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# EXPERIMENTAL PROOFS

(WITH ILLUSTRATIVE ENGRAVINGS)

THAT THE

SURFACE OF STANDING WATER

IS

NOT CONVEX

BUT

# HORIZONTAL.

WITH A CRITICAL EXAMINATION OF THE RECENT ATTEMPT TO DECIDE THE QUESTION—"IS THE EARTH A GLOBE OR A PLANE?" FOR A WAGER OF ONE THOUSAND POUNDS, MADE BETWEEN JOHN HAMPDEN, ESQ., SWINDON, WILTS; AND ALFRED WALLACE, ESQ., F.R.G.S., REGENT'S PARK, LONDON; WITH MR. W. CARPENTER AND DR. COULCHER AS REFEREES, AND J. H. WALSH, ESQ., LONDON, EDITOR OF "THE FIELD," AS UMPIRE.

BY

**"Parallax,"**

(Author of "Zetetic Astronomy," "Patriarchal Longevity," & other works.)

LONDON:

WILLIAM MACKINTOSH, 24, PATERNOSTER ROW,  
AND ALL BOOKSELLERS.

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## EXPERIMENTAL PROOFS, ETC.

In the year 1865 I published a work called "Zetetic Astronomy,"\* the object of which was to prove that the Earth is not a revolving Globe, but an irregular Plane without orbital or axial motion, and the only known material world in the Universe. For many years previously I had delivered lectures upon the subject in most of the principal towns of Great Britain and Ireland.

In October of last year (1869), whilst lecturing in the Westbourne Hall, Bayswater, near London, I received a letter dated Swindon, and signed "John Hampden." In it the writer stated that he had then recently obtained a copy of the work above alluded to (Zetetic Astronomy); had been greatly interested in its perusal, and was perfectly satisfied from the evidence it contained that the doctrine of the Earth's rotundity and the Newtonian system of Astronomy altogether was fallacious. An almost daily correspondence seeking and giving information upon various points connected with the subject was afterwards maintained for several weeks. The questions were incidentally asked "how long had I held such convictions, and what steps had I taken to make them known to the world?" on replying that I had been labouring in various ways—lecturing, debating, writing, &c., &c., for upwards of thirty years, he expressed himself as greatly surprised that he had never heard of the matter until lately, and stating that he was so completely satisfied of the truth of what I had written and published, that he would at once begin to do his utmost for its diffusion and establishment.

Arrangements were made that he should make extracts from and reprint a given section of my work in the pamphlet form; and he thenceforth laboured in every way which lay in his power,

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\* Simpkin, Marshall & Co., London.

sparing neither expense nor effort. He wrote in local papers fiercely denouncing the Newtonian system and all who held it to be true. Epithets were used and charges made which no man has a right to employ or make against those who simply differ in opinion or conviction. I wrote complainingly of the style of advocacy he had adopted; and endeavoured to show him that as the world had been educated to believe in the Newtonian philosophy as true and satisfactory, it was our duty to treat such educated conditions with respect and consideration. That we must seek to uneducate or educate afresh and not to denounce and abuse. And I here take the opportunity of earnestly advising *all the converts to the Zetetic Philosophy to treat their opponents as at least equally sincere and honorable as themselves*. It is the first duty of an advocate to be respectful, patient, free from all special pleading and calmly reliant upon the force of truth plainly and solemnly presented. It should ever be borne in mind that all men wish to be right in their convictions. They do not wilfully cling to error. If they appear to do so it will be found that, however false their opinions may be in reality, they at least appear to be true, or they could not be conscientiously defended. Men are often stupid enough in refusing to listen to evidence for opinions contrary to their own, and many there are who are incapable of strict logical reasoning, who cannot trace effects to their legitimate causes, and who are unable to follow out the sequences of the evidence presented. Such people are often more troublesome and obstinate in discussion than those who are gifted with higher degrees of mental power. Evidence appears to have little weight with them, and any change of conviction seems to be the result of some accidental impression, rather than the direct effect of a reasoning process: but until that evidence has penetrated and changed their minds they are to be considered as equal to ourselves in every worthy characteristic. Any other course is persecutive, unjust, and injurious to the cause it is intended to serve.

I deeply regret that, as in many other instances, the advice I gave was not regarded; an unmistakeable and unfriendly defiance arose. For several weeks our correspondence was suspended, and I was entirely ignorant during that time of what was being done; but at



length I was startled by reading in the daily and weekly newspapers the following announcement :—

“500*l.* has been offered and accepted on the result of a scientific investigation as to whether the surface of the earth and water is level or convex. The challenge was made by Mr. Hampden, of Swindon, and has been accepted by a fellow of the Royal Geographical Society of London. The 1,000*l.* has been lodged at Coutts's, and the survey is to be made before the 15th of March, in the county of Cambridge. The editor of an old-established London paper has been chosen umpire; each party names a referee. Much interest in the decision is felt by the innumerable advocates of the Newtonian and Copernican theory of the rotundity and revolution of the earth, which Mr. Hampden affirms to be a downright fiction and a fraud, in the face of all the philosophy and science of the United Kingdom.” Shortly afterwards my attention was drawn to an article in “The Field” of March 5th, 1870, from which the following is an extract :—

**“EXPERIMENTAL PROOF OF THE ROUTUNDITY OF  
OUR EARTH.**

“For some years a correspondent of a provincial journal, signing himself “Parallax,” has attempted to revive the long-exploded theory that the earth on which we live is a plane, and that, while the North Pole is in the centre of this great flat, the South is not a point, but a margin of ice, which is the sole obstacle to an exploring party reaching the edge. The theory is so opposed to numberless facts well known to scientific men, that no member of the latter class has until now, as far as we know, thought it worth confuting; but—whether from this cause, or from its novelty, or from its inherent truth, it matters not—the fact remains that “Parallax” has obtained a numerous following, and among others a gentleman of the name of Hampden, residing at Swindon. So convinced is he of the existence of this plane, that he has for some time offered to test it experimentally, and to risk £500 on the result, on condition that a similar sum is also deposited by the opposite side. For a time no one thought of taking up the cudgels, but at length Mr. A. Wallace, a fellow of the Royal Geographical Society, thinking it desirable to disabuse the

minds of the disciples of "Parallax" of this fallacy, as he assumes it to be, offered to comply with Mr. Hampden's conditions, by proving the convexity of the surface of a length of water (six miles) by ocular demonstration; and for this purpose £500 aside have been deposited in our hands, the whole sum to be handed over either to Mr. Hampden or Mr. Wallace, according to the success or failure of the latter in proving the disputed convexity."

I wrote enquiring as to the nature of the experiments to be made and the place and time and persons concerned in the matter; but could get no information. I was kept in entire ignorance of the whole affair until it was over. I could not but feel that this was altogether injudicious on the part of Mr. Hampden and his referee, Mr. Carpenter; and very unfair both to myself and to the public. Common justice ought to have suggested to them that no such attempt to settle so important a matter should have been made without an invitation to the author to be present. More especially should this have been done when it is known that both Mr. Hampden and Mr. Carpenter were literary, and not scientific gentlemen. They knew little or nothing of the nature of the instruments employed in the experiments, and became literally the helpless victims of their more philosophical and practical opponents. What could be more unwise than for Mr. Hampden to deposit the sum of £500 against the same amount from Mr. Wallace, and then to allow Mr. Wallace to dictate his own experiment and to use and manipulate his own instruments? In such a procedure common sense and practical justice were ignored. The only proper plan would have been for both gentlemen to stand aside, and allow two distinctly and separately engaged Surveyors to take the level of the water; the referees noting the result, and the particulars afterwards given to the umpire. But even then it was the duty of these gentlemen to first repeat the experiments which I had made and published in my work, in which at pages 10 to 13 the following account occurs:—

"If the earth is a globe there cannot be a question that, however irregular in form the *land* may be, the *water* must have a *convex surface*; and as the difference between the true and apparent level, or the degree of curvature would be 8 inches in one mile (statute measure),



and in every succeeding mile 8 inches multiplied by the square of the distance, there can be no difficulty in detecting either its actual existence or its proportion. Experiments made upon the sea have been objected to on account of its constantly changing tidal altitude, and the existence of banks and channels which produce currents, 'crowding' of waters, and other irregularities. Standing water has therefore been selected, and many important experiments have been made, the most simple of which is the following:—In the County of Cambridge there is an artificial river or canal, called the 'Old Bedford.' It is upwards of twenty miles in length, and passes in a straight line through that part of the fens called the 'Bedford Level.' The water is nearly stationary, often entirely so, and throughout its entire length has no interruption from locks or water gates; so that it is in every respect well adapted for ascertaining whether any and what amount of convexity really exists. A boat with a flag standing five feet above the water was directed to sail from a place called 'Welche's Dam' (a well known ferry passage), to another place called 'Welney Bridge.' These two points are six statute miles apart. The observer, with a good telescope, was seated in the water, with the eye not exceeding eight inches above the surface. The flag and the boat were *clearly visible throughout the whole distance!* as shown in the following diagram.

Fig. 1.

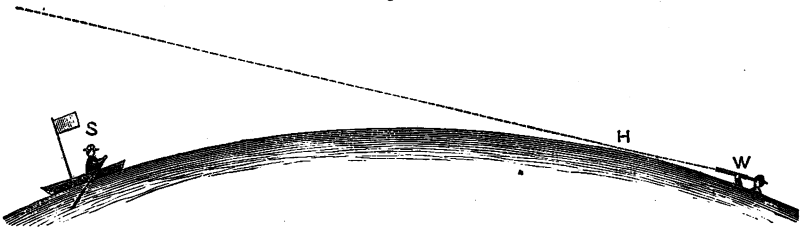


“ From this experiment it was concluded that the water *did not decline from the line of sight!* As the altitude of the eye of the observer was only eight inches, the highest point, or the horizon, or summit of the arc, would be at one mile from the place of observation; from which point the surface of the water would curve downwards, and at the end of the remaining five miles would be 16 feet 8 inches *below the horizon!* The top of the flag, being 5 feet high, would

have sunk gradually out of sight, and at the end of the six miles would have been 11 feet 8 inches *below the eye line!*"

This will be rendered clear by the diagram.

Fig. 2.



W the position of the observer, S the flag-staff six miles away, and H the intervening horizon.

"From this observation it follows that the sur'ace of standing water is not convex, and therefore that the *Earth is not a Globe!* On the contrary this simple experiment is all sufficient to demonstrate that the surface of the water is parallel to the line of sight and is therefore *Horizontal*; and that the earth cannot possibly be other than a **PLANE!**"

After such an account as the above had been published, and as both Mr. Hampden and his referee Mr. Carpenter were, in a practical sense, perfectly ignorant of the whole matter, having never tried such experiments but relied entirely on the statements made in my work\* to agree to make any other kind of experiment without having first tested the truth of my statement as above given, was, to say the least, unfriendly, very foolish, unjust, and logically irregular. The most simple and decisive should always be first employed, and then made use of to test and rectify the more complicated. My own long experience as an experimental investigator has proved to me that however complicated and conflicting may be the mere systems and opinions of men, the great principles and truths of nature are always simple and consistent. It is therefore imperative that in every enquiry after

\* "Mr. Carpenter was engaged to decide a disputed question, of which he and his principal (Mr. Hampden) professed to be practically ignorant."—*Field*, March 26, 1870.

truth, the simplest possible means should first be adopted. Had Messrs. Hampden and Wallace conferred with, or invited me to take part in their operations, I could have shown and satisfied them that what they proposed to do and the instruments they were about to employ as well as their mode of application, were in every sense unsuitable for the object they had in view, and could not lead to definite and satisfactory results. Was there any justice in pretending to test the truth of my teachings by any other method than that of repeating the experiments which I had made, and the particulars of which I had published to the world? Were Mr. Wallace and his referee Dr. Coulcher in preparing and carrying out their peculiar and special operations, and neglecting to test my statements, doing that which they could approve if done by others towards themselves? Was it not the duty of Mr. Hampden and his referee Mr. Carpenter to insist upon the experiments described in my work being repeated? Before agreeing to any other course were they not bound in honor, as gentlemen, saying nothing of the friendly feeling which might have been expected from their recent conversion to the "Zetetic Astronomy," to have informed me of their intentions and to have invited me to take part in their proceedings? Their not having done so was to myself individually a needless insult; towards the public an unwarrantable deception—a mockery—a make-believe of a sincere desire to settle an important question; and to the cause of truth and progress an ill-conceived, injurious retardation. There never was an instance where in deed and in truth it could have been more justly said "save me from my friends." For their folly and injustice they have had to forfeit five hundred pounds, but the opposite party have not fairly received it. All concerned were the victims of self-imposed instrumental deception. All were evidently ignorant of the possible behaviour or reading of the telescope and the spirit level when applied in the way they had mutually agreed upon. This will be seen by a very brief examination of the report of their proceedings which appeared in the "*Field*" of March 26, 1870, and referred to in a leading article in the same number by the Editor who was also umpire in the case. The Editor says:—

"In the remarks which we ventured to make on the 5th inst., we

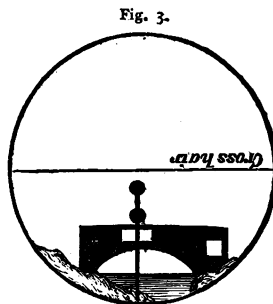
endeavoured as far as possible to state exactly what Mr. Wallace engaged to prove, namely, that by fixing three discs at equal distances, 12 feet from the surface of this level, one being at *each end* and the third in the middle; according to the received theory, the middle disc ought to range 5 feet (in round numbers) above the level of the terminal discs; while Mr. Hampden risks his £500 on the assumption that the three will range in a straight line."

From the above it will be seen that *three discs* were to have been used, one *immediately near* to the telescope, one in the middle, or three miles from the telescope, and one at the end of six miles. Now clearly the conditions of the experiment were *not carried out!* At the end of six miles instead of a *disc* an oblong flag was placed; a disc was erected in the middle position only! and *nothing whatever* was fixed *close to the telescope!* Thus the agreement was completely violated. Those who suggested and those who were idly and carelessly present, and agreed to such a procedure, were alike greatly to blame. The very life of a great cause was at stake. The wager of twice five hundred pounds, even if the puzzled heads of the wagers had been thrown in, were as nothing in value compared with the importance of the question to be decided: and the least that ought to be said is, that every man connected with the operations was entirely unfit for the duty. Had a disc 12 feet, or any other altitude, been placed *close to the telescope*; another of exactly the same make at the distance of three miles, and a third of the same character and altitude at the end of the six miles, and the telescope placed *immediately close to* and the line-of-sight directed *over* the first disc, the conditions of the agreement would have been properly fulfilled, and the result would have proved the water surface to have been equidistant from the line-of-sight throughout the whole distance of six miles. Let any one select a long row of lamps of equal altitude, and on truly horizontal ground. Let him elevate himself until he is able to place a telescope just *above* the first lamp; on directing it along the whole row he will find that the line-of-sight will pass at the same distance just over each lamp to the end of the series; but let him ignore one half the lamps immediately nearest to him, and so place the telescope that he *must* observe the *last* or farthest

lamp, and he will see all the intervening lamps *standing above* the line-of-sight—the *nearest* to him apparently the highest, and the degree of elevation more or less according to the power of the telescope. This is precisely the case of the first observation made by the gentlemen who so strangely risked their money and their scientific reputation on the result! From the careless and logically dishonest manner in which this experiment was conducted, the agreement broken, the most essential condition neglected, the whole matter falsified as compared with their published programme, it is clear that the recipient of the prize of £500 is not honestly in its possession. The money was neither won nor lost. The race was not run; the experiment agreed upon was not tried, and therefore the stakes should be returned. So far nothing was proved except the childish carelessness of the operators.

The second experiment, that with the spirit-level, was equally valueless in determining the form of the surface of the water: as will be seen from the following representation and report taken from "The Field," of March 26th, 1870.

"Fig. 3. Diagram showing what was seen in the telescope of a sixteen-inch Troughton level, accurately adjusted and placed in the same position and height above the water as the large achromatic."—



In this experiment there was the omission of an important element, as in the observation made with the large telescope, viz. a disc close to the spirit level. It had been agreed that there should be three signals, one at each end of the six miles of canal, and one in the

middle. But *only two* were employed ! What could be the motive of Mr. Wallace in thus leaving out one of the three signals and that one the most important ? It is useless to say that the third signal was the spirit-level or the telescope, because it had been distinctly decided that *three objects* should be fixed at the same altitude above the water, and three miles apart ; and that they were to be observed through the telescope of the spirit-level, whether they ranged in a straight line or gradually declined from the cross-hair of the instrument ; therefore the instrument (the spirit-level) was intended to have been used in *addition* to the *three signals*. It was absolutely necessary that three signals should be fixed. It had been seen and admitted to be necessary. It was agreed that it should be done ; and yet it was not done ! Only two signals were fixed ; and it is right that those who suggested the omission, or who neglected to carry out the conditions previously agreed upon, should be made to see that by so doing they caused the whole of their proceedings to be utterly worthless ; besides laying themselves open to disagreeable charges of improbity. The third but omitted signal properly placed would have given a certain determination to the line-of-sight which could have been used as a test of declination or curvature both in fact and in amount, from the point of observation.

But let us examine the case as it stands, and as it is represented in fig. 3. First, the spirit-level was accurately adjusted, that is, it was "levelled : " which means that, if the earth is a globe, the axis of the telescope was at right angles to the direction of gravity or the immediate radius of the earth. The line-of-sight indicated by the cross-hair (seen in the diagram) was therefore a tangent. From this tangent, or from the cross-hair, the top of the upper disc should have sunk six feet. It is known (by previous measurement) that the two discs on the first signal pole, three miles away, were *four feet apart*. Now if we take a pair of compasses, or a scale of equal parts, and measure the apparent space between the centres of the two discs as they appear in the diagram, we shall find them to be two-sixteenths of an inch apart. Therefore two-sixteenths represent the previously known space of *four feet*. Now measure from the top of the upper disc to the cross-hair and it will be found to be one-sixteenth of

an inch, which only represents a fall of *two feet*; but if the earth is a sphere of 25,000 statute miles in circumference, the fall in three such miles would be *six feet*. Here then, if the top of the upper disc appearing to be below the cross-hair is taken to represent the downward curvature of the water there is an error in the reading and appearance of *four feet*.

Again, if we measure the distance between the centre of the lower disc and the centre of the white flag or signal on the bridge, we find it to be one-and-a-half-sixteenth of an inch, representing *three feet*. Now, as previously demonstrated, from the known distance apart of the two discs, one-sixteenth of an inch represents the actual space of two feet, and as the centre of the white flag, or farthest signal, on the bridge, is not more than five-sixteenths of an inch below the cross-hair, it is thus represented as being only ten feet below the tangent, or line-of-sight; but the curvature in six statute miles would be *twenty-four feet*. So that if the appearance in the field of view, as given in diagram fig. 3, is taken to represent the downward curvature of the water in the canal, there is a demonstrable error or deficiency of seven-sixteenths of an inch, representing in practice *fourteen feet*! The only alternative is that the earth, if a globe at all, is very much larger than has hitherto been affirmed! Demonstrably then the appearances in the telescope of the spirit-level cannot be taken to represent declination of the surface of the water; and if the observers will only be wise enough to gather experience from failure, and wisdom from experience, they will never again attempt to decide so important a question as that of the earth's convexity or non-convexity by the use of such an instrument as a "Troughton's spirit-level." If they had employed a good Theodolite it would have been better, because the appearances could have been somewhat tested and rectified by taking the "dip" or angle subtended by the apparent depression to the several signals. As it is we must seek some other explanation of the appearances given in the diagram. Every scientific surveyor of large experience knows that the very best theodolites and spirit-levels require very careful adjustment, that they are all liable to error from various causes, viz. *collimation, parallax, refraction, aberration, spherical confusion, and*



*chromatic dispersion* ; and that when adjusted in the most perfect manner possible, there will still be minute errors in distances of a few hundred yards. In a work entitled "A Treatise on Mathematical Instruments," by J. F. Heather, M.A., of the Royal Military College, Woolwich, published by Weale, High Holborn, elaborate directions are given for examining, correcting and adjusting the collimation &c., and at page 103 these directions are concluded by the following words, "the instrument will now be in complete practical adjustment, for any distance not exceeding ten chains, the maximum error being only  $\frac{1}{1000}$ th of a foot."

The principal instrumental error is that of collimation, or slight divergence from the true axis of the eye : and as this might easily amount to  $\frac{1}{1000}$ th of a foot in ten chains (220 yards,) in the most perfect instrument when manipulated by the most experienced surveyors, we see at once the cause of the appearance in the diagram fig. 3. The top of the disc on the signal pole, three miles away, appears to be one-sixteenth of an inch below the cross-hair. The centre of the signal flag placed against the bridge, six miles away, is five-sixteenths of an inch below the cross-hair. It has already been shown that these distances below the cross-hair are not such as could have appeared from downward curvature in the water. It is now demonstrable, that they were the result of inevitable collimation or unavoidable divergence of the pencils of light passing through the glasses of the instrument : and as we have seen by the quotation from Mr. Heather's work, this divergence cannot be prevented, and might amount in the most perfect instruments to  $\frac{1}{1000}$ th of a foot in 220 yards, how much greater would it be in distances of three and six miles ? Now as the collimation or optical divergence was only  $\frac{1}{16}$ th of an inch in three miles, and  $\frac{5}{16}$ ths in six miles, it necessarily follows that the instrument used was a very good one, and that the *utmost care had been exercised in its adjustment*. The unavoidable instrumental errors were indeed so minute that if a hundred additional observations had been made, so far as appearances were concerned, they might never again have given such perfect results.

The folly and injustice with which all the parties connected with these observations have here been charged, consisted in their having

agreed to rely upon such appearances, without knowing their actual cause, or not being able to give them their proper interpretation; and in so persistently and unaccountably leaving out one of the three signal poles in both experiments. It is painful to hear the remarks which are made, on every hand, respecting the conduct of Mr. Wallace and Dr. Coulcher in acting so very suspiciously. Again and again have the expressions been heard, "they knew their game," "they determined to win the money, and 'cooked their case' accordingly." Their having thus acted has certainly rendered it difficult to defend them. The thought of such scientific gentlemen having been actuated by any other than the most honorable motives ought not to be tolerated: yet from their strangely unscientific procedure the suspicion is not un-natural; and there is *only one way* of successfully destroying it: viz. their acceptance of the invitation given at page 19, the use of their large Achromatic Telescope in the manner there indicated and their agreeing to stand by the consequences. The charge rests upon Mr. Hampden and Mr. Carpenter for not insisting upon a repetition of the telescope-and-boat experiment described at page 5, fig. 1, and neglecting to invite or confer with the author as the originator of all such observations, and the Founder of the "Zetetic Astronomy" erected upon the results. Had this been done all the confusion and ill feeling which have arisen, as well as the misappropriation of the wager, and the very awkward position in which the Editor of the *Field* must have found himself when called upon to act as umpire in the case might have been prevented. He was required to decide an important question, not from unquestionable results, but from optical appearances only; and as these were not understood, and therefore neither challenged nor allowed for, he could not have done otherwise than hand over the one thousand pounds to Mr. Wallace. But as the principal points in the agreement were not carried out—both parties being at fault; and as the positions of the signals were only apparent and had really no connection with the question at issue, the five hundred pounds ought to be returned to Mr. Hampden, and the whole affair looked upon as a "drawn battle"—as a contest, the conditions of which were not fulfilled.

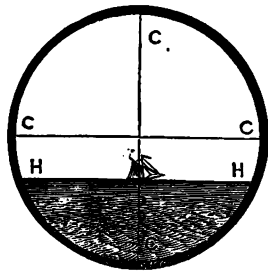
As long ago as 1838 I made a number of observations on the old "Bedford Canal," and soon found the necessity of specially studying the structure and peculiarities of all the different kinds of levelling instruments; and many times since that period have taken part in levelling experiments with some of the first surveyors and engineers of the day. In all the experiments in which I have been thus engaged I have been able to state before-hand—to predicate, what appearances would be observed in the field of view, and in almost every instance have satisfied the operators that what they saw was simply the admitted unavoidable peculiarities of the instruments, and not indications of the earth's rotundity. So important is this explanation that I deem it right to offer to the reader a simple demonstration. Let him find a piece of ground—a terrace, promenade, line of railway, or embankment, which shall be perfectly horizontal for, say, five hundred yards. Let a signal staff five feet high be erected at one end, and a theodolite or spirit-level fixed and carefully adjusted to exactly the same altitude at the other end. The top of the signal will then be seen a little *below* the cross-hair, although it has the *same actual altitude* and stands upon the same horizontal foundation. If the position of the signal staff and the spirit-level be then reversed the same result will follow. Another proof will be found in the following experiment: Select any promontory, pier, lighthouse-gallery, or small island; and, at a considerable altitude, place a smooth block of wood or stone of any magnitude. Let this be "levelled." If then the observer will place his eye close to the block and look along its surface towards the sea he will find that the line-of-sight will touch the distant horizon. Now let any number of spirit-levels, or theodolites be properly placed and accurately adjusted; and it will be found that in every one of them the same sea-horizon will appear in the field of view considerably *below* the cross-hair. Thus proving that the telescopic or instrumental readings are not the same as those of the naked eye.

The above illustration will be still more striking if a strong tube, without lenses or glasses of any kind, be "levelled" and directed towards the sea-horizon. On looking through it the surface of the water will appear to ascend to nearly the centre of the open end or "field of view," as shown in fig. 4; H H the sea-horizon.

Fig. 4.



Fig. 5.



On looking in the same direction through the telescope of the spirit-level, when "levelled" the sea-horizon will appear considerably *below* the cross-hair or centre of the tube, as represented in fig. 5:—

C C C C the horizontal and vertical cross-hairs, H H the sea-horizon, some distance below C C. This depression below the cross-hair is found to be greater as the altitude of the observer and therefore the distance of the horizon increases: which is precisely the phenomenon observed in the field-of-view of the spirit-level represented in fig. 3, page 9, where the flag signal on the bridge, being the farthest away, appears lowest, and lower than the disc on the signal pole—which is only half the distance. The top of the bridge in fig. 3, may be compared to the horizon H H in fig. 5.

Thus the apparent depression of the horizon through the influence of the lenses which constitute a telescopic levelling instrument is visibly the same, and arises from the same cause as the apparent depression of the signals observed in the experiment on the Bedford canal.

On repeating the above-named experiments and seeing with his own eyes the actual results, the practical observer cannot fail to be satisfied that when distant objects are seen below the cross-hair of an optical instrument like the spirit-level, the cause is simply *aberrance of light* or "wandering" of the eye-line from the true line, or axis of vision, in passing through the glasses or lenses of the telescope: and not because really depressed in consequence of declination or curvature from the line-of-sight. Hence all such appearances are entirely

out of place and valueless in connection with the subject of the earth's true form and magnitude.

These very simple experiments will satisfy the observer that there is in every such instrument more or less divergence of the line-of-sight; and that however small the amount—perhaps inappreciable in short distances—it is necessarily considerable in several miles. He will then be fully satisfied that what was seen on the Bedford Canal by Messrs. Hampden, Wallace, Carpenter and Coulcher, was not the convexity of standing water, but telescopic aberration, and instrumental “error of collimation.” This will again be rendered clear and certain by such an experiment as going into the water of the canal with a telescope and observing a receding boat with a flag affixed, for the distance of six miles, as represented in fig. 1, page 5. In such an observation instrumental error will be neutralized and the surface of the water proved beyond all doubt or cavil to be horizontal.

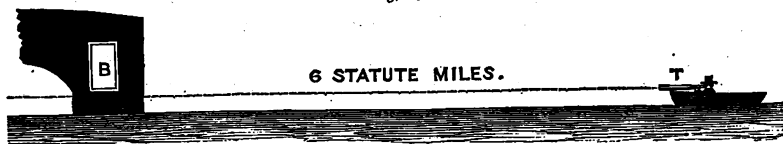
On seeing the reports of the referees in the “Field” of March 26th, and the editorial article in the same number, I determined to visit the scene of their operations and to make some experiment or observation so simple in character that no possible doubt as to its value in deciding the question at issue could be raised. I left London on Tuesday Morning last, April 5th, 1870, and arrived at the old Bedford Sluice Bridge at twelve o'clock. The atmosphere was remarkably clear and the sun was shining brightly upon the bridge and the various objects around it. I immediately made the following measurements:—

	ft.	in.
Height of Arch .. .. .	12	8
„ „ Sluice Gate .. .. .	5	8
„ „ Abutments .. .. .	3	8
„ „ Bottom of notice board (or table } of rules for navigating the river) }	6	6
Length of ditto .. .. .	7	2
Width of ditto .. .. .	5	2
Height of a turf boat moored close to } Abutment.. .. . }	2	6

I then obtained assistance and had the turf boat worked into the middle of the canal. A good telescope was then directed along the water and immediately the archway of Welney Bridge came distinctly into view. I saw through the arch, and for a considerable distance beyond it. If the earth is a Globe of 25,000 miles circumference, the convexity of the water between Bedford and Welney Bridges, the distance being 6 statute miles, would be such, that allowing 2 miles for the altitude of the observer's eye (30 inches), the remaining 4 miles would curve from the summit of the arc of water 10 feet 8 inches. The highest part of the arch of Welney Bridge is 7 feet, so that the top of the arch should have been 3 feet 10 inches *below* the line-of-sight. Whereas not only the top of the arch, but the springs and abutments were distinctly visible. Therefore the surface of the water was *not convex*, but *perfectly horizontal*.

A train of several empty turf boats had just previously entered the canal from the river Ouse; and was about proceeding to Ramsey in Huntingdonshire. I arranged with the captain to place the lowest or shallowest boat the last in the train, and to take me on to Welney Bridge. The telescope was placed on the lowest part of the stern; and was exactly 18 inches above the water. The Sluice gate 5 feet 8 inches high; the turf boat from which I had made the observation to Welney Bridge, 2 feet 6 inches high, and the white notice board, 6 feet 6 inches high, were all before me. The sun was shining strongly upon them; the air was exceedingly still and clear; and the surface of the water "smooth as a molten mirror;" so that everything was extremely favourable for observation. At 1.15 p.m. the train of boats started for Welney, the objects above named were plainly visible as the boats receded, and were kept in view during the whole distance; as represented in fig 6, T the telescope and B the notice board.

Fig. 6.



d

On reaching Welney Bridge I made very careful and repeated observations and finding several men upon the banks of the canal I called them to look through the telescope. They all saw distinctly the white board and the sluice gate, and the black turf boat moored near them. Now the telescope being 18 inches above the water the line-of-sight would touch the horizon at 1 mile and a half away—if the water is convex; the curvature of the remaining 4 miles and a half would be 13 feet 6 inches: hence the turf boat would have been 11 feet; the top of the sluice gate 7 feet 10 inches, and the bottom of the white notice board 7 feet *below the horizon*, as shewn in fig. 7, T the telescope, H the horizon, and B the notice board.

Fig. 7.



It was *not* so; and the unavoidable conclusion is that *the surface of standing water is not convex, but horizontal.*

Previous to leaving London it had been suggested to me that the names and addresses of several persons should be obtained certifying to the observations which had been made. This I did; but recollecting that many years previously I had obtained formal certificates from respectable persons in the locality; and that when these documents were referred to in proof of my statements, it was said that no evidence existed that they were genuine—"they might be forgeries" &c., &c. I resolved not to publish them. I considered it would give more dignity and certainty to the cause of truth, to make and describe the simplest possible experiments: to refer to fixed well-known objects, which were selected for observation, and to formally challenge the scientific world to visit the locality and to make observations for themselves. I most earnestly and solemnly invite all those who take a serious interest and who feel the great importance of the subject to make a special journey to the Bedford Canal; and, above all things, to repeat the simple experiments which



I have here described. The whole matter ought to be taken out of the pale of controversy. Believing and disbelieving should have no place in connection with it. It is capable of demonstration to the eyes and judgment of every one who may desire to be satisfied, and who will take the trouble to visit the place and observe for himself. Why not establish a scientific pilgrimage to the Fens of Cambridgeshire? Every man who sees the magnitude and the logical consequences of the question "is the surface of standing water horizontal?" should make it an important part of his education to visit at least once in his life the old Bedford Sluice or Canal, and make such experiments as will for ever satisfy him that the surface of the water is not convex, and that therefore, and of mathematical necessity the EARTH IS A PLANE.\*

As many have expressed a degree of doubt that the curvature upon the surface of a Globe 25,000 statute miles in circumference amounts to 8 inches in the first mile, and increases as the square of the number of miles multiplied by 8 inches, the following quotation will be useful:—

"If a line which crosses the plumb-line at right angles be continued for any considerable length it will rise above the earth's surface (the earth being globular), and this rising will be as the square of the distance to which the said right line is produced; that is to say, it is raised 8 inches, very nearly, above the earth's surface at 1 mile's distance; four times as much, or 32 inches, at the distance of 2 miles; nine times as much, or 72 inches, at the distance of 3 miles, &c. &c."

"The preceding remarks suppose the visual ray to be a straight line, whereas on account of the unequal densities of the air at different distances from the earth, the rays of light are incurvated by refraction. The effect of this is to lessen the difference between the true and apparent levels, but in such an extremely variable and uncertain manner that if any constant or fixed allowance is made for it in formula or tables, it will often lead to a greater error than

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\* The Author is willing to accompany any specially arranged party, on receiving due notice—addressed "Parallax," care of the Publisher, (Wm. Mackintosh), 24, Paternoster Row, London.

what it was intended to obviate. For though the refraction may at a mean compensate for about a seventh of the curvature of the earth, it sometimes exceeds a fifth, and at other times does not amount to a fifteenth. We have therefore made no allowance for refraction in the foregoing formulæ.—*Encyclopædia Britannica*, article "*Levelling*."

It will be seen from the above that, in practice, refraction need not be allowed for. Indeed it can only exist when the line-of-sight passes from one medium into another of different density; or where the same medium differs at the point of observation and the point observed. The Ordnance surveyors of England have found that 1-12th of the altitude of an object may be allowed for refraction. Taking this amount from the different altitudes referred to in the several experiments made upon the old Bedford Canal, it will make very little difference in the actual results. For instance, in the experiment represented by fig. 2, page 6, the top of the flag would be reduced 1-12th, leaving it 10 feet 8 inches instead of 11 feet 8 inches as there given.

Others not being able to deny the fact that the surface of the water in the old Bedford and other canals is horizontal, have thought that a solution of the difficulty was to be found in supposing the canal to be a kind of "trough" cut into the surface of the earth: and have considered that although the earth altogether is a globe, yet a canal or "trough" might exist as a chord of the arc terminating at each end. This however could only be possible if the earth were motionless. But the theory which demands rotundity in the earth also requires rotatory motion: and this produces centrifugal force. Therefore the centrifugal action of the revolving earth would of necessity tend to throw the waters of the surface away from the centre. This action being equal at equal distances; and being retarded by the attraction of gravitation, which is also equal at equal distances; the surface of every distinct and entire portion of water must stand equidistant from the earth's centre; and therefore must be convex, or an arc of a circle. Equidistance from a centre means, in a scientific sense, "level" or convex. Hence the necessity for using the term horizontal to distinguish between "level" and "straight."

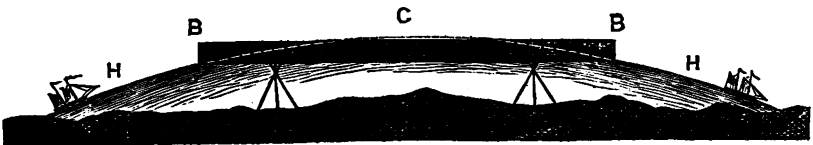
In addition to the evidence already advanced, that, as the surface of standing water is not convex but horizontal, and that therefore it is impossible that the earth can be a globe, the appearance of the horizon at sea may be referred to. The sea horizon, to whatever distance it may extend to the right and left of an observer on land, always appears as a perfectly straight line, as represented by H H in fig. 8.

Fig. 8.



Not only does the sea horizon *appear* to be straight as far as it extends, but it may be *proved* to be so by the following simple experiment:—At any altitude above the sea level and opposite to the sea horizon fix a long board (say from six to twelve or more feet in length) edgways upon tripods or any other kind of stand. Let the upper edge of the board be perfectly smooth and truly “levelled.” On placing the eye behind this upper edge and looking over it towards the sea, the distant horizon will be observed to run perfectly parallel with it throughout its whole length! If the eye be now taken backwards to some distance, so that in looking to the right and to the left at considerable angles over the ends of the board, there will be no difficulty in observing a length of from ten to twenty miles, according to the altitude of the position; and this whole distance of twenty miles of sea horizon will be seen as a perfectly straight line! This would be impossible if the earth were a globe, and the water of the sea “level” or convex. In twenty miles there would be a curvature on each side from the centre of the distance of 66 feet; and instead of the horizon touching the board along its whole length it would be seen considerably below the two extremities, as shown in the following diagram, fig. 9.

Fig. 9.



B B the upper edge of the board; and H H the horizon depressed below the centre C 66 feet in 10 statute miles ( $10^2 \times 8$  inches = 66 feet 8 inches.)

If H were really 66 feet below B, what influence could possibly operate to make it appear at B?

A very striking illustration of the horizontal character of the sea horizon may be observed from the high land at the head of Portsmouth Harbour. Looking along the Harbour across Spithead to the Isle-of-Wight, the base or margin of the land from the extreme east to the "Needles" in the west appears and may be proved by the means just described to be a perfectly straight line: as shown in fig. 10.

Fig. 10.



As the Island is 22 statute miles from east to west it is evident that the two extremities E and W would be the square of half that distance (or 11 miles) times 8 inches or 80 feet below the centre! As 80 feet 8 inches is the amount of declination, which would exist if the earth were a globe such as the Newtonian philosophy affirms it to be, and as no such declination, but the very reverse can be proved to exist, it follows necessarily that in this important particular that philosophy is fallacious—false absolutely! If it be said that upon a globe of such magnitude as the earth a declination of 80 feet could not be recognised or detected, in the distance between the Isle-of-Wight and the head of Portsmouth Harbour, the following experiment will give the answer. Let a long rod or string, S S, in fig. 10, be tightly stretched and "levelled" before the observer, and raised a little above the horizon, so that it cuts the tops of the high lands, or hills, of the Island. It will then be seen that the slightest alterations of altitude—the ascents and descents of the various mountain peaks, and the gradual declinations of the land at its extremities can all be followed and read with the utmost precision. If such gradual and trifling alterations or differences of altitude can thus be read in relation to the line

S S, what can prevent a declination of 80 feet in the horizon E W being detected by the same means? But as such declination cannot be detected, the natural conclusion is that it does not exist! As it cannot be found or proved to exist, then the doctrine of rotundity is a fallacy in idea, and an impossibility in fact. That the earth is a PLANE is thus experimentally, logically, and formally demonstrated.

A few words may here be useful respecting the suggested pilgrimage to the scene of the previously described experiments. For ages past our philosophers, with very few if any exceptions, have indulged in and been quite content with the practice of theorizing or forming hypotheses for the purpose of explaining phenomena. This has necessitated special experimentation. They could not, so long as they desired to maintain their theories, be fearless and impartial in their search for evidence. Experiments and observations specially suggested were all that could be tolerated. To the very last degree is this the case, in our own day, with too many of our otherwise extraordinarily gifted scientific men. What praiseworthy efforts have been made by our Royal and other learned Societies, as well as by individual philosophers, to arrange and amply provide for the most difficult expeditions to various parts of the world; and what immeasurable daring and noble sacrifice have been shewn by those who have been commissioned to carry them out! After such immense sums have been expended, such risks incurred, such great and fearful sacrifices, and the most prolonged physical and mental sufferings and deplorable deaths—often cheerfully borne for the sake of science and philosophy, but, unfortunately, too often, for the purpose of giving additional importance, or, if possible, evidence, in support of some prevailing and favourite hypothesis, will they join in a well-concerted expedition to that comparatively unknown region, called the "Bedford level?" Will they see that their acceptance of such an invitation is only what society has a solemn right to expect? They are looked up to as the very *foci* of human learning: and they surely would not feel themselves undignified in becoming the Judges and Umpires in so momentous a problem as that of whether the Earth is a Globe or a Plane—involved as it is in the previous question, is the surface of standing water convex or the horizontal? Let them take up the subject with a full determination to do it justice, and to settle the question once and for ever. Their means are abundant; their time is at their own command; the proper locality is within a few hours from London; the problem to be solved is deeply important, and the Author (who may at any time be found through his publisher), is ready and anxious to join them and to stand or fall by the results.

For the long period of thirty-one years he has laboured single-

handed to bring this important subject before the world: not simply by recording and publishing his convictions, but by constant efforts in lectures, discussions—both on the platform and in local journals, and travelling from place to place—never resting longer than a few months in one locality, but like, as it may be said, a scientific or philosophic gypsy breaking up his tent and pitching it “here, there and everywhere” in order to debate this great question, and draw to it the attention of all classes and degrees of intelligence (and as a matter of course has had to bear every possible form of opposition, the bitterest denunciations—often amounting to threats of violence and personal danger, the foulest misrepresentations, the most reckless calumny, and the wildest and most desperate efforts to stay his career and counteract his teachings), but only recently and indirectly has the challenge received public and formal attention. A perfect stranger, a gentleman whom the Author has never yet seen, was so deeply impressed with the truth of what he had read in “Zetetic Astronomy,” and its vast importance, that he determined to do his utmost to bring it to something like a practical culmination. He soon found however that upon the breast of Modern Astronomy lay a terrible incubus,—a dead-weight which no amount of argument could lift; a mass of gravitating cohesion which all the truth-love in the world acting conjointly and even centrifugally could not dis sever; truth, reason, consistency and magnitude of consequence were all powerless in its presence: but, as in all other human combinations, there was an element of weakness—one little point in the structure was vulnerable. A sum of money was offered to any one who could prove the convexity of water, and when all other means had failed to draw the attention of the scientific to the subject, this one little obtrusive element, self, began to operate and an attempt was made to win the prize. Pseudo and meagre and improper as it was, by its influence the philosophic world has been disturbed. It has drawn and fixed the attention of thousands who otherwise would have remained in permanent opposition or indifference; and the hope may reasonably be held that ere long the Royal Astronomical and Geographical or other Societies will feel it their duty to step forward and give that aid and attention which the subject most undoubtedly demands.

The gentlemen who so courageously advanced the thousand pounds in order to bring the matter to an issue, although their proceedings were altogether faulty and insufficient for the purpose, deserve the warmest thanks of all those who feel that every error is injurious, and that truth alone is the real and abiding friend of humanity.

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Part II, containing some of the *consequences* of the fact that the Earth is a Plane, is preparing for Publication.