The history of the determination of the velocity of light to the mid twentieth century

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ABSTRACT

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Title: THE HISTORY OF THE DETERMINATION
OF THE VELOCITY OF LIGHT TO THE
MID TWENTIETH CENTURY.

RICHARD MALCOLM GOLIGHTLY

The thesis looks at the various attempts at measuring the velocity of light from ancient times to 1940. It concentrates on astronomical and optical methods apart from mentioning electrical methods where this was considered necessary in the historical development.

In the early part of the study the ancients considered that light travelled faster than sound and controversy arose as to whether it had a finite or infinite velocity.

A brief look is taken at the theories of Alhazen and Roger Bacon before turning to the work of Galileo and his attempts to produce an experimental verification of the finite velocity of light.

The experiments of Roemer and the first astronomical verification of the finite nature of the velocity using the satellites of Jupiter are considered in some detail. Here mention is made of the work of Descartes and the independent verification by Bradley in 1729.

Next the rival wave and corpuscular theories of light are considered as in trying to explain the phenomena of refraction each theory gave rise to a different value for the velocity of light as it travelled through a more dense transmitting medium. Thus the velocity of light became a crucial factor in deciding which theory had more merit.

Wheatstone's use of a revolving mirror to measure small time intervals is mentioned as well as the Fizeau method on comparing the velocity of light in air and water.

The main part of the thesis concentrates on the various terrestrial optical methods of the nineteenth century starting with the experiments of Foucault, Cornu and Fizeau.

The work of Young and Forbes is given in detail since their series of experiments were made so that each observation was to be an accurate measurement of the velocity.

The classic experiments of Michelson spanning 1879 - 1930 are considered in detail as well as mentioning the work of Newcomb and Perrotin.

The work of de Bray is mentioned along with a comparison of modern determinations.

The concluding chapter draws attention to the emergence of the 'experimental method' in Renaissance times and the requirement of progress in scientific technology before accurate measurements can be taken. The transition from the single scientist working in isolation developing into the team effort as is common practice today is also mentioned.

THE HISTORY OF THE DETERMINATION OF THE VELOCITY OF LIGHT TO THE MID TWENTIETH CENTURY

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APPENDICES

DECLARATION

No material contained in the thesis has previously been submitted for a degree in this or any other university.

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CHAPTER 1.

FROM ANTIQUITY TO THE TIME OF GALILEO

In Antiquity, prevalent ideas on the nature of light were very different from those of today. It was true that in the pre-Socratic period there were philosophers like Democritus(i) who, through his general atomic theory regarded light as small particles emitted from a source and moving with finite velocity. But this theory was never widely accepted. The problem of the velocity of light was frequently mentioned during discussion on the relative merits between the rival emission and atomic theories of light (see later). It was generally accepted that since lightning comes before the thunder then light travels faster than sound.

The Greek philosophers arrived at two main theories concerning the nature of light, both of which involved the use of particles. Firstly Democritus and the Pythagoreans(ii) considered that vision is caused by the projection of particles of light from the object seen, into the pupil of the eye. Secondly Empedocles(iii) Euclid(iv) and the Platonists(v) held the doctrine of ocular beams where the sense of vision was considered to be similar to the sense of touch. The eye itself emits a stream of particles - rays of light; these rays go out and "apprehend" the object seen (1) see Lucretius(vi) (2) see Plato.

(i) Democritus of Abdera (approx 460 - 370 B.C.). Philosopher.
(ii) Pythagoras of Samos (560 - 480 B.C.) Philosopher.
(iii) Empedocles of Acragas (492 - 432 B.C.)
(iv) Euclid (c 300 B.C.) Alexandrian Mathematician.
(v) Plato (427 - 348/347 B.C.) Philosopher.
(vi) Titus Lucretius Carus of Rome (96 - 55 B.C.) Philosopher.

Aristotle(i) in turn, rejected the atomic theory, falling back on the view originating with the Pythagoreans that the essence of matter was to be found in four primary and fundamental qualities, existing in contrasted and opposite pairs - the hot and the cold, the wet and the dry. He also objected to the Euclidian model. If rays of light were emitted from the eyes, then how was it that when we open our eyes we see things immediately? One could say that light travels very fast; but we see even the distant stars instantaneously, and the stars are very far away in anyone's cosmology. Perhaps the light waves travelled with infinite speed, but this idea was abhorrent to Aristotle (3).

By the time we reach Mediaeval times the velocity of light had been one of the most debated subjects concerning natural philosophy, especially since Ibn al-Haitham (Alhazen)(ii), Ibn Sina(iii) and others of the Arab school had insisted that to enable the human eye to see, the existence of an external 'something' of a physical nature was necessary. Two opposing theories existed: one suggested that this 'something' was endowed with a very high but finite velocity, while the other maintained that the velocity was infinite. The failure of every attempt made to measure this velocity strengthened the faction that held the view that the velocity was infinite. It is true that in most cases the reason for believing that the velocity of light was infinite was dictated by metaphysical considerations and often by observations which were both superficial and wrongly interpreted. On the other hand there was great confusion of ideas. One group thought in terms of the velocity of visual rays, and the fact that as soon as they

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(i) Aristotle of Stagira (384 - 322 B.C.) Athenian Philosopher
(ii) Ibn Al-Haitham of Basra (965-1039) Founder of Cairo University
(iii) Ibn Sina of Bukhara (980-1037)

opened their eyes they could see extremely distant objects such as the stars, seemed to justify their conclusion that rays had an infinite velocity. Another group thought in terms of the velocity of the species, and repeated the same reasoning as that used for the visual rays without realizing that this reasoning, when applied to the species, was not logical.

From the philosophers of the time, Ibn al Haitham was significant in assuming a finite velocity of light. He tried to explain refraction by a theory on which the velocity was split into one component parallel to the surface between the two media, and another perpendicular to it. When light was passing from a less dense to a more dense medium the parallel component, he maintained, was diminished so that the angle of refraction became smaller than the angle of incidence. Al-Haitham did not succeed in discovering the exact law of refraction although his theory did lead to a reduced velocity of light in a denser medium.

Mention should also be made of Roger Bacon(i), a disciple of the English scholar Robert Grossteste(ii) who attempted the creation of a completely new and comprehensive philosophical system by which Christianity could be defended against Islam. Bacon knew of Ibn al-Haitham's optical investigations and followed his Arab predecessor in assuming a finite velocity of light.

In 1604 there appeared the book <u>Ad Vitellionem Paralipomena</u> in which Kepler(iii), under such a modest title exposed many

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(i) Roger Bacon, (1219 - 1292) Franciscan Scholar at Oxford.

(ii) Robert Grossteste (1168-1253) Chancellor of Oxford 1215/1251 Bishop of Lincoln

(iii) Johannes Kepler (1571-1630) Imperial Mathematician at Prague and Court Astronomer.

fundamental concepts. In the first chapter he gave 34 Propositions summarizing the physical properties of light and its relation with colour (4). Kepler considered that light had infinte velocity but it should be noted that this Proposition suffered considerably from the scarcity of experimental data.

CHAPTER 2

THE INFLUENCE OF GALILEO ON THE THEORIES CONCERNING THE VELOCITY OF LIGHT.

We must now turn our attention to the works of Galileo(i) on the determination of the speed of light. He had as late as 1623 entertained the notion that light was transmitted instantaneously. (5) However it was in his book Dialogues Concerning Two New Sciences that he proposed an experiment to calculate its velocity.

In the Dialogue, the roles of the interlocutors were clearly defined with Salviati, Galileo's spokesman, representing the mathematical intellect of the new science; Sagredo, the mind already freed from any prejudices of Aristotelian tradition and the illusions of common sense, a mind which was therefore capable of grasping the new truth of the Galilean arguments; Simplicio represented common sense, believing in the authority of Aristotle and of official science, struggling under the burden of tradition.

Salviati and Sagredo started to discuss the recent publication of Father Cavalieri(ii) on the subject of the burning glass (specchio ustorio).

(6) SALVIATI - "Hence I do not understand how the action of light, although very pure, can be devoid of motion and that of the swiftest type."

SAGREDO - "But of what kind and how great must we consider this speed of light to be? Is it instantaneous or momentary

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(i) Galileo Galilei (1564 - 1642) Professor at Pisa, Padua etc. Philosopher to the Duke of Florence.

(ii) Buonaventura Cavalieri (1598 - 1647) Jesuit Priest and Prior at Bologna.

or does it like other motions require time? Can we not decide this by experiment?"

SIMPLICIO - "Everyday experience shows that the propagation of light is instantaneous; for when we see a piece of artillery fired, at a great distance, the flash reaches our eyes without lapse of time; but the sound reaches the ear only after a noticeable interval."

SAGREDO - "Well, Simplicio, the only thing I am able to infer from this familiar bit of experience is that sound, in reaching our ear, travels more slowly than light; it does not inform me whether the coming of the light is instantaneous or whether, although extremely rapid, it still occupies time. An observation of this kind tells us nothing more than one in which it is claimed that 'As soon as the sun reaches the horizon, its light reaches our eyes'; but who will assure me that these rays had not reached this limit earlier than they reached our vision?"

SALVIATI - "The small conclusiveness of these and other similar observations once led me to devise a method by which one might accurately ascertain whether illumination, i.e., the propagation of light, is really instantaneous. The fact that the speed of sound is as high as it is, assures us that the motion of light cannot fail to be extraordinarily swift. The experiment which I devised was as follows:

Let each of two persons take a light contained in a lantern, or other receptacle, such that by the interposition of the hand, the one can shut off or admit the light to the vision of the other. Next let them stand opposite each other at a distance of a few cubits and practice until they acquire such skill in uncovering and occulting their lights that the instant one sees the light of his companion he will uncover his own. After a few trials the response will be so prompt that without sensible error the uncovering of one light is immediately followed by the uncovering of the other, so that as soon as one exposes his light he will instantly see that of the other. Having acquired skill at this short distance let the two experimenters, equipped as before, take up positions separated by a distance of two or three miles and let them perform the same experiment at night, noting carefully whether the exposures and occulations occur in the same manner as at short distances; if they do, we may safely conclude that the propagation of light is instantaneous; but if time is required at a distance of three miles which, considering the going of one light and the coming of the other, really amounts to six, then the delay ought to be easily observable. If the experiment is to be made at still greater distances, say eight or ten miles, telescopes may be employed, each observer adjusting one for himself at the place where he is to make the experiment at night; then although the lights are not large and are therefore invisible to the naked eye at so great a distance, they can readily be covered and uncovered since by aid of the telescope, once adjusted and fixed, they will become easily visible."

SAGREDO - "This experiment strikes me as a clever and reliable invention. But tell us what you conclude from the results."

SALVIATI - "In fact I have tried the experiment only at a short distance, less than a mile, from which I have not been able to ascertain with certainty whether the appearance of the opposite light was instantaneous or not; but if not instantaneous it is extraordinarily rapid - I should call it momentary; and for the present I should compare it to motion which we see in the lightning flash between clouds eight or ten miles distant from us. We see the beginning of this light - I might say its head and source - located at a particular place among the clouds; but it immediately spreads to the surrounding ones, which seems to be an argument that at least some time is required for propagation; for if the illumination were instantaneous and not gradual, we should not be able to distinguish its origin - its centre, so to speak - from its outlying portion."

Descartes(i) qualified this experiment as "useless" and offered an alternative which he outlined in a private letter in 1634 but never included in his published writings. Descartes' correspondent had suggested an experiment similar to the one Galileo proposed: an observer would move a lantern in front of a mirror placed at a quarter of a mile and the interval between moving the lantern and perceiving its reflection in the mirror would afford a measure of the velocity of light. Descartes replied that there was another experiment "often performed by thousands of careful observers that showed that there was no lapse of time between the moment light (7) left the luminous object and the moment it entered the eye." This experiment was provided by the eclipse of the moon. Descartes' correspondent had conjectured that the speed of light was such that it could cover the quarter of a mile to and from the mirror in one pulse beat. Descartes generously

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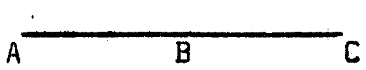
(i) René du Perron Descartes (1596 - 1650) Philosopher proposed to increase this value by 24 times to 1/24th of a pulse beat for a quarter mile or 1/6th for one mile. Assuming the current values of 50 earth radii for the distance of the moon and 600 miles for the length of the earth's radius, this would entail that light takes 5000 pulse beats or roughly one hour to travel from the earth to to the moon and back again.



Now along a line ABC, let A, B and C represent the positions of the sun, the earth, and the moon respectively; and suppose that from the earth at B the moon is being eclipsed at C. The eclipse must appear at the moment when the light emitted by the sun at A, and reflected by the moon at C, would have arrived at B if it had not been interrupted by the earth. On the assumption that it takes one hour for the light to make the return journey from B to C, the eclipse should be seen one hour after the light from the sun reaches the earth at B. In other words, the eclipse should not be observed from the earth until one hour after the sun has been seen at A. But this is false since, when the moon is eclipsed at C, the sun is not seen at A an hour earlier, but at the same moment as the eclipse. "Hence", Descartes declared, using the same word he was to apply to Galileo's suggestion, "your experiment is useless". (8) The issue of the instantaneous or temporal propagation of light was peripheral to Galileo's physics but it played an important role in Cartesian mechanism where it illustrated the casual efficacy of contact action in a world permeated with subtle matter. Descartes saw the instantaneous propagation of light as experimental evidence for his theory and he was even prepared to admit that if an interval of time were detected "my entire philosophy would be completely subverted." (9).

CHAPTER 3

THE FIRST DETERMINATION OF THE VELOCITY OF LIGHT.

IMMEDIATE BACKGROUND

Ole Roemer (Romer)(i) in September 1676 announced to the (Paris) Academy of Science that the eclipse of the innermost satellite of Jupiter would occur exactly ten minutes later than the time calculated on the basis of previous eclipses. He explained that the delay was caused by the simple fact that astronomers considered light to be propagated instantaneously rather than gradually. Once his prediction had been confirmed by observation, Roemer told the Académie that the speed of light was of such magnitude that it would require about 22 minutes to traverse the diameter of the annual orbit of the earth.

Prior to Roemer's work the finiteness of the velocity of light was considered by Roger Bacon who although (was) in perfect agreement with Alhazen's conclusions on this subject, felt that he must show, nevertheless, that they were arrived at on no proper basis. Bacon was interested in Astrology (in which he believed implicitly) and was interested in the means whereby the astral influences, as well as starlight, were transmitted through space. In his Opus Majus he said that (10) "all authorities make this statement (that light travels instantaneously) except Alhazen who attempts to prove this view false..... But these reasons of Alhazen do not have any weight." Essentially Bacon showed that the sort of

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(i) Ole Christensen Roemer (1644 - 1710) Professor at Copenhagen and Scientific Adviser to the King of Denmark.

reasoning used by Alhazen was identical with that of the scientists who attempted to prove the opposite view. Yet Bacon merely replaced Alhazen's argument by his own which was equally metaphysical to conclude that

"Aristotle's statement that there is a difference between the transmission of light and that of other sensory impressions is not to be understood as consisting in the fact that light is transmitted instantaneously and the other impressions require time... this difference is not one of instantaneousness and time, but a less time and more time". (11)

Now Francis Bacon[i] felt that the velocity of light was finite. His remarks on the subject were a classical example of the confusion exhibited by a first rate mind attempting to be reasonable with no scientific basis to act as a guide. (12)

"Even in sight, whereof the action is most rapid, it appears that there are required certain moments of time for its accomplishments.... (It is not surprising that we do not see the actual passage of light, for there are) things which by reason of the velocity of their motion cannot be seen - as when a ball is discharged from a musket. This fact, when others like it, has at times suggested to me a strange doubt, viz., whether the face of a clear and starlight sky be seen at the instant at which it really exists, and not a little later; and whether or not, as regards our sight of heavenly bodies, (there is) a real time and an apparent place which is taken account of by astronomers in the correction for parallaxes (whether or not) the images or rays of heavenly bodies take a

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(i) Francis Bacon (1561 - 1626) Viscount St. Albans. Lord Chancellor of England.

perceptible time in travelling to us. But this suspicion as to any considerable interval between the real time and the apparent afterwards vanished entirely... What had most weight of all with me was, that if any perceptible interval of time were interposed between the reality and the sight, it would follow that the images would oftentimes be intercepted and confused by clouds rising in the meanwhile, and similar disturbances of the medium."

The theoretical background of science at this time was in a state of flux with "the whole scientific mode of thought in these times corrupted as it was by theology and scholastic divinity" (13) being very evident in the works of Kepler (4). Kepler wrote two treatises on optics; one concerned completely with refraction, (14) and the other, an earlier work, a type of commentary and supplement to Vitellius(i), a treatise on the whole science of light. In this earlier work (15) he begins (page 6) by working out that a sphere, considering its centre, radius and surface may be a representation of the Trinity. Later on he analysed the characteristics of light, stating that from each luminous point an infinite number of rays travel out to infinity The propagation takes place instantaneously because light has neither mass nor weight (page 9). Therefore having no mass, the light can offer no resistance to the moving force and according to Aristotelian mechanics, giving the light an infinite velocity.

Beeckman(ii) seems to have been certain, not only that the velocity of light was finite, but that this fact could be

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(i) Vitellius (1230 - 1275) Polish Physicist/Philosopher see <u>Vitellionis</u>, Nuremberg, 1535. Third Edition edited by F.Risner.

(ii) Isaac Beeckman (1588 - 1637) Dutch Physicist. Rector at Dordrecht. Author of <u>Mathematico Physicarum Meditationum Quaestionum Solutionum Centuria</u>. Utrecht 1644 verified experimentally and the magnitude of that velocity determined. In his journal (March 19th 1629) it states: (16) "Distet homo ab alio per tot miliaria per quod (lege quot) bombardi explosi lumen potest videri; &quo spatium hoc fit majus, stet uterque in monte excelso, ne quid in medio abstet quo minus lux vel flamma ignis accensi videri possit. Verisimile autem est, magnum spatium requiri ad differntiam aliquam notandam tempore, ob incredibilem luminis in movendo celeritatem. Uterque homo habeat exactissimum horologium portatile, & uterque, tam is qui bombardo exploso astat quam qui tam longe ab eo remotus est, uterque, inquam, eo momento quo lumen videt, in horologij celerrima rota notet punctum aliquod, vel atramento vel alio modo, quo exacte potest scire quot denticuli tacti fuerint dum sibi invicem in via occurrerint. Uterque enim cum horologio suo ad socium proficiscatur; at que ubi sibi occurrerint, unusquique numeret quot denticul in suo horologio transierint; idque saepius permutatus horologijs. Verisimile mihi videtur, non tantam esse lucis celeritatem, quin illi deprehasuri sint, plures dentes traniojsse in horologio ejus qui bombardo exploso adliterat." which translates as:

"Let one man stand at a distance from another over as many miles as to allow the light from a burning flare to be visible (to be within range of vision). Where the distance is greater than this, then let each of them stand on a high hill to avoid any obstacle in between preventing the light or flame from a lighted fire being seen. However it is probable that a long distance is required to measure a quantifiable difference in time, on account of the unimaginable speed of light in motion. Each man should have a portable clock exactly synchronised with each other; each man, both the one who stands close to the burning flare and the one who is far removed from it, each man, I repeat, at the precise moment that he sees the light should mark on the clock's second hand (lit. 'the fastest wheel') a point either with dye or by some other method, by which he can determine precisely how many teeth (on the cog-wheel, I imagine) have been 'clicked' (i.e. elapsed time) by the time the two men meet each other on the road. For each man should set off towards his opposite number with his clock; when they meet, each one should then count how many teeth have ticked away on his own clock. This count should be done repeatedly - and the clocks exchanged. I think it probable that the speed is not so fast that they will not be able to observe that more teeth have ticked away on the clock kept by the one who was positioned where the flare was lit."

Beeckman tried to convince Descartes that the velocity of light was finite but without much success. Descartes, defending his belief in instantaneous propagation, had worked out what seemed to him to be final and complete proof that his belief was the only one tenable. In a letter to Beeckman (August 22nd, 1634), he reviewed all of their previous correspondence and interchange of ideas on the subject.(17) Descartes' argument in favour of instantaneous propagation was, in principle, scientifically sound as opposed to unconfirmed hunches (Galileo) and metaphysical arguments (Bacon, Alhazen). The mistake he made was in the estimation of how large the velocity of light might be if it were finite: his value being much too small.

He considered an eclipse of the moon, caused by the moon, earth and sun being in a straight line, with the earth interposed between the other two. Should it take an hour, say, for light to travel from the earth to the moon. Then the moon will not become dark until exactly one hour after the instant of collinearity of the three bodies. Similarly one would not observe (on earth) the moon's darkening until the passage of another hour, or until two hours after the moment of collinearity. But in this time, the moon will have moved in its orbit and the three bodies will no longer be collinear. Hence, Descartes argued this is contrary to experience, for one always observes the eclipsed moon at the point of the ecliptic opposite to the sun (so that, for example, one never sees the sun and the eclipsed moon simultaneously). Hence light does not "travel in time" but in an instant. Descartes asserted in his two works on optics, that light travels instantaneously. Yet, in neither of these did he give the above observation as his basis for his assertion.(18) He instead reasoned using in one instance, a blind man who feels the impact of his stick upon a stone the moment the stone is struck, and in the other, a pile of elastic balls, where a movement of one of the balls at the bottom of the pile is transmitted instantaneously to those at the top. Descartes was perfectly willing to admit that the concept of instantaneous transmission was difficult to grasp.(19) Mersenne(i) questioned him on this point, being bothered by the seeming exclusion of priority of place (i.e., if light travels instantaneously, how can it be first in one place and then in another, for that would imply a lapse of time between the instants of being in

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(i) Marin Mersenne (1588 - 1648) Priest at the Place Royale the two places). Descartes replied only "pour la difficulté que vous trouvez en ce qu'elle, se communique en un instant, il y a de l'équiuoque au mot d'instant..."(20) Descartes was of the opinion that light depends on a pressure which is propagated instantaneously and he thought of it as being similar to the pressure in a liquid. In his Discours premier (21) he stated "that light in the body we call luminous is simply a given motion or a given action which is very quick and lively and which moves towards our eyes passing through the air and other transparent bodies, in the same way as the movement or the resistance of bodies met by this blind man passes to his hand through the stick."

In his Discours Seconde, Descartes studied reflection, diffusion and refraction of projectiles rather than of light. Once the laws were established for projectiles he extended them to light with only slight variations being necessary. He proved the law of refraction by following Alhazen's reasoning but added in mathematical form that the ratio between the sines of the angles of incidence and of refraction is constant. Furthermore when light passed from air to water the ray is bent towards the normal to the surface of separation which in turn led to the conclusion that the normal component of motion had increased. The conclusion of this was that the velocity of light should be greater in the denser medium. Now if light were supposed to have an infinite velocity, it is not at all clear what such a statement would mean.(22) Grimaldi(i) in his book de Lumine considered the theory of

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(i) Francesco Maria Grimaldi (1618 - 1663) Priest at the Jesuit College, Bologna.

of Descartes, where he emphasised that Descartes' theory required an increase of velocity in the denser medium when refraction occurs. "In reality I consider that this opinion in itself and in its sole exposition appears to me improbable".(23)

Finally looking at the works of Robert Hooke(i) who tried to grasp at the idea of a wave theory of light. In his Micrographia (24) he said that light was essentially a motion that was "exceeding quick", he added that light "may be communicated or propagated... to the greatest imaginable distance in the least imaginable time: though I see no reason to affirm that it must be in an instant. For I know not any one experiment or observation that does prove it ... (And as for most statements on the subject) I have this to answer. That I can as easily deny as they affirm. If indeed the propagation were very slow, tis possible something might be discovered by Eclypes of the Moon; but though we should grant the progress of the light from the Earth to the Moon, and from the Moon back to the Earth again to be full two minutes in performing, I know not any possible means to discover it; nay, there may be some instances perhaps of Horizontal Eclypes that may seem very much to favour this supposition of the slower progression of Light than most imagine. And the like may be said of Eclypes of the Sun etc."

(i) Robert Hooke (1635 - 1702) Secretary of the Royal Society and Professor of Geometry at Gresham College.

CHAPTER 4.

ROEMERS' ASTRONOMICAL OBSERVATIONS

One of the first projects of the Academie Royale des Sciences was the preparation of maps less defective than those in use at the time. This project was quite feasible since the pendulum clock invented by Huygens(i) in 1657 was reliable enough to serve in the determination of longitude. However an astronomical phenomenon was required capable of simultaneous observation from a point of known longitude and the place whose longitude was to be determined. Such a phenomenon was the eclipses of the first four satellites of Jupiter discovered by Galileo in January 1610.(25) Before they could be used for this purpose tables of their motion were needed. The earliest tables of this sort that enjoyed any confidence at all among astronomers were those published by Cassini(ii) in 1668, together with his later set published in 1693. (see Appendix (iii)).

The first observations of the eclipses of the satellites of Jupiter made at Paris were those of Jean Picard(iii) and were taken (26) before Cassini had arrived from Italy to be director of the Observatoire.(27) Picard had first to determine the precise longitude of Uraniborg on the island of Hveen so that proper use could be made of all previous observations. He started to make observations on September 6th 1671 with the help of Erasmus Bartholin(iv) and Ole Roemer. There the

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(i) Christiaan Huygens (1629 - 1695) Academie des Sciences, Paris

(ii) Giovanni Domenico Cassini (1625 - 1712) Professor of Astronomy at Bologna.

(iii) Jean Picard (1620 - 1682) Prior of Rille, Professor at the College de France.

(iv) Erasmus Bartholin (1625 - 1698) Physician, Copenhagen University.

party observed a series of eclipses of the first satellite of Jupiter while Cassini made simultaneous observations in Paris.(28)

Ole Roemer of the University of Copenhagen studied under Bartholin and eventually joined the Académie as assistant to Picard and Cassini. He made many observations, both in Paris and in other parts of France. He displayed great mechanical and inventive genius and constructed a Jovilabium which is of note since it enabled him to account for some of the irregularities in the motions of the satellites.

Du Hamel(i) mentioned a paper which Roemer read to the Academie in 1677 in which he discussed Descartes' proof of the Law of Sines (29) explaining that the admission that light would travel faster in a denser medium was questionable. He preferred the seemingly more logical view that was the direct opposite of Descartes on which basis he gave a synthetic proof of the Law of Sines, similar to the analytical proof given by Fermat.(ii)

Since the satellites of Jupiter were of extreme practical importance, it was necessary to know as much as possible of their irregularities. According to Maraldi(iii) the nephew and collaborator of Cassini -

"On appelle premier inégalité des planetes celle qui vient de leur excentricité au Soleil, & qui est réellement dans leur cours, par rapport à cet Astre, & seconde inégalité, celle qui vient de ce qu'elles sont vûes de la terre, & non du Soleil"(30).

(i) Jean Baptiste du Hamel (1623 - 1706) Secretary to the Académie des Sciences.

(ii) Pierre de Fermat (1601 - 1665) Counsellor in the Parlement.

(iii) Giacono Filippo Maraldi (1665 - 1729) Académie des Sciences.

Cassini announced this 'seconde inégalité' in August of 1675, remarking that this - "seconde inégalité parâit venir de ce que la lumière emploie quelque temps à venir du satellite jusqu'a nous, et qu'elle met environ dix à onze minutes à parcourir en espace égal au demi-diamètre de l'orbite terreste."(31) But "M.de Cassini ne demeura pas longtemps dans la pensée que la propagation successive de la lumiere produisit cette seconde inégalité".(32) In fact when Roemer read his classical paper on the subject, one of the strongest objectors was Cassini himself.

Roemer predicted in September 1676 that the eclipse of the first satellite of Jupiter which was supposed to take place on the following November 9th at 5h 25m 45s would be 10 minutes late. On November 9th, this eclipse was observed at the Observatoire Royal at 5h 35m 45s, in perfect confirmation of his prognosis. On the following November 21st he read another paper to the Académie in which he explained the delay in the eclipse of the preceeding November showed the necessity of his new equation, the equation of light (allowance for the time spent in light's passage) and that the time required for light to cross the diameter of the earth's annual orbit was about 22 minutes.(33) Roemer stressed what was for him the central point, that observations of immersions of the first satellite gave a smaller period of revolution than similar observations of emersions.(34) (see appendix (i)).

Cassini, although having once entertained the idea himself, objected vigorously. Not that there was any difference of opinion between them as to the fact of the delay. The sole disagreement lay in accounting for the delay. Cassini "perceived that the successive propagation of light explained the irregularities in the eclipses of the first satellite when the Earth was in different positions of her orbit; but finding that it did not account in an equally satisfactory manner for the irregularities of the other satellites, he rejected it altogether, and instead of it he used in the tables of the first satellite an empiric equation depending on the relative positions of the Earth and Jupiter."(35)

Huygens read the account of Roemer's discovery and wrote to him on September 6th 1677 (36) asking for more information and asking too, whether or not the figure 22 minutes were correct. In his reply Roemer listed a set of four reasons for the fact that similar computations based on the other three satellites would give no results and also attempted to show why the delay could come from no other cause. In the end Huygens was completely convinced.(37).

Although many of the academicians were convinced of the necessity of the equation of light, the Cassini family remained a stronghold of reaction. A paper was delivered by Maraldi who had devoted much time to the study of Jupiter's satellites. In this paper of 1707 (38) he admitted, in common with his uncle, that the equation of light gave a very satisfactory explanation and account of the errors of the first satellite; but, he maintained, it should vary from the perihelion to the aphelion of Jupiter's orbit. Also the errors should be the same for all the satellites.

Roemer said that he had collected more than 70 observations of the first satellite, these made by Picard and himself since 1668 and had divided them into the following nine periods:

Period	I	Earth	receding from	Jupiter	Mar.1671-May 1671	EMERSIONS
"	II	"	approaching	"	Oct.1671-Feb.1672	IMMERSIONS
"	III	"	receding from "	"	Mar.1672-June 1672	EM
"	IV	"	approaching	"	Nov.1672-Mar.1673	IMM
"	V	"	receding from	"	Apr.1673-Aug.1673	EM
"	VI	"	receding from	"	Jul.1675-Oct.1675	EM
"	VII	"	approaching	"	May 1676-June 1676	IMM
"	VIII	"	receding from	"	Aug.1676-Nov.1676	EM
"	IX	"	approaching	"	June1677-July1677	IMM

The observations of the first satellite number 67. Meyer(i) has computed the mean period of revolution of the first satellite to get (39)

Period	I	1d	18h	28m	47s	Emersions
"	II	1	18	28	18	Immersions
"	III	1	18	28	35	Emersions
"	IV	1	18	28	27	Immersions
"	V	1	18	28	46	Emersions
"	VI	1	18	28	48	Emersions
"	VII	1	18	28	20	Immersions
"	VIII	1	18	28	47	Emersions
"	IX	1	18	28	30	Immersions

This confirmed Roemer's statement that the mean period was always greater when calculated on the basis of emersions than when calculated on immersions.

Roemer chose these observations made during the years 1671, 1672 and 1673 to obtain his figure of 22 minutes for light to cross the diameter of the earth's orbit because he had at his disposal a large number of observations for that period of time. Further during this period Jupiter offered

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(i) Kirstine Meyer (1861 - 1941) Professor at the Roemer Institute Denmark.

comparatively few variations in its movement and distance from the sun (1672 marked the aphelion passage of Jupiter).

The mean period of revolution for the first satellite was computed as follows:

1671 - 1672	1d	18h	28m	30s
1672 - 1673	1d	18h	28m	31s

When one looks at the immersions of January 12th 1672

1671	October	24d	18h	15m		Solar time
				-15m	45s	Equation of time
		297d	17h	59m	15s	Mean Time
1672	January	12d	8h	59m	22s	Solar Time
				+ 9m	23s	Equation of Time
		12d	9h	8m	45s	Mean Time

Subtracting one gets

1672	12d	9h	8m	45s
1671	297d	17h	59m	15s
	79d	15h	9m	30s

For the same period of time, the mean period of revolution was 1d 18h 28m 30s and as there were 45 revolutions of the satellite between October 24th 1671 and January 12th 1672 the eclipse should have taken place at 45(1d 18h 28m 30s) = 79d 15h 22m 30s. Thus the immersion of January 12th occurred 13 minutes earlier than it would have been expected. But during this period (between the two eclipses used in the computation) the distance from the Earth to Jupiter had diminished by 1.21r (r is the radius of the earth's orbit) from which the time required for light to traverse the distance r as $\frac{13\text{min}}{1.21} = 10\text{m } 45\text{s}$

or about 11m as given by Roemer.

Meyer however looking at the increment in the distance from

Jupiter to Earth (August 23 and November 9th 1676) was 1.14r showing that he would have got a 10 minute delay $\frac{10 \text{ min}}{1.14} = 8.7$ min

Roemer's innovation was not generally accepted in France; indeed such was not the case until the startling independent confirmation by Bradley(i) in January of 1729. But by that time, the idea that the velocity of light was finite had gained much headway in England and elsewhere.

In England, Hooke alone was not convinced by Roemer. In the pre-Roemer period Hooke doubted the instantaneous transmission of light. After Roemer's demonstration he doubted finite transmission. He said:

"Supposing this (Roemer's demonstration) may prove it (light) to be temporary and not instantaneous, yet we find that it is so exceeding swift that 'tis beyond imagination; for so far he thinks indubitable, that it moves a space equal to the Diameter of the Earth, or near 8000 miles, in less than one single second of time, which is in as short time as one can well pronounce 1,2,3,4; and if so why it may not be as well instantaneous I know no reason." (40)

Halley(ii) was convinced of the necessity of this new equation and in 1694 he published Cassini's tables of the first satellites of Jupiter (reduced to the Julian style and to the meridian of London); in the introduction, discussing the second inequality, he remarked that Cassini admitted that: "Monsieur Roemer did most ingeniously explain (this second) inequality) by the Hypothesis of the progressive Motion of

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(i) James Bradley (1693 - 1762) Savilian Professor of Astronomy at Oxford and Astronomer Royal.

(ii) Edmond Halley (1656 - 1742) Savilian Professor of Geometry at Oxford and Astronomer Royal.

Light; to which Cassini by his manner of calculus seems not to assent, though it be hard to imagine how the Earth's Position in respect to Jupiter should any way affect the motion of the Satellites. But what is most strange, he affirms that the same Inequality of two Degrees in the Motion, is likewise found in the other Satellites, requiring a much greater time, as above two Hours in the fourth Satellite: which if it appeared by Observation, would overthrow Monsieur Roemer's Hypothesis entirely. Yet I doubt not herein to make it demonstratively plain that the Hypothesis of the Progressive motion of Light is found in all the other Satellites of Jupiter to be necessary, and that it is the same in all."(41)

He listed some observations of his own and some of Flamsteed's(i) and noted that Roemer's figure of 11 minutes was too large and that the figure computed by Cassini (as a time of delay, with no clue to the cause) was too small, being only 7m 5s. The correct figure, said Halley was closer to 8.5m.

Sir Isaac Newton(ii) has made two direct references to the velocity of light. In the first of these in Opticks (42) he mentions that most people consider light to be propagated instantaneously and so initially he defined rays, refractions etc. in accordance with that belief.

"But by an argument taken from the Aequations of the times of the Eclipses of Jupiter's Satellites, it seems that Light is propagated in time, spending in its passage from the Sun to us about seven minutes of time; And therefore I have chosen to define Rays and Refractions in such general terms as may

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(i) John Flamsteed (1646 - 1719) Astronomer Royal

(ii) Isaac Newton (1642 - 1727) President of the Royal Society agree to Light in both cases."

It seemed that Newton wished to avoid commitment to one point of view or the other as long as possible. But when he reached that part of the book where "the last proposition depended on the velocity of light", he introduced the proposition (43) that "Light is propagated from luminous Bodies in time, and spends about seven or eight Minutes of an Hour in passing from the Sun to the Earth". He added that this "was observed first by Roemer, and then by others, by means of the Eclipses of the Satellites of Jupiter".

Bradley discovered the aberration of light and published his findings in 1729 (44) confirming Roemer's "mora luminis" independently. (see Appendix ii). He deduced from his value of the constant of aberration that the time required for light to travel from the sun to the earth should be 8m 12s , (45) a figure much closer to Newton's and Halley's than Roemer's : remarking that

"It is well known that Mr. Roemer supposed that it (light) spent about 11 Minutes of Time in its Passage from the Sun to us: but it hath since been concluded by others from the like Eclipses, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypothesis (the aberration) is as it were a Mean betwixt what had at different times been determined from the Eclipses of Jupiter's satellites." (46)

Bradley's work led to the final acceptance of the finite propagation of light. Even the Cassini family had to give in. Maraldi(i) published a paper in 1741 in which he showed that the equation of light explained much of the irregularity

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(i) Giovanni Domenico Maraldi (1709 - 1788) in the motion of the third satellite.

Delambre(i) wrote that from an examination of the eclipses of Jupiter's satellites the figure he had arrived at was 8m 13.2s (48). Whittaker(ii) mentions an inaugural dissertation of 1875 (49) by Glasenapp(iii) who, discussing the eclipses of the first satellite between 1848 and 1870, derived values between 8m 16s and 8m 21s, the most probable being 8m 20.8s. He also mentioned the work of Sampson(iv), who in 1909 derived the value 8m 18,64s from his own reductions of the Harvard observations and 8m 18.79s from the Harvard reductions, with probable errors of I0.02s.

A more recent determination for the time of transit from the Earth to the Sun is that of Brouwer(v) who from a value of 8.8030 "(10.0020)" for the solar parallax, derived the value 8m 19s.

It is pf little or no consequence that the figure arrived at by Roemer was too large being a little less than a third larger than more recent values. He offered a means of contradicting the general belief that the velocity of light was instantaneous that convinced the major portion of the scientists of his time. Even if his figure was a little large, it was, in any case, of the right order of magnitude. (50)

(i) Jean Baptiste Joseph Delambre 1749-1822) Secretary of the Academy of Sciences.

(ii) Edmund Taylor Whittaker (1873 - 1956) Professor of Mathematics at Edinburgh.

(iii) Sergei von Glasenapp (1848 -) Professor of Physics at St.Petersburg

(iv) Ralph Allen Sampson (1866 - 1939) Professor at Durham and Edinburgh. Astronomer Royal for Scotland.

(v) Luitzen Egbertus Jan Brouwer (1881 - 1966) Professor of Mathematics at Amsterdam.

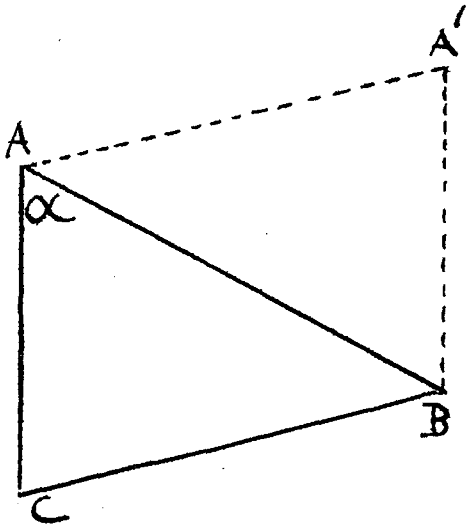
BRADLEY AND THE ABERRATION OF LIGHT

James Bradley published his discovery of the aberration of light in 1729. By aberration it was meant the apparent displacement of a heavenly body due to the combination of the orbital velocity of the earth with the velocity of light. His success was due to a combination of his excellent instrumental means, his own perfect experimental technique together with his thoroughness and persistence.

Bradley did not simply discover aberration for his determination of it was, considering his instrumental means, extremely accurate. He concluded that the maximum aberration was included between 40" or 41", the value of the constant of aberration accepted today is 20"47 (that is 40"94 for the whole axis). He deduced from this value the speed of light, and found that the sunlight would reach us in 8m 13 sec (present estimate (8m 19 sec).

Bradley observed the star γ in the head of Draconis with the object of discovering its parallax, and had found that during the winter of 1725 - 1726 the transit across the meridian was continually more southerly, whilst during the following summer its original position was restored by a motion northwards.

Such an effect could not be explained as a result of parallax. In order to investigate the problem further he had a new telescope erected at Wanstead and there observed this apparent motion of a number of stars over a long period, finally arriving at the complete solution. He considered the matter in the following manner :



If a 'particle of light' moves from A to B while the eye moves from C to B, the axis of an observing telescope must take up the position CA so that the light from A reaches the point B when the axis has gone from CA to the parallel position BA.

The tangent of the 'angle of aberration' is given by $\tan \alpha = \frac{CB}{AB} = \frac{W}{V}$, where W is the earth's velocity perpendicular to the line of sight and V is the velocity of light.

He then proceeded on this basis to a consideration of the apparent movement of actual stars with the motion of the earth around the sun; and from the results of his observations deduced that the angle of aberration ϕ was 20.2" and that the ratio of the velocity of light to the velocity of the earth's motion in its orbit was therefore 10,210 to 1. This gave a value for the velocity of light of 301,000 km/sec.

CHAPTER 5

THE WAVE AND CORPUSCULAR THEORIES

During the C17th and C18th there were two competing views concerning the nature of light. One considered that it was a wave motion, the other that beams of light consisted of streams of corpuscles. The wave theory was developed by Huygens, but was not generally accepted at first because it was overshadowed by the reputation of Newton, who favoured the corpuscular theory.

Descartes, one of the main formulators of the corpuscular theory, applied himself to a study of the nature and the properties of light. Although his researches in optics do not rank in importance with those in mathematics or philosophy because much that is contained in his works is to be found in earlier writers. Nevertheless, the importance of La Dioptrique* is great because of the emphasis placed upon the practical aspect of the science.

The phenomena of reflection and refraction were well known in these times and both the wave and corpuscular theories could easily explain how these phenomena took place. However, in an attempting to explain the refraction of light as it passed from a less dense to a more dense transmitting medium (say) the corpuscular theory indicated that the light travelled faster in the denser medium whereas the wave theory required the light to travel slower in the denser medium.

Descartes seems to have been the first writer to attempt to explain the bending of a ray of light as it passes from one

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* Discours de la Méthode. Plus La Dioptrique etc., Leyden, 1637.

medium to another. He presented the law of refraction as a deduction from theory using the aid of an analogy of a moving ball when rays of light meet ponderable bodies "they are liable to be deflected or stopped in the same way as the motion of a ball or a stone impinging on a body; for it is easy to believe that the action or inclination to move, which I have said must be taken for light, ought to follow in this ~~the~~ same laws as motion."(51)

Descartes assumed that the bending of the ray of light resulted from the unequal speeds of the light, and further, that the speed of propagation depended only on the nature of the medium through which it passed. Furthermore in order to make the analogy with the moving ball relevant, he was forced to make the light travel faster in the denser medium and to explain this he argued that the texture of the rare body was such as to hinder the passage of the light through it.

In his Discours Seconde, Descartes considered that a ray of light is refracted across a plane interface from one medium into another. (52)

Let a light corpuscle, whose velocity in the first medium is v_i, be incident on the interface, making an angle i with the normal to the interface, and let it be refracted at an angle r into the second medium, in which its velocity is v_r.

Therefore $\frac{v_r}{v_i} = \mu$ (say) because the ratio depended only on the nature of the media (see above). Assuming also that the component of velocity parallel to the interface is unaffected by the refraction

then $v_i \sin i = v_r \sin r$

Should $i > r$ then the velocity would be greater in the second or denser medium which turned out to be in contradiction with experimental fact.

Descartes' conclusions were attacked by many of his contemporaries notably Hobbes(i), Fermat and Roberval(ii). Hobbes wrote to Mersenne from Paris (53)(7th February 1641) drawing attention to blemishes in La Dioptrique. Descartes did not take Hobbes' criticism seriously and indeed did not welcome his observations.

Fermat argued that light should travel with diminished speed in the denser medium. This, he thought, followed from a principle which the ancients (especially Hero) had accepted as a corollory to the equality of the angles of incidence and reflection, a principle which later was to be known as the Principle of Least Action. It was known in antiquity that so long as the light travelled in the same medium it would always take the shortest path. Fermat generalised the principle, arguing that it would still hold if the light passed from one medium to another, so that light travelling from a point in one medium to a point in the other would so adjust its path that it would traverse the distance in the shortest possible time. Applying his rules of maxima and minima, which he had now perfected, to such a case he showed that the resistance encountered in the two media would be inversely proportional to the series of the angles of incidence and refraction. He arrived at the solution in 1661 and wrote:

(i) Thomas Hobbes (1588 - 1679) Author of '<u>Tractatus opticus</u>'
(ii) Gilles Personne de Roberval (1602 - 1675) '<u>Traité de méchanique</u>'

"The result of my work, has been the most extraordinary, the most unforeseen, and the happiest, that ever was; for, after having performed all the equations, multiplications, antitheses and other operations of my method, and having finally finished the problem, I have found that my principle gives exactly and precisely the same proportion for the refractions which Monsieur Descartes has established."(54).

Descartes wrongly believed that the speeds would be inversely proportional to the sines of these same angles and he further stated that light must travel more readily through water than through air, and still more readily through glass, results which were experimentally disproved by Foucault in 1850.

The usefulness of Fermat's work was summarised by Whittaker as follows:

"Although Fermat's result was correct, and, of high permanent interest, the principles from which it was derived were metaphysical rather than physical in character, and consequently were of little use for the purpose of framing a mechanical explanation of light. The influence of Descartes' theory was therefore scarcely at all diminished as a result of Fermat's work."(55)

Huygens in his Traité de la Lumière could not accept that corpuscular light could penetrate matter without, at the same time, undergoing some sort of disarray and diffusion. He considered that light was the movement of the matter existing between the object seen and the eye itself. After careful consideration he felt able to conclude that:

"there is no doubt that light also comes to us from a luminous body by some motion impressed on the matter in-between, since as we have already seen, this cannot be by the transport of a body which passes from the luminous object to us".(56) Huygens preferred motion to matter. He considered that the existence of the finite speed of light denied by Descartes but which had been determined by Roemer in 1675 was an argument in favour of his views. He also had adopted the finite velocity of light as a hypothesis several years before Roemer announced his results. Huygens in fact had devised his theory to account for those phenomena which Descartes' theory had tried to explain : namely, rectilinear propagation, the fact that rays of light may cross one another without hindering or impeding one another, reflection and ordinary refraction in accordance with the sine law. His aim was to give a clearer and more plausible explanation than the unsatisfactory inconsistent comparisons proposed in Descartes' Dioptrique and his starting point was exactly those physical problems which the Cartesian theory had left unsolved.

Hooke in his Micrographia (57) said

"the constitution and motion of the parts must be such, that the appulse of the luminous body may be communicated or propagated through it to the greatest imaginable distance in the least imaginable time; though I see no reason to affirm, that it must be an instant."

Hooke here questioned Descartes' hypothesis of the instantaneous propagation of light. He did not actually assert that the velocity of light must be finite. But that he favoured such a view (at the time of writing the Micrographia) may be gathered from the following discussion of Descartes' arguments from the eclipses of the moon:

"I know not any one Experiment or observation that does prove it (viz. instantaneous propagation). And, whereas it may be objected, that we see the Sun risen at the very instant when it is above the sensible Horizon, and that we see a star hidden by the body of the Moon at the same instant, when the Star, the Moon and our Eye are all in the same line; and the like observations, or rather suppositions may be urged. I have this to answer That I can as easily deny as they affirm; for I would fain know by what means any one can be assured any more of the Affirmative, than I of the Negative. If indeed the propagation were very slow, 'tis possible some thing might be discovered by Eclypses of the Moon; but though we should grant the progress of the light from the Earth to the Moon, and from the Moon back to the Earth again to be full two Minutes in performing, I know not any possible means to discover it; nay, there may be some instances perhaps of Horizontal Eclypses that may seem very much to favour this supposition of the slower progression of Light than most imagine. And the like may be said of the Eclypses of the Sun, etc." (58)

He did not himself produce any positive arguments, experimental or theoretical, to support successive propagation. But the picture which he gave in the fifth remark clearly depicted the propagation of light as a process taking place at finite speed.

"in a Homogeneous medium this motion is propagated every way with equal velocity, whence necessarily every pulse or vibration of the luminous body will generate a Sphere, which will continually increase, and grow bigger, just after the same manner (though indefinitely swifter) as the waves or rings on the surface of the water do swell into bigger and bigger circles about a point of it, whereby sinking of a Stone the motion was begun, whence it necessarily follows, that all the parts of these spheres emdulated through an Homogeneous medium with the Rays at right angles."(59)

The above ideas represented a definite advance towards a wave theory. However, one cannot assume that he necessarily understood the vibrations in the light bearing medium to be transverse, that is, at right angles to the direction of propagation. Nor does he say that the pulses or waves follow one another at regular intervals.

Hooke in Micrographia p.57 considered what happens to a pulse or wave-front when it passed from one medium into another. Looking at the construction, the velocity of light must be greater in denser media, since it was based on the Cartesian relation giving the sines in inverse ratio to the velocities.

Now in Huygen's construction for refraction the wave front must be perpendicular to the direction of propagation after refraction.

This construction thus yields a law according to which one must adopt the opinion opposite to that of Descartes regarding the velocity of light in different media.

Whittaker again comments:

"The above represented a decided advantage on the treatment of (60) the same problem by Descartes which rested on mere analogy. Hooke tried to determine what happened to the wave-front when it met the interface between two media;

and for this end he introduced the correct principle that the side of the wave-front which first meets the interface would go forward in the second medium with the velocity proper to that medium, while the other side of the wave-front, which was still in the first medium was still moving with the old velocity; so that the wave-front would be deflected in the transition from one medium to the other."

Huygens later suggested in the Traité (61) that "the progression of these waves ought to be a little slower in the interior of the bodies, by reason of the small detours which the same particles cause."

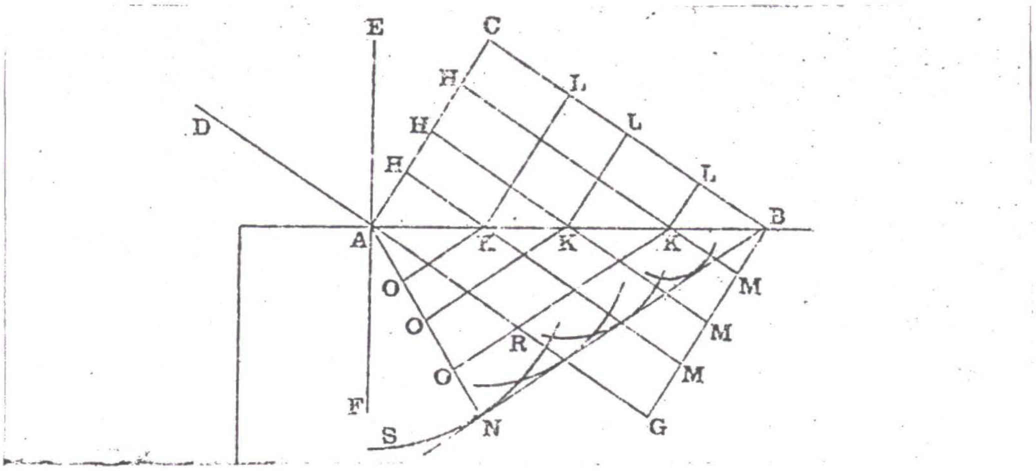
Huygens first adopted the finite velocity of light about three years before Roemer's discovery not because any terrestrial experiment had forced him to do so, but simply because this hypothesis was required for a clear understanding of the phenomena and particularly, of, refraction. It was implied in the Traité that Huygens had changed his mind also about Descartes' eclipses arguments before Roemer announced his results. One of the reasons given by Huygens confirmed the preceding account of the development of his thought.

"I have then made no difficulty, in meditating on these things (Descartes' eclipses arguments), in supposing that the emanation of light is accomplished with time. For it has always seemed to me that even Mr. Des Cartes, whose aim has been to treat all the subjects of Physics intelligibly, and who assuredly has succeeded in this better than anyone before him, has said nothing that is not full of difficulties, or even inconceivable, in dealing with Light and its properties.

But that which I employed only as a hypothesis, has recently received great seemingness (vraisemblance) as an established truth by the ingenious proof of Mr. Romer..."

Huygens Treatise p.7

It would seem that Huygens accepted Roemer's demonstration not so much because he saw in it an impressive revelation of facts but, rather, because it was in agreement with what he had adopted as a physical hypothesis which he had required for a clear explanation of the properties of light. Using the principle of secondary waves, Huygens was able to devise a construction for ordinary refraction (62).



AC being a plane wave front, obliquely striking a separating surface AB. Let V_i be the velocity of the light in the medium above and t the time taken for C to arrive at B. Let V_r be the velocity in the lower medium.

Huygens was able to demonstrate that since the Angle of incidence EAD is equal to the angle CAB and the angle of refraction FAN is equal to the angle ABN, then

$$\frac{\sin i}{\sin r} = \frac{CB}{AB} \times \frac{AB}{AN} = \frac{CB}{AN} = \frac{V_i t}{V_r t} = \frac{V_i}{V_r} = n$$

"This law implies that when the angle of refraction is smaller than the corresponding angle of incidence, the velocity must have been diminished by refraction. And since light in passing from a rare into a dense medium is deflected towards the normal, it must be concluded that the velocity of light is greater in rarer media. Huygens' law is the same as that deduced by Fermat (from the least time principle) and maintained by Pardies(i) and Ango.(ii) But whereas Ango and (perhaps) Pardies simply assumed the wave-front to be perpendicular to the direction of propagation after refraction, this is presented by Huygens as a consequence of regarding the wave-front as a resultant wave composed from the secondary waves generated successively at the surface of the refracting medium."(63)

Newton considered the Cartesian proof in his Optical Lectures of 1669 - 1671:

"The Ancients determined Refractions by the Means of the Angles, which the Incident and refracted Rays made with the Perpendicular of the refracting Plane, as if those Angles had a given Ratio... the Ancients supposed, that the Angle of Incidence ... , the Angle of Refraction ..., and the refracted Angle ... are always in a certain given Ratio, or they rather believed it was a sufficiently accurate Hypothesis, when the Rays did not much divaricate from the Perpendicular ... But this estimating of the Refractions was found not to be sufficiently accurate, to be made a Fundamental of Dioptricks. And Cartes was the first, that thought of another Rule,

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(i) Ignace Gaston Pardies (1636 - 1673) Author and Lecturer at Le Grand College, Paris.
(ii) Father Pierre Ango (1650 - 1700) Author of <u>Optique</u> 1682

whereby it might be more exactly determined, by making the Sines of the said Angles to be in a giving Ratio ... The Truth whereof the Author had demonstrated not inelegantly, provided he had left no room to doubt of the Physical Causes, which he assumed." (64)

By the mid C18th there were two rather distinct lines of development in natural philosophy. Newton's wish to refrain from hypotheses and his deliberate avoidance of unequivocal statements about the causes of forces and the nature of matter, allowed two different overall views of his works, dependent upon the current interests of the interpreter.

SUMMARY BY SCHOFIELD

"Now it is only by implication, and that not a clear one, that Newton's theory of matter can be determined. There is no doubt that he was a corpuscularian, nor that he had modified that belief, rejecting the notion that all natural phenomena were explicable simply in terms of the various sizes, shapes and motions of these fundamental particles of nature. But Newton scholars are still divided as to whether, in the end, he believed that the corpuscles also acted upon one another, at a distance, by means of unexplained immaterial forces of attraction and repulsion, or that an intermediary aether subtle, elastic, and electric, provided the mechanism for their action. For our purposes, the amswer to this problem is essentially irrelevant for we need rather to know that eighteenth-century natural philosophers believed that Newton believed. Unfortunately, it appears that this conflict of opinion divided eighteenth - as well as twentieth-century Newtonians. In the long run the most influential view was probably that of the aetherial school, in which more-or-less traditional materialists successfully reconciled their views with Newton's aether into a series of imponderable fluids each of which carried the properties essential to explain the various phenomena they had been created to solve. Nevertheless, there remained a clear line of British investigators, starting early in the century, who adopted the notion of forces and ignored that of the aether."(65)

Newton in his '<u>Opticks</u>' had developed the arguments for the ratio of the sine of incidence to the sine of refraction of a ray of light. He stated "That bodies refract light by acting upon its Rays in lines perpendicular to their surfaces." John Michell(i) wrote more explicitly: "For let us suppose with Sir Isaac Newton that the refraction of light is occasioned by a certain force impelling it towards the refracting medium, an hypothesis which perfectly accounts for all the appearances." (66)

Newton argued that the velocity of light could be related to the ratio of the sine of incidence and refraction as follows: "If any Motion or moving thing whatsoever be incident with any Velocity on any broad and thin space terminated on both sides by two parallel Planes, and in its Passage through that space be urged perpendicularly towards the farther Plane by any force which at given distances from the Plane is of given Qualities; the perpendicular velocity of that Motion or Thing, at its emerging out of that space, shall be always equal to the square Root of the sum of the square of the perpendicular velocity which that Motion or Thing would have at its Emergence, if at its Incidence its perpendicular velocity was infinitely little."

(i) John Michell (1724 - 1793) Rector of Thornhill.

Newton proceeded with mathematical demonstration to show that the sine of incidence was to the sine of refraction, "in a given ratio." He then added his usual cautionary statement: ..."And this Demonstration being general, without determining what Light is, or by what kind of Force it is refracted, or assuming anything further than that the refracting Body acts upon the Rays in lines perpendicular to its Surface; I take it to be a very convincing Argument of the full truth of this Proposition.

So then, if the ratio of the Sines of Incidence and Refraction of any sort of Rays be found in any one case, 'tis given in all cases; and thus may be readily found by the Method in the following Proposition."(67) see also (64).

John Robison(i) as late as 1788 argued strongly for the Newtonian scheme and was somewhat scathing concerning the rival wave theory:

"The other hypothesis is that of Mr. Huyghens and Dr.Hooke.(68) These gentlemen suppose that, as hearing is produced by means of the tremulous motion of elastic air, which affects the ear, so vision is produced by the tremulous motion of elastic light, which affects the eye. This hypothesis was announced and applied to the explanation of phaenomena in very general terms, and did not, for a long while, engage the attention of the learned. The celebrated mathematician Mr. Euler(ii) has lately brought it into credit, having made some alterations in it. He supposes, that vision is produced by the tremulous motion of an elastic fluid which he calls aether, and which

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(i) John Robison (1739 - 1805) Professor at Edinburgh

(ii) Leonhard Euler (1707 - 1783) Professor of Mathematics at St. Petersburg.

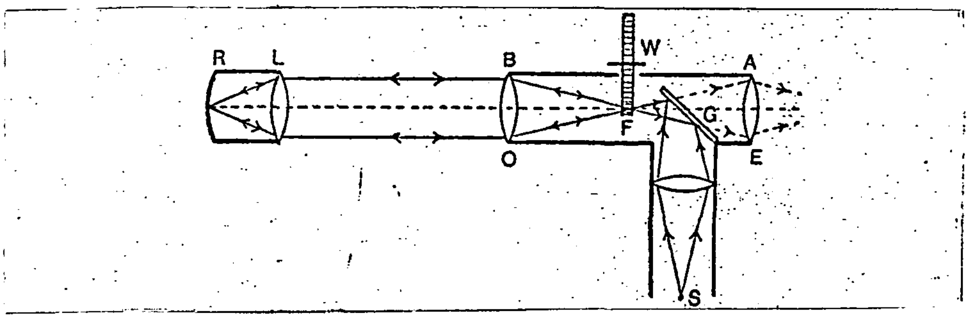
he supposes to pervade all bodies. He attempts to show that the propagation of this tremulous motion is analogous to the appearances in the reflection and refraction of light. I confess that I cannot admit his reasonings on this subject to be agreeable to the principles of mechanics; and I am decidedly of opinion, that the propagation of the tremulous motion of an elastic fluid is totally inconsistent with those facts in vision where no refraction or reflection is observed. But I shall reserve my objections till another opportunity, when I propose to submit to this Society a mechanical explanation of this hypothesis, and I shall admit for the present that Mr. Euler's explanation of refraction and reflection is just. It is an essential proposition in this hypothetical theory, that the velocities of the incident and refracted light are proportional to the sines of incidence and refraction, and therefore that light is retarded when it is refracted towards the perpendicular. It seems a necessary consequence that, in this case, the particles of aether are actuated by forces tending from the refracting body. I shall, therefore, consider what effects must result from the combination of this retardation with the motion of the refracting body. If time will allow, I shall consider what will be the effects produced on the motion of light by the motion of the visible object. These are so different in the two hypotheses, that it is very probable that some natural appearance may be found which will give us an opportunity of determining whether either of these hypotheses is to be received as true."

Robison was correct in his suggestion that the two hypotheses could be subjected to experimental comparison if it could be determined whether light were accelerated or retarded upon entering a medium of higher index of refraction. But this experimental determination did not occur until after 1849, with the work of Fizeau and Foucault. Until then, the choice between the two hypotheses remained largely a matter of personal preference. It was clear that Robison opted for the Newtonian system.

CHAPTER 6

THE WORK OF FIZEAU

The problem of measuring the time interval occupied by light in travelling a relatively short distance on the earth's surface was first overcome by H.L.Fizeau(i)(1849) and in doing so he introduced a principle of fundamental importance in the field of measurement. Instead of trying to measure the short interval occupied by one return journey of light, he arranged for a regular repetition of the journey and observed some parameter, in this case the intensity of the light returned, which reached an optimum value when the time of repetition agreed with the time of travel. The time measurement was thus replaced by the measurement of the rate of repetition or frequency, which is a far easier technical problem.



With reference to the diagram : W was a toothed wheel whose rim was at the principal focus, F, of an objective lens, O. Light from a source, S, was reflected at the surface of the glass plate, G, and brought to focus at F, from which it emerged as a parallel beam. This beam, after traversing a distance of several kilometres, fell on a reflector and hence onto a focus on the surface of a concave mirror, R. The optical centre of the lens, L, was at the centre of

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(i) Armand Hippolyte Louis Fizeau (1819 - 1896) Director of the Bureau des Longitudes.

curvature of R, and so the incident beam of parallel light was returned parallel and fell on O, which formed a real image of the source at F.

Now rays of light eminating from a luminous source diverge in all directions from their position of origin; thus the further a given surface is from the source of light, the less it receives. Therefore in this experiment where distances of several kilometres were involved, the mirror would only receive an insignificant quantity of light, moreover only a very small portion of that light would come back to strike the eye so that very little would be seen. Thus to minimise loss of light the lens system as above was arranged. In fact, the two converging lenses were objectives of two telescopes placed at the extremities of the distance over which the light travelled, and directed towards each other so that the image formed by the objective of one was seen at the focus of the other. (69)

"This arrangement succeeds very well, even when the telescopes are separated by considerable distances. With telescopes of 6 centimetres ($2\frac{4}{10}$ inches) aperture, the distance can be 8 kilometres (nearly 5 miles) without the light becoming too feeble. We thus see a luminous point like a star, and formed by the light, which, setting out from this point, has traversed a distance of 16 kilometres, (nearly 10 miles) then returned and passed exactly through the same point to reach the eye.

"It is exactly at this point that the teeth of the revolving disk must pass to produce the effects spoken of. The experiment succeeds very well, and we observe that, according to the greater or less velocity of rotation, the luminous point shines brilliantly or is totally eclipsed."

In the circumstances in which the experiment was made, the first eclipse was produced in about twelve turns and six-tenths of a turn in one second. With double the velocity the star shines again, with a triple velocity a second eclipse takes place; for a quadruple velocity the point shines again, and so on.

"The first telescope was placed on the terrace of a house at Suresnes, near Paris; the second on the heights of Montmatre, at an approximate distance of 8,633 metres (28,516 feet or 5,3645 miles).

The disk, carrying 720 teeth, was attached to wheel-work moved by weights, and constructed by M.Froment; a register gave the number of revolutions. The light was obtained from a lamp so disposed as to give a very bright beam."

The time occupied by the light to travel 2 x 8633 metres was thus $\frac{1}{2\times720}$ of a revolution or $\frac{1}{2 \times 12.6 \times 720}$ seconds, so that the velocity of light in air was $2 \times 8633 \times 12.6 \times 2 \times 720$ metres/sec or 3.13×10^{10} cm per second.

Doubling the speed of rotation would result in a maximum intensity and it is clear that the precision of setting increases with the speed and the number of teeth that are by-passed. Fizeau however never reported the details of his experiments apart from a single result which was stated to be the average of 28 measurements. This value was given as 70.948 leagues of 25 to the degree, corresponding to the above value in modern units.

The project developed by Arago(i) in 1838 had shown the possibility of measuring the velocity of light and that

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(i) Dominique Francois Jean Arago (1786 - 1853)
Secretary of the Académie des Sciences

it would have to move over a short distance on the earth's surface to determine this velocity. The experiment of Fizeau based on an entirely different method was the first determination of the velocity of light on earth and whose agreement with that which astronomers had arrived at from sidereal observations was satisfactory for a first attempt of this kind.

It was for this experiment at Suresnes that the Institute of France awarded to Fizeau, at its annual meeting in 1856, the triennial prize of 30,000 francs founded by the Emperor for the work or the discovery which, in the opinion of the five academies of the Institute, has done most honour and service to the country.

THE VELOCITY OF LIGHT AND WAVE THEORY

According to the emission theory - the velocity of light in passing from a rarer medium into a denser should be increased. For example, the refractive index in passing from air to water is 4/3; thus according to the emission theory, the velocity of light in air to the velocity in water should be 3/4. Against this stands the wave theory. According to this theory, the velocity of light in passing from a rarer to a denser medium should be diminished; in the case of air and water the ratio of velocities would be reversed and become 4/3.

Arago suggested submitting the question to an experimental (70) test and proposed the idea of using a rotating mirror to carry out the idea using a suggestion of Wheatstone(i). He himself was unable (71) to carry out the experiment due to failing eyesight.

(i) Charles Wheatstone (1802-1875) Professor at Kings' College, London.

Arago conceived a ray of light to fall upon the plane surface of a reflecting mirror set perpendicular to the direction of the light, whence the latter would be sent back along the path by which it entered. If the reflecting surface be oblique to the direction of the light, the latter will be reflected in some other direction; should a second reflecting mirror be set perpendicular to this latter direction, the light will be reflected from this in the direction of the perpendicular, will again strike the other mirror, and be finally sent back by the latter through the aperture by which it entered. In this case the ray has suffered two reflections from the intermediate mirror; and if it is true that light requires time in passing from one point to another, these two reflections cannot occur contemporaneously. A certain portion of time, however small, will be required for the journey to and from one mirror to the other. Now when the aperture and the two mirrors are perfectly still, the path of the light in coming will coincide with its path in returning; but while on its route between the two mirrors, should the position of the first mirror be changed, e.g. become more inclined to the direction of the ray, then the latter will not be reflected along the line of its approach, but will be reflected somewhere to the side of the aperture. This change in the position of the mirror during the almost infinitesimal portion of time occupied by the light on its passage between the mirrors is accomplished by giving the mirror a high angular velocity. Thus this gives a means of comparing the velocity of light in air with its velocity in water. The less time occupied by the light in performing its double journey between the two mirrors, the less the divergence ought to be and vice versa. Hence, if the

Newtonian theory were true, the introduction of a column of water six feet long ought to bring the reflected image of the aperture nearer to the aperture itself; and if the wave theory were true, the introduction of such a column should make the divergence greater.

Such experiments although simple enough in principle, demanded considerable delicacy of manipulation. In order to observe the extremely small divergence Foucault(i) made use of a small square (72) aperture furnished with a number of vertical bars of fine platinum wire; eleven of these fitted in the space of one millimetre, and between each two there was a small space through which the light entered. The image given by this was a small field furrowed with alternate black and white stripes. The light after entering through this aperture fell upon a lens by which it was converged, but before it came to a focus on the opposite side it fell upon the rotating mirror; and it was then cast upon a concave mirror placed about 6 feet away, which reflected it back again. Foucault was able to compare the divergence of the black and white stripes in the image from the platinum wires and their intervening spaces. "I have already proved," said Foucault, "by two successive operations that the deviation of the image after the journey of the light through air is less than after its journey through water. I have also made another confirmatory experiment, which consists in observing an image formed partly by light which has passed through air, and partly by

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(i) Jean Bernard Léon Foucault (1819 - 1868)
Paris Observatory.

light which has passed through water For small velocities, the stripes of this mixed image were apparently continuations of each other. But by the acceleration of the motion the image is transported, and the stripes are broken at the point of junction of the air image with the water image. The stripes of the latter take the advance in the sense of the general deviation. Further, on taking into account the length of water and of air traversed, the deviations are found to be proportional to the indices of refraction. These results indicate a velocity of the light which is less in water than in air, and, according to the views of M.Arago, fully establish the theory of undulation."

Fizeau and Breguet(i) published work on the same subject almost simultaneously. "We have realized with great exactitude the (73) experiment described in our note presented to the Academy during its session of the 6th of May last; an experiment which we felt called upon to make, although M.Foucault in the same session had read an extended paper upon this subject, in which he announces that he has already obtained decisive results. We have thought that, for the solution of a capital question like the present, the proofs could not be too much multipled, and that experiments made under different circumstances could not but contribute to render our knowledge of an important fact more certain. We have applied ourselves to the solution of the question as proposed by M.Arago in 1848; that is to say, How can the two opposite theories regarding the nature of light be submitted to a definite test? We have adopted such measures

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(i) Louis François Clement Breguet (1804 - 1883)
Designer to the Bureau des Longitudes

as are calculated to exhibit in a striking manner the differences of the phenomena as deduced from the one or the other theory.

As remarked in our preceding communications the observation (74) was made simultaneously on two bundles of light; the one having traversed air, the other a column of water.

For each of these bundles the path was as follows: A telescope was so disposed that its object glass was very near the rotating mirror; a little rectangular prism was placed in the focus of the telescope, in such a position that the solar rays falling upon it form a convenient lateral opening near the eye-glass, were totally reflected towards the object glass. Beyond the rotating mirror, and at a distance which for the ray that passed through water amounted to two metres, there was a fixed reflector designed to send back the light to the rotating mirror by a normal reflection.

The focal distance of the telescope was such that the image of the little prism placed at its focus formed itself distinctly upon the fixed reflector just mentioned. After having been reflected from it, the light returned to the rotating mirror, was sent on through the telescope, and on passing the focus formed an image which exactly covered the prism.

By the rotation of the mirror we give birth to a number of images which succeed each other very rapidly, and the superposition of which produces the sensation of a permanent image.

When the rotation became sufficiently rapid the permanent image was pushed forward in the direction of rotation, this direction being the result of the angular motion of the mirror during the time occupied by the light in passing twice over the space which separated it from the fixed mirror.

A second similar fixed mirror was placed beside the former : it permitted us to make the experiment with air and water simultaneously.

If the lengths traversed had been equal for both media, the times occupied in passing them would be in the ratio of 4:3 or of 3 : 4, according to the one or the other theory, and the deviations produced by the rotation of the mirror would have been in the same ratio.

Instead of equal lengths we have adopted equivalent lengths; that is to say, lengths traversed by the light in equal times. These lengths are very different, according as they are calculated from the one or the other theory. The length for water being 1, the equivalent length for air would be 3/4 by the theory of emission, and 4/3 by the theory of undulation.

If the experiment be made by adopting the length 3/4 for air, that of water being 1, according to the theory of emission the times occupied by the two bundles of light in passing over these spaces will be equal, and consequently the deviations will be equal. By the other theory, on the contrary, the times occupied by the light in passing through both media will be very different; these times will be for water and for air in the ratio of 16 to 9, and the deviations will be in the same ratio.

To coincide with the one or the other theory, it will therefore be sufficient to prove, either that the deviations are equal, or that one is nearly double the other.

If the equivalent lengths calculated from the theory of undulation be taken, the results will be similar but inverse.

According to the theory of emission, the deviations will be in the ratio of 16:9, according to the other theory, they will be equal.

We have made these two experiments, and the results obtained are very exact. The phenomena observed are altogether in accordance with the theory of undulation, and in manifest opposition to the theory ofmemission.

In the first arrangement the deviation is greater for water than for air; it is nearly double. The difference is sensible with a velocity of 400 or 500 revolutions per second; with a velocity of 1500 revolutions it becomes quite evident.

In the second arrangement the deviation is the same for air and water; and whatever be the velocity of the mirror, there is no sensible difference between the two deviations.

These experiments have been made in the meridian room of the observatory; the column of water was 2 metres long, and was contained in a crystal tube closed at the ends with glass. This length is more convenient than that which we first employed, namely 3 metres. The light is less weakened, and, after its double passage, retains an intensity which may be estimated at double of that which was obtained with the tube of 3 metres.

The deviations were observed at a distance of 1.50 millimetres from the rotating mirror."

CHAPTER 7

CHARLES WHEATSTONE: The use of a rapidly revolving mirror to measure small time intervals.

Wheatstone was investigating the direction and velocity of the electric spark. His original apparatus (see diagram) was not a great success since he was unable to observe any deflection of the spark. However, a brief description will be given for completeness sake.

Fig.1 shows the apparatus, which was to the spindle of a whirling machine (at a) so that a rapid rotary motion could be given to it. Both the upper and lower parts were brass being insulated from each other by a glass rod de with bc being a wood disc. The ball h was connected with a by tinfoil and it was possible to vary the distance separating the two balls. The ball f was placed so that an electric spark could pass between f and the generator as well as a spark passing between g and h. Wheatstone considered that should the angular motion of the balls be in some proportion to the velocity of electricity then there would be a deviation between the upper and lower ends of the line. With the instrument revolving from left to right, and the motion of the spark be downwards, the deflection of the line should be as in Fig.2; and if the motion was upwards it should be deflected as in Fig.3.

When the apparatus was made to revolve rapidly no deviation of extremeties of either of the two sparks from the same vertical line was observed. The apparatus could revolve at 50 times per second and a difference of 1/20 part of the circumference described by the balls could easily have been observed and hence Wheatsone concluded that the spark passed jointly through the air and the metallic conductor

Fig: 1.

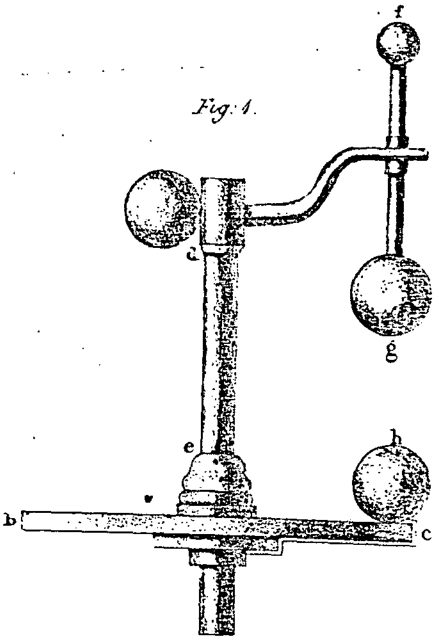

Fig 2.

Fig 3

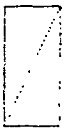

Fig 9.

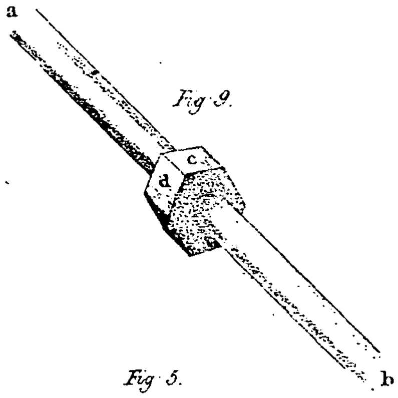

Fig: 4.

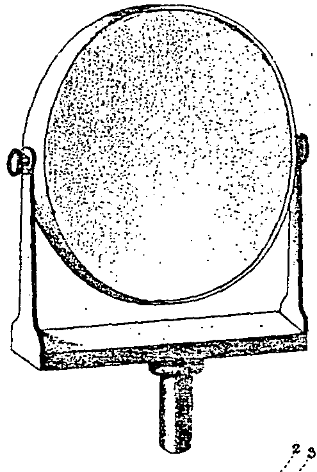

Fig 5.

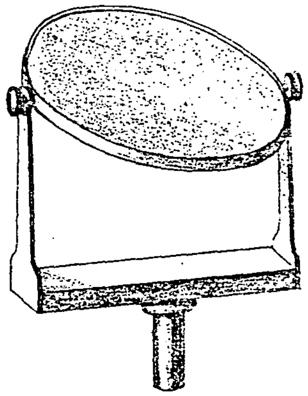

Fig: 6.

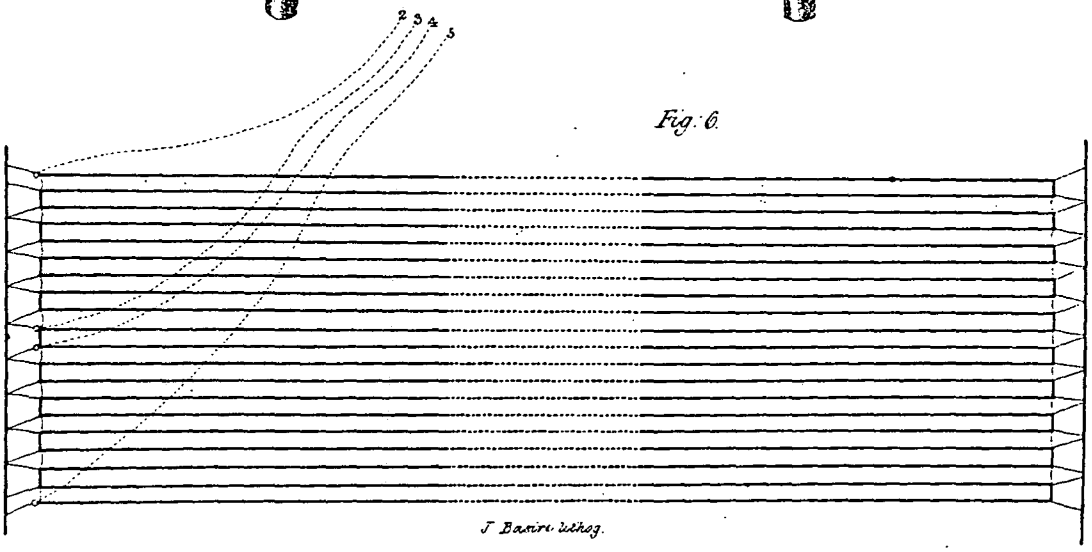

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Fig. 7.

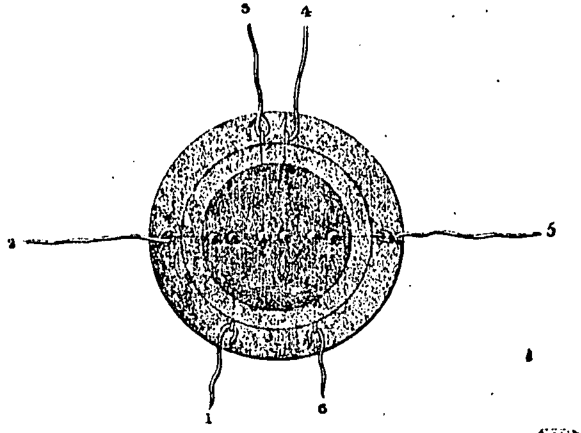


Fig 10.

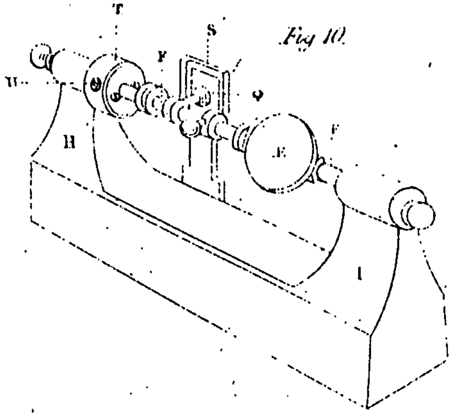
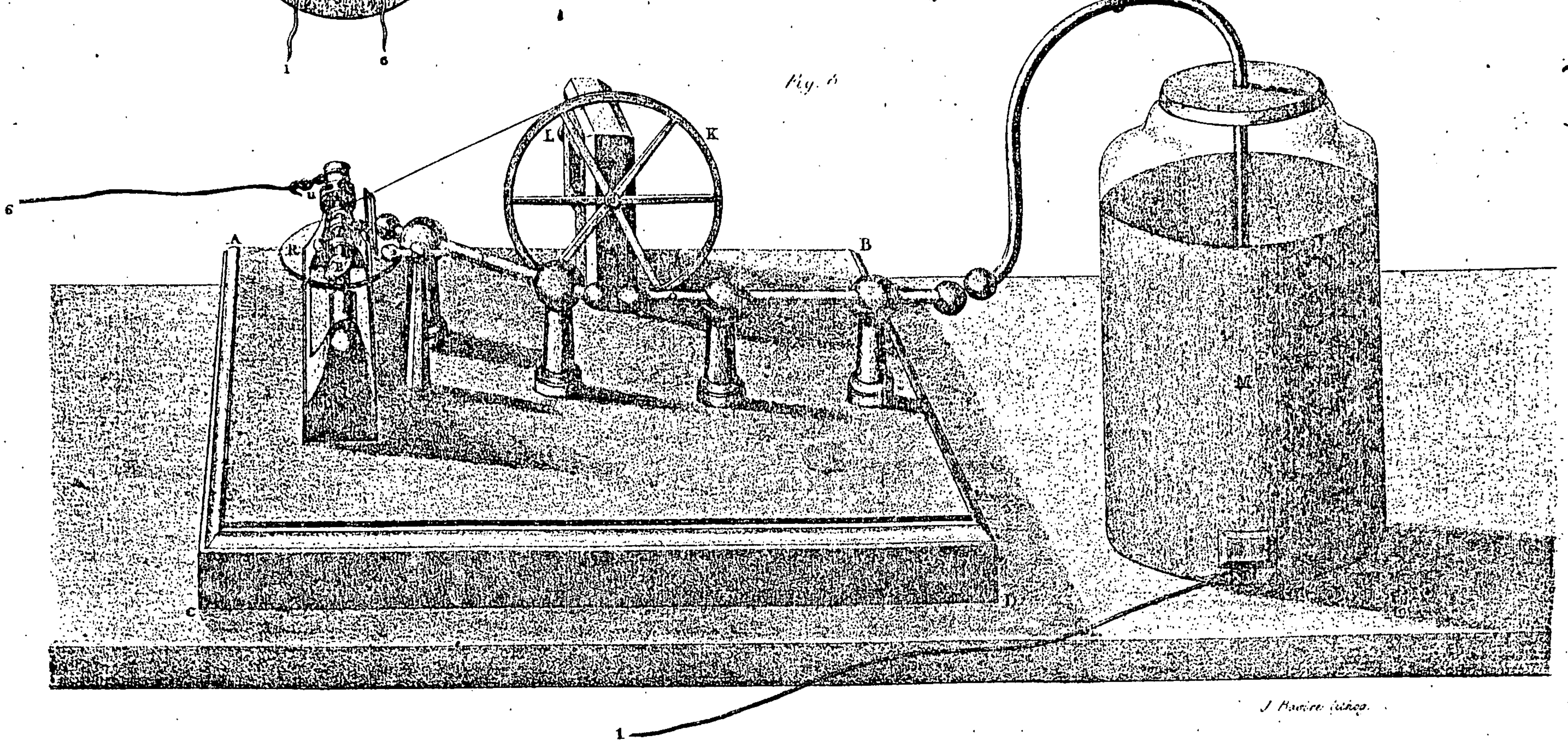


Fig. 6



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in less than 1/1000 of a second.

Wheatstone then continued as follows: (75)

"Having failed to observe any deflection of the spark by the means just mentioned, I found it necessary, if I would continue the inquiry, to contrive some more effectual means of prosecuting it. It occurred to me that the motion of the reflected image of the electric spark in a plane mirror would answer all the purposes of the motion of the apparatus itself connected with the spark. Several advantages, it was evident, would result from this substitution; the apparent motion of the reflected image in a small moving mirror would be equal to an extensive motion of the object itself; the same mirror might be presented to any object to be examined, thus forming, with its moving machine, an independent and universally applicable instrument; and many experiments might be tried, which, without this experiment, would be difficult or impossible to perform, from the sine or immobility of the apparatus.

The most convenient form of the revolving mirror is represented in fig.4; it rotates on a vertical axis, and in its motion successively assumes every vertical plane.

If a luminous point, the flame of a candle for instance, be placed at any distance before this revolving mirror, the successive places of its reflected image will describe a circle, the radius of which is equal to the perpendicular distance between the luminous point and the axis of rotation.

The angular velocity of the image is twice that of the mirror; the entire circle is consequently described while the mirror makes a semi-revolution; and if the back of the mirror be also a reflecting surface, the image will describe two entire circles during one revolution of the mirror. If the motion exceed a certain rapidity, the successive images leave their impressions on the retina, and the eye, properly placed, takes in the view of a perfectly continuous line of light, being an arc of the circle described, which arc is larger in extent in proportion to the proximity of the eye to the mirror.

If now, while the mirror is in motion, the luminous point be moved in a direction parallel to the axis of rotation, the composition of the two motions of the image, the one depending on the motion of the object, the other on the motion of the mirror, will give rise to a diagonal resultant; and if the number of rotations made by the mirror in a given time are known, the direction and velocity of the moving point may be calculated.

By screwing the axis of the mirror to a machine with multiplying wheels, I was enabled to cause it to revolve fifty times in a second. The reflected image of a luminous point, therefore, passed over half a degree in the 72,000dth part of a second, the angular velocity of the image being, as before noticed, double that of the mirror. An arc of half a degree is easily estimated by the eye, and is equal to about an inch seen at the distance of ten feet. Supposing this to be the limit of distinct observation, though perhaps a much smaller arc might be distinguished even by the unassisted eye, we might expect, when a line of electric light is placed parallel to the axis of the revolving mirror, to ascertain two things: first, the duration of the light at each point where it appears; and secondly, the time which elapses between the appearance of the light in two successive points of its path; provided that the time, in either case, be not less than the 72,000dth part of a second. The first would be indicated by the horizontal elongation of the reflected image, and the second by the distance between two lines drawn from the images perpendicular to the horizontal plane. If the duration and velocity were both rendered sensible by the mirror, the reflected image would appear as a deflected band of light.

I successively presented to the mirror, sparks four inches in length drawn from the prime conductor of a powerful electrical machine; the explosions of a charged jar; a glass rube four feet in length, exhibiting a spiral of electric sparks passing between dots of tinfoil; an exhausted glass tube six feet in length, through which the sparks passed, and produced an unbroken line of attenuated electric light; various pictures, such as birds, stars, &c., formed of electric sparks. But in all these cases, when the reflected images occurred within the field of view, they appeared perfectly unaltered, and precisely as they would have done had they been reflected from the mirror while at rest.

When sparks were made to follow each other quickly, several reflected images were simultaneously seen in different positions, owing to the images having been renewed before the visual impression caused by the first had disappeared. The exhausted tube being held near a prime conductor, when looked at directly, will sometimes appear to gleam with a continuous light; but examined in the mirror, this apparent continuity is seen to be owing to a rapid succession of transient flashes."

Wheatstone moved the position of the revolving mirror for other experiments. When the reflecting surface was inclined to the axis of rotation (fig 5) then the angular velocity of the image was equal to that of the mirror, and both moved in the same direction. Whereas in the former case the image moved with double the velocity of the mirror, and in the opposite direction.

He went on to remark on the early experiments to determine the velocity of the transmission of electricity through conducting bodies where attempts were made to measure the time interval supposed to occur between two discharges made at opposite ends of a wire which were brought close to each other so that they might be seen at the same time. In 1747 at Shooter's Hill, Dr. Watson(i) constructed a circuit 4 miles in extent; but the discharge appeared to be perfectly simultaneous, as in all similar experiments. Wheatstone did not consider this surprising since he knew that the eye was unable to distinguish time intervals less than 1/10 second and that with a circuit of four miles in extent, the velocity of a few miles per second would be the most observable by such means. He decided to repeat such experiments but using a revolving mirror to eliminate errors caused by eye judgements.

"The experiment was tried at the Gallery in Adelaide Street.(76) The insulated wire, the total length of which was half a mile, was disposed as in fig.6. The parallel portions of the wire were each 120 feet in length, and six inches apart, and were tied to the balustrade with silk loops six inches

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(i) Sir William Watson (1715 - 1787)
Censor of the Foundling Hospital.

long. The swagging of the wire was prevented by silk cords extending across the gallery; and to keep the lengths at their proper distances apart they were tied to the cords wherever they crossed them. The ends of the wire marked 2,3,4,5, were continued to the similarly marked wires of the spark-board, fig.7, which was so fixed against the wall beneath the gallery, that the balls between which the sparks were to pass were in the same horizontal line. The striking-distance between each spark was the tenth of an inch, and the spark-board itself was three inches and a half in diameter. The conducting wire I employed was of copper, and its thickness the fifteenth of an inch.

Fig.8 represents the measuring instrument with its appendages; and fig.10 shows in a more distinct manner some of its essential parts. A B C D is a solid board of well baked mahogany one foot in length, and eight inches in breadth. E is a circular mirror of polished steel one inch in diameter, so fixed to the horizontal axle F G, that the axis of rotation is in the plane of the mirror. The pivots of the axle work in the uprights of the brass frame H I. Motion is communicated from the wheel K to the axle by means of a thread passing over grooves made on the circumferences of both; and a band passing over the wheel L, on the same axis with K, may be attached to the wheel of any machine capable of giving to it a rapid motion. In the experiments I have made with this instrument the train of wheels was so arranged that the axle carrying the mirror would have made 1800 revolutions while the wheel to which the motion was first communicated was turned round once, had there been no retardation to have been taken into consideration arising from the slipping of the bands.

M is a small Leyden jar, the inner coating of which is to be constantly supplied, through the chain N, with electricity, either positive or negative, from a machine; the bent wire proceeding from the inner coating of the jar is in immediate contact with the fixed discharger O P, and the spontaneous discharge of the jar is to be regulated by varying the distance between the two balls. The wire 1 in connection with the outer coating of the jar, and the wire 6 attached to the knob of the brass frame, are continued to the similarly numbered wires of the spark-board. When the jar is fully charged, and the arm Q, revolving with the axle, is brought opposite the knob of the discharge, the discharge of electricity, or disturbance of electric equilibrium, passes through the entire circuit, and the three sparks appear perfectly simultaneous to the eye. When the face of the mirror is level with and turned towards the spark-board, and is so adjusted as to form an angle of 45° with the horizontal plane, the eye looking directly downwards sees the reflected images of the three sparks. The plane glass or lens R is for the purpose of preventing the eye approaching too near the mirror, and for accommodating the vision of long- or short-sighted observers. The arm Q is so placed that the circuit may be completed when the mirror is in the position just described; the other arm serves merely as a counterpoise. To obviate the inaccuracy which would result from discharges taking place when the arm is in different positions with respect to the knob of the discharger, a plate of mica, S, is interposed, having a very small horizontal slit exactly opposite the axis of the discharger; this fixes within narrow limits the occurrence of the discharge, and with whatever rapidity the mirror moves, the sparks are generally within the field of view.

It was a point of essential importance to determine the angular velocity of the axle carrying the mirror. No confidence could be placed in the result obtained by calculating the train of wheels, as in such rapid motion many retarding causes might operate and render the calculation uncertain: it was necessary, therefore, to devise a means independent of these sources of error; and which should immediately indicate the ultimate velocity. Nothing appeared more likely to effect this purpose than to attach a small syren to the instrument, the plate of which should be carried round by the axle of the mirror. T is a small hollow box an inch in diameter, into which wind was conveyed through a tube placed to the aperture u. On the face of this box a number of equidistant apertures were arranged in a circle, and a disc moving before it having the same number of apertures, periodically intercepted the issuing current, and produced a sound corresponding to the frequency of the impulses. It is obvious that the number of revolutions would be ascertained by dividing the number of vibrations in a second, corresponding to the sound, by the number of apertures. I at first employed ten apertures: when the motion was slow, the sound could be easily determined; but on augmenting the velocity it became inappreciable. I then reduced the number of apertures to five, but with no better success, and ultimately to two; but the sound was then so feeble, compared with the accompanying noises, that it could not be distinctly heard.

The difficulty was at last overcome by employing the arm Q itself to produce the sound. A small slip of paper was held to it; and as at every revolution a blow was given to the paper, its rapid recurrence gave rise to a sound the pitch of which varied with the velocity of the motion. When the machinery was put in motion with the maximum velocity I employed in my experiments, the sound $G^{\#}$ was obtained, indicating 800 revolutions of the mirror in a second. I am not aware that anything can have interfered with the accuracy of this result; the same sound was heard when different pieces of paper or card were used; and on moderating the velocity, the sound descended through all the degrees of the scale below it, until distinct percussions were perceived."

The mirror revolved 800 times per second and in this time the image of a stationary point would describe 1600 circles: the elongation of a spark through half a degree (equal to one inch seen at 10 feet), would indicate that it existed for 1,152,000th part of a second. The deviation of half a degree between the two sparks (the wire being half a mile long) indicated a velocity of 576,000 miles per second. This estimated velocity was on the supposition that the electricity passed from one end of the wire to the other: if, however, the two fluids in one theory, or the disturbances of equilibrium in the other, travel simultaneously from the two ends of the wire, the two external sparks will keep their relative positions, then the middle one will be alone deflected, and the velocity measured would be only half that in the former case, i.e. 288,000 miles per second.

CHAPTER 8

ARAGO'S PROJECTED EXPERIMENT ON THE VELOCITY OF LIGHT

An experiment was projected by Arago and communicated to the Académie des Sciences on December 3rd, 1838. In this project, it was not proposed to measure the velocity of light, but simply to compare the velocities with which light moved in air, or in a liquid such as water or carbon bisulphide; it was proposed to find by experiment which of the two velocities was the greater which in turn would decide which of the two systems of propagation more accurately explained optical phenomena.

"I propose to show in this communication how it is possible(71) to decide, unequivocally, whether light be composed of little particles emanating from radiating bodies, as Newton supposes, and as the greater part of modern geometers admit; or whether it is simply the result of the undulations of a very rare and elastic medium which physicists have agreed to call ether. The system of experiments which I am about to describe will no longer permit, it seems to me, to hesitate between these two rival theories. It will decide mathematically, (I use designedly this expression); it will decide mathamatically on of the grandest and most debated questions of natural philosophy.

Besides, my communication is the fulfilling of a sort of engagement to the Academy I accepted at one of its last secret sittings.

I discussed the admirable method, by the aid of which Mr. Wheatstone attempted the solution of the problem of the velocity of electricity over metallic conductors. I had hardly terminated the enumeration of the important results obtained by that ingenious physicist, when several of our members, whose names are authority in such matters, stated that my report was far too approbative. 'In supposing it well determined, the inferior limit assigned by Mr. Wheatstone to the velocity of electricity will not have,' said one, 'any marked influence on the progress of the sciences; besides limits of the same order, and even more extensive, can be deduced indirectly from various electric or magnetic phenomena. As to the method of the revolving mirrors, it does not seem to be susceptible of application, but to the simple questions already studied by the inventor.' I tried to refute this last opinion. I believed myself that the new instrument, suitably modified, would lead to results that Mr. Wheatstone was not aware of. I already foresaw that, even in supposing it enclosed in the narrow limits of a small room, it could serve to measure the comparative velocities of light moving through air and through a liquid. I was not slow in learning, and without having hardly the right to be astonished or to complain that my assertion had been received with incredulity. Nevertheless, I intend to vindicate it to-day in all its parts.

Principle of the method: Let a ray of light fall upon a plane polished mirror; it will be reflected, as everyone knows, in forming with the surface of the mirror an angle of reflection exactly equal to the angle of incidence.

Let us now suppose that the mirror turns through an arc a around the point of its surface from which the reflection takes place. If this motion, for example, increases by the quantity a, the original angle of incidence, it will diminish as much the original angle of reflection. The latter will, therefore, after the displacement of the mirror, be smaller than the first by the quantity 2a: thus it must be increased to 2a to render it equal to the new angle of incidence; hence that angle increased 2a will give the direction of the reflected ray in the second position or the mirror; and thus the incident ray remaining the same, an angular motion a of the mirror occasions a double angular motion in the reflected ray.

This mode of reasoning applies as well to the case where the motion of the mirror, acting in a contrary direction, would diminish the first angle of incidence. The principle is, therefore, general; and it is also that of all reflecting nautical instruments.

The reflection from the plane mirrors can serve to project the luminous rays in all parts of space, without, however, altering the relative positions; two rays parallel before reflection will be parallel after their reflection; those at first inclined to each other 1 minute, 10 minutes, or 20 minutes &c., will form precisely the same angle after the reflection has deviated them.

Instead of a single ray, let us consider two horizontal rays setting out from two neighbouring points situate in the same vertical. Admit that they strike on two points of the median line (also vertical) of a plane vertical mirror. Suppose that this mirror revolves on itself uniformly and in a continuous manner around a vertical axis whose prolongation coincides with the median line just mentioned, the direction in which the two horizontal lines will be reflected will depend evidently upon the moment they may reach the mirror, since we have supposed that it turns. If the two rays have set out simultaneously from the two continuous radiating points, they will also reach simultaneously the mirror. Their reflection will take place at the same instant; consequently in the same position of the turning surface: consequently as if that surface was stationary with respect to them. Therefore their primitive parallelism will not be changed.

In order that the rays which primitively were parallel may diverge after their reflection, it is necessary that one of them should arrive at the mirror later than the other.

It is necessary that in its course from the radiating point to the reflecting and turning surface, the velocity of the ray should be accelerated, or what will be precisely the same thing, it is necessary (the velocity of the first ray remaining constant) that that of the second should experience a dimunution. It is necessary, finally, that the two rays should be reflected one after the other; and, consequently, from two distinct positions of the mirror, forming with each other a sensible angle.

According to the theory of emission, light moves in water notably faster than in air. According to the wave theory, it is precisely the opposite which takes place: the light moves faster in air than in water. Suppose that one of the rays (the upper ray, for example) has to traverse a tube filled with water before it strikes the mirror. If the theory of emission be true, the upper ray will be accelerated in its progress; it will reach the mirror first; it will be reflected before the lower ray; it will make with it a certain angle, and the direction of the deviation will be such that the lower ray will appear in advance of the other, that it will appear to have been deviated more by the turning mirror.

Circumstances remaining the same, let us admit for a moment the truth of the wave system. The tube of water will retard the progress of the upper ray; the ray will arrive at the reflecting mirror after the lower ray; it will be reflected not the first, as in the former case, but the second in order, and from a position of the polished reflecting face in advance of the position that it had when it reflected the upper ray a moment before; these two rays will make with each other the same angle as in the other hypothesis, except (and we should well remark it) the deviation will take place precisely in an opposite direction; the upper ray will now be in advance, always indicating thus the direction in which the mirror revolves.

To recapitulate: two radiating points, placed near each other on the same vertical line, flash instantaneously before a revolving mirror. The rays from the upper point cannot reach the mirror until after traversing a tube filled with water; the rays from the second point arrive at the mirror without meeting in their course any other medium than the air. To be more definite, we will suppose that the mirror, seen from the position the observer occupies, turns from the right to the left. Well, if the theory of emission be true; if light be material, the upper point will appear to the left of the lower point. It will appear to its right, on the contrary, if light results from the vibrations of an ethereal medium.

Instead of two isolated radiating points, suppose that we instantaneously present to the mirror a vertical luminous line. The image of the upper part of this line will be formed by rays which have traversed the water; the image of the lower part will result from the rays which have throughout their whole course traversed the air. In the revolving mirror the image of the single line will appear broken; it will be composed of two vertical luminous lines, of two lines, which will not be prolongations of each other.

The upper rectilinear image, is it behind the one below? Does it appear to its left?

Light is a body.

Does the contrary take place? The upper image, does it show itself to the right?

Light is an undulation.

All that precedes is theoretically, or rather speculatively exact. Now, (and here is the delicate point), it remains to prove that, notwithstanding the prodigious velocity of light, that notwithstanding a velocity of 190,000 miles a second, that notwithstanding the small length that we will be obliged to give to the tube filled with liquid, that notwithstanding the limited velocities of rotation that the mirrors will have, the comparative deviations of the two images, towards the right or towards the left, of which I have demonstrated the existence, will be perceptible in our instruments."

Arago then went into the most minute details of all the parts of the experiment and then terminated as follows: "Suppose in the experiment that I propose to execute we make use of electric sparks, or of lights successively screened and unscreened by the use of rotating disks, as their emissions should only last during a few thousandths of a second, it may happen that an observer, looking in the mirror from a given direction, and with a telescope of limited field, will only by chance perceive the light. To this I immediately reply that in renewing very often the apparitions of light - every second, for example - that if, instead of a single mirror, we rotate a vertical prism of eight or of ten facets, that with the concurrence of several observers, placed in different directions, and each with his telescope, we cannot fail to have numerous and clear apparitions of the reflected rays. But these are details on which I shall not dwell today. I will reserve for another communication the exposition of the system of experiments in which we will render sensible, and in which we will measure, to a certain degree, the absolute velocity of light without having recourse to celestial phenomena."

CHAPTER 9

FOUCAULT'S EXPERIMENTS

Arago, some eleven years after first proposing his method for the determination of the velocity of light, requested the attention of members of the Académie des Sciences (April 29th, 1850) to his suggestions.

"That communication established that, according to readily admitted hypotheses as to the angular deviations susceptible of being observed in an ordinary telescope, it would not be impossible to determine the comparative velocity of light in bisulphide of carbon and in air, without having recourse to an extreme length of tube, or to a mirror, making more than 1,000 turns in a second. But the mirror which M. Wheatstone used made only 800 turns in the same interval of time.

It was evident that in this method of observation, and for a given angular deviation, the length of tube containing the liquid ought to be so much the shorter, as the movement of rotation of the mirror is more rapid. This is the reason I propose to add to this deviating motion of rotation, which cannot surpass certain limits, a combination of several revolving mirrors.

The two rays (one having traversed the liquid, the other the air) strike the first mirror, and form a certain angle; this angle is doubled when the rays fall upon a second mirror turning in the same direction with the same velocity; the angle is tripled if these rays are reflected from a third revolving mirror, and so on. We can thus, by the multiplication of the revolving mirrors, arrive at the same result given by a single mirror turning with a double, triple, &c., velocity of that which it is possible to obtain with the certainty of not destroying the teeth of the wheel, or of overheating the axis.

My friend, M.Bréguet, jr., undertook to accomplish this end by means of a mechanism, in which the communication of motion was given by wheel-work. He executed a special arrangement of cog-wheels, the invention of which is due to White. At one of the former industrial exhibitions could be seen the system of these movements.

In observing the image reflected by the mirror attached to the third piece of wheel-work, the effects observed should be identical with those which should be given by a revolving mirror making 3,000 turns per second. From this moment the success of the projected experiment was placed beyond doubt. It was only to be regretted that, by the three successive reflections from three different mirrors, the light necessarily experienced a considerable loss in intensity. It was, therefore, desirable to arrive at the result by a single reflection; and it is to this that the experiments which I am going to relate seem to lead.

In his investigations into the causes which prevented us revolving a mirror more than 1,000 turns per second, M.Bréguet proposed to relieve the last axis of the weight of the mirror with which it was charged, to turn the axis alone; and he succeeded, not without surprise, in giving to this axis 8,000 per second. The obstacle which prevented us giving the same axis, when it carried the mirror, a velocity greater than 1,000 turns per second appeared evident. It was, one would think, the resistance of the air. I myself thought of the existence of that cause, and all our thoughts were directed to the means of revolving the mirror in a vacuum. We immediately constructed a metallic receiver destined to hold the revolving apparatus. This receiver had several apertures, of which one was to give entrance to the rays of light after having traversed the two columns of air and of liquid. Before the others were to be the objectives of the telescopes, with which to observe the rays reflected by the rotating mirror, the necessary communications were established by means of stuffing-boxes between the apparatus and the driving weight. A special tube put the interior of the receiver in communication with an air-pump.

All was arranged and placed upon a stone column in the meridian room of the observatory. It only remained to make the observation. ... The mirror, contradicting all our anticipations, turned hardly any faster in the vacuum than in the air. This circumstance again showed the truth of the proverb, 'Le mieux est l'ennemi du bien:' (Better is the enemy of good enough). It was necessary to think of returning to the first apparatus composed of three pieces of wheel-work and of three separate mirrors, the apparatus which I had given up only to obtain a greater intensity in the reflected ray.

I was convinced of the necessity of going back to the first method of experiment at the time when my enfeebled sight would not allow me to undertake it. My pretensions, therefore, ought to be limited to having posed the problem, and of having given the certain means of solving it. These means may, during its accomplishment, experience modifications, which will render them applicable, with more or less facility, without changing their essential character.

As to myself, I have delayed a long time the realization of that which I had announced that has been owing in large part to the obligations which M.Bréguet, my collaborator, had contracted with the government for the supply of electric telegraphs, and to the desire that I had to operate, as I have already said, with a mirror making 8,000 turns per second.

Probably, also, I may remain content with the thought that no one will execute, without my authorization, an experiment founded on principles and methods of execution which I have exposed to the world in their most minute details.

M.Bessel,(i) after my publication in the Compte Rendu, announced to me that he had thought of a modification of my apparatus composed of three successive pieces of wheel-work, each carrying a mirror. He receives the image reflected by the first rotating mirror not upon a second revolving mirror, but upon a fixed mirror, which sends the ray back to the first mirror. After this second reflection, the rays fall again upon a fixed mirror, from which they are reflected a third time to the turning mirror, &c. It is after the last reflection from the single revolving mirror

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(i) Friedrich Wilhelm Bessel (1784 - 1846)
Director of the Königsberg Observatory.

that M.Bessel proposes to measure the angular departure of the ray. This method, more simple than the one I proposed, in so far as it required only one piece of wheelwork, had the very grave inconvenience of diminishing much more light, since he had more reflections from the mirrors than in the other method.

M.Silbermann,(i) without knowledge of the prior communication of M.Bessel, made me a proposition similar to that of the illustrious observer of Koenisberg.

Things were in this state when M.Fizeau determined by his so ingenious experiment the velocity of light in the atmosphere. That experiment was not indicated in my memoir. The author, therefore, had the right to make it without exposing himself to the slightest reproach for want of due consideration of the rights of others.

As to the experiment on the comparative velocity of light in a liquid and in air, the author wrote to me: 'I have not yet made any attempt in that direction, and I will not occupy myself with it but on your formal invitation.'

This loyal reserve could only add to the esteem with which the character and the works of M.Fizeau had inspired me, and I willingly authorized M.Breguet to lend him one or several of my rotating mirrors.

M.Foucault, whose inventive genius is well known to the Academy, came also to inform me of the desire he had to

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(i) Jean Thiebaut Silbermann (1806 - 1865)
Technician at the Conservatory of Crafts.

submit to the test of experiment a modification which he had devised in my apparatus.

I can only, in the present condition of my sight, accompany with my good wishes the experimenters who desire to follow my ideas, and to add a new proof in favor of the wave system to that which I have deduced from a phenomenon of interference too well known to physicists to need recalling here."

The above communication was no sooner printed when the Academy received a communication (May 6th) from J.B.Foucault. Here he announced that he had realized with complete success the projected (72) experiment of Arago;(70) he further announced the modifications he had made to the original arrangement which had allowed him to arrive at the important result of the truth of the wave theory of that of emission.

Two important modifications had been made by Foucault. The first of these was to make the execution of the experiment much easier. In the original proposals the light was to be transmitted from a luminous line shining only for an excessively short time; and that one beam of that light, having travelled in air, and the other beam in a liquid, were then required to fall on a rapidly rotating mirror and that finally, having been reflected from this mirror would arrive on an observing telescope. Now the direction of the reflected ray depended essentially upon the position occupied by the mirror at the instant the reflection took place, and as the motion of the mirror and the reflection of the light from its surface were independent of each other, it is only by chance that the mirror would be found in a specified position; the observer could not know in what direction he should place the telescope in order to receive the reflected light. To reduce this difficulty Arago supposed that the observer, being stationed anywhere in the space the reflected rays could reach, and having directed the telescope towards the rotating mirror, would repeat time and again the emission of the light onto the rotating mirror, so that at least some of the reflected rays would fall on the objective of the telescope. Arago realized the extremely low chance of a reflected ray being in the correct place to be received by the telescope and as such spoke of substituting for the single mirror a vertical prism of eight or ten facets, and employing at (70) the same time several observers placed in different positions, each being provided with a telescope. However, Foucault modified the instrument so that the reflected rays left the rotating mirror in a predetermined direction so that the observer could position himself to receive all the reflected rays. This was accomplished by allowing the light to fall on the rotating mirror in a continuous manner so that it was reflected in all directions around its axis; in one of these directions the reflected light met a fixed mirror on which it fell perpendicularly and which caused it to return over its path and sending it again to the rotating mirror; there it underwent a new reflection which set it to the place it set out from. Therefore the observer could place himself near to the object to receive the reflected rays which were turned to one side by means of a transparent glass plate inclined at 45°.

"A direct ray of light, penetrating a square opening, meets, very near the aperture, a reticule of eleven vertical wires of platinum to the millimetre, (.03937 of an inch); thence it passes towards an excellent achromatic lens of long focus, placed at a distance from the reticule less than double the principal focal distance. The image of the reticule of greater or less dimensions would be formed on the other side, but, after having traversed the lens, the pencil, before its convergence at the focus, falls upon the surface of the revolving mirror, and, animated with an angular motion double that of the mirror, it forms in space an image of the vertical wires, which is displaced with great rapidity. During a small portion of its revolution this image meets the surface of a concave mirror, whose centre of curvature coincides with the centre of figure and the axis of rotation of the revolving mirror, and, during all the time it passes over its surface, the light which has concurred to form it retraces its path and falls upon the reticule itself, producing there its image, equal to it in size. In order to observe this image without shutting out the original beam, we place obliquely to the beam of light, near the reticule, between it and the object glass, a glass plate, and we observe with a powerful ocular the image thrown to one side. The mirror, in revolving, causes this image to reappear at each revolution, and, if the velocity of the motion of rotation is uniform, it remains immovable in space. For velocities which do not surpass thirty turns per second, its successive appartitions are more or less distinct, but over thirty turns give a persistence to the impressions on the eye, and the image appears absolutely fixed.

It is easy to demonstrate that the mirror, in revolving with greater or less rapidity, will displace this image in the direction of the motion of rotation. In fact, the light which passes between the wires of the reticule does not return to the wires until it has received from the revolving mirror two reflections, separated by the time it takes to run over double the path from the revolving mirror to the concave mirror. But, if the mirror revolves very fast, this time taken by the light to go and come back, even over the small length of 4 metres, (13.12 feet), cannot be regarded as inappreciable, and the mirror has had the time to change sensibly its position, which is shown by the change of position in the image formed by the returning beam.

Rigorously speaking, this effect takes place as soon as the mirror turns, even slowly; but it cannot be observed until the mirror has acquired a certain velocity, and only when we employ certain precautions in the experiment. All my efforts have tended to render this deviation as apparent as possible.

The principal obstacle to surmount is that, in so complicated a path, the light cannot converge to the focus in a neat, clear image. The deadening which the pencil experiences, in being reflected twice from a turning mirror of small surface, necessarily destroys the nicety of the image, and produces in its contour an unavoidable mistiness. It is

for this reason that we have chosen for source of light the equi-distant linear spaces between the wires of a very fine net. Although the image obtained is never clear, yet it is presented under the form of a system of white and black stripes, similar to colourless diffraction badds, each having a well defined maximum and a minimum of light.
Like the wires of the net, these luminous or obscure spaces are distant from each other one-eleventh of a millimetre, (a millimetre equals .03937 of an inch), and if, to observe them, we place in the occular a micrometer divided into tenths of a millimetre, the two systems of lines will operate, by their relative displacements, as a vernier, and will permit us to measure in the image, with certainty, a displacement of the one hundredth of a millimetre.
After the known velocity of light, with an objective of 2 metres (6.56 feet), we find that we need not give to the mirror an extreme velocity (six or eight hundred revolutions per second) in order to obtain displacements of two and three-tenths of a millimetre.
Such is the construction of the optical apparatus which has permitted me to show the successive propagation of luminous rays. My first attempts succeeded in the air with a mirror which made only twenty-five to thirty turns per second, the length of the double path being four metres.

In order to make the experiment with water, we have only to place between the revolving mirror and the concave mirror a column of this liquid, held between two parallel plates of glass in a conical metallic tube, varnished inside with copal, so that the water would remain clear, to take the necessary precautions that the terminal plates were not strained in their frames, and to obviate the inconvenience of the change of focus by the interposition of a liquid layer of 3 metres (9.84 feet) thickness, having parallel surfaces. In the end we succeeded in easily obtaining, with the feeble and green ray which has traversed the water, an image as distinct as that which is formed without the interposition of the liquid. Therefore it is required but to turn the mirror and to measure with precision its velocity of rotation if we desire to deduce the absolute velocities in air and in water, or to operate simultaneously on these two media if we wish to know only the character and difference of these velocities." (78)

The second modification introduced by Foucault was concerned with means by which the mirror could have extremely rapid rotation which could last for a sufficient period of time. He introduced pressurised steam from a boiler to drive a small turbine which was fitted to the axis carrying the mirror. Foucault announced the results he had already obtained as follows:

"In confining myself to the determinations of the velocity (78) (of the mirror) by the sound, (produced by the action of the steam on the little turbine), as I have already proved by two successive observations that the deviation of the image after the passage of light through the air is less than after its passage through the water, I have also made another confirmatory experiment, which consists in observing the image formed in part by the light which has traversed the air, and in part by the light which has traversed the water. During low velocities the stripes of the compound image were sensibly the continuations of each other, and, by the acceleration of the moment of rotation, the image is carried to one side, and the stripes are broken at the boundary line, at the junction of the air image with the water image, the stripes of the latter being in advance in the direction of the common deviation. Moreover, in taking into account the lengths of air and water traversed, the deviations were seen to be proportional to the indices of refraction. These results demonstrate a velocity of light less in water than in air, and fully confirm, according to the views of Arago, the indications of the theory of undulations."

The apparatus which allowed Foucault to determine that light moves faster in air than in water was not designed solely for that comparative experiment; its principal aim was to measure the absolute value of the velocity of light. The apparatus was such that the reflected rays gave rise to the formation of a permanent image which was displaced transversely by a distance which increased the more rapidly the rotating mirror revolved. This distance indicated the amount the mirror turned during two successive reflections of the light from its surface in going and in returning - that is while the light had moved over twice the distance of the rotating mirror from the fixed mirror; therefore if the exact velocity of rotation of the mirror was known one could deduce the time interval between the two successive reflections i.e. the time taken for the light to make the double journey from the revolving to the fixed mirror and hence the value for the velocity of light.

A saw-toothed disc was made to have a uniform rotation by means of a wheel mechanism. This disc made exactly one revolution per second. The disc had 400 teeth; so that the time taken for one tooth to take the place of its neighbour was exactly 1/400 second. The disc was so placed that its edge cut the plane of the field of view of a microscope which was used to observe the return image from the mirror. Should the field be constantly illuminated then the teeth of the disc would appear to pass before the observer with the velocity of their motion. However, the light only enters the microscope field at the instant a reflection occurs from the rotating mirror; the microscope field and the edge of the disc are only illuminated by successive flashes of light, where the flashes are governed by the rotation of the mirror. Since the mirror made exactly 400 turns per second, then the time interval between two successive illuminations of the microscope field was exactly equal to the time taken by each tooth to take the place of its neighbour. Thus at the moment of successive illuminations a tooth of the disc was always seen at the same position in the field of view, and hence the disc appeared stationary. Should the disc revolve at less than 400 turns per second; then whilst it makes one revolution each tooth of the disc has to travel a little further to take the place of the preceding tooth. So at the moment of successive illuminations the teeth which replace one another do not appear at exactly the same point in the field of view. They appear a little in advance in the direction of motion of the disc, so that the disc appears to have a slow movement of rotation in the direction of its real motion. On the other hand should the mirror rotate at more than 400 turns per second, the teeth of the disc at the moment of illumination appear more and more behind a fixed position, and the disc appears to turn in a direction the reverse of its real motion. Thus once the speed of rotation of the disc has been adjusted so that its edge appears stationary in the field of view of the microscope then one is certain that the mirror is making one revolution whilst the edge of the disc progresses one division, and consequently whilst the mirror makes exactly 400 revolutions per second. Foucault further used compressed air instead of steam for driving the small turbine attached to the axis of the mirror. The air being provided by a constant pressure blower designed by Cavaillé-Coll. He also increased the length of the path of light between the two reflections from the revolving mirror from 4 metres to 20 metres (13 feet 1.48" to 65 feet 7.4") by means of successive reflections from intermediate fixed mirrors.

Foucault by September 1862 found the velocity of light in air to be 298,000 kilometres (185,177 miles) per second which was below the result arrived at by Struve (308,000 kilometres or 191,391 miles) per second from the value of aberration (20.45 seconds).

Foucault was indebted to Froment for devising the clockwork mechanism which gave the disc a uniform speed of rotation of exactly one revolution per second.

In the final form of the apparatus a beam of solar light was

reflected horizontally from a heliostat through the aperture S. The sight used was a microscope scale consisting of fine graduations 1/10 m.m. from each other. Thisscale was positioned at Sand its image was viewed at a in the field of the observing microscope. The observer thus saw the displacement of the image of this scale. The revolving mirror was a piece of glass silvered and polished on one face being supported on a strong ring frame having a diameter of 14 m.m. The radius of curvature of the fixed mirror M was 4 metres and by the use of 5 fixed mirrors the distance D (R to M) was increased to 20 metres. The lens L, having a focal length of 1.9 metres was placed between the revolving mirror and the first fixed mirror (see lower sketch) and not between the revolving mirror and the aperture. The observed displacement of the scale was 0.7 m.m. giving rise to the final result for the velocity shown above.
It was shown that the velocity of light in air (ν) was

$$\nu = \frac{8\pi n a D^2}{x(b + D)}$$

where a = distance of the lens from the slit

b = distance ofthe lens from the revolving mirror

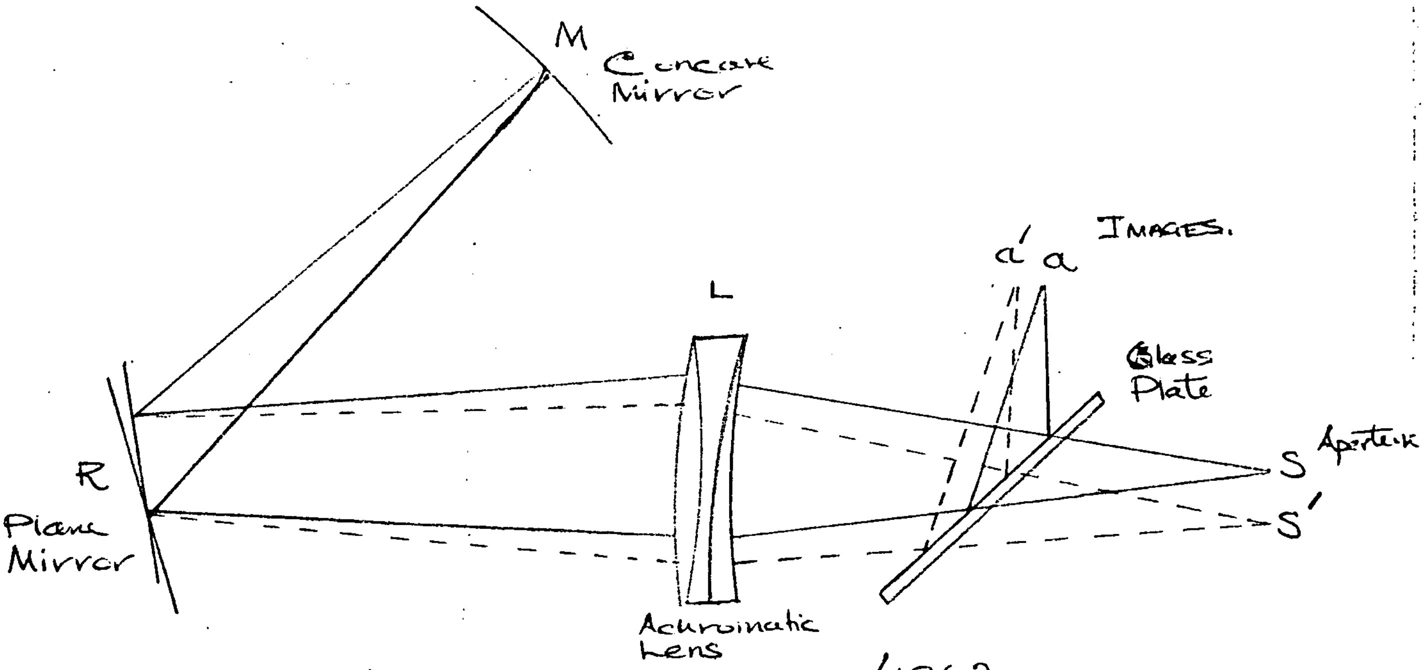
$x = aa'$ the distance of the image displacement

n = number of revolutions per second of the mirror

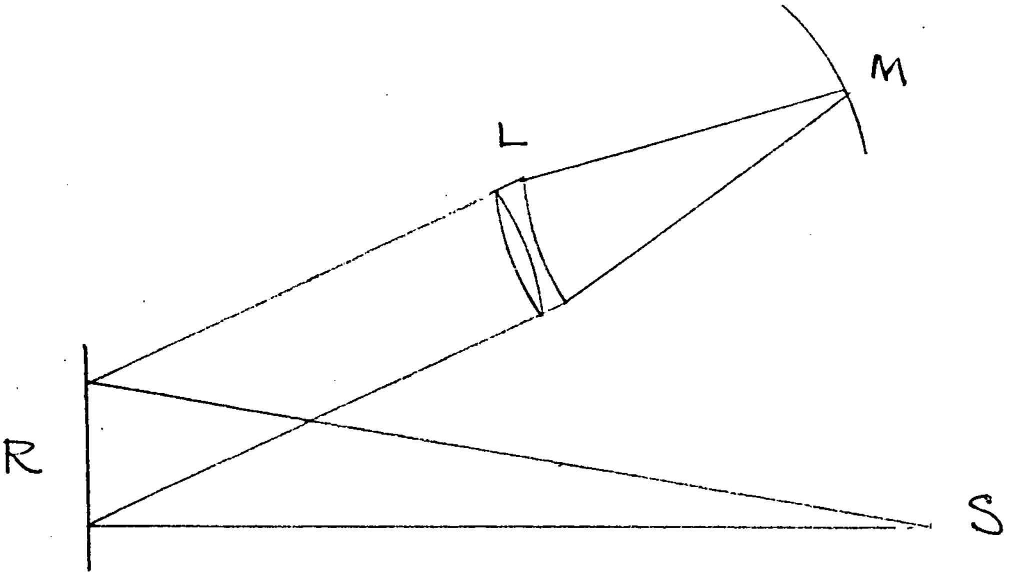
D = the distance R to M

Even in the final form of the apparatus the distance D was still small being 20 metres with the use of 5 fixed mirrors. Thus it was not ppssible to have a large angular deviation of the image and there was a serious loss of intensity due to the several reflections. Further the angular deviation of the return image, for a given speed of the revolving mirror, increases with the distance D, and for a given angular deviation the displacement of the image is proportional to the distance between the source and the revolving mirror (radius). Hence for a large displacement of the image the distance between the mirrors, the radius, and the speed should each be as large as possible. However the second condition is in conflict with the first, for the slit and the fixed mirror must be situated in the conjugate foci of the lens L.

When the lens is placed between the revolving mirror and the slit (upper sketch), the quantity of light returned by M to R varies inversely as the distance D. This quantity is further reduced by atmospheric vibration, diffusion and absorption. On the other hand, when the lens is placed between the revolving mirror and the fixed mirror (lower sketch) it can be seen that if R and M are in conjugate foci of L, then the light reflected from R will fall upon M as long as the axis of the reflected beam falls upon the lens, however great the distance D may be. This is an impossible arrangement for it is the slit S and not the mirror R that must be in the conjugate focus of M; nevertheless it may be approximated to by bringing the slit close to the revolving mirror.



M.L. FOUCAULT'S EXPERIMENTS 1850/1862



ARTICLE PUBLISHED BY M. FOUCAULT, IN THE "JOURNAL DES DEBATS," ON THE REALIZATION OF THE EXPERIMENT OF ARAGO.

(Number of Tuesday, April 30th, 1850)

TO THE EDITOR - Sir: I will not wait for the expiration of the fortnight to give you an account of what most occupied the Academy of Sciences during their meeting of yesterday. All knew that M.Arago was to continue the account of his beautiful researches of polarization and of photometry. The attendance was large, and the Academy recorded at its session a foreign associate and two corresponding members - Mr. David Brewster, Lord Brougham, and M de la Rive, of Geneva. But what was not expected was, that M.Arago recalled attention to one of the most beautiful projected experiments that the genius of a savant has ever produced, and he declared that, after having conceived it, he had left to the young generation the care and the honor of performing it. This experiment has more than once occupied the attention of the Academy; it proposes to decide, by means of a revolving mirror, whether light moves faster in air than in water, and to seek, in the probable result of this experiment, the confirmation of the theory at present adopted to explain all optical phenomena. You may judge, sir, of the emotion with which I heard this generous declaration; I, who for several days had in my hands the experimental solution of this great problem! Nevertheless I thought it proper to postpone to the next meeting the reading of the paper in which I have recorded my results. In the mean time permit me, sir, to announce, in a few words, the results which I have observed.

Light employs more time to run over the same path in water than in air, and the time which it takes to traverse these two different media is shown by the deviation of the ray which is reflected at a given moment from a mirror revolving with a great velocity. All things remaining equal, the deviations were found to be proportional to the indices of refraction of air and of water. It is not possible to entertain the least doubt as to the reality of these results; they have been obtained by two different methods. The two deviations were first observed successively and found unequal for the same velocity of the mirror. They were then observed simultaneously, which rendered the observation still more certain.

Permit me to limit myself to the rather technical expression of these new results. When the columns of the Journal are unoccupied I shall enter into such developments as will render these propositions more intelligible to your readers.

Receive, sir, &c., &c.,

LEON FOUCAULT.

JOURNAL DES DEBATS

(Number of Saturday, May 4th, 1850)

We published last Tuesday a letter of M.Foucault announcing the success of an optical experiment originally devised by M.Arago, and which, in giving the relative velocities of light in air and in water, accomplished the overthrow of the emission theory in favor of the theory of undulation. The sun having appeared during the few days past, they have been able to repeat several times the experiment in presence of a certain number of French and foreign savants, and already the methods which have insured success are generally known to the public. In waiting for the communication which will be given at the meeting of the Academy next Monday, we will concisely indicate the fundamental parts of the experiment.

A beam of sunlight reflected from a heliostat in a fixed direction penetrates horizontally a dark room; it first passes through a small opening of 2 millimetres (.0797 of an inch) square, then a reticule extended behind this opening, and formed of eleven platinum to the millimetre. Passing through this reticule, the beam of light meets an objective of a focus of two metres placed at a distance from the reticule less than the double of its principal focal length, and it tends to form beyond a magnified image of the reticule. But before the formation of this image the converging pencil is reflected from a small mirror which, capable of rapidly revolving around a vertical axis, we will call the revolving mirror. After its reflection, the converging beam will form an image before the mirror at a distance of 4 metres, and when the mirror turns, this image moves in space, describing circles double of the number of the turns of the mirror supposed to reflect from its two faces. In sweeping through space this image meets a concave mirror whose centre of curvature corresponds with the centre of figure of the revolving mirror and with the centre of the axis of rotation; it thence results that during all the time that the image of the reticule falls on the concave mirror the light is thrown back to its point of departure by the revolving mirror and returns to form at the reticule its image of natural size. This image coincides exactly with the reticule, when the revolving mirror being at rest is placed at the proper angle of incidence; but as soon as it moves, the image is deviated and deflected in the direction of the morion. In order conveniently to observe this deviation we place obliquely to the path of the entering beam a glass plate which throws this image to one side. This image appears like colorless diffraction bands, striped with vertical lines, distant from each other the eleventh of a millimetre; they are examined with a powerful ocular, having at its focus a micrometer divided into tenths of a millimetre. The stripes of the image bear the relation to the divisions of the micrometer as a scale to its vernier, so that deviations to the one-hundredth of a millimetre can be read off. Calculation shows that a deviation should be observed for thirty turns of the mirror in a second; and in fact that it is seen for that velocity; for greater velocities the deviation is measurable. If we wish to measure the velocity of light in water we place between the revolving mirror and the concave mirror a tube three metres long, filled with perfectly clear water, and its ends closed by plates of glass of parallel surfaces. All things remaining the same, the deviation observed when we interpose the tube of water is always greater than when this tube is not placed between the revolving and the concave mirror. But it is better, to operate simultaneously in the air and in the water, to employ two comcave mirrors of the same radius of curvature and both facing the revolving mirror;

one destined to receive and send the rays through the water, and the other through the air only. The mirror in revolving causes the two images, corresponding to the two reflections, alternately to appear, but the rapid succession of their apparitions makes them appear superposed; to distinguish them from each other we cover a good part of the height of the concave mirror which reflects the image through the air, which reduces the light of the brighter image; the remainder of the field is occupied by the image which has traversed the water. The vertical stripes of these two images should correspond, and indeed do correspond, for low velocities of the revolving mirror. But as the velocity of rotation increases, the two rays are deflected unequally, the stripes break at the line of junction, and the deviation is greater for the dull and green image which has traversed the water than for the luminous and white image which has progressed only through the air. This last experiment, although difficult to repeat with apparatus improvised in a hurry, has the advantage to appeal directly to the eyes; it has been repeated before several distinguished savants, who, in reference to it, no longer retain the least doubt.

To give to the mirrors rapid and constant velocities M. Foucault uses a small steam-turbine, which was constructed with the greatest care by M.Froment. We cannot at present enter into the details of its construction. It will be noticed hereafter, as well as the applications of this new method of experimenting, when the paper in which it is described has been presented to the Academy of Sciences.

CHAPTER 10

EXPERIMENTS OF A. CORNU 1874/75

His first series of experiments were conducted in 1872 between (79) the École Polytechnique in Paris and Mount Valerien covering a distance of 10,310 metres (6½ miles). The apparatus used was that basically tried by Fizeau but with some additional improvements. Cornu considered the main difficulty to be the measurement of the angular motion or velocity of the wheel to which the velocity of light was to be compared. The simplest solution was to give a uniform motion to the wheel as performed by Fizeau. Cornu, however, considered such a uniform motion not obtainable in practice and so he developed an electric recording apparatus to register the continuous increase of motion of the wheel. Using this recorder it was not necessary to have the wheel turning at an exact uniform velocity and hence the observer was able to know the exact moment when the wheel was revolving at the required velocity by means of an electrical signal from the recorder.

A second improvement was the substitution of a pair of observations of the return rays, when reduced to a pre-determined low intensity, for the single observation of a total extinction.

The experiment gave rise to a velocity of light of 298,000 kilometres per second with a probable error of 1%.

In 1874 acting under the orders of the Council of the Paris Observatory, Cornu made a further series of determinations of the velocity of light between the Paris Observatory and the tower of Montlhéry (22,910 metres, 14½ miles). The two main parts of the apparatus were placed at an increased distance apart and improved sites for the optical and mechanical parts chosen. The first part was provided with a telescope (0.38 m aperture and 9m focal length) and this together with the toothed wheel, recorder and clocks etc. were placed in a specially constructed hut at the Observatory. The reflection telescope in a cast iron tube was placed on the top of the tower of Montlhéry.

A total of 508 pairs of observations were made in 1874 giving rise to an average value of 300,400 km/sec.

The main problem with the method used was that it was not possible to measure accurately the brightness of the image. The eclipses did not occur suddenly at well-marked speeds but were gradual so that it was difficult to say precisely when they occurred.

CHAPTER 11

THE DETERMINATION BY YOUNG AND FORBES (1880-81)

A series of experiments to determine the absolute velocity of white light was performed by Young(i) and Forbes(ii) between 1880 and 1881.(80) They intended that each observation should give an accurate measurement rather than rely upon the mean of a number of experiments. They considered that the chief importance of a determination of the velocity of light was that it gave the means of determining the solar parallax by combining the result with the constant of aberration determined by astronomers. Further they felt that the experiments were of interest due to Clerk Maxwell's theories on the propagation of light being an electro-magnetic phenomenon, and its velocity should be the same as that of the propagation of an electro-magnetic displacement. They also believed that the different colours of white light did not travel with the same velocity, but that the more refrangible rays travelled more rapidly through a vacuum, and that this difference was quite marked and so could be determined by independent tests.

In general the theory of the method resembled the experiments of Fizeau whereby the velocity of light (V) was determined by $V = 4mND$

where m = the number of teeth on the wheel

N = number of revolutions a second at the time of the first eclipse

D = distance between the toothed wheel and the distant reflector

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(i) James Radford Young (1811 - 1883) Industrial Chemist and Philanthropist.

(ii) George Forbes (1849 - 1936) Engineer and Professor of Physics at Glasgow.

Young and Forbes modified the apparatus by replacing the one distant reflector by two, nearly in the same line, but one of them being at a greater distance than the other and a little to one side of it. With the most distant reflector being indicated as A and the other reflector B then the light reflected from A should be eclipsed with a slower revolution of the toothed wheel than that from B; because the number of revolutions required is N, you have $N = \frac{V}{4mD}$.

However D_A (the distance to A) $>$ D_B (the distance to B);

thus N_A (speed of revolution) (producing the first) (eclipse at A.) $<$ N_B (the speed of revolution) (producing the first) (eclipse at B.)

After the light from A has been eclipsed it starts to increase in brightness, whilst that from B is still diminishing, and the method of the experiments was to determine the speed of revolution when the two lights appeared to be of equal brightness. When the second and third eclipses were considered the difference in speed required for an eclipse increased and they thought that at a certain speed the speed of light from A reached a maximum at the time when that from B was at a minimum and vice versa.

The superiority of this method over that of Fizeau seemed to be that instead of having to determine the instant at which a light disappears they had only to determine the instant at which two lights seemed to be of equal brightness.

They showed by mathematical theory that

$$V = \frac{2m\ (n = n^1)\ D_B}{r} \quad 1 - \frac{\delta}{g + p} + \frac{\delta^2}{(g+p)^2} - \ldots\ldots$$

where V = velocity of light

m =. number of teeth in the wheel

n = number of revolutions per second made by the wheel

D = distance of the wheel to the distant reflector

r = time taken by the light to perform the double journey

p = phase

$$g = \frac{r + 1}{r} - \delta$$

Apparatus

The general optical arrangement was that devised by Fizeau with modifications by Cornu.

The observing telescope was pointed towards the distant reflectors. The revolving toothed wheel being placed at its focus with a diagonally inclined piece of unsilvered glass.

The reflector consisted of a telescope pointing towards the observing telescope, but instead of an eyepiece, it had at its principal focus a silver reflector.

As the wheel rotates so that at least ten teeth pass per second the observer would see (as the speed increased) the spot of light disappear, then re-appear, attain its full brightness, diminish, disappear, reappear etc. passing through similar phases with perfect regularity.

A further important modification was used in that a method was devised for determining at any instant the velocity of rotation of the toothed wheel. This method was based on that suggested by Cornu whereby an electrical connection was made between the toothed wheel mechanism and a chronograph so that a mark was made every 100 revolutions of the toothed wheel. At the same time a clock marked seconds and through a vibrating spring mechanism, tenths of a second were marked. The observer could make a fourth mark the instant he wished the velocity to be determined.

The apparatus could be considered to consist of seven main parts viz.: 1) the telescope, 2) the reflectors, 3) the toothed wheel, 4) the clock, 5) the chronometer, 6) the dynamo-electric device and 7) the lamp.

The telescope had a 5 inch achromatic object-glass and a focal length of 7 feet. A Bohnenberger's(i) eye-piece was employed, consisting of an erecting eyepiece with a piece of plain glass in front of the field lens and inclined to the axis of the telescope at an angle of 45°.

Certain optical difficulties occurred mainly in the illumination of the field of view and four major improvements were introduced.

IMPROVEMENTS

1) A circle of black velvet was attached to the centre of the object-glass on the inside to reduce reflection from the centre of the object-glass.

2) On using powerful lights a blaze was reflected from the toothed wheel. To reduce the effect

a) The toothed wheel was smoked

b) A highly polished wheel was bevelled and by tilting the revolving mechanism, the reflected light was absorbed in the blackened adaptor.

3) A silvered reflector was substituted for the diagonal glass reflector which allowed one-half of the light to pass through. This arrangement doubled the intensity of the light and due to the darkness of the field its superiority over the glass reflector was enormous.

(i) Johann Gottlieb Friedrich Von Bohnenberger (1765 - 1831) Professor at Tübingen.

4) To minimise slight illumination from the toothed wheel a strip of metal with holes was placed in the secondary focus of the eye-piece. When the apparatus was fully aligned without its use, the strip was inserted, using so small a hole as to show only theddistant reflectors and two teeth of the wheel.

<u>THE TOOTHED WHEEL</u> This was constructed by E. Dent(i) & Co. It was necessary for the wheel to revolve at great speed and must be capable of going at least for some minutes as to avoid the necessity of continually winding it up. It was driven by a weight mechanism attached to five separate pinion arrangments giving a multiplication factor of 10,000,fold. An electrical contact device was attached so that for every 100 revolutions of the toothed wheel a pulse of electricity was transmitted to the chronographs. The best shape for the teeth was saw teeth and the best results were obtained with a wheel having 400 teeth.

<u>THE REFLECTORS</u> The two reflecting collimators had identical construction by Troughton(ii) amd Simms(iii). The achromatic object lens was 3 inches in diameter and had a 3 foot focal length. At the other end was a circular silver mirror ground into a spherical form so placed that its centre of curvature lay on the object lens. The collimator was so designed that it could be adjusted for a) focus b) centering c) direction.

(i) William Dent (1793 - 1860) Instrument maker.
(ii) Edward Troughton (1753 - 1835) Instrument maker.
(iii) Frederick Walter Simms (1803 - 1865) Instrument maker.

THE CLOCK Made by E. Dent & Co. specially for this series of experiments. It was driven by a weight attached to an endless chain passing over a drum. The clock could function without attention for up to two hours.

The abor of the scape wheel had a wheel of 120 teeth for making electrical contact once a second by means of which a mark was made on the chronograph.

THE CHRONOGRAPH In the experiments prior to 1880 a portable chronograph by Hypp of Neufchatel was used. Although a large number of observations were made with this apparatus and was admirably adapted for observatory work it was not suitable for accuracy of more than $\frac{1}{100}$ th second. Thus a new device was constructed by Elliott Brothers which depended on uniformity of motion on the inertia of the apparatus.

It was their object to get rid of all clockwork and by making use of a fly-wheel, which had no work to do, to get rid of a host of irregularities which affected all other chronographs. By the use of a microscope and vernier and with the apparatus revolving at the rate of about one revolution a second accurate measurements could be taken with one division of the vernier corresponding to $\frac{1}{10,000}$ th second.

DYNAMO A Siemen's(i) 3 horse-power unit was used rotating at 1,400 turns per minute.

LIGHT A Siemen's electric lamp was used in conjunction with a condensing lens which threw an image of the incandescent carbon, after reflection by the diagonal reflector, onto the toothed wheel.

The light source and objective piece were set up in Kelly

(i) Sir William Siemens (1823 - 1883) Chairman of A. Siemens Engineers.

December 21, 1880, No. 5.					
Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
			7,953		
1,197			17,419	16,742	
	1,964	2,179	24,695		294
3,161			..	(16,448)	
..	1,972	5,133	41,143		320
7,104			50,225	16,128	
			57,271		
Signal at 3,482			Signal at 33,692		
$v'=\cdot001968$			$\bar{s}=32,958$ $4f=\ 308$		
$n=\frac{v}{v'}=417\cdot58$			$\bar{v}=\ \cdot8224$ Correction for friction, &c. $=-\ \cdot0006$ $v=\ \cdot8218$		

December 21, 1880, No. 6.					
Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
			5,952		
8,448			14,799	19,866	
	2,110	9,503	25,818	19,699	339
10,558			34,498	(19,527)	282
..	2,083	12,641	45,345	19,417	
14,723			53,915		
Signal at 12,241			Signal at 32,524		
$v'=\cdot002086$			$\bar{s}=35,616$ $4f=\ 285$		
$n'=\frac{v}{v'}=469\cdot14$			$\bar{v}=\ \cdot97640$ Correction for friction, &c. $=+\ \cdot00224$ $v=\ \cdot97864$		

$$\text{From Nos. 5 and 6}\left\{\begin{array}{r} 2m\dfrac{(n+n')D_B}{r}=188{,}484 \\ 1-\dfrac{\delta}{g+\rho}=0\cdot999583 \\ \text{Product }=V=188{,}405 \end{array}\right\}\text{Correction for second term }=-79.$$

SAMPLE RESULTS (YOUNG & FORBES) REF. (80)

following is a summary of these results :—

12th and 13th equalities.

1880 December 21, Nos.	1	and	2	V=187,707 miles per second.
„ „	5	„	6	188,405 „
„ „	8	„	9	187,676 „
„ „	9	„	10	186,457 „
„ „	10	„	11	185,788 „
1881 January 20, Nos.	3	„	4	186,495 „
„ „	5	„	6	187,003 „
Mean for 12th and 13th equalities :				V=187,076 „

13th and 14th equalities.

1880 December 21, Nos.	2	and	3	V=186,190 miles per second.
1881 January 20, Nos.	6	„	7	186,830 „
„ „	7	„	8	187,266 „
„ January 21, Nos.	2	„	3	188,110 „
„ „	3	„	4	188,079 „
Mean for 13th and 14th equalities :				V=187,295 „
General mean of both sets . .				V=187,167 „

plying this by the mean refractive index of air (=1·00029) we obtain the value velocity in vacuo, viz.: 187,221 miles per second.
must be corrected for the rate of our clock.
second of our clock is equal to 0·999723 of a mean solar second.
ling the value found for V by this quantity, we obtain the final value for the of the white light from an electric lamp in vacuo, viz. :—

V=187,273 miles per second (log=5·2724757)
=301,382 kiloms. per second (log=5·4791167)
Using STRUVE's constant of aberration 20″·445.
The resulting parallax of the sun is =8″·77.
Distance of the sun =93,223,000 miles.

value obtained by CORNU,* using the method of FIZEAU, was 300,400 kiloms. d. He nearly always used the DRUMMOND (or lime) light. A few experiments were made with a petroleum lamp.

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* "Annales de l'Observatoire de Paris" (Mémoires, tome xiii.), 1876.

SUMMARY OF RESULTS (YOUNG & FORBES) REF (80)

House, Wemyss Bay with the reflecting collimators on the hills behind the village of Innellan. The distances to the two reflectors were calculated as 18,210.6 feet and 16,825.3 feet. These results then were subject to 6 sets of corrections after which

$\mathrm{CA} = 18{,}212.2 \text{ feet} \equiv 3.44928 \text{ miles}$

$\mathrm{CB} = 16{,}835.0 \text{ feet} \equiv 3.18845 \text{ miles}$

$$g = \frac{\mathrm{DA}}{\mathrm{DB}} = 1.08181 \qquad \delta = \frac{13}{12} - \frac{\mathrm{DA}}{\mathrm{DB}} = +\,0.00152$$

$$\frac{r+1}{r} = \frac{13}{12} = 1.08333 \qquad \delta' = \frac{14}{13} - \frac{\mathrm{DA}}{\mathrm{DB}} = -\,0.00484$$

$$\frac{r+2}{r} + 1 = \frac{14}{13} = 1.07697$$

A.A. Michelson (81) using a modification of the method of Foucault obtained a value of 299,940 km per second. He used the light of the sun when near the horizon. Grouping the three sets in order we have:

	Usual source of light	Method	Result for velocity km/sec.
MICHELSON	The sun near horizon	Deflection by a mirror	299,940
CORNU	Lime light	Toothed wheel and eclipses	300,400
YOUNG & FORBES	Electric light	Toothed wheel and equalities	301,382

Distinctive colour observed in the return light

In the observations using sunlight at Pitlochry in 1878 and those using electric light at Kelly in 1880-1881, Young and Forbes were disturbed by the presence of colour in the stars, one of them appearing reddish and the other bluish. This colouration made it difficult to judge accurately the equality of the two lights and hence to gauge the exact speed which produced the equality in the lights. They considered that the colours arose from a want of accurate adjustment of the distant reflectors. The quality of light which was reflected back into the observing telescope depended on the accuracy of focus of the reflecting collimator, hence if the objective lens was not accurately achromatised then one reflector lens could be focused for blue rays and the other for red rays. Thus one star could be intrinsically redder than the other.

On the 11th February, 1881 observations were made for the speed of revolution to give the 12th, 13th and 14th equalities corresponding to speeds of 410, 450 and 490 revolutions per second. These speeds were obtained by using three, four or five weights to drive the mechanism.

RESULTS: February 11th, 1881 A and B very bright and steady
1. 3 weights B increasing with increase of speed. B reddish A bluish
2. 4 " A " " " " " A " B "
3. 5 " B " " " " " B " A "
4. 4 " A " " " " " A " B "
5. 3 " B " " " " " B " A "

More observations were made at different speeds see (80) p 274 and a statement was recorded in the observation book:
"Always the light which is increasing with respect to the other, with increase of velocity (of the toothed wheel) appears red; and the other one blue."
"These observations clearly proved to us that the colour which we had often observed was not always due to the adjustment of the distant reflectors. For here sometimes the one and sometimes the other was the red one. At each successive equality (e.g. the 11th and 12th, the 12th and 13th, etc.) the colours of A and B are reversed.

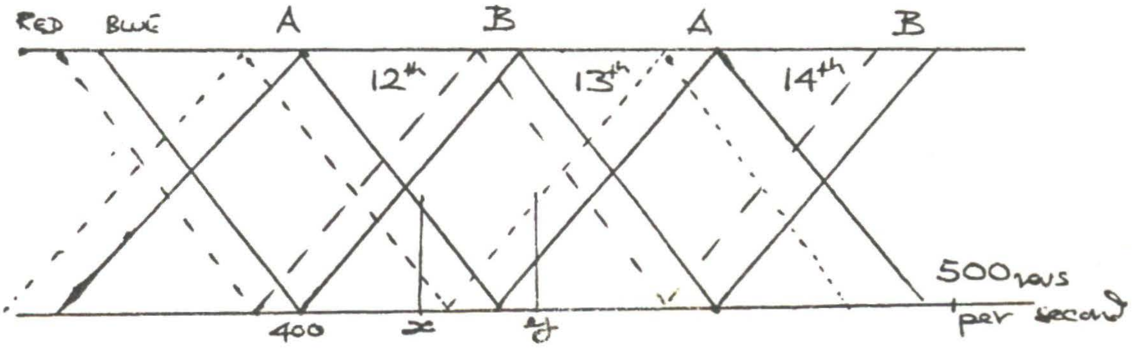
Since February 11th there certainly have been many days when the colour-differences were not perceptible. It may perhaps have been because the stars were not steady or were flickering or indistinct. On these occasions the atmospheric refraction disturbs the course of the rays, so that the teeth of the wheel being extremely minute, a ray of light which, if there were no irregular atmospheric refraction, would not reach the reflector, does so under these circumstances. In such a case the stars do not alter their intensities, with change of speed of the toothed wheel, so regularly as they do when the atmosphere is not unequally heated and disturbed.

The general result however was established by the observations on February 11th, 1881, but it is not a common observation."

Explanation of the Colours

They considered that the different colours travelled with different velocities, the more refrangible rays, or those with shortest wavelength, travelling quickest.

If the red light travelled slower than the blue a smaller velocity is required to produce an eclipse with red light than with blue. Thus the curve representing intensity in terms of speed of rotation for red light should have its maxima and minima lagging more and more behind those for blue light.



Since the speeds of rotation which produced the 12th, 13th and 14th equalities were being considered then during the small speed variation involved the lines representing the red and blue light can sensibly be drawn as parallel. At x the light of A is diminishing with increase of speed, and the abscissa corresponding to blue light is greater than that corresponding to red light. Hence, when the intensity is diminishing with increase of speed the star should have a blue tinge. But at y the light of A is increasing with increase of speed, and the abscissa corresponding to red light is greater than that corresponding to blue light. Hence, when the intensity is increasing with increase of speed the star should have a red tinge. Observations confirmed these statements and could be explained on the assumption that blue light travelled quicker than red light.

The speed of rotation necessary to give equality of lights must be greater for blue light than for red light and further that the difference in the speed of rotation for red and for blue light bears the same relation to the absolute speed of rotation for either of those colours as the difference in velocity between rays of red and blue light bears to the absolute velocity of that colour.

Young and Forbes determined the speed which produced an equality 1) in the ordinary way with the white light of the electric lamp, and 2) with the eye screened by a piece of ruby red glass. The differences between the velocities of red and white light were small and the speeds of rotation finally deduced from the chronograph records were as follows:

		Difference
Observation No. 13 (red) speed of rotation	$456^{r}.84$	$4^{r}.14$
" No. 13 (white) " " "	$460^{r}.93$	
" No. 14 (red) " " "	$494^{r}.85$	$1^{r}.57$
" No. 14 (white) " " "	$496^{r}.42$	

Difference of velocity (red and white)= 0.90 per cent from No.13

Absolute velocity (white) 0.32 per cent from No.14

The differences were small; but on the whole they suggested a greater speed for white light than for red light. However these small differences could be due to irregularities in the working of the chronograph and so it was decided to choose two colours of light of considerable difference in wavelength whereby the chronograph could be discarded as the absolute measurer of the speed since a greater difference in speed should be noticed.

After a great deal of searching to obtain a blue medium which would sufficiently keep out the red rays, it was found that a copper nitrate solution gave the least quantity of red.

The first differential observations for red and blue light were made on February 11th. A thick piece of rubber tubing was attached to the top of the pulley which supported the weights driving the toothed wheel. At its upper end it was attached to a string over a fixed pulley and in turn was held by the observer. The system was such that as the weights descended, the rubber was stretched and so diminished the effective driving weight. This produced an extremely gradual diminution of velocity accompanied by a gradual increase in the brightness of the two stars. The blue solution was placed between the lamp and the diagonal reflector. Once equality of light was obtained counting in seconds began and the blue solution was replaced by the ruby glass. When equality once again was restored the difference in the time interval was noted. It was then necessary to measure by means of the chronograph the diminution in velocity produced by the action of the rubber during a given number of seconds. This time interval was taken as 18 seconds and the average loss of speed was 0.49 revs/secs. The average time interval between the equality of red and blue lights was 23.5 seconds. This gave a difference of 11.5 revs per second (23.5 x 0.49) or about 2.82% of the speed producing equality of white light (410 revs per second).

TABLE OF % DIFFERENCE IN THE VELOCITY OF RED AND OF BLUE LIGHT

Feb.21	Feb.23	Feb.24	Feb.25	Feb.27	Mar. 1	Mar. 8	Apr.26	Apr.27
+ 3.36		-	+ 0.29	+ 1.28	+1.35	+ 3.20		
1.14			0.43	1.22	1.56	1.80		
2.40			0.70	1.71	0.90	1.00		
2.88	+ ve			1.55	0.90	1.60	+ ve	+ ve
5.40	effect			3.14		2.10	effect	effect
2.46				1.10		1.20		
2.40				1.40		1.40		
2.52				0.68		1.30		
						3.20		
						4.60		
						2.00		
						1.90		
						1.80		
						1.40		
+ 2.82	+	-	+ 0.47	+ 1.51	+1.17	+2.03	+	+

They were unable to account for the apparently negative effect on the 24th February except that the observation only lasted a very short time and that the appearance of the negative effect was extremely faint. On the other hand the positive effect was most marked and indubitable. They went on to affirm that the wavelengths changed from about = 50 tenth-metres to about λ = 60 tenth-metres then the velocity changed about 1.8%, or in any case somewhat over 1 percent.

This difference was so great that, in the absence of other support, the effects observed were not generally accepted as due to a difference in the velocities of the various rays, but it was surmised that the colouring was rather due to some extraneous cause not yet fully determined.

Cornu(i) drew attention to the diffraction effects arising (82) 1) from the waves of light travelling to the distant telescope and just grazing the nearer one and 2) from the use of a mirror with a central hole instead of a glass plate, in order to increase the brightness of the image. From this it followed that the telescopes received diffracted pencils from the edge of the central hole and sent back waves diffracted by the edges of their objectives. To these diffraction effects he attributed the high value of the velocity obtained by Young and Forbes and the difference in the observed velocities of the blue and red rays.

Such a great difference as 1.8 per cent in the velocities

(i) Marie Alfred Cornu (1841 - 1902) Professor of Physics at the École Polytechnique.

of the red and blue rays should have been detected in the other methods of estimating the velocity of light. Thus in Foucault's method, the image of the slit, should have been drawn out into an elongated spectrum but no such colouring or elongation has been observed.

CHAPTER 12

THE EARLY EXPERIMENTS OF A.A. MICHELSON

Michelson(i) took an early interest in optics because he was told to teach the subject at the Naval Academy. Whilst demonstrating Foucault's experiment, Michelson noticed that the return beam was displaced a mere 0.8 m.m., a distance he considered too small to be measured accurately. During November 1877 he developed a (83) modification for the experiment and subsequently redesigned the apparatus by replacing the concave mirror with a plane mirror, altering the lens position and increasing the light path.
Unknown to Michelson, Simon Newcomb(ii) the director of the Nautical Almanac Office in Washington, was also interested in measuring the velocity of light. He had laid plans for such experiments a long time before Michelson's attempt and had been kept up to date on Michelson's progress.

" Department of Physics and Chemistry
U.S. Naval Academy
March 25,1878

Prof. Newcomb. (84)

Dear Sir,

Thinking you would be interested to know how Michelson's plan for measuring the velocity of light is coming on, I can tell you it promises entire success. The original plan has been considerably changed so that any distance can

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(i) Albert Abraham Michelson (1852 - 1931) Professor at the Case Institute

(ii) Simon Newcomb (1835 - 1909) Superintendent of the Nautical Almanac Office be used. The arrangement admits of such precise adjustment that I think that when we have arranged to count the revolutions of the mirror, the results will be good. The large photo-heliostat silvered on its front face is used as the fixed mirror. The rotating mirror, also silvered on one face, is a little more than one inch in diameter. At a preliminary trial on Saturday with a distance of about 250 ft. and about 125 revolutions we obtained a deviation of 1/25 inch. The fixed mirror is now placed at a mile distance and the mirror will be given a velocity of 200 turns."

Michelson, on learning of Newcomb's interest, wrote to him

"

U.S. Naval Academy
Annapolis, Md.
April 26th, 1878.

Professor Newcomb: (85)

Dear Sir,

Having read in the "Tribune" an extract of your paper on a method for finding the velocity of light, and hearing through Capt. Sampson and Capt. Howell that you were interested in my own experiments, I trust I am not taking too great a liberty in laying before you a brief account of what I have done. (Here Michelson describes his experiment, adding that the distance between mirrors might be considerably increased.)

Unfortunately, as I was about to make an accurate observation the mirror flew out of its bearings and broke. It would give me great pleasure, dear sir, if you could honor me with an interview, in which you could advise me how to arrange some of the details so as to insure good results.

Believe me, sir,
Your obedient servant,
Albert Michelson,
Ensign U.S.N. "

Newcomb immediately replied _ _ _ "To have obtained so large a deviation from apparatus so extremely simple, seems to me a triumph, upon which you ought to be most heartily congratulated. So far as I know, it is the first actual experiment of this kind ever made on this side of the Atlantic." (86)

Newcomb hurried down to see the apparatus whereupon he gave some advice as to the use of a concave mirror and to place the rotating mirror in a vacuum.

He wrote (see 86), "Still, I am not at all sure but that your plan is better than mine. Certainly it is simpler and cheaper."

Newcomb sent a letter to Rear Admiral Anman, Chief of the Bureau of Navigation (June 5th, 1878) suggesting that the work of Michelson be well worthy of the encouragement of the Department and of Congress.

However the Senate Appropriations Committee did not look upon Michelson's work with favour and failed to earmark any funds in the Naval Appropriation Bill for the purpose of measuring the velocity of light.

Newcomb also approached the National Academy of Sciences but did mention the independent work of Michelson.

However, since Newcomb had submitted his own proposals before (87) Michelson, the Appropriations Committee decided that Newcomb's plan be the one to receive a grant and as such he received $ 5000 whilst Michelson had to look elsewhere for funds. He in fact received $ 2000 from his grandfather Albert Heminway.

THE EXPERIMENTS OF A.A. MICHELSON

The determination of the velocity of light was considered to be of national importance in the U.S.A. and as such in 1879 Congress made an appropriation for the work and gave Newcomb the responsibility for doing it. At this time Michelson was preparing to make an independent determination and it was arranged that he should assist in Newcomb's work.

Now the main source of error in Foucault's method rested on the small displacement of the light image. In November, 1877, a modification suggested itself which could improve matters. The first experiment tried with the revolving mirror produced a deflection considerably greater than that obtained by Foucault. The first crude system was set up in May 1878 using a distance of 500 feet and a deflection (88) was obtained of about twenty times that obtained by Foucault. Ten results were obtained giving a mean value of 186,500 ± 300 miles/sec or 300140 km/sec.

The apparatus was further modified and by the end of May 1879 everything was ready for a long series of observations.

SITE PLAN (89)

A building was erected 45 feet by 14 feet and raised so that the line along which the light travelled was 11 feet above the ground. A heliostat (h) reflected the sun's rays

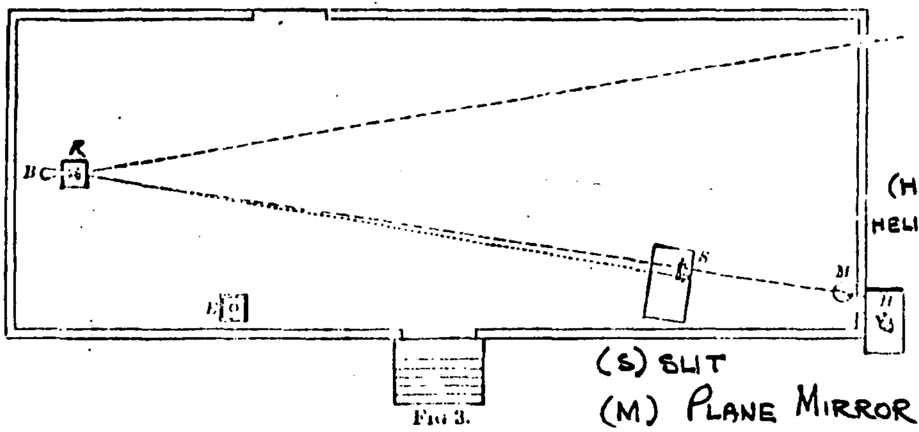


Fig 3.

(R)
REVOLVING MIRROR

(H)
HELIOSTAT

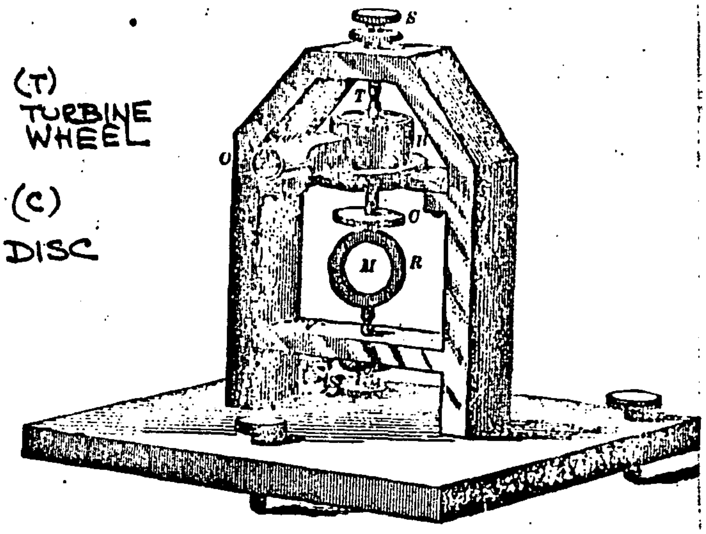
(S) SLIT

(M) PLANE MIRROR

LEVELLING SCREWS
BINDING SCREWS

(T)
TURBINE WHEEL

(C)
DISC



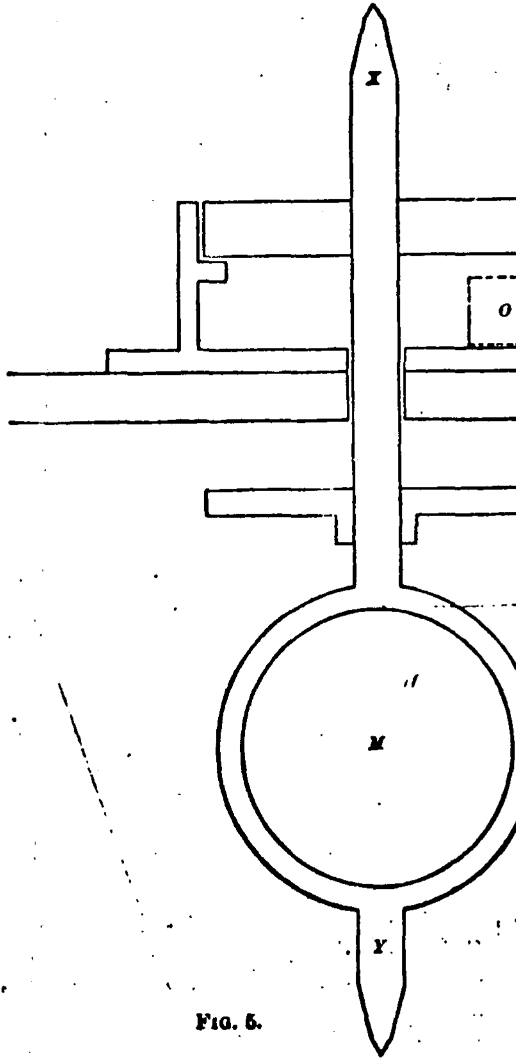
(M) MIRROR

(R) RING

FIG. 4.

AIR ENTERS AT (O)

AIR ROTATES IN BOX (B)



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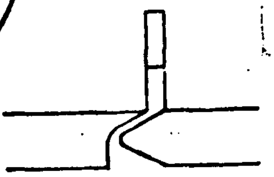


FIG. 5.

FIG. 6.

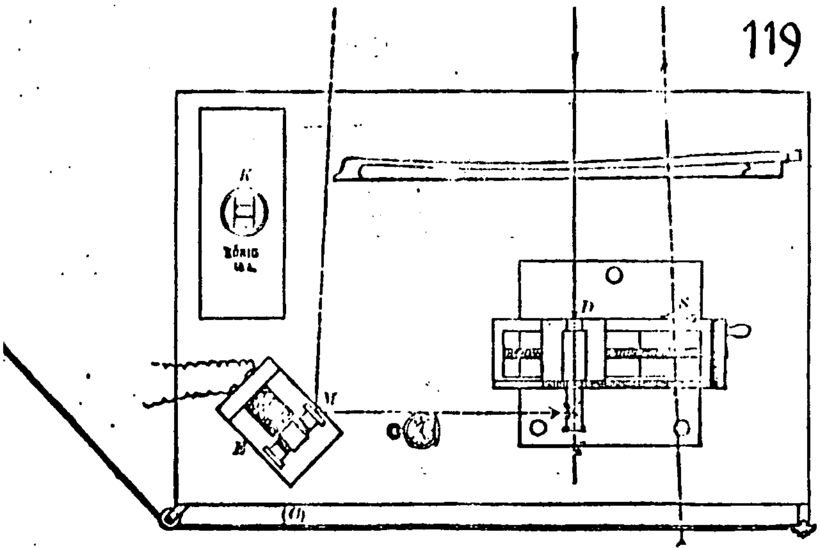


Fig. 9.

(S) SLIT

(D) IMAGE OF SLIT

(E) ELECTRIC FORK

(M) STEEL MIRROR

(K) STANDARD FORK ON RESONATOR

PLAN VIEW, TURBINE, BOX & TUBE

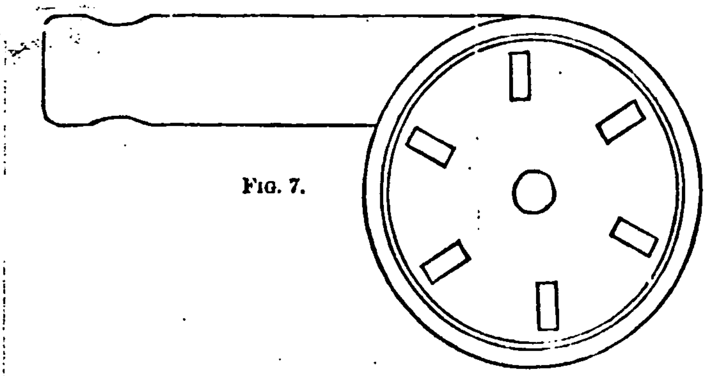


Fig. 7.

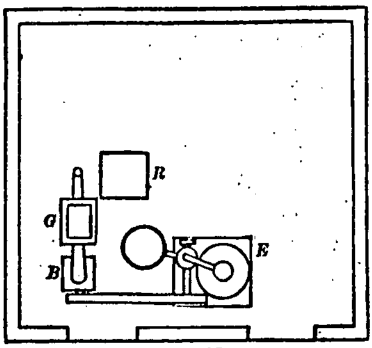
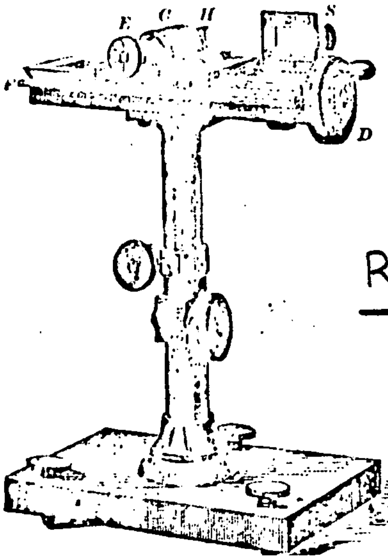


Fig. 10.

(B) ROOT'S BLOWER

(G) AUTOMATIC REGULATOR



MICROMETER

REF (89)

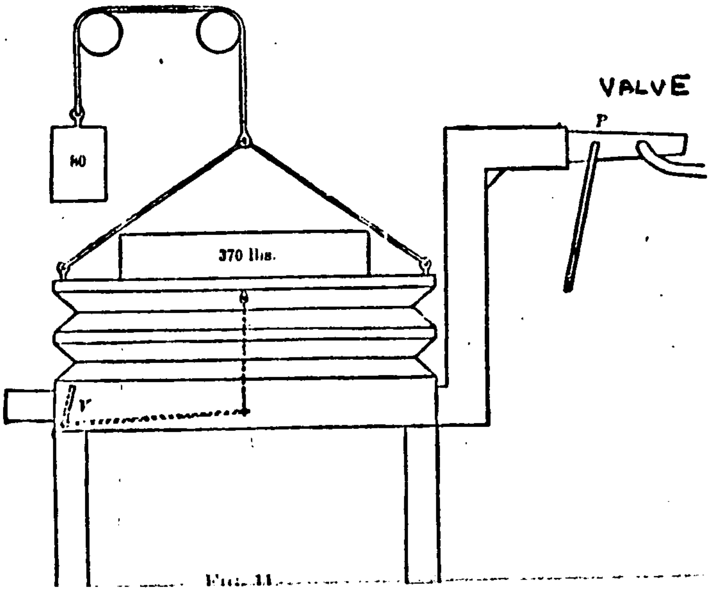


Fig. 11.

Fig. 12.

through the slit (s) to the revolving mirror (R) then through a hole in the shutter, through the lens and to the distant mirror.

The revolving mirror was supported in a cast-iron frame and could be inclined forward or backward whilst making the observations. The mirror itself was a disc of plane glass about $1\frac{1}{4}$ inch in diameter and 0.2 inch thick. It was silvered on one side only, the reflection taking place from the outer or front surface. Further a type of turbine wheel (T) was held on the axle by friction. When all the necessary adjustments were made the apparatus could revolve with a highly regular motion with great speed.

To measure the deflection, the eye-piece of the micrometer was moved until the cross-hair bisected the slit, and the reading of the scale and divided head gave the position. This measurement was not repeated unless the position or width of the slit was changed. Then the eye-piece was moved until the cross-hair bisected the deflected image of the slit; the reading of the scale and head were taken again and the difference in readings gave the deflection.

Measurement of the speed of Rotation

A tuning fork, bearing on one prong a steel mirror was used to measure the speed of rotation. This was kept in vibration by electricity from five 'gravity' cells. The fork was arranged so that the light from the revolving mirror was reflected onto a piece of plane glass, in front of the eye-piece of the micrometer, inclined at an angle of 45°, and then into the eye. When the fork and the revolving mirror were both at rest, an image of the revolving mirror was seen. When the fork vibrated, the image was drawn out into a band of light.

On rotation, this band was broken up into a number of moving images of the mirror. When the mirror made as many turns as the fork made vibrations, the images were reduced to one stationary image. This also happened when the number of turns was a submultiple. When it was a multiple or simple ratio, you observed more images.

Hence by pulling the cord attached to the valve it was possible to make the mirror execute a certain number of turns by ensuring that the images of the revolving mirror came to rest.

The electric fork made about 128 vibrations per second and at each set of observations it was compared with a standard Ut_3 fork, the temperature being noted at that time.

The comparison was made using beats counted over a period of 60 seconds. As long as the electric fork remained untouched and at the same temperature it did not change its rate more than one or two hundredths vibrations per second.

The lens was 8 inches in diameter having a focal length of 150 feet and was not achromatic. Since the diameter was so small in comparison with its focal length the need for achromatism was inappreciable. For the same reason the effect of parallax was too small to be noticed.

The fixed mirror was about 7 inches in diameter and was capable of adjustment in a vertical and horizontal plane.

Being wedge-shaped it was silvered on the front surface.

The fixed mirror was adjusted by means of a theodolite.

The mirror being moved until an observer, looking through the hole in the shutter through the telescope saw the image of the telescope reflected centrally in the mirror. This adjustment had to be repeated before every series of experiments.

By means of the pressure regulating apparatus (see diagram) a pressure was built up of about half a pound per square inch. It was possible to keep the mirror at a constant speed for three or four seconds at a time which was sufficient for an observation to be taken.

It was found that the only time of day when the atmosphere was sufficiently steady to obtain a distinct image was the hour after sunrise and the hour before sunset.

The boiler was lit about half an hour before the observations in order to raise the 40/50 pounds of steam pressure. The mirror was adjusted and the heliostat placed in position and adjusted.

Next the revolving mirror was inclined to the right or left, so that the direct reflection of light from the slit, which otherwise would have flashed into the eye-piece at every revolution, fell either above or below the eye-piece.

The revolving mirror was then adjusted by being moved about, and inclined forward and backward, until the light was seen reflected back from the distant mirror.

The distance between the front face of the revolving mirror and the cross-hair of the eye-piece was then measured by stretching from the one to the other a steel tape. A drop of the catenary of one inch was made so as to counterbalance the error of the stretch of the tape with that due to the curvature. The position of the slit was then determined

and the electric fork started. The temperature was noted and the beats between it and the standard fork counted for 60 seconds.
The eye-piece of the micrometer was then set and the revolving mirror started, the mirror being inclined forward or backward till the image came into sight.

Next the cord connected with the valve was pulled to the left or right until the images of the revolving mirror, represented by the two bright round spots to the left of the cross-hair came to rest. Then the screw was turned till the cross-hair bisected the deflected image of the slit. This was repeated until ten observations were taken. Usually five sets of such (ten) observations were taken each morning and evening.

Determination of the constants.

(i) Comparison of the steel tape with the standard yard
(ii) Determination of the value of the micrometer — see set
(iii) Measurement of the distance between the mirrors — of results
(iv) Measurement of the rate of Ut_3 fork

The formulae employed were:

(i) $\tan \phi = \frac{d_1}{r}$ and (ii) $V = \frac{2592000'' \times D \times n}{\phi''}$

ϕ = angle of deflection
d_1 = corrected displacement (linear)
r = radius of measurement
D = twice the distance between the mirrors
n = number of revolutions per second
α = inclination of plane of rotation

d = deflection as read from micrometer

B = number of beats per second between electric Vt_2 fork and standard Vt_3

Cor = correction for temperature of standard Vt_3

V = velocity of light

T = value of one turn of screw

Substituting for d, its value or d x T x sec α (log sec α = 0.00008),

and for D its value 3972.46, and reducing to kilometres we have (iii) $\tan \phi = C_1 \frac{dT}{r}$; log ε_1 = 0.51607

and (iv) $V = C \frac{n}{\phi}$; log C = 0.49670

D and r were expressed in feet and d in m.m. Vt_3 made 256.070 vibrations per second at 56°F

D = 3972.46 feet

Tan α = 0.02

The electric fork made $\frac{1}{2}$ (256.070 +B + cor) vibrations per second.

MICHELSON REF (89)

Date.	Distinctness of image.	Temperature, Fahr.	Position of deflected image.	Position of slit.	Displacement of image in divisions.	Difference between greatest and least values.	B.	Cor.	Number of revolutions per second.	Radius of measurement, in feet.	Value of one turn of the screw.	Velocity of light in air, in kilometers.	Remarks.
June 5 . .	3	76	114.85	0.300	114.55	0.17	+ 1.423	— 0.132	257.36	28.672	0.99614	299850	Electric light.
7 . .	2	72	114.64	0.074	114.56	0.10	1.533	— 0.084	257.52	28.655	0.99614	299740	P. M. Frame inclined at various angles.
7 . .	2	72	114.58	0.074	114.50	0.08	1.533	— 0.084	257.52	28.647	0.99614	299900	P. M. Frame inclined at various angles.
7 . .	2	72	85.91	0.074	85.84	0.12	1.533	— 0.084	193.14	28.647	0.99598	300070	P. M. Frame inclined at various angles.
7 . .	2	72	85.97	0.074	85.89	0.07	1.533	— 0.084	193.14	28.650	0.99598	299930	P. M. Frame inclined at various angles.
7 . .	2	72	114.61	0.074	114.53	0.07	1.533	— 0.084	257.42	28.650	0.99614	299850	P. M. Frame inclined at various angles.
9 . .	3	83	114.54	0.074	114.47	0.07	1.533	— 0.216	257.39	28.658	0.99614	299950	P. M. Frame inclined at various angles.
9 . .	3	83	114.54	0.074	114.46	0.10	1.533	— 0.216	257.39	28.658	0.99614	299980	P. M. Frame inclined at various angles.
9 . .	3	83	114.57	0.074	114.47	0.08	1.533	— 0.216	257.39	28.662	0.99614	299980	P. M. Frame inclined at various angles.
9 . .	3	83	114.57	0.074	114.50	0.06	1.533	— 0.216	257.39	28.660	0.99614	299880	P. M. Frame inclined at various angles.
9 . .	2	83	114.61	0.074	114.53	0.13	1.533	— 0.216	257.39	28.678	0.99614	300000	P. M. Frame inclined at various angles.
10 . .	2	90	114.60	0.074	114.52	0.11	1.517	— 0.300	257.29	28.685	0.99614	299980	P. M.
10 . .	2	90	114.62	0.074	114.54	0.08	1.517	— 0.300	257.29	28.685	0.99614	299930	P. M.
12 . .	2	71	114.81	0.074	114.74	0.09	1.450	— 0.072	257.45	28.690	0.99614	299650	A. M.
12 . .	2	71	114.78	0.074	114.70	0.05	1.450	— 0.072	257.45	28 690	0.99614	299760	A. M.
12 . .	1	71	114.76	0.074	114.68	0 09	1.450	— 0.072	257.45	28.690	0.99614	299810	A. M.
13 . .	3	72	112.64	0.074	112.56	0.09	1.500	— 0.084	257.49	28.172	0.99614	300000	A. M.
13 . .	3	72	112.63	0.074	112.56	0.10	1.500	— 0.084	257.49	28.172	0.99614	300000	A. M.
13 . .	2	72	112.65	0.074	112.57	0.08	1.500	— 0.084	257.49	28.172	0.99614	299960	A. M.
13 . .	3	79	112.82	0.260	112.56	0.06	1.517	— 0.168	257.42	28.178	0.99614	299960	P. M.
13 . .	3	79	112.82	0.260	112.56	0.13	1.517	— 0.168	257.42	28.178	0.99614	299960	P. M.
13 . .	3	79	112.83	0.260	112.57	0.07	1.517	— 0.168	257.42	28.178	0.99614	299940	P. M.
13 . .	3	79	112.82	0.260	112.56	0.06	1.517	— 0.168	257.42	28.178	0.99614	299960	P. M.
13 . .	3	79	112.83	0.260	112.57	0.11	1.517	— 0.168	257.42	28.178	0.99614	299940	P. M.
13 . .	3	79	113.41	0.260	113.15	11	1.517	— 0.168	258.70	28.152	0.99614	299880	P. M. Set micrometer and counted oscillations.

MICHELSON REF (89)

Date.	Distinctness of image.	Temperature, Fahr.	Position of deflected image.	Position of slit.	Displacement of image in divisions.	Difference between greatest and least values.	B.	Cor.	Number of revolutions per second.	Radius of measurement, in feet.	Value of one turn of the screw.	Velocity of light in air, in kilometers.	Remarks.
June 13 . .	3	79	112.14	0.260	111.88	6	+ 1.517	— 0.168	255.69	28.152	0.99614	299800	Oscillations of image of revolving mirror.
14 . .	1	64	112.83	0.260	112.57	0.12	1.500	+ 0.012	257.58	28.152	0.99614	299850	A. M.
14 . .	1	64	112.83	0.260	112.57	0.05	1.517	+ 0.012	257.60	28.152	0.99614	299880	A. M.
14 . .	1	65	112.81	0.260	112.55	0.11	1.517	0.000	257.59	28.152	0.99614	299900	A. M.
14 . .	1	66	112.83	0.260	112.57	0.09	1.517	— 0.012	257.57	28.152	0.99614	299840	A. M.
14 . .	1	67	112.83	0.260	112.57	0.12	1.517	— 0.024	257.56	28.152	0.99614	299830	A. M.
14 . .	1	84	112.78	0.260	112.52	0.06	1.517	— 0.228	257.36	28.159	0.99614	299790	P. M. Readings taken by Lieut. Nazro.
14 . .	1	85	112.76	0.260	112.50	0.08	1.500	— 0.240	257.33	28.159	0.99614	299810	P. M. Readings taken by Lieut. Nazro.
14 . .	1	84	112.72	0.260	112.46	0.08	1.483	— 0.228	257.32	28.159	0.99614	299880	P. M. Readings taken by Lieut. Nazro.
14 . .	1	84	112.73	0.260	112.47	0.09	1.483	— 0.228	257.32	28.159	0.99614	299880	P. M.
14 . .	1	84	112.75	0.260	112.49	0.09	1.483	— 0.228	257.32	28.159	0.99614	299830	P. M.
17 . .	2	62	112.85	0.260	112.59	0.09	1.517	+ 0.036	257.62	28.149	0.99614	299800	A. M.
17 . .	2	63	112.84	0.260	112.58	0.06	1.500	+ 0.024	257.59	28.149	0.99614	299790	A. M.
17 . .	1	64	112.85	0.260	112.59	0.07	1.500	+ 0.012	257.58	28.149	0.99614	299760	A. M.
17 . .	3	77	112.80	0.260	112.54	0.07	1.500	— 0.144	257.43	28.157	0.99614	299800	P. M. Readings taken by Mr. Clason.
17 . .	3	77	112.77	0.260	112.51	0.08	1.500	— 0.144	257.43	28.157	0.99614	299880	P. M. Readings taken by Mr. Clason.
17 . .	3	77	112.77	0.260	112.51	0.11	1.500	— 0.144	257.43	28.157	0.99614	299880	P. M. Readings taken by Mr. Clason.
17 . .	3	77	112.77	0.260	112.51	0.09	1.500	— 0.144	257.43	28.157	0.99614	299880	P. M. Readings taken by Mr. Clason.
17 . .	3	77	112.78	0.260	112.52	0.08	1.500	— 0.144	257.43	28.157	0.99614	299860	P. M. Readings taken by Mr. Clason.
18 . .	1	58	112.90	0.265	112.64	0.07	1.500	+ 0.084	257.65	28.150	0.99614	299720	A. M.
18 . .	1	58	112.90	0.265	112.64	0.10	1.500	+ 0.084	257.65	28.150	0.99614	299720	A. M.
18 . .	1	59	112.92	0.265	112.66	0.07	1.483	+ 0.072	257.62	28.150	0.99614	299620	A. M.
18 . .	2	75	112.79	0.265	112.52	0.09	1.483	— 0.120	257.43	28.158	0.99614	299860	P. M.
18 . .	2	75	112.75	0.265	112.48	0.10	1.483	— 0.120	257.43	28.158	0.99614	299970	P. M.
18 . .	2	75	112.76	0.265	112.49	0.08	1.483	— 0.120	257.43	28.158	0.99614	299950	P. M.

Date.	Distinctness of image.	Temperature, Fahr.	Position of deflected image.	Position of slit.	Displacement of image in divisions.	Difference between greatest and least values.	B.	Cor.	Number of revolutions per second.	Radius of measurement, in feet.	Value of one turn of the screw.	Velocity of light in air, in kilometers.	Remarks.
June 20 . .	3	60	112.94	0.265	112.67	0.07	+1.517	+ 0.063	257.65	28.172	9.99614	299880	A. M.
20 . .	3	61	112.92	0.265	112.65	0.09	1.517	+ 0.048	257.63	28.172	0.99614	299910	A. M.
20 . .	2	62	112.94	0.265	112.67	0.07	1.517	+ 0.036	257.62	28.172	0.99614	299850	A. M.
20 . .	2	63	112.93	0.265	112.66	0.03	1.517	+ 0.024	257.61	28.172	0.99614	299870	A. M.
20 . .	2	78	133.48	0.265	133.21	0.13	1.450	— 0.156	257.36	33.345	0.99627	299840	P. M.
20 . .	2	79	133.49	0.265	133.23	0.09	1.500	— 0.168	257.40	33.345	0.99627	299840	P. M.
20 . .	2	80	133.49	0.265	133.22	0.07	1.500	— 0.180	257.39	33.345	0.99627	299850	P. M.
20 . .	2	79	133.50	0.265	133.24	0.13	1.483	— 0.168	257.39	33.345	0.99627	299840	P. M.
20 . .	2	79	133.49	0.265	133.22	0.06	1.483	— 0.168	257.38	33.345	0.99627	299840	P. M.
20 . .	2	79	133.49	0.265	133.22	0.10	1.483	— 0.168	257.38	33.345	0.99627	299840	P. M.
21 . .	2	61	133.56	0.265	133.29	0.12	1.533	+ 0.048	257.65	33.332	0.99627	299890	A. M.
21 . .	2	62	133.58	0.265	133.31	0.08	1.533	+ 0.036	257.64	33.332	0.99627	299810	A. M.
21 . .	2	63	133.57	0.265	133.31	0.09	1.533	+ 0.024	257.63	33.332	0.99627	299810	A. M.
21 . .	2	64	133.57	0.265	133.30	0.11	1.533	+ 0.012	257.61	33.332	0.99627	299820	A. M.
21 . .	2	65	133.56	0.265	133.30	0.13	1.533	0.000	257.60	33.332	0.99627	299800	A. M.
21 . .	3	80	133.48	0.265	133.21	0.06	1.533	— 0.180	257.42	33.330	0.99627	299770	P. M.
21 . .	3	81	133.46	0.265	133.19	0.10	1.500	— 0.192	257.38	33.330	0.99627	299760	P. M.
21 . .	3	82	133.46	0.265	133.20	0.05	1.500	— 0.204	257.37	33.330	9.99627	299740	P. M.
21 . .	3	82	133.46	0.265	133.20	0.08	1.517	— 0.204	257.38	33.330	0.99627	299750	P. M.
21 . .	3	81	133.46	0.265	133.19	0.08	1.500	— 0.192	257.38	33.330	0.99627	299760	P. M.
23 . .	3	89	133.43	0.265	133.16	0.08	1.542	— 0.288	257.32	33.345	0.99627	299910	P. M.
23 . .	3	89	133.42	0.265	133.15	0.06	1.550	— 0.288	[illegible]	33.345	0.99627	299920	P. M.
23 . .	3	90	133.43	0.265	133.17	0.09	1.550	— 0.300	257.32	33.345	0.99627	299890	P. M.
23 . .	3	90	133.43	0.265	133.16	0.07	1.533	— 0.300	257.30	33.345	0.99627	299860	P. M.
23 . .	3	90	133.42	0.265	133.16	0.07	1.517	— 0.300	257.29	33.345	0.99627	299880	P. M.

MICHELSON REF (89)

MICHELSON REF.(89)

Date.	Distinctness of image.	Temperature, Fahr.	Position of deflected image.	Position of slit.	Displacement of image in divisions.	Difference between greatest and least values.	B.	Cor.	Number of revolutions per second.	Radius of measurement, in feet.	Value of one turn of the screw.	Velocity of light in air, in kilometers.	Remarks.
June 24	3	72	133.47	0.265	133.20	0.15	+ 1.517	— 0.084	257.50	33.319	0.99627	299720	A. M.
24	3	73	133.44	0.265	133.17	0.04	1.517	— 0.096	257.49	33.319	0.99627	299840	A. M.
24	3	74	133.42	0.265	133.16	0.11	1.517	— 0.108	257.48	33.319	0.99627	299850	A. M.
24	3	75	133.42	0.265	133.16	0.06	1.517	— 0.120	257.47	33.319	0.99627	299850	A. M.
24	3	76	133.44	0.265	133.18	0.10	1.517	— 0.132	257.45	33.319	0.99627	299780	A. M.
26	2	86	133.42	0.265	133.15	0.05	1.508	— 0.252	257.33	33.339	0.99627	299890	P. M.
26	2	86	133.44	0.265	133.17	0.08	1.508	— 0.252	257.33	33.339	0.99627	299840	P. M.
27	3	73	133.49	0.265	133.22	0.11	1.483	— 0.096	257.46	33.328	0.99627	299780	A. M.
27	3	74	133.47	0.265	133.20	0.06	1.483	— 0.108	257.44	33.328	0.99627	299810	A. M.
27	3	75	133.47	0.265	133.21	0.09	1.483	— 0.120	257.43	33.328	0.99627	299760	A. M.
27	3	75	133.45	0.265	133.19	0.09	1.467	— 0.120	257.42	33.328	0.99627	299810	A M.
27	3	76	133.47	0.265	133.20	0.08	1.483	— 0.132	257.42	33.328	0.99627	299790	A. M.
27	3	76	133.45	0.265	133.19	0.10	1.483	— 0.132	257.42	33.328	0.99627	299810	A. M.
30	2	85	35.32	135.00	99.68	0.05	1.500	— 0.240	193.00	33.274	0.99645	299820	P. M. Mirror inverted.
30	2	86	35.34	135.00	99.67	0.06	1.508	— 0.252	193.00	33.274	0.09645	299850	P. M. Mirror inverted.
30	2	86	35.34	135.00	99.66	0.10	1.508	— 0.252	193.00	33.274	0.99645	299870	P. M. Mirror inverted.
30	2	86	35.34	135.00	99.66	0.09	1.517	— 0.252	193.00	33.274	0.99645	299870	P. M. Mirror inverted.
July 1	2	83	02.17	135.145	132.98	0.07	1.500	— 0.216	257.35	33.282	0.99627	299810	P. M. Mirror inverted.
1	2	84	02.15	135.145	133.00	0.09	1.500	— 0.228	257.34	33.282	0.09627	299740	P. M. Mirror inverted.
1	2	86	02.14	135.145	133.01	0.06	1.467	— 0.252	257.28	33.311	0.99627	299810	P. M. Mirror inverted.
1	2	86	02.14	135.145	133.00	0.08	1.467	— 0.252	257.28	33.311	0.99627	299940	P. M. Mirror inverted.
2	3	86	99.85	0.400	99.45	0.05	1.450	— 0.252	192.95	33.205	0.99606	299950	P. M. Mirror erect.
2	3	86	66.74	0.400	66.34	0.03	1.450	— 0.252	128.63	33.205	0.99586	299800	P. M. Mirror erect.
2	3	86	50.16	0.400	47.96	0.07	1.467	— 0.252	96.48	33.205	0.99580	299810	P. M. Mirror erect.
2	3	85	33.57	0.400	33.17	0.06	1.450	— 0.240	64.32	33.205	0.99574	299870	P. M. Mirror erect.

The following table gives the results of different groupings of sets of observations. Necessarily some of the groups include others :

Electric light (1 set)	299850
Set micrometer counting oscillations (2)	299840
Readings taken by Lieutenant Nazro (3)	299830
Readings taken by Mr.Clason (5)	299860
Mirror inverted (8)	299840
Speed of rotation, 192 (7)	299990
Speed of rotation, 128 (1)	299800
Speed of rotation, 96 (1)	299810
Speed of rotation, 64 (1)	299870
Radius, 28.5 feet (54)	299870
Radius, 33.3 feet (46)	299830
Highest temperature, 90° Fahr. (5)	299910
Mean of lowest temperatures, 60° Fahr. (7)	299800
Image, good (46)	299860
Image, fair (39)	299860
Image, poor (15)	299810
Frame, inclined (5)	299960
Greatest value	300070
Least value	299650
Mean value	299852
Average difference from mean	60
Value found for π	3.26
Probable error	± 5

ERRORS

The value of V depended on three quantities D, n and ϕ The distance between the two mirrors could be in error either by a false determination of the length of the steel tape used, or by a mistake in the measurement of the distance by the tape. The total error due to D was considered to be at most 0.00004.

The speed of rotation depended on any error in the rate of the standard, any error in the count of the sound beats between the forks and an error in the estimate of the moment when the image of the revolving mirror was at rest. The total error was thought to be less than 0.00002.

The deflection was measured by its tangent where $\tan \phi = \frac{d}{r}$.

Here the total error was considered to be 0.00015.

The final error was considered to be ± 0.00017 corresponding to an error of ± 51 kilometres.

The headings of the columns in the following table of results signify as follows:

t = temperature, Fahrenheit.
B = number of beats of *st* with *ef* per second.
c = correction of *st* for temperature.
ef = rate of "electric" fork.
n = number of turns of revolving mirror per second.
m = micrometer reading of deflected image.
z = micrometer reading of slit.
$d = m - z$.
$\varDelta$ = difference between greatest and least values of d.
e = mean error of one determination of d.
T = value of one turn of micrometer screw in mm.
r = radius.
φ = deflection in seconds.
φ_0 = angular deflections corresponding to $d = 138^{\text{mm}}$.
$\varphi_1 = \varphi_0 - \varphi$.
V = velocity of light in kilometers per second, in air.
S = source of light (s = sun, e = electric light).
no = number of observations.
v = distinctness of image (poor = 1, fair = 2, good = 3).
w = weight of the set of observations.
l = logarithm.

Date.	*t*	B	*c*	*ef*	*n*	*m*	*z*	*d*	*Δ*	*c*	T	φ_1	*la*
Oct. 12	75.0	1.250	—.032	129.127	258.254	138.182	.262	137.920	.15	.038	.99629	0	.5144372
12	75.0	1.333	—.032	129.010	257.871	138.000	.258	137.742	. .	. .	.99629	0	.5144372
12	75.0	1.333	—.032	129.010	258.754	138.500	.267	138.233	. .	. .	.99629	0	.5144372
14	71.0	1.198	.000	129.107	258.214	138.009	.076	137.933	.27	.060	.99629	0	.5144372
16	73.2	1.038	—.017	129.021	258.042	137.927	.027	137.900	.21	.045	.99629	0	.5144372
18	61.5	0.954	+.075	129.029	258.058	137.977	.060	137.917	.19	.049	.99629	0	.5144372
19	56.0	0.988	+.118	129.106	258.212	138.100	.063	138.037	.17	.040	.99629	0	.5144372
19	54.7	1.000	+.129	129.129	258.258	138.130	.063	138.067	.17	.070	.99629	0	.5144372
20	58.0	0.938	+.103	129.041	258.082	137.831	.057	137.774	.25	.056	.99629	0	.5144372
21	64.3	0.983	+.053	129.036	258.072	137.941	.054	137.887	.20	.033	.99629	0	.5144372
24	56.8	0.952	+.112	129.064	258.128	138.068	.058	138.010	.25	.090	.99629	0	.5144372
25	59.0	0.952	+.095	129.047	258.094	137.957	.060	137.897	.09	.032	.99629	0	.5144372
25	59.0	0.952	+.095	129.047	258.094	137.965	.060	137.905	.26	.077	.99629	0	.5144372
26	59.0	0.944	+.095	129.039	258.078	137.931	.058	137.873	.35	.102	.99629	0	.5144372
31	73.0	0.923	—.016	128.907	257.814	137.819	.065	137.754	.12	.035	.99629	0	.5144372
31	73.0	0.923	—.016	128.907	257.814	137.852	.065	137.787	.22	.066	.99629	0	.5144372
Nov. 4	53.0	0.947	+.142	129.089	193.634	103.632	.060	103.572	.20	.055	.99603	11′.6	.5143215
8	56.0	0.936	+.118	129.054	193.581	103.532	.062	103.470	.12	.036	.99603	11′.6	.5143215
8	56.0	0.936	+.118	129.054	193.581	103.534	.062	103.472	.11	.027	.99603	11′.6	.5143215
11	70.5	0.923	+.004	128.927	193.390	103.421	.069	103.352	.09	.027	.99603	11′.6	.5143215
11	70.5	0.923	+.004	128.927	128.927	68.976	.069	68.907	.10	.036	.99585	23′.2	.5142354
14	40.5	0.955	+.241	129.196	129.196	69.115	.045	69.070	.07	.024	.99585	23′.2	.5142354
14	40.5	0.955	+.241	129.196	129.196	69.136	.045	69.091	.11	.036	.99585	23′.2	.5142354

REF(90)

Date.	r	ln	lr	ld	$l \sin \varphi$	φ	$l\varphi$	lV	V	S	no	v	w
Oct. 12	33.350	.4120470	.5230958	.1396272	.1309787	2788.7	.4454018	.4769369	299883	*s*	40	3	7
12	33.350	.4414025	.5230958	.1390664	.1304079	2785.1	.4448408	.4768536	299816	*s*	.	3	5
12	33.350	.4128871	.5230958	.1406117	.1319532	2795.0	.4463818	.4767971	299778	*s*	.	3	5
14	33.350	.4119798	.5230958	.1396681	.1310196	2789.0	.4454485	.4768231	299796	*s*	56	2	3
16	33.351	.4116904	.5231089	.1395643	.1308927	2788.2	.4453239	.4766583	299682	*e*	25	2	5
18	33.356	.4117173	.5231740	.1396178	.1308811	2788.1	.4453083	.4766008	299711	*e*	65	3	4
19	33.354	.4119765	.5231479	.1399956	.1312850	2790.7	.4457132	.4765552	299611	*s*	19	3	6
19	33.356	.4120537	.5231479	.1400899	.1313793	2791.3	.4458065	.4765390	299599	*e*	10	3	2
20	33.355	.4117578	.5231609	.1391421	.1304185	2785.2	.4458564	.4771933	300051	*s*	22	3	3
21	33.355	.4117409	.5231609	.1395234	.1307998	2787.6	.4452305	.4768023	299781	*s*	68	2	9
24	33.355	.4118351	.5231609	.1399106	.1311870	2790.1	.4456198	.4765072	299578	*s*	20	1	1
25	33.356	.4117779	.5231740	.1395549	.1308182	2787.7	.4452460	.4768237	299796	*s*	10	3	10
25	33.356	.4117779	.5231740	.1395800	.1308433	2787.9	.4452772	.4767926	299774	*e*	30	2	2
26	33.355	.4117510	.5231609	.1394792	1307556	2787.3	.4451837	.4768591	299820	*s*	10	1	1
31	33.355	.4113065	.5231609	.1391042	.1303806	2784.9	.4448096	4767888	299772	*s*	15	3	8
31	33.355	.4113065	.5231609	.1392073	.1304837	2785.6	.4449188	4766796	299696	*e*	11	2	2
Nov. 4	33.360	.2869816	.5232260	.0152424	.0063379	2093.0	.3207692	4765040	299573	*s*	30	3	2
8	33.357	.2868627	.5231870	.0148144	.0059489	2091.2	.3203956	.4767588	299748	*s*	20	3	6
8	33.357	.2868627	.5231870	.0148228	.0059573	2091.2	.3203956	.4767588	299748	*e*	46	3	10
11	33.357	.2864340	.5231870	.0143189	.0054534	2088.8	.3198969	.4768288	299797	*v*	20	3	10
11	33.357	.1103430	.5231870	.8382633	.8293119	1392.3	.1437328	.4769030	299851	*e*	20	3	6
14	33.362	.1112401	.5232521	.8392895	.8302728	1395.4	.1446987	.4768422	299809	*s*	6	2	7
14	33.362	.1112401	.5232521	.8394215	.8304048	1395.8	.1448232	.4767177	299723	*e*	20	2	4

SUPPLEMENTARY MEASURES
REF. (90)

Measurement of the velocity of light in air and water.

Because of differences in the results obtained by Michelson and Newcomb for the value of the velocity of light in air it was agreed that Michelson should repeat his experiments. No instructions or suggestions were made to him from the Navy Department except such as related to the investigation of possible sources of error in the application of his method.

The same micrometer was used as before but was now supported (90) on a brick pier. The distance from the surface of the mirror to the slit was obtained and the sine of the deflection was measured instead of the tangent. The same revolving mirror was used but was furnished with new sockets. The fixed mirror was now made slightly concave and had a diameter of 15 inches. The rate of the standard fork was again measured by the use of beats.

The weighted mean of the observations was 299771 kilometres per second with an error of ± 12 kilometres.

The various sources of error were discussed in the previous experiment and thus assuming that these errors affected the result in the same manner, the total error was less than 60 kilometres.

i.e. Value from table 299771

Reduction to vacuum + 82

Final result 299853 ± 60 kilometres/sec.

Young and Forbes in their paper "Velocity of White and (80) Coloured Light" remarked, "In Michelson's observations the image of the slit was described as indistinct and covering a sensible space. From our results it would appear that the width of his spectrum between mean red and blue would be about 2 millimetres. But it would be a very impure spectrum, and it is only by employing absorptive media, or part of a pure spectrum, to give colour to the light used, that we should expect him to detect the difference."

In response to the above, Michelson presented a drawing of the image seen in the eye-piece drawn with a magnification of approximately 5 times. The colour of the central portion was yellowish and both borders on occasion violet. The width of the image was 0.25 m.m. when the slit width was 0.19 m.m.

Michelson suggested that if there were to be a difference of velocity between the red rays and the blue rays, then the image drawn to the same scale would have presented a spectrum covering about 10m.

Further an experiment was conducted in which one-half of the slit was covered with red glass. On observing the two halves of the image, both the upper white and the lower red, were exactly in line.

As a postscript to the above series of experiments, Michelson (15.8.1883) repeated Foucault's experiments to check on the velocity of the wave theory. His apparatus was essentially the same as that of Foucault with distilled water being placed in a tube 10.03 feet long. The distance between the mirrors was 17.63 feet with the "radius" being 32.41 feet, whilst the speed of rotation was 256 turns per second.

The results confirmed Foucault's work which showed that the velocity of light in water was less than in air and further that the ratio between these velocities was equal to the refractive index of the water. This for yellow light at ordinary temperatyre was given as 1.333

Ratio of V/V_1 for 6 independent experiments was

No.	1	2	3	4	5	6	
	1.33	1.33	1.34	1.33	1.35	1.30	Mean: 1.330

Michelson produced a second report on experiments concerned (90) with the velocity of white light in Carbon Disulphide and of the difference of velocities of red and blue light in Carbon Disulphide.

The arrangement of the apparatus was essentially the same as in the previous experiments. In this experiment in order to produce the required "deflection" a column of liquid ten feet long was required. It further proved difficult to obtain a sharp image but this sharpness could be improved by limiting the aperture of the tube by a rectangular opening to sacrifice light.

In the following observations:

r = "radius", or distance from micrometer to revolving mirror

a = length of air column between mirrors

b = length of liquid column between mirrors = 3.07 metres

d = linear displacement of image

m = number of turns per second

n = ratio of velocity of light in liquid to that in air, which last may be taken at V = 300,000,000 metres.

M = 1,000,000

Z = reading of micrometer for undeflected image

D = reading of micrometer for deflected image

36 sets of observations were taken

OBSERVATIONS—Continued.

No. 25.		No. 26.		No. 27.		No. 28.	
Z	D	Z	D	Z	D	Z	D
115.176	114.508	115.170	114.524	115.165	114.350	115.165	114.341
168	516	164	541	. .	368	. .	348
172	508	158	510	. .	371	. .	360
153	505	170	523	. .	370	. .	350
161	520	170	520	. .	354	. .	350
167	511	158	526	. .	359	. .	345
170	521	171	510	. .	360	. .	362
170	510	163	521	. .	360	. .	350
158	519	161	523	. .	354	. .	352
162	499	161	530	114.360	359	114.351	351
115.167	114.512	115.165	114.523	$d = .805$	114.360	$d = .814$	114.351
114.512		114.523		$r = 3.39$		$r = 3.39$	
$d = .655$		$d = .642$		$a = 3.68$		$a = 3.68$	
$r = 3.39$		$r = 3.39$		$m = 320$		$m = 320$	
$a = 3.68$		$a = 3.68$					
$m = 256$		$m = 256$					

No. 29.		No. 30.		No. 31.		No. 32.	
Z	D	Z	D	Z	D	Z	D
115.210	114.378	115.190	114.384	115.176	114.515	115.187	114.356
187	366	. .	386	188	514	. .	374
176	367	. .	397	204	517	. .	360
188	372	. .	370	189	511	. .	371
192	366	. .	382	180	513	. .	370
195	363	. .	380	183	526	. .	362
195	362	. .	385	200	518	. .	369
196	363	. .	388	182	515	. .	358
184	370	. .	386	184	516	. .	351
178	114.367	114.384	384	187	516	114.362	354
115.190		$d = .816$	114.384	115.187	114,516	$d = .825$	114.362
114.367		$r = 3.39$		114.516		$r = 3.39$	
$d = .823$		$a = 3.68$		$d = .671$		$a = 3.56$	
$r = 3.39$		$m = 320$		$r = 3.39$		$m = 320$	
$a = 3.68$				$a = 3.56$			
$m = 320$				$m = 256$			

MICHELSON REF. (90)

The following table gives the data and calculations. The headings of th columns have the same signification as already assigned, P. (3):

	r	a	m	Md	log Md	log m	log r	$\log \frac{Vd}{8\pi mr}$	No	No — a	n
1	6.336	3.61	256	1227	3.08884	2.40824	0.80182	0.95566	9.03	5.42	1.77
2	6.336	3.61	256	1251	3.09726	2.40824	0.80182	0.97406	9.20	5.69	1.85
3	6.336	3.69	256	1256	3.09899	2.40824	0.80182	0.96581	9.24	5.55	1.81
4	6.336	3.69	128	649	2.81224	2.10721	0.80182	0.98009	9.55	5.86	1.91
5	6.336	3.69	256	1227	3.08884	2.40824	0.80182	0.95566	9.03	5.34	1.74
6	6.336	3.69	192	889	2.94890	2.28330	0.80182	0.94066	8.72	5.03	1.63
7	6.336	3.69	128	587	2.76864	2.10721	0.80182	0.93649	8.64	4.95	1.61
8	6.336	3.69	256	1236	3.09202	2.40824	0.80182	0.95884	9.09	5.40	1.76
9	6.336	3.69	192	922	2.96473	2.28330	0.80182	0.95649	9.05	5.36	1.75
10	6.336	3.69	320	1544	3.18865	2.50515	0.80182	0.95856	9.09	5.40	1.76
11	6.38	3.66	256	1231	3.09026	2.40824	0.80482	0.95408	9.00	5.34	1.74
12	6.38	3.66	192	923	2.96520	2.28330	0.80482	0.95396	8.99	5.33	1.74
13	6.38	3.66	160	789	2.89708	2.20412	0.80482	0.96502	9.23	5.57	1.82
14	6.38	3.66	128	647	2.81090	2.10721	0.80482	0.97575	9.46	5.80	1.89
15	3.45	3.64	320	856	2.92942	2.50515	0.53782	0.96333	9.19	5.55	1.81
16	3.45	3.64	320	842	2.92531	2.50515	0.53782	0.95922	9.10	5.46	1.75
17	3.45	3.64	256	667	2.82413	2.40824	0.53782	0.95495	9.02	5.38	1.75
18	3.45	3.64	256	663	2.82151	2.40824	0.53782	0.95233	8.96	5.32	1.73
19	3.45	3.64	256	664	2.82217	2.40824	0.53782	0.95299	8.97	5.33	1.74
20	3.45	3.64	256	659	2.81889	2.40824	0.53782	0.94971	8.91	5.27	1.75
21	3.45	3.64	256	668	2.82478	2.40824	0.53782	0.95560	9.03	5.47	1.75
22	3.45	3.64	256	674	2.82866	2.40824	0.53782	0.95948	9.11	5.47	1.78
23	3.45	3.64	256	676	2.82995	2.40824	0.53782	0.96077	9.14	5.50	1.79
24	3.45	3.64	256	680	2.83251	2.40824	0.53782	0.96333	9.19	5.55	1.81
25	3.39	3.68	256	655	2.81624	2.40824	0.53020	0.95468	9.01	5.33	1.74
26	3.39	3.68	256	642	2.80754	2.40824	0.53020	0.94598	8.83	5.15	1.68
27	3.39	3.68	320	805	2.90580	2.50515	0.53020	0.94733	8.86	5.18	1.69
28	3.39	3.68	320	814	2.91062	2.50515	0.53020	0.95215	8.96	5.28	1.72
29	3.39	3.68	320	823	2.91540	2.50515	0.53020	0.95693	9.06	5.38	1.75
30	3.39	3.68	320	816	2.91169	2.50515	0.53020	0.95322	8.98	5.30	1.73
31	3.39	3.56	256	671	2.82672	2.40824	0.53020	0.96516	9.23	5.67	1.85
32	3.39	3.56	320	825	2.91645	2.50515	0.53020	0.95798	9.08	5.52	1.80
33	3.39	3.56	320	824	2.91593	2.50515	0.53020	0.95746	9.07	5.51	1.80
34	5.14	3.56	192	763	2.88252	2.28330	0.71096	0.96514	9.23	5.67	1.85
35	5.14	3.56	256	1009	3.00389	2.40824	0.71096	0.96157	9.15	5.59	1.82
36	5.14	3.56	320	1225	3.08814	2.50515	0.71096	0.94891	8.89	5.33	1.74

Mean value of n . . . . = 1.77

Weighted mean . . . . = 1.758

MICHELSON REF. (90)

The time t occupied by the light in traversing the distance between the mirrors is

$$t = \frac{a + b\,n}{V} - \frac{d}{8\pi rm} \qquad \text{i.e.} \quad n = \frac{\frac{Vd}{8\pi rm} - a}{b} .$$

The weighted mean of the observations was 1.758 which was about 7% higher than the theoretical value. Michelson found it difficult to account for this considerable difference by attributing it to errors of experiment for the result was fairly independent of the "radius" or of the speed of revolution of the mirror. A series of checking experiments were then performed without using the column of liquid. The result thus obtained for the velocity of light in air had an error of less than 2%

In a second series of experiments the light was passed through a direct-vision prism before reaching the slit. By turning the prism either the red or blue end of the spectrum could be observed. The selected colours were:

λ red = 0.000620 $\qquad$ λ blue = 0.000490

If dr represents the deflection for red light

If db represents the deflection for blue light

$$\text{then } \frac{dr}{db} = \frac{\frac{a}{v} + \frac{bnr}{v}}{\frac{a}{v} + \frac{bnb}{v}} = \frac{a + bnr}{a + bnb}$$

$$nb - nr = 2.8\,\frac{db - dr}{dr}$$

The following observations give the values found for $d_b - d_r$ in hundredths of a millimeter and for d_r in millimeters:

No. 1.		No. 2.		No. 3.	No. 4.	No. 5.	No. 6.	No. 7.
+2.4	3.9	1.8	2.9	+1.7	+ .6	+1.5	+ .4	+1.1
1.9	0.0	3.4	1.9	2.0	.2	.3	.4	1.5
.7	3.4	.7	2.0	1.6	−2.4	.9	1.1	2.3
2.3	3.2	2.6	2.0	.1	.3	1.8	.2	1.9
4.1	− .2	.5	1.7	.6	.9	2.5	.5	2.7
3.1	− .8	2.0	.8	2.7	.2	1.5	− .2	2.5
3.5	.6	4.0	. .	1.5	.9	.1	−1.9	0.0
2.9	5.2	.3	. .	1.1	− .3	+ 1.23	1.7	− .1
− .5	4.4	4.0	. .	3.2	1.7	d_r = 1.55	.7	1.0
8.2	− .2	1.7	. .	0.0	1.8		2.2	1.5
−1.1	− .5	Mean = + 2.02		+ 1.45	.6		.6	.2
−5.3	− .4	d_r = 1.55		d_r = 1.55	+ 0.41		+ 0.52	1.0
1.8	−2.1				d_r = 1.00		d_r = 1.25	1.3
.6	1.1							+ 1.30
3.0	4.1							d_r = 1.25
.8	− .9							
5.2	3.2							
4.3	1.8							
Mean = + 1.89								
d_r = 1.55								

No. 8.		No. 9.	No. 10.	No. 11.		No. 12.		No. 13.	
+ .8	0.0	+1.3	+1.4	+1.0	+ .6	+1.5	+1.2	1.0	1.0
2.1	1.4	3.1	.4	.8	− .6	0.0	1.2	.4	2.1
2.5	2.0	1.7	1.3	1.2	1.4	.5	.3	1.9	− .2
1.7	.2	1.8	.9	1.6	.7	− .4	.7	4.0	1.7
2.2	0.0	2.1	1.3	1.0	− .3	.6	− .4	1.5	2.5
1.2	−1.2	2.2	.4	.7	0.0	1.8	.2	1.0	.8
.1	1.1	.7	.6	.2	.7	1.3	0.0	1.0	. .
.6	1.2	2.4	1.8	1.0	.6	1.3	.8	1.5	. .
.3	−1.9	− .6	−1.8	− .2	.8	1.5	.3	1.0	. .
1.2	− .6	3.2	− .5	0.0	. .	1.6	. .	.5	. .
− .8	1.4	2.2	− .2	+ 0.59		− .2	. .	+ 1.36	
− .1	.4	2.5	+ 0.51	d_r = 0.81		+ 0.83		d_r = 1.24	
1.4	.8	1.7	d_r = 0.82			d_r = 0.83			
1.2	1.2	1.7							
− .3	−1.0	+ 1.86							
.1	.9	d_r = 1.60							
− .6	− .6								
−1.1	. .								
+ 0.51									
d_r = 0.65									

MICHELSON REF (90)

In the following table the results are collected together with the data. The letters have the same signification as before:

	r	m	$d_b - d_r$	d_r	$\frac{d_b - d_r}{d_r}$	$n_b - n_r$
1	6.34	320	.0189	1.55	.0122	0.034
2	6.34	320	.0202	1.55	.0130	0.036
3	6.34	320	.0145	1.55	.0094	0.026
4	6.34	213	.0041	1.00	.0041	0.011
5	6.34	320	.0123	1.55	.0079	0.022
6	6.34	256	.0052	1.25	.0041	0.011
7	6.34	256	.0130	1.25	.0104	0.029
8	6.34	128	.0051	0.65	.0079	0.022
9	6.34	335	.0186	1.60	.0116	0.032
10	3.39	320	.0051	0.82	.0062	0.017
11	3.39	320	.0059	0.81	.0073	0.020
12	3.39	320	.0083	0.83	.0100	0.028
13	5.14	320	.0136	1.24	.0110	0.031
	Mean value $n_b - n_r$				=	0.0245
	Theoretical value (VERDET)				=	0.025

If

$$n = 1.77$$

we have

$$\frac{n_b}{n_r} = 1.014 \text{ or } \frac{V_r}{V_b} = 1.014$$

It would appear, then, notwithstanding the rather wide divergences in the separate observations, that we are entitled to conclude from these experiments that orange-red light travels from one to two per cent. faster than greenish-blue light in carbon disulphide.

MICHELSON REF. (90)

CHAPTER 13

METHOD OF SIMON NEWCOMB

Simon Newcomb was an eminent member of the National Academy of Sciences, being President of the American Association for the Advancement of Science and Director of the Nautical Almanac Office.

A distinguished astronomer considerably older than Michelson he nevertheless had a long association with Michelson which was devoid of any petty jealousies although both men worked in the same field of research.

Newcomb encouraged Michelson and whenever he could he supported him financially or helped with equipment.

Newcomb in the years 1880 - 1882 measured the velocity of light using a revolving mirror. He decided not to measure the linear displacement of the image but rather its angular deviation. Furthermore he did not place a lens between the two mirrors (as did Foucault and Michelson) but instead placed it between the slit and the revolving mirror.

The main feature of the apparatus was that two telescopes (91) were arranged with their axes at right angles. Light from the sun reflected from a heliostat entered the slit S of the sending telescope, where after passing along the tube F was reflected by a plane mirror at C through the object-glass J. The light then fell onto the revolving mirror M and hence along the line Z to the distant fixed mirror. The farther end of the receiving telescope had a pair of microscopes, P and H, for taking readings of the graduated arc. The apparatus was adjusted so that the light reflected from the surface of the revolving mirror was prevented from entering the observing telescope. With this arrangement almost all the extraneous light was shut out and a very faint image of the slit was observed.

The revolving mirror was of interest. It consisted of a square steel prism. The four vertical faces being nickel plated and each face in turn acted as a reflector so that the brightness of the image was quadrupled. Above and below the prism, two sets of fans were attached upon which the air blast acted. Either set could be used separately, so that the mirror could be driven in either direction, or the two could act simultaneously in such a way that one counteracted and controlled the other.

The observing telescope was first set in a fixed position and the speed of the rotating mirror adjusted so that the returning image rested on the micrometer wires in the eyepiece. The regulation of the speed of rotation was achieved by opening and closing valves (T) using an endless cord X. The image could be kept on the cross-wires for two minutes whilst the chronograph furnished the speed of rotation.

It was found that the higher the velocity of the mirror the more steadily the image could be kept upon the wires, and that the steadiness deteriorated very rapidly when the velocity fell below 200 turns per second, thus most of the determinations were made with high speeds of rotation.

The mean result of each day's work is shown. Newcomb combined the separate means with the distances travelled and produced the following results for the velocity of light in air expressed in kilometres per second:

Observatory, 1880-81 Distance = 5101.90m Time = 0.0000170282sec
V = 299,615

Monument, 1881 Distance = 7442.42m Time = 0.0000248344sec
V = 299,682

Monument 1882 Distance = 7442.42m Time = 0.0000248275sec
V = 299,766

He considered that the differences of these results far exceeded the probable errors arising from the accidental differences between the separate daily means.

After lengthy discussion over errors and various consultations with Michelson, Lord Rayleigh and Cornu, Newcomb considered that the results should depend entirely on the measurements taken in 1882, thus:

Immediate result of measurement	V = 299,766 km/sec
Correction for curvature of mirror	+ 12
Reduction to vacuo	82
Concluded velocity in vacuo	V = 299860 km/sec

The results so far are as follows:

Michelson, Naval Academy, 1879	299910 km/s
Michelson, Cleveland, 1882	299853 km/s
Newcomb, Washington (selected results) 1882	299860 km/s
Newcomb, Washington (all results) 1882	299810 km/s
Foucault, Paris, 1862	298000 km/s
Cornu, Paris, 1874	298500 km/s
Cornu, Paris, 1878	300400 km/s
Young and Forbes, Scotland, 1880/81	301382 km/s

Newcomb made no special arrangements for detecting differences between the velocities of differently coloured rays. However, whilst his experiments were in progress he learnt of the work of Young and Forbes who had announced a detection of a velocity difference of 2 per cent. As a result he made a very careful examination of the return image. Had there been a difference in velocity of 1/1000, the resulting spectrum would have been 15" in breadth, and have had a well marked iridescence on its edges. No such observations could be detected.

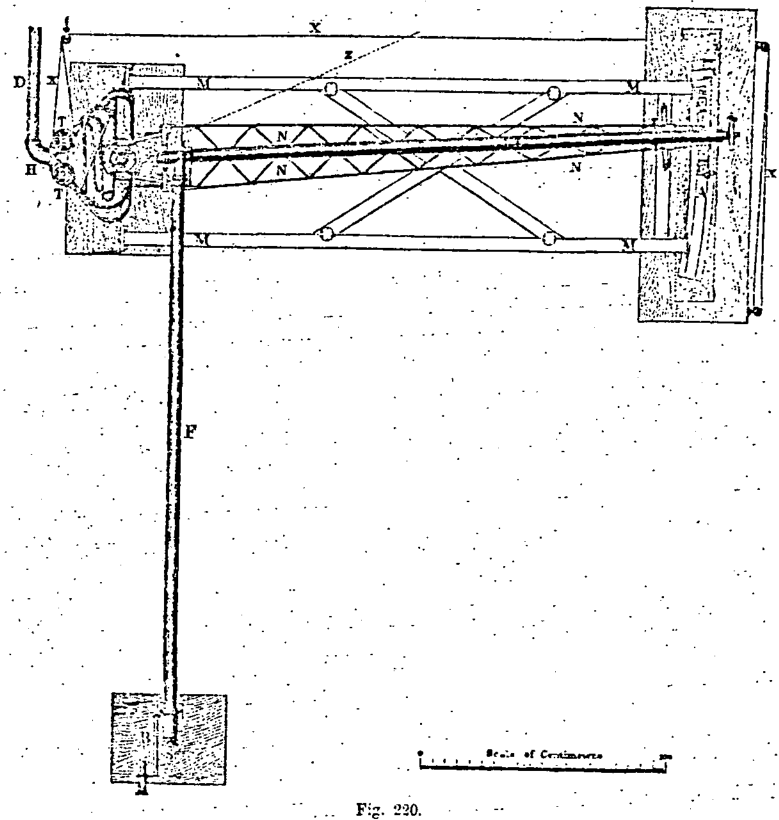


Fig. 220.

CHAPTER 14

MICHELSON AND INTERFEROMETRY

At the beginning of Michelson's first paper on aether-drift phenomena he stated his hypothesis as follows:

"The undulatory theory of light assumes the existence of a medium called the ether, whose vibrations produce the phenomena of heat and light, and which is supposed to fill all space. According to Fresnel,(i) the ether, which is enclosed in optical media, partakes of the motion of those media, to an extent depending on their indices of refraction. For air, this motion would be but a small fraction of that of the air itself and will be neglected.

Assuming then that the ether is at rest, the earth moving through it, the time required for light to pass from one point to another on the earth's surface, would depend on the direction in which it travels." (92)

He followed this by showing the mathematical feasibility of measuring the speed of two pencils of light travelling at right angles to each other and finally proposed that "We could find V the velocity of the earth's motion through the ether." (88)

Prior to the experiments of 1881, Michelson was given leave of absence from active duty and set off to obtain his post-graduate education in Europe.

Michelson was acquainted with the work of J.Clerk Maxwell(ii) on Aether and relative motion of the aether. Maxwell had made explicit the notion that the relative aether wind might

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(i) Augustin Jean Fresnel (1788 - 1827) Tutor at the École Polytechnique

(ii) James Clerk Maxwell (1831 - 1879) Professor of Physics at Cambridge possibly be used to determine the absolute velocity through space.

Maxwell assumed that light and electricity travelled with the same speed and were both vibrant disturbances in the aether. As Maxwell continued to work on his electromagnetic equations it became apparent that the value of (93) the speed of light represented an important relationship between electricity and magnetism.

In 1879 Maxwell wrote to D.P. Todd,(i) the Director of the Nautical Almanac Office. In the letter Maxwell considered that it would be impossible to measure "a quantity depending on the square of the ratio of the earth's velocity to that of light". (94)

He had already observed that the velocity of light in aether, accelerated by the earth's motion in orbit, would differ by an extremely small amount from its speed in an aether at rest. Looking at the order of the square of this ratio i.e. one part in one hundred million, he considered it would be too small to measure.

However, Michelson thought otherwise. All the experiments to detect aether drift up to that time had attempted to measure the ratio of the speed of the earth to that of light. Since each of these experiments had failed it was accepted that no first order effect could succeed. Michelson by this time had an advantage over the other workers in this field in that he had a refined value for the speed of light and was able to contemplate a second-order effect : the square of the speed of light, C^2, in relation to the square of the earth's speed, V^2. (95)

(i) David P.Todd (1855 - 1939) Professor of Astronomy at Smith College.

He began thinking about devising an instrument that could count and measure light waves with an accuracy far beyond what had been so far attained.

Michelson, having obtained leave of absence, enrolled at the University of Berlin in 1880. Here, having obtained a grant from the Volta Foundation he began designing his instrument. Using optical flats from Schmidt and Haensch he built what he called an interferential refractometer, which by the 1890's had become known as an interferometer.

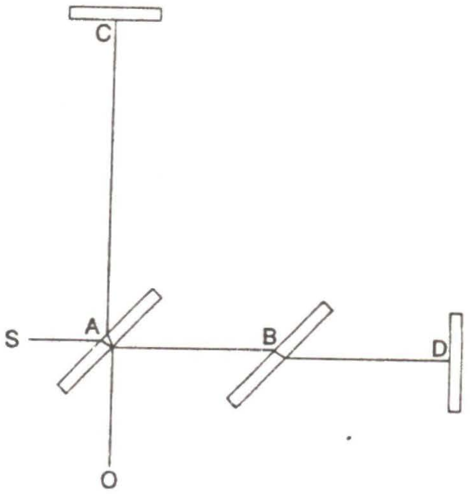
He was considering projecting a beam of light in the direction in which the earth was travelling in its orbit, and one at right angles to this. The first beam, he thought, would naturally be retarded by the flow of aether passing the earth. The second beam, crossing this current at right angles, although the distance is the same, should arrive ahead of the first by a length of time determined by the velocity of the earth.

Michelson was familiar with an instrument designed by Jamin(i) used to measure the refractive indices of gases (96) by the interference of light waves. Michelson rearranged the pieces into the shape of a cross and placed the 'beam splitter', a half-silvered mirror, in the centre. This half-silvered mirror allowed some light to penetrate it, whilst the rest was reflected to the plane mirrors, which in turn, brought the two separate pencils of light together again at the eyepiece.

Observations had to be made during the night as traffic vibrations caused an immediate shift of the fringes.

(i) Jules Célestin Jamin (1818 - 1886 Director of the Physical Laboratory in Paris. Professor of Physics at the Sorbonne.

Helmholtz[(i)] acting on Michelson's behalf arranged with H.C. Vogel[(ii)] the director of the Astrophysikalische Observatory at Potsdam to have all the apparatus moved to the observatory which was a much more secluded and quiet place for fringe observations.



Michelson obtained the conditions for producing interference of two pencils of light which had traversed paths at right angles to each other as follows: (97)
Light from a lamp (S) was passed through the optical flat (A), part going to the mirror (D), and part being reflected to the mirror (C). The mirrors C and D were plane and silvered on the front surface. From these mirrors the light was reflected to (A), where one was reflected and the other refracted, the two coinciding along AO. The distance AD = AC with a glass plate (B) being placed in the path of the ray AD to compensate for the thickness of the glass A, which is traversed by the ray AC, the two rays therefore travel over equal paths and thus

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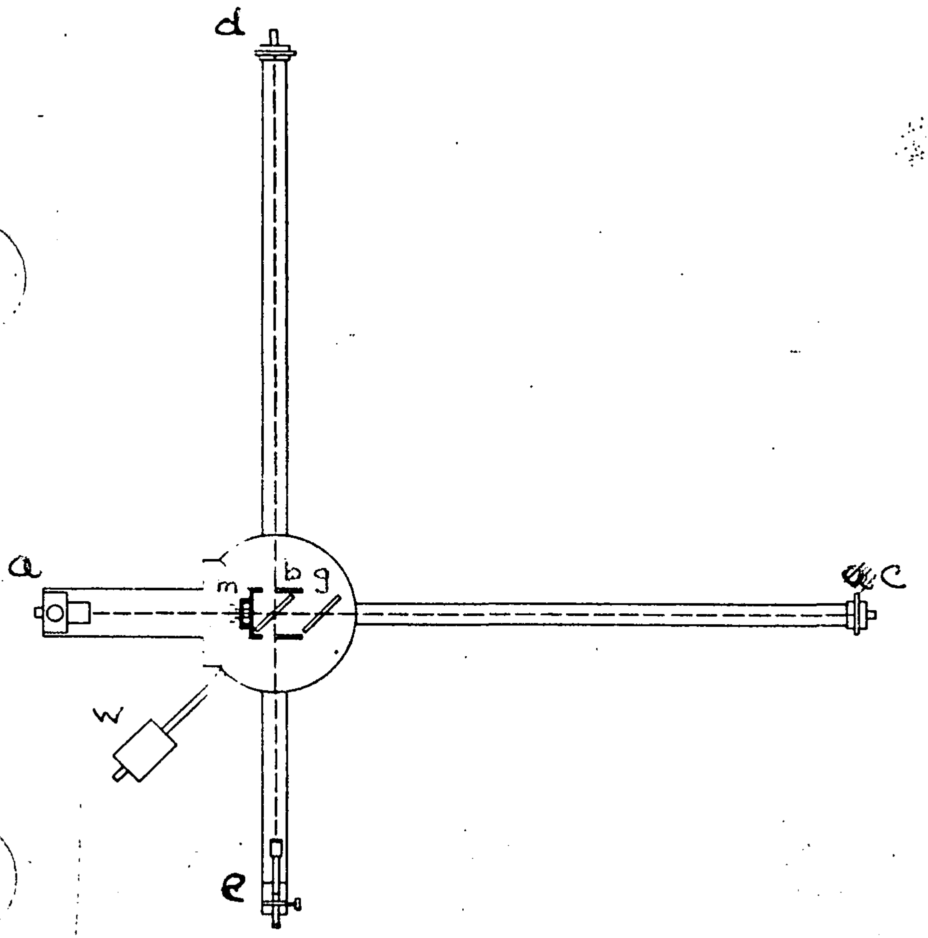
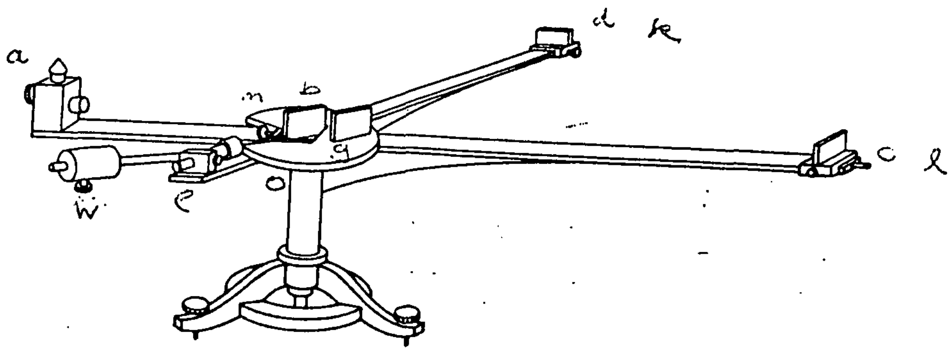
(i) Hermann von Helmholtz (1821 - 1894) Professor of Physics at Heidelberg.

(ii) Hermann Carl Vogel (1841 - 1907) Director of the Potsdam Observatory.



Michelson's first interferometer, 1880
(*Above*) A bird's-eye view; (*opposite*) in perspective
(Adapted from Michelson, "The Relative Motion of the Earth and the Luminiferous Ether," 1881)



are able to cause interference patterns.

The instrument is represented in plan and in perspective.

a = source of light (small lantern)
b/g = two glass plates (cut from the same piece of glass)
d/c = silvered glass mirrors
m = micrometer screw
e = observing telescope with micrometer eyepiece
w = counterpoise

The arms bd, bc, were covered by long paper boxes to guard against temperature changes. They were supported by pins (k, l,) and by a circular plate (o).

The apparatus was adjusted by moving the mirrors C and D as close as possible to the plate b and then using the screw (m) and a compass the distances bc and bd were made approximately equal. Next, using the lamp as a point source of light, b was moved until the two images of the point source coincided. With a sodium flame placed at (a) interference bands were observed which by moving b could be adjusted for width and sharpness. The lamp was then replaced and (m) turned till the bands reappeared.

At the time of the experiment, the earth's orbit coincided approximately in longitude with the estimated direction of the motion of the solar system. The direction of this motion was at an angle of + 26° to the plane of the equator, and the tangent of the earth's motion in its orbit made an angle of - 23½° with the plane of the equator; thus the resultant would lie within 25° of the equator. The nearer the two components were in magnitude to each other, the more nearly would their resultant coincide with the plane of the equator.

In this case, were the apparatus to be placed so that the arms pointed north and east at noon, then the arm pointing east would coincide with the resultant motion, and the other would be at right angles. Therefore, on rotation through 90°, the fringe displacement should be twice 8/100 or 0.16 of the distance between fringes. If, on the other hand, the proper motion of the sun be small compared to the earth's motion, the displacement should be 6/10 of 0.08 or 0.048. Taking the mean as the most probable displacement, Michelson looked for a displacement of 1/10 the distance between the fringes.

He was worried about temperature changes causing fringe displacement (hence the boxes) but a major error was displacement due to the bending of the arms during rotation. This proved to be so bad that the apparatus had to be returned to the makers to improve the ease of rotation. Even so a large displacement was observed in one particular direction which was due to the support. When the table of results was produced, the headings of the columns gave the direction to which the telescope pointed with the erroneous column being marked with an (x). The numbers in the columns being the positions of the centre of the dark fringes in twelfths of the distance between the fringes.

The result of the discussion on the results (shown) was that there was no displacement of the interference bands. The result of the hypothesis of a stationary ether was found to be incorrect and the conclusion that followed was that the hypothesis was erroneous.

	N.	N.E.	E.	S.E.	S.	S.W.	W.	NW.	Remarks.
1st revolution	0·0	0·0	0·0	−8·0	−1·0	−1·0	−2·0	−3·0	Series 1, footscrew marked B, toward East.
2d "	16·0	16·0	16·0	9·0	16·0	16·0	15·0	13·0	
3d "	17·0	17·0	17·0	10·0	17·0	16·0	16·0	17·0	
4th "	15·0	15·0	15·0	8·0	14·5	14·5	14·5	14·0	
5th "	13·5	13·5	13·5	5·0	12·0	13·0	13·0	13·0	
	61·5	61·5	61·5	x	58·5	58·5	56·5	54·0	
S.	58·5	W.	56·5		N.E	61·5	S.E.	60·0	
	120·0		118·0			120·0		114·0	
	118·0					114·0			
Excess,	+2·0					+6·0			
1st revolution	10·0	11·0	12·0	13·0	13·0	0·0	14·0	15·0	Series 2, B toward South.
2d "	16·0	16·0	16 0	17·0	17·0	2·0	17·0	17·0	
3d "	17·5	17·5	17·5	17·5	17·5	4·0	18·0	17·5	
4th "	17·5	17·5	17·0	17·0	17·0	4·0	17·0	17·0	
5th "	17·0	17·0	17·0	17·0	16·0	3·0	16·0	16·0	
	78·0	79·0	79·5	81·5	80·5	x	82·0	82·5	
S.	80·5	W.	82·0		N.E.	79·0	S.E.	81·5	
	158·5		161·5			160·0		164·0	
	161·5					164·0			
Excess,	−3·0					−4·0			
1st revolution	3·0	3·0	3·0	3·0	2·5	2·5	2·5	10·0	Series 3, B toward West.
2d "	18·0	17·5	17·5	18·0	18·5	19·0	19·5	26·0	
3d "	11·0	11·0	13·0	12·0	13·0	13·5	13·5	21·0	
4th "	1·0	0·0	0·5	0·5	0·5	0·0	0·0	14·0	
5th "	4·0	4·0	5·0	5·0	5·0	5·5	5·5	16·0	
	37·0	35·5	39·0	38·5	39·5	40·5	71·0	x	
S.	39·5	W.	41·0		N.E.	35·5	S E.	38·5	
	76·5		80·0			76·0		79·5	
			76·5					76·0	
Excess,			+3·5					+3·5	
1st revolution	14·0	21·0	15·5	17·0	14·0	14·5	14·5	16·0	Series 4, B toward North.
2d "	10·0	20·0	12·0	12·0	13·0	13·0	13·0	13·5	
3d "	14·0	25·0	15·0	16·0	16·0	16·0	16·0	17·0	
4th "	18·0	27·0	18·5	18·5	18·5	19·0	20·0	21·0	
5th "	15·0	24·0	15·0	15·0	15 0	16·0	16·0	16·5	
	71·0	x	76·0	78·5	76·5	78·5	79·5	84·0	
S.	76·5	W.	79·5		N.E.	73·5	S·E.	78·5	
	147·5		155·5			152·0		162·5	
			147·5					152·0	
Excess,			+8·0					+10·5	

In the first two series, when the footings of the columns N. and S. exceed those of columns E. and W., the excess is called positive. The excess of the footings of N.E., S.W., over those of N.W., S.E., are also called positive. In the third and fourth series this is reversed.

The numbers marked "excess" are the sums of ten observations. Dividing therefore by 10, to obtain the mean, and also by 12 (since the numbers are twelfths of the distance between the fringes), we find for

	N.S.	N.E., S.W.
Series 1	+0·017	+0·050
" 2	−0·025	−0·033
" 3	+0·030	+0·030
" 4	+0·067	+0·087
	4) 0·089	0·137
Mean =	+0·022	+0·034

The displacement is, therefore,

In favor of the columns N.S. .... +0·022
" " " N.E., S.W. .... +0·034

The former is too small to be considered as showing a displacement due to the simple change in direction, and the latter should have been zero.

The numbers are simply outstanding errors of experiment. It is, in fact, to be seen from the footings of the columns, that the numbers increase (or decrease) with more or less regularity from left to right.

This gradual change, which should not in the least affect the periodic variation for which we are searching, would of itself ecessitate an outstanding error, simply because the sum of the wo columns farther to the left must be less (or greater) than the sum of those farther to the right.

This view is amply confirmed by the fact that where the excess is positive for the column N.S., it is also positive for N.E., S.W., and where negative, negative. If, therefore, we can eliminate this gradual change, we may expect a much smaller error. This is most readily accomplished as follows:

Adding together all the footings of the four series, the third and fourth with negative sign, we obtain

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
31·5	31·5	26·0	24·5	23·0	20·8	18·0	11·0

or dividing by 20×12 to obtain the means in terms of the distance between the fringes,

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
0·131	0·131	0·108	0·102	0·096	0·086	0·075	0·046

If x is the number of the column counting from the right and y the corresponding footing, then the method of least squares gives as the equation of the straight line which passes nearest the points x, y—

$$y = 9{\cdot}25x + 64{\cdot}5$$

If, now, we construct a curve with ordinates equal to the difference of the values of y found from the equation, and the actual value of y, it will represent the displacements observed, freed from the error in question.

These ordinates are:

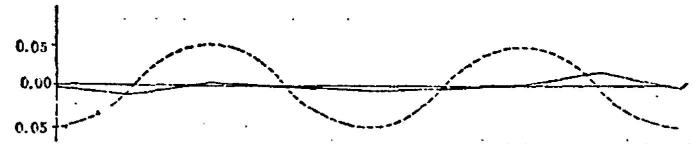
N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
−·002	−·011	+·003	−·001	−·004	−·003	−·001	+·018

N.	−·002	E.	+·003	N.E.	−·011	N.W.	+·018
S.	−·004	W.	−·001	S.W.	−·003	S.E.	−·001
Mean=	−·003		+·001	Mean=	−·007		+·008
	+·001				+·003		
Excess=	−·004			Excess=	−·015		

The small displacements −0·004 and −0·015 are simply errors of experiment.

The results obtained are, however, more strikingly shown by constructing the actual curve together with the curve that should have been found if the theory had been correct. This is shown in fig. 4.

4.



The dotted curve is drawn on the supposition that the displacement to be expected is one-tenth of the distance between the fringes, but if this displacement were only $\frac{1}{100}$, the broken line would still coincide more nearly with the straight line than with the curve.

CHAPTER 15

PERROTIN'S EXPERIMENTS 1902

In a communication on the 24th November, 1902, Perrotin(i) published a new series of measurements on the speed of light using the toothed wheel method developed by Fizeau. The stations used for the experiments were the dome of the Observatory at Nice and Mount Vinaigre in Estérel, being separated by a distance of 46 km.

The preliminary results measured over a distance of 12 km (98) and which were published in Comptes Rendus (15th November, 1900, Volume 131, page 731) gave some idea of the difficulties with the instrumentation and the atmospheric conditions and particularly with refraction effects.

Perrotin used an objective lens of 76 cm. diameter for the emission telescope and a 38 cm. for the diameter of the collimator, He was able to submit 1100 measurements obtained in very variable atmospheric conditions.

For the results see table.

He deduced that the velocity of light in vacuo was 299,860 ± 80 km/sec.

This result differed a little from that which he obtained from 1,500 observations using a station at Gaude, i.e. 299,900 km/sec.

(i) Joseph Perrotin (1845 - 1903) Director of the Bureau of Longitudes.

ORDER	Speed in vacuo km/sec.	Number of Observations	Weighting
XVI	300520	30	288
XVII	299720	35	381
XVIII	299600	32	392
XIX	300310	39	534
XX	300130	76	1156
XXI	299550	66	1109
XXII	299880	41	758
XXIII	299580	75	1519
XXIV	299860	86	1900
XXV	300030	141	3385
XXVI	299890	80	2081
XXVII	300240	49	1376
XXVIII	299720	48	1452
XXIX	300380	36	1170
XXX	300520	52	1810
XXXI	299730	76	2828
XXXII	299500	147	5834

The final value calculated from all the observations made was 299,880 km/sec ± 50 km.

CHAPTER 16

MICHELSON: THE EXPERIMENTS OF 1924

Michelson was invited by Dr. G.E. Hale(i), then Director of the Mount Wilson Observatory, to make a series of investigations for a more accurate determination of the velocity of light.

Hale had attended Michelson's lectures in 1888 on the application of the interferometer to astronomy. Here his imagination had been fired by the possibilities of such a device and he held Michelson's ability as an experimental scientist in high esteem.

He became a close friend of Michelson as well as a most supportive colleague.

Hale became Head of the National Research Council and spent several years trying to persuade Michelson to leave Chicago University and come to live in Pasadena where the new Observatory was being constructed.

Eventually, in 1920, Michelson agreed and moved to Pasadena, dividing his time between the California Institute of Technology and the Mount Wilson Observatory.

Michelson considered that a new determination of the velocity should be made hoping to obtain an accuracy from ten to twenty times better than that obtained in his previous work.

This constant he felt was not only of theoretical importance in Physics and in Astronomy, but may have an immediate bearing on the work of the Coast Survey in furnishing a means of measuring distances which may

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(i) George Ellery Hale (1868 - 1938) Director of the Mount Wilson Observatory.

furnish a valuable check on the results of trigonometric surveys. (99)

The summers of 1921, 1922 and 1923 were spent in trying to obtain the best conditions for such a series of observations.

Two stations were selected for distance of separation coupled with maximum visibility of the return image. The stations being Mount Wilson and Mount San Antonio which was about 22 miles away. This meant that it took the light 0.00023 seconds to complete the return journey. During this time an octagonal revolving mirror making 530 turns per second would rotate through $\frac{1}{8}$ of a turn, thus presenting the succeeding face to the return light at the same angle as though it were at rest.

The speed was then obtained by stroboscopic comparison with an electric fork making 132.25 vibrations per second, the fork being compared with a free seconds pendulum with the latter being compared with an invar gravity pendulum. When the reflections of the revolving mirror in the mirror attached to the fork were stationary the very small angle a_1 to the zero was measured.

The direction of rotation could be reversed and a new angle a_2 measured thus eliminating the measurement of zero.

It was shown that $V = \frac{16\ n\ D}{1 - \beta}$

where $\beta = \frac{a_1 - a_2}{\pi}$, n = number of rotations per second and D = length of the light path.

The final arrangement of the apparatus at the home station (100) is shown. This arrangement allowed the final reflection from the octagon to take place at nearly normal incidence which eliminated direct reflections as well as diffuse light.

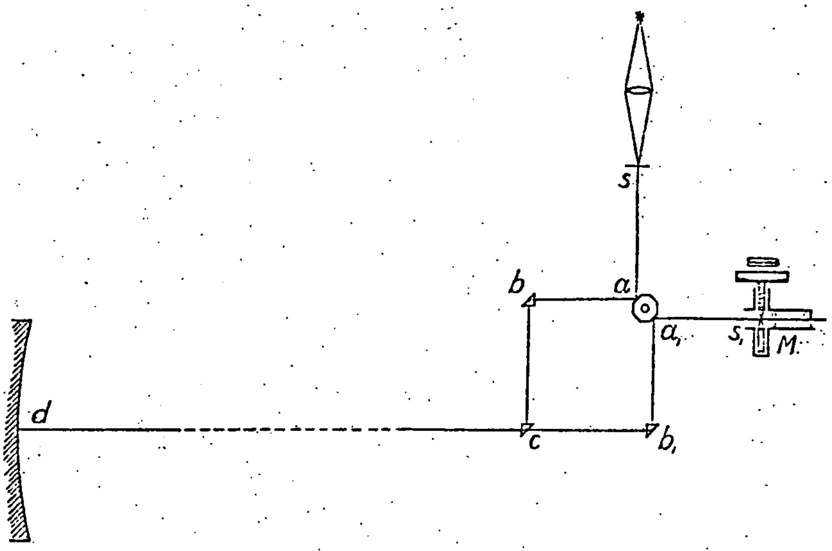

FIG. 1.

TABLE I.

Date	P_t	N/P_t	$1/C$	a_1-a_2/r	K
August 4........	1.00630	132.16	+0.07	+0.00020	132.20
August 5........	1.00630	132.16	−0.03	−0.00060	132.21
August 7........	1.00622	132.17	.04	.00054	132.20
August 8........	1.00628	132.16	.01	.00070	132.24
August 9........	1.00633	132.16	.01	.00040	132.20
August 9........	1.00633	132.16	.01	.00050	132.22
August 10........	1.00635	132.16	00	.00020	132.19
August 10........	1.00635	132.16	−0.05	−0.00030	132.15
Mean........	....	....	....	....	132.20±.006

$V=64KD$

$D=35385.5+40.8$[2]$=35426.3$ m.

Final result:

$V_1=299735$ km per sec. in air

$V=299820$ km per sec. *in vacuo*

[1] This measurement was carried out by Lieutenant Donal Pheley, of the United States Coast and Geodetic Survey.

[2] 35385.50 = distance between C.G.S. marks

30 ft. = focal length of mirrors

12 ft. (provisional) = distance from C.G.S. marks to focus of mirrors

$4\times 30+2\times 12=144$ ft. $=44$ m

Correction $\underline{3.2}$

40.8m

The light source was an arc, which was focussed onto the slit (S). The light then fell onto the face (a) of the octagon where it was reflected to a right angled prism (b) then to another at (c) and hence to the concave mirror (d) which had a 30 foot focus and 24 inch aperture. This mirror reflected the light as a parallel beam to the distant mirror. The light then went to a small concave reflector at its focus. An image of the slit was formed at the face of this small reflector, which allowed the light to return to the concave mirror at (d) where it passed over the prism at (c) to (b_1) where it was reflected onto a face of (a_1) of the octagon forming an image at (S_1) where it was observed by a micrometer eyepiece (M).

The rate of the electric fork in terms of the free auxiliary pendulum was measured by counting the number of seconds required for a complete cycle. If P_1 was the period of the auxiliary pendulum and C the number of seconds in the cycle then Michelson showed that the number of variations of the fork per second was

$$N = \frac{N_1}{P_1} + \frac{1}{C}$$

where N_1 was the nearest whole number (133) of vibrations in one swing of the pendulum.

The auxiliary pendulum acted as a make and break switch in the primary circuit of an induction coil, which gave a spark in the secondary, the spark being observed in a mirror attached to the fork. The Sperry(i) arc was then activated

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(i) Elmer Ambrose Sperry (1860 - 1930) Chairman of the Sperry Group of Companies.

and focussed on the slit. After adjustment the return light could be observed in the eyepiece as a brilliant starlike image. The air blast regulator was then opened until at about 40cm of mercury pressure, the image reappeared in the field. The speed was then regulated until the stroboscopic images (4 images of the polished facet) were just stationary. At this point the crosshairs of the eyepiece were adjusted to bisect the image. The observations were repeated 5 - 10 times, then the direction of rotation reversed and a similar set of observations taken. The difference between the means of the two sets divided by the distance r (crosshair to the face of the mirror) would give the angle $a_1 - a_2$.
The results are shown in the Table.
Michelson considered these to be provisional and to be correct to within one part in ten thousand.
The main source of error he considered to be in the inability to maintain a sufficiently constant speed of the rotating mirror. He considered this due to the lack of constant pressure of the air blast and not to any lack of precision in the measurements of the displacement of the image.

CHAPTER 17

MICHELSON: THE MOUNT WILSON/MOUNT SAN ANTONIO EXPERIMENTS

The preliminary experiments of 1924 gave a corrected value (for vacuum) of 299,802 km/sec.

A second series of experiments using the glass octagon was started in July 1925. The main difference being that the fork ($N \simeq 528$) was driven by a vacuum-tube circuit, which gave a much more constant rate than the previous make/break arrangement.

The above rate was measured by comparison with a free pendulum using an improved stroboscopic method.

As in the 1924 experiments, the octagonal mirror made 528 turns per second and rotated through $\frac{1}{8}$ of a turn during the time it took the light to travel from the revolving mirror to the distant station and return. Thus it presented the succeeding face of the mirror to the returning beam at (very nearly) the same angle as at rest.

The speed of the revolving mirror was increased until the stroboscopic image between the fork and the mirror was stationary. At this point the small angle a_1 was measured, being the angle of displacement by which the image differed from 90°. The direction of rotation was now reversed and a new angle a_2 was measured.

If $a = a_1 + a_2$, then the angle through which the mirror rotates during the time it took the light to travel the distance 2D will be

$\pi/4 - a/4$ with the velocity given by $V = \dfrac{16ND}{(1 - a/\pi)}$ (101)

It was calculated that $V = \frac{16D}{1-\nu} (N + n) (1 + \frac{a}{\pi})$

where 1/n was the period of the (optical) beats between the fork and the pendulum

where $1/\nu$ that of the coincidences between the C.G.S. pendulum and true seconds

where N = 528

Since α and n were small, then Michelson was able to write

$$V = \frac{16 \times 35425.15 \times 528}{1 - 0.00051} \quad (1 + \frac{a}{\pi} + \frac{n}{N})$$

$$D = 35425.1$$

$$V = 299425 (1 + \frac{\alpha}{\pi} + \frac{n}{N})$$

See Table I for results.

Michelson considered these to be preliminary results along with those from the 1924 experiments. The definite results coming between June and September 1926.

By a slight rearrangement of the apparatus at the observing station Michelson was able to achieve an increase in intensity as well as greater symmetry.

Using the improved layout and the small glass octagon a series of results was obtained, see Table II

(The numbers given being the means of three series of observations with each series containing six (double) observations).

It was stated that:

$a = a/\pi \qquad b = n/N \quad$ and $\quad c = \nu$

i.e. $V = 16DB (1 = a - b = c)$

$D = 35,425$ and $N = 528$

$V = 299,270 + V(a + b + c)$

As a result of a large glass octagon bursting during high speed rotation in 1925 a total of four mirrors were constructed. Two of these were glass but twice the size of the small octagon. The first having twelve facets and the second sixteen facets. The two other mirrors were constructed of nickel steel with eight and twelve facets. The driving power in all measurements was by air blast.

Using the glass twelve faceted mirror and the 1924 layout it was shown that V = 299,265 + 3 (a + b + c).
see Table III.

Table IV shows the results for the glass sixteen faceted mirror where

$$V = \frac{32DN}{1 + \frac{2a}{\pi}} \qquad D = 35424.5 \qquad V = 55\text{cm.}$$

The distance between Mount Wilson and San Antonio Peak was measured by William Bovis who at the time was in charge of the Geodesy Division of the U.S. Survey.

TABLE I

TEN SERIES OF OBSERVATIONS

	a/π	n/N	V_a
I	0.00077	0.00013	299,695
II	.00057	.00015	299,651
III	.00044	.00038	299,671
IV	.00037	.00047	299,677
V	.00054	.00045	299,722
VI	.00047	.00043	299,695
VII	.00032	.00068	299,725
VIII	.00017	.00070	299,686
IX	.00018	.00076	299,707
X	0.00021	0.00058	299,662
Mean velocity in air			299,689
Correction			+67
V in vacuo			299,756

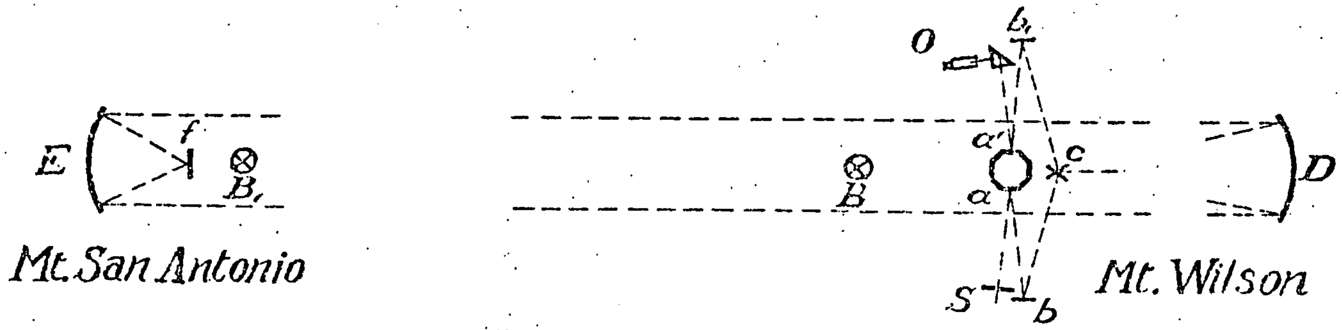

FIG. 1.—Arrangement of apparatus.

TABLE II

	a	*b*	*c*	*V*	Wt.
I	0.00059	0.00028	0.00072	299,747	2
II	.00046	.00040	.00073	299,747	2
III	.00045	.00038	.00073	299,738	3
IV	.00057	.00036	.00072	299,762	3
V	.00047	.00033	.00073	299,729	3
VI	.00052	.00038	.00073	299,759	3
VII	.00061	.00041	.00073	299,792	1
VIII	.00048	.00038	.00072	299,744	4
IX	.00049	.00036	.00072	299,741	4
X	.00047	.00040	.00072	299,747	4
XI	.00044	.00042	.00072	299,744	4
XII	0.00042	0.00042	0.00073	299,741	4
Weighted mean	....	....	....	299,746	....
Correction	....	....	....	+67	....
Velocity *in vacuo*	....	....	....	299,813	....

TABLE III

Twelve-Facet Glass Mirror

	a	*b*	*c*	*VS*	*V*	Wt.
I	−0.00018	0.00100	0.00075	471	299,736	1
II	.00047	.00040	.00073	480	299,745	3
III	.00058	.00026	.00073	471	299,733	3
IV	.00022	.00061	.00072	465	299,730	3
V	.00012	.00062	.00071	435	299,700	1
VI	− .00007	.00088	.00073	462	299,727	5
VII	.00020	.00098	.00073	453	299,718	5
VIII	.00004	.00085	.00073	462	299,727	5
IX	.00009	.00100	.00073	492	299,757	1
X	− .00021	.00114	.00074	501	299,766	2
XI	.00050	.00037	.00074	483	299,748	2
XII	.00071	.00009	.00073	459	299,724	5
XIII	.00052	.00034	.00073	477	299,742	5
XIV	.00003	.00073	.00075	453	299,718	5
XV	0.00004	0.00071	0.00075	450	299,715	5
Weighted mean	....	....	....	....	299,729	....
Correction	....	....	....	....	+67	....
Velocity *in vacuo*	....	....	....	....	299,796	....

TABLE IV

	a	*b*	*c*	*VS*	*V*	Wt.
$\mathrm{I_{I}}$	0.00159	−0.00089	0.00076	438	299,703	....
$\mathrm{I_{II}}$	.00115	− .00033	.00076	474	299,739	
I	$(2\times\mathrm{I_{II}}+\mathrm{I_{I}})/3$	....	....	....	299,727	1
II	.00051	.00045	.00071	501	299,766	2
III	.00038	.00052	.00071	483	299,748	2
$\mathrm{IV_{I}}$	.00006	.00073	.00071	450	299,715	....
$\mathrm{IV_{II}}$	.00079	− .00006	.00073	438	299,703	
IV	$(2\times\mathrm{IV_{II}}+\mathrm{IV_{I}})/3$	....	....	....	299,707	5
V	.00090	− .00009	.00073	462	299,727	5
$\mathrm{VI_{I}}$	.00111	− .00029	.00072	462	299,727	....
$\mathrm{VI_{II}}$	.00074	.00013	.00072	477	299,742	
VI	$(2\times\mathrm{VI_{II}}+\mathrm{VI_{I}})/3$	....	....	....	299,737	4
VII	.00085	.00011	.00072	504	299,769	3
VIII	.00097	− .00013	.00069	459	299,724	2
IX	.00104	.00008	.00070	498	299,763	2
X	.00081	.00002	.00071	450	299,715	4
XI	.00091	.00007	.00071	465	299,730	4
XII	.00112	.00027	.00069	462	299,727	2
XIII	.00117	.00027	.00069	477	299,742	2
XIV	.00098	.00009	.00070	477	299,742	3
XV	0.00104	−0.00009	0.00070	495	299,760	3
Weighted mean	....	....	....	....	299,736	....
Correction	....	....	....	....	+67	....
Velocity *in vacuo*	....	....	....	....	299,803	....

TABLE V

	a	*b*	*c*	*VS*	*V*	Wt.
I	0.00067	0.00029	0.00071	501	299,766	1
II	.00019	.00060	.00073	456	299,721	5
III	.00020	.00061	.00073	462	299,727	5
IV	.00053	.00029	.00074	468	299,733	1
V	.00039	.00034	.00075	444	299,709	5
VI	.00049	.00029	.00075	459	299,724	5
VII	.00066	.00009	.00073	444	299,709	2
VIII	.00064	.00010	.00073	441	299,706	2
IX	.00078	.00006	.00074	474	299,739	3
X	.00102	− .00015	.00072	477	299,742	3
XI	.00053	.00025	.00073	453	299,718	3
XII	.00059	.00018	.00072	447	299,712	1
XIII	0.00071	0.00023	0.00072	498	299,763	1
Weighted mean					299,722	
Correction					+67	
Velocity *in vacuo*					299,789	

A series of measurements with the steel twelve-facet mirror gave the results shown in Table VI.

TABLE VI

	a	*b*	*c*	*VS*	*V*	Wt.
$\mathrm{I_I}$	0.00063	0.00016	0.00072	453	299,718	
$\mathrm{I_{II}}$	.00030	.00046	.00072	444	299,709	
I	$(2\times\mathrm{I_{II}}+\mathrm{I_I})/3$				299,712	2
II	.00040	.00043	.00072	465	299,730	4
III	.00051	.00032	.00072	465	299,730	4
IV	.00052	.00030	.00072	462	299,727	5
V	.00052	.00031	.00072	465	299,730	5
VI	.00055	.00031	.00072	474	299,739	5
VII	.00001	.00078	.00072	453	299.718	2
VIII	.00054	.00027	.00073	462	299,727	2
IX	.00056	.000315	.000736	483	299,748	3
X	.00052	.00027	.00074	459	299,724	3
XI	0.00054	0.00023	0.00074	453	299,718	3
Weighted mean					299,729	
Correction					+67	
Velocity *in vacuo*					299,796	

Table VII gives the results obtained with the steel octagon.

TABLE VII

	a	*b*	*c*	*VS*	*V*	Wt.
I	0.00027	0.00057	0.00071	465	299,730	3
II	.00032	.00049	.00071	456	299,721	3
III	.00059	.00028	.00069	468	299,733	3
IV	.00057	.00025	.00069	453	299,718	5
$\mathrm{V_I}$	.00008	.00072	.00072	456	299,712	
$\mathrm{V_{II}}$	.00054	.00030	.00072	468	299,733	
$\mathrm{V_{III}}$	.00076	.00005	.00072	459	299,724	
V	Mean of 3				299,723	3
$\mathrm{VI_I}$	.00065	.00027	.00072	492	299,757	3
$\mathrm{VI_{II}}$	.00081	.00005	.00072	474	299,739	
VI	$(2\times\mathrm{VI_{II}}+\mathrm{VI_I})/3$				299,744	3
VII	.00035	.00049	.00072	468	299,733	3
VIII	.00059	.00024	.00072	465	299,730	5
IX	.00055	.00026	.00072	459	299,724	5
X	.00056	.00025	.00072	459	299,724	5
XI	0.00058	0.00025	0.00072	465	299,730	5
Weighted mean					299,728	
Correction					+67	
Velocity *in vacuo*					299,795	

These results are collected in Table VIII.

TABLE VIII

Mirror	Year	N	n	V	Wt.
Glass 8	1925	528	150	299,802	1
Glass 8	1925	528	200	299,756	1
Glass 8	1926	528	216	299,813	3
Steel 8	1926	528	195	299,795	5
Glass 12	1926	352	270	299,796	3
Steel 12	1926	352	218	299,796	5
Glass 16	1926	264	270	299,803	5
Glass 16	1926	264	234	299,789	5
Weighted mean				299,796±4	

When grouped in series of observations with the five mirrors the results show a much more striking agreement, as follows:

Glass 8	299,797
Steel 8	299,795
Glass 12	299,796
Steel 12	299,796
Glass 16	299,796

ART. XXI.—*The relative motion of the Earth and the Luminiferous ether;* by ALBERT A. MICHELSON, Master, U. S. Navy.

THE undulatory theory of light assumes the existence of a medium called the ether, whose vibrations produce the phenomena of heat and light, and which is supposed to fill all space. According to Fresnel, the ether, which is enclosed in optical media, partakes of the motion of these media, to an extent depending on their indices of refraction. For air, this motion would be but a small fraction of that of the air itself and will be neglected.

Assuming then that the ether is at rest, the earth moving through it, the time required for light to pass from one point to another on the earth's surface, would depend on the direction in which it travels.

Let V be the velocity of light.

v = the speed of the earth with respect to the ether.
D = the distance between the two points.
d = the distance through which the earth moves, while light travels from one point to the other.
d_1 = the distance earth moves, while light passes in the opposite direction.

Suppose the direction of the line joining the two points to coincide with the direction of earth's motion, and let T = time required for light to pass from the one point to the other, and T_1 = time required for it to pass in the opposite direction. Further, let T_0 = time required to perform the journey if the earth were at rest.

$$\text{Then } T=\frac{D+d}{V}=\frac{d}{v};\text{ and } T_1=\frac{D-d}{V}=\frac{d_1}{v}$$

From these relations we find $d=D\frac{v}{V-v}$ and $d_1=D\frac{v}{V+v}$ whence $T=\frac{D}{V-v}$ and $T_1=\frac{D}{V+v}$; $T-T_1=2T_0\frac{v}{V}$ *nearly*, and $v=V\frac{T-T_1}{2T_0}$.

If now it were possible to measure $T-T_1$ since V and T_0 are known, we could find v the velocity of the earth's motion through the ether.

In a letter, published in "Nature" shortly after his death, Clerk Maxwell pointed out that $T-T_1$ could be calculated by measuring the velocity of light by means of the eclipses of Jupiter's satellites at periods when that planet lay in different directions from earth; but that for this purpose the observations of these eclipses must greatly exceed in accuracy those which have thus far been obtained. In the same letter it was also stated that the reason why such measurements could not be made at the earth's surface was that we have thus far no method for measuring the velocity of light which does not involve the necessity of returning the light over its path, whereby it would lose nearly as much as was gained in going.

The difference depending on the square of the ratio of the two velocities, according to Maxwell, is far too small to measure.

The following is intended to show that, with a wave-length of yellow light as a standard, the quantity—if it exists—is easily measurable.

Using the same notation as before we have $T=\frac{D}{V-v}$ and $T_1=\frac{D}{V+v}$. The whole time occupied therefore in going and returning $T+T_1=2D\frac{V}{V^2-v^2}$. If, however, the light had traveled in a direction at right angles to the earth's motion it would be entirely unaffected and the time of going and returning would be, therefore, $2\frac{D}{V}=2T_0$. The difference between the times $T+T_1$ and $2T_0$ is

$$2DV\left(\frac{1}{V^2-v^2}-\frac{1}{V^2}\right)=\tau;\ \tau=2DV\frac{v^2}{V^2(V^2-v^2)}$$

or nearly $2T_0\frac{v^2}{V^2}$. In the time τ the light would travel a distance $V\tau=2VT_0\frac{v^2}{V^2}=2D\frac{v^2}{V^2}$.

That is, the actual distance the light travels in the first case is greater than in the second, by the quantity $2D\frac{v^2}{V^2}$.

Considering only the velocity of the earth in its orbit, the ratio $\frac{v}{V}=\frac{1}{10\,000}$ approximately, and $\frac{v^2}{V^2}=\frac{1}{100\,000\,000}$. If D = 1200 millimeters, or in wave-lengths of yellow light, 2 000 000, then in terms of the same unit, $2D\frac{v^2}{V^2}=\frac{4}{100}$.

If, therefore, an apparatus is so constructed as to permit two pencils of light, which have traveled over paths at right angles to each other, to interfere, the pencil which has traveled in the direction of the earth's motion, will in reality travel $\frac{4}{100}$ of a wave-length farther than it would have done, were the earth at rest. The other pencil being at right angles to the motion would not be affected

CHAPTER 18

THE WORK OF de BRAY

M.E.J. Gheury de Bray(i) published a complete Table of all the determinations of the velocity of light including a short discussion on each determination. De Bray, in a second publication said, (102)

"Reference to the original publications showed that several of the observers themselves had misquoted their own results, date, length of base, or even actual velocity, owing either to stating them from memory or to overlooked printer's slips. Successive writers of text books and compilers of Tables had copied these errors over and over again. Spurious determinations had arisen either from the rediscussion of the determination of some observer by armchair critics or by the averaging (after arbitrary weighting) of several determinations obtained by different observers, and these spurious values have been sometimes inserted in Tables, without discrimination or explanation, thereby adding to the confusion." (103)

The Table in question was updated in 1936 and contains all the values which seemed to the physicists themselves to be worthy of mention. He found that it was frequently impossible to ascertain exactly the date of a series of observations and in such cases after carefully examining the original publication, he adopted a date, which appeared to represent the most likely position of the resulting value on the chronological scale.

De Bray considered that except for the determinations of Fizeau and Foucault, which he felt justified in considering as

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(i) Maurice Edmund Joseph Gheury de Bray (1877 -) Lecturer at Woolwich Polytechnic. Director of the Patent Office.

being mere pioneer experiments for ascertaining the feasibility of the measurement of the velocity of light, all the values obtained up to the end of the 19th Century by Cornu, Michelson and Newcomb showed that the velocity of light was decreasing.

i.e.

DATE (DECIMAL)		VALUE
1874.8	CORNU (Helmert Treatment)*	299,990 km/sec
1879.5	MICHELSON	299,910 km/sec
1882.7	NEWCOMB	299,860 km/sec
1882.8	MICHELSON	299,853 km/sec

These results when plotted were found to be on a straight line, indicating that the decrease followed a linear law of variation, the equation of which was

$$V = 331291.65 - 16.6964\ T$$

where V is the velocity in km/sec and T is the time in years. (The equation was obtained by Cauchey's(i) method.) (104) He pointed out that the probable errors of observation were greater than the amplitude of the variation.

The publication of Perrotin's results of 1900 and 1902 raised doubts on the above conclusion. However, investigation showed that on looking at the final discussion given in the Annales de l'Observatoire de Nice (1908) Perrotin's correct value should be taken as 299,901 km/sec. No further discussion took place on this decrease until the publication of Michelson's results of 1925 i.e. 299,802 km/sec. Thus the velocity had resumed its decrease:

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* See: Rapports présentés au Congrès International de Physique de 1900 (volume 2) page 225.

(i) Augustin Louis Cauchey (1789 - 1857) Professor of Mathematics at the École Polytechnique.

DATE		VELOCITY
1902.4	PERROTIN	299,901 ± 84
1924.6	MICHELSON	299,802 ± 30
1926.0	MICHELSON	299,796 ± 4

Now, taking the observations over the longer bases for accuracy he concluded that $V = 307{,}480.98 - 3.99T$ and taking observations made in the 20th Century only

$$V = 308{,}376.22 - 4.455\,T$$

De Bray concluded that the velocity of light was not a constant and suggested two alternative possibilities: a continuous decrease or a periodic variation. (105)

Secondary considerations also supported his views:

a) Two pairs of observations were made at times very close to each other: (12) - (13) and (19) - (20) giving almost identical results; nevertheless, in each pair the earlier observation gave a higher velocity.

b) Perrotin made two determinations (14) and (17). Having given the same weight to the two observations the later velocity was the smaller.

c) Observations (3), (8) and (10) were rejected by their authors due to their being affected by important systematic errors. In all cases the results were smaller than those obtained later on so that "the only determinations (102) which were against a decrease of velocity were precisely those which their authors declared to be doubtful owing to systematic errors."

Should one agree with a decrease, then Perrotin's results pose a problem of alternatives: either his value of 299,901 in 1902.4 was essentially correct and as such the velocity of light had increased during a part of or the whole of 1883 - 1902, or it is not accurate (it had a rather large probable error).

De Bray considered that a simple solution was the most probable whether of the periodic or of the continuous type and as such welcomed a suggestion from F.K. Edmundson (1934) that a simple sine curve represented the observations very well:

$$V = 299{,}885 + 115 \sin \frac{2}{40} (T - 1901)$$

This equation gave excellent agreement with the actual observations having a period of 40 years. The deviations were all under 10 km/sec except Michelson's of 1924.6

Looking at subsequent optical determinations very briefly the following results were obtained.

a) Karolus and Mittelstaedt (1928) using a Kerr cell obtained a mean value of 299,784 ± 20 km/sec (106)

b) Michelson (deceased), Pease and Pearson at Mount San Antonio using a long tube recorded a mean value of 233 observations giving in (1935) 299,774 ± 11 km/sec (107)

c) Huttel in 1940 using a photocell gave a mean of 135 observations 299,768 ± 10 km/sec (108)

d) Anderson in 1941 using a photocell gave a mean of 3000 observations 299,776 ± 14 km/sec (109)

From about 1940, methods other than optical tended to be used to determine the velocity of light. Thus it proves difficult to continue the wave pattern suggested by de Bray. Modern determinations using Kerr Cells or Cavity Resonators have shown the velocity to be a constant and therefore de Bray's theories cannot be accepted as being valid.

No.	Average Date.	Investigator.	Method.	Length of base.	Base.	Velocity, km./sec.	Medium.	Remarks.
		FIRST PERIOD: PIONEER EXPERIMENTS						
1	1849·5	Fizeau	TW	8,633 m.	Suresnes—Montmartre	315,300	Air	
2	1862·8	Foucault	RM	20 m.	Paris Observatory	298,000 ± 500	„	
		SECOND PERIOD: CHIEFLY WITH SHORT BASES						
3	1872·0	Cornu (*a*)	TW	10,310 m.	Ecole Polytechnique—Mont Valérien	298,500 ± 300	Air	Preliminary value (rejected as doubtful).
4	1874·8	„ (*b*)	„	22,910 m.	Paris Observatory—Montlhéry	300,400 ± 300	Vac.	
5	„	Cornu-Helmert	„	„	„	299,990 ± 200	„	Cornu's results discussed by Helmert.
6	1878·0	Michelson (*a*)	RM	1986·23 ft.	U.S. Naval Academy	300,140 ± 300	„	Preliminary value (discarded).
7	1879·5	„ (*b*)	„	„	„	299,910 ± 50	„	Corrected value.
8	1880·9	Newcomb (*a*)	„	2550·9 m.	Fort Meyer—U.S. Naval Observatory	299,627	Air	Doubtful.
9	1881·0	Young and Forbes	TW	18,212·2 ft. and 16,835·0 ft.	Wemyss Bay—Hills behind Innellan	301,382	Vac.	Admittedly unreliable.
10	1881·7	Newcomb (*b*)	RM	3721·2 m.	Fort Meyer—Washington Monument	299,694	Air	Doubtful.
11	1881·8	„ (*c*)	„	—	—	299,810	Vac.	Mean of (*a*), (*b*), and (*d*).
12	1882·7	„ (*d*)	„	3721·2 m.	Fort Meyer—Washington Monument	299,860 ± 30	„	Final declared value.
13	1882·8	Michelson (*c*)	„	2049·532 ft.	Cave School of Applied Science, Cleveland	299,853 ± 60	„	
		THIRD PERIOD: WITH VERY LONG BASES						
14	1900·4	Perrotin (*a*)	TW	11,862·2 m.	Nice Observatory—La Gaude	299,900 ± 80	Vac.	Preliminary discussion (superseded).
15	1900·4	„ (*b*)	„	„	„	300,032 ± 215	„	Final discussion (discarded).
16	1901·4	„ (*c*)	„	—	—	299,880 ± 50	„	Mean of (*a*) and (*d*) (superseded).
17	1902·4	„ (*d*)	„	45,950·7 m.	Nice Observatory—Mont Vinaigre	299,860 ± 80	„	Preliminary discussion (superseded).
18	1902·4	„ (*e*)	„	„	„	299,901 ± 84	„	Perrotin's final declared value.
19	1924·6	Michelson (*d*)	RM	35,385·53 m.	Mt. Wilson Observatory—Mt. St. Antonio	299,802 ± 30	„	Preliminary (corrected) value.
20	1926·0	„ (*e*)	„	„	„	299,796 ± 4	„	
21	In progress	„ (*f*)	„	About 131 km.	Mt Wilson Observatory—Mt. San Jacinto	Not yet published	„	

NATURE, Oct. 22nd., 1927, p. 603

Date	Ratio of e.m.u./e.s.u.	Velocity km/sec in vacuo
1857	Weber and Kohlrausch	310,800
1868	Maxwell	284,300
1869	Thomson and King	280,900
1874	McKichan	289,700
1879	Ayrton and Perry	296,100
1880	Shida	295,600
1883	J.J. Thomson	296,400
1884	Klemenic	302,000
1888	Himstedt	301,000
1889	W.Thomson	300,500
1889	Rosa	300,090
1890	J.J. Thomson and Searle	299,690
1891	Pellat	301,010
1892	Abraham	299,200
1897	Hurmuzescu	300,190
1898	Perot and Fabry	299,870
1899	Lodge and Glazebrook	301,000
	Velocity of electromagnetic radiation in vacuo	
1891	Blondlot	297,600
1895	Trowbridge and Duane	300,300
1897	Saudners	299,700
1899	MacLean	299,100
	CAVITY RESONATORS	
1947	Essen and Gordon-Smith	299,792
1950	Essen	299,792
1950	Hansen and Bol	299,789

Chapter 19

CONCLUSION

The Greeks displayed creative genius in their studies of logic, metaphysics and mathematics; in certain areas such as astronomy they were able to exhibit observational powers and to indulge in speculation, but in the physical sciences they achieved comparatively little success. They found it difficult to progress from mere observation to include the art of experimentation. Most of the early Greeks attempted little or no experimental work in order to verify their speculations, although they had proved themselves outstanding men in everything that turned on wit and abstract meditation. According to some writers the glorious period of Greek intellectual endeavour came to an end round about the time of Aristotle's death in 322 B.C. However Archimedes, Euclid etc., all flourished after this date and certainly were using experimental methods. Archimedes used the experimental method in solving the 'problem of the crown' and Euclid used concave mirrors turned towards the sun in order to cause ignition.(110) Whilst the Greeks achieved more in physical research than the other nations of antiquity, nevertheless they accomplished less in this field than in other directions. Francis Bacon said(111):

"The proceeding has been to fly at once from the sense and particulars up to the most general propositions as certain fixed poles for the argument to turn upon, and from these to derive the rest by middle terms: a short way, no doubt, but precipitate; and one which will never lead to nature, though it offers an easy and ready way to disputation."

After the death of Aristotle, empirical and experimental methods acquired a modest foothold but they never developed mainly due to massive external forces such as the struggle of Christianity with the ancient religions.

As has been said previously, the idea that light was made of particles projected into the eye could be traced back to the Pythagoreans, whilst Empedocles and Plato considered that something was emitted from the eye as well. Aristotle on the other hand thought that light was an action in a medium. All these were mere guesses and as such equally worthless, whether right or wrong.

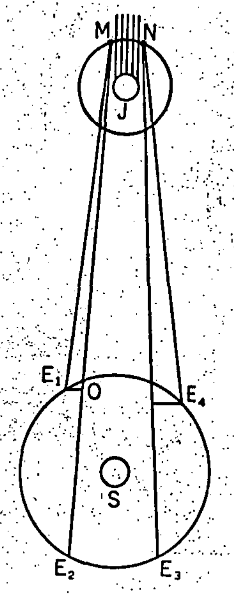
Although it was important to determine whether light had an infinite or finite velocity; no experimental methods were attempted and indeed taking into consideration the extremely high value for the velocity it was obviously quite beyond their experimental capabilities to measure its value whether or not they believed it to have a finite or infinite velocity.

During the middle ages and throughout the Renaissance there was much work done on the rediscovered Greek texts especially with the writings of Aristotle. Aristotle had been interested in the observation of nature, though his greatest strength lay in metaphysics and logic rather than in science. His works can be considered an encyclopaedia of the learning of the ancient world, and, save in physics and astronomy, he probably made a real improvement in all the subjects he touched with perhaps the greatest of Aristotle's advances in exact knowledge being those he made in biology. His physics were not objective like those of Democritus. To Aristotle, the concepts by which nature must be interpreted were substance, essence, matter, form, quantity, quality - categories developed in an attempt to express man's direct sense - perception of the world in terms of ideas natural to his mind. Throughout the Renaissance there developed a long tradition of intellectual work of commentating on these texts which began with the Greek commentators and extended by the Islamic philosophers. In 1450 man attempted no more than the comprehension of what the ancients had discovered, certain that this was the most that could be known. Whereas by 1630 the works of the ancients were available in various vernacular translations with the authority of the Greek and Roman past being under attack. The attack was so widespread that one could publicly defend the thesis that everything Aristotle had taught was false.

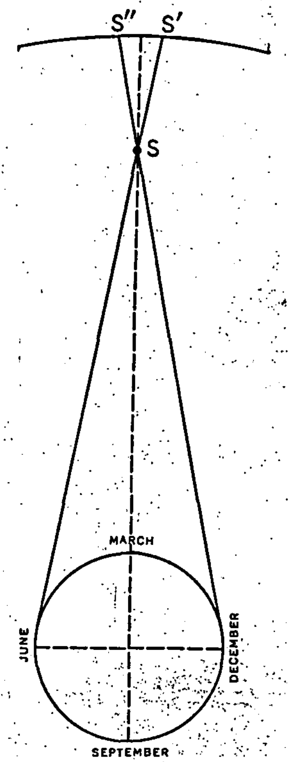
The most essential new element to be adopted by Natural Philosophers was to use practical experiment in their scholastic experimentation. Roger Bacon appealed for the development of experimental science and emphasised the importance of the 'scientia experimentalis'(112). The use of the experimental method gave the important advantage of permitting co-operative endeavour and allowing various kinds of minds to contribute equally to the progress of science. Francis Bacon considered that only science could provide the key to the truth and only empiricism could provide the key to science.

"The sciences stand where they did and remain almost in the same condition, receiving no noticeable increase, but on the contrary, thriving most under their first founder, and then declining. Whereas in the mechanical arts, which are founded on nature and the light of experience, we see the contrary happens, for these are continually thriving and growing, as having in them a breath of life; at first rude then convenient, afterwards adorned, and at all times advancing."(113)

Turning specifically to the investigations with the velocity of light. Galileo made a significant contribution in his Discourses on two New Sciences for he did not merely present experimental data but also showed a great deal of deductive reasoning. His experiments concerned with the uncovering of a lantern would never have given any results as we now know due to the high velocity of light and small time intervals involved. Like the Greeks, the scientific technology available at that time simply could not cope with an experiment of this nature. He was not able to settle the question of the velocity of light from his experiments but he did make a suggestion on a totally different problem which led other scientists to success. Whilst at Padua, holding the Chair of Mathematics, he heard rumours coming from Holland and Belgium concerning experiments on the construction of a telescope. He at once set to and manufactured his own telescope. Eventually he was able to construct an instrument which magnified an object nearly 1000 times and brought it 30 times nearer.(114) On January 7th, 1610 he turned his telescope towards Jupiter and observed the satellites and their rotation. He remarked that the frequent disappearance of Jupiter's satellites behind the planet might be made to serve in longitudinal determinations. This led Röemer to start the crucial experiments on the observed irregularities on the periodicity of the satellites of Jupiter which he felt must be explained on the supposition that the velocity of light was finite. He said that it required light 22 minutes to cross the earth's orbit. Thus for the first time we have experimental evidence, not only that the velocity of light is finite but that a reasonably accurate value could be given to it. (The more correct value is now taken to be 16m 36 sec.) Röemer based his calculations on the first satellite only and stated that similar calculations from observations on the three other satellites would not have led to success. This meant that the Académie des Sciences did not at once accept his theory.



[redacted].– **Römer's determination of the velocity of light.** When the earth moves from E_1 to E_2 the eclipses of the first satellite of Jupiter occurred several minutes later than the time computed from its average period of revolution. Römer interpreted this difference to be due to the time it takes light to travel the distance OE_2. When the earth passed from E_3 to E_4 the eclipses occurred earlier than predicted.



[redacted]. **Bradley's attempt to measure the parallax of γ draconis.** He expected the star to show apparent motion from S' to S'' from June to December, and in March and September to occupy an intermediate position on the celestial sphere. In fact, the positions were the same for June and December. He found no effect of parallax. But strangely, in March and September the star did not appear in the same place!

In England Edmond Halley supported the theory of Roemer and James Bradley was able to verify it by the 'aberration of light'. He considered that the progressive transmission of light, combined with the advance of the earth in its orbit, must cause an annual shifting of the direction in which heavenly bodies were seen by an amount depending upon the ratio of the velocities.

We have already looked at the rivalry between the wave theory and the corpuscular theory in Chapter five, but perhaps further mention of Newton's influence might be appropriate. Newton cited the finite nature of the velocity of light, without making any really firm statements as to its nature(115), (116).

Cantor in Optics after Newton states that "Whewell[(i)] considered that Newton's influence in the eighteenth century accounted for the dominance of the corpuscular theory"(117). For during the 1670s Newton was not averse to the wave theory but by 1706, when the first Latin edition of the Opticks was published, he was "strongly disinclined to believe light to consist in undulations merely"(118). However Cantor considers the position of Newton to be more ambiguous in that "there is a lack of evidence to show that his authority caused his views to be popular"(119)

Although the experimental work connected with the explanation of interference, diffraction and polarization was of the greatest importance in deciding in favour of the wave theory of light. Nevertheless, since the two

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(i) William Whewell (1794 - 1866) Master of Trinity College, Cambridge.

rival theories gave different values for the velocity in a denser medium following refraction there was obviously keen interest in any experiments on the velocity of light which could lend support to the various proponents of the two theories.

Both Wheatstone and Arago made important suggestions as to using rotating mirrors to ascertain the velocity of light and to find out whether the speed was greater in the more refracting medium. Although the idea was subsequently used with great success, the mechanical difficulties at the time (1830's) mainly concerned with stability and constancy of high speed rotation were too great for experimental success. It was not found possible to rotate a mirror at a constant speed of over one thousand revolutions per minute. Some scientists also considered that it was impossible for the eye to pick up the instantaneous image of the flash reflected from the mirror rotating at such a high speed and as such the whole project was considered unworkable. Bertrand(i) remarked that "an attentive and assiduous observer may according to computations of M.Babinet(ii) hope to catch the ray once in three years".(120)

The finite value for the velocity came to be accepted as an established fact by the early part of the nineteenth century. However the scientific world had to wait until

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(i) Joseph Louis François Bertrand (1822 - 1900) Professor at the Collège de France and the Ecole Polytechnique.

(ii) Jacques Babinet (1794 - 1872) Librarian at the Bureau of Longitudes.

the middle of the nineteenth century for the first terrestrial experiments on the velocity of light. Foucault in 1850 found that the velocity of light in water to be less than in air. This experiment on the relative velocity of light in air and water was yet another decisive experiment in upholding the wave theory of light.

The experiments of Fizeau were important in that he made the earliest determination of the absolute velocity of light which was not based on astronomical observations. Although not particularly accurate, his method wasadapted and refined by Cornu and by Young and Forbes. By now the velocity of light was assuming greater importance as electromagnetic theory was being developed by Helmholtz, Maxwell and others, in which the velocity of light (electromagnetic waves) figured prominently.

Young and Forbes, considering the problems of producing constant speed rotations and difficulties with timing mechanisms produced creditable results with the most interesting result being that they seemed to show that the blue rays travelled about 1.8 percent faster than the red. This result has always been challenged, for if true, stars should appear coloured just before and after an eclipse. Further Michelson (using Foucault's method) should have observed a spectral drawing out of the image of the slit, giving rise to a coloured image ten millimetres in width. However, try as he might he could never observe such a coloured image. Young and Forbes experiments also suffered from the base being insecure due to earth movement.

The most accurate determinations prior to electrical methods have been those of Newcomb and Michelson. In particular Michelson improved the arrangement as used by Foucault and was able to displace the return image through 138 mm or nearly 200 times that obtained by Foucault.

The velocity of light engrossed the attention of Michelson throughout his scientific career. Funds being made available by government circles as it was felt that not only was it a scientific (constant) worthy of an accurate determination but also that the experiments were bringing prestige to American scientific circles.

One must consider the methods of Fizeau and Foucault as pioneer experiments upon which most of the subsequent experimentalists based their researches. The results obtained between 1872 and 1888 by various scientists, although a great improvement on the two pioneer experiments, nevertheless contained many faults. In the main they suffered from the light travelling over a short base line, rotational problems, timing errors, and the frailty of the human eye as an image detector. There could also have been more mathematical vigour with the treatment of the results and the built in errors.

Michelson in his Mount Wilson experiments was able to use an extended base line of 22 miles which was a great improvement on previous experiments. The base line was accurately measured using a team of Army engineers from the land survey department. This being a good example of a large team effort which became an increasing feature of experimental science as one progresses through the twentieth century. Furthermore by using an octagonal revolving mirror which offered the possibility of receiving the return light on a succeeding face he was able to eliminate the measurement of the angular deflection of the returned beam. Michelson was working at the limits of technology and took the greatest possible care with the reduction of errors. As a result his results compare favourably with the later highly accurate electrical methods.

Quite apart from the modern definition of the velocity of light in terms of atomic oscillations, the various electrical methods which started to be used by various workers in the 1930's, 1940's and 1950's showed remarkable consistency in the value for the velocity of light (see graph). Taking the last results by Michelson which were obtained by optical methods and the electrical results of all researches in 1930's/1940's you can see that a constant value for the velocity of light was obtained. Thus the speculations of deBray are undoubtedly false. Although he carefully checked the results for errors, nevertheless the basic methodology used for measuring the various parts of the experiments were not accurate; certainly when compared to the instrumentation available by 1940. As a result, he was not looking at a sinusoidal variation of the velocity but a distribution of inaccurate values.

The velocity of light has always been considered an important physical constant but it gained increasing importance towards the end of the nineteenth centure in clearing up various theoretical questions.

The development of the electromagnetic wave theory by Clerk Maxwell and others showed that light was a form of electromagnetic radiation and that the velocity of other electromagnetic radiation and that of light travelling in vacuo were identical.

This meant that sophisticated electrical methods could be used to confirm the velocity of electromagnetic radiation and compare the values obtained with the optical methods.

With the publication of the theory of relativity, the velocity of light became even more important as it made its value in a vacuum, the highest speed possible in nature.

Finally, Einstein was able to show the now famous mass/energy relationship

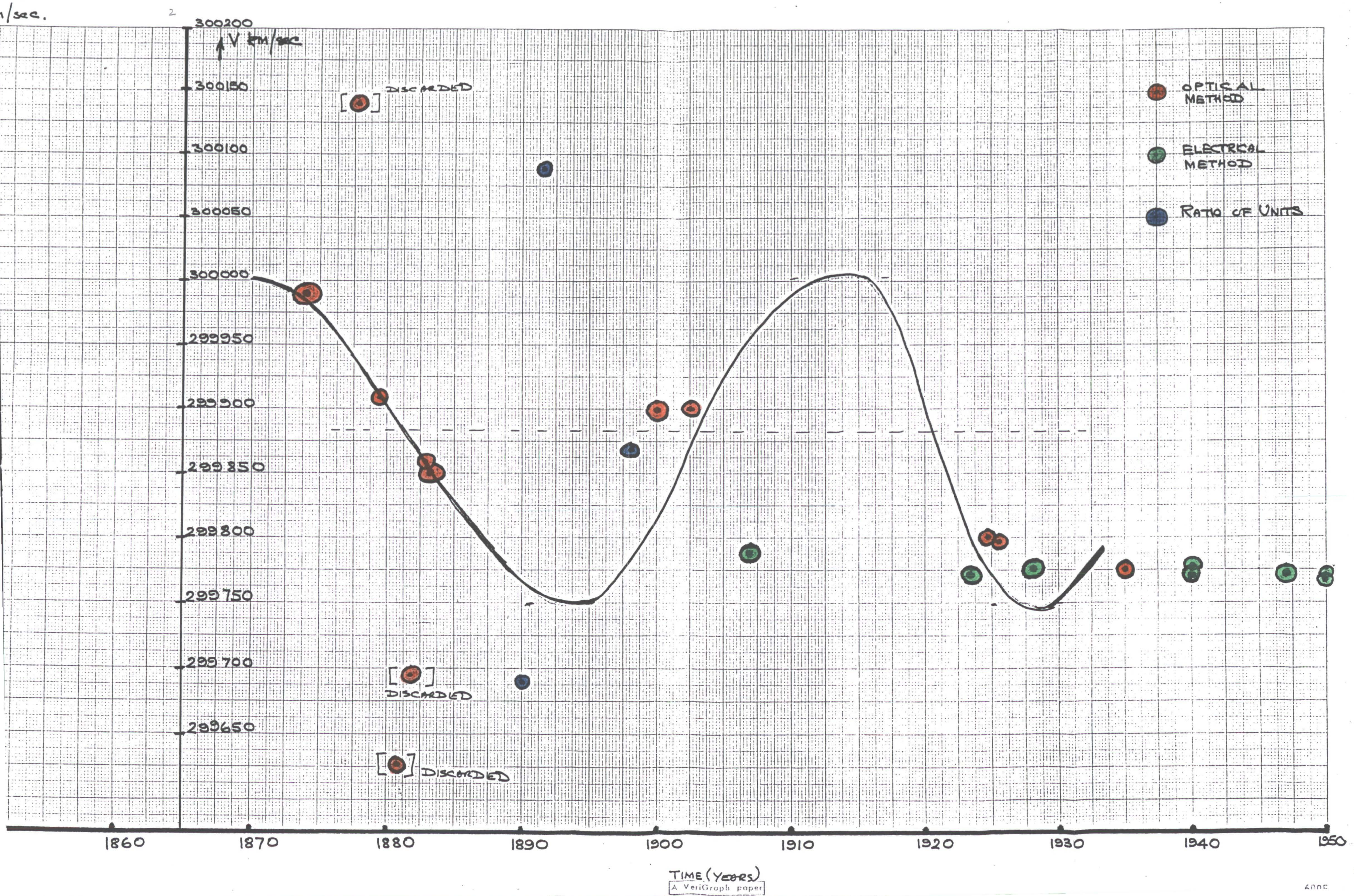
$$(EK = \Delta m C^2)$$

which showed a direct relationship between the kinetic energy of a substance and its change in mass, governed by the square of the velocity of light.

Phænomena.	*Corpuscular Explanation.*	*Undulatory Explanation.*
Reflection...............	..... Perfect	...... Perfect.
Ditto at boundary of transparent medium	...... Imperfect.....	.... Perfect.
Refraction (light homogeneous)	...... Perfect	...... Perfect.
Dispersion	...... Imperfect ...	...... Imperfect. (? Cauchy.)
Absorption	...... Imperfect	...... Imperfect.
Colours of thin plates (in general).........	...... Perfect .... (with subsidiary theory of fits)...	...... Perfect.
Central spot...........	...... None	...... Perfect. (Imperfect according to Mr. Potter.)
Airy's modification ...	...... None	...... Perfect.
Thick plates............	...... Perfect	...... Perfect.
Coloured fringes of apertures and shadows in simple cases	...... Imperfect .... (with subsidiary theory of inflection)	...... Perfect (Imperfect according to Mr. Barton.)
—— in more complex cases............	...... None	...... None.
Stripes in mixed light	...... None	...... Perfect.
Shifting by interposed plate..................	...... None	...... Perfect. (Imperfect according to Mr. Potter.)
Colours of gratings...	...... None	...... Perfect.
Double refraction	...... Perfect........	...... Perfect.
Polarization............	...... Imperfect .... (with subsidiary theory of polarity)	...... Perfect. (with subsidiary theory of transverse vibrations.)
Connexion with double refraction........	...... None	...... Perfect.
Law of tangents	...... None	...... Perfect.
Interferences of polarized light.........	...... None	...... Perfect.
Polarized rings........	...... Imperfect..... (with subsidiary theory of moveable polarization)	...... Perfect.
Circular and elliptic polarization: at internal reflection	...... None	...... Imperfect.
at metallic surfaces	...... None .... (? Sir D. Brewster)	...... None.
Conical refraction....	...... None..........	...... Perfect.

Figure 19. Powell's assessment of the theories in 1833. Source: Powell, 1833*b*: 416–17.

See REFERENCE (117) p 193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DISCARDED
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ELECTRICAL METHOD
RATIO OF UNITS
1860
1870
1880
1890
1900
1910
1920
1930
1940
1950
TIME (YEARS)
A VeriGraph paper
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ROEMER; – PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY, VOLUME XII, No 136, JUNE 25th, 1677

A Demonſtration concerning the Motion of Light, *communicated from* Paris, *in the* Journal des Sçavans, *and here made Engliſh.*

PHiloſophers have been labouring for many years to decide by ſome Experience, whether the action of Light be conveyed in an inſtance to diſtant places, or whether it requireth time. M. *Romer* of the *R. Academy* of the Sciences hath deviſed a way, taken from the Obſervations of the firſt Satellit of *Jupiter*, by which he demonſtrates, that for the diſtance of about 3000 leagues, ſuch as is very near the bigneſs of the Diameter of the Earth, Light needs not one ſecond of time.

Let (in *Fig. 11.*) A be the *Sun*, B *Jupiter*, C the firſt Satellit of *Jupiter*, which enters into the ſhadow of *Jupiter*, to come out of it at D; and let EFGHKL be the *Earth* placed at divers diſtances from *Jupiter*.

Now, ſuppoſe the *Earth*, being in L towards the ſecond Quadrature of *Jupiter*, hath ſeen the firſt Satellit at the time of its emerſion or iſſuing out of the ſhadow in D; and that about 42½ hours after, (*vid.* after one revolution of this Satellit,) the *Earth* being in K, do ſee it returned in D; it is manifeſt, that if the Light require time to traverſe the interval LK, the Satellit will be ſeen returned later in D, than it would have been if the Earth had remained in L, ſo that the revolution of this Satellit being thus obſerved by the Emerſions, will be retarded by ſo much time, as the Light ſhall have taken in paſſing from L to K, and that, on the contrary, in the other Quadrature FG, where the *Earth* by approaching goes to meet the Light, the revolutions of the Immerſions will appear to be ſhortned by ſo much, as thoſe of the Emerſions had appeared to be lengthned. And becauſe in 42½ hours, which this Satellit very near takes to make one revolution, the diſtance between the *Earth* and *Jupiter* in both the Quadratures varies at leaſt 210 Diameters of the *Earth*, it follows, that if for the account of every Diameter of the *Earth* there were required a ſecond of time, the Light would take 3½ minutes for each of the intervals GF, KL; which would cauſe near half a quarter of an hour between two revolutions of the firſt Satellit, one obſerved in FG, and the other in KL, whereas there is not obſerved any ſenſible difference.

Yet doth it not follow hence, that Light demands no time. For, after M. *Romer* had examin'd the thing more nearly, he found, that what was not ſenſible in two revolutions, became very conſiderable in many being taken together, and that, for example, forty revolutions obſerved on the ſide F, might be ſenſibly ſhorter, than forty others obſerved in any place of the Zodiack where *Jupiter* may be met with; and that in proportion of twenty two for the whole interval of H E, which is the double of the interval that is from hence to the Sun.

The neceſſity of this new Equation of the retardment of Light, is eſtabliſhed by all the obſervations that have been made in the *R. Academy*, and in the *Obſervatory*, for the ſpace of eight years, and it hath been lately confirmed by the Emerſion of the firſt Satellit obſerved at *Paris* the *9th* of *November* laſt at 5 a Clock, 35′. 45″. at Night, 10 minutes later than it was to be expected, by deducing it from thoſe that had been obſerved in the Month of *Auguſt*, when the *Earth* was much nearer to *Jupiter*: Which M. *Romer* had predicted to the ſaid Academy from the beginning of *September*.

But to remove all doubt, that this inequality is cauſed by the retardment of the Light, he demonſtrates, that it cannot come from any excentricity, or any other cauſe of thoſe that are commonly alledged to explicate the irregularities of the *Moon* and the other Planets; though he be well aware, that the firſt Satellit of *Jupiter* was excentrick, and that, beſides, his revolutions were advanced or retarded according as *Jupiter* did approach to or recede from the Sun, as alſo that the revolutions of the *primum mobile* were unequal; yet ſaith he, theſe three laſt cauſes of inequality do not hinder the firſt from being manifeſt.

James Bradley, - Philosophical Transactions
of the Royal Society, Volume XXV,
No 406, 1728, pp 637 – 661

IV. *A Letter from the Reverend Mr.* James Bradley *Savilian Profeſſor of Aſtronomy at* Oxford, *and F.R.S. to Dr.*Edmond Halley Aſtronom. Reg. &c. *giving an Account of a new diſcovered. Motion of the* Fix'd Stars.

SIR,

YOU having been pleaſed to expreſs your Satisfaction with what I had an Opportunity ſometime ago, of telling you in Converſation, concerning ſome Obſervations, that were making by our late worthy and ingenious Friend, the honourable *Samuel Molyneux* Eſquire, and which have ſince been continued and repeated by my ſelf, in order to determine the *Parallax* of the *fixt Stars*; I ſhall now beg leave to lay before you a more particular Account of them.

Before I proceed to give you the Hiſtory of the Obſervations themſelves, it may be proper to let you know, that they were at firſt begun in hopes of verifying and confirming thoſe, that Dr. *Hook* formerly communicated to the publick, which ſeemed to be attended with Circumſtances that promiſed greater Exactneſs in them, than could be expected in any other, that had been made and publiſhed on the ſame Account. And as his Attempt was what principally gave Riſe to this, ſo his Method in making the Obſervations was in ſome Mea-

Meaſure that which Mr. *Molyneux* followed: For he made Choice of the ſame Star, and his Inſtrument was conſtructed upon almoſt the ſame Principles. But if it had not greatly exceeded the Doctor's in Exactneſs, we might yet have remained in great Uncertainty as to the *Parallax* of the *fixt Stars*; as you will perceive upon the Compariſon of the two Experiments.

This indeed was chiefly owing to our curious Member, Mr. *George Graham*, to whom the Lovers of Aſtronomy are alſo not a little indebted for ſeveral other exact and well-contrived Inſtruments. The Neceſſity of ſuch will ſcarce be diſputed by thoſe that have had any Experience in making Aſtronomical Obſervations; and the Inconſiſtency, which is to be met with among different Authors in their Attempts to determine ſmall Angles, particularly the annual Parallax of the *fixt Stars*, may be a ſufficient Proof of it to others. Their Diſagreement indeed in this Article, is not now ſo much to be wondered at, ſince I doubt not, but it will appear very probable, that the Inſtruments commonly made uſe of by them, were liable to greater Errors than many times that Parallax will amount to.

The Succeſs then of this Experiment evidently depending very much on the Accurateneſs of the Inſtrument that was principally to be taken Care of: In what Manner this was done, is not my preſent Purpoſe to tell you; but if from the Reſult of the Obſervations which I now ſend you, it ſhall be judged neceſſary to communicate to the Curious the Manner of making them, I may hereafter perhaps give them a particular Deſcription, not only of Mr. *Molyneux*'s Inſtrument, but alſo of my own, which

which hath ſince been erected for the ſame Purpoſe and upon the like Principles, though it is ſomewhat different in its Conſtruction, for a Reaſon you will meet with preſently.

Mr. *Molyneux*'s *Apparatus* was compleated and fitted for obſerving about the End of *November* 1725, and on the third Day of *December* following, the bright Star in the Head of *Draco* (marked γ by *Bayer*) was for the firſt Time obſerved, as it paſſed near the Zenith, and its Situation carefully taken with the Inſtrument. The like Obſervations were made on the 5th, 11th, and 12th Days of the ſame Month, and there appearing no material Difference in the Place of the Star, a farther Repetition of them at this Seaſon ſeemed needleſs, it being a Part of the Year, wherein no ſenſible Alteration of Parallax in this Star could ſoon be expected. It was chiefly therefore Curioſity that tempted me (being then at *Kew*, where the Inſtrument was fixed) to prepare for obſerving the Star on *December* 17th, when having adjuſted the Inſtrument as uſual, I perceived that it paſſed a little more Southerly this Day than when it was obſerved before. Not ſuſpecting any other Cauſe of this Appearance, we firſt concluded, that it was owing to the Uncertainty of the Obſervations, and that either this or the foregoing were not ſo exact as we had before ſuppoſed; for which Reaſon we purpoſed to repeat the Obſervation again, in order to determine from whence this Difference proceeded; and upon doing it on *December* 20th, I found that the Star paſſed ſtill more Southerly than in the former Obſervations. This ſenſible Alteration the more ſurprized us, in that it was the contrary

way

way from what it would have been, had it proceeded from an annual *Parallax* of the *Star*: But being now pretty well ſatisfied, that it could not be entirely owing to the want of Exactneſs in the Obſervations; and having no Notion of any thing elſe, that could cauſe ſuch an apparent Motion as this in the Star; we began to think that ſome Change in the Materials, *&c.* of the Inſtrument itſelf, might have occaſioned it. Under theſe Apprehenſions we remained ſome time, but being at length fully convinced, by ſeveral Trials, of the great Exactneſs of the Inſtrument, and finding by the gradual Increaſe of the Stars Diſtance from the Pole, that there muſt be ſome regular Cauſe that produced it; we took care to examine nicely, at the Time of each Obſervation, how much it was: and about the Beginning of *March* 1726, the Star was found to be 20″ more Southerly than at the Time of the firſt Obſervation. It now indeed ſeemed to have arrived at its utmoſt Limit Southward, becauſe in ſeveral Trials made about this Time, no ſenſible Difference was obſerved in its Situation. By the Middle of *April* it appeared to be returning back again towards the North; and about the Beginning of *June*, it paſſed at the ſame Diſtance from the Zenith as it had done in *December*, when it was firſt obſerved.

From the quick Alteration of this Star's Declinanation about this Time (it increaſing a Second in three Days) it was concluded, that it would now proceed Northward, as it before had gone Southward of its preſent Situation; and it happened as was conjectured: for the Star continued to move Northward till *September* following, when it again became ſtationary, being then near 20″ more Northerly than in *June*, and no leſs than 39″ more Northerly than it was in *March*. From *September* the Star returned towards the South, till it arrived in *December* to the ſame Situation it was in at that time twelve Months, allowing for the Difference of Declination on account of the Preceſſion of the Equinox.

This was a ſufficient Proof, that the Inſtrument had not been the Cauſe of this apparent Motion of the Star, and to find one adequate to ſuch an Effect ſeemed a Difficulty. A *Nutation* of the Earth's Axis was one of the firſt things that offered itſelf upon this Occaſion, but it was ſoon found to be inſufficient; for though it might have accounted for the change of Declination in γ *Draconis* yet it would not at the ſame time agree with the Phænomena in other Stars; particularly in a ſmall one almoſt oppoſite in right Aſcenſion to γ *Draconis*, at about the ſame Diſtance from the North Pole of the Equator: For, though this Star ſeemed to move the ſame way, as a Nutation of the Earth's Axis would have made it, yet it changing its Declination but about half as much as γ *Draconis* in the ſame time (as appeared upon comparing the Obſervations of both made upon the ſame Days, at different Seaſons of the Year) this plainly proved, that the apparent Motion of the Stars was not occaſioned by a real Nutation, ſince if that had been the Cauſe, the Alteration in both Stars would have been near equal.

The great Regularity of the Obſervations left no room to doubt, but that there was ſome regular Cauſe that produced this unexpected Motion, which did not depend on the Uncertainty or Variety of the

Seaſons of the Year. Upon comparing the Obſervations with each other, it was diſcovered, that in both the fore-mentioned Stars, the apparent Difference of Declination from the *Maxima*, was always nearly proportional to the verſed Sine of the Sun's Diſtance from the Equinoctial Points. This was an Inducement to think, that the Cauſe, whatever it was, had ſome Relation to the Sun's Situation with reſpect to thoſe Points. But not being able to frame any Hypotheſis at that Time, ſufficient to ſolve all the Phænomena, and being very deſirous to ſearch a little farther into this Matter; I began to think of erecting an Inſtrument for my ſelf at *Wanſted*, that having it always at Hand, I might with the more Eaſe and Certainty, enquire into the Laws of this new Motion. The Conſideration likewiſe of being able by another Inſtrument, to confirm the Truth of the Obſervations hitherto made with Mr. *Molyneux*'s, was no ſmall Inducement to me; but the Chief of all was, the Opportunity I ſhould thereby have of trying, in what Manner other Stars were affected by the ſame Cauſe, whatever it was. For Mr. *Molyneux*'s Inſtrument being originally deſigned for obſerving γ *Draconis* (in order, as I ſaid before, to try whether it had any ſenſible Parallax) was ſo contrived, as to be capable of but little Alteration in its Direction, not above ſeven or eight Minutes of a Degree: and there being few Stars within half that Diſtance from the Zenith of *Kew*, bright enough to be well obſerved, he could not, with his Inſtrument, throughly examine how this Cauſe affected Stars differently ſituated with

reſpect to the equinoctial and ſolſtitial Points of the Ecliptick.

Theſe Conſiderations determined me; and by the Contrivance and Direction of the ſame ingenious Perſon, Mr. *Graham*, my Inſtrument was fixed up *Auguſt* 19, 1727. As I had no convenient Place where I could make uſe of ſo long a Teleſcope as Mr. *Molyneux*'s, I contented my ſelf with one of but little more than half the Length of his (*viz.* of about 12½ Feet, his being 24¼) judging from the Experience which I had already had, that this Radius would be long enough to adjuſt the Inſtrument to a ſufficient Degree of Exactneſs, and I have had no Reaſon ſince to change my Opinion: for from all the Trials I have yet made, I am very well ſatisfied, that when it is carefully rectified, its Situation may be ſecurely depended upon to half a Second. As the Place where my Inſtrument was to be hung, in ſome Meaſure determined its Radius, ſo did it alſo the Length of the Arch, or Limb, on which the Diviſions were made to adjuſt it: For the Arch could not conveniently be extended farther, than to reach to about 6¼° on each Side my Zenith. This indeed was ſufficient, ſince it gave me an Opportunity of making Choice of ſeveral Stars, very different both in Magnitude and Situation; there being more than two hundred inſerted in the *Britiſh* Catalogue, that may be obſerved with it. I needed not to have extended the Limb ſo far, but that I was willing to take in *Capella*, the only Star of the firſt Magnitude that comes ſo near my Zenith.

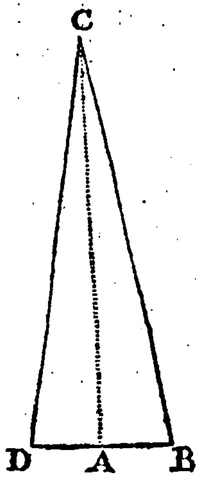
My Inſtrument being fixed, I immediately began to obſerve ſuch Stars as I judged moſt proper to give me light into the Cauſe of the Motion already mentioned. There was Variety enough of ſmall ones; and not leſs than twelve, that I could obſerve through all the Seaſons of the Year; they being bright enough to be ſeen in the Day-time, when neareſt the Sun. I had not been long obſerving, before I perceived, that the Notion we had before entertained of the Stars being fartheſt North and South, when the Sun was about the Equinoxes, was only true of thoſe that were near the ſolſtitial Colure: And after I had continued my Obſervations a few Months, I diſcovered, what I then apprehended to be a general Law, obſerved by all the Stars, *viz.* That each of them became ſtationary, or was fartheſt North or South, when they paſſed over my Zenith at ſix of the Clock, either in the Morning or Evening. I perceived likewiſe, that whatever Situation the Stars were in with reſpect to the cardinal Points of the Ecliptick, the apparent Motion of every one tended the ſame Way, when they paſſed my Inſtrument about the ſame Hour of the Day or Night; for they all moved Southward, while they paſſed in the Day, and Northward in the Night; ſo that each was fartheſt North, when it came about Six of the Clock in the Evening, and fartheſt South, when it came about Six in the Morning.

Though I have ſince diſcovered, that the *Maxima* in moſt of theſe Stars do not happen exactly when they come to my Inſtrument at thoſe Hours, yet not being able at that time to prove the contrary, and ſuppoſing that they did, I endeavoured to find out what Proportion the greateſt Alterations of Declination in different Stars bore to each other; it being very evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneaux*'s Obſervations, that γ *Draconis* altered its Declination about twice as much as the fore-mentioned ſmall Star almoſt oppoſite to it; but examining the matter more particularly, I found that the greateſt Alteration of Declination in theſe Stars, was as the Sine of the Latitude of each reſpectively. This made me ſuſpect that there might be the *like* Proportion between the *Maxima* of other Stars; but finding, that the Obſervations of ſome of them would not perfectly correſpond with ſuch an Hypotheſis, and not knowing, whether the ſmall Difference I met with, might not be owing to the Uncertainty and Error of the Obſervations, I deferred the farther Examination into the Truth of this Hypotheſis, till I ſhould be furniſhed with a Series of Obſervations made in all Parts of the Year; which might enable me, not only to determine what Errors the Obſervations are liable to, or how far they may ſafely be depended upon; but alſo to judge, whether there had been any ſenſible Change in the Parts of the Inſtrument itſelf.

Upon theſe Conſiderations, I laid aſide all Thoughts at that Time about the Cauſe of the fore-mentioned Phænomena, hoping that I ſhould the eaſier diſcover it, when I was better provided with proper Means to determine more preciſely what they were.

When the Year was compleated, I began to examine and compare my Obſervations, and having pretty well ſatisfied my ſelf as to the general Laws of the *Phænomena*, I then endeavoured to find out the Cauſe

Cauſe of them. I was already convinced, that the apparent Motion of the Stars was not owing to a Nutation of the Earth's Axis. The next Thing that offered itſelf, was an Alteration in the Direction of the Plumb-line, with which the Inſtrument was conſtantly rectified; but this upon Trial proved inſufficient. Then I conſidered what Refraction might do, but here alſo nothing ſatisfactory occurred. At laſt I conjectured, that all the *Phænomena* hitherto mentioned, proceeded from the progreſſive Motion of Light and the Earth's annual Motion in its Orbit. For I perceived, that, if Light was propagated in Time, the apparent Place of a fixt Object would not be the ſame when the Eye is at Reſt, as when it is moving in any other Direction, than that of the Line paſſing through the Eye and Object; and that, when the Eye is moving in different Directions, the apparent Place of the Object would be different.



I conſidered this Matter in the following Manner. I imagined CA to be a Ray of Light, falling perpendicularly upon the Line BD; then if the Eye is at reſt at A, the Object muſt appear in the Direction AC, whether Light be propagated in Time or in an Inſtant. But if the Eye is moving from B towards A, and Light is propagated in Time, with a *Velocity* that is to the Velocity of the Eye, as CA to BA; then Light moving from C to A, whilſt the Eye moves from B to A, that Particle of it, by which the Object will be diſcerned, when the Eye in its Motion comes to A, is at C when the Eye is at B. Joining the Points B, C, I ſuppoſed the Line CB, to be a Tube (inclined to the Line BD in the Angle DBC) of ſuch a Diameter, as to admit of but one Particle of Light; then it was eaſy to conceive, that the Particle of Light at C (by which the Object muſt be ſeen when the Eye, as it moves along, arrives at A) would paſs through the Tube BC, if it is inclined to BD in the Angle DBC, and accompanies the Eye *in its Motion from* B *to* A; and that it could not come to the Eye, placed behind ſuch a Tube, if it had any other Inclination to the Line BD. If inſtead of ſuppoſing CB ſo ſmall a Tube, we imagine it to be the Axis of a larger; then for the ſame Reaſon, the Particle of Light at C, could not paſs through that Axis, unleſs it is inclined to BD, in the Angle CBD. In like manner, if the Eye moved the contrary way, from D towards A, with the ſame Velocity; then the Tube muſt be inclined in the Angle BDC. Although therefore the true or real Place of an Object is perpendicular to the Line in which the Eye is moving, yet the viſible Place will not be ſo, ſince that, no doubt, muſt be in the Direction of the Tube; but the Difference between the true and apparent Place will be (*cæteris paribus*) greater or leſs, according to the different Proportion between the Velocity of Light and that of the Eye. So that if we could ſuppoſe that Light was propagated in an Inſtant, then there would be no Difference between the real and viſible Place of an Object, altho' the Eye were in Motion, for in that caſe, AC being infinite with Reſpect to AB, the Angle ACB (the Difference between the true and viſible Place) vaniſhes. But if Light be propagated in Time (which I preſume will readily be allowed by moſt of the Philoſophers of this Age) then it is evident from the foregoing Conſiderations, that there will be always a Difference between the real and viſible Place of an Object, unleſs the Eye is moving either directly towards or from the Object. And in all Caſes, the Sine of the Difference between the real and viſible Place of the Object, will be to the Sine of the viſible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

If Light moved but 1000 times faſter than the Eye, and an Object (ſuppoſed to be at an infinite Diſtance) was really placed perpendicularly over the Plain in which the Eye is moving, it follows from what hath been already ſaid, that the apparent Place of ſuch an Object will be always inclined to that Plain, in an Angle of 89° 56′½; ſo that it will conſtantly appear 3′½ from its true Place, and ſeem ſo much leſs inclined to the Plain, that way towards which the Eye tends. That is, if A C is to A B (or A D) as 1000 to one, the Angle A B C will be 89° 56′½, and A C B = 3′½, and B C D = 2 A C B = 7′. So that according to this Suppoſition, the viſible or apparent Place of the Object will be altered 7′, if the Direction of the Eye's Motion is at one time contrary to what it is at another.

If the Earth revolve round the Sun annually, and the Velocity of Light were to the Velocity of the Earth's Motion in its Orbit (which I will at preſent ſuppoſe to be a Circle) as 1000 to one; then tis eaſy to conceive, that a Star really placed in the very Pole of the Ecliptick, would, to an Eye carried along with the Earth, ſeem to change its Place continually, and (neglecting the ſmall Difference on the Account of the Earth's diurnal Revolution on its Axis) would ſeem to deſcribe a Circle round that Pole, every Way diſtant therefrom 3′½. So that its Longitude would be varied through all the Points of the Ecliptick every Year; but its Latitude would always remain the ſame. Its right Aſcenſion would alſo change, and its Declination, according to the different Situation of the Sun in reſpect to the equinoctial Points; and its apparent Diſtance from the North Pole of the Equator would be 7′ leſs at the Autumnal, than at the vernal Equinox.

The greateſt Alteration of the Place of a Star in the Pole of the Ecliptick (or which in Effect amounts to the ſame, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit) being known; it will not be difficult to find what would be the Difference upon this Account, between the true and apparent Place of any other Star at any time; and on the contrary, the Difference between the true and apparent Place being given; the Proportion between the Velocity of Light and the Earth's Motion in its Orbit may be found.

As I only obſerved the apparent Difference of Declination of the Stars, I ſhall not now take any farther Notice in what manner ſuch a Cauſe as I have here ſuppoſed would occaſion an Alteration in their apparent Places in other Reſpects; but, ſuppoſing the Earth to move equally in a Circle, it may be gathered from what hath been already ſaid, that a Star which is is neither in the Pole nor Plain of the Ecliptick, will ſeem to deſcribe about its true Place a Figure, inſenſibly different from an Ellipſe, whoſe Tranſverſe Axis is at Right-angle to the Circle of Longitude paſſing through the Stars true Place, and equal to the Diameter of the little Circle deſcribed by a Star (as was before ſuppoſed) in the Pole of the Ecliptick; and whoſe Conjugate Axis is to its Tranſverſe Axis, as the *Sine* of the Stars Latitude to the *Radius*. And allowing that a Star by its apparent Motion does exactly deſcribe ſuch an Ellipſe, it will be found, that if A be the Angle of Poſition (or the Angle at the Star made by two great Circles drawn from it, thro' the Poles of the Ecliptick and Equator) and B be another Angle, whoſe Tangent is to the Tangent of A as Radius to the Sine of the Latitude of the Star; then B will be equal to the Difference of Longitude between the Sun and the Star, when the true and apparent Declination of the Star are the ſame. And if the Sun's Longitude in the Ecliptick be reckoned from that Point, wherein it is when this happens; then the Difference between the true and apparent Declination of the Star (on Account of the Cauſe I am now conſidering) will be always, as the Sine of the Sun's Longitude from thence. It will likewiſe be found, that the greateſt Difference of Declination that can be between the true and apparent Place of the Star, will be to the Semi-Tranſverſe Axis of the Ellipſe (or to the Semi-diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick) as the Sine of A to the Sine of B.

If the Star hath North Latitude, the Time, when its true and apparent Declination are the ſame, is before the Sun comes in Conjunction with or Oppoſition to it, if its Longitude be in the firſt or laſt Quadrant (*viz*. in the aſcending Semi-circle) of the Ecliptick; and after them, if in the deſcending Semi-circle; and it will appear neareſt to the North Pole of the Equator, at the Time of that *Maximum* (or when the greateſt Difference between the true and apparent Declination happens) which precedes the Sun's Conjunction with the Star.

These Particulars being ſufficient for my preſent Purpoſe, I ſhall not detain you with the Recital of any more, or with any farther Explication of theſe. It may be time enough to enlarge more upon this Head, when I give a Deſcription of the Inſtruments &c. if that be judged neceſſary to be done; and when I ſhall find, what I now advance, to be allowed of (as I flatter my ſelf it will) as ſomething more than a bare Hypotheſis. I have purpoſely omitted ſome matters of no great Moment, and conſidered the Earth as moving in a Circle, and not an Ellipſe, to avoid too perplexed a *Calculus*, which after all the Trouble of it would not ſenſibly differ from that which I make uſe of, eſpecially in thoſe Conſequences which I ſhall at preſent draw from the foregoing Hypotheſis.

This being premiſed, I ſhall now proceed to determine from the Obſervations, what the real Proportion is between the Velocity of Light and the Velocity of the Earth's annual Motion in its Orbit; upon Suppoſition that the *Phænomena* before mentioned do depend upon the Cauſes I have here aſſigned. But I muſt firſt let you know, that in all the Obſervations hereafter mentioned, I have made an Allowance for the Change of the Star's Declination on Account of the Preceſſion of the

the Equinox, upon Suppoſition that the Alteration from this Cauſe is proportional to the Time, and regular through all the Parts of the Year. I have deduced the real annual Alteration of Declination of each Star from the Obſervations themſelves ; and I the rather chooſe to depend upon them in this Article, becauſe all which I have yet made, concur to prove, that the Stars near the Equinoctial Colure, change their Declination at this time 1″ ½ or 2″ in a Year more than they would do if the Preceſſion was only 50″, as is now generally ſuppoſed. I have likewiſe met with ſome ſmall Varieties in the Declination of other Stars in different Years, which do not ſeem to proceed from the ſame Cauſe, particularly in thoſe that are near the ſolſtitial Colure, which on the contrary have altered their Declination leſs than they ought, if the Preceſſion was 50″. But whether theſe ſmall Alterations proceed from a regular Cauſe, or are occaſioned by any Change in the Materials &c. of my Inſtrument, I am not yet able fully to determine. However, I thought it might not be amiſs juſt to mention to you how I have endeavoured to allow for them, though the Reſult would have been nearly the ſame, if I had not conſidered them at all. What that is, I will ſhew, firſt from the Obſervations of γ *Draconis*, which was found to be 39″ more Southerly in the Beginning of *March*, than in *September*.

From what hath been premiſed, it will appear that the greateſt Alteration of the apparent Declination of γ *Draconis*, on Account of the ſucceſſive Propagation of Light, would be to the Diameter of the little Circle which a Star (as was before remarked) would ſeem to deſcribe about the Pole of the Ecliptick, as 39″ to 40″,4. The half of this is the Angle A C B (as repre-

ſented

ſented in the *Fig.*) This therefore being 20″, 2, A C will be to A B, that is, the Velocity of Light to the Velocity of the Eye (which in this Caſe may be ſuppoſed the ſame as the Velocity of the Earth's annual Motion in its Orbit) as 10210 to One, from whence it would follow, that Light moves, or is propagated as far as from the Sun to the Earth in 8′ 12″.

It is well known, that Mr. *Romer*, who firſt attempted to account for an apparent Inequality in the Times of the Eclipſes of *Jupiter*'s Satellites, by the Hypotheſis of the progreſſive Motion of Light, ſuppoſed that it ſpent about 11 Minutes of Time in its Paſſage from the Sun to us : but it hath ſince been concluded by others from the like Eclipſes, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypotheſis, is as it were a *Mean* betwixt what had at different times been determined from the Eclipſes of *Jupiter*'s Satellites.

Theſe different Methods of finding the Velocity of Light thus agreeing in the Reſult, we may reaſonably conclude, not only that theſe *Phænomena* are owing to the Cauſes to which they have been aſcribed ; but alſo, that Light is propagated (in the ſame *Medium*) with the ſame Velocity after it hath been reflected as before : for this will be the Conſequence, if we allow that the Light of the Sun is propagated with the ſame Velocity, before it is reflected, as the Light of the *fixt Stars*. And I imagine this will ſcarce be queſtioned, if it can be made appear that the Velocity of the Light of all the *fixt Stars* is equal, and that their Light moves or is propagated through equal Spaces in equal Times, at all Diſtances from them : both which points (as I apprehend) are ſufficiently proved from the apparent Alte-

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ration of the Declination of Stars of different Luſtre; for that is not ſenſibly different in ſuch Stars as ſeem near together, though they appear of very different Magnitudes. And whatever their Situations are (if I proceed according to the foregoing Hypotheſis) I find the ſame Velocity of Light from my Obſervations of ſmall Stars of the fifth or ſixth, as from thoſe of the ſecond and third Magnitude, which in all Probability are placed at very different Diſtances from us. The ſmall Star, for Example, before ſpoken of, that is almoſt oppoſite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamſteed*'s Catalogue) was 19″ more Northerly about the Beginning of *March* than in *September*. Whence I conclude, according to my Hypotheſis, that the Diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick would be 40″, 2.

The laſt Star of the great Bear's-tail of the 2d Magnitude (marked η by *Bayer*) was 36″ more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greateſt Alteration of Declination of a Star in the Pole of the Ecliptick would be 40″, 4, exactly the ſame as was before found from the Obſervations of γ *Draconis*.

The Star of the 5th magnitude in the Head of *Perſeus* marked τ by *Bayer*, was 25″ more Northerly about the End of *December* than on the 29th of *July* following. Hence the *Maximum* would be 41″. This Star is not bright enough to be ſeen as it paſſes over my Zenith about the End of *June*, when it ſhould be according to the Hypotheſis fartheſt South. But becauſe I can more certainly depend upon the greateſt Alteration of Declination of thoſe Stars, which I have frequently obſerved about the Times when they become ſtationary, with reſpect to the Motion I am now conſidering; I will ſet down a few more Inſtances of ſuch; from which you may be able to judge how near it may be poſſible from theſe Obſervations, to determine with what Velocity Light is propagated.

α *Perſei Bayero* was 23″ more Northerly at the beginning of *January* than in *July*. Hence the *Maximum* would be 40″, 2. α *Caſſiopeæ* was 34″ more Northerly about the End of *December* than in *June*. Hence the *Maximum* would be 40″, 8. β *Draconis* was 39″ more Northerly in the beginning of *September* than in *March*; hence the *Maximum* would be 40″, 2. *Capella* was about 16″ more Southerly in *Auguſt* than in *February*; hence the *Maximum* would be about 40″. But this Star being farther from my Zenith than thoſe I have before made uſe of, I cannot ſo well depend upon my Obſervations of it, as of the others; becauſe I meet with ſome ſmall Alterations of its Declination that do not ſeem to proceed from the Cauſe I am now conſidering.

I have compared the Obſervations of ſeveral other Stars, and they all conſpire to prove that the *Maximum* is about 40″ or 41″. I will therefore ſuppoſe that it is 40″½ or (which amounts to the ſame) that Light moves, or is propagated as far as from the Sun to us in 8′ 13″. The near Agreement which I met with among my Obſervations induces me to think, that the *Maximum* (as I have here fixed it) cannot differ ſo much as a Second from the Truth, and therefore it is probable that the Time which Light ſpends in paſſing from the Sun to us, may be determined by theſe Obſervations within 5″ or 10″; which is ſuch a degree of exactneſs as we can never hope to attain from the Eclipſes of *Jupiter*'s Satellites.

Having thus found the *Maximum*, or what the greateſt Alteration of Declination would be in a Star placed in the Pole of the Ecliptick, I will now deduce from it (according to the foregoing Hypotheſis) the Alteration of Declination in one or two Stars, at ſuch times as they were actually obſerved, in order to ſee how the Hypotheſis will correſpond with the *Phænomena* through all the Parts of the Year.

It would be too tedious to ſet down the whole Series of my Obſervations; I will therefore make Choice only of ſuch as are moſt proper for my preſent Purpoſe, and will begin with thoſe of γ *Draconis*.

This Star appeared fartheſt North about *September* 7th, 1727, as it ought to have done according to my Hypotheſis. The following Table ſhews how much more Southerly the Star was found to be by Obſervation in ſeveral Parts of the Year, and likewiſe how much more Southerly it ought to be according to the Hypotheſis.

1727.	D.	The Difference of Declination by Obſervation. ″	The Difference of Declination by the Hypotheſis. ″	1728.	D.	The Difference of Declination by Obſervation. ″	The Difference of Declination by the Hypotheſis. ″
October 20th --		4½	4½	*March* -	24	37	38
November -	17	11½	12	*April* - -	6	36	36½
December -	6	17½	18½	*May* - -	6	28½	29½
- - -	28	25	26	*June* - -	5	18½	20
1728				- - -	15	17½	17
January -	24	34	34	*July* - -	3	11½	11½
February -	10	38	37	*August* -	2	4	4
March - -	7	39	39	*September* -	6	0	0

Hence it appears, that the Hypotheſis correſponds with the Obſervations of this Star through all Parts of the Year; for the ſmall Differences between them ſeem to ariſe from the Uncertainty of the Obſervations, which is occaſioned (as I imagine) chiefly by the tremulous or undulating Motion of the Air, and of the Vapours in it; which cauſes the Stars ſometimes to dance to and fro, ſo much that it is difficult to judge when they are exactly on the Middle of the Wire that is fixed in the common Focus of the Glaſſes of the Teleſcope.

I muſt confeſs to you, that the Agreement of the Obſervations with each other, as well as with the Hypotheſis, is much greater than I expected to find, before I had compared them; and it may poſſibly be thought to be too great, by thoſe who have been uſed to Aſtronomical Obſervations, and know how difficult it is to make ſuch as are in all reſpects exact. But if it would be any Satiſfaction to ſuch Perſons (till I have an Opportunity of deſcribing my Inſtrument and the manner of uſing it) I could aſſure them, that in above 70 Obſervations which I made of this Star in a Year, there is but one (and that is noted as very dubious on account of Clouds) which differs from the foregoing Hypotheſis more than 2″, and this does not differ 3″.

This therefore being the Fact, I cannot but think it very probable, that the *Phænomena* proceed from the Cauſe I have aſſigned, ſince the foregoing Obſervations make it ſufficiently evident, that the Effect of the real Cauſe, whatever it is, varies in this Star, in the ſame Proportion that it ought according to the Hypotheſis.

But leaſt γ *Draconis* may be thought not ſo proper to ſhew the Proportion, in which the apparent Alteration of Declination is increaſed or diminiſhed, as thoſe Stars which lie near the Equinoctial Colure: I will give you alſo the Compariſon between the Hypotheſis and the Obſervations of η *Urſæ Majoris*; that which was fartheſt South about the 17th Day of *January* 1728, agreeable to the Hypotheſis. The following Table ſhews how much more Northerly it was found by Obſervation in ſeveral Parts of the Year, and alſo what the Difference ſhould have been according to the Hypotheſis.

1727.	d.	The Difference of Declination by Obſervation. ″	The Difference of Declination by the Hypotheſis. ″	1728.	d.	The Difference of Declination by Obſervation. ″	The Difference of Declination by the Hypotheſis. ″
September -	14	29½	28½	*April* - -	16	18½	18
- - -	24	24½	25½	*May* - -	5	24½	23½
October - -	16	19½	19½	*June* - -	5	32	31½
November -	11	11½	10½	- - -	25	35	34½
December -	14	4	3	*July* - -	17	36	36
1728							
February -	17	2	3	*Auguſt* -	2	35	35½
March - -	21	11½	10½	*September* -	20	26½	26½

I find upon Examination, that the Hypotheſis agrees altogether as exactly with the Obſervations of this Star, as the former; for in about 50 that were made of it in a Year, I do not meet with a Difference of ſo much as 2″, except in one, which is mark'd as doubtful on Account of the Undulation of the Air, *&c.* And this does not differ 3″ from the Hypotheſis.

The Agreement between the Hypotheſis and the Obſervations of this Star is the more to be reguarded, ſince it proves that the Alteration of Declination, on account of the Proceſſion of the Equinox, is (as I before ſuppoſed) regular thro' all Parts of the Year; ſo far at leaſt, as not to occaſion a Difference great enough to be diſcovered with this Inſtrument. It likewiſe proves the other part of my former Suppoſition, *viz.* that the annual Alteration of Declination in Stars near the Equinoctial Colure, is at this Time greater than a Preceſſion of 50″ would occaſion: for this Star was 20″ more Southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the Preceſſion was but 50″. But I may hereafter, perhaps, be better able to determine this Point, from my Obſervations of thoſe Stars that lie near the Equinoctial Colure, at about the ſame Diſtance from the North Pole of the Equator, and nearly oppoſite in right Aſcenſion.

I think it needleſs to give you the Compariſon between the Hypotheſis and the Obſervations of any more Stars; ſince the Agreement in the foregoing is a kind of Demonſtration (whether it be allowed that I have diſcovered the real Cauſe of the *Phænomena* or not;) that the Hypotheſis gives at leaſt the true Law of the Variation of Declination in different Stars, with Reſpect to their different Situations and Aſpects with the Sun. And if this is the Caſe, it muſt be granted, that the Parallax of the fixt Stars is much ſmaller, than hath been hitherto ſuppoſed by thoſe,

who

who have pretended to deduce it from their Obſervations. I believe, that I may venture to ſay, that in either of the two Stars laſt mentioned, it does not amount to 2″. I am of Opinion, that if it were 1″, I ſhould have perceived it, in the great number of Obſervations that I made, eſpecially of γ *Draconis*; which agreeing with the Hypotheſis (without allowing any thing for Parallax) nearly as well when the Sun was in Conjunction with, as in Oppoſition to, this Star, it ſeems very probable that the Parallax of it is not ſo great as one ſingle Second; and conſequently that it is above 400000 times farther from us than the Sun.

There appearing therefore after all, no ſenſible Parallax in the fixt Stars, the *Anti-Copernicans* have ſtill room on that Account, to object againſt the Motion of the Earth; and they may have (if they pleaſe) a much greater Objection againſt the Hypotheſis, by which I have endeavoured to ſolve the fore-mentioned *Phænomena*; by denying the progreſſive Motion of Light, as well as that of the Earth.

But as I do not apprehend, that either of theſe Poſtulates will be denied me by the Generality of the Aſtronomers and Philoſophers of the preſent Age; ſo I ſhall not doubt of obtaining their Aſſent to the Conſequences, which I have deduced from them; if they are ſuch as have the Approbation of ſo great a Judge of them as yourſelf. I am,

Sir, Your moſt Obedient
Humble Servant

J. BRADLEY.

POSTSCRIPT.

As to the Obſervations of Dr. *Hook*, I muſt own to you, that before Mr. *Molyneux*'s Inſtrument was erected, I had no ſmall Opinion of their Correctneſs; the Length of his Teleſcope and the Care he pretends to have taken in making them exact, having been ſtrong Inducements with me to think them ſo. And ſince I have been convinced both from Mr. *Molyneux*'s Obſervations and my own, that the Doctor's are really very far from being either exact or agreeable to the *Phænomena*; I am greatly at a Loſs how to account for it. I cannot well conceive that an Inſtrument of the Length of 36 Feet, conſtructed in the Manner he deſcribes his, could have been liable to an Error of near 30″ (which was doubtleſs the Caſe) if rectified with ſo much Care as he repreſents.

The Obſervations of Mr. *Flamſteed* of the different Diſtances of the Pole Star from the Pole at different Times of the Year, which were through Miſtake looked upon by ſome as a Proof of the annual *Parallax* of it, ſeem to have been made with much greater Care than thoſe of Dr. *Hook*. For though they do not all exactly correſpond with each other, yet from the whole Mr. *Flamſteed* concluded that the Star was 35″ 40″ or 45″ nearer the Pole in *December* than in *May* or *July*: and according to my Hypotheſis it ought to appear 40″ nearer in *December* than in *June*. The Agreement therefore of the Obſervations with the Hypotheſis is greater than could reaſonably be expected, conſidering the *Radius* of the Inſtrument, and the Manner in which it was conſtructed.

CASSINI := TABLES FOR ECLIPSES OF THE FIRST SATELLITE OF JUPITER.
PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY
VOLUME XVIII, No 213, 1694, pp 237-256

II. *Monſieur* Caſſini *his New and Exact Tables for the* Eclipſes *of the Firſt* Satellite *of* Jupiter, *reduced to the* Julian *Stile, and Meridian of* London.

AMong the Books the *Royal Academy* of *Sciences* at *Paris* has lately gratified the World withal, there is one which has for Title, *Recueuil d'Obſervations faites en pluſieurs Voiages pour parfectionner l'Aſtronomie & la Geographie, Avec divers traitez Aſtronomiques.* In which thoſe *Sçavans* have ſet a very commendable Example in aſcertaining by undoubted Obſervations the true *Geographical Site* of all the Principal Ports of *France*, which it were to be wiſhed other Nations would imitate. By this Survey they have demonſtrated the Encroachments their *Geographers*, and particularly *Sanſon*, had made on the *Sea* to enlarge their *Kingdom*, and have retrenched more of their Uſurpations on the *Weſt*, *South*, and *North*, than all their *Acquiſts* on the *Eaſt* amount to twice told.

The Method they have uſed to determine the *Longitudes* of their Places, is by the Obſervation of the *Eclipſes* of the *Firſt Satellite* of *Jupiter*, which they find almoſt inſtantaneous, and with good Teleſcopes diſcernable almoſt to the very Oppoſition of *Jupiter* to the *Sun:* And it may be ſaid, that this Account of the *Longitudes* obſerved, has put it paſt doubt that this is the very beſt way, could portable Teleſcopes ſuffice for the Work. And could theſe *Satellites* be obſerved at *Sea*, a *Ship* at *Sea* might be enabled to find the Meridian ſhe was in, by help of the Tables Monſieur *Caſſini* has given us in this Volume, diſcovering with very great exactneſs the ſaid *Eclipſes*, beyond what we can yet hope to do by the *Moon*, tho' ſhe ſeem to afford us the only means Practicable for the *Seaman*. However before Saylors can make uſe of the Art of finding the *Longitude*, it will be requiſite that the Coaſt of the whole Ocean be firſt laid down truly, for which work this Method by the *Satellites* is moſt appoſite: And it may be hoped that either the true Geometrick Theory of the *Moon* may be diſcovered, by the time the Charts are

com-

compleated; or elſe that ſome Invention of ſhorter Teleſcopes manageable on Ship-board, may ſuffice to ſhew the *Eclipſes* of the *Satellites* at Sea, at leaſt thoſe of the Third *Satellite*, which fall at a good diſtance from the Body of *Jupiter*, being near three times as far from him as the firſt.

The laſt but moſt conſiderable Treatiſe of this Collection gives the aforeſaid Tables for computing the Motions of *Jupiter's Satellites*, but more eſpecially thoſe, for ſpeedy finding the Eclipſes of the firſt or innermoſt. Wherein Monſieur *Caſſini* has employed his Skill to make eaſie and obvious to all Capacities the Calculation of them, which is otherwiſe operoſe to the Skilful, and not to be undertaken by the leſs knowing, who yet perhaps would be willing to find the Longitude of the Places they live in.

Theſe Tables have for Principles, That the innermoſt *Satellite* revolves to the Sun in $1^{d.}\ 18^{h.}\ 28'.\ 36''$. ſo preciſely, that in 100 Years the difference is not ſenſible; That in the time of the Revolution of *Jupiter* to his *Aphelion*, which he ſuppoſes in $4332^{d.}\ 14^{h.}\ 52'.\ 48''$, this *Satellite* makes exactly 2448 Months or Revolutions to the Sun: and dividing the Orbite of *Jupiter* into 2448 parts, he has in a large Table of Æquation ſhewn what is the inequality of the Motion of *Jupiter* in each Revolution reduced to Time, aſſuming *Thirdly*, the greateſt Æquation of *Jupiter* $5^{o.}\ 30'$. whence the hourly Motion of the *Satellite* from *Jupiter* being $8^{o.}\ 26'\frac{1}{4}$, it follows, that the greateſt inequality (*Jupiter* paſſing the Signs of *Cancer* and *Capricorn*,) amounts to $39'.\ 8''$. of time, to be added in *Cancer*, ſubſtracted in *Capricorn*. *Laſtly*, As to the *Epocha* or beginning of this Series of Revolutions, he has determined the *Aphelion* of *Jupiter* about $1\frac{1}{2}$ Degree forwarder than *Aſtronomia Carolina*, and above 2 Degrees more than the *Rudolphine Tables*, *viz.* preciſely in $9^{o.}$ of *Libra*, in the beginning of this Century, which perhaps he finds the proper Motion of *Jupiter* about the Sun at this time to require; and the number of Revolutions ſince *Jupiter* was laſt in *Perihelio*, is here ſtiled *Num.* I.

A ſecond Inequality is that which depends on the diſtance of the Sun from *Jupiter*, which he ſays Monſieur *Romer* did moſt ingeniouſly explain by the Hypotheſis of the Motion of Light, to which yet *Caſſini* by his manner of *Calculus* ſeems not to aſſent, though it be hard to imagine how the Earth's Poſition in reſpect of *Jupiter* ſhould any way affect the Motion of the *Satellites*. This Inequality he makes to amount to two Degrees in the *Satellites* Motion, or $14'.\ 10''$. of Time, wherein he ſuppoſes the Eclipſes to happen ſo much ſooner when *Jupiter* Oppoſes the Sun, than when he is in Conjunction with him. The diſtribution of this Inequality he makes wholly to depend on the Angle at the Sun between the Earth and *Jupiter*, without any regard to the Excentricity of *Jupiter*, (who is ſometimes $\frac{1}{2}$ a Semidiameter of the Earth's Orb farther from the Sun than at other times) which would occaſion a much greater difference than the Inequality of *Jupiter* and the *Earth's* Motion, both of which are accounted for in theſe Tables with great Skill and Addreſs. But what is moſt ſtrange, he affirms that the ſame Inequality of two Degrees in the Motion, is likewiſe found in the other *Satellites*, requiring a much greater time, as above two Hours in the fourth *Satellite*: which if it appeared by Obſervation, would overthrow Monſieur *Romer*'s Hypotheſis entirely. Yet I doubt not herein to make it demonſtratively plain, that the Hypotheſis of the progreſſive Motion of Light is found in all the other *Satellites* of *Jupiter* to be neceſſary, and that it is the ſame in all; there being nothing near ſo great an Annual Inequality as Monſieur *Caſſini* ſuppoſes in their Motions, by his Table, *pag.* 9. and his *Præcepta Calculi*. The Method however uſed to compute this is very Curious; for having found that whilſt the Sun revolves to *Jupiter*, there paſs $398^{d.}\ 21^{h.}\ 13'$. wherein are made $225\frac{3}{4}$ Revolutions of the *Satellite* to *Jupiter*, the Number of Revolutions ſince *Jupiter* was laſt in Oppoſition to the Sun, is what he calls *Num.* II. in which the Inequality of the Earth's Motion is allowed for in the Months, and that of *Jupiter's* Orb by a Table of the Æquation of *Num.* II. amounting in all to $3\frac{1}{2}$ Revolutions of the *Satellite* to *Jupiter*. This in the Tables following I have thought fit to leave out, ſhewing how to find it by help of the former Æquation of *Num.* I. The Numbers are in effect the ſame with Monſieur *Caſſini's*, only reduced to our Stile and Meridian, and the form of them abridged, and 'tis hoped amended. *See* Philoſ. Tranſact. N^o. 136.

Epochæ Revolutionum primi Satellitis ad Jovis Umbram ſub Meridiano Londinenſi.

Anno Jul. Curr.	D. h. ′ ″	Num. I	Num. II	Anno Jul. Curr.	D. h. ′ ″	Num. I	Num. II
1660	0 11 5 48	968	200,6	1690	0 16 8 24	2263	81,0
61	0 1 17 24	1174	181,2	91	0 6 20 0	21	61,6
62	1 9 57 36	1381	162,9	92	0 15 0 12	228	43,3
63	1 0 9 12	1587	143,5	93	0 5 11 48	434	23,9
1664	1 18 49 24	1794	125,1	94	1 13 52 0	641	5,5
65	0 23 1 0	2000	105,7	1695	1 4 3 36	847	211,5
1666	0 13 12 36	2206	86,4	96	1 12 43 48	1054	193,1
67	0 3 24 12	2412	67,0	97	1 2 55 24	1260	173,7
68	0 12 4 24	171	48,6	98	0 17 7 0	1466	154,4
1669	0 2 16 0	377	29,2	99	0 7 18 36	1672	135,0
70	1 10 56 12	584	10,9	1700	0 15 58 48	1879	116,6
71	1 1 7 48	790	216,9	01	0 6 10 24	2085	97,3
72	1 9 48 0	997	198,5	02	1 14 50 36	2292	78,9
73	0 23 59 36	1203	179,1	03	1 5 2 12	50	59,5
1674	0 14 11 12	1409	159,7	04	1 13 42 24	257	41,1
75	0 4 22 48	1615	140,3	1705	1 3 54 0	463	21,8
76	0 13 3 0	1822	121,9	06	0 18 5 36	669	2,4
77	0 3 14 36	2028	102,5	07	0 8 17 12	875	208,4
78	1 11 54 48	2235	84,1	08	0 16 57 24	1082	190,0
1679	1 2 6 24	2441	64,7	09	0 7 9 0	1288	170,6
80	1 10 46 36	200	46,4	1710	1 15 49 12	1495	152,3
81	1 10 58 12	406	27,0	11	1 6 0 48	1701	132,9
82	0 15 9 48	612	7,6	12	1 14 41 0	1908	114,5
83	0 5 21 24	818	213,6	13	1 4 52 36	2114	95,1
1684	0 14 1 36	1025	195,3	14	0 19 4 12	2320	75,8
85	0 4 13 12	1231	175,9	1715	0 9 15 48	78	56,4
86	1 10 53 24	1438	157,5	16	0 17 56 0	285	38,0
87	1 3 5 0	1644	138,1	17	0 8 7 36	491	18,6
88	1 11 45 12	1851	119,7	18	1 16 47 48	698	0,3
1689	1 1 56 48	2057	100,4	19	1 6 59 24	904	206,3
				1720	1 15 39 36	1111	187,9

Tabula

Tabula Revolutionum primi Satellitis Jovis in Anno.

Januarius. D. h. ′ ″	Num. I	Num. II
0 0 0 0	0	0,0
1 18 28 36	1	1,0
3 12 57 12	2	2,1
5 7 25 48	3	3,1
7 1 54 24	4	4,1
8 20 23 0	5	5,2
10 14 51 36	6	6,2
12 9 20 12	7	7,2
14 3 48 48	8	8,2
15 22 17 24	9	9,3
17 16 46 0	10	10,3
19 11 14 36	11	11,3
21 5 43 12	12	12,3
23 0 11 48	13	13,4
24 18 40 24	14	14,4
26 13 9 0	15	15,4
28 7 37 36	16	16,5
30 2 6 12	17	17,5
31 20 34 48	18	18,5
Februarius.		
0 20 34 48	18	18,5
2 15 3 24	19	19,6
4 9 32 0	20	20,6
6 4 0 36	21	21,6
7 22 29 12	22	22,6
9 16 57 48	23	23,7
11 11 26 24	24	24,7
13 5 55 0	25	25,7

Februarius. D. h. ′ ″	Num. I	Num. II
13 5 55 0	25	25,7
15 0 23 36	26	26,7
16 18 52 12	27	27,7
18 13 20 48	28	28,7
20 7 49 24	29	29,7
22 2 18 0	30	30,8
23 20 46 36	31	31,8
25 15 15 12	32	32,8
27 9 43 48	33	33,8
Martius.		
1 4 12 24	34	34,8
2 22 41 0	35	35,8
4 17 9 36	36	36,8
6 11 38 12	37	37,9
8 6 6 48	38	38,9
10 0 35 24	39	39,9
11 19 4 0	40	40,9
13 13 32 36	41	41,9
15 8 1 12	42	42,9
17 2 29 48	43	43,9
18 20 58 24	44	44,9
20 15 27 0	45	45,9
22 9 55 36	46	46,9
24 4 24 12	47	47,9
25 22 52 48	48	48,9
27 17 21 24	49	49,9
29 11 50 0	50	50,9
31 6 18 36	51	51,9

Aprilis.

Tabula Revolutionum primi Satellitis Jovis *in Anno.*

D.	h.	′	″	Num. I.	Num. II.
Aprilis.					
0	6	18	36	51	51,9
2	0	47	12	52	52,9
3	19	15	48	53	53,9
5	13	44	24	54	54,9
7	8	13	0	55	55,9
9	2	41	36	56	56,9
10	21	10	12	57	57,9
12	15	38	48	58	58,9
14	10	7	24	59	59,9
16	4	36	0	60	60,8
17	23	4	36	61	61,8
19	17	33	12	62	62,8
21	12	1	48	63	63,8
23	6	30	24	64	64,8
25	0	59	0	65	65,7
26	19	27	36	66	66,7
28	13	56	12	67	67,7
30	8	24	48	68	68,6
Maius.					
0	8	24	48	68	68,6
2	2	53	24	69	69,6
3	21	22	0	70	70,6
5	15	50	36	71	71,6
7	10	19	12	72	72,5
9	4	47	48	73	73,5
10	23	16	24	74	74,5
12	17	45	0	75	75,5
14	12	13	36	76	76,4

D.	h.	′	″	Num. I.	Num. II.
Maius.					
14	12	13	36	76	76,4
16	6	42	12	77	77,4
18	1	10	48	78	78,4
19	19	39	24	79	79,3
21	14	8	0	80	80,3
23	8	36	36	81	81,3
25	3	5	12	82	82,3
26	21	33	48	83	83,3
28	16	2	24	84	84,2
30	10	31	0	85	85,2
Junius.					
1	4	59	36	86	86,1
2	23	28	12	87	87,1
4	17	56	48	88	88,0
6	12	25	24	89	89,0
8	6	54	0	90	90,0
10	1	22	36	91	90,9
11	19	51	12	92	91,9
13	14	19	48	93	92,9
15	8	48	24	94	93,8
17	3	17	0	95	94,8
18	21	45	36	96	95,7
20	16	14	12	97	96,7
22	10	42	48	98	97,7
24	5	11	24	99	98,6
25	23	40	0	100	99,6
27	18	8	36	101	100,6
29	12	37	12	102	101,5

Julius.

Tabula Revolutionum primi Satellitis Jovis *in Anno.*

D.	h.	′	″	Num. I.	Num. II.
Julius.					
1	7	5	48	103	102,5
3	1	34	24	104	103,5
4	20	3	0	105	104,4
6	14	31	36	106	105,4
8	9	0	12	107	106,4
10	3	28	48	108	107,3
11	21	57	24	109	108,3
13	16	26	0	110	109,3
15	10	54	36	111	110,2
17	5	23	12	112	111,2
18	23	51	48	113	112,2
20	18	20	24	114	113,1
22	12	49	0	115	114,1
24	7	17	36	116	115,1
26	1	46	12	117	116,0
27	20	14	48	118	117,0
29	14	43	24	119	118,0
31	9	12	0	120	119,0
Auguſtus.					
0	9	12	0	120	119,0
2	3	40	36	121	119,9
3	22	9	12	122	120,9
5	16	37	48	123	121,9
7	11	6	24	124	122,9
9	5	35	0	125	123,8
11	0	3	36	126	124,8
12	18	32	12	127	125,8
14	13	0	48	128	126,8

D.	h.	′	″	Num. I.	Num. II.
Auguſtus.					
14	13	0	48	128	126,8
16	7	29	24	129	127,7
18	1	58	0	130	128,7
19	20	26	36	131	129,7
21	14	55	12	132	130,7
23	9	23	48	133	131,7
25	3	52	24	134	132,7
26	22	21	0	135	133,6
28	16	49	36	136	134,6
30	11	18	12	137	135,6
September.					
1	5	46	48	138	136,6
3	0	15	24	139	137,6
4	18	44	0	140	138,6
6	13	12	36	141	139,6
8	7	41	12	142	140,6
10	2	9	48	143	141,5
11	20	38	24	144	142,5
13	15	7	0	145	143,5
15	9	35	36	146	144,5
17	4	4	12	147	145,5
18	22	32	48	148	146,5
20	17	1	24	149	147,5
22	11	30	0	150	148,5
24	5	58	36	151	149,5
26	0	27	12	152	150,5
27	18	55	48	153	151,5
29	13	24	24	154	152,5

October

Tabula Revolutionum primi Satellitis Jovis in Anno.

D.	h.	′	″	Num. I	Num. II
October.					
1	7	53	0	155	153,5
3	2	21	36	156	154,5
4	20	50	12	157	155,5
6	15	18	48	158	156,5
8	9	47	24	159	157,5
10	4	16	0	160	158,5
11	22	44	36	161	159,5
13	17	13	12	162	160,5
15	11	41	48	163	161,6
17	6	10	24	164	162,6
19	0	39	0	165	163,6
20	19	7	36	166	164,6
22	13	36	12	167	165,6
24	8	4	48	168	166,6
26	2	33	24	169	167,7
27	21	2	0	170	168,7
29	15	30	36	171	169,7
31	9	59	12	172	170,7
November.					
0	9	59	12	172	170,7
2	4	27	48	173	171,8
3	22	56	24	174	172,8
5	17	25	0	175	173,8
7	11	53	36	176	174,8
9	6	22	12	177	175,9
11	0	50	48	178	176,9
12	19	19	24	179	177,9
14	13	48	0	180	178,9
16	8	16	36	181	180,0

D.	h.	′	″	Num. I	Num. II
November.					
16	8	16	36	181	180,0
18	2	45	12	182	181,0
19	21	13	48	183	182,0
21	15	42	24	184	183,0
23	10	11	0	185	184,0
25	4	39	36	186	185,1
26	23	8	12	187	186,1
28	17	36	48	188	187,2
30	12	5	24	189	188,2
December.					
0	12	5	24	189	188,2
2	6	34	0	190	189,2
4	1	2	36	191	190,3
5	19	31	12	192	191,3
7	13	59	48	193	192,3
9	8	28	24	194	193,4
11	2	57	0	195	194,4
12	21	25	36	196	195,5
14	15	54	12	197	196,5
16	10	22	48	198	197,6
18	4	51	24	199	198,6
19	23	20	0	200	199,7
21	17	48	36	201	200,7
23	12	17	12	202	201,8
25	6	45	48	203	202,8
27	1	14	24	204	203,9
28	19	43	0	205	204,9
30	14	11	36	206	206,0

Tabula Primæ Æquationis Conjunctionum primi Satellitis cum Jove.

Num. I	Æquat. ′	″	Num. I	Æquat. ′	″	Num. I	Æquat. ′	″	Num. I	Æquat. ′	″
0	0	0	300	28	9	610	39	5	920	26	37
10	1	3	310	28	54	620	39	3	930	25	53
20	2	5	320	29	35	630	38	58	940	25	8
30	3	8	330	30	11	640	38	51	950	24	23
40	4	12	340	30	45	650	38	44	960	23	37
50	5	15	350	31	28	660	38	34	970	22	50
60	6	16	360	32	10	670	38	24	980	22	3
70	7	19	370	32	44	680	38	10	990	21	15
80	8	20	380	33	15	690	37	56	1000	20	26
90	9	23	390	33	49	700	37	40	1010	19	37
100	10	25	400	34	20	710	37	24	1020	18	47
110	11	25	410	34	51	720	37	5	1030	17	56
120	12	25	420	35	21	730	36	45	1040	17	5
130	13	25	430	35	47	740	36	25	1050	16	13
140	14	25	440	36	6	750	36	4	1060	15	19
150	15	22	450	36	26	760	35	40	1070	14	25
160	16	18	460	36	47	770	35	15	1080	13	32
170	17	17	470	37	8	780	34	49	1090	12	37
180	18	11	480	37	29	790	34	19	1100	11	42
190	19	9	490	37	44	800	33	49	1110	10	47
200	20	5	500	37	59	810	33	21	1120	9	52
210	20	56	510	38	16	820	32	50	1130	8	57
220	21	49	520	38	29	830	32	17	1140	8	0
230	22	41	530	38	39	840	31	44	1150	7	3
240	23	32	540	38	49	850	31	10	1160	6	7
250	24	20	550	38	55	860	30	32	1170	5	19
260	25	7	560	38	59	870	29	56	1180	4	13
270	25	57	570	39	3	880	29	19	1190	3	15
280	26	43	580	39	6	890	28	40	1200	2	19
290	27	27	590	39	8	900	27	59	1210	1	21
300	28	9	600	39	7	910	27	19	1220	0	24
			610	39	5	920	26	37	1224	0	0

Tabula Secundæ Æquationis Conjunctionum primi Satellitis *cum* Jove.

Num. II.	Æquat. add.	Num. II.	Æquat. add.	Num. II.	Æquat. add.	Num. II.	Æquat. add.
0	0′ 0″	28	2′ 4″	56	7′ 0″	84	12′ 0″
1	0 0	29	2 13	57	7 12	85	12 9
2	0 1	30	2 21	58	7 24	86	12 16
3	0 2	31	2 30	59	7 36	87	12 24
4	0 3	32	2 39	60	7 47	88	12 32
5	0 4	33	2 48	61	7 59	89	12 40
6	0 6	34	2 58	62	8 11	90	12 47
7	0 8	35	3 8	63	8 22	91	12 53
8	0 10	36	3 17	64	8 34	92	13 0
9	0 14	37	3 27	65	8 46	93	13 6
10	0 17	38	3 37	66	8 57	94	13 13
11	0 20	39	3 48	67	9 8	95	13 19
12	0 23	40	3 59	68	9 20	96	13 24
13	0 27	41	4 9	69	9 32	97	13 30
14	0 32	42	4 20	70	9 44	98	13 35
15	0 37	43	4 31	71	9 54	99	13 39
16	0 42	44	4 41	72	10 3	100	13 45
17	0 47	45	4 53	73	10 14	101	13 48
18	0 53	46	5 4	74	10 25	102	13 51
19	0 58	47	5 15	75	10 35	103	13 54
20	1 4	48	5 27	76	10 45	104	13 57
21	1 11	49	5 39	77	10 55	105	14 0
22	1 18	50	5 50	78	11 5	106	14 3
23	1 25	51	6 2	79	11 15	107	14 5
24	1 32	52	6 14	80	11 25	108	14 7
25	1 40	53	6 25	81	11 34	109	14 8
26	1 47	54	6 37	82	11 43	110	14 9
27	1 56	55	6 49	83	11 52	111	14 10
28	2 4	56	7 0	84	12 0	112	14 10

Tabula Dimidiæ Moræ primi Satellitis *in Umbra* Jovis.

Num. I.	H. ′ ″	Num. I.	H. ′ ″
0	1 4 56	1200	1 5 6
40	1 4 33	1240	1 4 48
80	1 4 12	1280	1 4 26
120	1 3 59	1320	1 4 7
160	1 3 48	1360	1 3 54
200	1 3 39	1400	1 3 38
240	1 3 38	1440	1 3 38
280	1 3 48	1480	1 3 44
320	1 4 1	1520	1 3 52
360	1 4 16	1560	1 4 7
400	1 4 36	1600	1 4 24
440	1 4 56	1640	1 4 42
480	1 5 18	1680	1 5 0
520	1 5 41	1720	1 5 22
560	1 6 1	1760	1 5 46
600	1 6 21	1800	1 6 10
640	1 6 39	1840	1 6 28
680	1 6 53	1880	1 6 45
720	1 7 3	1920	1 6 57
760	1 7 11	1960	1 7 7
800	1 7 15	2000	1 7 13
840	1 7 13	2040	1 7 14
880	1 7 9	2080	1 7 15
920	1 7 2	2120	1 7 15
960	1 6 54	2160	1 7 10
1000	1 6 39	2200	1 6 49
1040	1 6 22	2240	1 6 32
1080	1 6 5	2280	1 6 15
1120	1 5 45	2320	1 5 58
1160	1 5 26	2360	1 5 38
1200	1 5 6	2400	1 5 18
		2440	1 5 2

TABULA ÆQUATIONIS DIERUM
cum Solis loco adeunda.

	♈	♉	♊	♋	♌	♍
G.	′ S ″	′ A ″	′ A ″	′ S ″	′ S ″	′ S ″
0	7 45	1 11	4 3	0 59	5 43	2 8
1	7 26	1 24	4 0	1 15	5 45	1 53
2	7 7	1 37	3 56	1 29	5 46	1 37
3	6 48	1 49	3 51	1 42	5 47	1 21
4	6 29	2 1	3 45	1 54	5 48	1 5
5	6 10	2 12	3 39	2 6	5 48	0 48
6	5 51	2 23	3 32	2 19	5 48	0 30
7	5 31	2 33	3 25	2 32	5 46	0 12
8	5 11	2 43	3 17	2 44	5 44	0 A 7
9	4 51	2 53	3 9	2 56	5 40	0 26
10	4 31	3 3	3 0	3 8	5 36	0 45
11	4 11	3 13	2 51	3 20	5 31	1 3
12	3 52	3 22	2 41	3 32	5 25	1 21
13	3 33	3 30	2 31	3 43	5 19	1 40
14	3 14	3 37	2 21	3 54	5 13	1 59
15	2 55	3 43	2 10	4 4	5 6	2 19
16	2 37	3 48	2 0	4 14	4 58	2 40
17	2 19	3 53	1 49	4 24	4 49	3 1
18	2 1	3 57	1 37	4 34	4 39	3 22
19	1 43	4 1	1 25	4 43	4 30	3 44
20	1 26	4 5	1 13	4 51	4 20	4 6
21	1 9	4 8	1 1	4 59	4 9	4 29
22	0 52	4 10	0 49	5 6	3 57	4 51
23	0 35	4 12	0 37	5 13	3 45	5 13
24	0 19	4 13	0 24	5 19	3 32	5 35
25	0 3	4 11	0 10	5 24	3 19	5 57
26	0 A 12	4 9	0 S 3	5 29	3 5	6 19
27	0 27	4 8	0 16	5 33	2 51	6 41
28	0 42	4 6	0 29	5 37	2 37	7 2
29	0 57	4 5	0 44	5 40	2 23	7 23
30	1 11	4 3	0 59	5 43	2 8	7 44

TABULA

TABULA ÆQUATIONIS DIERUM.

	♎	♏	♐	♑	♒	♓
G.	′ A ″	′ A ″	′ A ″	′ A ″	′ S ″	′ S ″
0	7 44	15 34	13 25	0 59	11 48	14 36
1	8 5	15 42	13 7	0 27	12 4	14 29
2	8 25	15 48	12 48	0 S 5	12 19	14 21
3	8 45	15 53	12 29	0 35	12 35	14 13
4	9 5	15 57	12 10	1 4	12 50	14 4
5	9 25	16 1	11 50	1 33	13 5	13 55
6	9 44	16 5	11 30	2 3	13 19	13 46
7	10 3	16 7	11 10	2 32	13 32	13 37
8	10 22	16 8	10 49	3 1	13 44	13 27
9	10 41	16 9	10 28	3 29	13 55	13 17
10	11 0	16 9	10 6	3 57	14 5	13 7
11	11 19	16 9	9 42	4 25	14 14	12 56
12	11 38	16 8	9 17	4 53	14 22	12 44
13	11 57	16 7	8 51	5 20	14 29	12 32
14	12 15	19 5	8 25	5 48	14 35	12 19
15	12 33	16 1	7 58	6 15	14 40	12 6
16	12 50	15 56	7 31	6 42	14 45	11 52
17	13 7	15 50	7 5	7 9	14 50	11 37
18	13 22	15 44	6 38	7 34	14 54	11 21
19	13 36	15 37	6 12	7 58	14 56	11 4
20	13 49	15 30	5 45	8 21	14 58	10 46
21	14 2	15 22	5 19	8 45	14 59	10 28
22	14 14	15 13	4 52	9 8	15 0	10 10
23	14 26	15 3	4 26	9 31	15 0	9 52
24	14 37	14 52	3 58	9 53	15 0	9 34
25	14 47	14 40	3 30	10 13	14 58	9 16
26	14 57	14 27	3 1	10 32	14 55	8 58
27	15 7	14 13	2 31	10 51	14 51	8 49
28	15 16	13 58	2 1	11 10	14 47	8 22
29	15 25	13 42	1 30	11 29	14 42	8 4
30	15 34	13 25	0 59	11 48	14 36	7 45

This

This laſt Table of the Æquation of Natural Days might have been ſpared, as being publiſht in ſeveral other places, but it was thought proper to have all the *Elements* of this *Calculus* together, that there might be no occaſion of any other Book to perform it.

The Uſe of the Tables.

To any given Year, Month, and Day, to find the next Eclipſe of the firſt Satellite *of* Jupiter.

I. In the Table of *Epochæ* (*pag.* 240.) find the Year of our Lord, and ſet down the Day, Hours, Minutes, and Seconds, with the Num. I. and Num. II. thereto annext; and (in *pag.* 241 and the following) ſeek the Month, and day of the Month, with the Hours and Minutes, and Num. I. and II. affixt, and add them together: and the reſpective Sums ſhall ſhew the mean time of the middle of the Eclipſe ſought, with Num. I. and Num. II. required. But it muſt be obſerved, that in *January* and *February* in the Leap-Year one Day is to be added to the Day thus found.

II. If Num. I. be found leſs than 1224 with Num. I; or if greater than 2448, Subſtracting 2448 therefrom, with the reſidue, enter the Table, *pag.* 245. and you will have the firſt Æquation to be *added* to the mean Time before found. But if Num. I. be leſs than 2448, but greater than 1224, Subſtract it from 2448, and entring the ſame Table with the remainder, you ſhall have the firſt Æquation to be *ſubſtracted* from the mean Time. Then Divide the Minutes of the ſaid firſt Æquation by 11, or rather $\frac{76}{7}$, and the *Quote* ſhall be the Æquation of Num. II. (anſwering to the Eccentrick Motion of *Jupiter*) to be *added* thereto when the firſt Æquation *Subſtracts*, and *è contra ſubſtracted* when that *adds*.

III. If

III. If Num. II. thus æquated exceed 225,4, Subſtract 225,4 therefrom; and if the remainder or Num. II. be leſs than 113, with the ſaid remainder or Number; or if greater than 113, with the complement thereof to 225,4. ſeek in Table *pag.* 246. the ſecond Æquation, which being added to the Time before found, gives the true Time of the middle of the Eclipſe.

IV. With Num. I. in *Tab. pag.* 247, ſeek the half Continuance of the Total Eclipſe, which is to be added for the *Emerſion* when the æquated Num. II. is leſs than 113, or if more than 225,4, it be leſs than 338. But if it exceed 113 or 338, then is the *Semimora* to be ſubſtracted for the *Immerſion*.

V. Laſtly, with the Sun's true Place take out the Æquation of Natural Days (in *Tab. pag.* 248.) which added or ſubſtracted according to the Title, gives the time of the *Immerſion* or *Emerſion* ſought.

Now how few Figures ſerve for this Computation, will beſt appear by an Example or two.

Anno 1677. *September* 17th. 8h. 9′. 40″. at *Greenwich*, Mr. *Flamſteed* obſerved the firſt *Satellite* to begin to *Emerge*; that is 8h. 9′. 20″. at *London*.

					Num. I.	Num. II.
1677.	0d.	3h.	14′	36″	2028	102,5
Sept.	17	4	4	12	147	145,5
Sept.	17	7	18	48	2175	248,0
Æquat. 1.	—		26	11.	2448	2,3 +
	17	6	52	37	273	250,3
Æquat. 2.	+		1	39		225,4
Semimora	+	1	7	0		24,9
Equal Time	17	8	1	16	11)26,2(2,3 +	
Æquation	+		9	25	☉ in ♒ 5,00	
Appar. T.	17	8	10	41		
Obſer.		8	9	20		
Error	—		1	21		

Again,

Again, *Anno* 1683. *November* 30th. 16h. 48'. 40". under the Meridian of *London*, the *Immerſion* of this *Satellite* was obſerved by *E. Halley.*

						Num. I.	Num. II.
1683.		0d.	5h.	21'	24"	818	213,6
Novemb.		30	12	5	24	189	188,2
Novemb.		30	17	26	48	1007	401,8
Æquat. 1.	+			19	52		1,8 —
Æquat. 2.	+			6	0	11)20(1,8 —	400,0
Novemb.		30	17	52	40		225,4
Semimora	—		1	6	36		174,6
Temp.æquat.		30	16	46	4		50,8
Æquat. T.	+			6	3	⊙ in ♐ 19°. 20'.	
Novemb.		30	16	52	7	Temp. appar.	
Obſer.			16	48	40		
Error	—			3	27		

A Third Example ſhall be the *Emerſion* obſerved at *Paris* by Monſieur *Caſſini Anno* 1693. *January* 14th. 10h. 40'. 28". that is, at *London* at 10h. 30'. 48".

						Num. I.	Num. II.
1693.		0d.	5h.	11'	48"	434	23,9
Jan.		14	3	48	48	8	8,2
Æquat. 1.	+			36	8	442	32,1
Æquat. 2.	+			2	13		3,2 —
Semimora	+		1	4	57	11)36,(3,2 —	28,9
Temp.æquat.		14	10	43	54		
Æquat.	—			13	15	⊙ in ♒ 5°. 40'.	
Januarii		14	10	30	39	Temp. app.	
Obſer.			10	30	48		
Error	+			0	9		

After this manner I have compared theſe Tables with many good and certain Obſervations, and ſcarce ever find them err above three or four Minutes of Time; which proceeds, as may well be conjectured, from ſome ſmall Eccentricity in its Motion, and from the Oval Figure of *Jupiter*'s Body, whoſe quick diurnal Rotation has by its *Vis Centrifuga* dilated his Equinoctial, and made his *Meridians* much *Elliptical*, ſo as to be diſcernable by the Teleſcope. Mr. *Newton* has ſhewn that his Polar Diameter is to that of his Equinoctial as 40 to 41 nearly. But we may hope future Obſervations may ſhew how to divide thoſe compounded cauſes of Error, and correct them; which Errors are exceeding ſmall in compariſon of the ſhort time that the *Satellites* have been diſcovered, and argue the Skill and Diligence of the deſervedly Famous Author of theſe *Tables.*

I had almoſt forgot the Conſtruction of the Table, *pag.* 247. ſhewing the half continuance of theſe Eclipſes: In this the Semidiameter of the ſhadow of *Jupiter* is made by *Caſſini* juſt 10 Degrees, and that of the *Satellite* 30'; and the *Satellites* Aſcending Node being ſuppoſed in 15°. of *Aquarius*, at the end of this Century, (that is, 55°. 20'. before the *Perihelion* of *Jupiter*) it will thence follow, that Num. I. being 816 or 2102, *Jupiter* paſſes the *Nodes* of the *Satellites* Orb, and conſequently theſe Eclipſes are Central, and of the greateſt Duration. But Num. I. being 215 or 1481, the *Satellite* paſſes the ſhadow with the greateſt Obliquity, *viz.* 2°. 55' from the Center, whence the *Semimora* becomes of all the ſhorteſt. This Table is not however ſo nicely computed, but that it may admit of Correction in the Seconds, if a ſmall part of a Minute were conſiderable in this affair.

The Tables of the other *three Satellites* not being ſo perfect or exact as thoſe of the *firſt*, having greater inequalities, are here given in another form, requiring the aſſiſtance of the Tables of *Jupiter*'s proper motion. The Periods of their Revolutions to *Jupiter*'s ſhade are as follows:

Period. Secundi.	$3^{d.}$	$13^{h.}$	17′.	54″.	3‴	*ſive* $2\frac{1}{113}$ *Rev. primi.*
Period. Tertii.	7	3	59	39	22	*ſive* $4\frac{1}{22}$ *Rev. primi.*
Period. Quarti.	16	18	5	6	50	*ſive* $9\frac{7}{15}$ *Rev. primi.*

Whence the Table of the firſt Æquation of the *Firſt Satellite*, *pag.* 245, or Monſieur *Caſſini's* larger Table, may by an eaſie Reduction ſerve the other three; the Æquation of the *Second* being $2\frac{1}{113}$, or twice the Minutes with half ſo many Seconds as there are Minutes in the Æquation of the *Firſt*, and the greateſt Æquation thereof $1^{h.}$ 18′. 35″. The Æquation of the *Third* is $4\frac{1}{20}$ times greater than that of the *Firſt*, and when greateſt amounteth to $2^{h.}$ 38′. 29″. And the Æquation of the *Fourth* being $9\frac{7}{15}$ times that of the *Firſt*, is had by Subſtracting $\frac{1}{2}$ and $\frac{1}{30}$ from *ten times* the Æquation of the *Firſt*, whence the greateſt becomes $6^{h.}$ 10′. 28″. So that Num. I. and Num. II. as here collected for the *Firſt*, may indifferently ſerve all the reſt.

As to the Second Æquation of the other *Satellites*, Monſieur *Caſſini* has, by his *Præcepta Calculi* (as is before mentioned) ſuppoſed the Minutes thereof to be increaſed in the ſame proportion; as inſtead of 14′. 10″. in the *Firſt*, to be 28′. 27″. in the *Second*, 57′. 22″. in the *Third*, and no leſs than $2^{h.}$ 14′. 7″. in the *Fourth*; whereas if this ſecond Inequality did proceed from the ſucceſſive propagation of Light, this Æquation ought to be the ſame in all of them, which Monſieur *Caſſini* ſays was wanting to be ſhewn, to perfect Monſieur *Romer's* Demonſtration; wherefore he has rejected it as ill founded. But there is good cauſe to believe that his motive thereto, is what he has thought not proper to diſcover. And the following Obſervations do ſufficiently ſupply the Defect complained of in the making out of that Hypotheſis.

Anno 1676. *Octob.* 2. *Stil. Nov.* $6^{h.}$ 10′. 37″. *app.* but $5^{h.}$ 59′. 37″. *æq. time*, Monſieur *Caſſini* at *Paris* obſerved the *Emerſion* of the *Third Satellite* from *Jupiter's* ſhadow.

And again, *Novemb.* 14 following, $6^{h.}$ 20′. 55″. *app. Time*, but $6^{h.}$ 5′. 55″. *æq. T.* he obſerved the like *Emerſion* of the ſame *Satellite*. The obſerved Interval of Time between theſe *Emerſions* was $43^{d.}$ $0^{h.}$ 6′. 18″, which is 8′. 22″. more than 6 mean Revolutions of this *Satellite*, of which 4′. 27″. ariſes from the difference of the firſt Æquations and the greater continuance of the latter Eclipſe; ſo that the other 4 Minutes is all that is left to anſwer for the difference of the ſecond Æquations; and Num. II. in that time increaſing from 48 to 72, gives 4′. 36″. for the difference of the ſecond Æquations of the *Firſt Satellite*. So that here the ſecond Æquation of the *Third* is found rather leſs than that of the *Firſt*, but the difference is ſo ſmall, that it may rather be attributed to the uncertainty of Obſervation. Whereas according to Monſieur *Caſſini's* Method of Calculating, inſtead of four Minutes it ought to be 18′. 38″. and the Interval of theſe two *Emerſions* $43^{d.}$ $0^{h.}$ 21′. exceeding the Time obſerved by a whole quarter of an hour; which that Curious Obſerver could not be deceived in.

The like appears yet more evidently in the *Fourth Satellite*. By the Obſervation of Mr. *Flamſteed* at *Greenwich*, *Anno* 1682. *Sept.* $24^{o.}$ $17^{h.}$ 45′. T. app. but $17^{h.}$ 32′$\frac{1}{2}$ T. æq. the *Fourth Satellite* was ſeen newly come out of the ſhadow, ſo that about $17^{h.}$ 30′. T. æq. the firſt beginning of *Emerſion* was conjectured; and after five Revolutions, *viz.* *Decemb.* $17^{d.}$ $11^{h.}$ 16′. or $11^{h.}$ 18′. T. æq. he again obſerved the firſt appearance of the *Satellite* beginning to Emerge, that is, after an Interval of $83^{d.}$ $17^{h.}$ 48′; whereas this *Satellite* makes five mean Revolutions in $83^{d.}$ $18^{h.}$ 25′$\frac{1}{2}$. Here we have 37′$\frac{1}{2}$ to be accounted for by the ſeveral Inequalities. Of this 21′ is due to the firſt Æquations, which is reduced to 19′ by the greater continuance of the latter Eclipſe, *Jupiter* then approaching to his deſcending Node: So that there remains only 18′$\frac{1}{2}$ for the difference of the Second Æquations,

tions, whilſt the *Earth* approached *Jupiter* by more than the *Radius* of its own *Orb*; and the difference of the ſecond Æquations of the *Firſt Satellite* being according to *Caſſini* $8'.\,30''$, the ſaid difference in the *Fourth* ought to be $1^{h}\,20'\frac{1}{2}$ inſtead of $18\frac{1}{2}$; whence the Interval of theſe two *Emerſions* would be according to his Precepts, but $83^{d}\,16^{h}\,46'$, inſtead of $83^{d}\,17^{h}\,48'$. obſerved. And whereas $18'\frac{1}{2}$ may ſeem too great a difference; it muſt be noted, firſt, that Monſieur *Romer* had ſtated the whole ſecond Æquation $22'.\,00''$, (*vide Phil. Tranſ. Num.* 136.) which Monſieur *Caſſini* has diminiſhed to $14'.10''$; ſo that inſtead of $8'\frac{1}{2}$, Monſieur *Romer* allows above $13'$; and ſecondly, that in the firſt of theſe Obſervations, being about half an hour before Sun-riſe, the brightneſs of the Morning might well hinder the ſeeing of this ſmalleſt and floweſt *Satellite*, till ſuch time as a good part thereof was emerged.

But I have exceeded the Bounds of my intended Diſcourſe, and ſhall only Advertiſe, That theſe Tables are not Printed with the uſual Care of the *Imprimerie Royale à Paris*, That the *Tabula Revolutionum primi Satellitis Jovis in Annis* 100, *pag.* 13 *& ſeq.* is faulty in theſe Years, 16, 39, 55, 98 & 99; as is alſo the *Epocha* for the Year 1700, *pag.* 99. where *pro Num.* I. 1853 *lege* 1873, and *pro Num.* II. 1004, *lege* 1104: And that the Number of Revolutions of the Second *Satellite* in 100 Years, *pag.* 60, 61; of the Third, *pag.* 76, 77; and of the Fourth, *pag.* 90, 91, are by a groſs miſtake of the Calculator, all falſe and erroneous, and muſt be amended by whoſoever would uſe them. Which yet ought not in the leaſt to be attributed to the Excellent Author, but rather to the Negligence of thoſe employed by him. The Reader hereof is deſired to amend theſe following *Errata*, which were diſcovered when it was too late.

ERRATA. *Pag.* 238. *lin.* 24. *pro* $5^{\circ}\,30'$. *leg.* $5^{\circ}\,31'.\,40''$. *lin.* 25. *pro* $8^{\circ}\,26'\frac{1}{4}$. *leg.* $8^{\circ}\,28'\frac{1}{2}$.

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