

X. *An Answer to the Objections stated by M. De la Lande, in the Memoirs of the French Academy for the Year 1776, against the Solar Spots being Excavations in the luminous Matter of the Sun, together with a short Examination of the Views entertained by him upon that Subject. By Alexander Wilson, M. D. Professor of Practical Astronomy in the University of Glasgow; communicated by Nevil Maskelyne, D.D. F. R. S. and Astronomer Royal.*

Read January 16, 1783.

TO THE REV. DR. MASKELYNE.

DEAR SIR,

Glasgow Colleg<sup>e</sup>  
April 20, 1782.

HAVING lately seen a paper on the Solar Spots by M. DE LA LANDE, in the French Memoires for 1776, published in 1779, I found such mention made of mine upon that subject, as appears to me to require an answer; not only in justice to myself, but to that honourable Society who admitted an account of my discovery into their Transactions, and to philosophy in general, whose cause it becomes a duty upon her followers to vindicate from such errors as seem likely to prevail.

The universal circulation of the volumes of the French Academy makes me the more solicitous to be heard in answer; and to have that answer conveyed to the public through the channel  
of

of the Philosophical Transactions, as being the only one where an appeal can be made equally general and extensive.

It has been my aim to draw up the paper, which at this time I take the liberty of transmitting, with that freedom which the important interest of truth demands, and to observe at the same time those rules of decorum, of which too it becomes philosophers, of all other men, to set the example.

Give me leave, therefore, to request, as a very particular favour, that you will be so good as to communicate the inclosed to your colleagues in the Council of the Royal Society; as also this letter, in which I wish to express the most unreserved deference to your and their opinions and determination.

I am, &c.

MAC FARLANE Observatory, April 18, 1782.

IN the first part of my paper, published in the Philosophical Transactions for 1774, I have explained how, from the lucky accident of seeing the great solar spot of November 1769, in a certain critical situation upon the disk, its real nature was obtruded on my thoughts by a train of appearances the most obvious and unequivocal. The reader may there also see how, from phenomena perfectly similar in spots of the usual size, I was led to a general conclusion, and to believe that all spots, small as well as great, which consist of a dark nucleus, and surrounding umbra, are excavations in the luminous matter of the sun.

Before this time no opinions had been entertained of any third dimension belonging to the spots, but what agreed to the conception of their being something material extending more

or less above the common level of the sun's spherical surface. This idea, though so very prevalent, seems however to have originated rather from pre-conceived theoretical notions of the nature and constitution of that vast body, than from any phenomena of a constant and marked kind observable in the spots themselves.

The views which I have offered upon this subject disclaim, I believe, for the first time all alliance with hypothesis, and seek after a permanence in the strong holds of no imaginary system. They make a direct appeal to our senses, upon the testimony of which alone their merits must finally rest.

As for myself, situations will sometimes occur where it would be as inconsistent with the spirit of the most guarded philosophy to harbour doubts, as on other occasions to be weakly credulous. When in the year 1769 I first mentioned the discovery in the London Chronicle, and even at the time of sending a full account of it to the Royal Society, I considered it as too early to offer my sentiments in a manner intirely decided. But in the lapse of eight more years the subsequent observations have all conspired to strengthen, and even to perfect, my convictions. Now, therefore, I have no hesitation in pledging any credit which may belong to me, as an astronomical observer, to the present and to future times, for the reality of that discovery which is laid down in the first part of the paper above mentioned.

But though I maintain that any person, with a good telescope, and with good eyes practised in observing, cannot fail of beholding frequently in the sun the phenomena I have pointed out, it yet may require the more palpable dimensions of some future great spots, in order to satisfy people less accustomed to examine and to judge of objects by means of glasses. This is

the more to be expected, as I find, that even from amongst those the most profoundly skilled, a demand has lately been made upon me to uphold what I have advanced, and to remove what seem to be stated as unfurmountable objections.

Having some little time ago seen the *Memoirs of the French Academy* for the year 1776, published in 1779, I there find my paper on the Solar Spots has come under the notice of a member of that illustrious body, whose name is justly held in great esteem by all astronomers, and to whom astronomy itself has many obligations. The author alluded to is M. DE LA LANDE. Though I should have been much flattered to have found my views supported by an authority so truly respectable, yet, even in his endeavours to oppose me, I honour him as a philosopher who has taken so much pains to vindicate what he doubtless believes to be juster opinions. In the most perfect confidence of a generous indulgence on his part, and with equal attachments to philosophy and to truth, my present intention is very freely to offer what arguments occur to me in favour of the solar spots being such as I have described.

First of all, it has been urged, as an objection of great weight, that the absence of the umbra on one side, when spots are near the limb, as so fully explained in my paper, is not constant. As to the fact, the reader may there see, that I was myself sufficiently aware of it, having stated three cases from my own observations, when I did not perceive this change to take place. The rev. FRANCIS WOLLASTON, LL.B. F. R. S. is the only person who (in the *Philosophical Transactions*) has bestowed any remarks on my publication; and though he with great candour acknowledges that, generally, the umbra changes in the manner I have determined, yet he expresses a difficulty as

to my conclusions, on account of this circumstance not obtaining universally.

Under similar expressions M. DE LA LANDE, in the Memoire before me, produces from his own observations, which appear to have been long continued, only three cases of the same kind, and from the ancient observations of Mess. CASSINI and DE LA HIRE, four more. In regard to these last, I am not sure if such obsolete ones ought to be referred to in a question of the present kind. Those excellent observers, entertaining no thoughts that any thing of moment depended upon a nice attention to the form of the spots, might easily overlook less obvious circumstances, especially when they were found near the limb. We may add further, that, even when so situated, they retain the umbra at both ends, and that whole side of it which lies farthest from the center of the disk and these parts in the aggregate, they might sometimes mistake for the umbra as not deficient in any particular place. That they did not affect what must have appeared to them a needless refinement in accuracy may be collected from the following expression of M. DE LA HIRE, found in the Mem. Acad. 1704, p. 10. As to this spot, says he, “ Je ne donne point ici, les figures differentes sous “ lesquelles cette tache a paru ;” and, among other reasons for this, he gives the following: “ Car il me semble qu'on ne “ peut pas tirer d'utilité de ces sortes de figures qui changent “ continuellement.”

But even admitting the anomaly we at present consider to be much more frequent than can be contended for, still such cases can only be brought as so many exceptions from a certain *general law*, or uniformity of appearance, from which the condition of by far the greater number of spots is most undeniably deduced. The utmost, therefore, that can hence be alledged

is, that some few spots differ from all the rest, or from the multitude, and are not like these excavations in the sun. Such cases or exceptions will not surely warrant the conclusion, that no spot can be an excavation. This would be to reverse all the rules of a just induction, by opposing to an irrefragable general argument, the force of one extremely limited and feeble.

But notwithstanding these few instances where the umbra is not found to change, when we consider how perfectly all spots resemble one another in their most striking features, there naturally arises some presumption for all under that description we have given partaking of one common nature; and for this only difference in the phenomena depending upon something which does not necessarily imply a complete generical distinction.

It comes therefore to be inquired, how far spots, which when near the middle of the disk appear equal and similar in all things, may yet differ from one another considered as excavations, or as possessing the third dimension of depth, and how far the peculiar circumstances by which they may disagree can contribute to make some resist this change of the umbra, when near the limb, much more than others.

In order to this, suppose two spots which occupy a space upon the sun corresponding to the equal arches GD (fig. 1.); and let GM, DM, be drawn so as to coincide with the plane of the excavation in such case. The breadth of the nucleus being commonly equal to that of the surrounding umbra, if the base MD of the triangle GDM conceived rectilineal be divided in L, so as  $ML : LD :: MD : DG$ ; and if through L be drawn LS parallel to DG, then will DGSL be the section of two spots having this condition, and which as to sense would, when far away from the limb, be equal in all apparent measures;

measures; though very unequal in the third dimension HE or depth of the nucleus SL, and also in the inclination DGM of their sides to the spherical surface of the sun. Now it is manifest, from the construction of the figure, that the distances AB, AK, from the limb A, when the sides GS of the umbra disappear, must depend very much upon the last of these two circumstances; and that, according as the angle of inclination DGM is less, the respective spot will go nearer to the limb than the other before the side of the umbra GS vanishes. But those very exceptions to the general phenomena which we are at present examining are of this kind, and may, perhaps, from what has been now shewn, proceed wholly from the shallowness and the very gradual shelving of some few spots which break out in certain tracts of the sun's body over which the luminous matter lies very thinly mantled.

If, therefore, upon such principles it can be shewn, that spots, similar to the rest, may sometimes go to the limb without the one umbra contracting sensibly more than the other, the objection we are at present considering will be entirely removed, and it will be allowable to conclude, that even these few spots are excavations like all the rest, though shallower, as it would be quite unphilosophical to multiply distinctions concerning their nature, where there is found no necessity for so doing.

In order to avoid circumlocution, we may call that side of the umbra which lies nearest the limb the *nearest umbra*, and the side opposite the *farthest umbra*; and to enter more particularly into the consideration now before us, let us suppose a spot of 40'' over all, with its nucleus and umbra equally broad; then will the depth of the nucleus and the apparent breadth of the nearest umbra, when the plane of the farthest comes to coincide with the visual ray, be as expressed in the following examples,

examples, in which the apparent semi-diameter of the sun is supposed 16', and his parallax 8".5.

	Farthest umbra supposed to vanish when distant from the limb.	Depth of nucleus in English miles and in seconds.	Apparent breadth of nearest umbra.
I.	1 0	4.54 - 2118	8.58
II.	0 30	3.09 - 1442	6.02
III.	0 15	2.09 - 975	4.13
IV.	0 8	1.44 - 672	2.87

Now, because in every aspect of a spot, the real breadth of either the farthest or nearest umbra must be to the projected or apparent breadth, as radius to the sine of the angle which this respective plane makes with the visual ray, it follows, that at any time before the spot comes so near the limb as is expressed in the above examples, the apparent breadth of the nearest and farthest umbra cannot differ so much as by the quantity there set down for the apparent breadth of the nearest, when the other is supposed to vanish. Regarding, therefore, the farthest and nearest umbra of the spot in case IV. as two neighbouring visible objects which turn narrower by degrees as the spot goes toward the limb, we should undoubtedly judge that they contract as to sense alike, since so long as the farthest could be perceived, the other cannot appear to exceed it by a quantity that we could distinguish; and by the time the plane of the former coincides with the visual ray, the extreme nearness to the limb would prevent our forming any certain judgement of either.

From this last example, therefore, it appears manifest that a spot, answering to the description and conditions there mentioned, or one a little more shallow, would approach the limb, and



and finally go off the disk, without that peculiar change of the umbra on one side, which is so obvious on common occasions, notwithstanding it were an excavation, whose nucleus or bottom is so many miles below the level of the surface.

In the four cases above stated, the distance of the remotest part of the nucleus from the sun's limb when the visual ray coming from it is just interrupted by the lip of the excavation, or, in other words, the distance of the nucleus from the limb when it is totally hid was also computed. These distances are as follow :

Case 1.	-	16.93	Case 3.	-	4.70
2.	-	8.90	4.	-	2.70

and it is remarkable from the two last, how very near the limb a shallow spot of not more than 40'' in diameter may come before the nucleus wholly disappears.

Computations of this kind are very easily made, by supposing on (fig. 2.) GDLS a section of a spot so near to the limb A, that the visual ray VB coincides with GS the plane of the excavation. Let the straight line DF coincide with the other side, and draw the radii CA, CQ, so as to be at right angles to GB, DF, and draw the radius CH through the point M.

Now, because the versed sine AB, the apparent distance from the limb, when the side of the umbra GS vanishes is given, the arch GA of the sun's circumference is given, and from the known breadth of the spot, the arch GD and its half GH are each of them given, and consequently the arches HA, DA, and QA, are all given. From these data the angles and sides of the triangle GMD, supposed rectilinear, may be deduced, also HM the distance of the point of intersection M from the surface. When these particulars have been determined for any assumed distance BA, and assumed extent of the spot

spot GD, the depth of the nucleus HE, may be found by the following analogies.

By similar triangles  $MD : DG :: ML : LS$ . But LS being the breadth of the nucleus is, by hypothesis, equal to LD, the breadth of the umbra; therefore  $MD : DG :: ML : LD$ , or as  $ME : EH$  on account of the parallels SL and GD. By composition,  $MD + DG : DG :: MH : HE$ . Hence HE, the only unknown quantity of these proportionals, is found, and is the depth of the nucleus sought. If DP, the apparent breadth of DL, the side of the umbra next the limb be required, corresponding to the present aspect of the spot, this may be derived from the data by the resolution of the triangle DLP, whose hypotenuse DL and angle DLP, are known. Again, if the nearest apparent distance of the nucleus from the limb, when it wholly disappears, be sought, it will come out equal to the versed sine of the arch GA, when diminished by half the arch HA, which last is by construction equal to half of either of the acute angles of the triangle DGM. In order to see the reason of this, it must be considered, that the segments ML, LD, of the base MD, are in the same ratio with the sides DG, GM; on which account a straight line drawn from G to L, the last part of the nucleus which can be seen, must bisect the angle DGM. Therefore, before the visual ray or line ZGL can fall perpendicularly upon the radius CA, this must be drawn distant from the point A towards D, by an arch corresponding to half the angle DGM, that is, by an arch equal to half of HA.

Perhaps it may be urged, that very shallow spots ought always to be known from the rest, and to discover themselves, by a surrounding umbra very narrow compared to the extent of the nucleus; but we know far too little of the qualities of the luminous matter, and of the proximate causes of the spots,

to say any thing at all upon a point of this kind. The breadth of the umbra is, as assumed in the computations, commonly about equal to that of the nucleus, though sometimes it varies more or less; but how far these relative dimensions indicate depth or shallowness must be expounded only by observation, and not by any vague or imperfect notions of the nature and constitution of the sun.

The mention of a pit or hollow or excavation several thousands of miles deep, reaching to that extent down through a luminous matter to darker regions, is ready to strike the imagination in a manner unfavourable to a just conception of the nature of the solar spots as now described. Upon first thoughts it may look strange, how the sides and bottom of such vast abyfles can remain so very long in sight, whilst by the sun's rotation they are made to present themselves more and more obliquely to our view. But when it is considered, how extremely inconsiderable their greatest depth is, compared to the diameter of the sun, and how very wide and shelving they are, all difficulties of this sort will be entirely removed.

Unless, however, we duly attend to these proportions, our notions upon the subject must be very erroneous; and it seems the more necessary to offer this caution, as this very thing is inaccurately represented in fig. 9. belonging to the Memoire under review, and in a way that may lead to mistakes. Instead of exhibiting a spot as depressed below the surface of the sun one hundredth part of his semi-diameter, the section of it is there determined by two lines drawn from the circumference, and meeting in a point at the prodigious distance of one-fifth of the semidiameter below. This author's known clear and comprehensive ideas of every thing relating to the sphere have doubtless led him to think, that any particular attention to exactness was unnecessary in this representation;

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but as my design, on the present occasion, is to write and to explain matters in a popular way, rather than to astronomers, it will be proper to assist the conceptions of those who are but little versed in mathematical principles by such diagrams as will shew things in their just proportions. Any reader, therefore, who pleases, by turning to fig. 3. may see how very small a portion of the sun's body is made up of the luminous matter when supposed every where 3967 English miles deep. Fig. 3. A is a section of a spot of 50'' diameter situated in the deepest part of this resplendent substance.

For my own amusement I have pursued this subject further in the way of ocular proof, by a model of the sun and of the spots upon his body according to their proper dimensions. This I put into a convenient wooden frame, and viewed it afar off when set upon a stand, whilst the globe was turned slowly round, and subtended an angle at the telescope equal to the apparent diameter of the sun. By an object-glass micrometer I then took the distances from the limb when the farthest umbrae of different spots vanished, as also the distances of the nuclei just when disappearing. The apparent subtense of the umbra next the limb was also measured in this way, together with the visible extension of some great spots within the disk, when the extreme limits of the nearest umbra coincided with the limb. In all these experiments the effect was very striking, and the phenomena remarkably consonant to calculation, and to what I have often seen upon the real sun in the heavens.

The globe I got made consists within of two strong hollow hemispheres, formed by pasting slips of paper upon a well-turned ball of wood, and afterwards fastened together upon an iron axis in the way commonly practised. Over this were repeatedly laid coats of Spanish white and glue, applied when in a thick paste, till at length this outward shell

became of a considerable thickness. To give the whole a true form, the two projecting poles were locked up in two grooves when coinciding with the diameter of an iron semi-circle, whose inner edge was so fastened as to cut away the redundant parts of the last coat of the chalk nearly dry, whilst the globe was continuedly and slowly forced round. By thus repeatedly paring off the protuberances, and supplying new paste when deficient, and forcing the globe round against the cutting edge as before, it at length became quite smooth and spherical. After this, when slowly dried, it turned very white, and then the spots or excavations were made in its surface by boring instruments of steel as in fig. 4. constructed in all their cutting edges from a scale of parts of the globe's diameter. This done, I penciled the bottom of the hollows all over black with China ink, and distinguished the shelving sides from the full whiteness of the outward surface by a shade of the pencil which was darkest towards the external border. I hope the indulgent reader will excuse me for having been so particular in regard to this artificial sun, as possibly what I have mentioned may facilitate a like construction, should any person deem it worth while to entertain himself with such experiments.

But to proceed; what has now been insisted on at so much length concerning the shallowness and the more gradual shelving of some few spots, will also apply to another objection, which M. DE LA LANDE views in a strong light.

Here we find quoted the great spot in 1719, seen by M. CASSINI; and, for the second time, that of June 3, 1703, seen by M. DE LA HIRE; both which, on their arrival at the limb, are said to have made an indentation or dark notch in the disk; and this phenomenon is mentioned as absolutely incompatible with spots being below the surface.

It is most true, that if we look for any thing like this, when the plane which coincides with the external boundary of the spot, passes through the eye (the way that M. DE LA LANDE considers the matter, vide his fig. 9th) it must be very large indeed before the disk could be perceived deficient by any dark segment. But may not a spot, even no larger than M. CASSINI's, considered as an excavation, make, in a manner very different from this, something like a notch; for, by the way, this phenomenon is not in the Mem. Acad. nor any where else, that I know of, described with any sort of precision.

M. CASSINI's great spot, by which we understood the nucleus, was one of 30''; and supposing the umbra equally broad, its diameter over all must have been 1' 30''. It would, therefore, occupy an extent upon the sun's surface of 5<sup>o</sup> 22' fully. Now, suppose a circular space of that size upon the sun, distinguished from the surrounding lustre by such a failure of light as is peculiar to some spots, and suppose that it just touches the limb, it would still subtend an angle of more than 4''. This being the case, might not a dusky shade, more or less remarkable, according to the darkness of the umbra, commencing at the limb, and reaching inwards upon the disk, or, in other words, a notch be perceived? Had M. CASSINI's spot been a very shallow excavation, it appears by case fourth, formerly stated, that when viewed in this aspect, some small part of the nucleus might have been yet visible, and might have contributed along with the shade of the farthest umbra, and the still deeper and broader shade of the two ends of the umbra, to mark out the indentation.

Should it be said, that these notches are always distinct jet black impressions on the disk, of an obvious breadth, and originating entirely from the opaque nucleus conceived as some-  
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thing prominent above the general surface, this can be shewn inconsistent with some circumstances we find accidentally mentioned in the case of M. DE LA HIRE'S spot: for of this great one, it is said, that when only 8'' distant from the limb, the nucleus was seen as a very narrow line. This was on June 3, 1703, at six o'clock in the morning. Now, forasmuch as, at that time, its alledged elevation must have been to its apparent subtense, very nearly, as radius to cosine of that arch of the sun's circumference whose versed sine was the 8'' of distance from limb, it is impossible that its breadth could have increased sensibly in its further progress towards the limb; and how any obvious black notch could be produced by the elevation contended for in this case, is not conceivable.

I do not imagine, therefore, that the phenomena of notches in the disk, so inconsiderable and dubious as these seem to be, are by any means a proof of projecting nuclei, or that they are not reconcileable to spots being depressions in the sun. A large shallow excavation, with the sloping sides or umbra darker than common, may, as has been shewn, be more or less perceptible at the limb: and what, perhaps, is a further confirmation of this, and seems to evince that a concurrence of such circumstances is necessary, is, that sometimes even large spots make no indentation. M. CASSINI, in *Mem. Acad. tom. X. p. 581.* describes the great spot of 1676, which he saw at its entrance with a telescope of 35 feet, as an obscure line parallel to the limb, but no where mentions that it made a notch in it.

Though we now and then see the surrounding umbra darker than at other times, yet when spots are deep, and the umbrae but little dusky, it is indeed impossible that we should see any thing of them, even though large, very near the limb: for here even the nucleus which lies buried cannot in the least

contribute to the effect, as it may do a little before its state of evanescence when spots are very shallow: accordingly cases of this kind are perfectly agreeable to experience. This fact seems to have met SCHEINER as a stumbling-block, when he was intent upon bringing forward every possible argument for the spots being something which project beyond the surface. "Non raro contingit," says he, *Rosa Ursina*, p. 511. "ut magnæ folis maculæ antequam ad horizontem accedant, repente ita luce obruantur, ut omnino videri nequeant."

In reasoning concerning the nature of the spots, and particularly about their third dimension, the only arguments which are admissible, and which carry with them a perfect conviction, are those grounded upon the principles of optical projection. If, for example, by far the greater number of them be excavations, some thousands of miles deep, certain changes of the umbra would be observable when near the limb, as has been shewn at so much length. Were they very shallow, or quite superficial; both sides of the umbra would as to sense contract alike in their progress toward the limb: for if in case 4th, above stated, the spot had been supposed superficial, the apparent breadth of the side of the umbra next the center of the disk, would have then been only  $1''.62$ , and that of the side opposite  $1''.27$ . Now, the whole of either of these quantities, and much more their difference, would be quite insensible. Again, if the nucleus extended much above the common level whilst the surrounding umbra was superficial, we should behold manifest indications of this by such an opaque body when seen very obliquely being projected across the farthest side of the umbra, and by hiding the whole or part of it before the time it would otherways disappear. According to this or that condition of the spot, such things must infallibly obtain

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by the known laws of vision; and hence arguments resting upon such principles may be denominated *optical* ones. On the other hand, when spots are contemplated near the middle of the disk; a great variety of changes are observed in them, which depend not upon position, but upon certain physical causes producing real alterations in their form and dimensions. It is plain, that arguments derived from the consideration of such changes, and which, on that account, may be called *physical arguments*, can assist us but little in investigating their third dimensions; and, from the nature of the thing, must be liable to great uncertainty. The author of the *Memoire*, in p. 511. &c. takes new ground, and proceeds with a number of objections, depending upon that sort of reasoning which we have last defined. I must take notice, that a certain distinction has been here overlooked, which in my paper I endeavoured to point out. Presuming upon our great ignorance of many things which doubtless affect deeply the constitution of that wonderful body the sun, I offered, in part II. an account of the production changes and decay of the spots, considered as excavations, in the most loose and problematical manner, stating every thing on this head in the form of queries. This account, crude and imperfect as it is, appeared to me much less incumbered with difficulties than any other, and of this some striking examples are there set before the reader. But I have expressly owned, that many circumstances still remained unexplained; and upon the whole marked out the theory, if such it may be called, as very imperfect. Nature unquestionably abounds with numberless unthought-of energies, and modes of working most curiously and most wisely adapted to all situations in the material world: and in regard to that system of economy which is established in the sun, producing

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there such a strange fluctuation of appearances, human reason, even when aspiring by the most enlarged analogy, must recoil under a consciousness of the unfathomable resources of nature, and of its own dark and limited sphere. “Demiraberis qui-  
 “dem sine dubio” (says HEVELIUS, speaking of the Sun, Cometographia, p. 412.) “quod tam brevi tempore, spatio ali-  
 “quot dierum, quin horarum, adeo miris et horribilibus sub-  
 “jiciantur mutationibus, ac vicissitudinibus!”

Hence I would remark, that whatever inconsistencies are imagined in the account I have delivered in part II. though such may be justly chargeable upon certain principles there assumed, yet they ought not to be stated as presumptions against the spots being really excavations or depressions in the luminous matter of the sun. This opinion must rest entirely upon the evidence held forth in the first part of the paper, whatever be the fate of the account laid down in the second. It does not enter there as an hypothesis, but as a matter of fact, previously established by *optical arguments*; and from optical arguments alone can there arise even any just presumptions against it. The lameness of the views given in part II. may probably proceed, as we have said, from our very imperfect knowledge of the vast range of physical causes which obtain in the universe. But whatever be their defects, no doubts ought to arise, upon such grounds, of the spots being themselves what direct observation declares them, namely, excavations in the sun. Whether their first production and subsequent numberless changes depend upon the eruptions of elastic vapour from below, or upon eddies or whirl-pools commencing at the surface, or upon the dissolving of the luminous matter in the solar atmosphere, as clouds are melted and again given out by our air; or, if the reader pleases, upon the annihilation

and reproduction of parts of this resplendent covering; is left for theory to guess at. Though, however, many difficulties should occur in an attempt of this kind, it would certainly be unreasonable on that account to call in question the third dimension of the spots, as previously determined by arguments which are liable to no fallacy, and which are unconnected with every kind of theoretical reasoning.

Now, in the Memoire before me, this sort of distinction has escaped the notice of the author. His optical arguments, indeed, as they regard the first part of my essay, put on a just and proper claim to be heard, and have now, as we conceive, been fully answered. But superadded to these are many others, which, though they relate very properly to the view I have given in part II. and to that alone, yet finally are summed up along with the rest, as not only militating strongly against that particular view, but in order to disprove that the spots are excavations in the luminous matter.

I here think it but justice to that honourable Body of Gentlemen who, in the year 1774, composed the Council of the Royal Society, or the Committee of Papers, to mention, that the publication of the second part of mine was more owing to their having consented to my request, than to their own sentiments in regard to the fitness of so doing. But as I had bestowed some pains upon drawing up these views, and as care had also been taken to distinguish between fact and any thing like to theory, and as the latter was propounded only in the form of queries, there appeared to me no harm in letting that second part go forth also; especially as I flattered myself, that thereby a greater curiosity would have been excited, and the subject of course sooner inquired into by observation.

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As I conceive it, however, of some importance to have the distinction above treated of perfectly understood in future, I now purposely avoid entering upon any theoretical ground whatever. My wish therefore is, that the author of the *Memoire* may acquit me of every thing not perfectly respectful, though I do not follow him through that train of objection founded upon vague and incompetent physical arguments, which is to be met with in p. 511. &c. By further considering the particulars hinted at in p. 21. and 29. of my paper, several difficulties, perhaps, may be removed; but we forbear any illustration of this kind, chiefly to evince how little we concern ourselves whether the views delivered in part II. can stand of themselves or not. Those who do not like the principles there assumed, or the conclusions drawn from them; in short, those who will call part II. a theory, and who think it a bad one, may, if they please, mend it, or contrive a new and a better one of their own. But so long as they cannot, by irrefragable optical arguments, set aside the induction laid down in part I. we must demand of them, so to fabricate their theories as to account for the various circumstances of the spots, considered as things which possess three dimensions, *viz.* length, breadth, and depth, or, in other words, as excavations in the luminous matter of the sun.

This fact is the only one I am solicitous to maintain or to contend for; and for a very good reason, because I consider it as actually demonstrated by competent observations. As such, to indulge for a moment in a figure, it would be a pity not to rescue it from being drawn into the eddy of some treacherous theory, the nature of all which is to sweep into their vortex and finally to precipitate to the bottom every thing which obstructs their impetuous career.

Sir ISAAC NEWTON, perceiving too well this proneness to system, has laid down his fourth rule of philosophizing, that arguments of induction may not be evaded by hypothesis. It will become us, therefore, in all things, and in the present subject in particular, to have respect to so excellent a precept. In speaking hereafter of the solar spots, let us separate what things claim to be heard as matters of fact from what rest upon the sandy foundations of mere theory, and no longer confound them together.

Since upon this topic, I humbly beg the indulgence of the reader whilst I advert to a certain abuse of terms, which is but too prevalent in books of philosophy, both in our own country and upon the continent. What I have to say relates to this word *hypothesis*. “*Quicquid enim non deducitur ex phænomenis hypothesis vocanda est,*” are the words of Sir ISAAC NEWTON in his general scholium. And yet real discoveries, founded upon the best induction, are sometimes mentioned by the appellation of such and such a one's hypothesis. I have often thought, that this impropriety of language owes its continuance to the force of custom, and that it is one of those badges we still retain of that disgraceful state philosophy lay under before the æra of experiment and observation, when almost every thing was hypothesis and theory both in name and in reality.

Most kind of hypothesis regards true philosophy with so unfriendly an aspect, that we should be careful at least not to contaminate matter of fact and certain truth with so inauspicious a denomination. I would also remark, that none which do not carry with them great marks of probability should be brought into view, even in the way of hints or queries, for suggesting further experiments and observations; and that far

less-ought systems, built upon notions evidently incongruous, to have a place in any modern book of philosophy. This has a tendency still to favour that devious path, that *false taste*, which it concerns philosophy so much to guard against and to discourage.

It remains now only to make a few strictures upon M. DE LA LANDE'S theory of the solar spots, humbly submitting them to the consideration of the reader. The import of it is, "that the  
 " spots as phenomena arise from dark bodies like rocks, which  
 " by an alternate *flux* and *reflux* of the liquid igneous matter  
 " of the sun, sometimes raise their heads above the general sur-  
 " face. That part of the opaque rock, which at any time thus  
 " stands above, gives the appearance of the nucleus, whilst  
 " those parts, which in each lie only a little under the igneous  
 " matter, appear to us as the surrounding umbra."

In the first place it may be remarked, "that the whole  
 " proceeds upon *mere supposition*." This, indeed, the author himself very readily acknowledges. Though, therefore, it could not be disputed by arguments derived from observation, yet conjecture of any kind, if equally plausible, might fitly be employed to set aside its credit. Choosing, however, to avoid a tedious discussion of this kind, or to try it upon the phenomena which are enumerated in p. 511. &c. by entering into arbitrary and disputable principles, we shall confine ourselves to such particulars as appertain to the more obvious character of the spots, and which also seem to be irreconcilable with the theory; and first of all, in regard to the distinguishing features of the umbra.

M. CASSINI, *Mém. Acad.* tom. X. p. 582. plate 7: and M. DE LA HIRE, *Mem. Acad.* 1703, p. 16. and I may add all other observers, and all good representations of the spots, bear testi-

testimony to the exterior boundary of the umbra being always well defined, and to the umbra itself being less and less shady the nearer it comes to the nucleus. Now it may be asked, how this could possibly be according to M. DE LA LANDE'S theory? If the umbra be occasioned by our seeing parts of the opaque rock, which lie a little under the surface of the igneous matter, should it not always be darkest next the nucleus, and from the nucleus outward should it not wax more and more bright, and at last lose itself in the general lustre of the sun's surface, and not terminate all at once at the darkest shade, as in fact it does? These few incongruities, which meet us as it were in the very threshold of the theory, are so very palpable, that of themselves they raise unfurmountable doubts. For, generally speaking, the umbra immediately contiguous to the nucleus, instead of being very dark, as it ought to be, from our seeing the immersed parts of the opaque rock through a thin stratum of the igneous matter, is on the contrary very nearly of the same splendour as the external surface.

Concerning the nucleus, or that part of the opaque rock which stands above the surface of the sun, M. DE LA LANDE produces no optical arguments in support of this third dimension or height. Neither does he say any thing particular as to the degree of elevation above the surface. But from what has been already hinted in the course of this paper, it appears, that if this were any thing sensible, it ought to be discovered by phenomena very opposite to those which we have found to be so general.

Again, a flux and reflux of the igneous matter so considerable as sometimes to produce a great number of spots all over the middle zone, might affect the apparent diameter of the sun, making that which passes through his equator less than  
the

the polar one, by the retreat of the igneous matter towards those regions where no spots ever appear. But as a difference of this kind of nearly a thousandth part of the whole would be perceivable, as we learn from M. DE LA LANDE'S OWN observations, compared with those of Mr. SHORT, in *Histoire Acad.* 1760, p. 123. it would seem, that the theory had also this difficulty to combat. Further, when among spots very near one another, some are observed to be increasing, whilst others are diminishing, how is it possible that this can be the effect of such a supposed flux and reflux? This last inconsistency is mentioned by the author himself, who endeavours to avoid it, by making a new demand upon the general fund of hypothesis, deriving from thence such qualities of the igneous matter as the case seems to require; and such must be the method of proceeding in all systems merely theoretical.

But it is unnecessary to pursue at more length illusive speculations of this kind, especially as we lie under a conviction, founded upon fact, of the theory being utterly erroneous. It hardly differs in any respect from that proposed by M. DE LA HIRE, and a little amended by the writer of the *Histoire de L'Acad.* for 1707, p. 111. This near agreement, indeed, is taken notice of by M. DE LA LANDE himself, in his excellent astronomy.

The writer of the *Histoire de L'Acad.* for 1719, p. 76. after reviewing the merits of this theory, and comparing it with several phenomena of the spots which had been observed for the four preceding years, pronounces it unsatisfactory, and concludes his remarks with the following expression: " Il sera plus naturel de croire qu'il se fasse dans le soleil des generations nouvelles, dependantes de quelque cause plus ou moins forte selon les *circonstances inconnues* ou elle se trouvera."



Views, much of the same kind, were even entertained by some so long ago as the days of SCHEINER, as we find mentioned by that indefatigable author in his *Rosa Ursina*, p. 746. “Non equidem me latebat,” says he, “non deesse, qui putant maculas solares esse quasdam in sole prominentias et *veluti montes*; sed cum hæc ex anticipata mentis affectione, phænomenique solaris ignoratione procedant neglexi.” And further on he adds: “Astronomi sinceri est phænomenon sequi non antevertere: veritatem ex objecto accurate observato, non objectum invitum ad arbitraria figmenta trahere.”



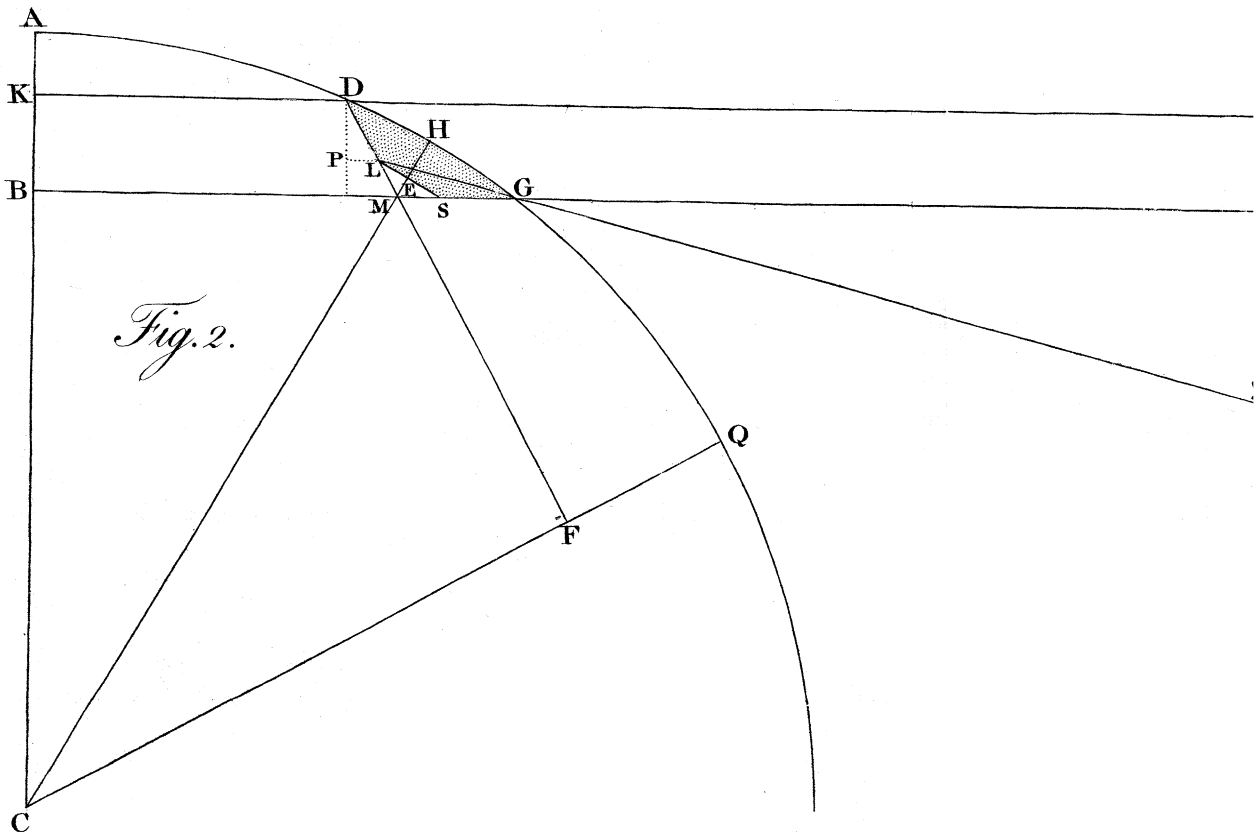
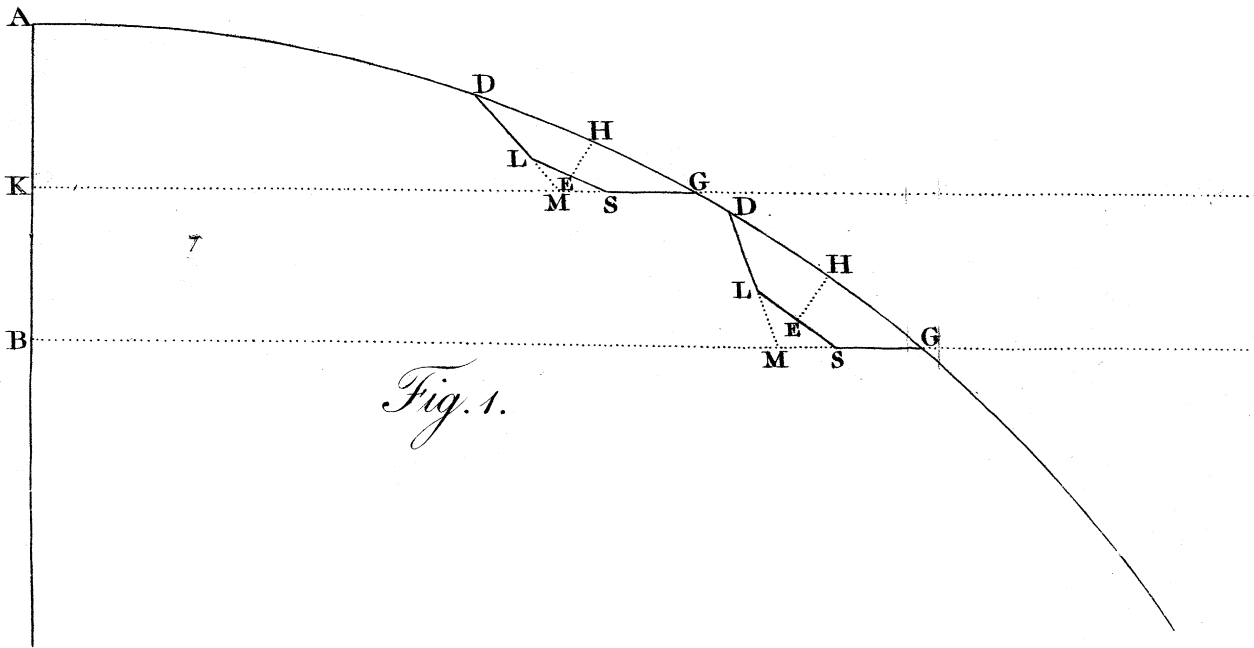
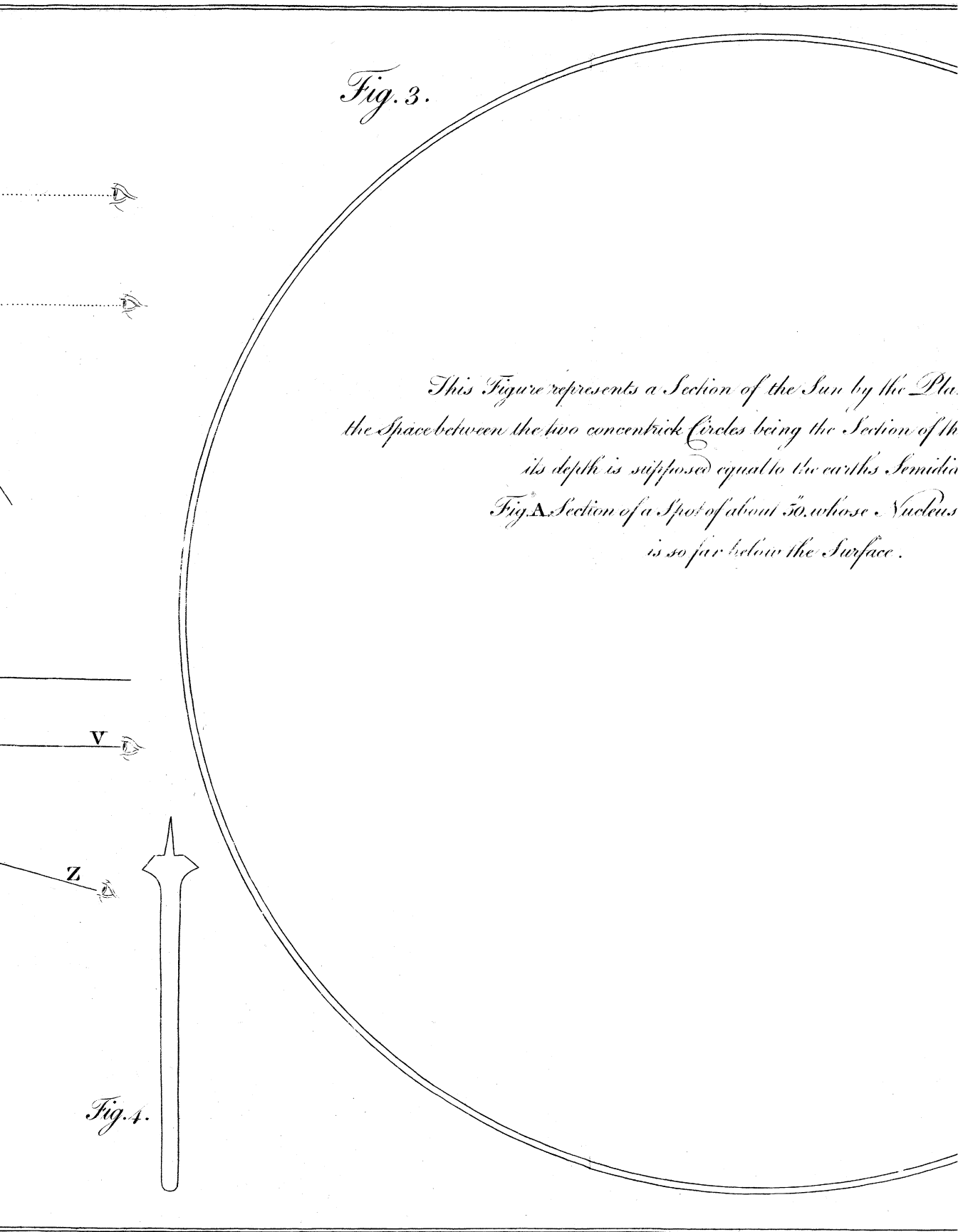
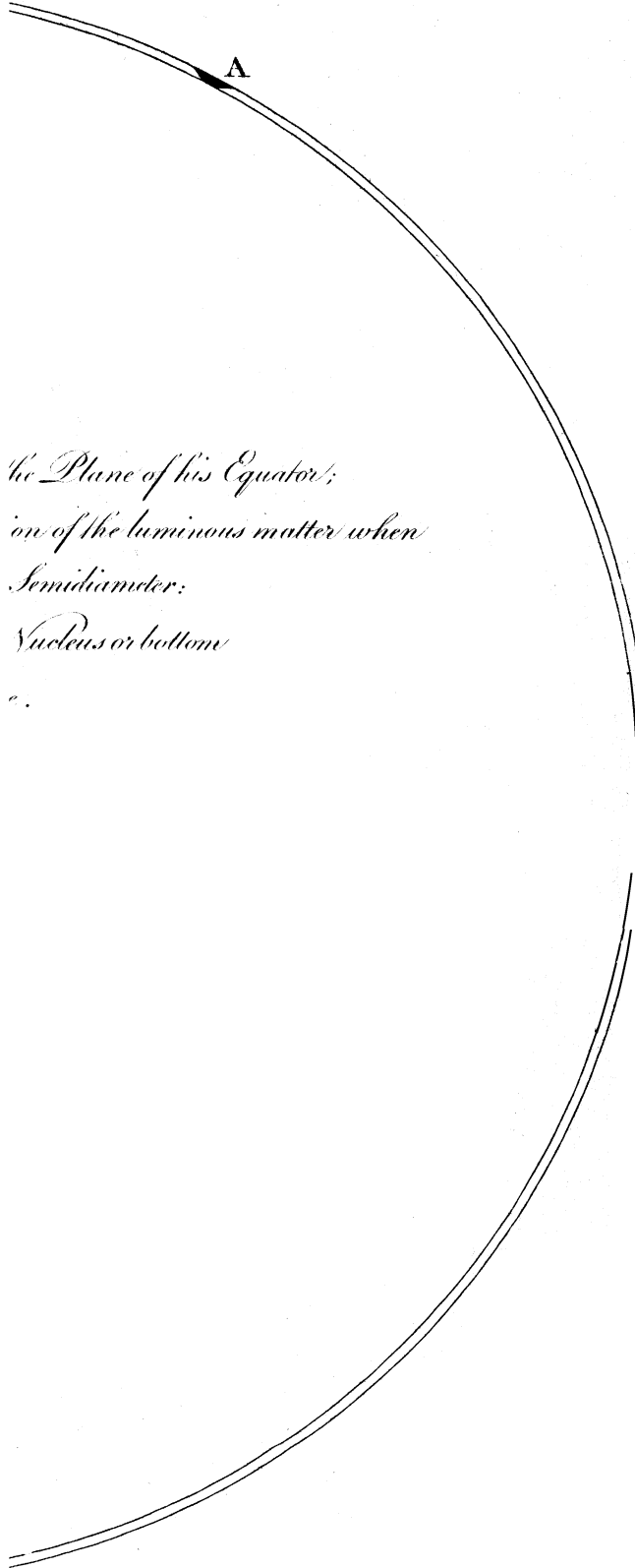


Fig. 3.

This Figure represents a Section of the Sun by the Plane  
the Space between the two concentric Circles being the Section of the  
its depth is supposed equal to the earth's Semidiameter  
Fig. A. Section of a Spot of about 50. whose Nucleus  
is so far below the Surface.

Fig. 4.





*the Plane of his Equator;*  
*ion of the luminous matter when*  
*Semidiameter:*  
*Nucleus or bottom*

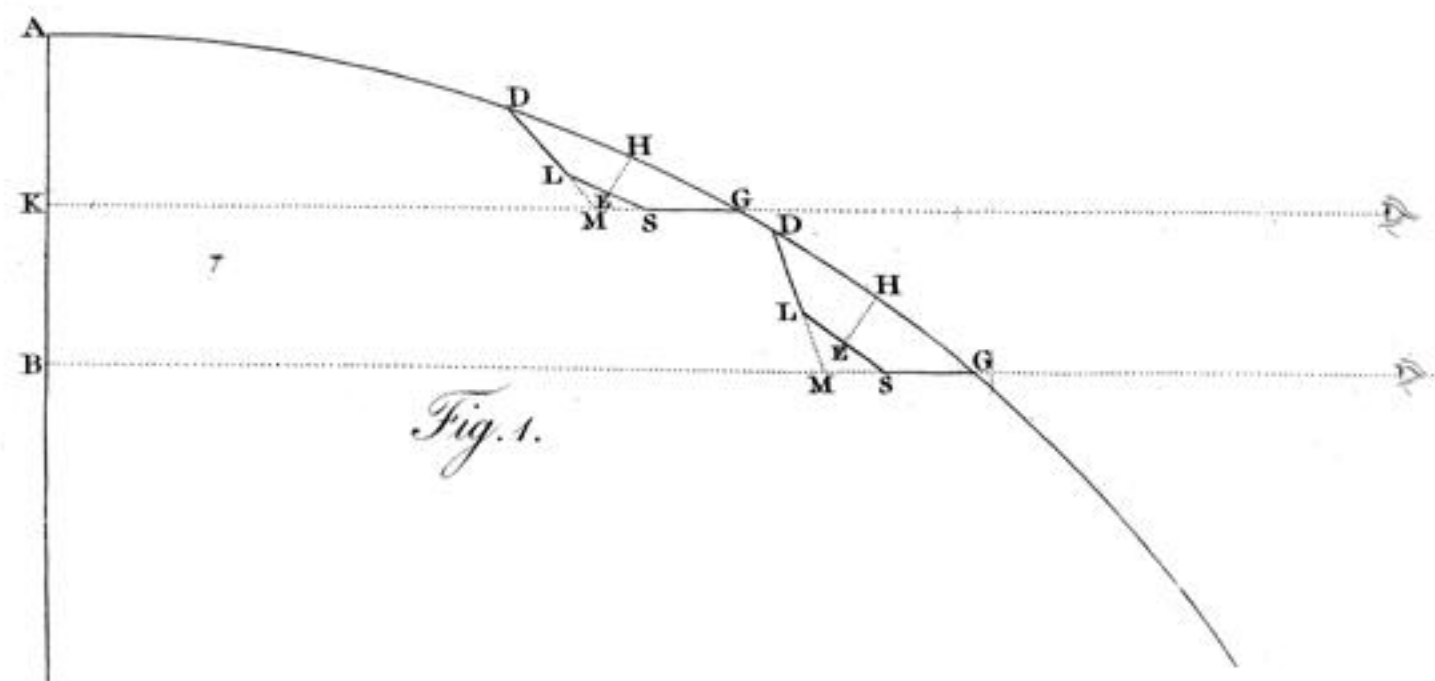


Fig. 1.

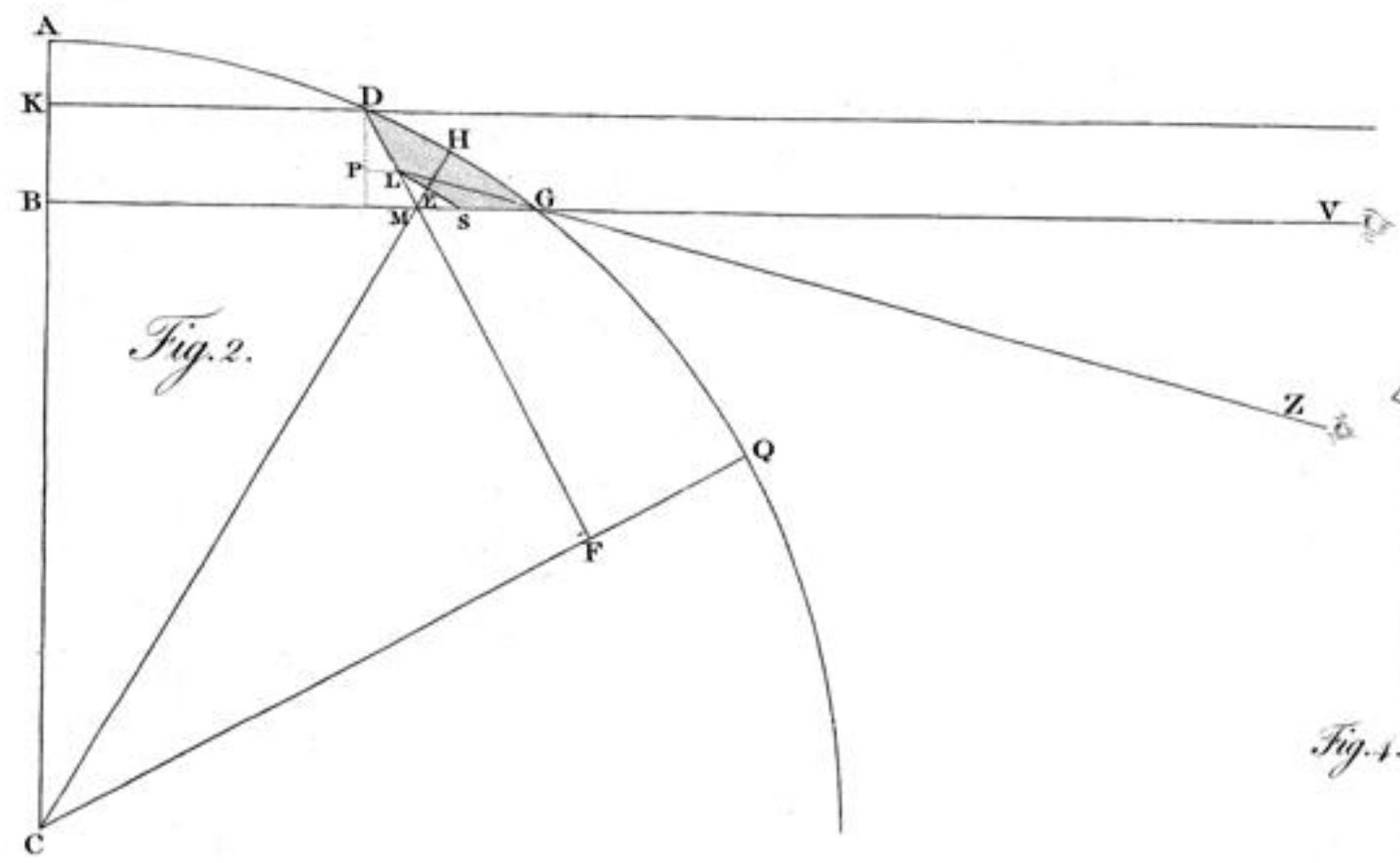


Fig. 2.

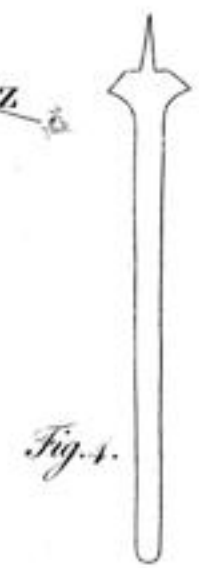
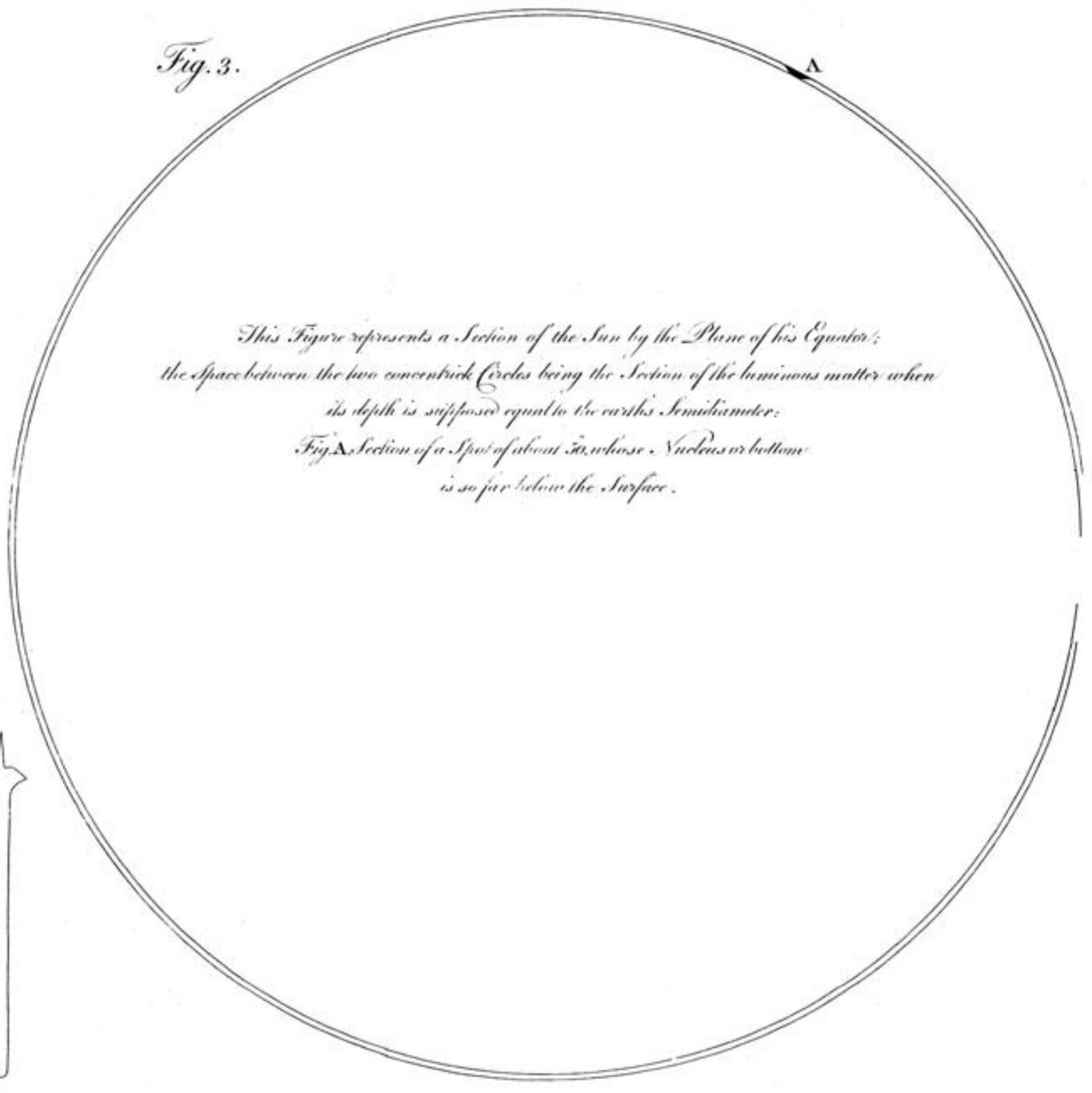


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