaking of the plumb-line, the Astronomer uses the plural " positions," but when speaking of the telescope he uses the singular " position," and he also adds the word " invariable ": thus leaving us, together with his translation of " same direction," no choice but to regard the telescopes as other than converging. The following diagram will show that the Astronomer Royal has not sufficiently gone into this matter, even from his own stand-point of a spherical world. I say, spherical world, because I can see quite well that the Astronomer Royal treats of a sphere, and both his diagram and his remarks show me plainly, that he treats of the sphere, for only on the sphere do the correct altitudes of luminaries form parallel lines, and which parallel lines he has mistaken for what he calls " the same direction " and the " invariable *position* of the telescope." The construction in spheroids demands large figures, which require to be reduced by photography, in order that a decently representative figure might be published in Book form; but even when reduced the figure should be of considerable size to be at all representative. For my part, I cannot see any signs of the Astronomer Royal having ever attempted to work in the spheroid *and to scale ;* but he cannot be excused for not having done so, as it was his especial duty to do so, and the expense would be quite trivial to his department ; and even the expense of reduction of his figures by photography would, I conclude, have been at Government expense, and to any department would be very trifling, though I, as a private individual, have found it to myself very expensive. The difference also between the results of the sphere and the spheroid are very *trivial* indeed, but though trivial, *most marked and important,* and the difference amounts to this, that those lines to the star or other luminary, and which the Astronomer, speaks of as *parallel,* are on the Oblate Spheroid *divergent.* I therefore know at once by the Astronomer's language of parallel lines, that he is treating of the sphere, though his diagram at page 55 is a poor illustration of either. When dealing with the third illustration, I have at great expense to myself constructed in the spheroid, and have had the figure reduced by photography, and then lithographed at considerable size, as may be seen ; and in order to claim the distinction of spheroid, diagrams should be as distinct and finished

as mine. In this illustration there is no necessity, in order to expose the fallacy of the Astronomer Royal's figure, to do more than pursue the construction as of a sphere. The diagram marked E shows the result, as on a sphere, should the telescope be really the same at each station, that is, as if screwed up at Shanklin and then removed to Balta, and set up levelled as before, the telescope being entirely unshifted. In this diagram Shanklin and Balta are placed in their *approximately* true latitudes, with 10 degrees between them, and in consequence an angle of 10 degrees at the centre. The result of the telescope being entirely unshifted is that the altitude is identically the same for the one position as the other ; the telescope being screwed up and fixed at altitude 61 degrees at Shanklin, could only show altitude 61 degrees at Balta, it, the telescope, graduated arc and plumb-line, all being strictly fastened and kept fastened at Balta. The consequence would be, that the plumb-line as the Zenith, would show (in diagram) the divergence of 10 degrees, just the divergence equal to the approximate latitude between the two places ; but the plumb-line has not altered the least in the world, for it is fastened and cannot move, any more than either telescope or graduated arc can move, and therefore though the altitude be exactly the same as at Shanklin, the position of the plumb-line is thrown at a divergence, by the *shift of the horizon,* which forming the base line is by the shape of the sphere, 10 degrees more to the right hand at Balta than at Shanklin, the swerve of the base line of course conforming to the number of degrees in between the two stations. This diagram then shows distinctly, that if the telescope be screwed up together with the graduated arc and plumb-line, that the plumb-line would show exactly the divergence which conforms to the Zenith of the place, *and that without any movement on its part whatever.* The exact position of the plumb-line is not shown in the diagram, as the scale is too small to show either telescope or plumb-line ; but the direction of the telescope is that of the altitude, the altitude being the produced telescope, or produced line of sight through the telescope, and the Zenith of the place is representative of the plumb-line, because on the sphere the fall of the plumb-line would be regarded as equivalent to the Zenith of the place, and the one may be read for the other. This diagram then affords *conclusive proof* that the Astronomer Royal has not paid sufficient attention to construction, *even from his own point of view,* and this is proved by the fact that when regarded from the theory of the sphere, as at present held, and expressed in the diagram, but all of which is totally false to nature and impossible as

reality,—when then so regarded for critical purpuses, the plumb-line would appear to conform to the Zenith of the place, or to the point of attachment to the telescope, and would also appear to diverge from the position at Shanklin by a divergence, which, when counted from the imaginary centre of the supposed sphere, would equal the number of degrees of latitude between the two stations, *and this without anything whatever being altered*, neither telescope, nor graduated arc, nor plumb-line. The Astronomer Royal himself argues for the divergence of the plumb-lines as equal to the latitude between the two stations, but he has committed himself to the extraordinary statement that there are twelve degrees instead of ten, but which latter number of *ten* he must know to be a very close approximation of the true latitude between the two places, and as taken from the authorised maps and charts: indeed, the circumference of his own diagram *is divided* as expressing ten degrees between Shanklin and Balta: but this slipshod *division* only aggravates the shortcomings of the diagram and its confliction with the context as 12 degrees. I have now to point out, that the plumb-line would appear to conform to the Zenith, not from any *law of attraction*, but because the theory of the sphere insists that each part is *on the top*, and thus the plumb-line would, though perfectly unmoved from its fastened position as at Shanklin, appear as in the Zenith from its point of attraction to the telescope, and at the necessary divergence, because it would thus be set up on an horizon, which, as a base line, would be inclined at an angle of ten degrees with the former horizon and base line at Shanklin coupled with the supposition that each station is to be regarded as the top. The idle and supposititious theory of gravitation is thus shown to be quite *superfluous* on account of the fall of the plumb-line, which, on the theory of each part of the sphere being on the top, and coupled with its necessitous and angular horizon as base line, would perforce fall straight to the centre as would all weights, thus rendering the additional force of gravitation *quite superfluous*, as meaning (which it *must mean* to have any force at all) *a force or weight in addition* to the actual weight of the article. On the other hand, should the idea of each part being the top be abandoned, then neither could the horizon even in theory be regarded as affording a level base line, but merely an angular superficies, nor could the Zenith of the place be regarded as the Zenith of the place, nor indeed, could the diagram be in any sense legitimate or representative, even in the *unreal domain of theory*. As regards the idea of each part being top, together with the alteration in the base line as sea level, I am thoroughly

confident that no honest man, or woman, can believe such statements for one moment, as forming part of nature, after giving the matter due and thorough as *conscientious* consideration: nothing then would remain, to support the plumb-line in its angular position, as per diagram, as the Zenith of the place, should the diagram be kept true with the North line on the top, except the as idle and impossible theory of gravitation, which latter has just been shown to be *superfluous*, to the spherical theory in its entirety. So far then as regards the plumb-line, my diagram shows that the divergence of the plumb-line would conform to the number of degrees of latitude between the two stations, and that such would be the case although each and every part of the Zenith Sector be strictly fastened as when set up at Shanklin; and this from nothing more than the mere *mechanical shape of the sphere*. Further this diagram lets into the secret, that there is not the *least connection* between *this divergence* and the *co-altitudes*, or in the words of the Astronomer Royal, "the direction of the vertical at the two places," but that the divergence of the plumb-lines would be practically the same, *for any number of North altitudes*, which might be taken at the second or each station.


ATTENTION DIVERTED FROM LEGITIMATE ISSUES.

I have already stated that the use of the Zenith Sector diverts attention from the legitimate issues of the observations, and directs that attention to false and illegitimate issues. The legitimate issues are, the altitudes, and the necessity to *turn* the telescope *in order to alter the reading on the graduated arc*. The numerical difference between the altitudes gives the angle of convergence of those altitudes towards the position of the luminary, when as in this instance both altitudes are towards the same North or South. When the altitudes are North and South, to the same luminary, then the co-altitudes added together give the angle of convergence at the meeting of the lines of altitude. When these altitudes are to the same luminary and are produced until they meet, this point of junction affords a ready means of obtaining a rough approximation of the height of the luminary, the luminary not being of necessity at the junction of the lines, and in fact being hardly ever at that junction, but being displaced to some considerable extent. Sufficient for the present that the legitimate issues have been distinctly stated, and that in this particular

instance we have to deal only with the difference of the altitudes, both as per my diagram, and secondly as altitudes to the same star, which latter important feature I am leading up to. To return then to the issues of the observations, and to the use made of them by the Astronomer Royal, it is clear that *he has diverted attention from the altitudes* and has fixed attention on the *plumb-lines*, giving us to understand, that the divergence of those plumb-lines is mysteriously connected with the co-altitudes, and that in fact the difference of the co-altitudes *governs* the divergence of the plumb-lines. I point out that he so *completely ignores* the altitudes (*on which all depends*) as not even to state what they were, but he satisfies himself by stating that the difference between the co-altitudes is equal to 12 degrees. The difference between the co-altitudes is the same in this case as that between the altitudes, but again the Astronomer Royal has diverted attention from the legitimate issue of *that* especial difference between the co-altitudes, such legitimate issue *being the angle at the meeting of the lines of observation*, (or supposed place of the luminary) and he has fixed attention on the 12 degrees as if those 12 degrees formed the *necessary* divergence of the plumb-lines *as controlling that divergence*. Both the diagram and the number of 12 (instead of 10) show that there is not the least *necessary connection* between the difference of the co-altitudes, and the supposed divergence of the plumb-line, but that the supposed fall of the plumb-line on the sphere would equal the latitude between the two places, for *each and every North altitude that could possibly be taken at those stations*, and that should a number of different altitudes be taken, the movement of the telescope would not otherwise alter the practicable fall of the plumb-line, than by causing it to be moved a few inches (or perhaps a yard as the utmost possible displacement) from one superficies to another, just in proportion to the altering of the altitude : *in this case a few inches at most*. I have now distinctly proved, that there is of necessity no numerical connection whatever between the Zenith of the place or its representative, as the plumb-line and the co-altitudes of the observations ; and that the difference between the Zeniths of the places, or the fall of the plumb-lines, or in the Astronomer's own words, " the difference of the direction of the vertical at the two places," is caused by the mechanical *shape* of the sphere itself, and *not of necessity by the numerical difference of the co-altitudes*. I can still extract further evidence of this fact by subtracting the co-altitudes as per diagram. The co-altitude at each station and as per my diagram amounts to 61 degrees deducted from 90 degrees. This leaves

co-altitude 29 at each station, and 29 deducted from 29 leaves *nothing*. Thus the plumb-lines on the sphere, as hanging from the strictly fastened telescope, and as *judged of from the difference of the co-altitudes* should be parallel, as not containing any divergence or angle whatever, and would thus by the Astronomer Royal's own rule for finding such divergence, conform to the plumb-lines as shown in my diagram marked G. That diagram represents Balta and Shanklin as on a plane flat Earth, and it may be seen that the similar altitudes of 61 degrees express parallel lines, and the fall of the plumb-lines are also exactly parallel, and apparently as if conforming to the want of difference between the co-altitudes. This appearance, is however *only a coincidence*, for on the plane surface the plumb-lines will always fall quite parallel, no matter what the altitudes or co-altitudes happen to be. I have then clearly proved that on the sphere the divergence of the plumb-line is not controlled as conforming to the co-altitudes of the observations, for the divergence equals ten degrees, whilst the difference of the co-altitudes equals nothing. This being most undeniably the case, it follows that the Astronomer Royal's expedition *was simply so much waste of time*, in that *no result* whatever could be obtained on *his plan of procedure* from the observations. It must be borne in mind, that whatever result the Astronomer Royal should decide upon, as the outcome of his expedition, that result was to be formed entirely from the observations, *and on the spot*, and by the well known rules which govern observations, and without any reference whatever to predetermined geometrical constructions. Had the Astronomer Royal done this, he would have been forced to the legitimate issues of the observations, which would have told him that his telescopes *were converging* towards the star, and would have given *him the angle of that convergence* together with an approximation of the star's distance, which would have shown the star to be comparatively *very close* to the Earth ; but the *very last thing that* he could have gained any sure and certain knowledge of by his process would have been an angularity of the plumb-line. It so happens that just as there is a coincidence between the parallel plumb-lines and the parallel altitudes in my Figure marked G, so in connection with the sphere there is *a single case of coincidence* between the altitudes and the angle embraced by the production of the supposed plumb-lines towards the centre of the sphere. *When* the difference of the altitudes *happen to conform* to the latitude between the stations, *then*, that difference, *perforce* coincides with what would be the divergence of the plumb-lines at those stations. This can be seen from my next diagram

marked F—in which the altitudes 61 degrees and 71 degrees are 10 degrees apart;—just the latitude between the two stations, and therefore the co-altitudes differ by 10 degrees, and coincide exactly with the divergence of the plumb-line; but *they do not cause the* divergence of the plumb-line, nor is there the *assumed control over the one by the other;* nor is there the *assumed control over the* plumb-line by *gravitation*. It so happens however that the Astronomer Royal has stated *that angle 12 degrees* is the difference between his co-altitudes, so that even this mere chance of a coincidence *turns against his* result of the expedition. I have still a few words to say on the subject of gravitation in connection with the plumb-line. I have to make it clear that the Astronomer Royal does infer that the plumb-line *was effected* by *gravitation*, and that it was *owing to gravitation* that a different reading was obtained on the graduated arc. In order to make this clear, I refer to another experiment, which is called the *Schehallien experiment*, and which is explained at page 258 of "Popular Astronomy." This experiment was conducted just in the same manner as that at Shanklin and Balta, but a novel term for the plumb-line was introduced into this experiment, and instead of being spoken of as the direction of the vertical, it was, in this case, regarded as " *the direction of gravity.*" In this case also the addition of a mountain was introduced
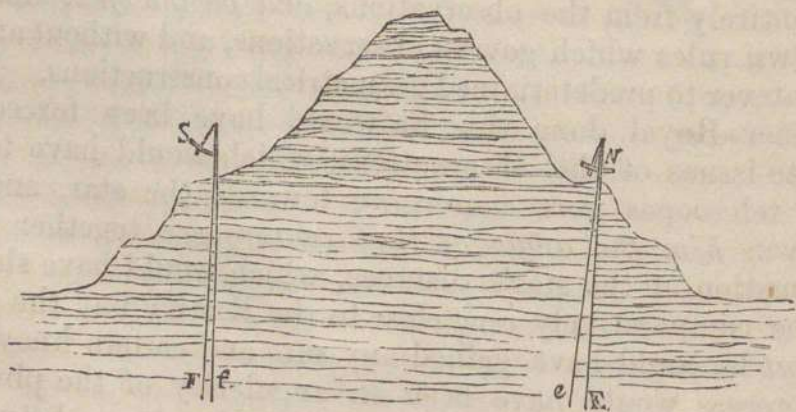
into the experiment, in order as is asserted to test the gravitation, quite overlooking the plain truth, that if gravitation really existed, as *other than the weight of the article* as a most wonderful and *directing force*,—then most assuredly such gravitation would exist all the world over, and would affect the plumb-lines at Balta and Shanklin just as distinctly as at the Schehallien mountain: so that the importation of a mountain into the experiment was entirely beside the mark. I now quote the actual words of the Astronomer

Royal from page 258, where it is stated that " the attraction of the mountain *would pull the plumb-line sideways.*" I have now made good my point, namely, that we are wanted to believe that the plumb-line *was diverted by gravitation*. In this case also, the effect ascribed to gravitation is 12 seconds, and at page 259 we are told by his own rule (but which I am confident *is a misuse of the instrument*), that the plumb-line is said to alter one second for every 100 feet, so that knowing our base line between Balta and Shanklin, I can at once, by simple arithmetic, reduce the 830 miles to feet, which equals 4,382,400 feet, and which at the rate of 100 feet to a second, gives 43,824 seconds as the amount which the *plumb-line would be displaced by gravitation alone* between the two stations, and which when reduced to degrees gives 12 degrees 10 minutes 24 seconds as the exact amount of divergence which the plumb-lines would have shown, actuated *simply and solely* by gravitation, and that *without* the telescope being moved; for we already have 12 minutes 24 seconds over and above the quoted angle of 12 degrees; so that it is finally and conclusively shown, that gravitation would induce us to believe that the telescope was not moved in the least, but that it might be taken screwed up from the one station to the other, and that if the plumb-line were then unscrewed, it would *mysteriously alter* 12 degrees 10 minutes 24 seconds, the telescope still being screwed up: all of which I denominate the very grossest of gross imposition on the British public. The diagram marked E shows plainly that the plumb-line would deviate ten degrees, *caused by the mechanical shape of the sphere* itself, and I now state that the spheroid would cause a trifle more deviation, amounting to possibly some 19 minutes in addition. I will therefore adhere to the sphere, and call the deviation ten degrees, the plumb-line being fastened. I *now loosen* the plumb-line: then by the Astronomer Royal's rule for gravitation I must allow 12 degrees *in addition*, which have to be added to the ten, and the result shows a deviation for the plumb-line of 22 *degrees*, instead of the 12 degrees asserted by the Astronomer Royal. This impossible as new direction for the Zenith is expressed in the diagram, marked E, and is marked with the words, *Zenith as per gravitation*, and forms with it *an angle of* 22 *degrees;* and this is the result (as the experiment stands) by which the 830 miles must be divided on the *stationary* Earth, as *he has assumed it to be*, and in consequence the degree would equal 37·7 *miles. But where is this folly to stop?* I now carry the instrument *back* from Balta to Shanklin, *and I screw up and fasten*

everything as before : the plumb-line representing this new Zenith of gravitation. At Shanklin I also obtain a new Zenith, displaced by the mechanical sphere, 10 degrees; and the plumb-line hangs as at Balta, and as having 10 degrees of divergence, in what even on the sphere, and on the supposition of each place being top, is seen to be *a strained and impossible position. I then unfasten* the plumb-line, when it instantly moves back 12 degrees, and conforms to the old position of the Zenith of the place, and this the plumb-line would do in this instance, because it was in *an impossible position* previously, and was retained there *in theory* by the *plumb-line being fastened.* The loosened plumb-line would undoubtedly return to its former position by its own weight, or its natural compulsion to hang plumb up and down, and from which tendency *its name* is derived :—but this natural tendency *is not the gravitation* of the Astronomer, *is not the mysterious force* which at Balta must be said to have *dragged* and *retained* in suspension the plumb-line 12 degrees *from its natural* position as the Zenith of the place at Balta. The upshot then at Shanklin so far is that the angle is 22 degrees as before and the degree 37·7 miles : but, *the folly must go on.* I will now start my Zenith Sector from latitude 40, and as before from the Zenith of the place, and everything screwed up and fastened. At Shanklin the *mechanics of the sphere* would displace the plumb-line 10 degrees, and it would conform as before to the Zenith of the place. I now *unfasten* the plumb-line, on which the Astronomer Royal's *mysterious* 12 *degrees* (as per rule from page 259) must be permitted *to drag the plumb-line* 12 degrees towards Balta *back* to its old *constrained impossible position,* and the remarkable upshot would be that at Shanklin we would have *two Zeniths of the place,* both caused by the displacement of the 12 degrees, and both Zeniths as per gravitation, as far as position goes. Such would be the result of this folly of supposing the plumb-line susceptible of being moved by gravitation and by *other than the genuine shift of the telescope itself.*

I have now shown the effect of gravitation to be *thoroughly impossible* in connection with the plumb-line, and as on either sphere or spheroid, and I have given conclusive proof that as long as the telescope be fastened, it would be *useless* to unfasten the plumb-line, from its first natural fall as at Shanklin, in that, whether fastened or unfastened, *all the conditions being the same,* and the bases levelled at each place, the plumb-line would not show the least perceptible deviation.

I must now deal especially with the angle as 12 degrees instead of the 22 degrees, *which there really would be.* The proof against

angle 12 in this particular of the plumb-line is *exactly the same* as that for angle 22. I therefore need not repeat it, but I point out that the final absurdity of two Zeniths at Shanklin, *both in positions as for gravitation* would remain for angle 2 degrees instead of 12, and that the smaller angle is *just as fatal* as the larger one, for the plumb-line cannot deviate, in the same spot, and hang in two distinct directions, no, not even by a hair's breadth. Such a deviation as 2 degrees would be *enormous,* and even the very smallest deviation that could be detected with the eye would be fatal to the many *purposes* of the plumb-line. This is a safe assertion, which is proved every day by the *mechanical use* of the plumb-line in house and ship-building, but happily I can add a distinct proof from the Astronomer Royal's own book.

I shall now prove from "Popular Astronomy" that a deviation of *even* 12 *seconds* would be quite *impossible,* from one and the same station. The description of the Schehallien experiment provides this proof at pages 258, 259, in which latter we are told that the amount of deviation between the two stations and due to gravitation was 12 *seconds,* and the rule that the plumb-line changes one second for each 100 feet of distance shows clearly that the stations could be nothing less than 400 yards apart, and therefore it would be impossible that the plumb-line could deviate *even one second* on a base of *less than* 100 *feet.*

It is therefore conclusively proved that *from one and the same* spot there could be no deviation whatever. It has also been proved that as with angle 22 so with angle 12, there would be the absurd and impossible necessity that at Shanklin the plumb-line advanced from latitude 40 degrees should hang in a Zenith for gravitation, *removed by 2 degrees* from the Zenith which would be necessary should the plumb-line be carried from Balta to Shanklin. It is therefore distinctly proved that *two* Zeniths would be *necessary* at Shanklin, *in both of which zeniths the plumb-line must be considered as hanging :* all of which we know to be *profoundly impossible.* This then is proved even of angle 12, but I point out that the diagram would *compel* me to insist on at least angle 22 for the sphere, and a trifle more for the spheroid. I now have to add that whatever angle should be ascribed to the plumb-line as the effect of gravitation would be an angle *utterly superfluous,* and this has already been proved by the *mechanics of the figure,* which mechanics of itself would *compel* the horizon or base line to an angle with any former station, and would thus permit of the fastened or unfastened plumb-line falling in *the only one position possible* under the assumed circumstances of a spherical or

spheroidal world. It is therefore unimportant as regards this point, whether the Astronomer's rule be strictly correct or not, as any other figures would be just as fatal to him as either his own angle of 12 degrees or that of 22 degrees, or as his partly quoted rule, or as the rule with the word (nearly) expressed in figures, or with the more correct distance between Balta and Shanklin, instead of his quoted distance of 830 miles. The *mechanics* of the figure *turn against* any possible *excess* for *gravitation*, and this is *final* and *conclusive*, and would always compel the *absurdity* of *two* Zeniths for gravitation, at the *central* station of three stations. Should the gravitation be held to differ in amount at the different stations, then the *additional absurdity of three Zeniths* would be absolutely necessary *at one and the same spot.*

There remains still one feature in connection with the reading on the graduated arc, and the assumed motive power of gravitation, and I note it, as an *additional* proof, that in connection with the graduated arc there is no room for any movement of the telescope whatever, in either the Balta-Shanklin or the Schehallien experiment. In this latter experiment we are told that the whole inclination of the plumb-lines was 53 seconds, out of which 12 was somehow or other devoted especially to the mountain, but we are also given distinctly to understand that the remaining 41 seconds were also due to the effect of gravitation, for page 258 has it : " If there was no mountain, the *direction of gravity* would be N.E., and in like manner, supposing that if there was no mountain the *direction of gravity* at S. would be SF," here then is afforded proof that the remaining directions of the plumb-lines embracing 41 seconds are held as caused by gravitation, and not by the movement of the telescope, and page 258 has it : " The direction of gravity at each station, you will observe, is the result of the gravity of the whole Earth, as considered for a moment independently of the mountain." I therefore record this as another proof that the telescope is treated of as unmoved in both these experiments. It would seem then from this Schehallien experiment that two gravitations were to be dealt with : one that of the Earth at large, or as the book has it " of the *whole Earth independently of the mountain*," amounting to 41 seconds, and a second one that of the mountain, amounting to 12 seconds, and strange to say that in the battle between these two gravitations the lesser one of 12 stands recorded as the more powerful than that of the greater as 41 seconds, and which the book distinctly mentions as brought into play. Here again is *more absurdity, more impossibility : all of which* the British public has been *called upon to believe.* Then again of the *distance* between

the stations on this mountain : are we to judge the distance from the 12 seconds, or the 41 seconds, or the 53 seconds? It is plain that the distance would be about 400 yards for 12 : 1366·6 for 41, or 1766·6 for 53 seconds : here is *more confusion*, and *more folly!* I have now made it clear that the Astronomer Royal does infer that the *plumb-line was moved by gravitation*, and that he does infer that it was owing to gravitation that the reading differed on the graduated arc, and that thus the telescope would remain as if screwed up at the same altitude as at Shanklin.

I now pass on from this feature of the plumb-line, feeling that the proof is most conclusively stated that the Astronomer Royal has succeeded in fixing especial attention on the plumb-line, and that he has ascribed very *false and impossible issues to its use.* I have also proved that the divergence of the plumb-line on the sphere or spheroid is *controlled by, as naturally following*, the inclination of the horizon of the place, or base line, and that neither on the sphere, spheroid, nor plane flat Earth could the direction of the plumb-line be controlled by the co-altitude or altitude, further than, that the shift of the telescope could pass it a few inches, or a few feet, at most, from one upright superficies to another. Further I have shown that the mechanics of the sphere or spheroid leave *no room* whatever for the control of the plumb-line by gravitation. Further I have shown that when gravitation has been *recklessly* introduced into this question, that the allowance for it has in two different experiments *equalled* and even *more than equalled* the difference *contended* for as the divergence of the plumb-lines between those places, and as judged from the quoted figures. Further I have shown that the introduction of gravitation has ended in the *absurdity* of the *necessity* for *two* Zeniths at a *central* station, for the plumb-line to hang in as per gravitation. Further I have shown that gravitation would render the angle between Balta and Shanklin 22 degrees instead of the 12 degrees contended for. Further, I have shown that 12 degrees cannot be the right angle, in that *only ten exist* in latitude between the two places, and in addition, that as the angles of the vertical, or directions of the Zenith on sphere or spheroid, must conform to the number of degrees of latitude between those places,—and that as there was not the control over the plumb-line which the Astronomer Royal assumed and ascribed to gravitation, *that it was quite impossible* that he could find the, or *any angular difference* of the assumed angles of the vertical as one and the same as the directions of the plumb-line from the experiments which he has described as at page 55 or as forming the Schehallien experiment. Finally I have

shown, that the *intense absurdity of each* part of the sphere being the *top* must be *insisted* upon, or else the whole mechanics of the sphere, as Zeniths and upright positions of the plumb-line, must be *abandoned as impossible.*

Shortly therefore I have proved that the experiment between Balta and Shanklin, as also the experiment between the stations on the Schehallien Mountain, *were sheer waste of time* as regards the objects sought to be determined, and that the descriptions of those experiments are public cruelties of a very gross and serious nature, published to the British public. I have already shown abundant proof that these descriptions *cannot be termed the mistakes* of a merely erring nature, and I shall proceed to furnish fresh proof that there is not the smallest excuse for refusing to term these descriptions wilful perversions made entirely from the controversial stand-point.

I shall now proceed to prove that the telescope *must have been moved* in order to see the same star at Balta as was seen at Shanklin !

Up to the present, in order to expose the fallacy of supposing that the plumb-line could be moved by gravitation so as to alter the reading on the graduated arc, I have been compelled to deal with the telescope as a screwed up fixture, because *there was no room* for its use in that gravitation *alone* was found sufficient to *supply the whole of angle* 12 degrees, thus altering the reading of the graduated arc, and in addition mechanics and gravitation enlarged that angle to 22 degrees : but I have proved gravitation to be impossible in the mechanics of the sphere or spheroid, in that I have proved, that on either sphere or spheroid the mechanics of the figure *as the angular base line would so completely supply the necessary angle of divergence of the plumb-line* as the vertical of the place, that *no movement of the plumb-line is wanted* or *even possible,* and that in consequence (the telescope being unaltered) the reading on the graduated arc must and would remain the same as 61 degrees at the two stations of Shanklin and Balta, and the plumb-line might be justly treated of as fastened up at those two stations, in that each and every legitimate detail assumed for the plumb-line, in angular divergence, as of the vertical of the place has been strictly accounted for ; and in that each and every legitimate detail assumed for the plumb-line in angular convergence as the direction of gravity in connection with either experiment has been strictly accounted for and satisfied, *the plumb-line being fastened ;* and such detail as is superfluous has been proved to be *superfluous* and impossible by unfastening the

plumb-line, and then considering the plumb-line acted upon by this *superfluous detail,* and which superfluous detail has in its turn been treated as *legitimate,* and then *judged by its results,* which have been found to lead to *impossibilities ;* and impossibilities which *happily* are such in *construction,* and happily leave no possibility of controversy as matters of reason, but which, though truly reason, *but for the shape and form* of the impossibility, as for instance as of *two Zeniths* in *one* place, might, and in astronomical quarters would be met with the *defiant insolence* of a reply in *words only.* Happily the impossibilities of construction *do not admit* of a reply in words only. I take this opportunity of pointing out that so rare is the talent of *genuine* geometry that the book of *Euclid* still remains the only genuine authority on the subject : the *immensity* of this fact has not received its just consideration.

Finally, and conclusively, I have proved that the plumb-line has no *motive power* independent of the motion of the telescope. This was the *first great feature* which it was necessary to prove.

I now pass on to prove the movement of the telescope !

THE MOVEMENT OF THE TELESCOPE.

In order to prove that the plumb-line could not alter of itself, and thus change the reading on the graduated arc, I have treated the instrument as screwed up at Shanklin, and then set down at Balta exactly fastened as before, and now that I wish to prove that the telescope *must be moved,* I treat it as screwed up and fastened as before, and by this means I obtain a *sure and certain starting point* as a base of operations, and one which could not be obtained in any other way. The altitude of 61 degrees, which I have made use of, and which I now introduce into the argument, as having some especial force, may not be the exact altitude which was really observed ; but this is a matter of no importance, as any altitude which I choose to fix upon *will serve to demonstrate* the experiment. The really important feature to be observed is that the altitude at Balta will *ultimately* be dealt with as showing 12 degrees in the reading on the graduated arc, between the two stations ; and also it will be necessary to deal with that altitude as in *just proportion* to the altitude at Shanklin, and this will *compel* the altitude at Balta to be 71 degrees.

I now call attention to the altitudes marked 61 degrees, both at Shanklin and at Balta, and in the diagram marked E. I also

again throw in the reminder that the altitudes are similar perforce, because the instruments are held to be screwed up, and I now draw attention to the fact, that even if the effect of gravitation be allowed, just for the sake of argument, and the Zenith as at Balta be accepted, then two impossibilities will be disclosed, and are now about to be offered, to the astronomical world, as two especially choice dilemmas. These two dilemmas offer another exceeding proof of the impossibility of gravitation, and this proof is perhaps more in place here, in this consideration of the altitudes, than it would have been in the argument touching the plumb-line, in its two characters as Zeniths and directions of gravity. I will deal with the altitude from *this Zenith of gravitation first*, because I can keep the work more in order by so doing, in that this Zenith of gravitation is so *foreign* to the construction that its consequences are confined to this diagram alone. The Zenith then being that as per gravitation and at Balta, it follows at once that with regard to the sphere the altitude is not altered in the least, in that there still remain 61 degrees *between* the *direction* of the telescope and the *base line* or horizontal direction of the place; but this impossibility is disclosed, that 12 degrees are *cut off from the quadrant* on the side of the co-altitude, and this is what would be the case at *either station*, and as on the sphere; the quadrant would therefore be *lessened* from 90 degrees to 78 degrees, which lessening we know to be *impossible*, as every quadrant contains, as an instrument, 90 degrees.

The altitude therefore being exactly the same, it is idle to contend whether the reading has been altered on the graduated arc or not. Could the reading be altered to the required 73 degrees, then it is plain that the 73 degrees *would not be degrees*, but would have to be something in measurement, *less* than a degree. So much for the sphere! On the spheroid, on the other hand, the new direction of the Zenith must be held as compelling an angular alteration in the base line, or horizontal direction of the place, and to an equal extent of 12 degrees, and in consequence the reading on the graduated arc must be held as altered in this case, and the altitude must be the required 73 degrees, and this must be the case at *both* stations, for at Shanklin we have the *two Zeniths* : at once 61 degrees from the *one* Zenith and 73 degrees from the *other : but this is impossible*. It is therefore once more *conclusively* proved, that the Zenith of the place *cannot be diverted by gravitation* from the mechanical Zenith of the place, and the mechanical Zenith *does not alter* the reading on the graduated arc; therefore

it is once more proved that gravitation, as *a mysterious force*, altering the plumb-line, and by it the reading on the graduated arc, *does not exist at all*. Again, to return to the Zenith of gravitation, and to the still screwed up telescope. It is evident that the telescope has not been moved, and that at each station the telescope must be held to be *exactly at the same altitude as before*. Firstly then at Shanklin we have the unfastened telescope showing *two different altitudes* even on the spheroid ; but this is impossible : therefore it is again proved that gravitation has no place whatever as a mysterious force, in connection with the spheroid. Secondly, we have the telescopes at a great divergence, and yet we must be *expected to see the same star* through those *diverging telescopes ;* and this is proved by the fact that when counting the Zenith at Shanklin as the Zenith of the place, plus the slight displacement for the spheroid, we have at Shanklin altitude 61 degrees plus a few seconds, and at Balta we have altitude 73 degrees plus a few seconds, so that we have in this case fully angle 12 degrees between the altitudes and co-altitudes according to the Astronomer Royal's angle, and should thus be able to see the same star through the diverted telescopes : *which is impossible*. Again, gravitation is shown to have *no possible part or lot whatever in the construction of spheroid and sphere*. Thirdly, counting the Zenith of gravitation at Shanklin as the Zenith, resulting from marching the Zenith Sector from latitude 40 to Shanklin, we then have exactly *similar* altitudes on the spheroid and at the two stations, both 73 degrees plus a few seconds : the co-altitudes are therefore exactly equal, and when deducted from each other leave *nothing ;* therefore, if the Astronomer Royal's method of finding the angle of the vertical, or direction of the plumb-line, as the *difference* of the co-altitudes, then it is plain that the plumb-lines should contain no angle whatever, but should fall strictly parallel to each other, as shown in my diagram marked G. In this case, though the difference of the *co-altitudes* equal *nothing*, the figure shows us, that the plumb-lines as Zeniths per gravitation would *diverge* 10 *degrees*, plus the odd seconds for the spheroid. Again therefore, it is *thoroughly* and *conclusively* proved that gravitation as a *motive power*, of great and mysterious force *over and above* the mere weight of an article, and as a *powerful attraction*, has *no existence whatever* in connection with the *spheroid*, and it has already been proved *impossible* in connection with the *sphere*, and consequently it is *impossible* in connection with *either* spheroid or sphere. One point still remains, and that is that Zeniths as per gravitation would produce a spheroid of proportions, quite *distorted* from the proportions which are said to

exist : for instance, we are told at page 62 of "Popular Astronomy" that the proportion of the two assumed axes of the earth is as 299 to 300. This proportion is *so slight*, that it requires a very large figure to show *any difference* in the *circumferences* of sphere and spheroid, and the angular divergence of the meeting points of the Zeniths towards the centre is so slight, that it requires a large figure to express them. The distortion produced by these assumed and impossible Zeniths may at once be surmised by the position of the lines forming angle 22 degrees, which fails to penetrate to *even one half* of the spherical radius. In addition, the necessity for the two Zeniths at Shanklin is so fatal, that it would be quite impossible to construct even a spheroid from those Zeniths, in that the meeting point of the other Zenith is distantly removed from the point of 22 degrees, by some fifteen hundred miles, and as *a radiating point*, and a necessity would arise for a figure with *two circumferences* at one and the same place, and these circumferences *widely* separated from each other by more than a thousand miles : all of which is grossly impossible. I have then once more, and I hope finally as regards this diagram marked E, proved that the introduction of gravitation into the geometry of the spheroid is an impossibility, and I pass on to denounce it as an impossibility of such a nature that the future insistance of gravitation as a possible part of spheroid or sphere will be nothing short of a *downright imposition and fraud on the British public.* I have already pointed out that the impossibilities, which are above-mentioned, as the conclusions which would follow from the Zeniths being 22 degrees apart, would also follow were the Zeniths reckoned as 12 degrees apart : at the same time, the geometry of the sphere or spheroid as expressed in diagram marked E is *conclusive*, that 22 degrees *would be the angle of divergence* of the Zeniths, as per gravitation between Shanklin and Balta. On the spheroid that divergence would be some trifle over and above the 22 degrees. I now also repeat what I have already stated, that in connection with this diagram, *there is no necessity to construct the spheroid.* The main reason, and I may add the only sound reason, why in another figure it has been necessary to construct the spheroid is to test whether the altitudes to the luminary *diverge* or not : on this figure the altitudes *diverge strongly*, so that the especial question of divergence is *settled at once ;* and thus in this diagram the figure of the sphere serves *all possible ends ;* and moreover, the Astronomer Royal's published figure cannot be called the spheroid, nor do I see that it deserves to be termed any part of a spheroidal Earth.

To return to the diagram marked E, the subject now being.

cleared of gravitation, I can deal directly with the altitudes, as the movements or directions of the telescopes. The latter being still screwed up as before, the altitudes are perforce 61 degrees at each station, and show an *immense divergence*, in a distance of less than 4,000 miles. This divergence proves clearly that it would be impossible to see the same star, the place of which is "unaltered," through those two telescopic directions. It is therefore clearly necessary that in order to see the same star, the place of which is "unaltered" as the Astronomer Royal says at page 57—the telescope *must be turned at Balta.* I therefore turn the telescope, and refer the reader to figure marked F. At page 57 the Astronomer Royal demands that the altitudes as directions of the telescope shall be parallel. His words are, "The telescope is to be pointed in the same direction whether we use it at Shanklin or Balta : or the line C D is parallel to c d." To meet these requirements of the Astronomer Royal, I have constructed a fresh figure, and I show the altitudes (in the first instance) as 61 degrees and 71 degrees, which form exactly parallel lines, and do so beyond doubt or question, and moreover are altitudes from Shanklin and Balta to the same star, in exact proportion by *rule*, such as the rule which is constantly used in navigation for finding altitudes. Such altitudes when in true proportion to the rule for navigation always produce parallel lines when set up on the sphere. I have taken a great deal of trouble to be certain of this fact, and I have constructed figures over and over again, on purpose to test this point ; and I have also constructed the sphere on a very large scale, so as to give the matter a very severe test, and I am quite satisfied that the result in parallel lines is true. The very fact therefore that the Astronomer Royal claims parallel lines shows me that he is dealing with a sphere, and not a spheroid, for on the latter the lines instead of being parallel would slightly diverge. The altitudes then are now parallel, but it is very plain that it would be *impossible to see the same unaltered star* through those parallel telescopes ; moreover the altitudes are only 10 degrees apart, and the co-altitudes are only 10 degrees apart, as respectively 29 and 19 degrees, which when deducted from each other leave 10 degrees ; but the Astronomer Royal insists, over and over again, as at pages 58 and 84, that the difference of the co-altitudes equals angle 12 degrees ; therefore neither will the parallel altitudes (his first insistance) suit as showing the same unaltered star, nor will *they permit of angle* 12 degrees *between* the co-altitudes. It therefore follows clearly that the Astronomer Royal's statements *most seriously conflict,* showing an utter contempt of and disregard for the truth,

and further, that in order to embrace his angle of 12 between the altitudes and of necessity the co-altitudes, the telescope must again be turned at Balta. I will just quote the Astronomer Royal's words to show that the co-altitude is with him the measure of what he terms at page 156 "how far the star is from the vertical." Again, at page 57, "the difference of the direction of the vertical at the two places" forms the Astronomer Royal's wording for the difference of the co-altitudes. I have added this quotation to save the reader from the possible necessity of referring back to a former mention of this matter. The telescope must then again be turned, and in order to concur with the Astronomer Royal's angle of 12 degrees, the telescope at Balta must be shifted until the reading on the graduated arc shows an altitude of 73 degrees. The co-altitudes will then be 29 and 17, and their difference will of course be 12 degrees. It can now be very plainly seen, that although the difference between the co-altitudes is thus expressed as 12 degrees, that difference *does not affect* the difference between the Zeniths of the place as making that difference 12 degrees. That difference is a *part of nature*, and *nature* has fixed it at a certain measure in miles, which we have termed 10 degrees in latitude; and 10 degrees it remains, for all altitudes alike that can possibly be taken at that one spot; but the 10 degrees are not governed by the co-altitudes as having control on the plumb-line, and this is quite plainly apparent when we see as in this case, that the difference between the co-altitudes is 12, whereas that between the Zeniths is 10 degrees: so that here we have plain and certain proof that the experiment as conducted by the Astronomer Royal was *sheer waste of time*, in that the difference of the co-altitudes had *not the connection* with the plumb-lines that he wishes us to believe: nor is the reading on the graduated arc *caused* by the mysterious action of the plumb-line, but that reading is and can be caused only by the action of the telescope which moves the graduated arc *past* the plumb-line, and in a greater ratio, than the telescope moves the plumb-line to meet the movement of the graduated arc. In fact, the movement of the plumb-line is *so small*, that it cannot be measured at all as practically differing from the Zenith of the place. The Astronomer Royal's own rule that it would be necessary to move 100 feet in order to detect a change amounting to one second in the reading of the instrument proves the fact, that the trifling movement of the plumb-line from one superficies to another *cannot be measured practically*: at the same time, the fact that the altitude can be anything we choose to make it, and that *any number* of altitudes *can be taken* at the same spot, prove most conclusively

that it is the *action of the telescope* that alters the graduated arc, and that no amount of marching from place to place can make the plumb-line alter, *unless the telescope is shifted*. It is not and cannot be that the plumb-line shifts one second for every hundred feet, but that the *telescope* would have to be altered one second for some such distance. The movement of the plumb-line then is so small from one superficies to another, that it is practically received as the Zenith of the place, even in supposition and on the sphere, for on the plane it is, in fact, the parallel of the Zenith, and *includes no angle whatever ;* but also on the sphere, as long as the two altitudes are in the same direction as North or South, the difference between the Zeniths of the plumb-line of observation would be exactly the same as the difference between the Zeniths of their latitudes, and this even in the region of differences so minute as to baffle expression : so that any idea of discrepancy between the Zenith of the place and the Zenith of the plumb-line may be at once and finally dismissed in this particular instance, and in practice in any instance whatever. It has been shown that the Zenith of the place cannot alter, in that it always conforms to the latitude between the two places ; and further it has been shown that the Zenith of the place cannot alter from gravitation, in that any such supposition *destroys* the *quadrant* of 90 degrees at once, *destroys* both sphere and spheroid, as requiring two circumferences, *two Earths* in reality, and leads to other conclusions *grossly impossible*. The Zenith of the place therefore *cannot alter*, and the Zenith of the plumb-line alters merely as the place of the plumb-line alters ; and in this instance merely a minimum distance of a few inches at most or probably less than one inch, as the telescope is passed from altitude 61 degrees to altitudes 71 degrees and 73 degrees : but the Astronomer Royal infers that the Zenith of the place, in its representative as the plumb-line does alter, and alter to the full extent of the reading on the graduated arc, and he infers this alteration of the plumb-line as the direction of the Zenith of the place at Balta, thus ignoring the mechanics of the sphere or spheroid ; and he distinctly asserts this alteration of the plumb-line, as the direction of gravity, in the Schehallien experiment : the direction of the Zenith, and the direction of gravity being merely the plumb-line as *upwards* or *downwards*. In addition the Astronomer Royal strengthens his assertion of the movement of the plumb-line by a small diagram, in which he has placed it beyond doubt, that we are to believe that the plumb-line does seriously alter, as the Zenith, upwards, and the direction of gravity downwards.

I append his diagram, and I re-quote his own words at page 258, "And this is the consequence : supposing that at N, if there were no mountain, *the direction of gravity* would be N E; then introducing the supposition of the mountain, the attraction of the mountain would *pull the plumb-line sideways* towards the centre of the mountain, and the direction of gravity would be at N E." Such are the Astronomer Royal's own words, and apart from the directions of gravity downwards, and of the Zenith upwards, his words are singular as reporting an actual experiment. Why does he speak of the *supposition* of the mountain ? If the experiment was really carried out, why suppose the mountain ? and if the results were as he states, why mention them in the doubtful language of the conditional mood ?
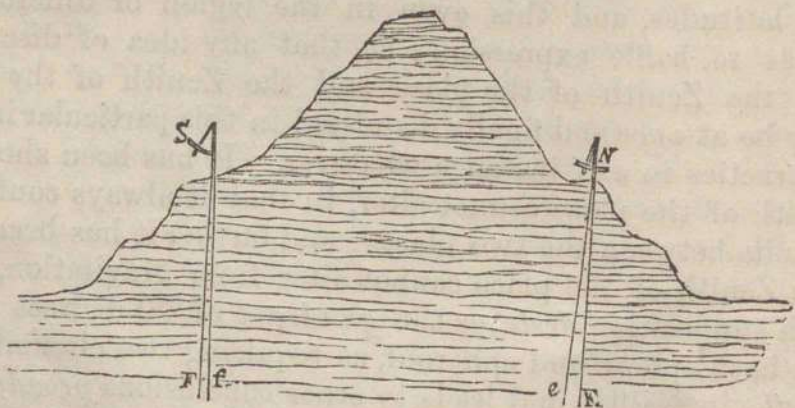
The Zenith has been proved not to alter, and it has been proved that the plumb-line cannot alter as a Zenith direction. The Astronomer Royal asserts that the Zenith direction does alter, and asserts that it alters through the medium of the plumb-line, and most seriously to the full deviation between two latitudes. It follows therefore, that the Astronomer Royal has stated that which is false, mischievously false, and that which his experiments ought to have taught him the very reverse of; and that which his *whole professional training* should have taught him *the very reverse of;* and this mischievous nonsense has been monstrously imposed upon the British public and the world at large.

The Zenith of the place then *never alters,* and the Zenith of the plumb-line is the Zenith of its attachment to the telescope, and it cannot alter of itself, and the movement of the plumb-line as passed from one altitude to another is so slight that even in supposition and on the sphere, it cannot be detected as angular measurement differing from the Zenith of the place ; therefore it remains that as

the altitude at one and the same place is altered, it must be altered by the shift of the telescope passing the graduated arc past the plumb-line, which latter always hangs truly parallel to its former position.

This parallelism of the plumb-line is departed from in supposition, and on the sphere ; still it is attested by the impossibility of *detecting* any angular difference between the Zenith of such place and the fall of such plumb-line : of course where no difference exists none can be detected. It is now therefore fully proved that the telescope must have been moved, and it is also fully proved that no practicable account can be taken of the movement of the plumb-line, angular or parallel : it therefore follows that the Zenith Sector has been proved to have been used in a most mischievous manner, and in a most illegitimate manner as diverting attention from the movement of the telescope, and fixing that attention in the most illegitimate and cruel manner on the as cruelly assumed motion of the plumb-line.

To continue the movement of the telescope, not only must it be turned, but in order to embrace the angle of 12 degrees, it must be turned until it shows altitude 73 degrees : but this movement of the telescope to altitude 73 degrees shows lines *converging* to the luminary, instead of the parallel lines claimed by the Astronomer Royal at page 57. A monstrous confliction therefore between lines claimed as parallel and lines converging to the luminary has now been proved. Further, the Astronomer Royal speaks of the star as *excessively distant,* and we know by this term that he means a distance approximating to (in his own figures and words at page 198) 63,000,000,000,000 miles, or "not far from two millions of times as great as the distance of the sun from the Earth." Such is the distance of the star by the Astronomer Royal, a distance inconceivable and ludicrous to mention as within the range of the human eye : the diagram on the other hand, shows the altitudes converging to the luminary at the comparatively short distance of under 16,000 miles : sixteen thousand miles even on the sphere, and when found by the Astronomer Royal's own angle of 12 degrees. Again then the Astronomer Royal is shown to be entirely at variance with the commonest regard for truth. Further it can easily be proved that in connection with this matter of parallel lines to a star, the Astronomer Royal has taken a very cruel advantage of the untrained intelligence and *kind credulity* which has been so readily at his service, as trusting implicitly in his statements. It is worthy of notice that the Astronomer Royal treats altitudes to stars as parallel lines, in both

these illustrations at pages 55 and 137, but when he deals with altitudes to the moon he treats those altitudes *as converging:* whereas the *same parallelism* is applicable to altitudes to *all* luminaries as set off upon the sphere. On the spheroid the difficulty is worse still, for the altitudes diverge, and that to *all* luminaries alike, and the Astronomer is totally without a shadow of excuse for treating altitudes to stars as parallel lines, but altitudes to other luminaries as the moon, as lines converging to the luminary. Of course it is plain to be seen that the cruel statement that the one set of altitudes to stars are parallel, whilst those to other luminaries converge, is all part of the insolent controversial plan to utterly deride the truth, and make a football of the grand confidences and holy trusts of the British people and the nations of the Earth. My published diagrams and those attached to this work, and in which some of the altitudes are to the moon, and some are to stars, show both sets as being in proportion, as of altitudes calculated to the same luminary and as per the rule used in navigation: for instance, my diagram marked D shows altitudes to the moon divergent on the spheroid, but which are exactly parallel on the sphere; and this present diagram marked F shows altitudes 61 degrees and 71 degrees which are to the same star, and are exactly parallel, and are in the proportion as of altitudes calculated for latitudes ten degrees apart.

I hope that I may safely now pass on from the movement of the telescope. Having proved that not only does the telescope suffer displacement, but that its displacement to angle 12 causes convergence to the luminary instead of parallel lines, and also having reasoned the plumb-line to a fixture, it remains that angle 12 itself, or any other angle that might have been named, such as ten, requires to be examined, so that it may be determined to *what legitimate issue* angle 12, or other angle, is to be finally relegated. I have already shown that the Zenith Sector has been mischievously used to divert attention from legitimate issues to false issues, and now I shall prove, that as regards angle 12 or other angle, attention has been diverted wholly and fully from the *true position* of such angle, and has been fixed upon a perfectly false issue which has already been shown to be perfectly groundless and indeed impossible. By rivetting attention on the plumb-line, not only has attention been completely diverted from the all-important movement of the telescope, but even the position of the angle, formed by the difference of the altitudes, as angle 12, has been falsely determined as the divergence of the plumb-lines, instead of what it really is,—namely the angle at the *junction* of the converging

lines towards the luminary. I say towards the luminary because the junction of the converging altitudes is not the place of the star or luminary. As far as I understand matters, and I have given a very express attention to this particular feature,—a luminary will be seen in a place different from its real place relatively to the distance that the observer is from it, and in this particular the luminary is not peculiar, but it suffers a *relative displacement* in common with *all* objects which we look at; but there is this difference with regard to the luminary, namely, that it is so much farther off, as compared to a church steeple, or distant mountain, so it suffers a greater displacement, and one which can thus be readily detected. In this matter it is by no means easy to be quite explanatory, for it is not only the object seen which suffers displacement, but the whole plumb-height that suffers displacement: in fact when dealing with a luminary, the whole space from the horizon up to the luminary suffers a foreshortening which increases as we increase our distance from the luminary: the consequence is that a luminary will have an apparent place for *each* latitude that it is viewed from, so that the altitudes from two different places go to *two* different apparent *aspects* of one and the same luminary; and thus it is that the junction of the altitudes is not of necessity the true place of the luminary, nor even of an apparent place, *common* to the two observers. There is then at the luminary an apparent displacement or parallax, and which has this peculiarity, that it is always in a *plumb-fall*, and has no displacement to the right or left. It has also a second and most important peculiarity, and that is that for two stations there are two plumb-parallaxes, and the difference between the amounts of parallax invariably equals the angle at the junction of the altitudes, to the luminary. This parallax is as it were the mechanical ladder of the altitudes, or it may be termed the hinge of the observations: but this much is certain, that without it navigation would be impossible.

There is also an exceptional case in which the apparent aspect of the luminary is common to both altitudes, and this is when the altitudes are North and South and similar. I published the following diagram explanatory of this exception some five years ago.

This diagram shows the altitudes each 50 degrees, and shows them meeting at one and the same aspect of the luminary, directly over head to the central observer C. This diagram also shows that the angle at the luminary equals the latitude between the two stations. The latitude equals 80 degrees, and the angle at the luminary equals 80 degrees, equals in fact the co-altitudes of the

altitudes. The altitudes equal 100 degrees, which deducted from 180 degrees, leave 80 degrees as the angle at the luminary. Each Observer is distant 40 degrees from the luminary, so that the base equals the two forties, or eighty degrees of latitude, and the angle at the luminary has been shown equal to eighty degrees; therefore, the angle at the luminary equals the latitude between the two stations. The angle at the luminary is the angle at the
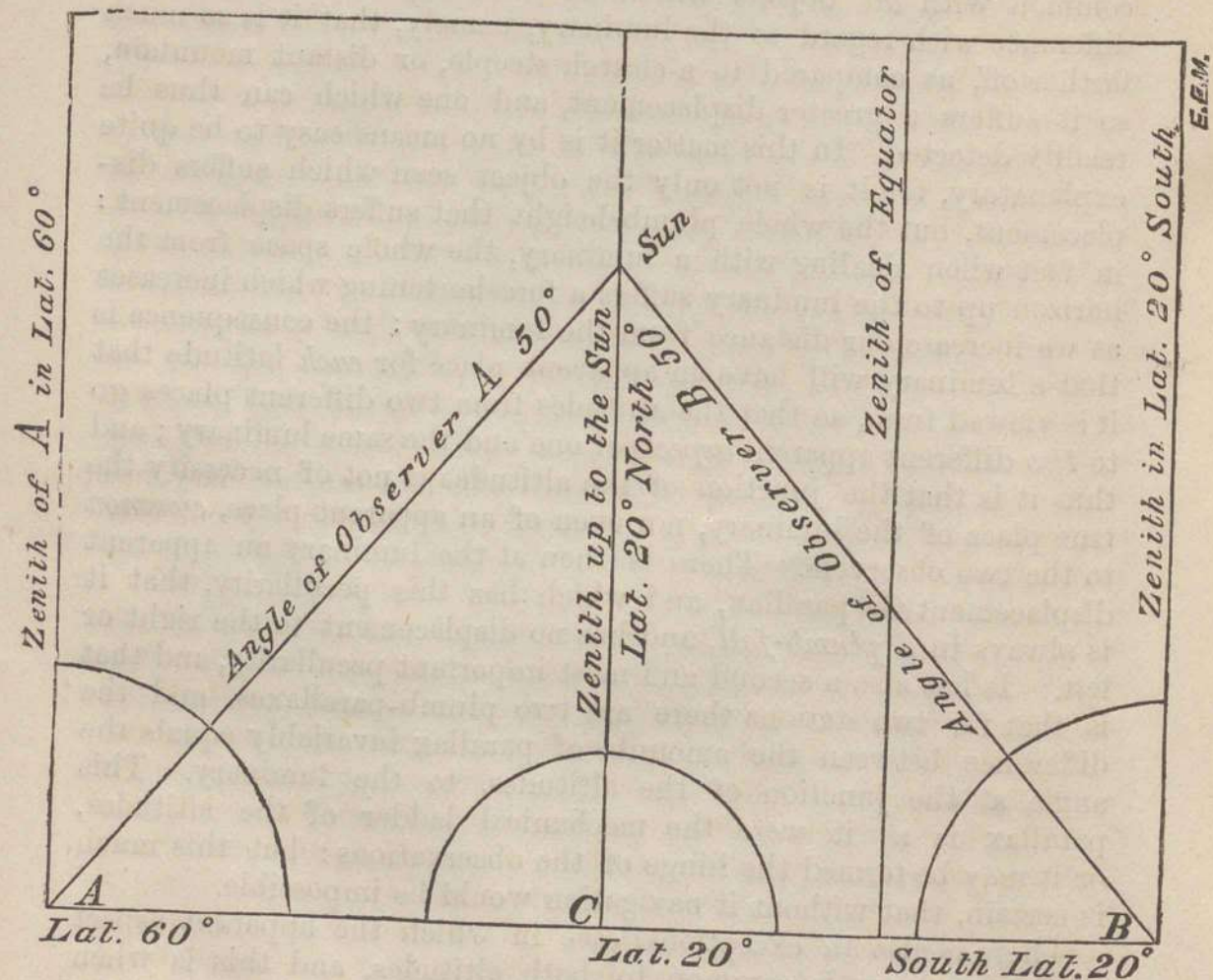
junction of the altitudes: therefore the angle at the junction of the altitudes equals the latitude between the two stations. *This is always the case*, and it brings us back at once to the Astronomer Royal's co-altitudes; with however in this particular instance the *sum* of the co-altitudes and not their *difference*, as between Shanklin and Balta. In this case as the altitudes are North and South, so the sum of the co-altitudes equals the angle at the junction of the altitudes; but at Shanklin and Balta, in that both altitudes are to the North, so the difference between

the co-altitudes equals the angle at the junction of the star: further the difference between the co-altitudes is at Shanklin and Balta, the same in amount as the difference between the altitudes. It follows that when the altitudes are both North, or both South, we can take the difference of the altitudes at once as the amount of the angle at the junction of the altitudes. *This is exactly what should have been done!* This is what would have formed the legitimate issues of the experiment, and it was not even necessary to find the co-altitudes, but the difference of the altitudes, either as 10 or 12 degrees, would have given the Astronomer Royal the angle at the *junction* towards the luminary and would have given him a ready approximation of the *distance* of the star, which distance as under 3,500 miles might have saved him from the supreme folly of quoting that distance as the equivalent of billions of miles. Instead of this straightforward and lucid acceptation of the experiment, he has failed to use the Zenith Sector for the purpose, amongst other purposes, of directing attention to the correct position and use of this angle, but has fixed attention on that angle as the result of differences *that do not exist at all in nature*, and as the result of an action of the plumb-line, which has been most conclusively proved to be impossible; and I regard this conduct as *totally without excuse*, and especially cruel to the British public.

I will now pursue the necessity that exists in navigation for this angle at the junction of the altitudes, as also the necessity for the equivalent of this angle in relative displacement or parallax, equalling the latitude between the two stations. In the diagram, the converging altitudes marked 61 degrees and 73 degrees, 12 degrees apart, contain at their junction an angle of only 2 degrees. This angle of 2 degrees tells me at once that the figure is all wrong, and as the latitudes of Shanklin and Balta are 10 degrees apart, I know that I ought to have an angle at the junction towards the luminary of ten instead of either two or twelve degrees, and I am therefore confirmed in my belief that the figure is all wrong, and that the Astronomer Royal has no right to such an angle as 12 degrees. Again, this is instantly confirmed by the fact that (and as the Astronomer Royal himself asserts, and this time truly, as part of the geometry of the sphere) the lines which converge should be parallel: therefore again on a third count the angle of 12 must be wrong, and the figure wrong in consequence. Here it is wise to pause and consider, that in connection with this matter there is one sure and most certain feature, namely, the approximate proportion of latitude between Shanklin and

Balta: *this we know without a shadow of possible question* to be approximately ten degrees: therefore as the lines to the same star would be parallel on the sphere, it is evident that the altitudes must have been such as will make those parallel lines, and such altitudes are those *only* which differ in amount, by the amount of the latitude between the two places. I therefore have here a sure and certain condemnation of angle 12, and I know that those converging lines 61 degrees and 73 degrees are not to the same star, and I know that angle 2 degrees at the junction is false, as it should be 12 degrees; and thus I know that the whole figure is false. I am then driven to altitudes 61 degrees and 71 degrees, and these I know are altitudes to the same star; and I know this because they are in the *true proportion* to the latitude between the two places, and as latitude approximately used for this demonstration. If I took altitudes approximately 50 and 60, they would be to the same star, in fact to the Polar star; and any altitudes I like to take, as long as they are 10 degrees apart, will be altitudes to the same star, and will give me the parallel lines demanded by the Astronomer Royal.

Altitudes 61 degrees and 71 degrees then in their turn give me the parallel lines, but the parallel lines are in themselves *fatal* to the figure in that I can neither *see* the same star through those parallel lines, nor can I *obtain an angle* of ten, or any other angle towards the luminary. I therefore know quite well that the figure is all wrong again. The figure would be quite right were the earth a sphere and the lines parallel; but the impossibilities attaching to this supposition prove that it is all wrong, and that the earth is not a sphere. As regards the parallel lines, I have a few words of caution to advance. I have to place on record the warning, that these lines which really are parallel, and which even the Astronomer Royal is compelled to accept as such, cannot in observation be treated as lines, which, though actually parallel, might appear to converge in the distance, as do *two lines of railway.* There is no analogy whatever between lines of railway and these lines: nor is there any analogy whatever between the position of a man standing between two lines of railway and viewing their convergence and the position of a man observing a luminary first at Shanklin and then at Balta. The lines of railway are *metals* and the man sees them *both* at once; but as regards these altitudes, neither is the man standing *between* them, nor are the lines on his right and left, as *visible* lines, but they are essentially invisible, and moreover the observer could deal only with *one* at a time: therefore in no sense is there any analogy between the lines of

railway and these lines, nor could the lines be at once converging and parallel. It can be proved, and should be commonly known, that these lines, and that other altitudes to luminaries such as conform to the altitudes for navigation, do form parallel lines on the sphere: it is also known that, *in nature*, the observer sees the star or luminary from the two stations, and it is thus known that he has really looked along *converging* lines, and therefore it is *soundly* concluded that he has not made his observations on either a sphere or the oblate spheroid. The only kind of spheroid that remains is the prolate spheroid: but the difference between the two spheroids is *enormous*, and a change from the oblate to the prolate spheroid would be quite ruinous to astronomy. The distance of the sun would fall directly from the ninety odd millions of the oblate to less than one million on the prolate. There are other reasons also why the prolate spheroid would be just as contradictory to nature as the oblate spheroid or sphere, and with this remark I dismiss it because the prolate has been examined and found impossible, and there is no immediate necessity to discuss it any further. The flat planar Earth is therefore the only possible shape remaining, and it proves to be that shape which admits of diagrams soundly explanatory of this angle or its working equivalent in relative displacement or parallax, at the apparent position of the luminary, or at the junction of the converging lines of altitude.

The necessity for this angle or its equivalent in parallax, or relative displacement, is apparent when the increase or decrease of the altitude is considered. For instance, the observer at Shanklin with altitude 61 degrees marches and sails towards Balta, taking altitudes as he progresses, and his altitudes *increase* day by day to the same star which is North of Balta: but how do they increase, and where does the increase come from? How does the angle at the base get *larger* and larger daily? Plainly only by some other angle getting *smaller* and smaller daily, and that angle is the angle, or its *equivalent* at the luminary. To prove this, I start him off from Shanklin at 61 degrees, and passing a declination through the luminary, I have at once a triangle formed of angles 90 degrees and 61 degrees at the base, and of angle 29 degrees at the supposed luminary. What is angle 29 degrees? *The co-altitude*, and its position is *forced* as the *third* angle of the triangle. I then march my man on to Balta, where I have a triangle formed of angles 90 degrees and 71 degrees at the base, and of angle 19 degrees at the supposed and displaced luminary, thus proving beyond the smallest shadow of a doubt, that an angle or its equivalent has been formed

between 29 degrees and 19 degrees, and at the junction to the luminary, and that angle is 10 degrees : just the angle wanted, and with angles at the base 61 degrees and 109 degrees, which with 10 degrees at the place of *junction* make up the triangle of 180 degrees.

It is therefore plain and evident, that the increase in the altitude between 61 degrees at Shanklin and 71 degrees at Balta is rendered possible by the relative displacement of the apparent luminary, or the angle at the apparent luminary, and which as the co-altitude is 29 degrees at Shanklin and 19 degrees at Balta, with a difference of 10 degrees : now the distance between Balta and Shanklin in statute miles is by the received count 690 miles, which divided by 10 gives 69 miles for the degree of latitude, and gives 10 degrees of latitude between Balta and Shanklin. Here we see that the latitude is *subordinate* to the angle at the luminary, and this is the outcome of the geometry of the plane, from which true geometry alone, the rules for observations in navigation, distinctly appear to me to have been derived. I am confident that the false geometry of the sphere has not supplied and could not supply the rule for finding meridian altitudes with the mariner's sextant.

To return to the diagram marked F, the angle at the junction towards the luminary instead of being 12 degrees is only 2 degrees, and it is evident that the increase of the altitude at Balta is due entirely to the mechanics of the sphere, which creates a new horizon at Balta, and thus alters the base line, and renders *the natural triangle impossible ;* and if it were held that the altitudes 61 degrees and 73 degrees are to the same luminary, then nature would be contradicted again, in that there would not be that relative displacement of the luminary which we have a right to expect as between Shanklin and Balta, and which the commonest observation of a lighthouse, church-steeple, or distant mountain, will assure us does really take place in nature. Again, there is a complete want of analogy between angle 2 at the luminary and the 10 degrees of latitude between the two stations. In addition there is no guide whatever by means of which a rule for observations could be deduced.

At every step in this investigation of angle 12 evidence accumulates to show that the angle should have been 10. I however have been compelled to treat of this angle as 12, and I also have been fully authorised in finding the approximate height of the star, or point of junction, by that angle, for in so doing I have disproved the gross estimation in *distance* quoted for the star by the Astronomer Royal, and this I have done from his own figures.

Angle 12 is further disproved by the parallels of 61 degrees and 71 degrees, which are undoubtedly by the rule, in the *proportion* of altitudes to the same luminary, on a departure of ten degrees of latitude. Further, diagram Figure G offers evidence, which however I do not advance as more than evidence of great weight that angle 12 degrees is not an angle formed by the junction of altitudes to the same star. This evidence lies in the fact, that should the point of junction of the altitudes be regarded as even the approximate place of the star, then the high improbability and as I think impossibility is brought prominently forward, that the greater altitude 73 degrees would give a less plumb-height to the star than the smaller altitude of 61 degrees, and as found from the same station ; and the very meaning of the word altitude is rendered of less than its genuine weight : the word altitude relatively to latitude as subtended by a luminary means of course height, and it follows that the greater altitude of 73 degrees should give a greater height than 61 degrees, and such I feel certain is the rule in altitudes when correctly demonstrated. The diagram shows the reverse ; shows the greater assumed altitude as giving the less height, and therefore it is suspicious from that fact alone, that angle 12 degrees cannot be formed by the junction of altitudes to the same star, and it is impossible as altitudes, which altitudes shall conform to the proportion in the rule commonly used in navigation.

Diagram Figure G shows Balta and Shanklin as on a plane extended Earth, and altitudes 61 degrees and 71 degrees are drawn as the true altitudes to the same star, and their point of junction forms an angle of ten degrees, equal to the difference of the co-altitudes of these two altitudes, and though the point of junction marking angle ten is not the place of the star, yet that point gives us an approximate distance for the star, and from Balta, and that distance at sixty miles to the degree of latitude I find to be 3,022 miles. The plumb-height would be less than 3,000 miles, but these amounts are approximations, and the full height of the star will be nearer 3,400 and odd miles.

These amounts contrast *strangely* with the language of the Astronomer Royal, who never fails to speak of the distance of a star under millions or possibly billions of miles,—and in addition the whole body of astronomers are so much in error and so *little understand geometry*, that they never speak of the problem in its true character, as of the *height* of the star : the plumb-height from that part of the Earth, over which, as over a huge plane, the star or other luminary is in motion.

The two problems, the *distance* of the sun, and the *height* of the sun, are quite distinct, though on my plan they may be approximated from the same figure ; but the everyday use by the astronomers of the expression *distance* of the sun forms the strongest evidence that they have never advanced to a *true apprecia-tion of the genuine problem.*

A good deal more might be written on the subject of the angle at the luminary, but as such further examination of the subject would make this publication too bulky for my present purpose, I will terminate this work, and point out that in connection with the third illustration from page 137, and on the distance to the moon from Greenwich and the Cape of Good Hope, the angle at the moon could not possibly be $1\frac{1}{2}$ degrees as stated by the Astronomer Royal, but just as angle 2 degrees in diagram F is condemned for various reasons, but especially in that it does not equal the latitude between the two stations, so in like manner the angle $1\frac{1}{2}$ degrees claimed as the angle at the moon is condemned in that such angle should and must, at a point of junction formed by the production of the altitudes from Greenwich and the Cape of Good Hope—equal the latitude between those two stations ; and also there must be at the apparent luminaries an amount of relative displacement equal to an angle amounting to the latitude between those two stations : *otherwise navigation would be impossible.*

Finally with regard to these experiments, and especially to that between Balta and Shanklin, had the experiments been conducted with the *mural circle,* or even with the mariner's sextant, between Shanklin and Balta, the legitimate issues of the observations would have been *naturally apparent ;* and would have naturally been the subject of that attention which perforce would have gravitated to such legitimate issues, and which attention, formed the especial object of the expedition or experiment. The altitudes would then have been taken in the usual manner ; and as the mischievous element of the plumb-line would have been wanting, so attention could not have been diverted from the altitudes to the assumed divergence of the plumb-lines, nor could attention have been diverted from the movement of the telescope to the assumed move-ment of the plumb-line by gravitation : nor yet could attention have been diverted from the legitimate position of the angle coinciding with the difference of the altitudes, and have been foolishly fixed upon that difference as controlling the action of the plumb-line.

In short, the plumb-line would have been removed entirely out of the experiment, and there would not have been the smallest

excuse for neglecting the ordinary rules for observations ; nor the smallest pretext for the published confusion which has been widely issued to the public, and which as deception of an especially degrading nature, and as unholy and blasphemous, has been most unfortunately taught in public schools and universities, throughout Great Britain and the world at large, and has been persevered in and upheld by titles and honours conferred upon the especial offender, in spite of and neglect of protest, remonstrance, and warnings which would have constrained the attention of any honourable body of men in the world : but which has all been set aside with that defiant contempt which is the birthright of at once the dishonest and ungodly in all walks of life.

SOME OF THE AUTHOR'S WORKS.

THE INDIAN-TROOP POLICY. Published in 1871. I am the author of the late policy of employing Indian troops for European purposes. I published this policy with Mr. J. Camden Hotten during the French and German war, under the titles of "The John's Uncle Pamphlet" and "John's Uncle Addenda." The surprise which this policy created when actually carried out, confirms me in claiming an *especial originality* for a policy which is as sound as it is sagacious and bold, and which can certainly claim to have led to the salvation of the British Empire. My Pamphlets sold to the extent of several thousand copies in a few days' time, and not only did they originate the policy, but they did more, for they educated a great mass of people, and thus aided to render the policy the more possible.

MIDDLETON'S GREAT LIBERATOR PAMPHLET. Judd and Co. Sixpence.

THE CRUISE OF THE KATE: The Narrative of the Author's Extraordinary Voyage Single-handed round England in 1869. Longmans. Six Shillings.

THE ÆNEID OF VIRGIL. (First Two Books in Rhymed Verse.) Longmans. Six Shillings.

MIDDLETON'S NEW PROCESS OF MEASURING THE HEIGHT OF THE SUN. Judd and Co. One Shilling.

MIDDLETON'S MAP OF THE WORLD: The Great Puzzle Solved. Judd and Co. One Shilling.

MIDDLETON'S SCIENTIFIC CHART OF THE WORLD AND OBLATE SPHEROID. Quote the title in full, or you may receive the Map instead. This is of interest to sailors and yachtsmen, &c. One Shilling.

THE VARIATION OF THE NEEDLE. Second Edition. A most powerful work, proving the Earth an Extended Surface, and showing the New Method of finding the Longitude by Track-sailing and Rule of Three. Five Shillings.

MIDDLETON ON SPACE. Unpublished. As soon as you understand that the Earth is flat, it becomes a mere confident and well assured statement that the heavens are flat. However, though it may be safe and is certainly *easy to say* that the heavens are flat, it is a totally different matter to *prove* that the heavens are flat. My work on Space *proves* this important point by geometry. This work has now been some six years awaiting publication.

MIDDLETON'S BRIEF FOR THE PROSECUTION OF MODERN ASTRONOMY. This brief is the original work from which my impeachment has been softened down, and in some sort copied. The brief is a work of exceeding power, containing long and important charges, and powerful conclusions summarizing the matter from a legal point of view: though founded on the same evidence as is here published in my impeachment, it is a work of a totally different character, and with an interest quite its own. This work is at present unpublished.

Price One Shilling, Plain; Mounted on Canvas, Five Shillings.

MIDDLETON'S MAP OF THE WORLD, showing it to be a plane, and giving the distance to and from Australia, very well approximated to the distances of practical Navigation! This Map shows the antarctic courses from the capes to Australia, fairly clear of the sun's path. This is a purely skeleton-map, and anyone would be very much mistaken to expect the ordinary detail. The objects of this Map are to show the comparatively small East, and politicians are advised that to understand the Eastern question properly it is needful to understand the true size of the East, which I hold to be perhaps a half of what it is shown on the globe, as measured in the air! Another object of the Map is to represent the idea of the Earth as a plane, and to show a possible route from the Horn to Sydney in Australia clear of the sun's path. This Map is to scale, and I regard it, as it applies to Europe and Asia, with a great deal of reverence, for though I have given much time to map-making and have since made several maps, I have found it exceedingly difficult to surpass this one as regards Europe and Asia, and in conformity with the rest. The Map has many and evident errors, but it is the *initial* step in the right direction, and should be respected.

Ready for Immediate Publication.

THE TRUE PHYSICS OF THE AXIS OF THE EARTH! This work proves that the axis of the Earth cannot represent a series of parallel lines in various parts of the Earth's supposed orbit around the sun, and that therefore the changes of the seasons would be impossible on a globe. Gravitation is proved impossible, and the work ends with asking—what becomes of gravitation in a gale of wind?

THE VARIATION OF THE NEEDLE, in connection with the Earth's supposed revolution in orbit around the sun. This work contains some very beautiful diagrams, illustrative of the Earth in its supposed orbit! It enters into nutation and precession, showing an enormous nutation of 47 degrees, and a precession of twelve hours in six months, as resulting from the supposed revolution of the Earth around the sun. It, as the above work, proves the motion of the Earth quite impossible, in connection with the variation of the magnetic needle.

ON THE ECLIPSE OF THE MOON FOR AUGUST 23rd,

1877. This important Paper is unpublished! It especially draws attention to the fact that the Moon *contacted* with the obscurity exactly on the *wrong side* to suit the theory that the Earth's shadow causes the eclipse. The Moon was to the east of the obscurity and travelling west, whereas it should have been to the west of the obscurity, and further to suit the impossibility of astronomy, the Moon should have been travelling to the east, catching up the obscurity, which latter should have been east of the Moon, in that the Moon must be held to travel much the fastest. The very reverse occurred in Nature. It was this eclipse which led directly to the charges of my Brief.

The sacred verses which appear below are from the original document: they were written on myself when an infant, and their history is as follows. The lines were studiously concealed by Mrs. Middleton from either myself or my sister, and I came to a knowledge of their existence purely accidentally, and comparatively a short time ago. I took them at once to my mother, who had apparently forgotten their existence, but who told me the *name of the writer*, and that the document was presented to her by the writer. Mrs. Middleton resides at No. 9, Anglesea Place, Southampton. I find that I published a pamphlet on April 9th, 1873, and the verses were most certainly not known to me then; for if they had been, they would have been published as part of that pamphlet. It is most singular that my life has gravitated towards the prayer of those verses, *without any knowledge on my part whatever of the said verses*. The photograph shows the age of the document very well, by bringing out a number of duplicate lines formed by the paper having been folded during a great number of years.

On seeing POPSY MIDDLETON *asleep on board the Swift Packet, on her passage from Falmouth to Barbadoes.*

Sleep and while slumber weighs thine eyelids down
May no foul phantom over thy pillow frown
But brightest vision deck thy tranquil bed
And angels' wings o'er-canopy thy head
Sleep on sweet boy may no dark dreams arise
To mar thy rosy rest thou babe of Paradise.

Bid oh Almighty Father, God, and Friend
Religious glories on thy steps attend
To shine through all the dreamy storms of life
A splendid beacon in the world of strife
And when to thee recalled you sink in death
May prayer and praise still bless your parting breath.

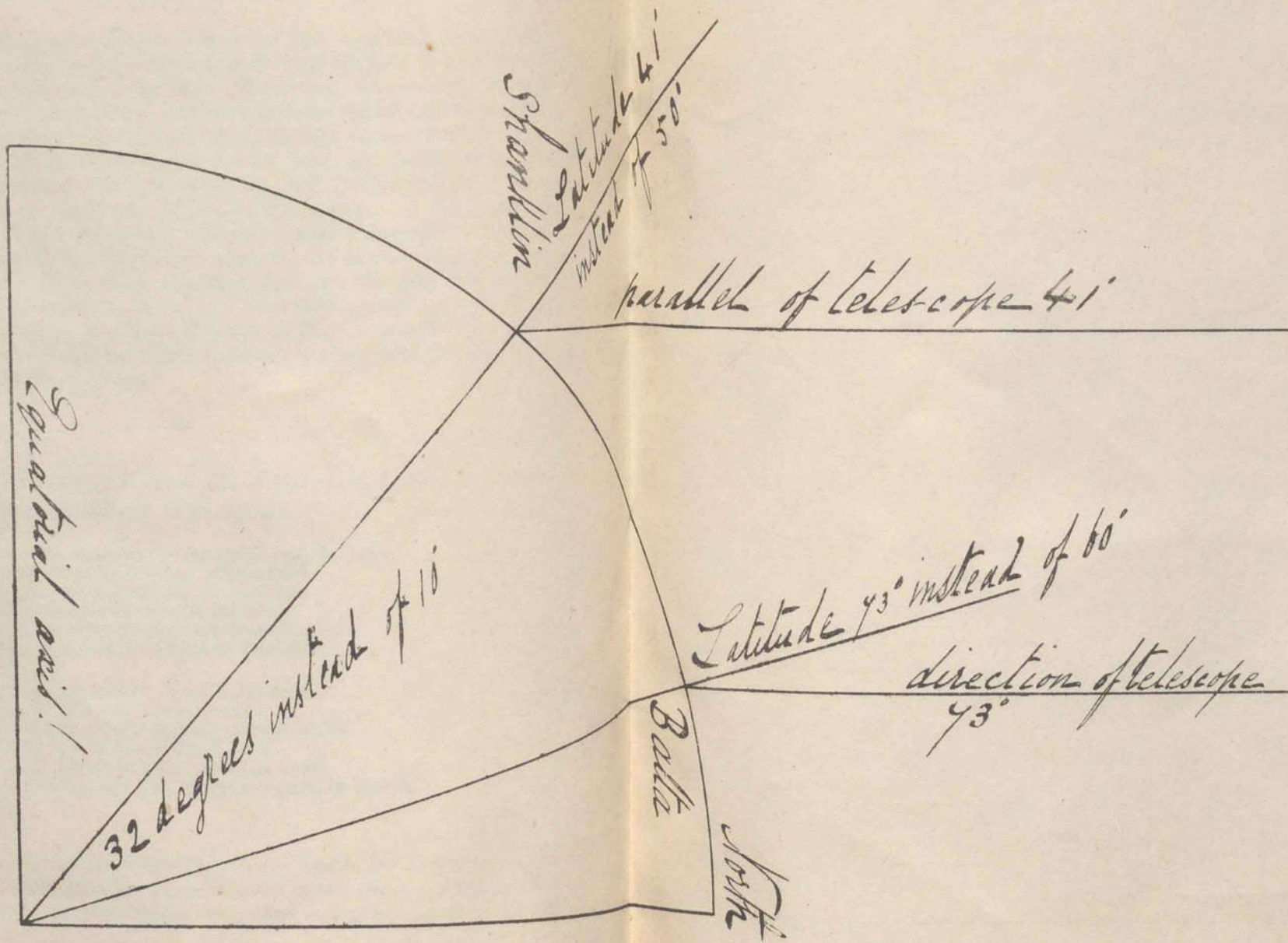Saturday, 24th April, 1841.   N.

Photographs of the original document of these verses, and to the full size, can be had for 2s. 6d. each. No Photographer is authorised to sell them. Photographs of these verses have been sent to the leading Societies and other Authorities in England.

The leading Publishers in York have seen the original document of these verses.

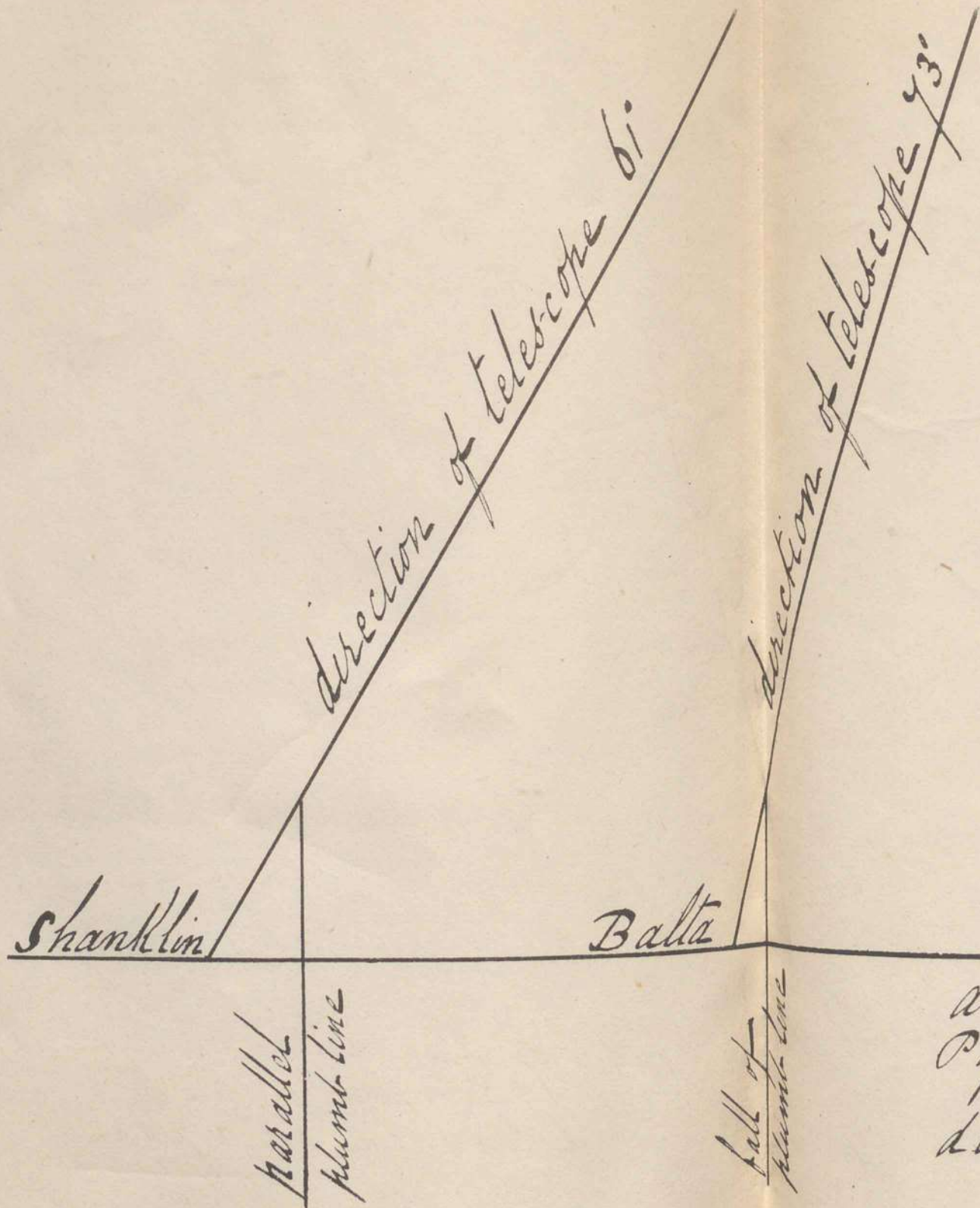I regard these verses with the greatest veneration and reverence.

Figure A

Latitude 41'
instead of 55°

Shoreline

parallel of telescope 41'

Equatorial axis!

32 degrees instead of 10

Latitude 73° instead of 60°

direction of telescope
73°

Baltic

North

direction of telescope 61°

direction of telescope 43°

Figure B

Shankllin

Balta

parallel plumb line

fall of plumb line

an approximation of
Popular Astronomy page
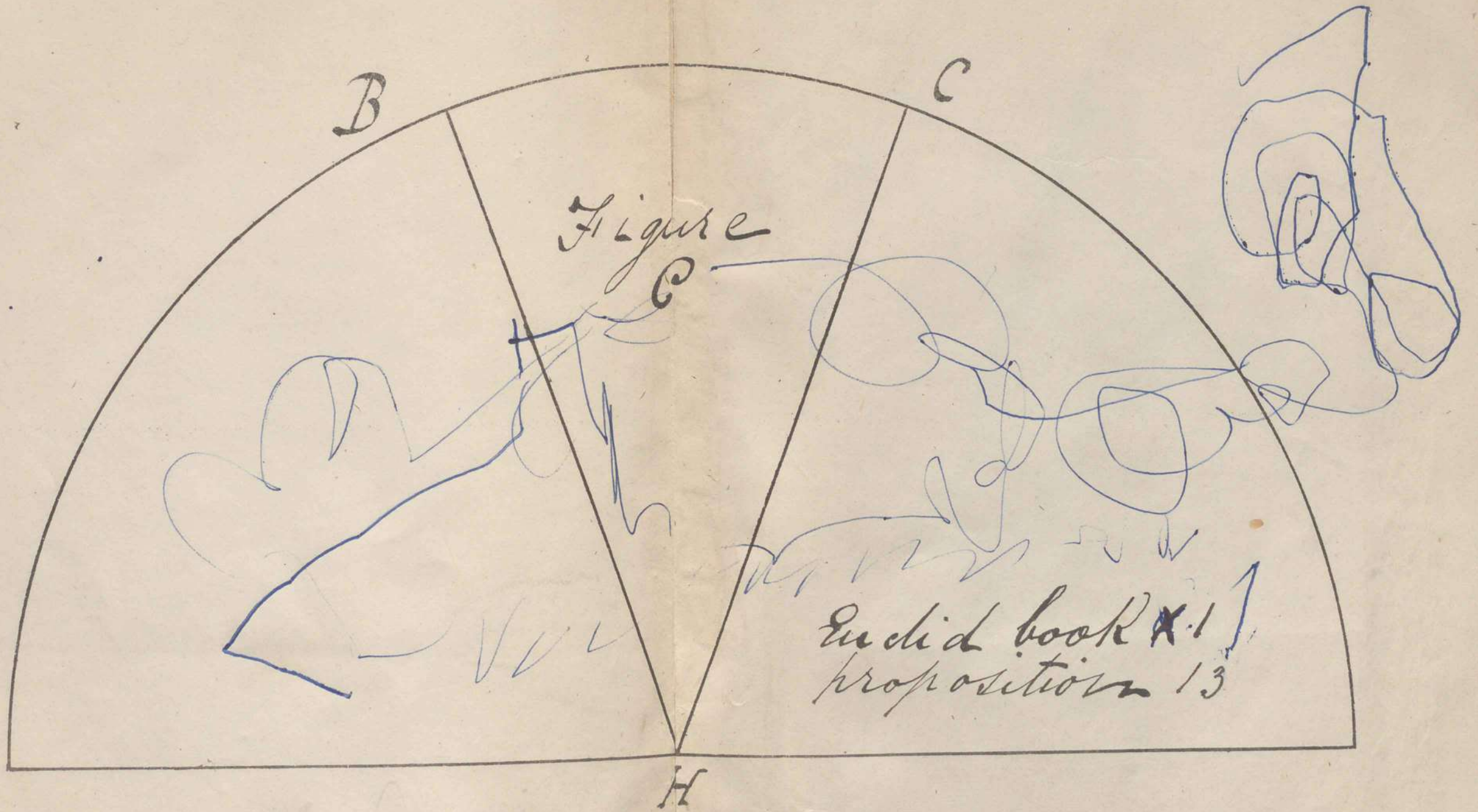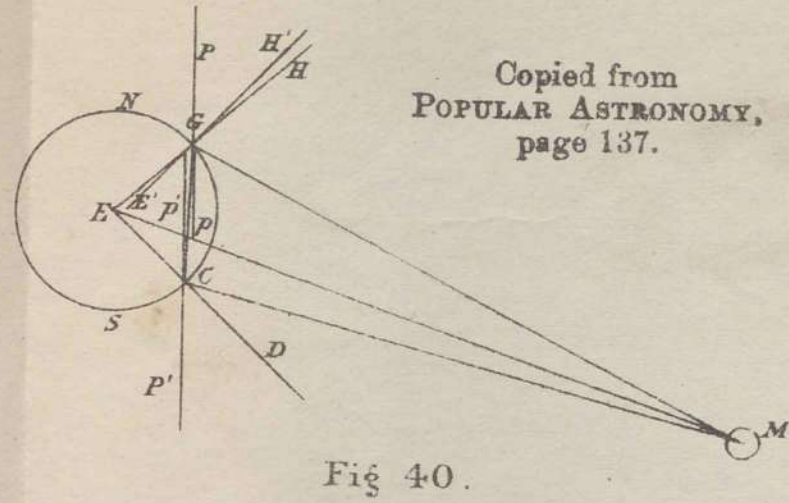155 as more correctly
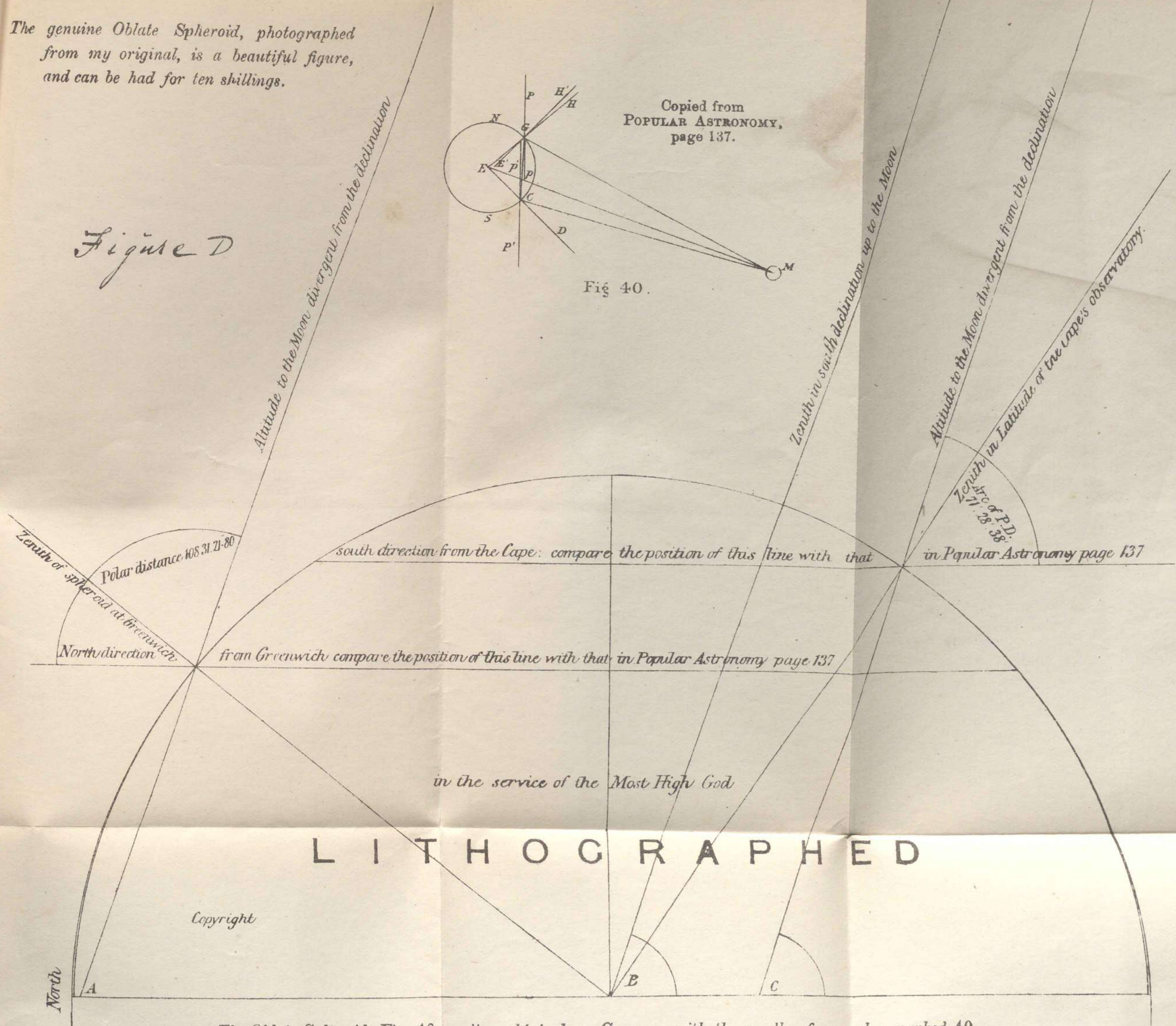delineated on a plane.

B

C

Figure
C

Euclid book X·1
proposition 13

H

The genuine Oblate Spheroid, photographed from my original, is a beautiful figure, and can be had for ten shillings.
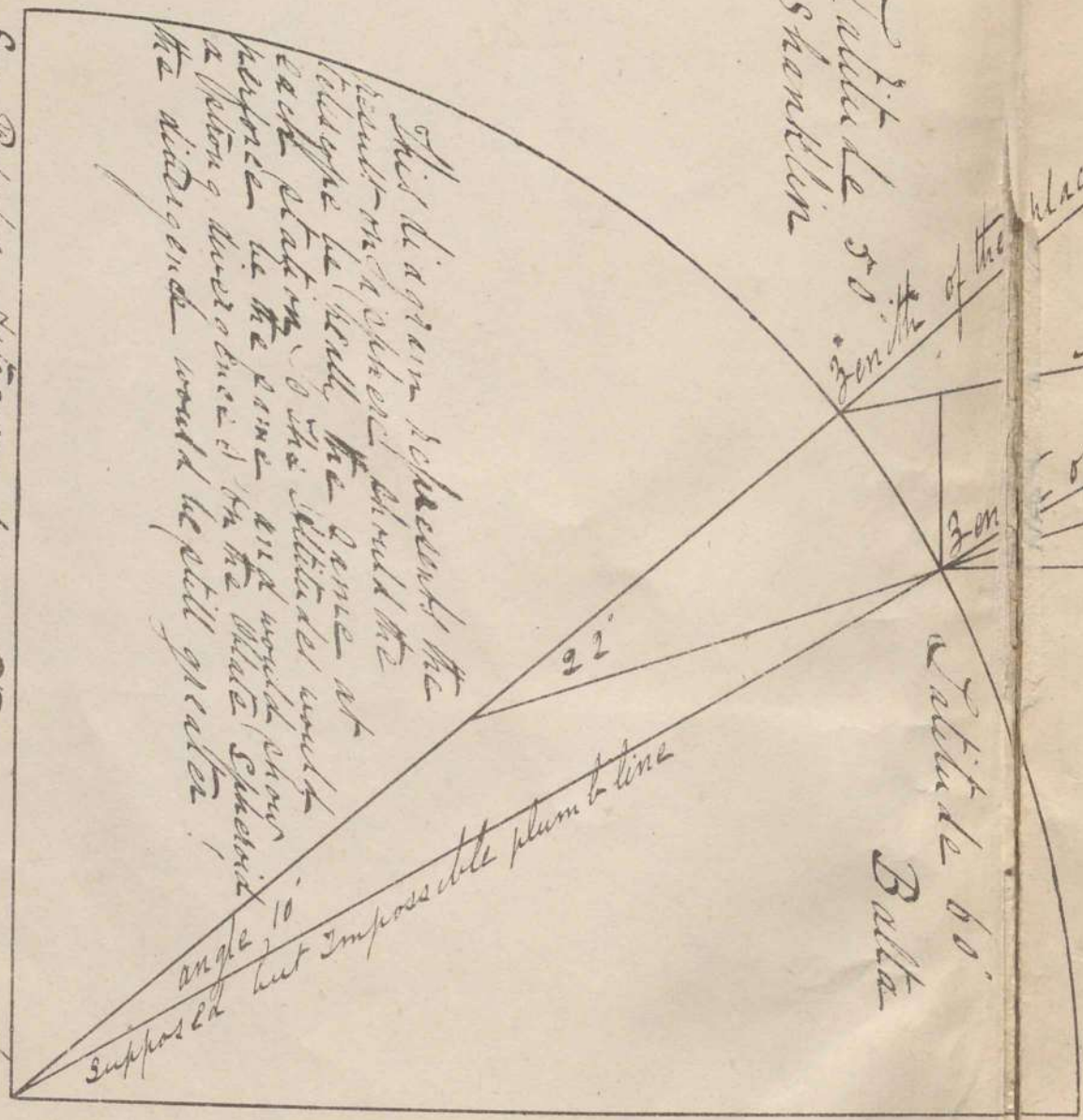
Figure D

Fig 40.

Altitude to the Moon divergent from the declination

Zenith in south declination up to the Moon

Altitude to the Moon divergent from the declination

Zenith in Latitude of the Cape's observatory.

Zenith of spheroid at Greenwich

Polar distance 108.31.21·80

south direction from the Cape: compare the position of this line with that

Arc of P.D. 71.28.38.

in Popular Astronomy page 137

North direction — *from Greenwich compare the position of this line with that in Popular Astronomy page 137*

in the service of the Most High God

LITHOGRAPHED

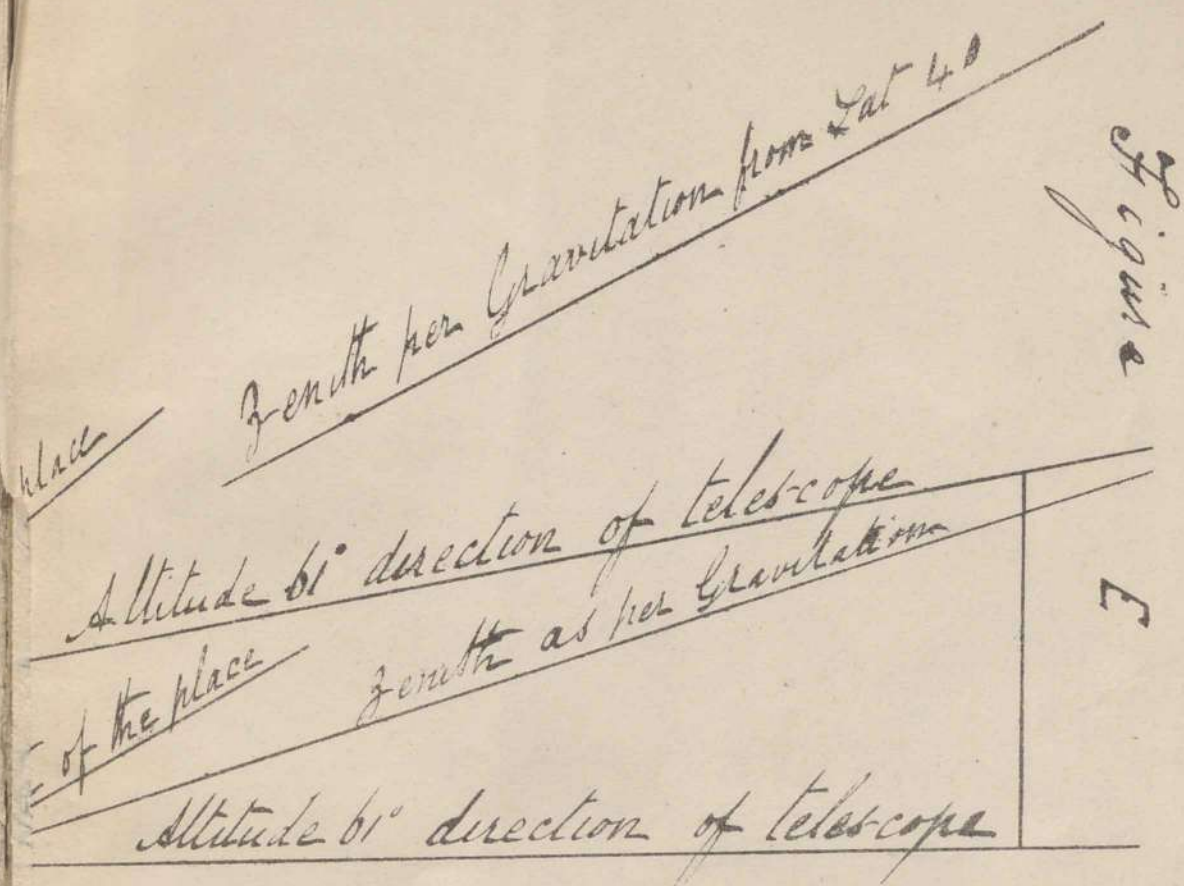Copyright

North   A   E   C

The Oblate Spheroid, Fig. 40, as it ought to be. Compare with the smaller figure, also marked 40.

This figure forms as terrible and complete an exposure of Astronomy as can possibly be printed.

Figure E

Zenith per Gravitation from Lat 40

Altitude 61° direction of telescope
of the place

Zenith as per Gravitation

Altitude 61° direction of telescope

North

Latitude 50° Shackleton

Zenith of the place

Latitude 60° Ball

Zen

22°

angle 10°

Supposed but Impossible plumb line

This diagram is intended to illustrate the absurdity of the current theory. As the telescope would be inclined to the zenith, so the latitude would be altered and each star would appear in a different place. A plumb line, which would have to be inclined to the zenith, a plumb line which would be still greater —

See Popular Astronomy page 53

Copyright!

Figure F.

Zenith of the place

Lat 50'
Shanklin

parallels

Altitude

Lat 61'
Balta

North

10°
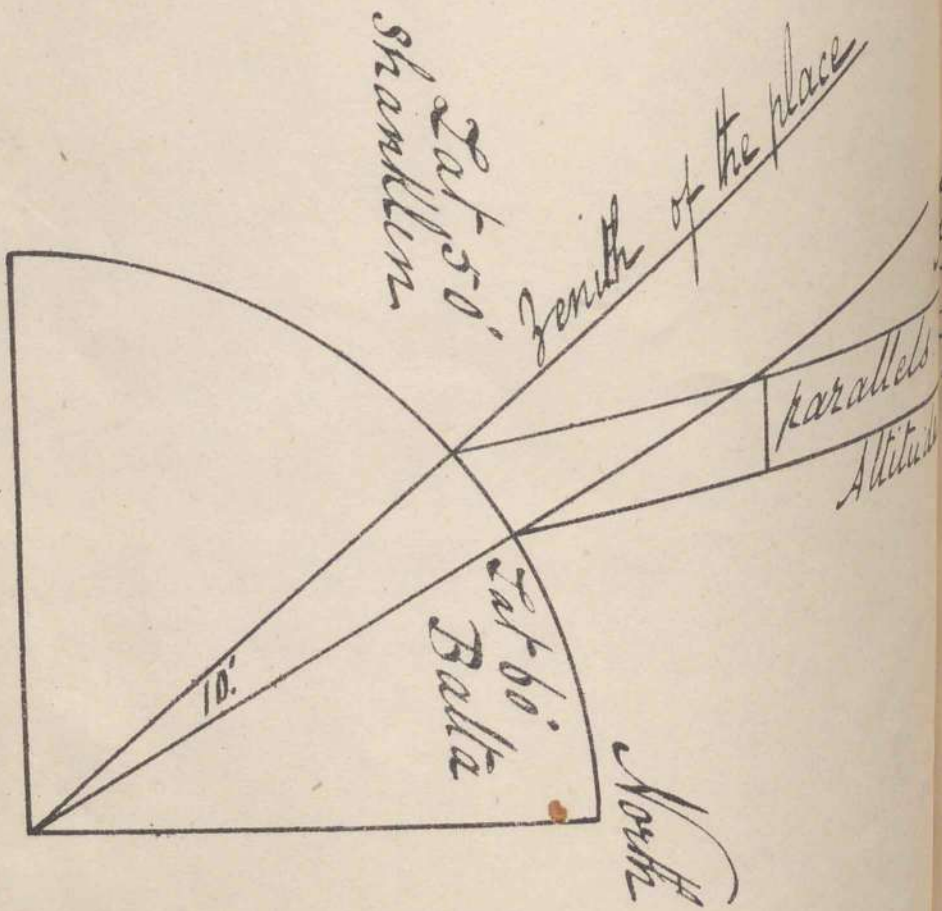
itude 61'

Altitude 73° distance about 15,541 miles

Copyright!

Altitude 61°

fall of plumb line

Zat 50

Zat 60

streamline

Balls

Altitude 61°

parallel of plumb-line

Altitude 73°

Altitude 71°

12°

angle 10°

Figure 5
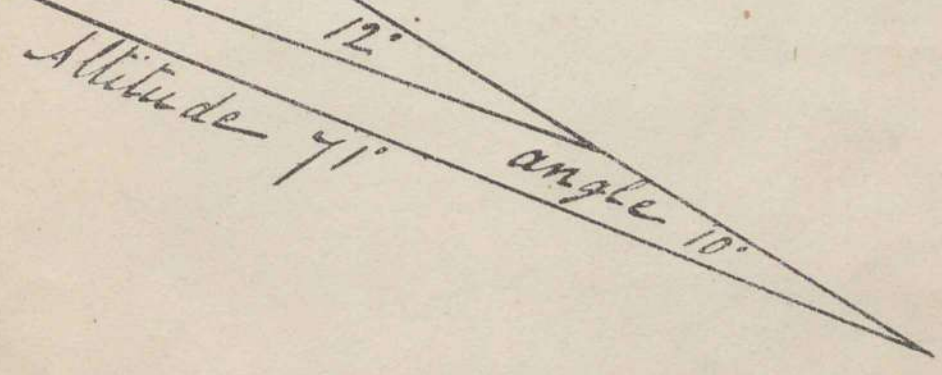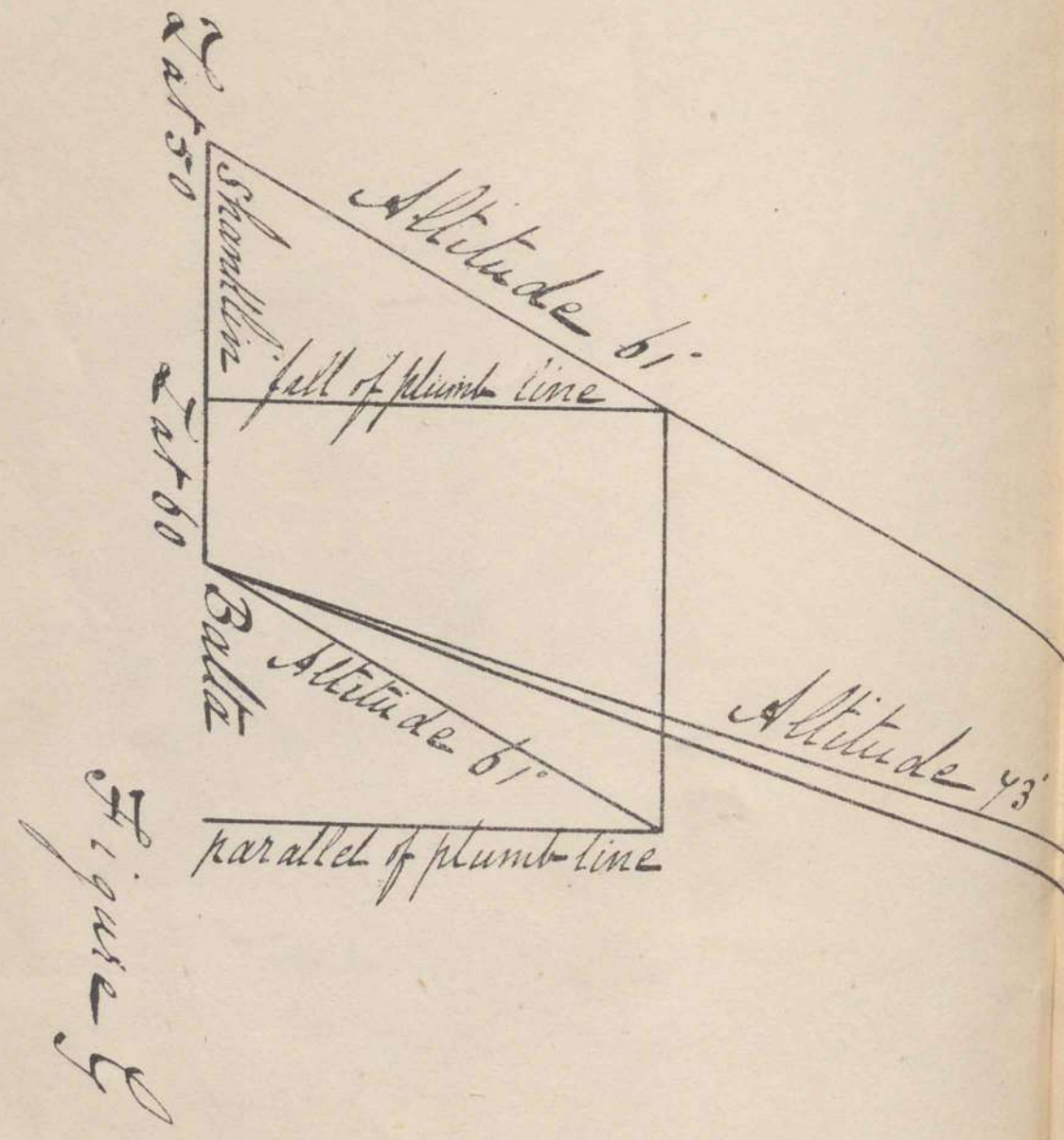
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