SCIENCE FOR EVERYONE

S.R. FILONOVICH

THE GREATEST SPEED

$3 \cdot 10^8$ m/s

MIR

С. Р. Филонович

Самая большая скорость

Под редакцией
В. А. Фабриканта

Издательство «Наука» Москва

S. R. Filonovich

The Greatest Speed

Russian edition edited by
Prof. V. A. Fabricant, D.Sc.

Translated from the Russian
by Michael Burov

Mir
Publishers
Moscow

First published 1986
Revised from the 1983 Russian edition

На английском языке

Preface

This book covers the history of the determination of the velocity of light during the past three centuries. The problem originated in a seemingly abstract question: is the velocity of light finite or infinite? The answer was found in the 17th century, and the problems of measuring the light velocity has been luring scientists ever since. Step by step, the velocity of light was promoted from a commonplace number in optics to a fundamental constant playing a most important role in physics. Numerous experiments in the history of measuring this constant illustriously support Albert Einstein's idea: science is not and will never be a completed book because each essential success brings new questions, and with time every development reveals newer and deeper problems.

The reason why the determination of the velocity of light is still intriguing scientists lies in that the velocity of light in vacuum is a constant belonging to many branches of physics. The velocity of light, as it were, 'unites' these areas, thus substantiating convincingly the general philosophical concept on the unity of the world around us.

However, the interest in the history of this particular problem can be accounted for by not only the role of this constant in modern science.

The experiments conducted to measure the velocity of light contributed to the development of physics on the whole. Starting from the 17th century, these experiments show the progress of methods and techniques in physical experiments. Quite recently, when the Russian edition of this book has already been out of print (it ran into 150,000 copies), the velocity of light drew the attention of every experimenting physicist again: in late 1983, a basically new decision was adopted to define the metre, one of the most important physical units, and the velocity of light played the main role in the definition.

The English edition of this book involved some updating and the introduction of new material, most of which reflects the latest achievements.

The author acknowledges his sincere gratitude to Dr. A. A. Gurshtein, Dr. M. M. Dagaev, Dr. E. M. Klyaus, Prof. N. N. Malov, and Dr. E. I. Pogrebyskaya, who had read the Russian manuscript of this book: their suggestions helped essentially improve the text. V. L. Ginzburg, Member of the USSR Academy of Sciences offered his advice and further suggestions while the book was being prepared for the English edition. The author is especially grateful to V. A. Fabrikant, whose valuable advice and comments were encouraging in the preparation of the book.

S. Filonovich

Contents

Introduction

It is typical for the science of physics that its laws are quantitative: the laws of Ohm, Newton or Coulomb and any other laws are invariably expressed as mathematical relationships between physical quantities. Many relationships expressing the laws of physics contain certain physical constants: for instance, the gravitational constant in Newton's universal law of gravitation, specific heat in the thermal balance equation, and the velocity of light in the Einstein's law relating the mass of a body and its total energy.

Many physical constants are called so conventionally, e.g., friction coefficient, specific heat, relative density, resistivity, etc. However, there are truly universal constants: for instance, the gravitational constant does not depend on whether the interacting masses are those of lead or steel. Be it copper or gold, the electrons they contain possess the same charge (by the way, the fact was verified in experiments quite recently, in the early 20th century). The velocity of light in vacuum, designated c, is just as invariably universal. Due to their universality, such constants are called *fundamental*. The values of the fundamental constants determine the most essential features of the entire physical world, from the elementary particles to the largest astronomical objects.

The velocity of light belongs to a small group of the fundamental constants; however, it occupies an outstanding position even within this group. First of all, it is encountered in very different branches of physics. The constant *c* is included in the Lorentz transformation in the special theory of relativity, it is used when one deals with the equations of the classical electrodynamics, and the Einstein's formula $E = mc^2$ makes it possible to calculate the amount of energy released in nuclear reactions. In addition, the velocity of light was the first ever measured fundamental constant.

In modern physics, the ubiquitous constant *c* shows that the progress of the science of nature is on the right path. It is a clear manifestation of the unity of the physical world although the understanding of this unity did not come all of a sudden. More than three hundred years have passed since the value of *c* was determined for the first time. The constant revealed its secrets for scientists gradually, step by step. Sometimes the measurement of this value required many years for purposeful research and improvement in the scientific techniques and instruments. The value of 3×10^8 m/s could surprisingly appear in experiments, thus giving rise to problems concerning the profound depth of the science of physics. Measuring the value of *c* could either upset or support a physical theory, but it always promoted the advance of technology. It will come as no exaggeration if one says that the story of the determination of the velocity of light is a 'concise history of physics'. And this story has not come to an end yet. Let us follow how and why the velocity of light was measured, and try and see the significance of this quantity for physics.

Chapter 1

The Origin of the Problem

The history of the problem began in Italy in the 1630s. One of the books of the period described a talk between three men of science: Salviati, Sagredo, and Simplicio. One of the problems they discussed was the melting of metals by focussed solar light. Then they turned from this particular problem to a more general one, the motion of light:

Salviati:

"We see other fires and dissolutions to be made with motion, and very swift motion; behold the operation of lightning, and of gunpowder in mines and bombs. We see how much the use of bellows speeds the flames of coals mixed with gross and impure vapors, increasing their power to liquefy metals. So I cannot believe that the action of light, however pure, can be without motion, and indeed the swiftest.

Sagredo:

"But what and how great should we take the speed of light to be? Is it instantaneous perhaps, and momentary? Or does it require time, like other movements? Could we assure ourselves by experiment which it may be?

Simplicio:

"Daily experience shows the expansion of light to be instantaneous. When we see artillery fired far away, the brightness of the flames reaches our eyes without lapse of time, but the sound comes to our ears only after a noticeable interval of time.

Sagredo:

"What? From this well-known experience, Simplicio, no more can be deduced than that the sound is conducted to our hearing in a time less brief than that in which the light is conducted to us. It does not assure me whether the light is instantaneous or time-consuming but very rapid. Your observation is no more conclusive than it would be to say: "Immediately on the sun's reaching the horizon, its splendor reaches our eyes." For who will assure me that the rays did not reach the horizon before [reaching] our vision?

Salviati:

"The inconclusiveness of these and the like observations caused me once to think of some way in which we could determine without error whether illumination (that is, the expansion of light) is really instantaneous. The rapid motion of sound assures us that that of light must be very swift indeed, and the experiment that occurred to me was this. I would have two men each take one light inside a dark lantern or other covering, which each could conceal and reveal by interposing his hand, directing this toward the vision of the other. Facing each other at a distance of a few braccia, they could practice revealing and concealling the light from each other's view, so that when either man saw a light from the other, he would at once uncover his own. After some mutual exchanges, this signaling would become so adjusted that without any sensible variation either would immediately reply to the other's signal, so that when one man uncovered his light, he would instantly see the other man's light.

"This practice having been perfected at a short distance, the same two companions could place themselves with similar lights at a distance of two or three miles and resume the experiment at night, observing carefully whether the replies to their showings and hidings followed in the same manner as near at hand. If so, they could surely conclude that the expansion of light is instantaneous, for if light required any time at a distance of three miles, which amounts to six miles for the going of one light and the coming of the other, the interval ought to be quite noticeable. And if it were desired to make such observations at yet greater distances, of eight or ten miles, we could make use of the telescope, focusing one for each observer at the places where the lights were to be put into use at night. Lights easy to cover and uncover are not very large, and hence are hardly visible to the naked eye at such distance, but by the aid of telescopes previously fixed and focused they could be comfortably seen.

Sagredo:

The experiment seems to me both sure and ingenious. But tell us what you concluded from its trial.

Salviati:

Actually, I have not tried it except at a small distance, less than one mile, from which [trial] I was unable to make sure whether the facing light appeared instantaneously. But if not instantaneous, light is very swift and, I may say, momentary; at present I should liken it to that motion made by the brightness of lightning seen between clouds eight or ten miles apart. In this, we distinguish the beginning and fountainhead of light at a particular place among the clouds, followed immediately by its very wide expansion through surrounding clouds. This seems to me an argument that the stroke of lightning takes some little time, because if the illumination were made all together and not by parts, it appears that we should not be able to distinguish its place of origin and its center from its extreme streamers and dilatations."

This talk is a quotation from *Discourses and Mathematical Demonstrations Concerning Two New Sciences* (1638) by the great Italian scientist Galileo Galilei (1564-1642). Salviati expresses Galileo's views, while Sagredo is to pose problems for Salviati and Simplicio to answer. Sagredo appraises the opinions of his companions from the viewpoint of the common sense. Simplicio is a peripatetic, i.e., a supporter of the then dominating teaching of the ancient Greek scientist Aristotle (384-322 B. C.). The course of the conversation, so skilfully produced by Galileo, assures his reader that the peripatetic views are erroneous and reveals the advantages of the new method of studying nature based on experiment.

Galileo Galilei (1564-1642)

In the opinion of both Salviati and Sagredo (and therefore of Galileo himself), precisely an experiment and only an experiment can answer the problem of whether the velocity of light is finite or infinite.

The problem concerning the velocity of light belongs to optics. In Galileo's time, optics was considered to be one of the most essential branches of 'natural philosophy', as physics was called then. Geometrical optics had been considerably developed by the early 17th century. The law of light reflection was known since the times of Euclides (4th century B. C.). This law made it possible to solve a number of problems concerning light reflecton from mirrors of various shapes. The ancient scientists were also familiar with the phenomenon of light refraction. During the Middle Ages, people learned to correct defects of vision with the aid of glasses. At the very beginning of the 17th century, telescopes were produced for the needs of navigation, warfare, and astronomical observations. About 1620, the light refraction law was discovered and became the basis for the design of telescopes. Consequently, the branches of optics immediately related to practical needs were advancing fairly well.

At the same time, the nature of light was a very problematic issue. Many scientists even regarded the nature of light as a subject-matter of philosophy rather than physics and therefore consciously avoided it. This is what the German astronomer, physicist, and mathematician Johannes Kepler (1571-1630) did when he prepared his work in optics for publication. This viewpoint seems to have been well substantiated: the experimental basis in optics was so primitive, and the volume of knowledge in physical optics was so infinitesimal that one could hardly expect any satisfactory hypotheses on the nature of light to appear. The phenomena of interference, *diffraction*, and *polarization* were *only discovered* in the late 17th century. The methods of optical measurements were still in their infancy.

The problem of the velocity of light allowed two approaches. On the one hand, the value of this velocity is determined by the nature of light, and without learning this nature it is difficult to arrive at the final conclusion on whether this velocity is finite or infinite. On the other hand, one may try and solve this problem in experiment even without knowing the true nature of light, as it follows from Galileo's suggestion.

Note that Galileo's experiment was a qualitative one. The point of the excerpt from his *Discourses* is whether the 'expansion of light' is instantaneous or 'time-consuming'. The experiment could only be qualitative because the techniques of physical measurements was very poor. While conducting his first experiments in mechanics, Galileo used his own pulse to measure time. Although later Galileo used a water clock, i.e., a more reliable method, he seems to have guessed that the available instruments did not make it possible to measure the velocity of light.

However, a qualitative experiment does not always help solve a physical problem. The conclusion becomes only convincing when certain quantitative relationships are found between the quantities characterizing the physical phenomenon under study. The determining factor in Galileo's experiment is the relationship between the delay due to man's response and the delay due to the hypothesized propagation of light with a finite velocity. Neither Galileo nor his followers from the Florence Academy of Experiments succeeded although the latter tried greater distances than Galileo did. Galileo could not even make any estimate *a priori*, as it is commonly practiced nowadays, because physics had not yet any concept of the scale required for a plausible approximation of the velocity of light. The velocity of sound in the air, i.e., about 330 m/s, was the greatest velocity scientists dealt with by the time Galileo's book was published. One can hardly blame Galileo's contemporaries since they could not have supposed that velocity of light is almost a million times greater than that of sound!

However, the inquisitive human mind has never been blocked by obstacles. Scientists formulate their hypotheses even when they cannot be verified at the moment. Sometimes it happens that truth is born in a debate over an erroneous hypothesis, and this truth may have remained unknown for years without the debate.

This was precisely the paradoxical role played by the theory of light suggested in 1637 by the outstanding French philosopher René Descartes (1596-1650). The advance of science proved only two fragments in his theory of optics to be correct: the formulation of the refraction law and the explanation of the rainbow. However, it was found out that the refraction law had been formulated by the Dutch scientist Willebrord Snellius (1580-1626) while Descartes's deduction of the law was wrong. Nevertheless the role of Descartes in the history of optics and particularly in

René Descartes (1596-1650)

the history of the determination of the velocity of light is very essential.

According to Descartes, light is an influence instantaneously propagating in the rarefied matter filling the interstices of the bodies. To explain the phenomena of *light reflection* and *refraction*, Descartes suggested that propagation of light is similar to the motion of a ball struck with a racket. (The similarity between the motion of light and that of body had been first considered by the famous Arabian scientist Alhazen (965-1039), who was also the first to suggest the resolution of a speed vector into components.) Descartes expounded his views of the nature of light in his treatise *Discourse on the Method of Rightly Conducting One's Reason and Seeking Truth in the Sciences* (*Le Discours de la méthode*, 1637). Here is how, for instance, Descartes described light refraction:

> "... these rays of light, when they pass only through one transparent homogeneous body, should be represented as straight lines; however, if the rays meet other bodies, they are deflected or stopped just like the motion of a ball or a stone thrown in the air is changed because of the obstacles they meet... In order to explain this third comparison completely, one should bear in mind that the bodies a ball can meet in its flight through the air may be soft, solid or liquid; if the bodies are soft, they stop the motion of the ball entirely, for instance, when it strikes a cloth, sand or dirt; if the bodies are solid, they immediately deflect it to another direction depending on the surface... Finally, note that if the ball in its motion meets obliquely the surface of a liquid body, through which it can pass more or less easily as compared with the medium from which the ball enters, it is deflected and changes its direction in the course of penetration: for instance, should a ball in the air in point A (Fig. 1) be pushed to B, it moves rectilinearly from A to B if only

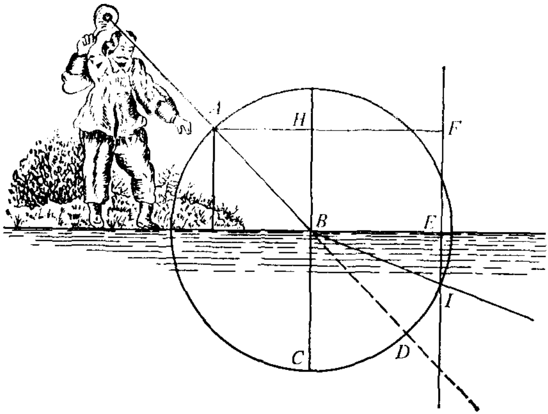


Fig. 1. A picture from Descartes's *Le Discours de la méthode* demonstrating the similarity between the motion of light and the propagation of ight.

its weight or any other special reason does not interfere; however, the ball in point B, where it meets water surface BE, is deflected, directed to I, and also moves from B to I rectilinearly, which can be easily verified in experiment."

Descartes's assumptions on the motion of a ball similar to the motion of light and giving rise to his deduction of the refraction law boil down to the following. The motion of a ball is described by a magnitude and a direction, i.e., a vector, and when the ball passes from one medium into another, the component of the motion (i.e., the velocity) parallel to the interface does not change. Besides, Descartes assumed that while the ball-light passes the interface and enters a denser medium, the velocity component that is normal to the surface increases. Thus in order to obtain the correct refraction law, Descartes had to contend that the velocity of light in a denser medium is greater than that in a less dense one. The reader may ask a question: if the velocity of light is regarded to be infinite, what is the sense of this last assumption? Scientists revealed this contradiction before Descartes died. There is one more point in Descartes's paradox: how should one resolve an infinite vector into components?

Consequently, Descartes's approach to refraction was at odds with the basic assumptions of his own theory of light. Notwithstanding, the refraction law was in fine keeping with experimental data. Scientists could not tolerate the situation, and they endeavoured to deduce the refraction law avoiding the contradictions. The velocity of light was the central point in the solution of the problem.

Descartes tried to solve the problem using concrete ideas on the nature of light, so there were two paths for his opponents. On the one hand, one could try and deduce the law of refraction without a hypothesis on the nature of light. On the other hand, there was a chance that another hypothesis would permit them to deduce the law without contradictions. Further developments showed that both possibilities were used.

The first one brought about the famous Fermat's principle, or, as it is not quite accurately called, the 'least-time principle'. Here is its history.

When there were no scientific journals yet, scientists communicated their results either personally or through correspondence. During the

Pierre Fermat (1601-1665)

first half of the 17th century, an important role in the exchange of scientific information was played by the French monk and scientist Marin Mersenne (1588-1648). He corresponded with almost every prominent scientist of the time: René Descartes, Christian Huygens (1629-1695), Blaise Pascal (1623-1662), Evangelista Torricelli

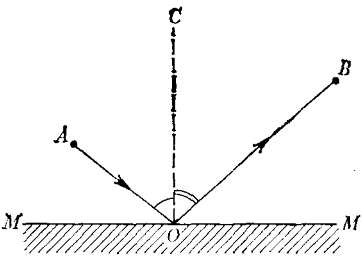


Fig. 2. Explaining the Heron's principle.

(1608-1647), Pierre Fermat (1601-1665), and many others. Mersenne's letters to 78 correspondents are extant. Of special importance were the letters he exchanged with the scientists who published their results rarely and by no means immediately.

Thus, prior to the publication of Descartes's *Discourse*, Mersenne sent the first chapters of the work to the outstanding French mathematician Pierre Fermat and asked for his judgement. Before long, Fermat wrote an answer indicating the contradictions; for instance, he doubted whether it was admissible to extrapolate the properties of bodies, moving with a finite and variable velocity, to light, whose velocity was considered infinite by both Fermat and Descartes. Descartes knew Fermat's critical remarks and answered them, but each of them still remained of the same opinion.

Despite the apparent futility of this discussion, it had an essential consequence because it stimulated Fermat to look for a more acceptable deduction of the refraction law. Of no small significance was the fact that Fermat was familiar with a book on optics written by his friend de la Chambre. The author substantiated the law of reflection with 'Heron's principle', or the 'least-travel principle', the gist of which is that light travelling from point A (Fig. 2) to point B is reflected from the flat mirror MM at point O so that its path $AO + OB$ is minimal. It is not difficult to show that, if this is the case, angle of the incidence AOC is equal to the angle of reflection COB.

However, de la Chambre was confused by the fact that there was a number of cases when Heron's principle in the above-mentioned formulation proved to be invalid, i.e., the light followed the longest path rather than the shortest. He asked Fermat to think over possible reasons for the exceptions.

It appears that the contemplation brought Fermat to the idea of reformulating Heron's principle so that it were applicable to refraction. Evidently, the principle cannot be applied to refraction readily because it implies that light cannot be refracted at all. After a long time, Fermat made a conclusion that light does not just travel over shortest paths, but rather over the most 'easy' ones in the sense that the resistance to light propagation should be minimal. But then the velocity of light should be finite because it is absurd to discuss the resistance of the medium if the velocity is infinite. So Fermat discarded his erstwhile idea and accepted the hypothesis that the velocity of light is finite.

According to Fermat, the postulate on the minimal resistance is tantamount to an assumption

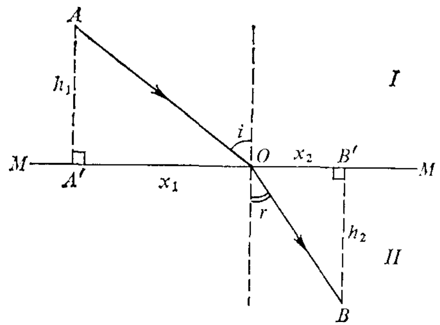


Fig. 3. Explaining the refraction law deduced from the Fermat's principle.

that light travels over paths requiring the least time. Besides, Fermat contended that light propagates in a homogeneous medium with a constant velocity. These assumptions allowed Fermat to deduce the refraction law.

Proceeding from Fermat's premises, let us also try and obtain the expression relating the angles of incidence and refraction. However, I shall not follow Fermat's considerations: they appear to be too cumbersome because Fermat used the terms of geometry rather than those of mathematical analysis.

Suppose MM is the interface between two media, I and II (Fig. 3), and the velocity of light in the medium I is v_1 and that in the medium II is v_2. It is required to find the path of light such that the travel from point A in the medium I to point B in the medium II would take the least time. If the velocity of light in a homogeneous medium is constant, then, in compliance with the least-time principle, light travels in the medium along a straight line; therefore, we have to consider for our purposes the path of light consisting of two line segments of straight lines AO and OB. In addition, suppose h_1 and h_2 are the distances from points A and B to the interface MM, respectively. Then the total time for the light to travel from A to B equals

$$t = \frac{l_1}{v_1} + \frac{l_2}{v_2},$$

where $l_1 = AO$ is the path of light in the medium I, while $l_2 = OB$ is that in the medium II. Let $A'O = x_1$ and $OB' = x_2$; naturally,

$$x_1 + x_2 - d = \text{const.}$$

Then

$$t = \frac{\sqrt{h_1^2 + x_1^2}}{v_1} + \frac{\sqrt{h_2^2 + (d - x_1)^2}}{v_2}.$$

Therefore, the light propagation time has been presented as a function of one variable x_1. The condition for a function to reach its minimum (or maximum) is that its first derivative is equal to zero, so the Fermat principle can be put down as

$$\frac{dt}{dx_1} = 0,$$

whence

$$\frac{x_1}{v_1\sqrt{h_1^2+x_1^2}}-\frac{d-x_1}{v_2\sqrt{h_2^2+(d-x_1)^2}}=0.$$

Evidently, this can be written in the form

$$\frac{\sin i}{v_1}=\frac{\sin r}{v_2},$$

or

$$\frac{\sin i}{\sin r}=\frac{v_1}{v_2}.$$

We have found the required law of refraction. It is clear from this relationship that if $v_1 > v_2$, then $\sin i > \sin r$, and hence $i > r$: this is the inequality that is true for the transition of light from air to, for instance, glass or water. Fermat believed the conclusion that the velocity of light decreases when it enters a denser medium to be more natural than Descartes's assumption that the velocity of light increases.

Fermat offered his proof of the refraction law, proceeding from the least-time principle, in 1662. Far from every scientist regarded the principle positively. For instance, Descartes's followers considered it to be 'nonphysical' because it required light to behave 'consciously'. There also were scientists who were not satisfied because Fermat did not explain the exceptions concerning the reflection from concave mirrors. However, the prejudice against Fermat's principle was gradually dispelled, and some time later it came into wide use as a convenient mathematical technique.

Although Fermat apparently paid more attention to the mathematical aspect of the problem rather than its physical interpretation and did not use any concrete hypothesis on the nature of light, the least-time principle formulation confronted the physicists with a question: who was right, Descartes or Fermat? Moreover, there were two aspects in this question. Firstly, it was necessary to establish whether the velocity of light is finite; secondly, it was required to determine the change in the velocity of light in its transition from one medium to another. The first question was answered in the 17th century while the second one waited almost two hundred years for a crucial experiment.

Why did such a profound thinker as Descartes adhere so firmly to his assumption that the velocity of light is infinite when it entailed obvious logical difficulties? One should be careful with the great thinkers' errors: they are by no means accidental. This is true with respect to Descartes because his conviction had a clear physical basis, and in this sense he was head and shoulders above his opponents, who nevertheless finally proved to be right. This paradox is a historical fact.

About 1634 Descartes discussed the velocity of light in the correspondence with his friend, the Dutch scientist Isaak Beckman (1570-1637). Independently from Galileo, Beckman suggested his own experiment for the determination of the velocity of light. A man should hold a torch in front of a mirror at a fairly great distance. Periodically waving the torch, he observes the motion of the torch image in the mirror. The gist of the experiment is that if the observer could notice a delay in the motion of the image with respect to the motion of the torch, it would be a proof of the finite velocity of light. Apparently, this experiment is similar to that suggested by Galileo, but although it has the advantage of the participation of only one observer, which decreases possible observation errors, it could not bring the correct solution of the problem either.

Descartes wrote to *Beckman*:

> "You trusted this experiment to such an extent that claimed that you would consider your entire teaching to be wrong if the observer were unable to notice at least a small lapse of time between the moment at which a motion were seen in the mirror and the *moment* at which it were felt by the hand. I, contrariwise, said that if only this lapse of time could be perceived, then my entire teaching would be upset completely."

Evidently, Descartes was sure of the result *a priori*. Why so? He did not even make any test observations. Descartes's confidence was based on a numerical estimation, which permitted him to determine a lower bound for the velocity of light. He presented the idea of this estimation in his letter to Beckman:

> "On the next day, in order to put an end to the whole debate and free you from useless work, I reminded you that there is another experiment, verified very thoroughly and more than once by many people, which makes it absolutely clear that there is no lapse of time between the moment light leaves a luminescent body and the moment it reaches the eye....
>
> "... In order to present the experiment, first of all I asked whether you agree that the Moon is lit by the Sun, and that the eclipses

occur because the Earth can be between the Sun and the Moon or the Moon can be between the Sun and the Earth? You answered in the affirmative. Then I asked in what manner, in your opinion, the light of stars travels to us, and you answered: along straight lines, so when we look at the Sun, we do not see it where it actually is but where it was at the moment the light, by virtue of which we see the Sun, left it. Finally, I asked what should the minimal sensitively felt lapse of time be between the moment of moving the torch and the moment it is reflected in the mirror at the distance of 250 steps."

Beckman's conjecture was that light, to pass this distance, requires a lapse of time Δt equal to the lapse of time between man's two pulse beats, i.e., about one second. Apparently, Descartes considered this estimate to be too great and suggested to assume Δt to be 1/24 of the value suggested by Beckman.

"I said that this lapse of time, which clearly cannot be felt in your experiment (naturally, you agree with this), would be felt distinctly in mine. Indeed, taking into account that the Moon is at a distance of 50 Earth's radii from the Earth..., light would take at least an hour to travel twice the distance between the Moon and the Earth, as calculations show it."

Descartes suggested to try and use the data on the Moon's eclipses to determine the velocity of light: if it is finite, the astronomers would register some delay of the observed eclipses with respect to the calculated moments when the

Earth pass between the Sun and the Moon. No such delay had ever been measured, so Descartes concluded that the velocity of light was infinite.

It is perfectly clear at present that Descartes's reasoning was not conclusive because the velocity of light he took to estimate the delay was too small. However, it appears that in the mid-17th century an assumption that a velocity a million times that of sound could exist must have seemed absurd. That is why Descartes's conclusion was only discarded when the first estimation of the velocity of light was obtained in experiment. The outstanding Dutch physicist Christian Huygens wrote in his *Treatise on Light* (1690) that if only Descartes had assumed a greater value of the light velocity, then the effect would have been too small to be noticed. Huygens should have said it 15 years earlier!

The ideas and polemics discussed in this chapter naturally belong to the 'pre-history' of the story about the velocity of light. The true history of its determination began with the first experiments. The need in the value of c increased, and the problem of the velocity of light turned from being purely speculative into the one related to both the nature of light and the analysis of astronomical observations. The history of physics once again demonstrated that, as a rule, scientific problems are solved when they are especially urgent. Paradoxically, Descartes proved to be right, although not in his conclusion on the infinity of the velocity of light but in his approach to the problem: the answer should be sought in astronomy.

Chapter 2

Astronomy Provides an Answer

The history of physics is full of riddles for researchers. There are such riddles in the history of the velocity of light as well. From the viewpoint of modern physics, it only seems evident that in the 17th century the problem of the finite value of the velocity of light could be solved with the aid of astronomy because by that time astronomy had developed the most accurate methods of measurement. In addition, one could only hope to obtain at least a rough estimate of the value of c by measuring the durations required for light to travel over distances of the astronomical scale. However, this viewpoint was far from being generally accepted in the 17th century. The idea was only evident for Descartes, as it follows from his letters to Beckman.

The great French philosopher showed his amazing intuition in this case. But why was it Descartes who said that astronomical observations were needed to solve the puzzle of the velocity of light? Aren't Descartes's main theories in physics based on his imagination rather than an analysis of scientific facts? Why wasn't this idea brought forth by Galileo or Huygens, whose contribution to physics was much greater than that of Descartes? This is another puzzle, from the psychology of scientific creativity, and it will hardly ever be solved. The first determination of the velocity of light was not the result of observations conducted specially for this purpose but was a 'spill-over' obtained while solving a particular applied problem.

The late 17th century, the period I am going to discuss, was the time when science became to be organized. The first scientific journals appeared, and scientific societies and academies were established. The scientists, members of these academies, were first of all required to solve urgent practical problems. One of such problems, seemingly far away from the velocity of light, was the determination of geographical longitude at a given point.

The 16th century, the century of great geographical discoveries, both enriched our knowledge of the Earth and challenged the scientists with new applied problems. The ever expanding navigation practice demanded a handy and reliable technique for finding the latitude and longitude at any point on the Earth. As to the latitude, seafarers were able to determine it by the solar altitude at noon since the 3rd century B. C. However, the problem of finding longitude failed to be solved for any practical purposes although the principle had long since been known, i.e., that the geographical longitude is proportional to the difference between the local time at a given point and the time at an accepted prime meridian. The idea seems to be very simple and is used to determine latitude until now. To apply it, navigators should have an accurate watch to keep the time, the watch being set at the port of departure (whose longitude is naturally known). The local time at any point can be determined through astronomical observations, and the difference between the local time obtained through observations and the reading of the watch allows the observer to find his longitude. However, there were no precise enough chronometers in the 17th century, so the scientists had to look for another, roundabout way to solve the problem.

Here is an idea of such 'roundabout' solution: if there is an astronomical phenomenon that is frequent enough and whose times at the meridian of origin are known and put in tables, then the longitude can be found by comparing the time of the phenomenon and the local time registered simultaneously.

The problem of determining the longitude was so important that enormous money was offered as the reward: in 1604 the Spanish King Philip III was willing to give 100,000 écus, in 1606 the States-General of the Netherlands announced a prize of 100,000 florins, somewhat later Louis XIV, King of France, allocated 100,000 French livres for the purpose, and the English parliament intended to pay 20,000 English pounds. So it's a small wonder that the problem was attractive for many scientists and amateurs. To a considerable extent, the problem stimulated Christian Huygens, who was elaborating and improving his pendulum watch for a long time.

However, many scientists of the period regarded a purely astronomical method to be more promising. One of the astronomical phenomena suitable, in their opinion, for the determination of longitude was the eclipses of Jupiter's satellites. The astronomers of the 17th century paid no less attention to these eclipses than the astronomers of today to the research on quasars. Jupiter's satellites were discovered in 1610 by

Galileo Galilei with the aid of the telescope he invented. Galileo discovered the four 'Galilean' satellites Io, Europa, Ganymede, and Callisto (in the order of their distance from Jupiter). The total number of Jupiter's satellites known to date is sixteen. From the viewpoint of the longitude determination problem, the most suitable was Io, the first satellite. Its period of revolution around Jupiter is approximately 42.5 hours; just as the other satellites, Io periodically enters Jupiter's shadow and is then invisible: this is its eclipse.

To use these eclipses for the longitude determination, it was needed to compile a table of them related to the local time at a certain point. This was the research undertaken by two astronomers, Jean Picard (1620-1682) and Jiovanni Domenico Cassini (1625-1712), working in the Paris observatory in the late 17th century. Cassini, an Italian, had worked for some time in Bologna, Italy. This was where he succeeded in compiling and publishing the first satisfactory tables for the Jupiter's satellites. Before long, in 1668 Louis XIV invited Cassini to head the Paris observatory, which had just been built. By the time Cassini arrived in Paris, Picard had already been working there. Picard was the first to observe the Jupiter's satellites in the Paris observatory and Cassini continued the observations.

The scientists at the Paris observatory were engaged in a wide variety of problems, analysed the data obtained by other astronomers, and published a lot of materials. Within the framework of a vast programme of research, there was a trip to Denmark planned to obtain more accurate geographical coordinates of Tycho Brahe's (1546-1601) famous observatory, where this outstanding Danish astronomer had performed most of his observations. The need for more accurate coordinates was dictated by that it was impossible to use his data without knowing the coordinates precisely. Besides, Parisian astronomers intended to perform simultaneous observations of the eclipses of the first Jupiter's satellite in Paris and on the island of Hveen, where Brahe's observatory was located. Picard was to carry out the mission, and he left Paris for Copenhagen in July 1671.

Arriving in Copenhagen, Picard requested Erasmus Bartholin (1625-1698), the professor of mathematics at Copenhagen University, to help carry out the project developed by French astronomers. Professor Bartholin was famous then throughout Europe; he is known in the history of science mostly because in 1669 he discovered double refraction (birefringence) of iceland spar. Picard and Bartholin went to Brahe's observatory in the company of the young Olaf (Ole) Roemer, Bartholin's student. This budding scientist was destined to play the principal role in the history of the first determination of the velocity of light.

Ole Roemer was born on September 25, 1644, into a family of a small-time merchant in Aarhus, eastern Jutland. The boy received his elementary education in the local cathedral school, and since 1662 he became a student at Copenhagen University. He studied medicine first, and then became Bartholin's pupil. Ole was on friendly terms with Bartholin: he lived at his home and some time later became his son-in-law.

Olaf (Ole) Roemer (1644-1710)

By the time Picard arrived in Denmark, Brahe's observatory was almost completely ruined. Nevertheless, aided by his skilful assistants (Bartholin and Roemer), Picard managed to conduct the required observations. It appears that the French astronomer was greatly impressed by the

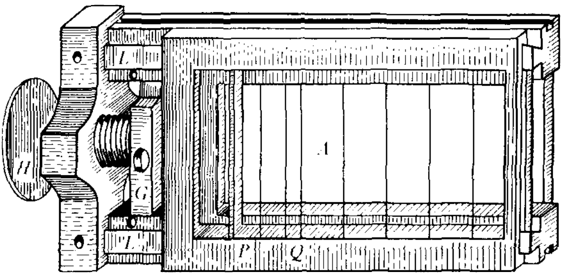


Fig. 4. Roemer's micrometer.

energy and talents of the young Roemer because Picard decided to invite him to work at the Paris observatory.

Roemer accepted Picard's invitation and arrived in Paris. Besides his work at the observatory, he was charged with a responsible duty to teach the dauphin mathematics, the eldest son of the King of France and heir to the throne. Yet Roemer still found time for diverse engineering problems; for one, he took part in the construction of fountains in Versailles and Marly. He invented and became famous for his planispheres, models, by means of which one can follow the motion of one celestial body around another; Jupiter's planisphere (Jovilabium) played an essential role in the determination of the irregularities in the observable motions of Jupiter's satellites.

Roemer improved the design of a micrometer (Fig. 4) needed to measure angular distances between closely spaced celestial objects. Roemer's micrometer is a system of two frames with parallel wires (*A*) stretched within them. One frame (*L*) remains fixed while the other (*PQ*) is moved by means of the screw *H*. The displacement of the wires of the sliding frame is determined owing to the use of two systems of wires. This micrometer was far better than other currently applied instruments for measuring small displacements, so this micrometer came into general use very soon.

Consequently, Roemer set about his assiduous scientific work in Paris. Collaborating with Cassini, he was bound to tackle the problems the head of the observatory was interested in. One of such problems was compilation of tables for Jupiter's satellites, as was mentioned above.

Cassini's nephew Jacques Philippe Maraldi was also engaged in the problem of motion of Jupiter's satellites. It was he who introduced the term 'inequality' meaning any deviation of the observed motion of planets from periodicity. Maraldi distinguished the 'first inequality', springing from the fact that the orbits are actually ellipses rather than circles and the 'second inequality' caused by that the observation is performed from the Earth rather than the Sun.

In August 1675, Cassini suggested that

> "the second irregularity in the motion of the first Jupiter's satellite may be due to that light takes some time to reach us from the satellite, and it takes from ten to eleven minutes to pass the distance equal to half the diameter of the Earth's orbit."

So what, was the puzzle of the velocity of light solved yet? Then what had Roemer to do with it? These questions are quite just, and they occurred to the historians of science more than once.

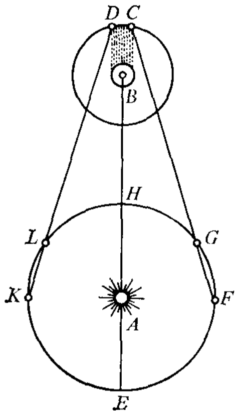


Fig. 5. A diagram from Roemer's paper concerning the velocity of light.

The point is that Cassini's hypothesis went unnoticed by the scientists, and Cassini was not firm with respect to his own idea, which was typical for his entire scientific career. The irony of fate was that the head of a major observatory of the world adhered to erroneous views on every essential problem in astronomy. Cassini did not insist on his hypothesis although it was correct! Moreover, when Roemer supported it by observations and calculations, Cassini denounced it and became one of the most steadfast Roemer's opponents. This course of events makes it possible to surmise that Cassini's remark was more or less accidental, and the hypothesis was one of many that came to his mind.

Roemer's standpoint was different. Having analysed the results of perennial observations, in September 1676 the Danish astronomer announced to the members of the Paris Academy of Sciences his prediction that the eclipse of the first Jupiter's satellite on November 9 of the same year due, according to calculations, to occur ostensibly at 5 h 25 min 45 s would be observed ten minutes later. Roemer explained this delay by the finite velocity of light: in his opinion, light takes about 22 min to transit the diameter of the Earth's orbit. The observations of the November eclipse stunningly supported the prediction. This made it possible for Roemer to make an oral presentation of his results on November 21 and publish a paper containing them in December in *Journal des Savants*, the ever first scientific periodical issued in Paris since 1655. In summer 1677, a translation of Roemer's paper appeared in the *Philosophical Transactions* of the Royal Society of London.

An excerpt from Roemer's first publication on his discovery well illustrates the course of his cogitation (Fig. 5):

> "Let *A* be the Sun, *B* Jupiter, *C* the first satellite of Jupiter which enters into the shadow of Jupiter, to come out of it at *D*; and let *EFGHLK* be the Earth placed at diverse distances from Jupiter.
>
> "Now suppose the Earth being at *L*, ... has seen the first satellite at the time of its emersion or issuing out of the shadow at *D*; and that about $42\frac{1}{2}$ hours after, viz. after one revolution of this satellite, the earth being in *K*, see it returned to *D*; it is manifest, that if the light require time to traverse the interval *LK*, the satellite will be seen returned later to *D*, than it would have been if the Earth had remained at *L*..."

Now Roemer made a guess that light takes one second to traverse a distance equal to the Earth's diameter. This gave a 3.5 min estimate of the time of the eclipse delay due to the advancement of the Earth between two successive emersions from eclipses. Then the Earth moves toward Jupiter, the same time of lead will be observed between two eclipses in the respective points (F and G) of the orbit. So the difference between the periods of revolution (i. e., between the eclipses), observed at the opposite sides of the Earth's orbit, will amount to 7 min. However, Roemer pointed out that such a difference was not registered. And he immediately added: but it does not follow from this that light does not require time to travel. This is where Roemer's approach differs from that of Descartes in principle. Roemer felt that the velocity of light he had taken as a guess could be too small, and this in its turn could lead to an overestimate of the difference between the periods of revolution. Because the effect was not observed, it only meant one thing, that the velocity of light was greater than expected. Then what was to be done to determine it? Roemer provides a clear-cut answer:

> "... what was not sensible in two revolutions, became very considerable in many being taken together."

Let us try and perform a calculation similar to the one carried out by Roemer. Let T be the true period of the satellite's revolution around Jupiter. Suppose time is counted on the Earth since the satellite emerges from the shadow of Jupiter, and the Earth is nearest in its orbit to Jupiter. Let us count the satellite's eclipses as we see them until the moment the Earth passes the point most distant from Jupiter. Consequently, the observations will be finished at the time t when the satellite emerges from its nth eclipse.

If the Earth were stationary with respect to Jupiter, we could have written

$t = nT,$

It is clear that using the last formula it would have been possible to calculate the time of the end (or beginning) of any particular eclipse.

However, the distance between Jupiter and the Earth varies. Therefore the light reflected from Jupiter's satellite has to travel to the Earth a distance that is greater in the end of the observations than at the outset. The additional path the light has to traverse is evidently about the diameter d of the Earth's orbit. That is why the end of the nth eclipse will be registered on the Earth at a moment t' that is later by $\Delta t = d/c$ than the moment t calculated by the formula $t = nT$. Thus

$$\Delta t = t' - nT = d/c$$

and

$$c = \frac{d}{t' - nT}.$$

Naturally, these considerations can only give an approximate value of c: we disregarded the displacement of Jupiter over time t, assumed that the light from the satellite at the end of the observations would have to pass exactly the distance of the diameter of the Earth's orbit, and the like. In addition, nothing was said about the determination of the true period of revolution of the satellite around Jupiter. Without considering every admission, let me only mention that the revolution period determined from one revolution of the satellite differs *insignificantly* from the true one (Roemer himself noticed this). However, every assumption should only be accounted for if one intends to determine the value of c as accurately as possible. But Roemer was not interested in exactly this aspect: it was important to obtain an estimate of c to the order of magnitude and thus prove the velocity of light to be finite. For the purpose, our rough consideration will do.

Roemer was cautious: when he first reported his discovery he did not even mention any concrete value of the velocity of light. His cautiousness was quite understandable because the diameter of the Earth's orbit was only known then very approximately. The value $c = 214{,}000$ km/s, which is often quoted as the velocity of light calculated by Roemer, is nothing but a result of later estimates on the basis of Roemer's observations. We have no grounds to begrudge the accuracy of the first determination of the velocity of light because the main purpose, to prove that it is finite, was attained.

Few Roemer's contemporaries appreciated his work. I have already mentioned that Cassini rose against the explanation of the eclipse delay given by Roemer. Cassini offered a number of other reasons to observe the delays: that the satellite's orbit was stretched, that its motion along the orbit was irregular due to unknown causes, etc. When he published the results of his own observations on Jupiter's satellites, he resolved to announce those supporting Roemer's inference to be 'unreliable'.

Roemer's result was also not appreciated because of the 'nepotism' in the Paris observatory: every member of Cassini's family working there was up and against the idea of the finite velocity of light. Only one reason of the Cassini family appears to have deserved attention: there were no similar distinct irregularities in the motion of other Jupiter's satellites. Roemer was unable to give an answer to this because the theory of the motion of satellites of larger planets with their mutual influence was not yet well developed. His paper was published ten years before Newton's *Philosophiae Naturalis Principia Mathematica* (1687), where the law of gravitation was formulated.

Roemer's work was appreciated abroad: by Christian Huygens in the Netherlands, Isaac Newton, John Flamsteed, James Bradley, and Edmund Halley in England, and Gottfried Wilhelm Leibniz in Germany, and it was only in France, the country where the discovery was made, that it was not recognized. Roemer's position was aggravated by two more factors. First, he was not a member of the Paris Academy of Sciences (he became its foreign member, together with Newton, in 1699). Second, Roemer was a Protestant, and his presence in France was tolerated because of the Edict of Nantes, a decree issued in 1598 by Henry IV of France, granting restricted religious and civil liberties to Protestants. In the late 1670s, the political and religious settings in France began to change, the situation became intolerable for many scientists who were Protestants, and they started leaving the country. Even such outstanding scientist as Huygens, one of the first members of the Paris Academy of Sciences and its actual leader, had to leave for his native Netherlands. The Edict of Nantes was revoked in 1685 by Louis XIV, but Roemer did not wait to see it and returned in 1681 to Copenhagen, where he had long since been invited to occupy the chair of mathematics at the University. His destiny was unusual.

Soon after Olaf Roemer returned to his homeland, the Danish King Christian V appointed him the Royal Astronomer. Thanks to this, Roemer was allowed to use a good observatory in the Round Tower, established in the early 17th century. Before long, the King saw what a fine expert was on his service, and rushed a galore of appointments on Roemer. He became the master of the Mint, the surveyor of the harbours, the inspector of naval architecture, the advisor on pyrotechnics and ballistics, and the head of a commission to inspect and measure the highways of the realm. And after distinguished service in all the capacities the King endowed him with, Christian V made him a member of his Privy Council.

However, Roemer was not only a fine astronomer and engineer, he appeared to have had other talents. Frederick IV, who inherited the Danish throne in 1699, made Roemer a senator and then the head of the State Council. Roemer seemed to have no time for science. However, when he lived in Denmark, he did not at all reduced his scientific activity. Moreover, he even managed to expand its applied aspects.

When Roemer died, 54 instruments that he had invented were found in his private observatory, among them a transit circle (also known as meridian circle, or meridian transit), which is widely used in astronomical studies until now. Roemer was justly called the 'Northern Archimedes' for his inventive talents. And Roemer's authority in the organization of astronomical observations was so impressive that Leibniz himself sought his advice on the institution of an observatory.

Little is known on the results of Roemer's astronomical observations he made in Denmark:

most of his notes was burnt during a fire in 1728. The destiny of Roemer's heritage is the more so regrettable because, according to some evaluations, the volume of his observations was no less than that of Tycho Brahe, but they were surely made with far greater accuracy. The very small part of Roemer's notes, which his devoted disciple Peter Horrebow managed to save during the fire, was treated by German astronomers in the mid-19th century. This once again shows the significance of the observations made by the outstanding Danish astronomer. Roemer did not live to see the corroboration of the discovery that immortalized his name. He passed away on September 19, 1710.

* * *

The discovery of many physical phenomena and laws was the result of long and purposeful research. However, there are numerous cases in the history of science when scientists, looking for a certain effect, arrived at quite unexpected results. In particular, this concerns the discovery of an astronomical phenomenon of *aberration*, which independently confirmed Roemer's inference on the finite velocity of light.

During the last years of his life, Roemer worked on another problem which was also related to astronomy: that of the star *parallax*.

Generally, a parallax is the change in the apparent relative orientations of objects when viewed from different positions. In astronomy, the apparent changes in the position of a star due to changes in the position of the Earth as it moves around the Sun is called a 'parallactic displacement', also known as a 'parallactic shift'. Because an observer on the Earth participates in the yearly motion around the Sun, the direction toward a star would change in the course of one year: the star would seem to follow a closed trajectory in the sky. The form of the trajectory (Fig. 6) depends on the position of the star

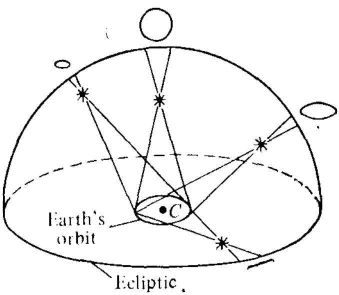


Fig. 6. Parallactic displacement of stars (C is the Sun). The trajectories of stars as observed from the Earth are different depending on the star position with respect to the plane of ecliptic.

with respect to the ecliptic (the ecliptic is the apparent annual path of the Sun among the stars, or the intersection of the plane of the Earth's orbit with the celestial sphere). To characterize the parallactic displacement, astronomers introduced a value called annual parallax and designated π: the angle at which an observer on the star would see the mean radius of the Earth's orbit while the direction to the star is perpendicular to the radius. Evidently, $\Delta = \alpha_0/\sin \pi$, where Δ is the distance from the Sun to the star and a_0 is the radius of the Earth's orbit. Inasmuch as $a_0 \ll \Delta$, we can assume $\Delta = a_0/\pi$.

The quest for the star parallax was triggered by Copernicus's ideas (1543) on the structure of the solar system. Naturally, the finding of a parallax would have been the most impressive evidence of the Earth's motion around the Sun. However, years and then decades passed by while nobody succeeded in revealing a parallax. Why so?

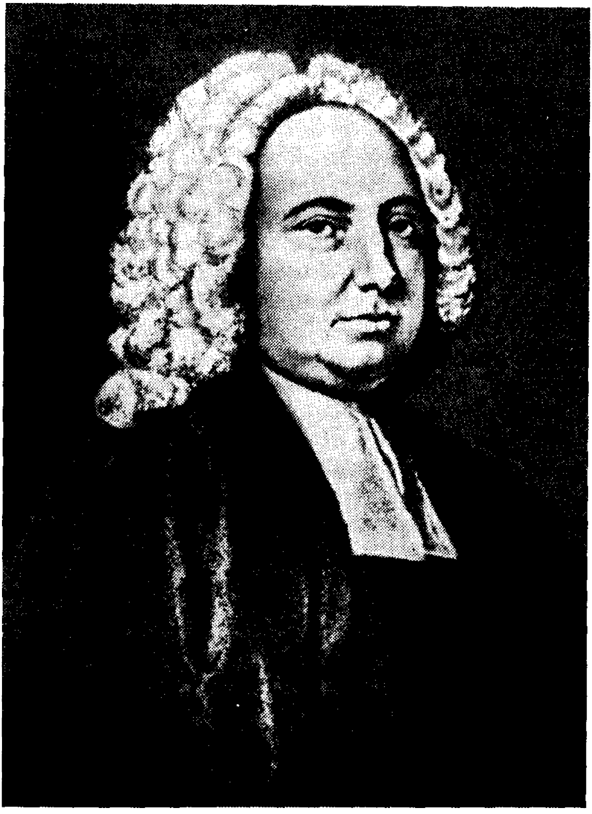
The point is that without knowing the annual parallax of a star it was also impossible to determine its distance from the Sun. This means that one could not estimate the accuracy to which it was necessary to conduct observations in order to discover a parallax because there always were two unknown values, Δ and π in the relationship $\Delta = a_0/\pi$.

It is now well-known that Proxima Centauri, the star that is the Sun's nearest neighbour, has $\pi = 3.6 \times 10^{-6}$ rad $= 0.76''$, $\Delta = 4.04 \times 10^{13}$ km, and $a_0 = 1.5 \times 10^8$ km. Therefore, in order to observe an annual parallax, the observations should be made in the course of a year within the accuracy better than $1''$. However, the 17th century scientists could not know it, and although the astronomical instruments of the period were not accurate enough, the astronomers carried out their observations in the hope to reveal the parallax displacement of a star.

From time to time, astronomers from various countries reported their 'observation' of a parallax. Thus, in 1671 Picard announced a $40''$ change in the position of Polaris, but the analysis of the results brought him to the conclusion that the effect could not be related to the parallax displacement. In 1674 the outstanding English scientist Robert Hooke (1635-1703) conducted similar measurements; he revealed a shift and considered it to be the sought parallax. John Flamsteed (1646-1719), the first director of the Greenwich observatory in England, was engaged in the search for a parallax displacement from 1689 to 1697. His interpretation of the observation results was in keeping with that of Hooke. However, in 1695 Cassini proved that the star parallax displacements should look different from what was observed by Hooke and Flamsteed. The problem of the existence of the parallax remained to be open for debates.

In 1725 Samuel Molyneux, a well-to-do English amateur astronomer, became interested in the problem. He possessed the latest astronomical instruments but did not feel to be competent enough to conduct the involved measurements, so he invited Professor James Bradley, who was experienced in astronomical observations, to participate.

James Bradley was born in 1693 in Sherbourn (Gloucestershire). As every boy in his family, he studied theology at Oxford, and in 1719 he became the deacon at a small town of Wanstead. However, he was engaged in astronomical observations starting from 1715: his uncle James Pound taught him astronomy. Although Pound was a priest, he was considered to be the best astronomer in England for many years. Bradley was a keen student, and Edmund Halley (1656-1742), the famous astronomer who later became the director of the Greenwich observatory, introduced him into the English scientific circles after only two years of study. Bradley's progress exceeded the best expectations, and in 1721 he,



James Bradley (1693-1762)

an alumnus in theology at Oxford, was invited to his alma mater to be the professor of astronomy.

The history of the observations made by Bradley both individually and jointly with Molyneux is very instructive; the result of them was the discovery of light aberration. Fortunately, we can follow every detail of the investigation because the first communication on the discovery was in the form of a letter Bradley wrote to his *senior friend and colleague* Halley. A letter is not a scientific paper, one can both present the results and describe the stages in the study, noting mistakes and errors corrected later. Bradley *was amazingly persistent and consistent* while working on a problem he was interested in. That is why his letter to Halley, which reflects the soundness of Bradley's scientific style, is read with unabating interest.

In the beginning, Bradley write to Halley that he had been stimulated to observe the 'fixed' stars because he wanted to verify Hooke's conclusions, who contended that he had found a parallax. Bradley felt sure that the discovery of this phenomenon required a greater accuracy than that reached by Hooke. Then Bradley did justice to Molyneux as the initiator of the observations, and to George Graham, a London optician,

> "to whom the lovers of astronomy are also not a little indebted for several other exact and well-contrived instruments."

To conduct observations with as great accuracy as possible, the telescope should be oriented *toward the desired object and then firmly fixed* in the wall. The fixation of the telescope was necessary in order to avoid very small inaccuracies in its orientation. The astronomers intended to *determine the changes in the position of a star* within the field of vision of the telescope which was for a long time (several months) to remain rigidly fixed.

Molyneux's instruments were ready for observations at his home in Kew in late November 1725, and the astronomers started with the observation of "the bright star in the head of Draco", viz., γ Draconis, on December 3. The choice was by no means accidental: the position of this star on the celestial sphere is such that the expected annual parallax displacement had to have the form of a circle (because the star is near the ecliptic pole, see Fig. 6) This made it possible to hope that if a displacement were found, it would be most probably related to the parallax phenomenon.

Further observations of γ Draconis were conducted on December 5, 11, and 12, and no noticeable displacement was revealed. It seemed that this time of year new observations were not needed because there was no hope to find the effect for such a short period. Only curiosity, as Bradley confessed, made him prepare for observation on December 17. The result was quite unexpected: the star was found a little to the south from the point it had been observed. The first guess was that the effect was due to errors in measurement. On December 20 the displacement of γ Draconis to the south was greater. This was the more so amazing because the shift was perpendicular to what could be expected as the consequence of the annual parallax. Inasmuch as now the effect could not completely be related to errors in measurement, the scientists surmised a change in the properties of the materials of which the instruments were made. However, a verification let them to discard this point. Moreover, the bulk of the obtained results made them look for a systematic cause of the effect.

In early March 1726 the star was observed 20″ to the south from the position it had at the beginning of the research. There was no noticeable shift of the star during this period, and this brought Bradley to the idea that the star reached its maximum displacement to the south. Indeed, in mid-April it began to move to the north, and in early June it was at the same distance from the ecliptic pole as it was in December. The rapid change in the star position in June (it moved at the rate of a second of arc in three days' time) evidenced that the star would further deviate to the north. In September its position was almost stationary, 20″ north of the June position. Then γ Draconis began to shift to the south.

It became finally clear by that time that the apparent shift of the star was not related to any errors in the observation. However, before offering a hypothesis for a new reason for the displacement, Bradley analysed every possible known reasons. The most evident was the nutation of the Earth's axis, i.e., the small perturbations in the motion of the Earth's axis of rotation caused by the gravitation of the Sun and the Moon. However, if the displacement of γ Draconis were caused by nutation, no explanation could be found for the results of observation of other stars. The strict periodicity of the effect indicated its possible relation to the position of the Sun, but Bradley failed to formulate any plausible hypothesis.

In the meanwhile, Bradley was more and more intrigued by the problem. He could not only be satisfied with the measurements carried out jointly with Molyneux because every time he had to go to Kew, where the equipment was. Therefore Bradley decided to install even better instruments, produced by Graham according to his indications, at his home in Wanstead.

Bradley began his independent observations on August 19, 1727; he estimated their accuracy within 0.5″. To offer a hypothesis concerning the apparent displacement of γ Draconis, it was necessary to observe other stars as well. This is what Bradley set about at his observatory. Several months later he was firmly convinced that the effect of displacement was of general nature and would be observed for every star. Bradley gained more and more data, and the facts evidenced the role of the Earth's revolution in the apparent displacement. Bradley's persistence was striking: he restrained himself trying not to haste in the offering of a new hypothesis:

> "Upon these considerations I laid aside all thoughts at that time about the cause of the forementioned phenomena, hoping that I should the easier discover it, when I was better provided with proper means to determine more precisely what they were."

Bradley's caution was fruitful: his proofs and conclusions are unambiguous and convincing even for the most rigorous criticism.

Bradley only started analysing his data following a year of observations. Once and again he turned over in his mind possible causes: (1) the nutation of the Earth's axis, (2) the deviation of the plumb line from the vertical, and (3) the astronomical refraction (the bending of a ray of celestial radiation as it passes through atmospheric layers). But no, neither of the suggestions produced the results in keeping with observations. And finally he brought forth a new hypothesis of his own: the apparent displacement of the stars is related to the motion of the Earth along its orbit and to the finite velocity of light. Now I give the floor to Bradley himself:

> "I consider this matter in the following manner. I imagine *CA* to be a ray of light falling perpendicularly on the line *BD* (Fig. 7); then if the eye be at rest at *A*, the object

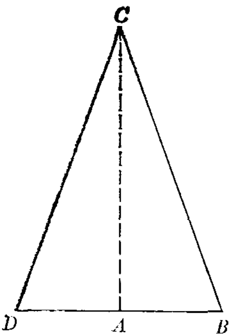


Fig. 7. A diagram from Bradley's paper explaining aberration of light.

> 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*, ...] 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*... In the like manner, if the eye moved to the contrary way, from *D* towards *A*, with the same velocity; then the tube must be inclined in the angle *BDC*...

"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, although the eye were in motion, for in that case, *AC* being infinite with respect to *AB*, the angle *ABC* (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."

Consequently, Bradley's explanation of the displacement of celestial bodies is similar to the explanation of oblique traces of rain on the window of a moving train when there is no wind.

The treatment of the aberration of light in Bradley's letter is followed by a detailed analysis of the observations on the basis of the offered hypothesis. He showed that the calculated displacements of stars almost exactly coincide with the observations: in neither of 29 Bradley's cases the difference between calculations and observations exceeded 2″ at the maximum magnitudes of displacements of the order of 35″. (It is clear that to estimate the accuracy of calculations, one should take maximum displacements because the relative uncertainties of measurement are minimum in these cases.)

And at long last, Bradley came to a conclusion:

> "I will therefore suppose that it [the angle *DCB* in Fig. 7] is $40^1/_2$″, or (which amounts to the same) that light moves, or is propagated, as far as from the sun to us, in $8^m\ 13^s$."

It is easy to conceive the manner in which Bradley calculated the time t light takes to traverse the distance equal to the radius a_0 of the Earth's orbit if one uses the above-mentioned relationship $\sin(\widehat{DCB}/2) = v/c$. The linear speed of the Earth moving along its orbit is $v = 2\pi a_0/T$, where T is the time of one revolution of the Earth around the Sun (a year). But $t = a_0/c$, and hence $\sin(\widehat{DCB}/2) = 2\pi t/T$. Knowing the angle *DCB* and the time T, we can determine t.

Then Bradley wrote:

> "The near agreement I meet with among my observations induces me to think that the maximum [measured magnitude of the angle *DCB*] (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^s or 10^s; which is such a degree of exactness as we can never hope to attain from the eclipses of Jupiter's satellites."

Thus Bradley gave a new estimate for the time light requires to pass an interval equal to the radius of the Earth's orbit. Recall that, in Roemer's opinion, light takes 11 minutes to traverse the same distance. Therefore the value of c according to Bradley's data should be about 1.4 times more than it follows from Roemer's results. However, pay attention to the cautiousness each of the scientists exhibited: neither mentioned the absolute value of the velocity of light. This is far from being accidental: I have already mentioned the fact that to do so, it was necessary to know the mean radius of the Earth's orbit. In the time of Roemer and Bradley, this value was not determined accurately enough, and therefore the numerical value of c could only be found with an error considerably greater than the time light takes to travel from the Sun to the Earth. According to modern data, light takes 8 min 19 s to pass this distance, and therefore it is only just to contend that Bradley perfectly estimated accuracy of his measurement ($\simeq 2\%$). Indeed, it follows from Bradley's calculations that the error in the determination of the time light takes to travel from the Sun to the Earth is $\Delta t = 10$ s, hence, with the mean time of passage $t = 8$ min 13 s, the relative error $\varepsilon = \Delta t/t$ amounts to about 2%. Note that any estimate of measuring error is a subtle and sophisticated field, so even now it is very rare that a subsequent measurement of a fundamental constant gives a value well within the accuracy of the preceding measurements.

Bradley's discovery was far more lucky than Roemer's work: when Bradley revealed the phenomena of aberration, nobody doubted any more the finite velocity of light. Besides, the determination of the velocity of light according to Bradley's data can hardly be called an estimate because the error of 2% was quite satisfactory for the 18th century.

Bradley's excellent work recommended him well. In 1729 he was invited to lecture on 'experimental philosophy' (i. e., physics) at Oxford, where earlier Bradley was giving a course on astronomy. Bradley continued to give lectures till 1760, and his authority among the physicists and astronomers was great. It was a small wonder that when Halley died in 1742, it was Bradley who became the Royal Astronomer, i.e., the director of the Greenwich observatory. In 1748 he won a Copley medal, one of the highest awards of the Royal Society of London; other awards were conferred on him as well.

Bradley's scientific heritage is enormous: over 60,000 observations. He determined the position of well over a hundred stars, which were later included in star catalogues. Besides aberration, his most important contribution to astronomy was the discovery of the nutation of the Earth's axis. James Bradley passed away on July 13, 1762.

Consequently, Bradley completed the first stage in the history of measuring the velocity of light. It was reliably established that the velocity of light is finite. Its magnitude was determined proceeding from Bradley's data on the time the light of the Sun covers the distance to the Earth and taking into account calculations of the radius of the Earth's orbit by the annual solar parallax: $c = 284{,}000$ km/s. The agreement between Roemer's and Bradley's results allowed Bradley to make an important inference that light, while being reflected from Jupiter's satellites, essentially does not change its velocity.

From the modern viewpoint, it seems to have been very handy that the first measurements of c were provided by astronomy because it gave the chance to determine the velocity of light in a vacuum. However, there was still much room for further investigation: recall that the problem of the velocity of light was still not connected with the problem of the nature of light. The situation changed with the advance of a new stage in the development of optics.

Chapter 3

The Struggle of Theories

In a sense, the development of science reminds of the volcanic activity. For many years, decades, and sometimes centuries, a certain field in science looks like an extinct volcano, and it may seem to an observer that it did not develop practically at all. And then, all of a sudden, an 'eruption' of new facts and ideas occurred in this 'quiet' field, and a new 'cone' of knowledge was formed thus proclaiming a transition of this area of science to a new stage of development. Naturally, each 'eruption' in science, just as any eruption of real volcanoes, is underlain by profound causes. The quest for such causes is the utmost pursuit for a historian of science. However, even the knowledge of the laws of the development of science cannot subdue the admiration for the potent impulses of cognition.

There were such periods of 'volcanic' activity in the history of optics. The first period occurred in the late 17th century. The discoveries that laid the foundation for physical optics were made in less than fifteen years' time: suffice it to have a glance at a concise list of the main events:

1665 Francesco Grimaldi's book, describing the experiments on the diffraction of light, is issued; Robert Hooke publishes his book, where he treats the colouration of thin films, i.e., a manifestation of the interference of light.

1669 Erasmus Bartolin reports his observation of double refraction in iceland spar.

1672 Isaac Newton publishes the memoir presenting his experiments and proving the objective nature of colours.

1676 Olaf Roemer proves that the velocity of light is finite.

1677 Christian Huygens conducts experiments on the polarization of light.

Thus the science of optics was transformed within a historically short period of time. However, the 17th century was not only marked with the excellent experimental discoveries: this was the period of the delivery of two theories of light underlain by different views on its nature.

The proponents of the *corpuscular* theory considered light as consisting of a stream of particles. The other, *wave* theory, assumed that light was the motion of a fine matter, or the ether. The development of the corpuscular theory is commonly related to the name of Newton although his views on the nature of light and the mechanism of its propagation cannot unambiguously be called corpuscular. (In particular, Newton was the first to draw attention to the periodicity in certain optic phenomena.) The utmost contribution into the initial development of the wave theory of light was made by Hooke and Huygens.

However, one should not think that the wave theory of light took its final shape during the period: neither Hooke nor Huygens could explain a number of optic phenomena and, in addition, their views were in many respects different from the modern ones. For instance, Huygens formulated the principle that bears his name: each point a light disturbance reaches may be regarded as a source of secondary waves; however, in Huygens's opinion, the light disturbances propagating in ether lack any periodicity whatsoever, i.e., his theory considered the propagation of impulses rather than waves. Clearly, these views could not provide an explanation to the phenomena of diffraction and interference, both caused by periodic waves.

There was no crucial evidence to the advantage of either of the light theories in the late 17th century. For a number of reasons, Newton's authority among them, a simplified (as compared with Newton's) corpuscular theory was vastly popular in the 18th century. Just a few scientists still adhered to the theory of Hooke and Huygens: Leonhard Euler, Benjamin Franklin, and Mikhail Lomonosov. The 18th century, or the 'Age of Reason', as it is sometimes called, did not enrich the science of optics by any new ideas. This was a period of stagnation in the development of the theory of optic phenomena.

A new rise in physical optics is related to the early 19th century. Thomas Young (1773-1829), an English physicist (and physician) published his papers, where he formulated the principle of the interference of light. Following an analysis of numerous experiments, Young concluded that light disturbances are periodic, and they are waves, which can superimpose, thus giving rise to either mutual amplification or annihilation. Young widely used the analogy between optical and acoustic phenomena and between light and waves on the surface of water.

Augustin Jean Fresnel (1788-1827), an outstanding French physicist, followed Young in the further development of the wave theory of light. Fresnel complemented the Huygens principle with the idea of the interference of secondary waves (now this is called the Huygens-Fresnel principle), he explained the mechanism of diffraction and why light propagates in straight lines in free space (this fact was the stumbling-block for the proponents of the wave theory of light).

The polarization of light brought about a bitter discussion between the advocates of the wave and corpuscular theories. Although Fresnel offered the idea of the transverse nature of light waves, it was not believed to be convincing enough because the explanation of transverse waves required the carrier of light disturbances, elastic ether, possess paradoxical properties: it had both to be solid and frictionless.

On the whole, however, the wave theory was gradually winning the recognition of physicists. The mathematically sophisticated corpuscular theory extended by the French scientists Jean Baptiste Biot (1774-1862) and Siméon Denis Poisson (1781-1840) looked rather far-fetched when compared with Fresnel's theory. Nevertheless, it was necessary to carry out a *crucial* experiment (experimentum crucis), the result of which could *only* be explained on the basis of the wave concept, otherwise it would not finally win over. The idea of a crucial experiment in optics had long since been known. It was related to the topic of this story, the measurement of the velocity of light, and consisted in the comparison between the velocities of light in the media with different indices of refraction.

The explanation of the refraction of light offered by the corpuscular theory was very much like that of Descartes. A particle of light arriving from a less dense medium at the interface with a denser medium does not change the component of its velocity that is parallel to the interface while the component that is perpendicular to the interface increases. This seemed to be the only way of explaining experimental facts. For instance, when light passes from air into glass or water, the angle of incidence is greater than the angle of refraction. Let me remark that this viewpoint is not at all absurd: a similar effect can be observed when a charged particle, e.g., an electron, passes through a thin mesh capacitor the potential across which accelerates the electron. The similarity between the laws governing the motion of charged particles and the laws of geometrical optics made it possible later, in the 20th century, to establish a new scientific field, electron optics.

But back to the theory of light. The most essential postulate of the corpuscular theory was that the velocity of light in a medium is more if the index of refraction, inherent in the medium, is more. (I shall neglect the phenomenon of dispersion of light, i.e., the dependence of the index of refraction on the wavelength, or colour, of light.) Since the time of Huygens, the wave theory asserted the opposite. The proof of the refraction law on the basis of the Huygens' principle is known to everybody who is familiar with elementary physics. The inference is that the relative index of refraction equals the ratio between the velocities of light in the two media involved, i.e.,

$$n_{12} = \frac{v_1}{v_2};$$

as to the absolute refractive indices (n_1 and n_2) in two media, it holds true that $n_1 < n_2$ if $v_1 > v_2$.

Consequently, a comparison between the velocities of light in different media could meet the most substantial requirement to a crucial experiment: the advantage of a theory could even be revealed if the experiment were purely qualitative. For instance, if the velocity of light in air proves to be greater than in water, then the wave theory should be recognized to be valid, while if the result of the experiment is reverse, then the corpuscular theory wins over.

The idea is clear, as are the difficulties facing its implementation. The basic problem was that the experiment had to be conducted on the Earth because astronomy was of no help in the matter. Therefore the distances light would have to cover were not to exceed several kilometres. Hence, the challenge was to elaborate a method for registering processes occurring within very small fractions of a second: if the path of light $l = 30$ km, then the time of its travel $t = 10^{-4}$ s. The present-day physicists would consider such a lapse of time to be 'immense', but to measure it was no easy job for the technology of the early 19th century.

* * *

The history of physics has seen many examples when the invention of an instrument or a new measurement technique has brought about a breakthrough in the areas seemingly far from the purpose of the invention. This concerns the history of the first 'terrestrial' attempts to measure the velocity of light.

In the mid-1830s, the French physicist Dominique-François Arago, the permanent secretary of the Paris Academy of Sciences, submitted a proposal to nominate the English physicist Charles Wheatstone (1802-1875) for the vacant position of a corresponding member of the Academy. At present Wheatstone is known mainly by the method of measuring electrical resistance with the aid of the 'Wheatstone bridge'. The irony of fate is that the inventor of the bridge was not Wheatstone but the English scientist Samuel Christie. At the same time such an important Wheatstone's invention as a rheostat does not bear his name.

Arago was well aware of Wheatstone's work, and considered his experiments on "the velocity of transmission of disturbance of electric equilibrium" to be salient enough to nominate Wheatstone for membership in the Paris Academy of Sciences. This series of Wheatstone's experiments is essential for a number of reasons. Firstly, it related, although indirectly, the velocity of light and the velocity of transmission of disturbance of electric equilibrium. Secondly, it gave an impetus to the determination of the velocity of light in different media. Thirdly, Wheatstone's experiments can be regarded as an anticipation of Hertz's experiments, in which the existence of electromagnetic waves was proved. However, the main point in Wheatstone's experiments was the development of a new techniques of investigating rapid processes.

Wheatstone began his paper on the experiments as follows:

> "The path of a luminous or an illuminated point in rapid motion, it is well known, appears as a continuous line, in consequence of the after duration of the visual impression. There is nothing, however, in the appearance of such a line by which the eye can determine either the direction or the velocity of the motion which generates it. It occurred to me some years since, that if the motion which described the line in these cases were to be compounded with another motion, the direction and velocity of which were known, it would be easy, from an inspection of the resultant straight or curved line, to determine the velocity and direction of the former. Following up this idea, I made a series of experiments relating to the oscillatory motion of sonorous bodies..."

Wheatstone successfully conducted his acoustic experiments and set about studying the processes in electric circuits.

The electric circuit in Wheatstone's experiment included the following elements: two long (400 m each) pieces of insulated copper wire, a Leyden jar (a capacitor), which was charged by means of an electrical machine, and a spark-board with a set of discharge gaps. Three gaps *1-2*, *3-4*, and *5-6* are shown in Fig. 8. The fourth gap is not shown; it was arranged between two movable metal balls. When the balls were put close to each other, a spark appeared, this changed the potentials in the conductors connecting the fourth gap with the set of gaps *1-2*, *3-4*, and

5-6, and sparks appeared in these gaps as well. The electric circuit was such that the sparks in the gaps *1-2* and *5-6* would appear simultaneously, be the velocity of transmission of 'electrical disturbance' finite or infinite. As to the gap *3-4*, it was connected through the long wires. Therefore, if the 'electrical disturbance' (i.e., a change

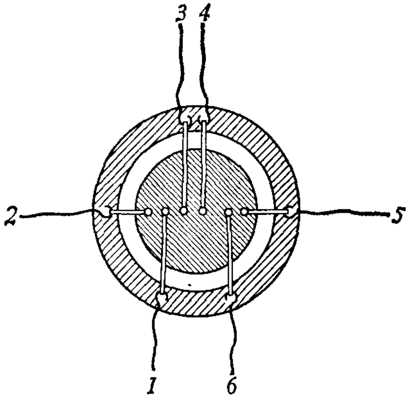


Fig. 8. The discharge gaps in Wheatstone's experiment.

of potential) was transmitted along the wires with a finite velocity, the spark in this gap would appear somewhat later than in the gaps *1-2* and *5-6*. Determining the delay of one spark with respect to the other two and knowing the length of the wires, one could find "the velocity of transmission of disturbance of electric equilibrium". Wheatstone intended to calculate this velocity at the second stage of his study while proving that it was finite at the first stage.

The most essential part of the installation was an optical system designed by Wheatstone. The discharge gaps were fixed in the spark-board, which was mounted on the wall so that the line passing through the centres of the gaps was horizontal. The most important element was a mirror, whose axis of rotation was also horizontal

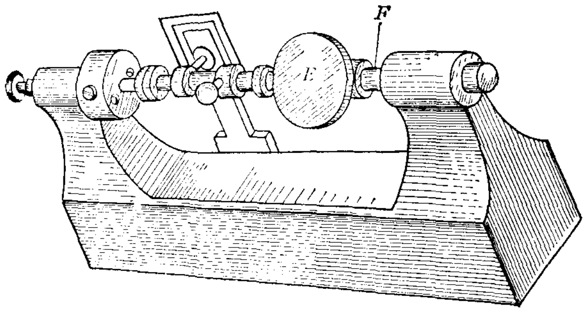


Fig. 9. A part of Wheatstone's apparatus with a rotating mirror E; F is the axle driven by a belt transmission.

(Fig. 9). A discharge in each of the gaps was sustained for a finite time t, in the course of which the mirror turned by an angle α. The image of the spark in the rotating mirror displaced, and the eye, with its *inertia* of perception, registered a luminous segment of a straight line rather than a point flash. An observer in Wheatstone's experiment saw three segments, i.e., three traces of the three sparks. If all the three discharges had occurred simultaneously, the corresponding ends of the luminous segments would have been aligned. However, in Wheatstone's experiment the sparks in the left and right gaps appeared simultaneously and before the spark in the middle gap, so the trace of the middle spark was displaced with respect to the other two traces. The direction of the shift depended on the direction of rotation of the mirror.

Figure 10 shows the Wheatstone's installation without the spark-board. Its principal parts are

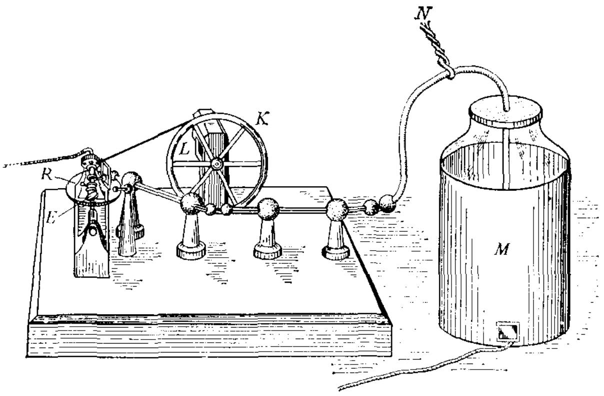


Fig. 10. A general view of Wheatstone's apparatus.

the rotating mirror *E* fixed to the axle driven by belt transmission from the wheel *LK*, the Leyden jar *M* charged through the conductor *N*, and an observation lense *R* located above the mirror. The discharges were synchronized with the rotation of the mirror, so that if the rotation speed was constant, the observer saw fixed spark traces.

The result achieved by Wheatstone supported the hypothesis of the finite velocity of transmission of electric disturbances. However, Wheatstone failed to obtain a reliable quantitative estimate: he reached an erroneous conclusion that this velocity was greater than the velocity of light.

Arago appreciated Wheatstone's experiments not because of the obtained results but because of the *possibility* to apply the *rotating mirror* technique for a wide spectrum of other problems. In Arago's opinion, it was practicable to use it for measuring the velocity of light in different media. The fact that it was Arago who drew attention to the chance to apply the technique for the experiments which were so important in optics was not accidental. Arago was researching in optics for many years, and the validity of the wave theory of light was of special interest to him. Like a tuning fork, his scientific intuition immediately responded to Wheatstone's experiments although, at first glance, they did not seem to be related to optics. Arago's response revealed his broad scientific views.

Dominique-François-Jean Arago (1786-1853) was an alumnus of the famous École Polytechnique, which had been established in Paris during the French Revolution of 1789-1799, and where the standards of teaching were high. Arago's first scientific work was his participation in the measuring of the length of the Earth's meridian, which was necessary for the elaboration of the reference metre, a most essential unit of a new, metric system of units developed by French scientists. In 1805 Arago and Biot went to Spain to conduct some measurements. Arago was arrested there during a revolt of Spaniards against French invaders because he was from a hostile state, but he managed to flee to Algeria. Arago set out for Marseilles but was captured by Spaniards again. Next time he was free, he could not get to France because of a storm and arrived at the Island of Sardinia. Then he served in

Algeria as an interpreter, and it was only in 1809 that Arago was able to return to his homeland.

Arago's scientific interests were related mainly to optics and astronomy. Science is obliged to him for his important research in the field of polarization of light. Optics drew Arago to the work of the young engineer Augustin Fresnel. In many ways Arago promoted the success of Fresnel's work in wave optics, and they conducted a number of experiments together. Fresnel and Arago's experiment proving the existence of the 'Poisson's spot' is especially salient. While discussing a Fresnel's paper in the Academy of Sciences, Poisson, an active supporter of the corpuscular theory of light, remarked that, according to Fresnel's wave theory, there may appear a bright spot in the centre of the shadow of a small object. In Poisson's opinion, this contradicted the common sense. However, a control experiment, conducted by Arago and Fresnel, revealed the existence of the bright spot and thus gave a new evidence of the validity of the wave theory.

Arago lived through a tumultuous period in the history of France. He saw the demise of the Napoleon's Empire and witnessed the Bourbons' restoration, the July 1830 revolution, and the July 1832 rebellion. Arago participated in the political life of the period. In 1832, when the École Polytechnique was subordinated to the Ministry of War because the students had taken part in the rebellion, Arago resigned his professorship as a token of protest. He was a member of the Chamber of Deputies and used his position to influence the policy of the government. During the 1848 revolution, Arago was a member of the Provisional Government. When Napoleon III came to power, Arago was bold enough to champion the citizens exiled after the revolution. Arago died in 1853, leaving behind both many essential scientific papers and biographies of a number of prominent physicists, mathematicians, and astronomers. Arago had been performing the duties of the permanent secretary of the Academy of Sciences for many years, and one of these duties was to give eulogies, i.e., praise deceased members of the Academy in a speech. Arago prepared these speeches meticulously, and because he was familiar practically with everybody of whom he had to speak, either personally or by correspondence, his *Biographies* remain to be a valuable source for the history of science.

Now let us return to Arago's scientific work. In late 1838 he reported at a session of the Academy of Sciences and suggested the idea of an experiment for comparing the velocities of light in the air and a liquid. Note that this was almost exactly 200 years since Galileo had published his *Dialogs* in 1632, where he had described a hypothetical experiment to determine whether the velocity of light is finite. How drastically the approach to physical experiment had changed!

Galileo's suggestion had been rather speculative, and he had said nothing on the possibility of obtaining a positive result. Arago's approach was different although like Galileo he began with a description of the principle of the experiment.

Two beams of light a and a' (Fig. 11) from point sources A and A' fall on a flat mirror rotating about the vertical axis MM; the beams a and a'

belong to a common vertical plane. Suppose the duration of flashes of sources A and A' is infinitely short and both of them occur simultaneously. If both beams travel over equal optical paths, the reflected images of points A and A' will belong to the same vertical line. If the mirror does not move, the result will not change when

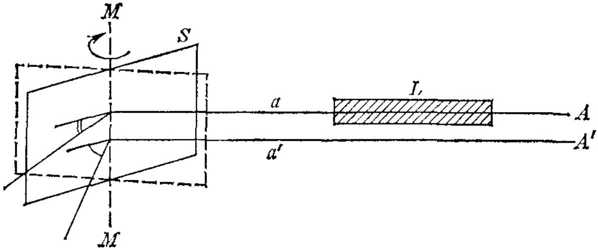


Fig. 11. A diagram showing the idea of Arago's experiment. A and A' are sources of light, L is a tube filled with water, and S is a mirror rotating around the axis MM.

the optical paths are different. Now let the upper beam a pass on its way to the mirror through a tube filled with water while the beam a' passes through the air. Both the wave and corpuscular theory agree that light from the sources A and A' will take different time to travel to the mirror. However, the theories disagreed as to which beam would reach the mirror sooner. It is clear that if it rotates, the reflected beams will not be parallel. Indeed, the mirror will turn by an angle α during the time between the two reflections, and the beams will diverge by an angle 2α. One can try and register this divergence. It is more convenient to conduct observations if a narrow vertical slit rather than two point sources is used, and the light from the upper half of the slit passes through the tube with water while that from the lower half passes through the air. If the source is a slit, the observer will see the images of the two parts of the slit displaced. If the mirror rotates in the clockwise direction and the lower half of the image is to the right of the upper one, then the inference of the wave theory that the velocity of light in air is greater than in water will be supported, and if the result is reverse, then the velocity of light in air must be less than in water as the corpuscular theory predicted.

> "The above-mentioned ideas are theoretically, or, still better, speculatively exact. And now, this is a delicate point, it remains to prove that despite the immense rapidity of light, despite its velocity of about 80 000 lieues* per second, despite we have to use only short tubes filled with water, and despite the limited speed of rotation of the mirror, the comparative deviation of the two images (to the right or to the left), the existence of which I have shown, should be perceptible in our instruments."

This excerpt from a 1838 Arago's paper distinctly shows the difference between his approach to the experiment and that of Galileo. The paper gives a thorough analysis of the details of the suggested experiment. The following questions are posed in succession:

(1) What speed of rotation of the mirror is possible?

---

* 1 lieu was equal to 3.75 km in the 19th century France.

(2) What must be the duration of the spark discharge so that it is just long enough for the eye to register an image?

(3) Will light be able to pass through the required length of liquid?

(4) What must be this length?

Answering the first question, Arago believed that with gears the speed of rotation of the mirror was about 1000 revolutions per second. However, Arago contended that this speed could be increased if the rotating mirror with its drive could be fixed to a frame rotating with the same speed. It was possible thus to double, triple or even quadruple the speed of rotation with respect to stationary sources of light.

Analysing Wheatstone's results, Arago concluded that the use of an electrical spark satisfied the requirements of the experiment to the duration of the flash.

To answer the third question, Arago made use of the research on the absorption of light conducted in the 18th century by the French physicist Bouguer, one of those who founded photometry.

And last, Arago estimated the needed length l of the tube with water. The estimate depended on the accepted theory of light, the greater length (the worst case) being required if the corpuscular theory was valid. Arago had to consider this case: physicists always have to consider the most unfavourable situation while producing preliminary estimates.

Arago reasoned as follows. Using an ocular, the observer can register as small divergence of the beams as $2\alpha = 1'$, which corresponds to a possible delay of one beam with respect to the other for the time the mirror turns by an angle of $\alpha = 0.5'$. At the speed of 1000 rps it takes the time $\Delta t = 2.3 \times 10^{-8}$ s. According to the corpuscular theory, the velocity of light in water is $n = 1.33$ times greater than in air. The delay of Δt, appearing when the beams pass the same length in different media, equals

$$\Delta t = \frac{l}{v} - \frac{l}{nv},$$

where l is the length of the tube and v is the velocity of light in air.
Hence

$$l = v\,\Delta t\ \frac{n}{n-1}.$$

The velocity of light in air could be accepted for the estimate equal to the velocity of light determined from astronomical observations, from which $l = 28$ m. Similar calculations proceeding from the wave theory give $l = 21$ m.

Arago was aware that with such a length of the tube the observer would hardly be able to notice the image of the spark appearing for a very brief time. However, the situation could be radically changed if the speed of the mirror were increased. Besides, it was actually possible, with the aid of an ocular, to register angular distances less than 1'. There were also other suggestions Arago advanced to improve the experiment. On the whole, Arago concluded that the crucial experiment to decide which of the two theories of light was correct was possible.

Arago's report was approved. It was published in the journal of the Academy of Sciences and drew attention abroad. Physicists were eager to learn the result of the experiment Arago intended to conduct. But years went by while there was no news on the experiment. What was the reason to put it off?

Now, a century and a half later, we can easily point out the essential drawbacks in Arago's experiment making it unfeasible. First of all, if the sources of light were separate, it was unthinkable to synchronize the flashes accurately enough. I have already mentioned that Arago himself suggested an improved version of the experiment using one long source of light. However, there were some stumble-blocks even in this case. Inasmuch as the rotation of the mirror was not synchronized with the flashes of the source, one could not indicate the direction in which the image of the slit should be observed. Each flash could produce the image in a new direction. Arago suggested to carry out the experiment with several observers seated along a circle and each armed with his own ocular. However, the chance to notice the shift of the images was poor if the flashes were short and the observation could not be repeated.

I have listed the difficulties which could have been expected. Ten years later Arago himself described other, purely technical problems he encountered while trying to conduct the experiment.

Arago invited Lois Bréguet, a well-known Paris mechanic and watch-maker, to make a system with the rotating mirror. Bréguet managed to produce a system of three rotating mirrors. The use of three mirrors allowed Arago to step up the divergence between the beams and therefore to make the tube with water shorter. However, the spark was practically invisible after three reflections. Arago had to give up the idea of multiple reflections, and he tried to increase the speed of rotation of the only mirror. But there were unforeseen difficulties in this as well.

The axle with the mirror rotated at a maximum speed of 1000 rps, while without the mirror the speed was up to 8000 rps. Arago thought that the reason was the resistance of the air at high speed, so a special casing for the installation was produced, from which the air could be pumped out. The system was mounted on a stone column in the Paris observatory, but ... the mirror with the air pumped out would not rotate any faster. Arago remarked in this connection that his failure once more confirmed the proverb, "The best is the enemy of the good."

The experimenters returned to the system with three metal mirrors trying to increase their reflectivity. The work was not brought to an end: Arago's vision severely deteriorated and this made him give up the experiment. So the physicists had to wait for the first 'terrestrial' determination of the velocity of light for more than another ten years.

I would like to draw the attention of the reader to the fact that the experiment suggested by Arago was purely qualitative. Although in the end of his report Arago said that he reserved his right to suggest a scheme of an experiment for the determination of the absolute velocity of light, he did not offer one. At the same time the measurement of the value of c in terrestrial conditions was a matter of principle because, on the one hand, it could confirm the correctness of the astronomical methods and, on the other hand, it would be the first step towards the experiment

Léon Foucault (1819-1868)

on the determination of the velocity of light in different media.

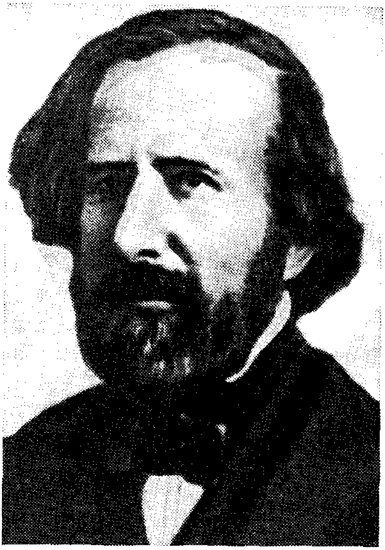
Thus, thanks to Wheatstone's and Arago's research, the idea of the crucial experiment was firmly substantiated, and a great preparatory work was carried out. However, the credit of the implementation of the crucial experiment belongs to scientists of the next generation.

The problem of the 'terrestrial' determination of the velocity of light became a milestone in the life of two scientists at the same time: Hippolyte Fizeau and Léon Foucault. It is rare that the beginning of life of two scientists interested in the same problem is so much similar as with these two outstanding French physicists.

Foucault was born on September 19, 1819, in Paris into the family of a rather well-known publisher and book dealer. Foucault was a frail and sickly child showing no special interest in learning. Because of his poor health, he only studied from time to time and completed his secondary education very late. However, Foucault revealed an exceptional talent to handiwork. When he was thirteen, he succeeded in producing a model of a steam engine almost without any tools at all. This talent predetermined Foucault's original intention to become a surgeon. The prospects for this medical student were bright, but... the sight of blood and human suffering was too much for Foucault, and he was made to give up surgery. However, medicine helped the young man find his true vocation. Thanks to medicine, he made the acquaintance of the teacher of clinical microscopy Donné, who invited Foucault to join him in his research.

Foucault performed his first independent studies in the newly-born field of optics, photography, or, as it was called then, daguerreotypy. It was in connection with this work that he applied for consultation to another young physicist, Hippolyte Fizeau.

Fizeau was younger than Foucault by only four days. He was the elder son of a well-to-do physician. His father occupied the chair of internal pathology at the Medical Department of Paris University. The young Fizeau wanted to go in his father's footsteps and set about studying medicine but had to drop out for he was ill



Armand Hippolyte Fizeau (1819-1896)

and needed to change the climate. When he returned to Paris, his interest in medicine gave way to the desire to go in for physics. Fizeau did not enlist as a student of any educational institution but began to attend the lectures of Regnault, a well-known physicist and a member of the Paris Academy of Sciences, and followed his brother's notes of the lectures given at the École Polytechnique. Fizeau's good luck brought him to meet Arago, who invited the young man to work in the Paris Observatory.

Fizeau's first scientific papers, like Foucault's, were related to his attempts to improve the process of photography, for one, to simplify the development of negatives. When Foucault's and Fizeau's paths crossed, Fizeau was a mature specialist in the field. Their first scientific contacts grew into cooperation. Working under Arago's influence, Fizeau and Foucault jointly conducted a series of significant experiments confirming the wave nature of light. The history of science knows their observations of light interference at great path differences between two beams: in 1846 they succeeded in observing interference at a path difference of 7000 wavelengths. The essential point in the success of these experiments was that they used monochromatic light. It is interesting that their scheme of producing monochromatic light was almost the same as the one employed by Kirchhoff and Bunsen to discover spectral analysis. However, despite their fruitful cooperation, Fizeau and Foucault stopped working together in the late 1840s and entered a period of their scientific competition. Its goal was to measure the velocity of light in terrestrial conditions.

Fizeau and Foucault took different paths in the solution of the problem. Fizeau intended first to measure the velocity of light in air and only then carry out the crucial experiment while Foucault decided to conduct the crucial experiment first.

In principle, Fizeau's scheme of the experiment reminds that of Galileo's hypothetical experiment although the second observer was replaced by a fixed mirror (Fig. 12). Light from a source A passed through an objective O_1 to a semitransparent mirror m_1 and was partially reflected from it. The reflection focussed the image of the source at a point a located in the plane of rotation of a toothed wheel breaking the light beam into impulses. Having passed through the toothed wheel and the optical system O_2, the light was directed in a parallel beam to an objective O_3. The light was focussed on a flat mirror m_2, which reflected it and returned it to the

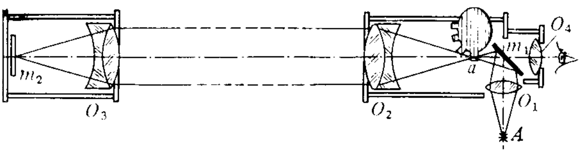


Fig. 12. The scheme of Fizeau's experiment, A is the source of light, m_1 is a semitransparent mirror, m_2 is a flat mirror, a is a toothed wheel, O_1-O_3 are objectives, and O_4 is the ocular.

toothed wheel. If during the time of motion of the light from the wheel to the mirror m_2 and back the wheel turned so that there was a tooth instead of the slit through which the light initially passed, then the field of vision for the observer behind the semitransparent plate m_1 and an ocular O_4 was dark. However, if another slit appeared in the path of light, the field of vision was illuminated. Evidently, periodic darkening and illumination would be seen with a gradual increase in the speed of rotation of the wheel. The first darkening appeared when during the time the light took to travel to the mirror m_2 and back the wheel turned by half an angular distance between its two teeth, i.e., by an angle of $\alpha_1 = 360°/2n$, where n is the number of teeth of the wheel; the field of vision was illuminated at twice as great angle of turn, the next darkening occurred at $\alpha = 3\alpha_1$, and so on.

The parameters of Fizeau's apparatus were as follows. The source of light and the mirror m_1 were fixed in the house of Fizeau's father near Paris, and the mirror m_2 was located in Montmartre, the distance between the mirrors thus being $l \simeq 8.66$ km. The wheel had 720 teeth and was driven by a clock mechanism triggered by a descending weight. Using a revolution counter and a chronometer, Fizeau found that the first darkening was observed at the speed of the wheel rotation $\nu = 12.6$ rps. The time of light travel was $t = 2l/c$, whence

$$\frac{2l}{c} = \frac{360^\circ}{2n\omega},$$

where $\omega = 360^\circ\nu$, and thus

$$c = 4nl\nu.$$

Substituting the values of n, l, and ν gave $c = 3.14 \times 10^8$ m/s, i.e., a number slightly greater than that obtained from astronomical observations. Despite a considerable error of measurement, Fizeau's experiment was important in that it proved the possibility of measuring the velocity of light by the 'terrestrial' means.

Why didn't Fizeau, beyond any doubt familiar with Arago's work, use in his first experiments the method suggested by Arago? It appears that there were two possible reasons. First, the younger scientist wanted to invent something new. Second (I have already mentioned this), Arago's experiment was qualitative while Fizeau intended to measure the absolute value of c. The advantage of Fizeau's technique over Arago's was that the use of a wheel with a large number of teeth made great speeds of rotation unnecessary. Besides, Fizeau only compared in his experiment the illumination of the field of vision at various speeds of rotation of the wheel. The only thing to be measured was the speed of rotation, and it was not difficult at speeds of several dozens of revolutions per second. There was much more trouble in the adjustment of the system, whose parts were at a considerable distance from each other. However, this laborious operation was to be carried out only once. Still, we should not regard Fizeau's experiment as simple and easy: just imagine, the scientist had to make use of a kerosene lamp as the source of light!

On completing these experiments, of which Fizeau reported in 1849, he set about Arago's problem. By this time Foucault had already advanced very much in its solution. Both physicists showed their scientific honesty and integrity in staging the experiments.

The situation was not so easy from the moral viewpoint. The use of someone other's ideas is always fraught with unpleasant priority fights. By the way, Arago had already once been made to protect his priority in the problem of measuring the velocity of light in 'terrestrial' conditions. When Wheatstone had become familiar with Arago's paper of 1838, he wrote to Arago that a similar method to reveal the validity of one of the two theories of light had been suggested in 1835 by the well-known English astronomer John Herschel. And although Herschel's suggestion had been a far cry from Arago's in its analysis of the problem, Arago reported this letter to the Academy. He also reported a letter from the prominent German astronomer and mathematician Friedrich Wilhelm Bessel. In his letter Bessel considered possible improvements of the experiment for comparing the velocities of light in different media. The last lines in Bessel's letter are remarkable:

> "Although it seems to me that my method is simpler, it is still a modification of your experiment, and I did not try to carry it out. The idea of the experiment is yours, and you proved its possibility; the result it will yield will also belong to you."

Arago wrote about the way Fizeau and Foucault behaved in the situation:

> "The condition of the problem was poor (i.e., nobody tackled the experiment.—*Author.*) when M. Fizeau determined the velocity of light in the atmosphere in his *ingenious experiment. This experiment* was not described in my memoir, and M. Fizeau had every right to conduct it without ever fearing to be reproached in dishonesty. As to the experiment on comparing the velocities of light in a liquid and the air, M. Fizeau wrote to me: 'I haven't conducted a single experiment in this field, and I am not going to until I get your formal invitation.' "This reserved loyalty only made my opinion of M. Fizeau's work and character still better, and so I hurried to allow M. Bréguet to lend one or several of my rotating mirrors to M. Fizeau.
>
> "M. Foucault, of whose inventiveness the Academy is aware, came to me himself to inform me of his wish to verify experimentally a modification he wanted to introduce into my equipment. Because of the con-

dition of my vision, I could only provide my recommendations to the experimenters who desired to follow my ideas..."

Therefore, Arago gave his blessing to the young physicists for carrying out the experiment he

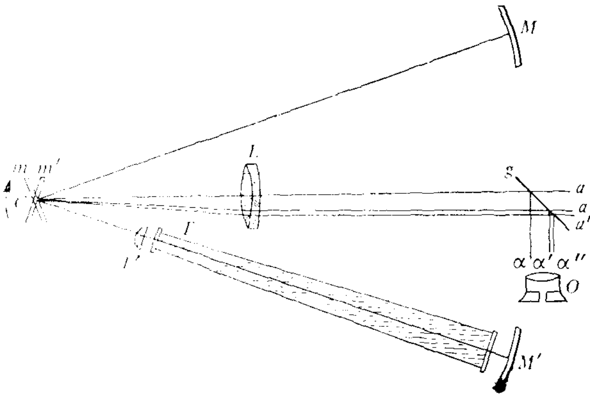


Fig. 13. The scheme of Foucault's experiment. *a* is a beam of light from the source, *L* and *L'* are lenses, *g* is a semitransparent p ate, *m* (*m'*) is a flat rotating mirror, *M* and *M'* are concave mirrors with the centre of curvature at point *C*, *T* is a tube with water, and *O* is a telescope.

invented. However, both Foucault and Fizeau (the latter working in collaboration with Bréguet) introduced essential modifications into Arago's scheme. Foucault's apparatus was especially original.

Foucault's scheme is shown in Fig. 13. Light from a source *a* passes through a semitransparent plate *g* and a collecting lens *L* and falls on a flat mirror, which can rotate about the vertical axis. The mirror in position *m* reflects the light to a fixed concave mirror *M* with the radius of curvature *R*. The optical centre *C* of the mirror *M* is in the rotation axis of the flat mirror. Suppose the flat mirror does not turn. Then the light reflected from the mirror *M* returns to the flat

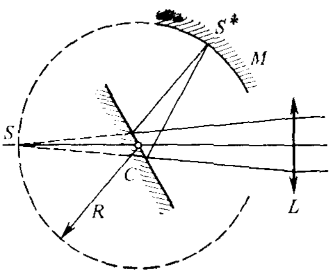


Fig. 14. A diagram showing the formation of the source image in Foucault's apparatus.

mirror and is reflected again to the lens *L* and passes to the plate *g*. It is partially reflected from it in a direction α to a telescope *O* used for the observations. The lens *L* is located so that it forms converging rays, which would produce, if there were no flat mirror, an image of the source at a point *S* at a distance *R* from the point *C* (Fig. 14). Such a position of the mirrors and the lens results in that the reflection of the imaginary source *S* formed by the flat mirror (the points S^* in Fig. 14) describes, when the mirror rotates, a circumference whose radius equals the radius of curvature of the mirror *M*. Inasmuch as the optical axis of the mirror *M* is at an acute angle with the optical axis of the lens, the whole light sent by the flat mirror to the mirror M is reflected and again arrives at the flat mirror. After two reflections from the flat mirror and one from the concave mirror, the image of the source is formed at the point S irrespective of the orientation of the flat mirror.

The same considerations hold true for the case when the flat mirror is in a position m' and reflects the light to a fixed mirror M'. There is a tube filled with water in the path of the light from the flat mirror to the mirror M'. To compensate a shift of the image of the source appearing due to the passage of the light through water, a lens L' is used. Consequently, the position of the image of the source visually determined through the telescope O depends neither on the orientation of the plane of the flat mirror nor on which of the concave mirrors reflected the light.

Now imagine that the flat mirror rotates. Because during the time it takes the light to travel from the flat mirror to M (or M') and back the flat mirror turns over an angle, the image of the source after the three reflections will not coincide with S. The shift of the image after the reflection from the mirror M will be in the same direction as the shift of the image produced by the light reflected from M'. However, because of the difference between the velocities of light in water and air, the shifts of the images observed through the telescope will be different (α' and α'' in Fig. 13). To decide where the velocity of light is greater, suffice it to determine which image shifts more: if a shift is greater, it takes the light a greater time to travel between the two reflections from the flat mirror, and therefore the respective velocity of light is less. Inasmuch as the image formed by the light reflected from the mirror M' shifted more, Foucault arrived at the conclusion on the validity of the wave concept of light.

What is the advantage of Foucault's scheme over Arago's? In his doctoral thesis Foucault formulated the basic principle of the experiment as "the production of a fixed image of a rapidly moving image." The double reflection from the flat mirror and clever positioning of the concave mirrors allowed Foucault to observe the images which changed rapidly but appeared at the same place and were perceived, due to visual inertia, as fixed and stable. In addition, the use of the concave mirrors produced a light-gathering effect, i.e., increased the brightness of the image.

Some technical details of Foucault's installation are interesting. The object, whose image was observed through the ocular, was either a single thin platinum wire or a system of such wires at a definite distance from each other. The system of wires, combined with a micrometric scale of the telescope O, defined a kind of a vernier, an adjustment for a more accurate reading of shifts, like the one applied, for instance, in calipers. In order to distinguish better between the images produced by the reflection from the two concave mirrors, Foucault applied a green filter for the light passing through the tube with water (the green portion of the light spectrum is less absorbed by water) and invented special 'masks' for the mirrors M and M' such that the vertical dimensions of the wire images were different. For the source of light, Foucault used a heliostat, an instrument focussing sunlight and driven by a clock mechanism orienting the heliostat mirror so that the image of the Sun, regardless of its motion, appears at the same point.

Water was contained in a 3 m long lead pipe sealed by flat glass plates at both ends. Foucault was apprehensive that the centripetal forces would destroy Bréguet's system of gear wheels, and therefore he designed an original apparatus communicating rotation to a light round mirror

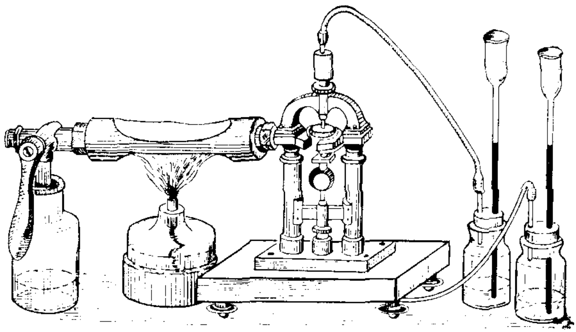


Fig. 15. A general view of Foucault's apparatus.

14 mm in diameter, where the number of the moving parts was as small as possible (Fig. 15). The mirror was mounted on the axle of a small steam turbine, similar to a siren widely applied at the time for acoustical experiments. Steam was produced by a miniature model of Watt's steam engine with an alcohol burner as the source of heat.

The turbine could rotate at about 800 rps, but Foucault made his observations at a speed of some 500 rps. The speed was measured stroboscopically, that is, a gear wheel of a clock mechanism, whose pace was known, was lighted by reflections from the flat mirror; if the wheel seemed to be fixed, then the speed of rotation of the mirror was an integer number times greater than the speed of the wheel.

Foucault's first experiments were purely qualitative, but, given some alterations, his installation allowed him also to perform absolute measurements, which were more accurate than Fizeau's. Foucault spent twelve years to perfect his experiment and measure the velocity of light in air, but the value of the velocity obtained by Foucault was much closer to the one accepted nowadays than Fizeau's result. Foucault arrived at the value of $c = 298{,}000 \pm 500$ km/s. The light in Foucault's 1862 apparatus was reflected from several concave mirrors, and thus the path between the two reflections from the flat mirror was 20 m. The relatively high accuracy in his experiment was determined by that the mirror rotated with a constant speed, and the measuring process was not related to subjective impressions as in Fizeau's method.

The issue of *Comptes rendus*, the journal of the Paris Academy of Sciences, which carried Foucault's memoir, also contained a report on the experiment prepared by Fizeau and Bréguet. No observations had been performed by the moment of publication, but later Fizeau and Bréguet supported Foucault's conclusions, and therefore the wave theory of light passed the examination in the crucial experiments.

After his successful experiments of 1850, Foucault gave up optics for a time. In the next year he conducted his famous experiment with a pendulum which proved the rotation of the Earth on its axis. He was awarded the cross of the Honoured Legion for this fine experiment. In 1853 Foucault was given a position in the Paris Observatory. His work was widely recognized The Royal Society of London awarded him a Copley medal. Foucault was elected a foreign member of the Petersburg and Berlin Academies of Sciences and a member of the French Bureau des Longitudes and became an officier de la Legion d'honneur. In 1865 he was elected a member of the Paris Academy of Sciences to fill a vacancy when the physicist Clapeyron died.

Foucault did not rest on his laurels but diligently continued his work in almost every field of physics. In mechanics he invented a gyroscope and mechanical *governors* which were far better than Watt's governors. In electricity Foucault studied electrical conduction in liquids and the induction currents induced in massive conductors in an alternating magnetic field; later these currents were called Foucault currents. Foucault continued his research in optics. He succeeded in developing a new design of photometer. He enriched astronomical optics with the idea of using metallized glass to produce mirrors; the idea essentially simplified the production of mirrors and improved their quality. Foucault invented a method for checking the quality of lenses and mirrors. Foucault's interest in astronomical optics was related to his research in astrophysics.

Evaluating Foucault's achievements on the whole, we can say that he was an outstanding master of fine physical experiments. Unfortunately, Foucault rarely used mathematics in his work, and this made some people regard it with haughtiness: they considered him a 'lucky tinker' rather than a scientist. This attitude was seen in that the well-known French astronomer Urbain Leverrier interfered with Foucault's astrophysical work in the Paris Observatory. We can only regret this injustice because Foucault devoted his whole life, which was rather short, to science. He died in 1868 because of a cerebral disease.

Fizeau lived much longer than his colleague and rival. He was elected to the Paris Academy of Sciences, and the Royal Society of London awarded him a Rumford medal and elected him its foreign member.

Before Fizeau conducted his first experiments for measuring the velocity of light, he had published a memoir where he had considered the propagation of light from a moving source. Independently from the Austrian physicist Christian Doppler, Fizeau arrived at the conclusion that the frequency of light registered by an observer is a function of the light source's velocity. Fizeau's work exceeded that of Doppler, and although Fizeau's memoir was published six years later (in 1848), the change in the observed frequency of light and sound due to relative motion of the source and the observer is now often called the Doppler-Fizeau effect.

In 1849 Fizeau and the engineer E. Gounelle endeavoured to apply the interruption technique to measure the speed of propagation of electrical disturbances. They used in their apparatus a wheel composed of alternate conducting and insulating elements. However, the researchers did not succeed in obtaining reliable, unambiguous results.

Following the experiments conducted in 1850 jointly with Bréguet, Fizeau became interested

7–0541

in the determination of the velocity of light in moving bodies. He obtained fundamental results in the field, and I shall tell about them later.

At the end of his life, Fizeau was still engaged in optical problems; for one, he researched into polarization of light. He also gave much attention to the study of thermal expansion exhibited by physical bodies.

Here is an excerpt from an article by the permanent secretary of the Paris Academy of Sciences Émile Picard (1856-1941) devoted to Fizeau's life and work:

> "Those, of whom there are only a few now, who knew him during his last years, can remember a venerable old man with a shock of hair and a thick beard, whose behaviour was impressive though rather cold. The interests of science only made him forget his habitual reserve; although Fizeau did not like arguments, he became an adversary not to be disregarded whenever there was a discussion. I remember a session of a committee of our Academy when Fizeau, considering the merits of two candidates, was speaking on the findings of one and the discoveries of the other; his sympathy was not with the author of the findings."

Fizeau was just in his strict attitudes. His own scientific activity was a chain of indefatigable researches and discoveries. He had every right to be proud of his contribution to science. Fizeau passed away in 1896, several days before his seventy-seventh birthday.

* * *

Fizeau and Foucault made a very essential step in the exact determination of the velocity of light: *they proved that it is possible in principle to measure it in 'terrestrial' conditions*. However, neither the method of a toothed wheel nor the method of a rotating mirror were advanced to perfection by their authors. On making the first step, they became engaged in other problems. In the meanwhile, the importance of measuring the velocity of light as accurately as possible was evident, and therefore both methods were developed later by other scientists.

The method of a toothed wheel was improved by the French physicist Marie Alfred Cornu (1841-1902). The main drawback in Fizeau's method was that the observer, who regulated smoothly the speed of rotation of the wheel, had to catch a moment of darkening, stop changing the speed of rotation, and measure it. The moments when the darkening was observed and the speed was measured were different, and this was a serious source of error. To eliminate it, Cornu designed a system of automatic registration in order to fix the change in the speed of rotation and later determine the speed at any given moment.

Cornu's interest in finding a more accurate value of c was not purely academic. His work was stimulated by the Council of the Paris Observatory hoping to achieve an 0.1% accuracy of measurement. The French astronomers intended to use the more accurate value of the velocity of light while processing the data from the observation of the transit of Venus over the disk of the Sun on December 9, 1874.

During a number of years, Cornu carried out several hundreds of measurements, but they did not yield the desired goal: at the mean value of $c = 298{,}500$ km/s, the spread of the obtained results was about 5%. It was noticed that the values of c were above the mean if they were determined at low speeds of wheel rotation. This indicated a systematic error. Cornu supposed that the inertia of visual perception might bring about an incorrect determination of the moment when the field of vision was completely dark.

The British physicists J. Young and James Forbes decided to overcome this difficulty, and I shall tell about their experiment in detail in Chapter 5.

Foucault's method was successfully improved by the outstanding American physicist Albert Michelson, whose work will also be discussed in detail.

In 1856, quite unexpectedly, experimenters came across a value with the dimension of speed and very close to c in quantity. At first glance, this speed had nothing to do with optics because it was obtained in the process of electromechanical measurements. What was it, an accidental occurrence or a corollary of some general laws of nature? The answer to this question was provided by the theory of the electromagnetic field developed by the British physicist James Maxwell.

Chapter 4

The Enigmatic Constant

Every child learns about the electromagnetic nature of light waves in school nowadays. However, there was a long period in the history of science when electrical, magnetic, and optical phenomena were considered to belong to different domains in physics. The progress of optics was far more advanced than that of the teaching on electricity: the theory of light was firmly established and regarded as almost entirely completed part of science in the early 19th century, following the work of Thomas Young, Augustin Fresnel, and other physicists developing optics. As to electricity and magnetism, this was a period of intense accumulation of experimental facts which were to be generalized within a common theory yet.

Scientists pondered over possible relationship between optical and electromagnetic phenomena since the mid-18th century. The appearance of a spark in an electrical discharge seems to have been the first indication of such a relationship. The guess was confirmed by Benjamin Franklin (1706-1790), who proved the electrical nature of a lightning in 1747. The great Russian scientist Mikhail Lomonosov (1711-1765), who was engaged in both electrical and optical research, intended to check whether light would be "refracted differently in electrified glass or water." He remarked in his *Chemical and Optical Notes*: "Look for the electrical force in the focus of a lens or concave mirror." It is not, however, known whether Lomonosov attempted to carry out such experiments. The English physicist and physician Thomas Young (1773-1829) also indicated a possible relationship between optical and electrical phenomena.

A discovery made by the outstanding English physicist Michael Faraday (1791-1867) became the most essential stage in the unification of optical and electrical phenomena. In 1846 he observed a magnetooptical effect, which consists in the rotation of polarization of a beam of linearly polarized light when it passes through matter in the direction of an applied magnetic field (the Faraday effect, or Faraday rotation). Light waves, like the waves in an elastic rubber string, are transverse. In 1816 Fresnel and Arago had conducted special experiments and showed that the oscillations in the light waves occur in the direction perpendicular to the direction of the waves' propagation. Faraday established that when light passes through matter placed in a magnetic field, the plane of light oscillations changes its orientation, and the angle of rotation of the plane depends on the external magnetic field's strength.

The consideration of this phenomenon brought Faraday to the idea of possible existence of electromagnetic waves and their relation to light waves. This idea was far ahead of the science of the time, and, because of a number of reasons, Faraday's views had not gained recognition before Maxwell's theory of electromagnetic field was published.

By the time Maxwell began his research into electromagnetism, physicists had been aware of the experimental fact which should have attracted everyone's attention to the problem of relationship between optical and electrical phenomena. It was found that the ratio of a charge expressed in the electrostatic system of units (ESU) to the same charge in the electromagnetic system of units (EMU), is a constant (the *electromagnetic constant*) whose dimension is that of speed and whose value is close to the velocity of light. However, most scientists believed this fact to be an obscure curiosity.

It is easy to understand this attitude to the electromagnetic constant. Indeed, the selection of a system of units is optional, and therefore any comparison between the units in different systems is but of practical significance. Was it worth posing any problems of principle in this connection?

The idea of arbitrary selection of a system of units is in keeping with what is called 'common sense'. However, as it often happened in the history of science, further developments showed that this 'natural' viewpoint had been rather narrow-minded.

The story of the electromagnetic constant began in the late 18th century. By that time the scientists knew next to nothing about the relationship between magnetic and electrical phenomena, and these phenomena were studied independently. The most essential step in the evolution of the teaching on electricity was made by Charles Augustin de Coulomb (1736-1806), who discovered in 1785 the law of interaction between point charges (known as the Coulomb's law). However, scientists could not for a long time succeed in deducing a sufficiently general law of magnetic interaction. It was only when in 1820 Hans Christian Oersted discovered the magnetic effect of an electric current and André Marie Ampère established the law of interaction between currents, that physicists were able to proceed from a reliable basis while conducting quantitative measurements in magnetism. Further investigations confirmed the close relationship between magnetic and electrical phenomena. A special role in the establishment of this connection was played by Michael Faraday's 1831 discovery of the effect of electromagnetic induction.

The many years of 'independent' studies of magnetic and electrical phenomena brought about essential consequences in physics: two systems of units appeared, magnetic and electrical. The emergence of the two systems had been predetermined by the general approach of physicists to the problem of measurements, or, in other words, to metrology. This approach consists in that whenever a new, earlier unknown property, is found out (for instance, the ability of charged bodies to attract or repel each other), physicists strive to introduce quantitative characteristics in order to describe this property in a *most convenient manner. A prerequisite is that* the new characteristics must be expressed through the earlier introduced physical quantities. Let me elucidate this idea with the example of the determination of the unit measuring an electric charge.

Using an instrument called torsion balance, Coulomb proved that the force of interaction between two electric charges is inversely proportional to the square of the distance between them. Note that the same conclusion supported by experiments had been reached by the English physicist and chemist Henry Cavendish a long time before Coulomb. However, the technique used by Cavendish did not allow him to determine the dependence of the force of interaction on the magnitude of the electric charges, or, as it had been spoken in the 18th century, 'the magnitude of electric fluid.'

There was a curious situation in the problem of finding 'the law of electric attraction and repulsion'. The dependence of the force of interaction between charged bodies on the magnitudes of the charges communicated to them can be established, as it seems, only knowing the magnitudes of the charges. However, the charges may only be determined due to the interaction force between them. So this is a vicious circle. At first glance, the situation is hopeless, but there still is an escape. It consists in a correct organization of the measuring procedure. For instance, it could be carried out as follows.

A torsion balance is an instrument, consisting essentially of a straight vertical torsion wire whose upper end is fixed while a horizontal beam is suspended from the lower end. Two identical balls are mounted on the ends of the horizontal beam, and there is another ball fixed within the box of the torsion balance on the same level. Unknown charges q_1 and q_2 are imparted to the stationary ball and to one of the balls on the beam, respectively. Then the stationary ball with the charge q_1 is touched by an identical ball carrying no charge, following which the charge is equally distributed between the two balls. (This can be expected proceeding from the symmetry, but the equality of the charges can also be checked in experiment.) Now the force of interaction between the balls with charges $q_1/2$ and q_2 is measured. The procedure can be repeated in the same vein, and if each force is a half of the previous force, then it can be deduced whether the interaction forces are proportional without knowing the absolute values of the charges.

After Coulomb established the law, it became evident how to determine the unit of quantity of electricity (the unit charge), i.e., how to express it through the known quantities. The most advanced branch of physics in the 18th century was mechanics, where the basic units are those of time, length, and mass. The unit force is determined through them. Using Coulomb's law

$$F = K \frac{q_1 q_2}{r^2}, \qquad (4.1)$$

we define a unit charge as the charge that interacts with a unit force with an equal charge at a unit distance away. Depending on the magnitude of the factor K, different unit charges may appear. Thus, for instance, the CGSE system has $K = 1$, while the modern international system of units (SI) has $K = 9 \times 10^9$, and therefore the relationship between the unit charges in the two systems is 9×10^9 (CGSE charge units) $=$ $=$ 1 coulomb.

To introduce a unit charge, one can make use of not only the effect of electrostatic interaction, but also any other phenomenon influenced by the magnitude of a charge. Because electric current is the ordered motion of charged particles, the magnitude of charge can be determined considering a phenomenon related to electric current. In the last analysis, the selected phenomenon will define the convenience and accuracy of the measurements.

Following Oersted's discovery, the measurement of the interaction between two conductors carrying electric current made it possible to introduce a unit current intensity (also known as current strength). However, an independent introduction of a unit current intensity and a unit charge does not make sense from the viewpoint of physics. I should add, however, one more word to the expression 'from the viewpoint of physics' and say 'modern physics'. The point is that the identity of the two kinds of electricity, static and 'galvanic'*, was not readily understood. Therefore there is a historical explanation to the fact that the two closely related units of measurement were introduced into physics independently. Once the physicists found out that the situation is inconvenient, they tried to find a relationship between the unit charge determined on the basis of measuring the magnetic effect of current and the one obtained using electrostatic measurements.

We have come here to a point where I can try and answer the question that is obviously lurking in the mind of the reader: what has this particular problem of the relationship between the two unit charges to do with the problem of measuring the velocity of light?

---

* I.e., pertaining to electricity flowing as a result of chemical reactions (from the name of the Italian physiologist Luigi Galvani (1737-1798), who was the first to observe such phenomena).

Let us determine the dimensions of the ratio Q/Q', where Q is the 'electrostatic' unit charge, and Q' is the unit charge introduced through magnetic measurements.

Any unit physical quantity is either fundamental or derived. For instance, the SI uses length, time, mass, and some others as fundamental quantities. The unit velocity in this system is a derived one and can be expressed through the fundamental units. The concept of dimensions of a physical quantity is closely related to the concept of units of measurement, but there still is a difference. Without considering the general definition of dimensions, let us discuss their properties by way of examples.

Given there is a system with the fundamental units being those of length (let us designate L every quantity with the dimension of length), time (T), and mass (M). The velocity $\mathbf{v}$ of a moving material point is defined by the relationship $\mathbf{v} = \Delta\mathbf{s}/\Delta t$, where Δt is the time the body takes to cover the distance $\Delta\mathbf{s}$. Evidently, the modulus $|\Delta\mathbf{s}|$ of the distance has the dimensions of velocity:

$$[V] = LT^{-1} \tag{4.2}$$

It may seem that what has just been said above is only a sham science exposure of the well-known fact that velocity (speed) is measured in centimetres per second, metres per second, or kilometres per hour, etc. However, the introduction of the concept of dimensions is actually not at all useless. Relationship (4.2) reflects a very general fact consisting in that if the fundamental units of measurement in a certain system include a unit length and a unit time, then the dimensions of velocity will always be defined by relationship (4.2), and it is inessential whether length is measured in kilometres or nanometres and time in hours or days: the dimensions of velocity, unlike its unit of measurement, are going to be the same.

Another important property of dimensions consists in that the dimensions of a physical quantity whose unit of measurement is derived may depend on the particular relationship chosen to define them. Let me show it by the example of introducing the dimensions of force. Using the above-mentioned system of units, force may be defined proceeding from the second Newton's law:

$$\mathbf{F} = m\mathbf{a}, \quad \text{i.e.,} \quad [F] = MLT^{-2},$$

where $[F]$ is the dimensions of force. However, the selection of the second Newton's law to define $[F]$ is quite arbitrary. We may just as well take for it, for instance, Newton's law of gravitation in the form

$$F = \frac{m_1 m_2}{r^2},$$

and then it will be

$$[F] = M^2 L^{-2}!$$

Why so? Isn't it absurd that a physical quantity can have different dimensions within the same system of units?

There is no logical contradiction whatsoever in our considerations. The possibility to define the dimensions of a physical quantity in several varieties is a corollary of profound reasons, so we just have no chance to consider them here in detail. (Let me note, however, that as far as force is concerned, the most important role in the example is played by the equivalence of inertial and gravitational mass.) Inasmuch as this situation is not quite convenient, physicists decided to introduce the dimensions of derived quantities through certain relationships. Thus, if the dimensions of force are defined, the second Newton's law is preferred. This results in the appearance of a gravitational constant G, possessing dimensions, in the Newton's law of gravitation:

$$F = G\frac{m_1 m_2}{r^2};$$

this is the only way to reach the equality of the dimensions of force in Newton's law of gravitation and the second Newton's law. Let us define the dimensions of the gravitational constant. This can easily be achieved using the principle of uniformity of physical quantities, which asserts that a physical quantity participating in the expressions of different laws in physics must have the same dimensions. According to this principle,

$$[G]\, M^2 L^{-2} = M L T^{-2}, \tag{4.3}$$

i.e.,

$$[G] = M^{-1} L^3 T^{-2}.$$

Recall that, using the SI fundamental units of length, time, and mass, $G = 6.67 \times 10^{-11}\ \text{m}^3 \cdot \text{kg}^{-1} \cdot \text{s}^{-2}$.

Now let us return to the problem of determining the relationship between the two unit charges. Let us regard it from the viewpoint of dimensional analysis. Let the fundamental physical units be those of length, time, and mass as before. What are in this case the dimensions of the quantity of an electric charge, if Coulomb's law written in the form

$$F = \frac{q_1 q_2}{r^2}$$

is used as the defining relation? Evidently, to find the dimensions $[Q]$ of charge, one should use an equality similar to (4.3):

$$[F] = [Q]^2 L^{-2},$$

or, taking into account only the fundamental units,

$$MLT^{-2} = [Q]^2 L^{-2}.$$

Hence

$$[Q] = M^{1/2} L^{3/2} T^{-1}.$$

Another method to introduce a unit charge is to apply Ampère's law giving the interaction between two current conductors:

$$F' = \frac{i_1 i_2}{r}. \qquad (4.4)$$

A numerical factor determined from the geometry of the experiment has been omitted in formula (4.4). Here i_1 and i_2 are the current intensities in the interacting conductors and r is the distance between the conductors. The designation F' in the formula is the force acting per unit length of a conductor. Current intensity is known to be related to the quantity of charge passing through a conductor as $i = q/t$, and therefore $[I] = [Q'] T^{-1}$. Recall that Q' is a unit charge introduced through magnetic interaction. Now we can find $[Q']$:

$$MT^{-2} = [Q']^2 T^{-2}L^{-1}.$$

Hence

$$[Q'] = M^{1/2}L^{1/2}. \quad (4.5)$$

The fact that $[Q] \neq [Q']$ is a small wonder because different defining relationships were used to find them. Now find the dimensions of the proportion between the quantities of charge in the electrostatic system $[Q]$ and the electromagnetic system $[Q']$:

$$\frac{[Q]}{[Q']} = LT^{-1}.$$

Thus the sought relationship possesses the dimensions of velocity. The fact as it is can hardly attract any attention because fixed charges interact in case of electrostatic measurements and moving ones in case of magnetic measurements. Possibly, this accounts for the fact that this relationship has the dimensions of velocity. However, what is the absolute value of this ratio? The German scientist Wilhelm Eduard Weber was the first physicist who endeavoured to answer the posed question in experiment.

In the mid-19th century, Weber (1804-1891) was a salient figure in the world of science. He was born into the family of the professor of theology at Wittenberg University. The respect for science was inherent in the family of Weber, so three brothers Weber out of four became scientists, and Wilhelm was the most outstanding. When he was only thirteen years old, he took part in his elder brother's experiments purporting to study waves in water and the air. In 1822 Weber became a student of Halle University,

Wilhelm Weber (1804-1891)

where he was well-educated in physics and mathematics. After he graduated from the University, Weber started independent research. In 1828 the young scientist participated in the Congress of German Natural Scientists and Physicians in Berlin. His paper on organ pipes attracted the attention of Karl Friedrich Gauss (1777-1855), the 'king of mathematicians'. This was the time when Gauss, under the influence of the famous

German naturalist Alexander Humboldt, enthused over studying the Earth's magnetic field. To carry out their vast programme of research on geomagnetism, Gauss and Humboldt needed a skilful coworker, so Gauss invited Weber.

Gauss and Weber began their work together in 1831 when Weber received the chair of physics at Göttingen University, where Gauss was a professor as well. Their joint research continued for six years, which was an era in not only geophysical measurements but electromagnetic measurements in general.

Weber did not have the chance to discover a single new physical phenomenon in electromagnetism, by contrast to Faraday's research. However, unlike Faraday, whose experiments were mainly qualitative, Weber strove to introduce into electromagnetic research the accuracy and unambiguity inherent in mechanical measurements. The basic idea followed by both Weber and Gauss was to reduce magnetic and electrical measurements to mechanical ones and therefore express electromagnetic quantities through the units of time, length, and mass. Weber implemented this idea in the design of his sensitive magnetometers and other instruments. Weber's research in metrology was one of the reasons of his interest to the determination of the electromagnetic constant. On the whole, Weber's and Gauss's geomagnetic research advanced the technology of measurements to a qualitatively new level.

Weber's and Gauss's collaboration was interrupted because of the events having nothing to do with science. In 1837, following the death of King Wilhelm IV, the liberal constitution of the Hanover State, to which Göttingen be-

Rudolf Kohlrausch (1809-1858)

longed, was revoked. Weber was among the seven university professors who filed a petition of protest to the government. The scientists paid for this courageous action with their positions, and even the interference of such influential people as Humboldt and Gauss could not help.

Weber had to leave Göttingen. In 1843 he succeeded in receiving the chair of physics at Leipzig;

Weber carried out there his essential theoretical research in electricity and magnetism. The research was crowned by the law of 'electrical forces' suggested by Weber in 1846.

Weber's goal was to unify the electrostatic and magnetic interaction of charges in a single formula, as well as to describe the phenomenon of electromagnetic induction. The attempt to solve such a general problem resulted in that the expression for the Weber's law of interaction between electric charges proved to be rather uncommon: the formula for the force of interaction includes some members depending not only on the magnitude of the charges but also on their relative velocity and even acceleration! Weber's formula included the electromagnetic constant, and its determination was of considerable interest for the author of the theory.

Thus the determination of the electromagnetic constant became the point of intersection between the two seemingly contradicting Weber's passions: metrology and the theoretical research in electromagnetism. Weber set up the experiment together with his colleague Rudolf Kohlrausch (1809-1858). The scientists formulated their problem in the following manner:

> "Given: a constant current which flows through a single circle of radius R mm of a tangent galvanometer, which produces a deflection of the magnetic needle of $2\pi/R\tau$ in equilibrium, where τ represents the intensity of the horizontal components of the earth's magnetism which deflects the compass needle. To be determined: the relationship between the quantity of electricity which flows in the case of such a cur-

> rent in one second through the cross section and the quantity of electricity on each of two [infinitely] small equally charged spheres which repel each other from a distance of 1 mm with a unit force."

Despite the slightly enigmatic words in the beginning of the quotation, the idea is very simplem, viz., to compare the charge flowing through the cross section of a conductor during a unit time while the *current intensity* is a given value (the first sentence determines the current intensity: actually, the tangent galvanometer is a kind of ammeter, and the value

$$\varphi = \text{arc tan}\,\frac{2\pi}{R\tau}$$

is the deflection of its needle), with the charge whose magnitude is determined by the electrostatic interaction.

The experiment was conducted in the following manner. First, it was necessary to determine the relative capacitance of a small Leyden jar (a capacitor) and a ball covered by tin foil, and thus the distribution of the charge between the jar and the ball, established when one of foil sheets of the jar was connected to the ball, became known.

Prior to experiment, the Leyden jar was to be completely discharged. Then a large charged ball was connected for a brief time with a foil sheet of the jar. The charge from the jar was passed through a multiplier (winding) with 5635 turns of wire. The current flowing through the winding caused a deflection of the magnetic needle. This deflection was registered as accurately as possible. In the meanwhile the large ball, which retained a part of its charge, was made to touch the fixed ball in the box of a torsion balance (like the one used by Coulomb and Cavendish). Because the sizes of the balls were known, it was possible to determine what proportion of the charge was transferred from the large ball to the fixed ball of the torsion balance. Then the large ball was put into the box of the torsion balance and gave a half of its remaining charge to a moving ball of the same size, and the turn of the torsion balance wire was measured. This measurement made it possible to determine in electrostatic units the absolute magnitude of the charge the large ball originally possessed. The deflection of the magnetic needle in the first part of the experiment gave the same magnitude in magnetic units.

If we try and reduce Weber and Kohlrausch's results to modern units of measurement, the average value of the sought proportion will prove to be 310,740 km/s.

One would think that the obtained result was bound to make Weber and Kohlrausch ponder about the proximity of the electromagnetic constant to the velocity of light. Actually, in 1846 Weber wrote a paper in which he formulated a law concerning the interaction between electric charges:

> "...I only have to remind of the latest Faraday's discovery of the effect of electric current on the propagation of light oscillations* which does not make improb-

---

* The point is the rotation of polarization of a linearly polarized light beam when it passes through matter in the direction of an applied magnetic field (see p. 102).

> able the surmise that the ubiquitous neutral electric medium is indeed the omnipresent ether creating and transferring light oscillations or, at least, that the two of them are so closely related to each other that observations of the propagation of light may provide an explanation to the action of the neutral electric medium."

The words testify that Weber guessed the electromagnetic nature of light. However, one should state that neither Weber nor Kohlrausch drew any conclusions with respect to the result they obtained. Possibly, they were too sure of Weber's theory.

This particular theory was a further development of Ampère's electrodynamics, and it retained Ampère's basic idea, that of the long-range action. In every theory based on the principle of long-range action, the force of interaction between physical bodies, be it gravitational attraction in Newton's law of gravitation or Coulomb's force of attraction or repulsion between two charges, depends only on the distance between them. The principle implies that when this distance changes, the force of interaction changes instantaneously however far the bodies might happen to be. Although the long-range interaction principle entails certain poignant methodological problems of gnoseology, i.e., the problems related to the methods of cognition, the theories under discussion are quite satisfactory in their explanation of physical phenomena when the interacting bodies are either static or their relative velocity is small enough. The pertinent point is the validity of a criterion for the determination of how small or great the velocity may be. What was there to compare it with? At present, we know the answer very well, but there was no such criterion in Weber's time, and most scientists regarded the long-range interaction to be an absolute physical principle. This seems to have been the reason for Weber's 'indifference' to his own results: there was no place for any fundamental velocities within the long-range interaction pattern of thinking: any interaction was considered to propagate instantaneously!

Consequently, the result obtained by Weber and Kohlrausch had to wait for correct explanation. An accurate interpretation of this result is only possible when there is a clear understanding of the mechanism of interaction transmission. Many physicists of the mid-19th century appear to have been aware of this requirement, and although formally the theory of long-range interacting Weber's forces did not have any serious opposition, there was an undercurrent of the idea of short-range interaction whose most important corollary is the finite velocity of interaction propagation.

However, the only thing needed for a thorough review of the currently prevailing outlook was a fundamentally new idea. Such an idea emerged in Faraday's work, who supposed that there is a special physical entity, which he called electromagnetic field.

The story of Faraday's idea of electromagnetic field is one of the most appealing ones in the history of science. I am bound to limit myself with only a brief account of how the great English scientist evolved the idea of field.

The most salient feature of Faraday's work was his amazing ability to combine fine experimental analysis with skilful generalization of the results and derivation of sound ideas without any mathematical methods. One of the first products of this pattern of Faraday's work was his surmise on the existence of the lines of force spreading continuously from one charged body to another. The very idea of the lines of force had been suggested prior to Faraday, but he was the first to consider them as the true carriers of physical interactions. According to Faraday, every physical body is surrounded by lines of force (electrical, magnetic, etc.); the effect of a charged body's presence cannot be reduced to the appearance of the interaction force with another charged body because it also modifies the physical properties of the space around. Faraday had to think long before he was able to offer this concept, but he was rewarded proliferously for the fruits of its consequences. One of the consequences consisted in that light waves can be produced by oscillations of the lines of force.

The world of science got acquainted with Faraday's point of view on the nature of light in 1846, when Faraday made a presentation filling the time left after Wheatstone's paper was presented; some time later Faraday's ideas became known from a published letter to his friend and coworker Phillips. Here is an excerpt from this letter:

> "Whatever the view adopted respecting them [the lines of force] may be, we can, at all events, affect these lines of force in a manner which may be conceived as partaking of the nature of a shake or lateral vibration. For suppose two bodies, *A* and *B*, distant from each other and under mu-

tual action, and therefore connected by lines of force, and let us fix our attention upon one resultant of force having an invariable direction as regards space; if one of the bodies move in the least degree right or left, or if its power be shifted for a moment within the mass (neither of these cases being difficult to realize if *A* and *B* be either electric or magnetic bodies), then an effect equivalent to a lateral disturbance will take place in the resultant upon which we are fixing our attention; for, either it will increase in force whilst the neighbouring resultants are diminishing, or it will fall in force as they are increasing...

"The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together."

However, physicists did not properly respond to the idea of the origin of radiation illustrated by the excerpt quoted above. The reasons for such an indifferent attitude to the outstanding ideas of this English scientist were partially related to his method of research. The adherents of long-range interaction were able to produce superficially more formidable arguments than could Faraday. All the mathematics then developed supported the long-range interaction theory whereas Faraday could only point to interesting analogies. This is why scientists at the time deprecated Faraday's theoretical opinions even though they had great respect for his genius for experiment. Before Faraday's ideas could be spread, it was necessary to interpret them

James Clerk Maxwell (1831-1879)

in mathematical terms. This tremendous task was carried out by James Clerk Maxwell.

Maxwell was born on June 13, 1831, in Edinburgh into the family of a lawyer who was enthusiastically interested in the problems of science and engineering. From his early childhood, the boy revealed his observant and inquisitive mind. Maxwell wrote his first scientific paper when he was only fourteen years old. The future founder of the electromagnetic theory studied first at an Edinburgh academy (secondary school) and then at Edinburgh University. Some three years after Maxwell began to study at Edinburgh University he went over to Cambridge University, famous for its traditionally good lecturers in physics and mathematics. Maxwell was one of the best among the students who graduated the same year and began to give lectures at Trinity College in Cambridge.

Maxwell moved rather often from one place to another, changing his positions and duties. Following Trinity College, he taught at Marischal College in the Scottish city of Aberdeen and then at King's College in London. From 1865 to 1871 Maxwell lived alone in his family estate in Scotland and had no official position. The last eight years of his life he occupied the chair of experimental physics at Cambridge University, the first such chair in its history. The history of science is obliged to Maxwell for the foundation of an outstanding centre for physical research, the Cavendish Laboratory.

During his entire life, Maxwell was always steadily working over the most essential problems in physics. He died in 1879 (and thus lived for only 48 years), leaving a scientific heritage that is unique in its diversity and in-depth analysis. He had conducted advanced research in the theory of colour vision and was one of the founders of thermodynamics and statistical physics. He investigated the stability of Saturn's rings and showed that they are neither rigid nor fluid but consist of meteorites. However, his foremost achievement was the development of the theory of electromagnetic field.

To create a unified theory of electromagnetism, a scientist should combine the sharp intuition typical of outstanding experimenters with the mathematical talent usually inherent in a theorist. Maxwell possessed these complementary qualities masterfully. Faraday appears to have understood this because he once wrote to the twenty-six-year-old Maxwell:

> "...There is one thing I would be glad to ask you. When a mathematician engaged in investigating physical actions and results has arrived at his own conclusions, may they not be expressed in common language as fully, clearly, and definitely as in mathematical formulae? If so, would it not be a great boon to such as we to express them so—translating them out of their hierogliphics that we also might work upon them by experiment. I think it must be so, because I have always found that you could convey to me a perfectly clear idea of your conclusions, which, though they may give me no full understanding of the steps of your process, gave me the results neither above nor below the truth, and so clear in character that I can think and work from them."

Faraday wrote this letter after the publication of Maxwell's paper *On Faraday's lines of force*, where Maxwell was able to put some of Faraday's ideas into the terms of mathematics. Maxwell persistently worked on his overall theory of electromagnetic phenomena in 1860-1865. This theory proceeded from the concept of a field, a new physical entity whose introduction made it possible to reject the long-range interaction forces. In compliance with Maxwell's theory, the interaction between physical bodies occurs due to a field surrounding charges and currents; a change in the interaction (for instance owing to a shift of a charge) is only possible if there is a change in the field propagating from one point of space to another with a finite velocity. The most essential of Maxwell's conclusions is that of the existence of electromagnetic waves, which can propagate in space that is free of matter with the velocity equal to ... Weber's electromagnetic constant! Maxwell's theory both established the physical meaning of this constant and related the seemingly distant domains of optics and electromagnetism.

Maxwell's work presenting for the first time his theory in a sufficiently complete form was titled *A Dynamic Theory of the Electromagnetic Field*. It was published in 1864. Of the most importance for my story is part VI of this publication: *The Electromagnetic Theory of Light*. This is where Maxwell, proceeding from the fundamental equations of the theory, obtained an equation for an electromagnetic wave and found an expression for its velocity v*. Then he wrote:

* The notations in the following quotation are changed.

"This wave consists entirely of magnetic disturbances, the direction of magnetization being in the plane of the wave. No magnetic disturbance whose direction of magnetization is not in the plane of the wave can be propagated as a plane wave at all.

"Hence magnetic disturbances propagated through the electromagnetic field agree with light in this, that the disturbance at any point is transverse to the direction of propagation, and such waves may have all the properties of polarized light.

"The only medium in which experiments have been made to determine the value of K (K is a quantity proportional to the squared velocity of electromagnetic waves.— *Author*) is air... By the electromagnetic experiments of MM. Weber and Kohlrausch

$$v = 310{,}740{,}000 \text{ meters per second}$$

is the number of electrostatic units in one electromagnetic unit of electricity and this, according to our result, should be equal to the velocity of light in air or vacuum.

"The velocity of light in air, by M. Fizeau's experiments, is

$$V = 314{,}858{,}000;$$

according to the more accurate experiments of M. Foucault,

$$V = 298{,}000{,}000.$$

"The velocity of light in the space surrounding the earth, deduced from the coefficient of aberration and the received value of radius of the earth's orbit, is

$V = 308{,}000{,}000.$

"Hence the velocity of light deduced from experiments agrees sufficiently well with the value of v deduced from the only set of experiments we as yet possess. The value of v was determined by measuring the electromotive force with which a condenser of known capacity was charged, and then discharging the condenser through a galvanometer, so as to measure the quantity of electricity in it in electromagnetic measure. The only use made of light in the experiment was to see the instrument. The value of V found by M. Foucault was obtained by determining the angle through which a revolving mirror turned, while the light reflected from it went and returned along a measured course. No use whatever was made of electricity and magnetism.

"The agreement of the results seems to show that light and magnetism are affections of the same substance, and that light is an electromagnetic disturbance propagated through the field according to electromagnetic laws."

It is evident from this excerpt that Maxwell thought much of the determination of the electromagnetic constant. Although Maxwell was one of the greatest theorists in the history of the physical science, he did not shun experiments. Following the publication of *A Dynamic Theory of the Electromagnetic Field*, Maxwell decided to determine the number of electrostatic units in one electromagnetic unit of electricity by experiments. There was also a formal reason for Maxwell to set about tackling this purely experimental problem: he was a member of the British Association for the Advance of Science, which in 1862 concluded that it was necessary to compare the units in the two systems in order to set to rights the electrical units on the whole. To a certain extent, Maxwell could regard himself responsible for the implementation of this decision. Both reasons for Maxwell's interest to the problem were reflected in the title of his paper presenting the results of his work: *On a Method of Making a Direct Comparison of Electrostatic with Electromagnetic Force*; *with a Note on the Electromagnetic Theory of Light*. This paper was published in 1868 in the *Philosophical Transactions* of the Royal Society of London.

The idea of the experiment was to compare the force of electrostatic attraction between two charged disks (one of which was mounted at an end of the horizontal beam of a torsion balance, and the other was within the box of the balance and could only be moved by a micrometric screw) with the force of repulsion between two coils located near the disks and carrying current.

The scheme of the device designed by Maxwell is shown in Fig. 16. B_2 is a battery with a small electromotive power producing current y flowing through a mercury cup M, two identical coils A and A_1 mounted at the opposite ends of the balance beam, through a third coil (the disk fixed to the micrometer and the coil attached to the disk are indicated in the scheme by letter C), secondary coil G_2 of a galvanometer, and the double key K. The second, large battery B_1 consisting of 2600 elements produced current x

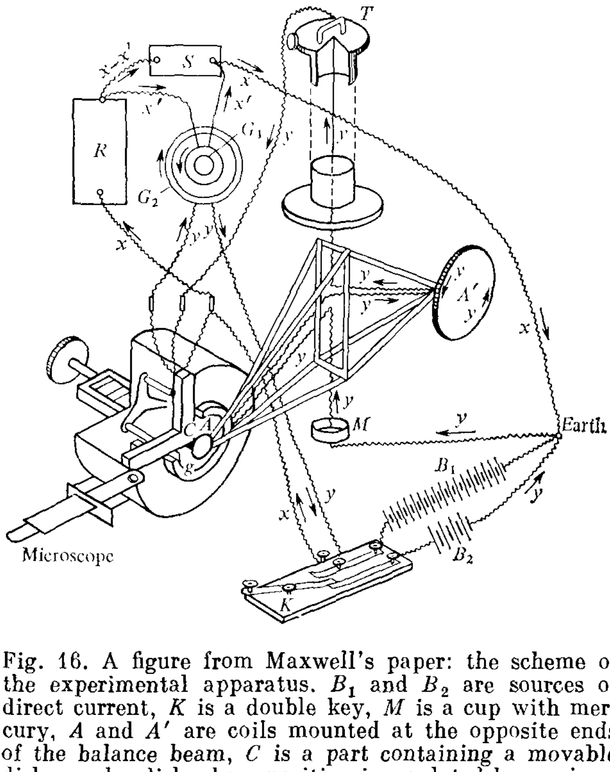


Fig. 16. A figure from Maxwell's paper: the scheme of the experimental apparatus. B_1 and B_2 are sources of direct current, K is a double key, M is a cup with mercury, A and A' are coils mounted at the opposite ends of the balance beam, C is a part containing a movable disk g and a disk whose position is regulated by a micrometric screw, R is a resistor, S is a shunt, G_1 and G_2 are galvanometer coils, T is the regulating screw of the torsion balance, and letters y, x, and x' indicate the currents in various parts of the apparatus.

flowing through the double key and a great resistance $R = 1\ M\Omega$. Then a part x' of the current flowed through the primary coil G_1 of the galvanometer while the remaining part $x - x'$ of the current flowed through a regulated shunt S. One of the poles of the battery, B_1, was connected with the micrometrically fixed disk C while the suspended disk with the attached coil C were connected with the opposite pole of the battery. The counterpoise disk and coil A' were used to balance weight and compensate for the influence of the Earth's magnetic field on the system. The disk and coil A were moved by means of the micrometric screw, and a microscope was used to register the shift. The beam could be turned by means of a tangent screw T fixed at the torsion head. Both the galvanometer and shunt were removed from the balance at a distance of about 3 m.

Maxwell conducted his experiments together with his assistant Hawkin. One of the observers regulated the distance between the attracting disks until their attraction was precisely compensated by the repulsion between the coils. In the equilibrium position, the wire to which the beam was suspended showed no torsion. At the same time the second observer regulated the shunt S so that the needle of the galvanometer would indicate equilibrium. These were the conditions when the forces produced by the currents flowing through the coils G_1 and G_2 cancelled each other. When the general equilibrium was attained, the first observer determined the distance between the disks.

The required proportion of the unit charges was determined in the following manner. Thanks to the use of the galvanometer with two coils and with the aid of additional measurements the experimenters were able to reach high accuracy in the determination of the intensities of the currents flowing through each element of the electrical circuit.

Because the interaction between the galvanometer coils was magnetic, the values of the current intensities were defined in the magnetic system of units. Knowing the values of currents and resistances, one could find the difference of potentials between the two disks. Then the magnitude of charge on the disks was determined, the disks defining a variable capacitor. This quantity was naturally expressed in the units of the magnetic system.

The force of repulsion between the coils carrying currents and attached to the disks could be calculated proceeding from the geometry of the coils and knowing the current intensities. In compliance with the design of the experiment, this force was equal to the electrostatic attraction between the disks. And now, knowing the geometry of the capacitor formed by the disks, it was easy to calculate the charge it stored. This calculation gave the magnitude of charge in the electrostatic units.

Consequently, proceeding from the independent (!) measurements, Maxwell succeeded in expressing the value of the charge in the same capacitor in the units of the two systems, and there was no trouble in finding the proportion between them.

A primary processing of the measurements have a value of v within 2.2×10^8 to 2.45×10^8 m/s. However, the evidently great discrepancy between this value and the velocity of light prompted Maxwell to look for some unaccounted errors. Both he and his assistant strained every effort in their search for possible sources of errors.

This resulted in that Maxwell's publication gave the value of $v = 2.88 \cdot 10^8$ m/s. As is evident from the result, Maxwell was unable to improve essentially the accuracy of measurements compared with Weber and Kohlrausch's experiment. He appears to have thought about other methods to measure v, as his extant notes suggest.

Following the measurements carried out by Weber and Kohlrausch, Maxwell, and other scientists, engaged in the determination of the electromagnetic constant, and especially after the elaboration of the theory of the electromagnetic field, the velocity of light expanded its 'sphere of influence'. In 1888 Heinrich Rudolf Hertz (1857-1894) proved experimentally the existence of electromagnetic waves. Consequently, the range of research related to the velocity of light widened: the investigators became interested in whether the velocity of electromagnetic wave with different frequency was the same. Now the optical problems concerning the velocity of light were also far from being completely solved. These are the problems I am going to deal with in the next chapter.

Chapter 5

The Many Velocities of Light

The year 1852 was remarkable for the story of the velocity of light although no fundamentally new experiments were set up then. In 1852 a boy was born whose name was later closely associated with the problem of the measurement of the velocity of light. The name of the boy was Albert Abraham Michelson, and he was born in Strzelno, in German-occupied Poland.

When Albert was barely two years old, his family moved to the United States of America. When he was sixteen, he graduated with honour from the San Francisco Boys' High School.

Poor financial circumstances of the proliferous family of Michelsons did not give much choice for Albert's further education. He passed an examination giving him the right to be chosen as a candidate to the Naval Academy from the State of Nevada, but another boy was appointed. However, he set off alone across the continent, riding one of the first trains of the transcontinental railway, with a letter of recommendation to the recently elected President Ulisses Grant. The boy was accepted at the White House, but President Grant had already filled his ten appointments-at-large. He boarded a train to San Francisco. Just as the train was about to leave, a messenger from the White House came aboard, calling out his name. Brushing regulations aside, Grant exceeded his quota and gave him the nomination for an eleventh appointment-at-large.

The Naval Academy in Annapolis was a good educational establishment, and the students were granted $ 500 a year. However, very little of the $ 500 remained after Michelson had paid for his food and lodging, laundry, and clothing, including his dress uniform.

Michelson was a good student, but not among the best. The management of the Academy especially regretted that he showed no interest in the subjects directly related to his future profession as a sailor and preferred the theoretical subjects: mathematics, optics, and acoustics. In 1873 Michelson graduated and then served on the ships of the U.S. Navy for two years. After the obligatory service, the young Michelson was offered a position of an instructor in physics and chemistry at the Naval Academy. Albert was only glad to receive the appointment, and this heralded the start of his research.

The decision to go in for research required certain courage of Michelson. There were no precedents of the kind at the Academy, so Michelson had to finance his experiments himself. His choice for research was not so simple either: it should not have been too complicated or too far away from the current fundamental problems. Michelson managed to overcome all difficulties, and his first scientific paper was devoted to the measurement of the velocity of light. Physicists working all over the world focussed on this problem again.

Why did Michelson set about solving this very much involved problem? There were several reasons for that. First, he was interested in optics from his early youth. Second, it had been just before Michelson started his research that Maxwell's famous book *A Treatise on Electricity and Magnetism* had been published, where the idea of the electromagnetic nature of light was consistently developed. Maxwell's discovery turned the velocity of light from being an ordinary number into a constant that grew to play a paramount role in physics on the whole. And the last but not the least factor, which had influenced Michelson, was that he happened to attend the lectures on optics given in the United States in 1876 by John Tundall, (1820-1893) a well-known English physicist and popularizer of science.

At the beginning, Michelson tried to improve Foucault's method of the rotating mirror, and before long he succeeded in achieving greater accuracy than Foucault did. Michelson communicated the obtained results to the distinguished American astronomer Simon Newcomb (1835-1909), who was sufficiently authoritative for the government circles and showed interest in the problem of measuring the velocity of light. Newcomb was so elated over the results reported by the young Michelson that he solicited the National Academy of Sciences for $ 5000 to be able to conduct new experiments in the field while Michelson had to look elsewhere for money. Moreover, he arranged to 'borrow' Michelson from the Naval Academy and in the late 1879 set him to work on the *Nautical Almanac*, of which Newcomb was the director, for further determination of the velocity of light. Newcomb explained that preparations for his own method had progressed so far by then that to change over to Michelson's plan would be impractical.

However, Michelson's and Newcomb's collaboration did not last long. The twenty-seven-year-old Michelson was poignantly aware of the need to advance his knowledge in optics. There seems to have only been one way out at the time, and in 1880, having been given a year's leave of absence from the Navy, Michelson sailed for Europe. Newcomb and Alexander Graham Bell, the American inventor, helped him to find a financial backing. Michelson was able to work with the famous German scientist Hermann Helmholtz at the University of Berlin, then at Heidelberg University, in Collège de France and École Polytechnique in Paris. While being in Europe, he became acquainted with the prominent physicists Marie Alfred Cornu, Éleuthère Élie Nicolas Mascart, and Lord Reyleigh.

Of special significance for the history of the determination of the velocity of light were Michelson's relations with John William Strutt (Lord Reyleigh). The immediate reason for their joint discussion of scientific problems was... an incorrect experiment.

I have already mentioned that J. Young and J. D. Forbes tried and improved Fizeau's method. To this end, they applied a scheme in which light was reflected from two mirrors (rather than from one) at different distances from the source of light and the gear-wheel. The observer saw two close luminous points whose brightness was generally different because of the different distances to the mirrors. The researchers selected a speed of rotation such that both points seemed equally bright: they believed that the comparison technique would allow them to avoid the errors inherent in Fizeau's 'absolute' measurements, in which it was required to determine the moment of the maximum possible darkening of the field of vision.

However, Young and Forbes's method only produced new difficulties. Observation of remote point sources of light is almost invariably accompanied by the phenomenon of diffraction, so it was wrong to apply the concepts of geometrical optics and to assume that light travels in straight lines: the image yielded by Young and Forbes's experiments was a system of rings rather than a point.

Inasmuch as the mirrors participating in the formation of the diffraction patterns in the telescope were at different distances from it, the diffraction patterns themselves were different. Naturally, it is far more difficult to determine correctly the moment when the brightness of two different images is compared than when identical images are observed. The details of the diffraction pattern depend on the light wavelength, i.e., on the colour. This induced Young and Forbes to investigate the velocity of light of different colours. Analysing the results of their experiments, they drew a conclusion that blue light propagates in air with a velocity exceeding that of red light.

These experiments were the first in which direct methods were used to obtain data on the relationship between the velocity of light and its colour, and naturally this was what attracted scientists. However, not every physicist regarded Young and Forbes's results with complete confidence.

They described their experiments in a paper in *Nature*. This weekly publication contains brief articles about research being conducted in the natural sciences. In a sense, this is a unique journal: it gave its pages for the first information on the discoveries of physicists and chemists from many countries, and is a method for establishing the priority of discovery. The publishers have always striven to make the papers accessible to everybody interested in the latest achievements in science. Very often this journal printed letters of scientists on problems that thus have started interesting discussions. A discussion of this kind arose from Young and Forbes's communication and was initiated by William Strutt.

William Strutt, the third baron of Rayleigh, was one of the many English scientists during the 19th century to make an ever-lasting contribution to the development of classical physics. Apropos, Rayleigh succeeded not only in physics: jointly with the chemist William Ramsay (1852-1916), he discovered the chemical element argon, a noble gas. Rayleigh's life was devoid of any glamorous events. He was born in 1842 into a family of a rather wealthy landowner. Unlike many other preeminent English naturalists who received their peerage as an award for their achievements, Rayleigh was a baron by birth. He was unable to graduate, because of his poor health, from Eton College, the famous public school for boys where aristocratic children were traditionally educated. Notwithstanding, he managed to be well-prepared for the entrance examinations in Trinity College at Cambridge. In 1865 he graduated from the university passing his last examination with the highest possible praise.

Owing to his academic success, the young physicist became a fellow of Trinity College. At the same time Rayleigh set about his independent research: he was attracted by the acoustics and physiology of auditory perception. He was also interested in the problems of colour vision, whose theory had been developed by T. Young, Maxwell, and Helmholtz. However, Rayleigh's main effort was directed to the elaboration of a mathematical apparatus for the solution of a comprehensive variety of problems related to diverse domains in physics.

Gradually Rayleigh developed his own style of research. He never used his ingenious mathematical technique without utter necessity, and the main point was invariably the physical content of a problem he tackled. Most often Rayleigh began his treatment with simplest cases revealing the basic outlines of the problem and then presented more intricate situations requiring sophisticated mathematical methods for their discussion.

In 1871 Rayleigh got married, left Cambridge, and settled at his family estate at Terling. Here Rayleigh equipped a home laboratory and became engaged in series of experiments. His was a rare combination of the abilities of a theorist and an experimenter. Rayleigh's first experiments concerned photography, and this intensified his interest to the problems of optics.

These experiments led Rayleigh to another problem, that of the scattering of light; the results Rayleigh obtained in the theory of scattering became classical.

In 1880 Rayleigh broke off his self-imposed exile to his family estate and agreed to a flattering offer to become Maxwell's successor in the position of the director of the Cavendish Laboratory. Rayleigh worked in this capacity for five years, and his activity advanced the Cavendish Laboratory as an education and research centre. Rayleigh scientific interests were focussed during the period on problems in acoustics and capillary phenomena.

Following his retirement from the directorship of the Cavendish Laboratory, Rayleigh retreated to his research at Terling, because he was now interested in electromagnetism.

In 1900 he published his famous work treating the blackbody radiation, an ideal body which would absorb all incident radiation and reflect none. Blackbody radiation was one of the central problems in the physics of the late 19th to early 20th century.

In 1904 Rayleigh was awarded a Nobel Prize for his research on gases and the discovery of argon. He was continuing his research until he died in 1919. A collection of Rayleigh's work contains 446 papers on almost every section of physics, but despite this proliferation there were practically no poor or erroneous presentations.

Since his youth Rayleigh was attracted by optics; a good half of his papers deal with this section of physics. The scientist regarded theoretical optics as a part of the theory of waves and exerted every effort to elaborate the methods and concepts applicable to waves of any nature. One of his letters to *Nature* was written in connection with this comprehensive overview of the physics of wave processes. The letter begins as follows:

> "The result announced by Young and Forbes... that blue light travels *in vacuo* about 1.8 per cent faster than red light raises an interesting question as to what it is really determined by observations of character."

Consequently, Rayleigh attracted attention to the need of thorough analysis of what was actually measured in the experiment. Then he referred to a paper he had published ten years before, where he indicated that it is very often that an analysis of wave phenomena involved two velocities rather than one:

> "It has often been remarked that, when a group of waves advances into still water, the velocity of the group is less than tha- of the individual waves of which it is comt posed; the waves appear to advance through the group, dying away as they approach its anterior limit. This phenomenon was, I believe, first explained by Stokes, who regarded the group as formed by the superposition of two infinite trains of waves of equal amplitudes and of nearly equal wavelengths, advancing in the same direction."

In fact, this problem was first considered in 1839 by the well-known Irish physicist William Rowan Hamilton (1805-1865), who developed mathematical theories encompassing wave and particle optics and mechanics. However, Hamilton did not publish the results of his research in full, and his merits were not valued for their worth for a long time. Rayleigh does not appear to have known of Hamilton's work, and therefore he referred to George Gabriel Stokes (1819-1903), a British scientist, as his predecessor.

Let us also try and consider what really is the velocity of waves and which velocity and when is measured in experiments.

A wave is the propagation of vibrations in space. A prime concept of this physical phenomenon is first formed intuitively. People take pleasure in gazing at the surge of sea waves. However, this 'surge' does not, in effect, bring about any essential displacement of great masses of water: when one observes a boat without a sail in a surging sea, one can notice that with time it is only rising and lowering while staying at the same point. This example seems to illustrate the above-mentioned definition of a wave well enough. Observing two boats along the line in which the waves 'surge', we can almost certainly see that the boats rise and lower 'off the beat'. This occurs because it takes the crest of a wave raising one boat a certain time to reach the other boat, and when it raises the second boat, the first one may turn to be not at a crest at all but in the trough or at an intermediate position. Physicists explain this by saying that waves propagate with a finite velocity. Let us deal with these qualitative considerations in terms of mathematics.

The simplest harmonic motion is a periodic vibration that is a sinusoidal function of time, that is, motion along a line given by the equation

$$A = A_0 \cos(\omega t + \varphi), \tag{5.1}$$

where A is the vibrating (oscillating) quantity (e.g., the position of the boat with respect to its mean position), and A_0 is the amplitude of the vibration (i.e., the maximum displacement of the boat from the mean position). The quantity ω is called angular frequency, or radian frequency, and is the rate of change of angular displacement. Another characteristic of any vibration, the period T, is the time between two consecutive moments corresponding to identical configurations of the vibrating system (e.g., two wave crests). Because the period of a cosine is 2π, ω and T are related, i.e., $\omega = 2\pi/T$. The value $\omega t + \varphi_0$ is called the oscillation phase. It defines the part of the disturbance since its beginning. Since the oscillation phase at $t = 0$ is equal to φ_0, the latter is called the initial phase. Note that formula (5.1) can describe harmonic vibrations of various kinds: displacement of a body, a force, the intensity of an electric field, etc.

Speaking of a wave, we imply that the vibration encompasses an area in space (the boats move at a distance from each other). Vibrations at different points may possess different phases and amplitudes: this explains the variation in the motion of the boats.

Figure 17 shows two 'instantaneous pictures' of a disturbance called a 'travelling wave'; they indicate the value of A at different points in space at time moments t_1 and t_2 ($t_2 > t_1$). It is clear from the figure that the crest A_0 of the wave is displaced during time $\Delta t = t_2 - t_1$ to the right at a distance Δx. If we could continuously follow the position of this particular crest, we should see it displace, or travel, hence the name of the wave.

This kind of propagation is described by the formula

$$A = A_0 \cos (\omega t - kx) \qquad (5.2)$$

Here the oscillation phase depends both on time t and coordinate x. The quantity k is called the wave number and it defines the least distance λ between two points moving with the wave at constant phase (the wavelength): $k = 2\pi/\lambda$.

Considering the 'pictures' of the wave yields a way of determining the velocity of a travelling wave. Figure 17 demonstrates that the wave travels as a unity, and therefore the velocity of a

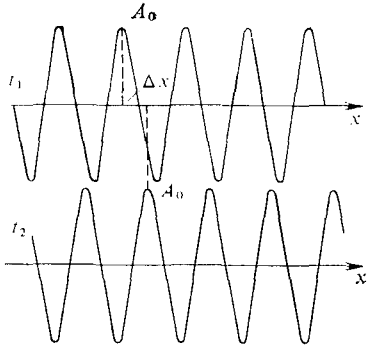


Fig. 17. 'Instantaneous pictures' of a travelling wave: it takes time $\Delta t = t_2 - t_1$ for the crest A_0 to travel the distance Δx.

crest can be interpreted as the velocity of the whole wave. Suppose at an instant t_1 the crest is at point x_1, and at an instant $t_2 = t_1 + \Delta t$ it is at point $x_2 = x_1 + \Delta x$. The physical sense of this statement is that the phases of vibration at the instant t_1 at point x_1 and at the instant t_2 at point x_2 are the same. Therefore we can write

$$\omega t_1 - kx_1 = \omega t_2 - kx_2 \text{ or } \omega\Delta t = k\Delta x.$$

The displacement velocity of a crest $v = \Delta x/\Delta t$, and thus the wave velocity is

$$v = \frac{\omega}{k}. \tag{5.3}$$

It is evident that the displacement of a crest can be regarded as the 'displacement of a phase', and this is why the velocity v we have just found is also called the *phase* velocity.

However, these considerations cannot be held as universal. Few natural waves are sinusoidal. There may occur a train of impulses ('crests') of any shape propagating in space. Physicists give preference to sinusoidal waves because any wave can be treated as a sum of a certain number (finite or infinite) of sinusoidal waves. The Fourier transform, a special mathematical function, makes it possible to find the frequencies and amplitudes of the component waves. Consequently, in order to grasp the wave propagation it is sufficient to consider the motion of the sinusoidal components. Let us see whether there are special features in the propagation of two sinusoidal waves rather than one.

Suppose that two waves with an identical amplitude A_0, angular frequencies ω_1 and ω_2, and wave numbers k_1 and k_2 propagate in the same direction along the x-axis. Let the angular velocities and wave numbers differ little from each other. The resultant vibration at a point in space with the coordinate x can be presented as

$$A = A_0 [\cos (\omega_1 t - k_1 x) + \cos (\omega_2 t - k_2 x)]$$
$$= 2A_0 \cos (\Delta\omega t - \Delta k x) \cos (\omega t - kx), \quad (5.4)$$

where

$$\Delta\omega = \frac{\omega_2 - \omega_1}{2}, \quad \Delta k = \frac{k_2 - k_1}{2},$$

$$\omega = \frac{\omega_1 + \omega_2}{2}, \quad \text{and} \quad k = \frac{k_1 + k_2}{2}.$$

Figure 18 shows 'instantaneous pictures' of this group of waves. It is obvious that although the envelope of the group almost does not change its shape with time, the graph within the envelope changes. In this case, according to (5.4), the vibration at any point of space is determined by the product of two periodic functions with essentially different angular frequencies, $\Delta\omega \ll \omega$. *How shall we determine the velocity of a group of waves?*

If we intend to follow the propagation of the wavefront (i.e., the surface of the constant phase), we have to observe the motion of one of the crests of the curve within the envelope (points O_1 and O_2 in Fig. 18); the slow growth of the crest O_1 and the decrease of the crest O_2 are related to the slowly changing term $\cos(\Delta\omega t - \Delta kx)$ in (5.4). If we neglect the change in the amplitude of a crest, the velocity of its motion can be found from the equality $\omega t - kx =$ const. The reasoning should be the same as we applied when deducing formula (5.3), and therefore the result will be identical, that is, the phase velocity of a group of waves, just as that of a single sinusoidal wave, is $v = \omega/k$.

However, we can deal with the velocity of a wave in another manner and consider the displacement of the maximum of the envelope (it is shown by the arrows B in Fig. 18). What is the displacement velocity of this maximum?

Since $\Delta\omega \ll \omega$, the factor $\cos(\omega t - kx)$ in (5.4) defines the distance between the maxima of the inner oscillation, while $\cos(\Delta\omega t - \Delta kx)$ defines the distance between the maxima of the envelope oscillation. This factor shows the motion of the envelope. Evidently, the envelope

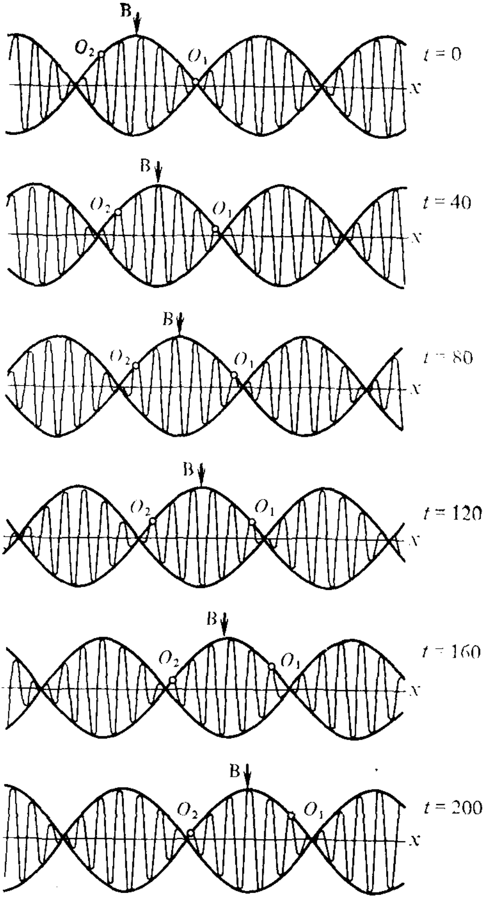


Fig. 18. 'Instantaneous pictures' of a group of waves. Each picture is taken at an indicated conditional time t.

maximum condition can be written as

$$\Delta\omega t - \Delta k x = 2\pi n \ (n = 0, 1, 2, \ldots) . \qquad (5.5)$$

In the course of time, the x coordinate of the maximum at a given n changes: it moves. Condition (5.5) for an instant $t + \Delta t$ can be written as

$$\Delta\omega (t + \Delta t) - \Delta k (x + \Delta x) = 2\pi n. \qquad (5.6)$$

Proceeding from (5.5) and (5.6), it is easy to obtain the relationship

$$\Delta\omega\Delta t = \Delta k \, \Delta x.$$

Then the velocity for the envelope maximum

$$u = \frac{\Delta x}{\Delta t} = \frac{\Delta\omega}{\Delta k}.$$

The limit to which u tends while $\Delta k \to 0$ is

$$u = \frac{d\omega}{dk}. \qquad (5.7)$$

The quantity u is called the *group* velocity of waves. Looking closely at Fig. 18, we can see that the displacement of the maximum shown by the arrow and that of the maximum indicated by the small circle occur in a different manner. Otherwise speaking, this is a picture of the case $v \neq u$.

Why do we need to introduce the concept of a group velocity? There are many cases when wave propagation is related to energy transfer. The shape of the envelope for a group of waves gives a general idea about the distribution of the wave energy in space while the velocity of its maximum, i.e., the group velocity, shows how fast the energy is transferred. Inasmuch as any information transfer is related to energy transfer, the determination of the group velocity of waves proves to be an important physical problem.

Is it always that the group velocity is different from the phase velocity? And if so, what are the consequences of the discrepancy? To answer these questions properly, we have to know the relationship between v and u. Proceeding from (5.7) and differentiating, we can easily obtain

$$u = v - \lambda \frac{dv}{d\lambda}. \qquad (5.8)$$

This formula was first deduced by Rayleigh in 1871 and bears his name. Rayleigh himself used it first of all while considering the waves on a water surface. There are two basic types of waves, viz., gravity waves and capillary waves. Gravity waves are primarily due to gravity forces with surface tension and viscosity being of secondary importance and are negligible. Gravity waves are very long. The principal force in the capillary waves, whose lengths are very small, is surface tension. In Rayleigh's time the phase velocities of these waves as functions of wavelengths λ were already known, and therefore it was possible to determine their group velocities. Let me note that since $v \sim \lambda^{1/2}$ for gravity waves, $v > u$ while the situation is reverse for the capillary waves, namely since $v \sim \lambda^{-1/2}$, we find $v < u$.

Rayleigh's theory can be applied, in principle, to waves of any nature. However, the direct treatment of light waves within the framework of this theory ran into difficulties.

Recall that, according to Huygens's theory, the velocity of light in matter and the refractive index n are related to each other. In the late 17th century Newton investigated the phenomenon of light dispersion and proved that light of different colours is refracted differently, so that each colour has its own refractive index. Consequently, by the end of the 17th century it was evident that the velocity of light in matter depends on the colour. In the early 19th century, the wave theory having won over, the idea of the velocity of light in transparent media being a function of its wavelength became generally accepted. So which of the velocities, the group or the phase velocity, was meant?

When Huygens deduced the law of refraction, he considered the positions of the wave fronts (i.e., the equiphase wave surfaces) of both incident and refracted waves. The above considerations indicate that if the motion of the wave front is taken into account, then only phase velocity makes sense. Therefore we may contend that physicists for a long time dealt with the dependence of phase velocity on the wavelength.

Rayleigh posed a problem of principle: which velocity is measured in the 'direct' experiments? ('Direct' experiments in the determination of the velocity of light are the ones where the path and the time of travel are measured directly rather than in a roundabout way.) The problem concerned both astronomical and 'terrestrial' methods. Rayleigh succeeded in showing that in each of the direct experiments the scientists measured the group velocity of light. He demonstrated his conclusion in an imaginative way:

> "If the crest of an ordinary water wave were observed to travel at the rate of a foot per second, we should feel no hesita-

tion in asserting that this was the velocity of the wave; and I suppose that in the ordinary language of undulationists the velocity of light means in the same way the velocity with which an individual wave travels. It is evident however that in the case of light, or even of sound, we have no means of identifying a particular wave so as to determine its rate of progress. What we really do in the most cases is to impress some peculiarity, it may be of intensity, or of wavelength, or of polarization, upon a part of an otherwise continuous train of waves, and determine the velocity at which this *peculiarity* travels. Thus in the experiments of Fizeau and Cornu, as well as in those of Young and Forbes, the light is rendered intermittent by the action of a toothed wheel; and the result is the velocity of the group of waves, and not necessarily the velocity of an individual wave (i.e., the phase velocity,—*Author*)."

The only thing left to add is that Foucault's method using a rotating mirror also makes it possible only to measure the group velocity of light.

However, any theory has to meet the challenge of an experiment. In this case the acid test was to overcome the difficulties entailed by the measurement of the velocity of light in matter. They were formidable per se, and on top of them it was required, in order to confirm Rayleigh's theory, to measure the velocities of light of different wavelengths. Note that, even in the media giving strong dispersion, the phase velocities of light waves with different λ deviate from each other by no more than several per cent. Thus the measuring error of the velocity of light should not have exceeded 1%. Suffice it to recall in this connection Cornu's unsuccessful attempts at improving the accuracy in measuring *c*.

True, there was an aspect in which Rayleigh's theory, after the results of Young and Forbes had appeared, proved its consistency: the discrepancy between their results and the forecast of the theory indicated that the experimental technique was far from being perfect. However, Rayleigh could hardly have been satisfied with the 'critical' aspect of his theory.

Rayleigh dreamt of improving the accuracy of the experimental determination of the velocity of light. This was young Michelson's aspiration, and this was the goal set by Young and Forbes for themselves. Every one of them was sincerely interested to establish the truth, and thus the burgeoning discussion proved to be bitter. Following Rayleigh's letter treating Young and Forbes's experimental results, *Nature* published Michelson's letter, where he disagreed with the conclusions of the British physicists. Rayleigh read the letter, and thus his correspondence with Michelson started, which lasted for many years since.

Michelson was very much encouraged by the interest Rayleigh revealed to his work on the measurement of the velocity of light. He set up test experiments on the determination of the velocity of light in air, which refuted the conclusions Young and Forbes had reached. Then Michelson set about measuring the velocity of light in water and carbon disulphide, and here he encountered an unexpected puzzle. The ratio of the velocity of light in air to that in water, as found in the measurements, was very close to the refractive index of water. However, things were startlingly different with carbon disulphide: its refractive index in the applied range of wavelengths did not exceed 1.66 (see the table

λ, μm	0.589	0.550	0.486
n	1.628	1.640	1.652

below). At the same time the measurements of the velocity of light produced the following result: $u = c/1.77$. The discrepancy between the calculated data and those of the experiment considerably exceeded, according to Michelson's estimates, the possible errors of the experiment. Therefore, despite that he could not explain the results himself, Michelson decided to publish the results.

A detailed explanation of Michelson's data was furnished almost simultaneously by Rayleigh and the American physicist Josiah Willard Gibbs (1839-1903). It was based on Rayleigh's theory on the relation between the group and phase velocities.

Inasmuch as Michelson basically used Foucault's method, which he improved, he measured the group velocity of light, while the refractive index characterizes the phase velocity. Figure 19 shows the ratio v/c, v being the phase velocity of light in carbon disulphide and c being the velocity of light in air, as a function of the light wavelength λ; the graph is plotted by the results presented in the table above. It is clear that the points are very close to the broken straight line, and therefore it is easy to determine the value of the derivative of the implied function (recall that the derivative of a function at a given point is numerically equal to the tangent of the slope of the curve of the function at the same point). It follows from Fig. 19 that the derivative $d(v/c)/d\lambda = 0.088 \mu m^{-1}$. Hence, taking into account Rayleigh's formula (5.8) and the middle wavelength $\lambda = 0.55\,\mu m$, one can find the magnitude of the group velocity $u = c/1.73$, which is close to the value of u obtained by Michelson ($u = c/1.77$).

Consequently, Michelson's unexpected experimental results became the first corroboration of Rayleigh's theory as applied to light. It became now possible to state boldly that light is characterized by two velocities, the phase and the group one, rather than by a single velocity.

One can qualitatively imagine the mechanism of propagation of electromagnetic waves in matter as follows. An incident electromagnetic wave excites forced oscillations of the electrons in the atoms and molecules of the medium. According to the laws of electrodynamics, any oscillating charge emits electromagnetic waves with the frequency of its own oscillations. Therefore, there occurs, as it were, 'reemission' of the electromagnetic waves in the matter, which releases the absorbed energy of light.

The electrons' forced oscillations always occur exactly with the frequency of the incident wave.

However, their amplitude is greater if this frequency is closer to the natural oscillation frequency of the electrons in atoms and molecules (the phenomenon of resonance). That is why the reemission is determined by the relationship between the natural oscillation frequency of the electrons of matter and the frequency of the

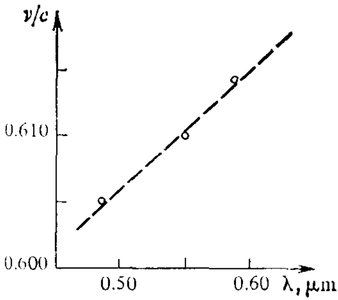


Fig. 19. The relative phase velocity of light v/c in carbon disulphide as a function of the light wavelength λ.

incident wave. As a result the velocity of propagation of electromagnetic waves in matter is bound to depend on their frequency. It is only in vacuum where wave dispersion (scattering of radiation) is absent, and this is the only case when $u = v$ holds exactly.

Michelson's experiments showed that the group velocity of light was less than its phase velocity. But is this relationship always valid? It follows from formula (5.8) that if the derivative $dv/d\lambda$ is positive, then $v > u$, but if it is negative, then $v < u$. Common dispersion corresponds in optics to the first case, when the refractive index n increases with a decrease in the wavelength:

Michelson carried out his experiments precisely within this range. However, anomalous dispersion is also known in optics, when the refractive index versus wavelength curve of a medium rises rather than falls, as shown in Fig. 20. Note that anomalous dispersion occurs in the vicinity of

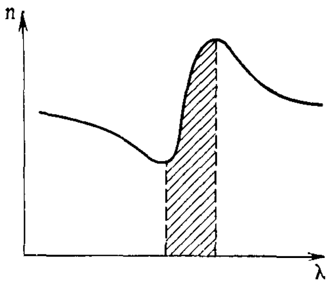


Fig. 20. The refractive index n of a medium as a function of the wavelength λ in the area of anomalous dispersion (indicated by hatching).

absorption lines or bands in the absorption spectrum of the medium, and therefore the absorption of incident light is great. In principle, the relationship $u > v$ can be valid in the spectrum range where anomalous dispersion is observed. (Naturally, the conclusion of the theory of relativity that the group velocity, i.e., that of energy transfer, cannot be greater than the velocity of light in vacuum holds true, see Chapter 6.) However, it turns out that the two velocities, the phase and the group ones, 'do not suffice' to describe the propagation of light in conditions of anomalous dispersion in a medium. When the dispersion is strong (i.e., when the derivative $dv/d\lambda$ is great, as is the case for anomalous dispersion), the absorption considerable or the spectrum of the wave signal is wide (i.e., when the signal is the sum of a number of sinusoidal waves with different λ), the concept of group velocity is not useful. The reason for that can be easily grasped from the consideration of Fig. 21, where the 'pictures' of a wave propagating along the x-axis are presented. The wave at an instant $t = 0$ is a periodic train of impulses that are almost rectangular in their shape. However, in actual fact, this wave is a sum of 15 sinusoidal waves with the frequencies ω, 2ω, . . ., 15ω. The wave propagates in a medium with strong dispersion, and therefore the phase velocities essentially differ. As a consequence, after a sufficiently long interval of time the shape of the wave impulses changes beyond recognition. Here we are unable to choose a point to determine the velocity of the wave, so other concepts are applied to describe the physical processes in this situation, in which again the velocity of energy transfer does never exceed the velocity of light in vacuum.

Note that the propagation of electromagnetic waves in matter described above in a simplified manner is characteristic for a steady-state wave motion, i.e., after initial transients or fluctuations have disappeared. In a more strict consideration it is necessary to take into account the features of the wave front motion, or that of the 'precursor', but this is beyond this presentation.

The moment of truth for the erroneous conclusions of Young and Forbes occurred at a conference of the British Association for the Advance-

Fig. 21. A change in the shape of a wave in the course of time in a medium with strong dispersion. The numbers attached to the graphs correspond to conditional time 'instances' *t*.

ment of Science in 1884 in Montreal. Rayleigh, the president of the Association, invited the conference participants to discuss the velocity of light in air. Although the results of Michelson's experiments on measuring the velocity of light in carbon disulphate had been published by that time, the opinions of scientists on the work of Young and Forbes differed. Thus, the well-known Irish theoretical physicist George Francis Fitzgerald (1851-1901) contended that blue light propagates faster than red one both in air and in vacuum as well. The discussion at the conference was won by Rayleigh supporters. Of no small importance in the victory was Michelson's presentation *On the Velocity of Light in Carbon Disulphide*.

The errors of Michelson's measurements were very small, and this attracted the attention of the oldest English physicist William Thompson (later Lord Kelvin) (1824-1907). Both Kelvin and Rayleigh urged Michelson to stage anew the inconclusive experiment he had tried to carry out during his stay in Europe in 1881. This experiment concerned the velocity of light propagation in moving media. The European physicists, whose authority in science was beyond any doubt, believed that the young American had the strength to solve the problem that had been on the agenda for several decades. Michelson took the cue and boldly set about the work.

Chapter 6

The Velocity of Light and Material Motion

Commonly the extension of a physical theory advances by way of solving ever more complicated problems it is related to. This holds true in optics as well. A natural sophistication in the determination of the velocity of light was a research on the propagation of light in moving bodies.

The problem originated in the late 17th-early 18th centuries. Aberration was the most essential optical phenomenon in astronomy which required to answer whether the motion of bodies influence the propagation of light. Recall that aberration is the apparent angular displacemet of the position of a celestial body in the direction of motion of the observer, caused by the combination of the velocity of the observer and the velocity of light. To give an adequate account of the effect, it is necessary to correlate the motion of light and the motion of the Earth with respect to 'fixed' stars. Bradley, who discovered the phenomenon and gave it its first theoretical substantiation, employed the law for the composition of velocities, which is a corollary of Galileo's principle of relativity.

This viewpoint seems to have been quite natural for the adherents of the corpuscular theory. In the late 18th century, a conclusive experi-

11—0541

ment was suggested. An astronomical telescope was to be filled with water and then the angle of aberration determined. Since the velocity of light in water, according to the corpuscular theory of light, was greater than that in air, the angle of aberration should have been decreased. This experiment was actually performed about a hundred years later. In fact, no change in the angle of aberration was observed.

The theory of aberration did not attract the attention of physicists until the end of the 18th century. The situation drastically changed because of the evolution of the wave theory of light. Thomas Young and Augustin Jean Fresnel regarded light as a wave process occurring in a special medium, the ether. The properties of this luminiferous ether were to be rather strange: it must have been of infinitesimal density while being highly elastic, i.e., being rigid for all practical purposes. However, the problem of its being dragged by moving bodies is of interest rather than its paradoxical properties in this context.

Many optical effects seemed to be different in their mechanism depending on whether the ether was fixed or dragged by moving bodies.

Thomas Young was the first to attempt to consider aberration on the basis of the wave theory. He proceeded from the hypothesis of the stationary ether and arrived at results that were in good agreement with the corpuscular theory.

Arago tried to determine whether ether is dragged experimentally several years later: he investigated light refraction in prisms with this aim in mind. Stars were used as the sources of light. According to Arago's working hypothesis, if the Earth moved towards a star, then the velocity of the Earth should be added to the velocity of light from the star, while if the Earth moved away from the star, the velocity of the Earth should be subtracted.

As was mentioned above, the wave theory related the magnitude of the velocity of light in matter (in Arago's case in the matter in his prisms) to its refractive index. Therefore the refractive index in a prism should be different depending on whether the Earth is going to or coming from the same star. This was expected to be seen as a change in the refraction angle of the light from the star. Arago had estimated that provided the assumption were valid the apparent divergence in the paths of light passing through the prism might be as much as 2′, a magnitude quite accessible for measurement. However, try though he would, Arago was unable to reveal any discrepancy in the light refraction when the Earth's annual motion with respect to a star changed. Arago could offer nothing to account for the fact, and therefore he applied to Fresnel, his friend and colleague, with a request to explain the negative result of the experiments. Fresnel answered with a letter, which was subsequently published in 1818. It has every right to be considered the outset of a whole section in optics, the optics of moving bodies.

The history of science evidences that Fresnel's investigation (his letter should be regarded as a serious scientific work) triggered the elaboration of not a particular but rather a fundamental problem, whose solution required a review of the basic physical concepts. Here is what Fresnel wrote in the letter:

"You have requested me to examine the results of the observations and see whether they can be in a better agreement with the theory according to which light is regarded as vibrations of a universal fluid (ether.—*Author*)...

"If one admits that our globe imparts its motion to the ether it is enveloped in, it would be easy to understand why the same prism always refracts light in the same manner, irrespective of the direction from which the light arrives. However, proceeding from this viewpoint, it appears to be impossible to account for the aberration of stars: at least, I could only comprehend this phenomenon clearly while supposing that the ether passes freely through the globe and the velocity communicated to this subtle fluid is only a small part of the velocity of the Earth and that it does not exceed, for instance, one hundredth of this velocity."

Fresnel did not only extend a qualitative consideration of the interaction between the ether and a moving body, he also offered a calculation of what was called 'the Fresnel drag coefficient' which indicated what part of the body's velocity is communicated to the ether. Fresnel's logic can be reduced to the following.

Light propagates in an elastic ether, its density ρ' in matter being greater than its density ρ beyond matter while the elasticity is the same. The velocity of propagation of light disturbances is in inverse proportion to the square root of the ether density, and therefore

$$n = \frac{c}{v} = \sqrt{\frac{\rho'}{\rho}},$$

where v is the velocity of light in matter and c is the one beyond the matter.

When a body moves, the density of the ether within it remains constant. According to Fresnel this occurs because some of the ether, i.e., that fraction corresponding to the ether excess in the body over that in the surrounding space, moves with the body.

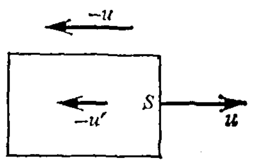


Fig. 22. To the calculation of the Fresnel ether drag coefficient.

Let a body move with a velocity u, i.e., the ether flows around it with the velocity $-u$. However, the velocity of the ether within the body with respect to the body is different. While the body moves, a condition of the continuity of the ether on the body surface S should hold (Fig. 22). This condition means that the quantity of the ether flowing in the body in a unit time through a unit surface must be equal to the quantity of the ether flowing out of a unit surface beyond the body:

$$u\rho = u'\rho'.$$

Inasmuch as $\rho \neq \rho'$, it follows that $u \neq u'$. Using the expression for the refractive index n, we can easily find the velocity of the ether moving within the body with respect to an external stationary observer:

$$u'' = u' - u\left(1 - \frac{1}{n^2}\right)u. \qquad (6.1)$$

It follows from formula (6.1) that, according to the external observer, the process of propagation of light in a moving body looks as if the ether moves with the velocity u' rather than the velocity u of the body. Therefore, depending on the direction of the body motion, the velocity of light within the body with respect to the stationary observer equals

$$v' = \frac{c}{n} \pm \left(1 - \frac{1}{n^2}\right) u,$$

the minus sign corresponding to the motion of light counter to that of the body and the plus sign corresponding to their motion in the same direction. This deduction makes evident why the coefficient

$$\mu = 1 - \frac{1}{n^2}$$

is called the *partial drag coefficient.*

Fresnel's work had an important advantage: it both explained the null results in Arago's experiments and brought about a conclusion allowing, in principle, an experimental verification, namely, that the velocity of light in a moving body differs from that in a fixed one.

Despite the seeming simplicity of such an experiment, which was devised to verify the whole theory offered by Fresnel, nobody could resolve to set it up for many years.

In the meanwhile, physicists tried to consider Fresnel's problem proceeding from other theoretical grounds. The most prominent was the work by Stokes, who obtained the same expression for the drag coefficient employing other method than Fresnel.

Fizeau was the first who dared investigate the propagation of light in a moving liquid. However, he did not pursue the goal to determine the absolute magnitude of the velocity of light in water, *either stationary or moving*. He was interested in the *dependence* of the velocity of light *on* the velocity of the moving water; in other words, he intended to find the value of the drag

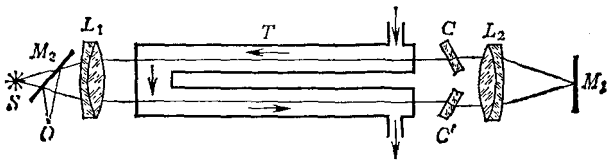


Fig. 23. A schematic representation of Fizeau's experiment on the measurement of the velocity of light in moving water. S is the source of light, C and C' are regulating plates, M_1 is a flat mirror, M_2 is a semitransparent plate, L_1 and L_2 are lenses, T is a tube filled with water, and O is the point in whose vicinity the interference pattern appears.

coefficient in experiment and compare the result with the forecast of Fresnel's theory.

Concerning Fizeau's early optical studies, I should mention that he was a master at interference experiments. A stroke of genius can also be seen in his 1859 experiment with moving water, a diagram of which is shown in Fig. 23.

Light from the source S passed through a semitransparent plate M_2 to a lens L_1; prior to the lens L_1 the light had to split passing through two slits, so that after L_1, there were two narrow parallel beams, each of which travelled through one length ($l = 1.487$ m) of a glass tube T filled with water. On leaving the tube, the light traversed two regulating plane-parallel plates C and C' and then passed through a converging lens L_2, a flat mirror M_1 being located in its focal plane.

Fizeau's apparatus is adjusted so that after being reflected from the mirror M_1 the beams meet, and when they reach the eyepiece O, one has traversed the length of the tube twice in the same direction as the flowing water, while the other has done the same in the opposite direction. Therefore the paths of the beams were exactly the same, which was necessary to avoid the influence of undesirable factors on the propagation of light (uneven heating of the parts could lead to a change in their dimensions and variation in the density of water, etc.). These factors influenced both beams in the employed scheme equally, and therefore the interference pattern in the vicinity of the point O was steady.

Thus Fizeau, to observe interference, used essentially Young's two-slit scheme: the wave from the source S was split into two parts, each of which travelled over the same optical path. Then the two beams superposed and interference occurred. The observer could see alternating fringes in the vicinity of point O; the eyepiece scale made it possible to measure the width of a fringe. The width of the interference fringes depended on the distance between the slit images formed by the optical scheme; the slits could be regarded as the sources producing the interference pattern. The closer these sources, the greater the width of the interference fringes.

Inasmuch as the inner diameter of the glass tube was 5.3 mm, the distance between the parallel light beams could not be very small. However, it is extremely difficult to adjust the apparatus in order to observe the interference when the distance between the sources was great. Therefore Fizeau had to employ an additional glass plate (it is not shown in Fig. 23) located in the path of light from one of the slits. This plate was inclined, and the refracted beam was displaced parallel to itself. The result was that the conditions of interference were changed as if the slits were closer to each other.

Fizeau applied a heliostat for his observations. The constant speed of the flowing water was provided by a pneumatic device pumping water through the tube by means of compressed air; the maximal speed of water reached 7 m/s.

The observations were carried out according to the following pattern. At the outset, the position of a fringe was registered when the water was still. Then the water was pumped through the tube. Fizeau's idea was that if the motion of a medium actually influences the propagation of light, then the interference pattern of the light passed through the moving medium must change. He constructed his experiment so that the two beams of light only differ in that one of them traverses the tube in the same direction as the water, and the other does so in the opposite direction. Therefore there should be a path difference resulting in a change in the interference pattern. The observer only need measure the displacement of the fringe as accurately as possible. Fizeau made two tests each time, passing water in the opposite directions. Naturally, the direction of the fringe displacement changed accordingly, and the equality of the displacement with respect to the still-water test evidenced the reliability of the results thus obtained.

Fizeau's expectations came true, and he succeeded in observing the displacement of the fringes, thus proving once and for all that the motion of a medium exerts an influence on the propagation of light. This refuted the theory supposing the ether to be stationary. At the same time, the decision of which theory was right, that of complete ether drag or Fresnel's theory of partial drag could only be made proceeding from an analysis of the magnitude of the displacement.

According to the theory of complete drag, the velocity of light in moving water with respect to a stationary observer equals

$$v' = \frac{c}{n} \pm u,$$

where c is the velocity of light in vacuum, n is the refractive index, and u is the velocity of the medium (as to the plus or minus sign, see p. 166). Thus it takes the time

$$t_1 = \frac{2l}{\frac{c}{n} - u}$$

for the light travelling opposite the water current, and the time

$$t_2 = \frac{2l}{\frac{c}{n} + u}$$

for the light travelling in the same direction with the water current to traverse a distance $2l$. Hence

$$\Delta t = t_1 - t_2 = \frac{4lu}{\left(\frac{c}{n}\right)^2 - u^2}.$$

Knowing the time difference Δt, one can find the path difference between the two beams: $\Delta = c\Delta t$. It is more convenient to consider a magnitude Δ/λ (λ being the light wavelength) indicating how many wavelengths there are in the path difference Δ. Inasmuch as $c/n \gg u$, we can write

$$\frac{\Delta}{\lambda} \simeq \frac{4l}{\lambda} \frac{u}{c} n^2.$$

In Fizeau's opinion, light is least attenuated, while passing through water, at $\lambda = 5260$ Å (1 Å $= 10^{-10}$ m; it is aprroximately the magnitude of an atomic radius and was introduced in 1868 by the Swedish physicist and astronomer Anders Jonas Ångström). Proceeding from the theory of complete ether drag, the estimate of Δ/λ at $u = 7$ m/s and $n = 1.33$ is

$$\frac{\Delta}{\lambda} = 0.46.$$

Fizeau's data, obtained through processing of the results of 19 series of measurements, yielded

$$\frac{\Delta}{\lambda} = 0.23.$$

The discrepancy between the experimental data and the forecast of the complete ether drag theory obviously exceeded possible errors of observations.

Fizeau's theory of partial drag gave other expressions for t_1 and t_2:

$$t_1 = \frac{2l}{\frac{c}{n} - u\left(1 - \frac{1}{n^2}\right)}$$

and

$$t_2 = \frac{2l}{\frac{c}{n} + u\left(1 - \frac{1}{n^2}\right)}.$$

Therefore

$$\frac{\Delta}{\lambda} \simeq \frac{4l}{\lambda} \frac{u}{c} (n^2 - 1).$$

An estimate of this quantity is $\Delta/\lambda = 0.2$. This number is in a far better agreement with the experimental data, although the discrepancy between the calculated value and the experimental result is too great, about 15%. The diligent Fizeau thoroughly investigated every possible source of the discrepancy. He drew a conclusion that the principal measuring error was related to the determination of the water current speed. This speed was measured by the quantity of water flowing out of the tube in a unit time. With good reason, Fizeau concluded that this technique made it only possible to find the average speed of water current. In point of fact, however, the speed near the axis of the tube was greater than that near the walls because of the viscosity and friction between the liquid and the walls.

The optical scheme was such that light propagated along the axis of the tube, and the cross-section of the light beam was only 1/5 of the inner cross-section of the tube. Therefore light travelled through the part of liquid moving with a speed greater than average. It is easy to see that an account of this circumstance (a greater value of u should be taken in Fresnel's theory) improves the agreement between the calculated data and the experimental data. Besides, Fizeau felt that errors in the determination of Δ could be related to the fact that the observer had to make the measurements very rapidly: the water was pumped from one reservoir into another, and their capacity did not allow the experimenter to sustain the needed stationary current of water for a long time. To overcome this difficulty, it was required, according to Fizeau, to introduce major changes into his experimental apparatus, and therefore he did not use this possibility to enhance the accuracy.

Fizeau experimented not only with moving water: he also endeavoured to reveal the influence of the motion of air on the velocity of light propagation. However, these experiments yielded a null result which Fizeau regarded as a corroboration of Fresnel's theory. The refractive index of air is very close to unity: $n = 1.0003$. It is easy to estimate the possible displacement of fringes in experiments with moving air at the same parameters of the apparatus as with moving water: $\Delta/\lambda = 1.5 \cdot 10^{-4}$. It follows from this estimate that if Fizeau had succeeded in observing a displacement of the fringes in experiments with moving air, this would have been an evidence contradicting Fresnel's theory.

A thorough analysis of the experimental conditions allowed Fizeau to arrive at the following conclusion:

> "It seems to me that the success of this experiment should be regarded as a confirmation of Fresnel's theory or at least of the law which he found for the change of the velocity of light due to the effect of motion of bodies. Indeed, although the law proved to be true, there is only one convincing evidence in favour of the hypothesis which only corroborates a corollary of it. It is possible that Fresnel's concept will seem so extraordinary and, according to some reviewers, very difficult to accept that it

> will require new evidence and in-depth mathematical analysis before it will be viewed as a reflection of the real situation."

However, Fizeau's experiments were not conclusive enough to solve the puzzle of the relationship between the ether and body motion. Rather, they may be thought to be the beginning of the road leading to the final conclusion. It follows from Fizeau's words that although Fresnel's formula has been corroborated, the theoretical premises from which it had been derived were rather far-fetched. For instance, it is not clear how Fresnel's theory could help explain the propagation of light in a moving medium with essential dispersion. The index of refraction is a function of the wavelength in a medium. So what does the dragging of an excess density of ether mean if there is dispersion?

Fizeau's experiment was rightfully regarded as one of the basic ones in the optics of moving bodies. Therefore it is a small wonder that this experiment was later repeated more than once. However, the idea only was taken as a point of departure, and both the scheme of the apparatus and the technique of measurement were steadily improved.

The first who dared set up in 1886 an experiment with a modified Fizeau's scheme were Albert Michelson in collaboration with Edward Williams Morley. As it was in almost every Michelson's optical investigation, the improvements were simple and highly efficient. Here is what Michelson wrote about this experiment:

> "... for the reason that the experiment was regarded as one of the most important in the entire subject of optics, it seemed to me

that it was desirable to repeat it in order to determine, not only the fact that the displacement was less than could be accounted for by the motion of the water, but also, if possible, how much less. For this

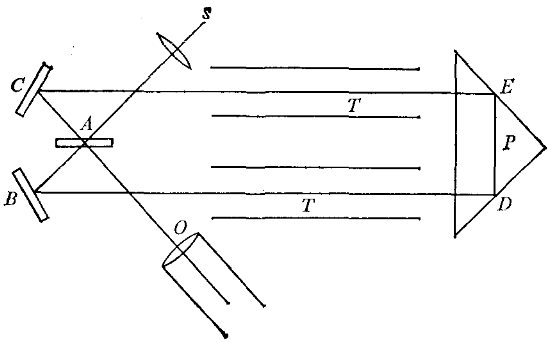


Fig. 24.

purpose the apparatus was modified in several important points, and is shown in Fig. [24].

"It will be noted that the principle of the interferometer has been used to produce interference fringes of considerable breadth without at the same time reducing the intensity of the light. Otherwise, the experiment is essentially the same as that made by Fizeau. The light starts from a bright flame of ordinary gas light [*s*], it rendered parallel by the lens, and then falls on the surface [of plate *A*], which divides it into two parts, one reflected and one transmitted. The reflected portion [from *A* and *C*]

> goes down one [upper] tube, is reflected twice by the total reflection prism *P* through the other tube, and passes, after necessary reflection [from *A* and *B*], into observing telescope [*O*]. The other ray pursues the contrary path [*SABDECAO*], and we see interference fringes in the telescope as before [by Fizeau], but enormously brighter and more definite. This arrangement made it possible to make measurements of the displacement of the fringes which were very accurate. The result of the experiment was that the measured displacement was almost exactly seven-sixteenths of what it would have been, had the medium [ether] which transmits the light waves moved with the velocity of the water."

Thus, Michelson and Morley improved Fizeau's data. Still more accurate measurements were carried out by the Dutch physicist Pieter Zeeman (1865-1943) and his colleagues in 1914. This group of experimenters employed photography to register the interference fringes. The Dutch physicists made Fizeau's dream come true: they reached greater accuracy in the determination of the velocity of water. To produce a steady-state water current, they used the Amsterdam water-supply system; water pressure was regulated precisely. The discrepancy between Zeeman's results and calculations by Fresnel's formula did not exceed 2.6%.

To test the influence of light dispersion on the experimental results, Zeeman and his coworkers investigated the propagation of light in moving solid bodies made of materials with essential dispersion characteristics. By the time these experiments were performed, the outstanding Dutch physicist Hendrick Antoon Lorentz (1853-1928) had obtained an expression for the drag coefficient taking into account dispersion:

$$\mu' = 1 - \frac{1}{n^2} - \frac{\lambda}{n}\frac{dn}{d\lambda},$$

which at small dispersion ($dn/d\lambda \to 0$) gave the Fresnel drag coefficient $\mu = 1 - 1/n^2$. Lorentz's theory proceeded from the concept of the electron structure of matter and was considered to be physically more substantiated than Fresnel's theory.

Zeeman's group experimented on cylinders made of quartz and flint (a special kind of glass); the cylinders were moved by means of a crank drive at a rate of about 10 m/s. The interference pattern was photographed first when the cylinders moved from the left to the right and then when they moved in the opposite direction, following which the position of the fringes was compared. The measurements were carried out in three narrow ranges of wavelengths 4750 Å, 5380 Å, and 8510 Å. The accuracy of the experiments made it possible to reveal the influence of dispersion on the results and confirm Lorent'z formula.

One more problem in the optics of moving bodies, which Fizeau's experiment could not test, consisted in the following. Fizeau's experiment proved the influence of the motion of water (or 'ponderable matter', as they were saying in the 19th century) on the propagation of light. But light can also travel in vacuum, which, according to the physical concepts of the last century, is filled with ether. Is it possible to register the motion of an observer with respect to the ether if the measurements are conducted in a medium whose index of refraction is close to unity?

This problem directly concerns the Galilean-Newtonian principle of relativity in mechanics, which was formulated in the 17th century and which asserts that the essential features of all uniform motions are independent of the frame of reference. The mathematical expression of this principle is the invariance of the equations of the Newtonian mechanics with respect to the Galilean transformations, which are used to relate the space and time variables of two uniformly moving (intertial) reference systems in nonrelativistic kinematics.

Following the elaboration of Maxwell's electrodynamics, it was natural to pose the following problem: is the principle of relativity valid in electrodynamics? *Unlike* equations in mechanics, the Maxwell field equations, i.e., the four differential equations which relate the electric and magnetic fields to electric charges and currents and form the basis of the theory of electromagnetic waves, changed their form when the Galilean transformations were applied. Besides, it followed from them, for instance, that the velocity of propagation of electromagnetic waves in vacuum (i.e., the velocity of light) does not depend on the velocity of both the source and the observer. This corollary of Maxwell's electromagnetic field theory contradicted the classical law of the composition of velocities, a corollary of the Galilean-Newtonian principle of relativity.

This was a conundrum to which various solurions were offered. The German physicist Heintich Rudolph Hertz (1857-1894) expounded his version of electrodynamics proceeding from the hypothesis of complete ether drag by moving bodies. Hertz's equations were invariant with respect to Galilean transformations. Therefore, Hertz endeavoured to extend the classical principle of relativity in mechanics to cover electromagnetic phenomena, thus rejecting the Maxwell field equations. However, Hertz's theory was clearly inconsistent with the results of Fizeau's experiment and was not accepted by physicists.

An alternative solution was to suppose that the ether existing in vacuum was stationary and could be regarded as an absolute reference system, thus refusing the principle of relativity in electrodynamics and, consequently, in optics.

The existence of the stationary ether could only be tested by experiment. However, the staging of the required experiments required overcoming immense difficulties, which had been pointed out by Maxwell. He wrote in his entry *Ether* for Encyclopaedia Britannica:

> "If it were possible to determine the velocity of light by observing the time it takes to travel between one station and another on the earth's surface, we might, by comparing the observed velocities in opposite directions, determine the velocity of the aether with respect to these terrestrial stations. All methods, however, by which it is practicable to determine the velocity of light from terrestrial experiments depend on the measurements of the time required for the double journey from one station to the other and back again, and the increase of this time on account of a relative velocity of the aether equal to that of the earth in its orbit would be only about one hundred

millionth part of the whole time of transmission, and would therefore be quite insensible.

"... The only practicable method of determining directly the relative velocity of the aether with respect to the solar system is to compare the values of the velocity of light

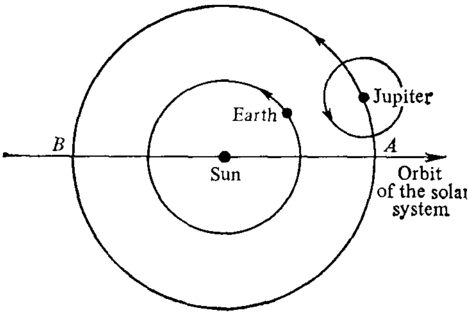


Fig. 25.

deduced from the observations of the eclipses of Jupiter's satellites when Jupiter is seen from the earth at nearly opposite points of the ecliptic."

Maxwell's idea as to how to determine the velocity of the ether with respect to the Solar system by observing the eclipses of Jupiter's satellites can be reduced to the following details. Let Jupiter pass point A in its orbit (Fig. 25) while the Sun moves as indicated by the arrow and, therefore, approaches point A. The orbital period of Jupiter's revolution around the Sun is about 12 years, and hence Jupiter's position in its orbit does not change much within a period of one year. Within this year, one can measure the velocity of light by observing the eclipses of a satellite of Jupiter. Note that once Jupiter during the year is somewhere in the vicinity of point A, the light from the satellite travels to the Earth opposite to the motion of the Solar system in its orbit around the centre of our Milky Way Galaxy. Therefore, *if the ether is stationary*, the velocity of light propagation should seem greater than if the Sun were 'fixed'. Six years later, Jupiter will be in the neighbourhood of point B, and, therefore, the light from the satellite will travel in the same direction as the Solar system. Hence, the velocity of light deduced from observing Jupiter's satellites will prove to be less than the one obtained six years earlier.

But what should the accuracy of the observations of the delays in the eclipses of Jupiter's satellites be in order to show up a discrepancy in the velocities of light? Let us define v to be the velocity of the Sun. Then the maximum delay in the eclipses of the satellite registered when Jupiter is in the vicinity of point A should be

$$t_1 = \frac{d}{c+v},$$

where d is the diameter of the Earth's orbit. The delay, when Jupiter is near point B, is estimated as

$$t_2 = \frac{d}{c-v}.$$

Were the Solar system stationary with respect to the ether, these two lapses of time would be the same and equal to $t_0 = d/c$. Obviously, the relative difference between the delays to be registered should be

$$\frac{t_2 - t_1}{t_0} = \frac{2v}{c\left(1 - \frac{v^2}{c^2}\right)}.$$

The velocity of the Sun in its orbit around the centre of the Galaxy amounts to about 250 km/s, whence

$$\frac{t_2 - t_1}{t_0} = 1.7 \times 10^{-3}.$$

This means that it is required to register an absolute difference of 1.6 s in the times of delay: even to date the accuracy of astronomical observations does not make it possible to record such a small magnitude and therefore reveal the effect Maxwell had in mind.

Consequently, we can only count on 'terrestrial' experiments, where light traverses a two-way 'closed' path, to and back. An estimate of the required accuracy for the experiment to test Maxwell's surmise in this case can be obtained from the following simple considerations. Suppose the ether is stationary, and the velocity of the Earth's motion is parallel to a segment connecting two points on the Earth's surface. If the distance between these points is l, then the complete time it takes light to travel from one point to the other and back is

$$t_1 = \frac{l}{c - v} + \frac{l}{c + v} = \frac{2lc}{c^2 - v^2}. \quad (6.2)$$

If the Earth's rotation had no effect on the propagation of light, this time would be determined by the expression

$$t_2 = \frac{2l}{c}$$

and therefore in order to test the problem in experiment it is required to register the time of light travel with an accuracy better than the difference

$$\Delta t = t_1 - t_2 = \frac{2l}{c} \frac{v^2}{c^2}.$$

Hence Maxwell's estimate is

$$\frac{\Delta t}{t_2} = \frac{v^2}{c^2}.$$

The velocity of the Earth's advance along its orbit is about 30 km/s, whence $\Delta t/t_2 \simeq 10^{-8}$. Therefore the relative accuracy of 'terrestrial' experiments to detect the Earth's motion with respect to the stationary ether must greatly exceed the relative accuracy of the relevant astronomical measurements, because it would require to take into account some values exceeding the second power in the ratio v/c of the Earth's motion to the velocity of light, and this led physicists to ponder over research on the 'second-order' experiments.

Michelson decided to undertake this formidable problem. He intended to detect the 'ether-drift' which might be observed in the Earth motion through a stationary ether. Michelson had set about this research in 1880, when he worked in Helmholtz's laboratory.

Michelson was well aware of the fact that it was hardly sufficient to improve the then existing optical schemes in order to attain a great enough accuracy in experiments. He invented an interferometer of his own design in hope to succeed in the measurements (Fig. 26).

The beam *sa* from a source falls on a semitransparent plate *m*, and part of it proceeds to a mirror *h* while another part is reflected in a perpendicular direction to a mirror *b*. On reflecting from

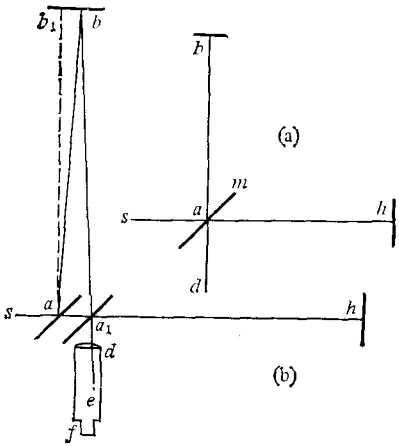


Fig. 26. (a) The paths of light in Michelson's interferometer. (b) To the calculation of the path difference in Michelson's interferometer.

the mirror *b*, the beam passes through the semitransparent plate in the direction *ad*; the beam returning from the mirror *h* is reflected from *m* in the same direction *ad*. Inasmuch as both beams travelling along *ad* originate from the same beam *sa*, they interfere with each other. The interference pattern can be viewed through a telescope.

Clearly, if the mirrors h and b are strictly at right angles to each other and the incident beams, a more or less bright spot will be seen through the telescope, depending on the path difference between the beams arriving from the two mirrors. The effect essentially relies on that the beams are just very narrow rays from the same source. If wider beams were used, the telescope would reveal alternating dark and bright concentric bands which are called the interference fringes of equal inclination. However, the concentric fringes were not observed in the real apparatus because the absolutely perfect adjustment of the mirrors is infeasible. The observer could see alternating dark and bright bands, the interference fringes of 'equal thickness'. Their origin is similar to that of the interference fringes appearing in the reflection of a parallel beam of light from a vertical soap film. Any slightest changes in the conditions of light propagation to the mirrors and back result in a displacement of the interference pattern.

The idea of the experiment Michelson conceived consisted in the following (Fig. 26). Let the interferometer be first oriented in a manner such that the beam ah is parallel to the Earth's motion along its orbit, and the beam ab_1 is perpendicular to the orbit. Then, according to the concept of a stationary ether, the beam sa will be reflected along ab, the tangent of the angle b_1ab being equal to v/c (considering that $v \ll c$, this tangent may be taken equal to the angle). The returning beam will follow the path ba_1 (it being that the angle $aba_1 \simeq 2v/c$). The beam ah will return from the mirror h by the erstwhile route but, on reflecting from the semitransparent plate

m at point a_1, it will follow a_1e. The angle

$$ha_1e = \frac{\pi}{2} - bab_1 = \frac{\pi}{2} - \frac{v}{c}$$

and, therefore, the second beam will travel in the same direction as the first.

Let us try and find the path difference between aba_1 and aha_1, supposing that the arms of the interferometer are exactly of the same length D. The time the light takes to travel from a to h and back equals (see (6.2))

$$t_1 = \frac{2Dc}{c^2 - v^2}.$$

The path traversed by the light in stationary ether during the time is

$$l_1 = t_1c \simeq 2D\left(1 + \frac{v^2}{c^2}\right).$$

The value of l_1 here is determined by neglecting the fourth order terms in v/c and above, I have made use of the relationship

$$(1 + x)^\alpha \simeq 1 + \alpha x \quad \text{if } x \ll 1.$$

It is easy to see that the path of the other beam is

$$l_2 = 2D\sqrt{1 + \frac{v^2}{c^2}} \simeq 2D\left(1 + \frac{1}{2}\frac{v^2}{c^2}\right).$$

Therefore, the path difference between the beams in a stationary ether should be

$$l_1 - l_2 = D\frac{v^2}{c^2}.$$

Now suppose that the interferometer slowly rotates around its vertical axis passing through the middle of the plate m. The conditions for light motion will be changing with the rotation of the apparatus, and hence the interference pattern will change: the fringes within the field of vision of the telescope will be displacing. For one, if the interferometer is turned by 90° relative to the initial position, the path difference between the two beams will be

$$l_1 - l_2 = -D\frac{v^2}{c^2}.$$

Hence the maximum displacement of the interference fringe is defined by the quantity of the path difference of

$$2D\frac{v^2}{c^2}.$$

Michelson's idea was that the experimenter should register the position of the interference fringes several times each day at different orientations of the interferometer arms.

Michelson needed funds to put his idea to the test of a decesive experiment. Dorothy Michelson-Livingston, Michelson's daughter, wrote:

> "Newcomb promptly brought Michelson's need to the attention of Alexander Graham Bell. Before patenting his telephone, Bell had also been pressed to find financial backing. He took an immediate interest in Michelson's idea and arranged to make him eligible for a grant from the Volta Foundation, which Bell had recently established for such purpose with French prize money he had won."

The necessary funds (£ 100) were furnished, and the first interferometer, as Michelson had designed it (Fig. 27), was produced at a German firm of instrument makers, although the precise optical flats were obtained from 'Maison Bréguet' in Paris. The source of light was a special lantern allowing Michelson to experiment with monochromatic sodium yellow line at $\lambda = 5890$ Å. The two optical paths were each $D = 120$ cm. The interferometer had two plates,

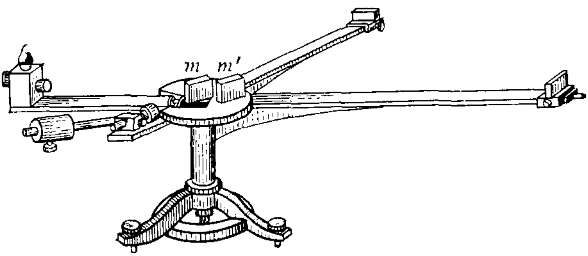


Fig. 27. A general view of the first Michelson's interferometer. m is a semitransparent plate ('beam splitter') and m' is a compensating plate.

rather than only one, at the centre. The second plate m' was cut from the same piece of glass as the plate m but was, unlike m, transparent. The additional plate of glass was employed because of the need to compensate for a path difference resulting from that one of the beams had to pass through the lightly silvered semitransparent plate twice while the other beam traversed it only once (the calculations did not take account of the breadth of this plate).

The interferometer was first put to test in Helmholtz's laboratory in Berlin. Michelson had discussed the experiment with Helmholtz who

Albert Michelson (1852-1931)

said that the difficulty was to keep a constant temperature. Michelson wrote that Helmholtz

> "doubted if they had the facilities for carrying out such experiments on account of the necessity of keeping a room at constant temperature.
>
> "With all due respect, however, I think differently, for if the apparatus is surrounded with melting ice, the temperature will be so nearly constant as possible."

However, when the apparatus was set up on a stone pier in the Physicalisches Institute of the University of Berlin, vibrations due to street horse-drawn traffic made observation of the interference fringes wholly impossible, except during brief intervals after midnight. So the apparatus had to be taken to the Astrophysicalisches Observatorium at Potsdam, where the delicate instrument was arranged in the cellar, whose circular walls formed the foundation for the pier of the large equatorial telescope. Only then Michelson really started the measurements. He wrote:

> "Here, the fringes under ordinary circumstances were sufficiently quiet to measure, but so extraordinary sensitive was the instrument that the stamping on the pavement about 100 meters from the observatory, made the fringes disappear entirely!"

Now what was the result the experiments produced? It was in the negative: no displacement of fringes possibly related to the hypothetical ether-drift was detected. However, was it really feasible to discover the effect with the aid of the first Michelson's interferometer? To answer this question, let us discuss the parameters of this apparatus. The fringes were viewed and measured on a scale ruled on glass in the small telescope, which was focussed on the fringes. The distance between two adjacent fringes was that between four rulings of the scale, and the position of the centre of a dark band could be registered within 1/4 of a division. In the final analysis, the accuracy of detecting the displacement of a fringe was 1/12 of the bandwidth. It can readily be calculated that the expected fringe displacement related to the path difference due to the rotation of the apparatus was $2Dv^2/(\lambda c^2) \simeq$ $\simeq 0.04$. Therefore, the anticipated shift was half as much as the accuracy of measurements. Hence, the experiment was inconclusive! Notwithstanding, Michelson published the results, and although he observed shifts in the position of the interference fringes when the apparatus was turned in azimuth, they were smaller than anticipated and, moreover, did not show the proper phase relationship to the Earth's motion, Michelson concluded:

> "The interpretation of these results is that there is no displacement of the interference bands. The result of the hypothesis of a stationary ether is thus shown to be incorrect."

This was the end of Michelson's European experiments. After completing the Potsdam research, Michelson remained for more than a year *in Europe*. Having written up the Potsdam experiment for publication, he spent the summer semester at Heidelberg attending the lectures of Professors Quincke and Bunsen on various aspects of optics. While Michelson was still in Europe, he was appointed Instructor in Physics in the Case School of Applied Science [in Cleveland, Ohio] at a salary of $ 2000 per annum and therefore resigned his commission in the U.S. Navy. In 1882 he returned to the United States and started his work at Case. He set about arranging a physics laboratory there, and this is where he made his research providing the basis for the application of the concepts of phase and group velocities to light. Working and giving lectures at Case, Michelson made the acquaintance of Professor Edward Morley (1838-1923), the distinguished chemist of Western Reserve University. When, having been urged by both Lord Rayleigh

Edward Morley (1838-1923)

and Sir William Thompson, Michelson decided to give another trial to the fundamentally important problem of ether-drift, he did not hesitate in turning to Morley for collaboration.

I have already mentioned the first result obtained by Michelson and Morley, the corroboration of Fizeau's conclusions on the propagation of light in moving media. After these tests were completed, the colleagues decided to repeat the Potsdam experiment. However, they only used the basic idea and they completely redesigned the apparatus. It was a masterpiece of the experimental state-of-the art.

Figure 28 shows a cross-section of the apparatus of the Michelson-Morley experiment. The interferometer was set up in a basement laboratory of the Case Main Building, a room having heavy stone walls and rather constant temperature conditions. The optical parts were mounted on a heavy sandstone slab 150 × 150 cm (five feet

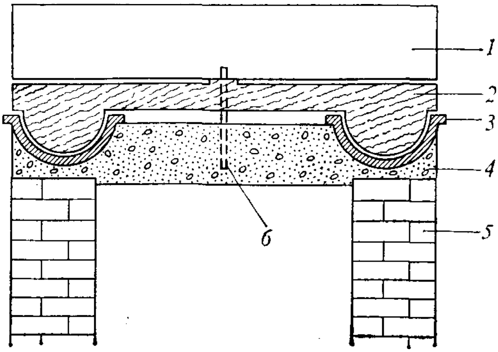
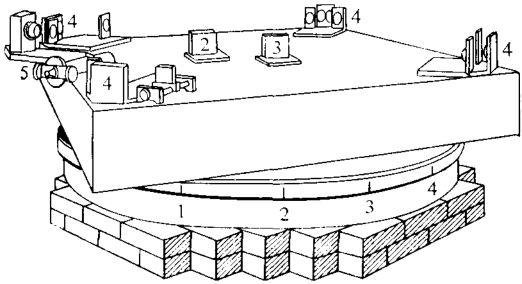


Fig. 28. The Michelson-Morley apparatus. *1*—sandstone slab, *2*—wooden float, *3*—trough filled with mercury, *4*—cement foundation, *5*—brick foundation, *6*—centering pin.

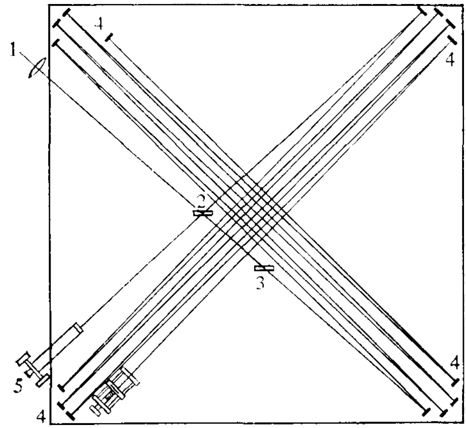
square) and 30 cm thick. It was placed on a doughnut-shaped wooden float supported by mercury contained in an annular cast-iron trough. The walls of the trough were barely 1.5 cm thick. The mercury-bearing removed practically all stresses and vibrations that had been so troublesome in Europe. The wooden float underneath the sandstone slab was prevented from bumping into the sides of the cast-iron trough by a centering pin and thus was always aligned. The trough was supported by a cemented brick pier resting on a basement which reached the bedrock underground. On the outside of the mercury tank, there were sixteen numbers indicating the orientation of the slab. The pin was engaged only while the interferometer was being set into rotation, and, once started, the apparatus would continue to turn freely for hours at a time.

A general view of the interferometer and the paths of light in it are shown in Fig. 29. In order to increase the general path travelled by the light to about 1100 cm, i.e., some 10 times more than in the Potsdam experiment, a system of mirrors was employed. They were made of a special alloy of tin, copper, and arcenic, and there were four of them in each of the four corners. The optical arrangement of the interferometer was protected from any extraneous influence by a wooden box, which is not shown in Fig. 29.

The adjustment of the apparatus took several months, and then the Case Main Building suffered a disastrous fire in October 1886. Luckily, the apparatus for the Michelson-Morley experiment was rescued by the students living in the nearby Western Reserve University dormitory known as Adelbert Hall, and the equipment was some time later reestablished in the southeast corner of Adelbert Hall basement. Finally, in July 1887, Michelson and Morley were able to make their definitive observations. The slab on the mercury cushion made a complete revolution in about six minutes once it had been gently pushed, and the observer kept walking around the apparatus and defining the position of the fringes at the sixteen orientations of the interferometer. The experiments which gave the published



(a)



(b)

Fig. 29. (a) A general view of the Michelson-Morley interferometer. (b) The light paths in the interferometer. *1*—light source, *2*—semitransparent plate, *3*—compensating plate, *4*—system of mirrors, *5*—telescope.

data were conducted at noon and at six o'clock p.m. of the days of 8, 9, 11, and 12 July 1887. The new interferometer allowed Michelson and Morley to measure very small displacements of

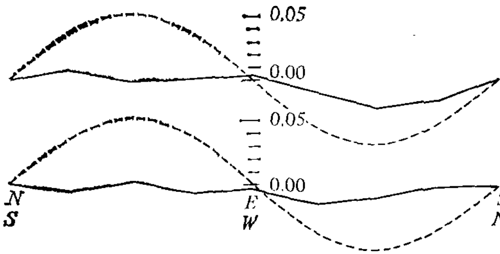


Fig. 30. The results of Michelson and Morley's measurements. The graph shows displacement of the fringes (expressed in parts of the fringe width) as a function of the interferometer orientation. The broken curve was calculated proceeding from the stationary ether theory (its ordinates were reduced by 8 times). The solid line connects the experimental points obtained in various orientations of the interferometer. The top graph shows noon data and the bottom one corresponds to 6 p.m. observations.

the fringes: the width of one fringe corresponded to 50 divisions of the ruled scale, thus making possible to determine a shift of 0.01 of the fringe width.

The processed data of the measurements are presented in Fig. 30. The graphs demonstrate that the observed displacement was less than 1/20 of that anticipated by the theory of stationary ether (and even possibly less than 1/40). The apparent shifts may be accounted for by periodical variations in the temperature, pressure, and other external factors whose influence could not be suppressed entirely.

Michelson wrote to Lord Rayleigh:

> "The experiments on the *relative motion* of the earth and ether have been completed and the result is decidedly negative."

The result of the Michelson-Morley experiment unambiguously evidenced the invalidity of the stationary ether hypothesis. This conclusion was a sensation in the world of science. It required a thorough review of the most essential concepts of physics. To explain the absence of ether-drift, Lorentz and Fitzgerald (independently) put forth an idea on the contraction of every material body in the direction of its motion. Although the Lorentz-Fitzgerald hypothesis had no reliable physical substantiation, its appearance indicated that the Michelson-Morley experiment results became a fundamental criterion of the validity of any physical theory concerning the properties of space and time.

Michelson and Morley sent a paper on the obtained results to the *American Journal of Science*. It was titled *The Relative Motion of the Earth and the Luminiferous Aether*. It received a shower of reviews from many countries. Far from everybody was enthusiastic over the conclusions of the American physicists. Primarily, there were doubts in the correctness of the interpretation of the received data. Was it possible to employ simplified considerations like the ones I used when calculating the delay of one of the beams? Some of the physicists probably recalled that in 1981, when Michelson had presented his first publication on the ether problem, he made an error due to which he had the magnitude of the delay twice as great as it really was. Possibly, there was another mistake in his new publication. Physicists thoroughly checked the outcome of the experiment for a probable influence of various factors: the width of the light beam, the position of the mirrors and the lightly silvered plate, the reflection from the moving mirrors, monochromaticity, etc. Although in the long run the validity of the simplified interpretation of the experiment which Michelson had given was established, Lorentz wrote to Lord Rayleigh six years after the experiment:

> "I am utterly at a loss to clear away this contradiction (i. e., the null result of the experiment.— *Author*), and yet I believe that if we were to abandon Fresnel's theory, we should have no adequate theory at all, the conditions which Mr. Stokes has imposed on the movement of the aether being irreconcilable to each other.
>
> "Can there be some point in the theory of Mr. Michelson's experiment which has as yet been overlooked?"

Lord Kelvin discerned the result of the experiment as part of a cloud obscuring

> "the beauty and clearness of the dynamical theory, which asserts heat and light to be modes of motion."

Lord Rayleigh called this result 'a real disappointment'. Even Michelson himself was evidently discouraged by the obtained data. Many years later he wrote:

> "The experiment is to me historically rich because it was for the solution of this problem that the interferometer was devised. I think it will be admitted that the problem, by leading to the invention of the interferometer, *more* than compensated for the fact that this particular experiment gave a negative result."

The formulation of the Nobel Committee, which conferred on Michelson the Prize in 1907

"For his optical precision instruments and for the spectroscopic and metrological investigations made with them", was a reflection of the general attitude towards the results of this historical experiment.

However, the fundamental nature of experiments in quest of the ether-drift was clear, and attempts to reveal it continued. Michelson verified the hypothesis that Earth drags ether almost completely so that the ether's relative velocity near the Earth's surface is small or equal to zero. In the mid-1890s he observed the interference of two beams of light passing over the perimeter of a vertical rectangular whose sides were 15 and 60 m long. The obtained results did not corroborate Michelson's initial guess.

Professor D.C. Miller at Case went through a prolonged verification of the Michelson-Morley experiment. Starting in 1897, first with Morley and then alone, he tried for almost 30 years to disprove the conclusion that the stationary ether theory was invalid. In the late 1920s, some of his data allowed the opponents of the theory of relativity to assert that Albert Einstein's theory was inconsistent. However, later it was shown that Miller's positive results were produced by side-effects.

In 1922 R. Kennedy, a young American physicist, introduced a number of improvements into the Michelson apparatus. However, the further increase in the accuracy of the experiment only resulted in the registration of the interference fringe displacement by 0.001 of its width, whereas the anticipated shift was 0.07 of the fringe width. Kennedy's experimental ingenuity was appreciated by Michelson. He told to Kennedy:

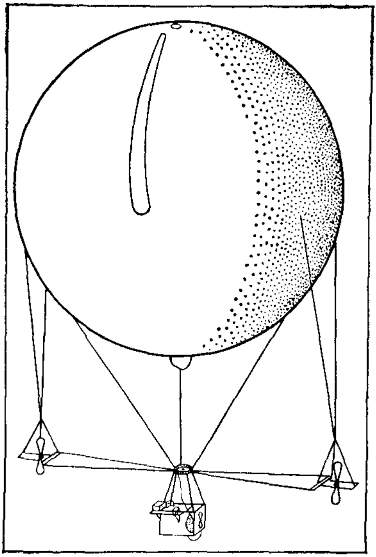


Fig. 31. Piccard's air balloon with the suspended apparatus for the Miche son experiment.

> "Your work renders my own quite superfluous. I should not have undertaken it had I known you were doing it so well."

In the late 1920s, the Swiss physicist Auguste Piccard (1884-1962) and the Belgian scientist E. Stahel repeated the Michelson-Morley experiment with the aid of a special smaller interferometer suspended under a balloon which ascended to 2.5 km (Fig. 31). The interference pattern was automatically photographed during the balloon's uniform rotation produced by miniature propellers. And this time again no ether-drift was revealed.

The results of the quest for the ether-drift were summed up at a conference held on February 4 and 5, 1927, at the Mount Wilson Observatory in California, USA, where Michelson had been working for some time. Together with Michelson, the conference was attended by Lorentz, Miller, Kennedy, and a number of other eminent physicists. On the whole, the conference helped strengthen the positions of the theory of relativity because no one presented any serious proofs questioning the negative results of the numerous repetitions of the Michelson-Morley experiment.

Later this experiment was repeated many times. Its accuracy was enhanced when new physical devices such as lasers, masers, and ultra-high-frequency generators were employed. One of the latest attempts to discover the ether-drift revealed that the ratio of the velocity of light in the direction of the Earth's motion to the velocity of light in the direction at right angles to the Earth's motion differs from unity no more than by 10^{-15}. To date, this is the most accurate repetition of the experiment which gave, in the words of the English physicist and philosopher John Desmond Bernal

> "the greatest of all negative results in the whole history of science".

* * *

We have considered in detail the experimental aspect of the quest for the ether-drift effects. Now what is the significance of the obtained negative results for the physical theory?

The Michelson-Morley experiment found its natural explanation in 1905 when Albert Einstein

Albert Einstein (1879-1955)

(1879-1955) published his work *On the Electrodynamics of the Moving Bodies*, in which he presented the fundamentals of the special theory of relativity.

Unlike his predecessors, Einstein did not produce any hypothesis on the influence of the moving bodies on the ether. He was bold and resolute: he refused to consider the ether as a physical object entirely. Einstein proceeded from two basic postulates in his theory. The first one, the relativity principle, can be formulated as follows:

> The course of any physical phenomenon is the same in inertial frames of reference, provided the initial conditions are the same.

In fact, this postulate extends the Galilean relativity principle over *all* natural phenomena. Although the results obtained by Michelson and Morley appear to have had no decisive role in the creation of the Einstein's special theory of relativity, they were an essential factor in the recognition of this theory by many physicists. There is no ether-drift, and this means that there is no special 'absolute' frame of reference; this indicates that the relativity principle can be applied to the electrodynamics as well. This is the basic significance of the Michelson-Morley experiment.

Einstein's second postulate is:

> The velocity of light in vacuum is the same in all inertial frames of reference in all directions and depends neither on the velocity of the source nor on the velocity of the observer.

This postulate brought about the most virulent discussions following the publication of Einstein's work. I'd like to emphasize that the results of the Michelson-Morley experiment cannot be considered as a corroboration of this postulate on the whole. The source in this experiment is *stationary* with respect to the observer, and therefore the experiment says nothing about the relationship between the velocity of light and the relative motion of the source and the observer.

Evidently, the second postulate is in contradiction with the classical rule of composition of velocities. Physicists were so accustomed to this rule that they could not believe that it had a limited validity, i.e. the classical rule of composition of velocities can only be applied if the component velocities are much less than the velocity of light. After Einstein's publication, attempts were made to produce theories in which the relativity principle was preserved while the second postulate was substituted by another, more traditional one, and which was in conformity with experiment. Thus the Swiss physicist Walter Ritz (1878-1909) advanced his 'ballistic' hypothesis in 1908. According to this hypothesis, the velocity of light emitted by a moving source is greater, with respect to a stationary observer, than the velocity of light emitted by a stationary source by the magnitude of the source velocity. Naturally, Ritz had to discard the Maxwell field equations. However, Ritz's theory was not accepted: it was refuted by astronomical observations carried out in 1913 by the Dutch astronomer Willem de Sitter (1872-1934).

Astronomers had long since known of binary stars, systems containing a pair of stars sufficiently close together in space to attract each other gravitationally so that they orbit around their common centre of gravity. In order to see the gist of the idea disproving the 'ballistic' hypothesis, let us consider a star from a binary system. Suppose this star moves along a nearly circular orbit (Fig. 32). If Ritz's theory were right, then it takes the light from the star at point A an interval

$t_1 = \frac{L}{c+v}$ to reach the Earth, where L is the distance from the star to the Earth and v is the linear velocity of the star in its orbit. The light emitted by the star at point B will reach the Earth in time $t_2 = \frac{L}{c-v}$.

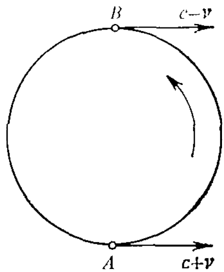


Fig. 32. To the calculation of the delay of light according to Ritz.

If we define T to be the period of semirevolution, then the period of semirevolution measured from the Earth as the interval of the apparent motion of the star from A to B equals $T + \frac{2Lv}{c^2-v^2}$, the corresponding period measured for the star moving from B to A equals $T - \frac{2Lv}{c^2-v^2}$. Since the distances to the stars are very great, the value $\frac{2Lv}{c^2-v^2}$ is comparable to T even if $v \ll c$.

It follows that if Ritz's hypothesis were valid, then observers on the Earth should see deviations from Kepler's laws in the motion of binary systems. But no such deviations have been observed. This militates against the hypothesis that the velocity of the star and the velocity of light can be composed. De Sitter used all the astronomical data then available to show that if the velocity of light c' in the frame of reference of the observer is presented as $c' = c + kv$, then it can be asserted that $k < 0.02$. The outstanding Austrian-born physicist Wolfgang Pauli (1900-

1958), who worked both in Europe and the USA, wrote in his book *The Theory of Relativity* that these data

> "allow us to consider the postulate of the constancy of the velocity of light to be almost certainly correct and regard the theory of emission offered by Ritz and others as leading to insurmountable difficulties".

Other experiments carried out to verify the second postulate of the special theory of relativity are known as well. Thus in 1923 R. Tomacek performed measurements in keeping with the Michelson-Morley scheme, but as to the source of light he used the moving astronomical bodies (the Sun, the Moon, Jupiter, Sirius, and Arctur). The observed displacements of the interference fringes amounted to no more than 1/8 of the theoretically anticipated ones.

The experiments conducted to verify the second postulate continue to date as well; the latest achievements of the physical science and technology are employed to carry them out.

The attention paid to the special theory of relativity by the scientists since its appearance can be accounted for by the unexpected and unusual inferences to which it leads. Analysing the space-time relations between events registered in different frames of reference, Einstein concluded that the classical rule of the composition of velocities had to be replaced by a new rule: if one inertial system K' moves with respect to another system K with the velocity v, then the signal propagating in the system K with the velocity u along the direction of the relative motion of the two systems will move, from the viewpoint of the observer stationary in the system K, with the velocity

$$u' = \frac{u+v}{1+\frac{uv}{c^2}} \tag{6.3}$$

(provided that the signal in the system K moves counter to the motion of the system K'). After contemplating this law, Einstein saw that it was impossible for a signal to propagate with a velocity greater than the velocity of light in vacuum in any inertial frame of reference. Indeed, it is easy to see that even if $u = c$, $u' = c$. *In principle*, this conclusion is not a purely logical corollary of the special theory of relativity. Regarding the possibility of transmitting signals with a velocity greater than the velocity of light in vacuum, Einstein wrote:

> "This result signifies that we must consider as possible a transmission mechanism that allows the intended action to precede the cause. Although from a purely logical point of view this result does not contain, in my opinion, any contradictions, yet it clashes so much with the character of our whole experience, that the impossibility of the assumption $v > c$ appears thereby to be sufficiently proven."

Therefore this Einstein's conclusion is based on the causality principle which is a generalization of our whole *experience*.

This does not mean, however, that the velocities greater than c are impossible within the special theory of relativity. Let us consider the propagation of a light impulse from point O to points A and B (Fig. 33). Assume that the distances OA and OB are so great that the impulse front

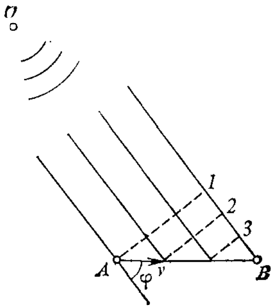


Fig. 33. A short light impulse emitted at point O runs, on reaching point A, along the straight line AB with the velocity $v = c/\cos\varphi$.

close to A and B can be consi'ered fla . The broken lines *1*, *2*, and *3* in Fig. 33 show the position of the impulse front for three consequtive intervals $t_1 < t_2 < t_3$. It is clear that the impulse first reaches point A (position *1*). Further prop-a_ation of the im_ulse can be regarded as its motion from A to B with the velocity $v = c/\cos\varphi$ (we are following the displacement of the point of intersection between the impulse front and the straight line connecting A and B). Evidently, the velocity of the impulse in this presentation can exceed c if φ is close to $\pi/2$. The impulse running from A to B is quite real: if we place a reflector in the path AB, a flash of light can be registered when the impulse arrives at it. However, the example we considered is not contrary to the special theory of relativity in any way because the theory does not forbid motion that is faster than light but only the *transmission of a signal* with a velocity greater than c. The impulse from A to B does not carry any information about point A and cannot be regarded as a *signal* from point A.

And still the problem of the existence of physical objects moving faster than light has not been finally solved. Thus during the past fifteen years or so, scientific journals have been publishing papers discussing the existence of super-relativistic particles. The most well-discussed are hypothetic particles possessing imaginary mass, which the American physicist Gerald Feinberg (born 1933) gave the name of tachyons.

Much theoretical work has been performed to analyse the consequences of the tachyon hypothesis, there were attempts to explain the results of a number of experiments by the existence of these super-relativistic particles.

However, to date the reality of tachyons has not been confirmed; moreover, the available experimental data testify against the idea of particles moving faster than light.

Nevertheless, one cannot say that a super-relativistic velocity is just a product of theoretical assumptions and inference similar to the example cited above. Fairly recently, astrophysicists were puzzled by observations carried out by means of the latest method in radioastronomy: very long base radiointerferometry (VLBI). Its implementation requires careful coordination of a network of radiotelescopes at distances as long as hundreds and thousands of kilometers from each other. The data processing obtained from individual telescopes is carried out by latest computers.

The main advantage of the method is that it allows scientists to study radioastronomic objects with a resolution reaching 0.0001″. Owing to so great a resolution, unavailable in the optical range, a number of most interesting objects have been investigated during the recent years, in particular the quasi-stellar objects, or quasars. They were discovered some twenty years ago, however to date, despite the considerable accumulated observation material, the nature of quasars has not been made clear. The quasars are characterized by very small angular dimensions because they are removed from us so far that it takes light several thousands of millions of light years to travel from them. These objects are sources of energy whose power exceeds the radiation power of whole galaxies, but the source of this immense energy is still enigmatic.

One way to clarify the mechanism of quasar radiation is to study the details of their structure, or, as radioastronomers say, quasar mapping. The implementation of the VLBI method made it possible to compile radio maps of some quasars, and while compiling one of such maps astrophysicists had to deal with a super-relativistic velocity.

The American scientists from the California Institute of Technology have been using this advanced technique to investigate the quasar 3C 273 since 1977. They have found out that the radioimage of this quasar seemed to consist of two parts: a nucleus and a small jet at a distance of about 0.006″ from the nucleus. Observations carried out during the period from July 1977 to July 1980 showed that the jet had traversed a distance of 25 light years with respect to the nucleus. This seemingly evidenced the motion of the jet with a velocity exceeding that of light by more than 9 times.

However, before long scientists drew a conclusion that this was only an 'apparent' velocity of the jet. At first glance the results of observations were amazing, but the reason may be that the jet moved almost directly to the Earth with a velocity amounting to 0.995 of the veloc-

[illegible]ty o[illegible] [illegible]g[illegible]. Here [illegible]s an interpretation of the data.

Imagine that a jet moving with a velocity of $0.995c$ along a straight line at a small angle to the direction of the Earth, emits two radiation impulses with an interval of 1 year in succession. How long will [illegible] rv[illegible]l b[illegible] arrival of these impulses to the Earth be? Since the jet moves almost directly to the Earth with a velocity close to that of light, the second impulse will 'lag' behind the first by the time much less than one year, namely, only 3.5 days. The reason is evident: during the time between the moments of emission of the two impulses, the jet will come closer to the Earth by the distance almost equal to one light year, and therefore the second impulse will have to traverse a less distance to the Earth than the first one. The recording of the impulse by an observer on the Earth is, in effect, the determination of the position of the jet. When he receives the two impulses, the observer records the two apparent positions: *1* and *2* (Fig. 34). The displacement of point *2* with respect to the direction *1*, the Earth (i.e., the segment *A-1*), may turn to be considerable, for instance 0.1 light years. Then the situation will

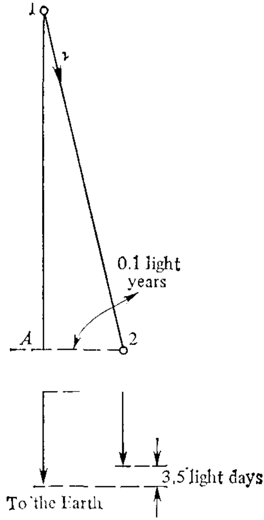


Fig. 34

seem for the observer that the jet moves in the direction perpendicular to the straight line 'jet-Earth' with the velocity exceeding *c*: it takes 3.5 days ($\simeq$ 0.01 years) for the jet to cover the distance of 0.1 light years. This is apparently what there is in the case of the quasar 3C 273, but the interval between the impulses registered on the Earth was 300 years rather than one year, and the interval between the moments of recording the signals from the jet on the Earth amounted to 3 years while the shift *A-1* was 25 light years. Consequently, the status of the velocity of light in vacuum as the greatest possible velocity for the propagation of a signal has been preserved.

Chapter 7

The Multifarious Constant

Let us leave for a time the problems engaging the physicists of today and return to the problems which interested the scientists of the late 19th century.

The theory of electromagnetic field created by Maxwell both solved a number of serious problems and posed a series of new ones. As to the story of the velocity of light, one of the fundamental questions was whether the electromagnetic waves predicted by the Maxwell theory exist in reality. The answer could only be provided by a crucial experiment.

In 1879 the Berlin Academy of Sciences announced a competition whose aim was an experimental proof of the existence of a magnetic field caused by an alternating electric field. Hermann Helmholtz pointed out this problem in a talk with his pupil Gustav Hertz. Hertz was looking for a topic for his research at the time and he was happy to take up the problem suggested by his renowned teacher. However, at the outset Hertz did not share Maxwell's viewpoint on the electromagnetic phenomena.

Hertz set about studying the phenomenon of electromagnetic induction appearing at a discharge of a Leyden jar, i.e., a capacitor. It was far from being accidental that he took up this re-

Heinrich Rudolph Hertz (1857-1894)

search. Back in 1847, Helmholtz wrote his book *On the Conservation of Force*, where he asserted that the discharge of a Leyden jar occurs in the form of oscillations. To date the reason for it is quite clear: since the conductors, by means of which the plates of the jar are short-circuited, possess certain inductance, the Leyden jar and the external conductors define an oscillatory circuit. In 1853 William Thomson (Lord Kelvin) gave a theoretical account of this phenomenon and calculated the period of the respective oscil-

[illegible]:

$$T = 2\pi \sqrt{LC}, \qquad (7.1)$$

where L and C are the inductance and capacitance of the circuit, respectively.

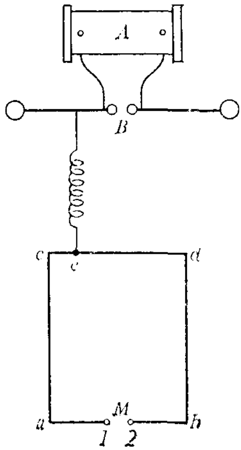


Fig. 35. The schematic of the first experiment conducted by Hertz. *A*—induction coil, *B*—primary discharger (oscillator), *abdc*—secondary circuit with discharger *1-2*, *e*—point of connection.

Prior to Hertz, in the late 1850s-early 1860s, the German scientist Wilhelm Feddersen had succeeded in his experimental research on the electromagnetic oscillations. He managed to observe oscillations whose period was about 10^{-6} s. It is interesting that in order to determine the period of oscillations Feddersen employed the rotating mirror method that had been suggested by Wheatstone. However, Feddersen introduced an essential change into the technique: he registered the images of the sparks on a photographic film.

At the outset, Hertz was interested in the electromagnetic induction of the discharge in one circuit effecting the processes occurring in another circuit. The schematic of his first experiment is shown in Fig. 35. A high-voltage Ruhmkorff coil *A* was connected to a spark gap (oscillator) *B* consisting of two rods with metal balls at their ends, and induced a 'shower of sparks' in the spark gap between the poles. A conductor connected one of the poles to a circuit with a spark gap *M*. The distance between the balls of the circuit *abdc* could be changed by means of a micrometer. At the first stage of the research, the main advantage that Hertz achieved was that he succeeded in creating electric oscillations whose period was a hundred times less than the period of oscillations studied by Feddersen. Hertz described the result of his experiment as follows:

> "During the action of the induction coil, we shall... observe in the micrometer [*1-2* in Fig. 35.—*Author*] a shower of sparks, sometimes reaching several millimeters in length.
>
> "This experiment shows, firstly, that intensive electrical motions at the moment of discharge occur not only in the spark gap but also in all conductors connected to it, and secondly, it reveals ... that these motions happen very rapidly and therefore we have to take into consideration even the interval during which the electric waves travel via short electric conductors."

The explanation of the result of Hertz's experiment is that a change in the potentials at various points in the circuit *M* does not occur simultaneously, because the propagation of the electric waves takes time. That is why there occurs a difference of potentials between the small balls *1* and *2* sufficient to produce an electric discharge. A confirmation of this reasoning is the fact that when a conductor connecting two circuits at point *e* is symmetrical with respect to balls *1* and *2*, no sparks are observed in the micrometer.

The Hertz experiment, as it has been described, is in effect a modification of the Wheatstone's experiment. However, while proving that the velocity of the propagation of electric disturbances was the final stage of Wheatstone's research, the experiment carried out by Hertz was the beginning of a vast series of new investigations.

First of all, Hertz found out the conditions at which the effect of one oscillatory circuit on the other is the most effective. He managed to show that these conditions hold in resonance, i.e., in the coincidence of the period of oscillations in the two circuits. Very soon Hertz saw that the connecting conductor was not necessary for the appearance of the sparks in the micrometric circuit while the reciprocal orientation of the primary and secondary circuit was essential.

This result appears to have played the main role when Hertz set about verifying the conclusions of the Maxwell theory. Indeed, when the disturbances were transferred over conductors, the results of the experiments could be more or less successfully accounted for both by means of the Maxwell field concept and within the framework of the long-range theory. However, the reciprocal effect of the circuits at a distance of 1.5 m between them was hardly in conformity with the conclusions of the long-range theory. At the same time, the discovery of transfer of electric disturbances without conductors could not yet be regarded the proof of the existence of the Maxwell electromagnetic waves: the theory of the electromagnetic field predicted the existence of waves of a certain type characterized by peculiar properties. The next stage in Hertz's research was studying the properties of the waves he discovered.

There is no place here to describe the diverse classical experiments Hertz conducted to prove that electromagnetic waves possess all the features predicted by the Maxwell theory. Let us only dwell on the experiments Hertz performed to determine the propagation velocity of his waves.

Obviously, the *existing optical methods* were inadequate for measuring this velocity because the intensity of the wave source was too small for the experimenter to use long distances. But the fact that the wavelength was about 10 m was a blessing in disguise.

In the course of his experiments, Hertz noticed that the reflection of the electromagnetic waves from the walls of the room gave rise to the so-called *standing* wave. The gist of this phenomenon is that in the space between the source of the waves and the reflecting surface of the wall there appear two waves, the incident and the reflected waves, travelling in the opposite directions. Naturally, these waves have the same frequency, and besides, the phase difference between the oscillations produced by the waves at a certain point in space is constant. Owing to such superposition of the waves, interference takes place. Mathematically speaking, this phenomenon can be described as follows. Let two waves defined by equations

$$E_1 = E_0 \cos(\omega t + kx), \tag{7.2}$$

and

$$E_2 = E_0 \cos(\omega t - kx - \pi) \tag{7.3}$$

propagate along the x-axis in the opposite directions.

Assume that the reflection of the wave E_1 occurs at point $x = 0$; this reflection gives rise to wave E_2. When a wave is reflected from an obstacle, the phase shift between the incident and the reflected waves depends on the conditions of reflection. In the case of an electromagnetic wave being reflected from a metal screen, the direction of the electric field vector reverses (i.e., the wave phase changes by π). Since this is what took place in Hertz's experiments, we shall in future use this notation for the reflected wave. It will also be necessary to recall that $k = 2\pi/\lambda$. The resulting oscillation at point x, where $x > 0$, is determined by the expression

$$E = E_1 + E_2 = -2E_0 \sin \omega t \sin kx. \qquad (7.4)$$

It is clear that the obtained expression is essentially different from equation (7.2) of the travelling wave. One of the differences between the standing and the travelling waves is the fact that $E = 0$ at points

$$x = \frac{n\lambda}{2} \quad (n = 0, 1, 2, \ldots)$$

at any values of t. These points are called the *nodes* of the standing wave. At points

$$x = \frac{2n+1}{4} \lambda$$

the quantity E periodically takes the values of $2E_0$ and $-2E_0$. These points are referred to as *antinodes* (*or loops*) of the standing wave. Evidently, if one measures the distance between two neighbouring nodes (or antinodes) or between a node and the adjacent antinode, one can determine the wavelength λ. If there is any way to measure independently the period T of oscillations, one can find the phase velocity of the initial wave

$$v = \frac{\lambda}{T}.$$

As to optics, this method of determining the wave velocity is unacceptable because it is difficult to measure independently the period of oscillations of a light wave. Things were much easier with the Hertzian waves. The oscillation period T can be determined from the Thomson formula (7.1) by the values of the capacitance and inductance of the oscillator, the source of the wave. These values can be relatively easily deduced from the geometrical dimensions of the oscillator.

Hertz conducted his experiment in the following manner. He fixed a sheet of galvanized iron on the wall and grounded it at various points in order to avoid the accumulation of charge due to insufficiently great conductivity of the metal. The oscillator was mounted at the opposite wall of the room, at a distance of about 10 m from the screen. A small vibrator with a spark gap served as a registering instrument. Hertz changed the orientation of the spark gap in both the receiver and the source, as well as the position of the receiver, in the course of his experiment. There were no sparks observed in the receiver when it was near the screen. While the receiver was being removed from the screen, the sparks appeared and their intensity was increasing up to the distance of 2 m from the screen; when the receiver was removed from the screen somewhat farther, the sparks began to diminish. The complete disappearance of sparks was observed at a distance of 4 m from the screen. Proceeding from his observations, Hertz arrived at the picture of the standing wave shown in Fig. 36. The nodes on the graph are designated C and A while the antinodes are noted by letters B and D. The displacement of the leftmost node of the wave in

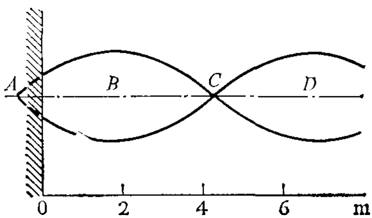


Fig. 36. The Hertzian standing wave. A and C are nodes, and B and D are antinodes.

the picture behind the 'mirror' is accounted for by the finite conductivity of the material of which the screen was made (the change of the wave phase in the reflection is not exactly equal to π).

I have described only the general idea of the method of measurements employed by Hertz. In actual fact, Hertz carried out many complementary experiments to receive the results allowing for an unambiguous interpretation, and he was able to determine the wavelength. However, while calculating the capacitance of the oscillator he made an error, which he later called a 'fatal' one: the calculated capacitance exceeded the real one by a factor of 2. This resulted in the velocity of the waves.

$$c = 200{,}000 \text{ km/s},$$

which is 1.5 times less than the true velocity. The French mathematician and physicist Henri Poincaré was the first to find out in 1891 the error. The account of Poincaré's correction leads to the value of c close to the velocity of light in vacuum as it had been predicted by the Maxwell theory.

As I have said, Hertz proved that the waves produced by the oscillator also possess other properties characteristic of electromagnetic waves. He observed their interference and diffraction and proved that they are transverse. Performing his experiments with shortwave radiation ($\lambda = 0.6$ m), Hertz corroborated the validity of the laws of the geometrical optics when the wavelength is essentially less than the dimensions of the mirrors, obstacles, etc.

The most essential conclusion Hertz made was that the properties of light and electromagnetic waves were identical. The diffference between these waves was only in the techniques used to generate and record them which depended on their wavelengths. Following Hertz's experiments, physicists faced the problem of 'joining' the electromagnetic waves obtained in the experiments with oscillators and the longwave infrared radiation of heated bodies, and it took about forty years to solve this problem.

A considerable stride in this direction was made by a famous Russian physicist Pyotr Lebedev (1866-1912). He managed to decrease the wavelength obtained by means of the 'electric' methods to 6 mm (1895).

The credit for the 'joining' the limits of the Hertzian waves and the infrared waves is due to the Soviet woman physicist Aleksandra Glagoleva-Arkadieva (1884-1945). Using a special source ('mass radiator'), she succeeded in obtaining the wavelengths within the range from 82 μm to several centimeters (1922).

Late in the 19th century, after the Hertz's experiments, other discoveries expanded the spectrum of electromagnetic waves. In 1895 the German physicist Wilhelm Konrad Roentgen (1845-1923) discovered a new kind of radiation later called after his name; the *x*-ray radiation (also called roentgen rays) corresponds to the range of $\lambda = 10$ to 0.1 Å. At the turn of the century, in 1900, the French scientist Paul Villard (1860-1934) discovered radiation of still shorter wavelengths, the so-called gamma rays while researching radioactivity.

The turn of the century was not only marked by numerous interesting experimental discoveries. During the first decade of the 20th century, a new hypothesis on the nature of light was brought forward, the idea of the quanta of light (photons). According to this theory, light is a stream of particles, photons, moving with the velocity c and possessing energy $h\nu$ (h is the Planck universal constant and ν is the frequency of light). It was proved that the quantum properties of light are stronger when the frequency of the photon is greater. Experiments with *x*-rays and gamma rays gave most important evidence for the validity of the quantum theory of radiation. The nature of light and electromagnetic radiation on the whole was summed up in the concept of radiation as a complicated phenomenon possessing both wave and quantum properties.

The expansion of the range of electromagnetic waves is of paramount importance for our story.

It turned out that the visible light is only a 'tip of the iceberg' within the entire range of the electromagnetic spectrum. According to the theory, electromagnetic waves (or quanta) of any frequency must possess the same universal velocity in vacuum. The experimental verification of this assumption is a fundamental problem. The main difficulty lies in that it is necessary to investigate a vast range of frequencies up to 10^{25} Hz!

Both the generation and recording technique change while the experimenters go to a different spectrum. To perform research over the entire scale of the electromagnetic waves is infeasible both for an individual physicist and for a group of investigators. This accounts for the diversity of the names of scientists and the research centres engaged in measurements of the velocity of propagation of electromagnetic radiation.

It is also infeasible to cover all the conducted experiments. I am only going to consider experiments related to two ranges of electromagnetic waves. First, we shall consider the research of the Soviet physicists Leonid Mandelshtam (1879-1944) and Nikolai Papaleksi (1880-1947) on the determination of the velocity of radio waves, and then the experiments of the American physicists David Luckey and John W. Weil on the measurement of the velocity of gamma quanta will be covered. These examples will show the significance attached to the advancement of measuring instruments and the discovery of previously unknown physical phenomena which make it possible to tackle old problems in a new manner.

Let me start with the experiments carried out

Leonid Isaakovich Mandelshtam (1879-1944)

by Leonid Mandelshtam and Nikolai Papaleksi. These scientists are rightfully regarded as the ones among the creators of radiophysics. They were engaged in a very wide number of problems concerning the generation, propagation, and reception of radio waves. During the period the 1920s-1930s, many problems in radio that are now in the textbooks were only at the stage of development. Recall that the first transmission of signals by means of electromagnetic waves in a 'free' space was demonstrated by the Russian scientist Aleksandr Popov (1859-1906) in 1895. By the time Mandelshtam and Papaleksi began their experiments, the radio engineering was not yet forty years old.

15—0541

The Soviet radiophysicists had many reasons to tackle the problem of measuring the velocity of radio waves. Firstly, due to the specific features of the waves of this range, the velocity of their propagation near the Earth's surface may be different from the velocity of light in air. In the early 20th century, the propagation of electromagnetic waves near a conducting surface was rather well understood. The upper layers of the soil possess noticeable conductivity, and the measurement of the velocity of radio waves made it possible to verify the theory. Secondly, in the early 1930s, radio waves found new applications. They were employed in radio navigation, radiogeodesy (for the determination of the distance between two points by means of radio waves), and for the investigation of the Earth's ionosphere (the ionosphere is the upper part of the atmosphere, it starts from about 50 km and contains large numbers of charged particles, viz., ions and electrons). The appearance of 'new' professions of radio waves required greater accuracy in the determination of their velocity. A great significance for the selection of the topic of investigation was, naturally, attached to the relationship between the velocity of radio waves on the Earth and the fundamental constant, the velocity of electromagnetic waves in vacuum.

Mandelshtam and Papaleksi's approach to the problem was profound and thorough. Prior to the experiments, they had analysed possible errors and determined the optimal conditions for the staging of experiments.

The measurements were conducted in the range of wavelengths from 130 to 450 m. The idea of

Nikolai Dmitrievich Papaleksi (1880-1947)

the experiments is very simple. A transmitting station I sends radio waves in the direction to station II. These waves are received by station II, which sends the waves coherent with the received waves back to station I. Because of the finite time of wave propagation from station I to station II and back, there is a phase difference φ between the wave sent by station I and the wave received by it from station II, which can be measured. Knowing the distance between the stations, one can determine the velocity of radio waves by the measured magnitude of the phase difference.

Considering this method, one can, of course, recall both Galileo's method and Michelson's interference method. However, the Mandelshtam-Papaleksi method has a number of peculiarities making it basically different. As to the optical experiments, a mirror was used as the reflector which returned part of the energy to the source of radiation. The employment of the mirror is dictated by that it is infeasible to produce two independent coherent sources of light. The possibility to apply mirrors is determined by the small wavelengths of light: one can obtain a narrow beam of light and avoid considerable diffraction effects. As to radio engineering, a reverse situation occurs. It is relatively easy to make two transmitters emit coherent radio waves. At the same time, while working with the wavelengths of about 300 m, it is infeasible to avoid diffraction phenomena, and there it is extremely difficult to obtain a narrow beam of radio waves. Diffraction brings about a rapid attenuation of radiation with distance. This, in its turn, makes scientists utilize, instead of a 'passive' mirror, a second transmitter controlled by the signal from the first one. Consequently, the differences in the properties of light and radio waves as if compensate each other, and the result is the interference method that is applicable in both ranges.

The employment of transmitter II is far from being the unique feature of the Mandelshtam-Papaleksi method. The frequencies of the transmitters I and II in the experiments were not the same. The reader may make a nasty remark: how come the waves of different frequencies interfere?! The answer to this question is not very evident, as it may seem at first glance. The interference between waves of different frequencies is impossible if they are produced by independent sources. As to the experiments under consideration, the radiation from transmitter I 'controls' (to be more exact, synchronizes) the functioning of transmitter II, and therefore the waves from both transmitters, at any point in the space between them, possess a definite phase difference and can interfere with each other. Different frequencies are used because of the difficulties of detecting a weak signal from a transmitter II against the background of powerful radiation from a transmitter I if the two transmitters worked at the same frequency. The scientists used transmitters whose frequency ratio was a rational number (usually 3 : 2 or 4 : 3).

This relationship between the frequencies allowed them to determine the phase difference from the *Lissajous figures*, that is, the closed trajectories described by a point moving in a plane when the components of its position along two perpendicular axes each undergo simple harmonic (sinusoidal) motions simultaneously and the ratio of their frequencies is a rational number. In the simplest case, when the frequencies and the amplitudes of the oscillations are identical, the point describes an ellipse (Fig. 37*a*). Its shape and orientation depends on the phase shift between the oscillations: we shall see a straight line if the phase shift $\varphi = 0$ or $\varphi = \pi$, an ellipse at $\varphi = \pi/4$, and a circle at $\varphi = \pi/2$. It is easy to see that the shape and orientation of the Lissajous figures can indicate the phase shift between oscillations of different frequencies. As an example, Fig. 37*b* shows the Lissajous figures produced by oscillations of a point according to the law $y = y_0 \sin(3\omega t + \varphi)$ by the y-axis and $x =$

x_0 sin $(2\omega t)$ by the x-axis. The difference between the figures is produced by the different values of φ. It is convenient to observe the Lissajous figures by means of a cathode-ray oscilloscope if a signal with a frequency ω_1 is applied to one pair of its

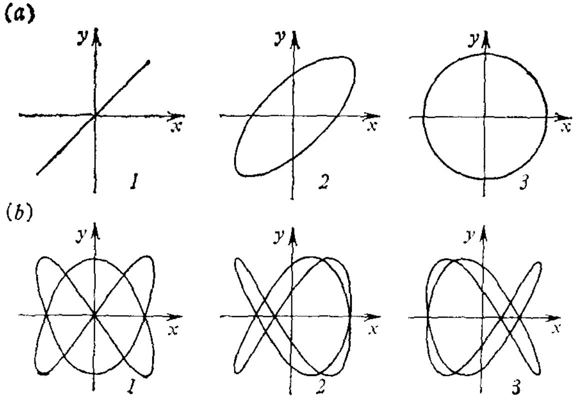


Fig. 37. The Lissajous figures. (*a*) $\omega_x = \omega_y$, (1) $\varphi = 0$; (2) $\varphi = \pi/4$; (3) $\varphi = \pi/2$; (*b*) $\omega_x : \omega_y = 2 : 3$, (1) $\varphi = 0$; (2) $\varphi = \pi/3$; (3) $\varphi = 2\pi/3$.

pick-up plates and a signal with a frequency ω_2 is applied to the other pair.

However, the study of the Lissajous figures makes it possible to determine only a part of the phase shift $\Delta\varphi$, which is related with the total phase shift as

$$\Delta\varphi = \varphi - 2k\pi,$$

where k is an integer unknown to the experimenter. (It is obvious that the Lissajous figures do not change if φ is incremented by a multiple of 2π.)

To find the velocity, it is required to know φ rather than $\Delta\varphi$. Therefore Mandelshtam and Papaleksi had to make the experiment more sophisticated. In a number of measurements they determined the change in $\Delta\varphi$ as a function of the distance between the transmitters, which allowed them to find φ and hence v; this method was called the 'radio lag technique'. The same result can be achieved if the frequency is smoothly varied while the distance between the transmitters is the same; this method of determining v is called the 'radio-range-finder technique'.

The implementation of both methods requires from scientists to be good experimenters and organizers. To calculate v, the phase shift which is introduced by the receivers and transmitter II should be taken into account; it is necessary to pay thorough attention to the meteorological conditions: they affect the soil conductivity, the air humidity, etc., which influence the velocity of the radio waves propagating near the Earth's surface.

The measurements required much preparatory work. During the period since autumn 1934 to autumn 1937, the measurements were conducted in three regions of the Soviet Union: in the Northern Caucasus and at the shores of the Black Sea and the White Sea. The experiments were continued during 1939 and 1940 in other regions of the country. A large number of Mandelshtam's and Papaleksi's junior colleagues took part in the experiments under their supervision. This work, in a sense, was a collective research. During the 1930s, the solution of complicated problems by large teams of scientists was only coming into research practice while to date this approach is generally accepted.

The result of the many years of Mandelshtam and Papaleksi's team research was the value

$$v = 299{,}500 \pm 80 \text{ km/s}.$$

This magnitude is the velocity of radio waves in vacuum; it was obtained on the basis of measurements with due regard to a number of factors (the finite conductivity of the soil, atmospheric effects, etc.). Unfortunately, the Second World War interfered with the plans of the Soviet scientists to derive inferences from the research immediately when it was finished. The results of the experiments were generalized by Mandelshtam and Papaleksi in their joint paper presented on February 16, 1943, in Kazan, where the USSR Academy of Sciences was evacuated.

The Second World War abruptly expanded the research in radiolocation. This resulted in the increase in the interest of physicists in the problem of determining the velocity of radio waves. To improve the accuracy of measurements, they began using the latest instruments and methods of measurements developed for military purposes. To date the accuracy of measuring the velocity of radio waves is essentially better, but the research of the Soviet physicists Mandelshtam and Papaleksi is nevertheless considered to be classical, from which a whole branch of radiophysics and metrology sprang up.

* * *

Now let us discuss the propagation velocity of gamma rays, the ultra-high-frequency region of the electromagnetic spectrum. Gamma rays were first discovered during the research on radioactivity. However, radioactive nuclei are not the only source of gamma rays. They can also appear, for instance, in the process of annihilation of electron-positron pairs: when these particles collide, they produce two gamma quanta.

There is a method of generating gamma radiation whose frequency can be varied in a rather wide range. It follows from the laws of electrodynamics that a charged particle moving with an acceleration must emit electromagnetic waves. Their frequency depends on the magnitude of the acceleration. Modern accelerators can produce charged particles (for instance, electrons) moving at velocities differing from the velocity of light by only a very smal part of one per cent. If a beam of such fast electrons is directed at and hits a target, the electrons will be stopped abruptly and will emit their energy as electromagnetic waves of high frequency. This radiation came to be called '*bremsstrahlung*' (the *braking radiation*). The frequency of this radiation depends on the energy of the electron arriving at the target.

Physicists applied various methods of generating gamma quanta when determining their velocities. I shall deal here with the experiments in which gamma bremsstrahlung was used. The schematic of one of the first experiments of this type is shown in Fig. 38. The required gamma rays appeared in a thin target *2* when a beam of electrons with the energy of 3.40×10^8 eV arrived at it. (1 electron volt $= 1.6 \times 10^{-19}$ J is a unit of energy equal to the energy acquired by an electron when it passes through a potential difference of 1 V in vacuum. An electron possessing an energy of 3.4×10^8 eV moves with a velocity $v = 0.999{,}998{,}6\ c$.) The braked electrons deviat-

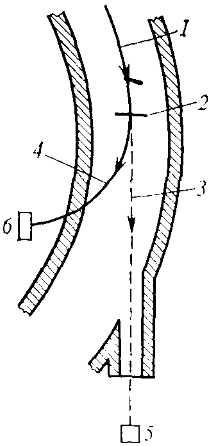


Fig. 38. A diagram of measuring the velocity of gamma bremsstrahlung. *1*—beam of electrons, *2*—target, *3*—gamma bremsstrahlung, *4*—trajectory of the braked electrons, *5*—movable counter of gamma quanta, *6*—stationary counter of braked electrons.

ed from their trajectory in a magnetic field and arrived at the counter *6* located in such a manner that the electrons with an energy of 1.4×10^8 eV were counted. The bremsstrahlung produced by this group of electrons was detected in the bulk of the gamma quanta arriving at the counter *5* by means of the so-called delayed coincidence counting circuit. This circuit is a special electronic device receiving signals from several counters. It is inessential what kind of counter is employed: for instance, in the described experiments one counter registered braked electrons while another detected gamma quanta. The electronic circuit 'compared', as it were, the arriving signals and registered only the pairs of impulses from the counters such that were delayed by a certain interval: therefore it detected delayed coincident events. If the lapse of time between the two impulses was more or less than a given one, the circuit did not register an occurrence of such an event.

The analysis of gamma-ray spectrum showed that the greatest number of coincident signals from the counters of electrons and gamma quanta were registered for the gamma quanta with an energy of 1.7×10^8 eV. This proved that the circuit registered the gamma quanta produced by the electrons whose energy was 1.4×10^8 eV. Putting counter *5* in different positions along the track of the gamma quanta and measuring the time of the delay in their registration with respect to that of the electrons, one could find the velocity of the gamma quanta.

In 1952 the American physicists David Luckey and John W. Weil conducted measurements using the synchrotron at Cornell to accelerate electrons. They determined the time of delay for four positions of the counter, the greatest difference between the positions being 13 m. When the resulting data were processed, the magnitude of c was obtained with an accuracy of 1%:

$$c = 2.974 \cdot 10^{10} \text{ cm/s},$$

which is in good agreement with other measurements of c.

The accuracy of experiments carried out with accelerators considerably improved in the course of time. In 1973 a team of American scientists from a Californian State University announced that they had conducted an experiment on the comparison between the velocity of the quanta of electromagnetic radiation in the visible range (the energy of the quanta being several electron volts) and that of gamma quanta with an energy of 7 GeV (1 GeV $= 10^9$ eV; the frequency of gamma radiation with the energy of 7 GeV equals 1.7×10^{24} Hz). The physicists employed gamma quanta of fast electron bremsstrahlung; visible light appeared due to the motion of the electrons in a magnetic field: this kind of rays is called synchrotron radiation. The conclusion drawn by the American physicists was that the relative difference between the velocities of quanta of different energies satisfied the condition

$$\frac{c\,(\mathrm{GeV}) - c\,(\mathrm{eV})}{c\,(\mathrm{eV})} < (1.8 \pm 6) \times 10^{-6}.$$

Consequently, within less than a hundred years of Hertz's discovery, the velocity of electromagnetic waves was determined in a very wide range of frequencies. There is neither place nor time to cover all the measurements performed during this period. We did not discuss the velocity of infrared, ultraviolet, and x-ray radiation. Although the accuracy of the experiments conducted in various ranges essentially varies, the general conclusion from the analysis of the obtained results is as follows: to date there are no grounds to suppose that in vacuum the velocity of propagation of visible light differs from that of electromagnetic waves of various frequencies. This bears witness of the truly fundamental nature of c.

Chapter 8

A Long Overdue Discovery

The history of science is sometimes puzzling. Many examples could be given of a theoretical result being obtained but then forgotten for decades, and then, having been rediscovered becoming the focus of everybody's attention. There are other situations in which a 'new' phenomenon is discovered and investigated only for it to become clear that the phenomenon had been observed several times before but it had not been perceived being anything new and so was overlooked. How and why these 'strange' events in the history of physics occur cannot be answered unambiguously. Although different circumstances are important in each particular case, there is a general pattern associated with the progress of science. This explains the interest of historians in these facts. In this chapter I shall recount the story behind a discovery of this kind, a discovery which is now one of the most valuable in physics. Naturally, this discovery is related to the main subject of the book, the velocity of light.

* * *

The early 1930s were the period in physics when the young quantum mechanics intensely

Pavel Alekseevich Cherenkov (Čerenkov) (born 1904)

developed its applications, nuclear physics rapidly advanced, and the physics of elementary particles made its first steps. Physicists' greatest attention was given precisely to the research in these fields. Therefore the topic of investigation of Pavel Cherenkov (Čerenkov), a postgraduate at the Lebedev Physical Institute of the USSR Academy of Sciences, seemed rather modest. His supervisor was Sergei Vavilov (1891-1951), a Member of the Academy of Sciences and he had assigned Cherenkov work concerning the luminescence of uranium salt solutions appearing under incident radium gamma radiation. As is usual, the postgraduate's topic was determined by the

Sergei Ivanovich Vavilov (1891-1951)

scientific interests of the supervisor: Sergei Vavilov was a leading specialist in the field of luminescence physics.

To study luminescence of the uranium salt solutions, Cherenkov applied a technique developed by Sergei Vavilov and Evgeny Brumberg, namely, the method of photometry (i.e., the determination of the intensity of light) by the threshold of vision as applied to the investigation of weak light sources. A scheme of the apparatus used by Cherenkov is shown in Fig. 39. A platinum vessel A contained the studied liquid. A radium preparation, which was the source of gamma radiation, was put into a glass tube and inserted either into the cavity R_1 under the bottom of this vessel or into the cavity R_2 parallel to the vessel axis (the cavity R_2 was used only for the investigation of radiation polarization).

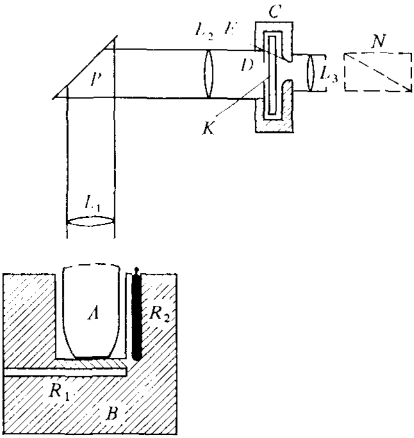


Fig. 39. Cherenkov's apparatus.

Both the vessel A and the tube with radium were put into a wooden block B. The radiation from the liquid passed through the optical system L_1PL_2 and the diaphragm D and was focussed as a small spot on the optical wedge K, which could be moved along the slot C perpendicular to the plane of the picture. Having passed through the wedge and lens L_3, the spot of light formed a rather large image on the retina of the observer's eye, which allowed neglecting the difference in the sensitivity of various areas of the retina and the influence of the involuntary eye movements. To investigate the spectral composition of the radiation, glass filters inserted into the slot *E* were employed. Polarization of the radiation was studied by means of the polarizing prism *N*.

The optical wedge played an essential role in the method of photometry by the threshold of vision. Different sites of the wedge were transparent to the light to different known degrees. In the course of experiment, the wedge was moved until the observer could no longer see the image. The most salient feature of the experiment was the adapting of the eye for darkness prior to experiment. The sensitivity of the observer's eye after a long period of being in darkness (during 1-1.5 hours) increased by a factor of tens of thousand times, and he could register light of very low intensity. It is important that the threshold of eye sensitivity (i.e., the minimum peceived intensity of light) does not practically change in the course of time. Therefore the intensity of light was measured proceeding from the sensitivity threshold: knowing how much the wedge attenuates the light in the position corresponding to the threshold, or loss of visual perception, one can express the sought intensity in the units corresponding to the eye sensitivity threshold. Note that the eye adaptation to darkness is easily broken, and therefore the positions of the wedge were registered from a special lighted scale by an assistant rather than by the observer. The assistance was also necessary in eliminating the observer's possible autosuggestion. Following an hour and a half adaptation, the measurements were carried out for no more than 2-2.5 hours to avoid the observer's eye fatigue and thus prevent the observation errors caused by it.

It is clear from this short description of the measurement procedure how labour-consuming such experiments are, and how much attention they require from the observer. Scientists did not have then sufficiently sensitive electronic detectors of radiation (photomultipliers), and the visual method was the only acceptable one.

The labouriousness of the applied technique did not prevent, however, the discovery of a completely new phenomenon. When Cherenkov first published the results of these experiments, he wrote:

> "In connection with the studies of the luminescence caused by gamma rays in uranil salt solutions, we have found that all 20 available pure liquids reveal weak glowing when the gamma rays pass through them. As the experiments with liquids with different degrees of purity showed, the phenomenon is not related to admixtures or contaminations."

Cherenkov's paper was published in the *Proceedings of the USSR Academy of Sciences* in 1934. It was presented in the journal by Academician Vavilov, who considered the results to be exceptionally interesting. How can this famous scientist's interest in the weak luminescence discovered by Cherenkov? The point is that this luminescence was essentially different from the types of luminescence Vavilov and his pupils had been studying for many years. Unlike other kinds of light emission, the main features of luminescence are that there is a lapse of time between the moment of excitation of atoms or molecules, which are the source of radiation, and the emission of this radiation. The value of this delay varies for different kinds of luminescent substances from 10^{-9} s to many hours, but it is always much greater than the period of oscillations of the emitted light wave. This feature points out the methods to verify whether a light emission is luminescence. One of the most effective methods of verification is the 'quenching' of luminescence. It can be brought about, for instance, by increasing the temperature of the solution or by adding a 'quencher' to the luminescent solution (silver nitrate, potassium iodide, and nitrobenzene are known to be efficient luminescent quenchers). The excitation of a molecule in these cases is imparted to the quencher molecules or an 'extraneous' molecule because of collisions due to intensive thermal motion, and light emission does not occur.

Following Vavilov's advice, Cherenkov carried out a series of experiments with different liquids and discovered in their radiation a number of properties that contradicted the accepted concepts of luminescence. It turned out that despite great variations in the composition of liquids, the brightness of their radiation was practically the same. The radiation spectrum also changed little from one liquid to another: most of the radiation energy was concentrated in the blue-green region of the spectrum. The brightness of the radiation could not be influenced by adding various luminescence quenchers or by heating the liquids. In addition, the radiation in all studied cases proved to be partially polarized, the plane of oscillation of the electric vector of the electro-

magnetic wave being parallel to the direction of the gamma ray beam.

Cherenkov's experiments were thorough. Whenever possible, check experiments with liquids in which the phenomenon of luminescence caused by gamma rays was clearly pronounced were carried out. No contradictions with the concepts of luminescence were observed in these cases. Moreover, in order to find out how important the energy of the gamma quanta was for the 'universal' radiation of liquids, Cherenkov attempted to induce radiation by means of x-rays. It was proved that x-rays, whose quanta possess essentially less energy than gamma quanta, do not induce light emission in most of the studied liquids.

The same issue of the *Proceedings of the USSR Academy of Sciences* that carried the first Cherenkov's article also contained Vavilov's paper "On the Possible Causes of Blue Gamma Radiation in Liquids", in which Vavilov made an attempt to elucidate the mechanism of this emission. Vavilov suggested in this paper that the source of radiation are the fast electrons which the gamma quanta knock out when moving through the liquid. Such electrons are called the Compton electrons, after the American physicist Arthur Holly Compton (1892-1962), who discovered the reaction between free electrons and x-rays in 1922. According to Vavilov, the Compton electrons, moving in a liquid, are braked and are therefore decelerated. In classical electrodynamics any charge that is decelerating must emit electromagnetic quanta. Therefore Vavilov's first hypothesis was that Cherenkov had discovered special kind of bremsstrahlung.

Although Vavilov's hypothesis explained many features of radiation of liquids under the effect of gamma rays (in particular, polarization, independence from the nature of the liquid, the absence of any noticeable effect in the case of using x-rays for excitation, etc.), it required a comprehensive experimental verification and substantiation by strict calculations. It should be said that few scientists knowing about the results of Cherenkov's experiments were interested in them. For instance, a prominent physicist is known to have said then that "they are engaged in the Physical Institute by the radiation of some trash". Nevertheless, Cherenkov's experiments continued.

One of the top priorities after the publication of Vavilov's paper was to validate his hypothesis that electrons were the source of the observed radiation. Therefore it was required to induce the radiation by a beam of fast electrons. It was not so easy to carry this out in those years: the technology of accelerators was yet in its infancy, and one had to employ a natural source of beta rays. Using a radium preparation in a thin-wall capsule, which did not significantly absorbs beta rays (unlike the tubes in the first experiment which did). Cherenkov demonstrated that the radiation induced by the beta particles (electrons) had the same properties as the radiation induced by the gamma rays.

Some time later one more experiment was staged and brought about important results. A liquid radiating under incident gamma rays was put between the poles of an electromagnet. The experiment studied the radiation appearing both with and without a magnetic field, it was

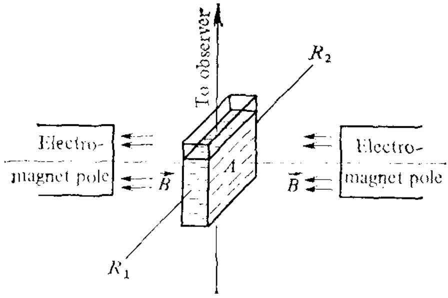


Fig. 40.

necessary to check whether the polarization of the radiation is changed by the magnetic field. If Vavilov's hypothesis that electrons were the source of radiation was correct, then the magnetic field, because it distorts the trajectories of the electrons moving perpendicular to the lines of magnetic induction, should cause a change in the polarization of the radiation. Although the polarization did change, it was not the main result of the experiment. It was proved that when the magnetic field was switched on, the brightness of the radiation was changed such that if the source of the gamma rays was at position R_1 (Fig. 40), then the magnetic field decreased the brightness of the radiation, but if it was at position R_2, then the field increased the brightness. This unexpected result indicated yet another property of the radiation, i.e., that it had a preferred direction. The data of the experiment showed that, with respect to the velocity of the electron, more light is emitted into the front hemisphere than into the back one.

Ilya Mikhailovich Frank (born 1908)

By the time the experiments with the magnetic field were carried out, a young Soviet physicist Ilya Frank (born 1908) was invited to participate in the development of the theory of the phenomenon. He closely collaborated with Cherenkov, discussed the set-up and the results of the experiments, and sometimes even acted as an assistant. Frank related the conclusions following from Cherenkov's experiments to Igor Tamm (1895-1971), one of the leading Soviet theorists. Tamm became interested and later took an active part in the advancement of the relevant theory.

The theorists had to reject the bremsstrahlung

Igor Evgenievich Tamm (1895-1971)

mechanism hypothesis because the calculations of its intensity carried out on the basis of this hypothesis led to values several orders of magnitude less than those determined experimentally. A new idea following from the favoured direction of the radiation can be explained as follows.

Let us consider a point electric charge moving in a medium uniformly along a straight line with a velocity v. Since the electric field in the vicinity of the point through which the charge passes is changed, classical electrodynamics requires this point to be regarded as the source of a spherical wave emitted the moment the charge passes through the point. The waves generated in two arbitrary points of the charge trajectory are coherent, the phase difference between them being determined by the time Δt between the moments the charge passes these two points: $\Delta t = l/v$, where l is the distance between the two

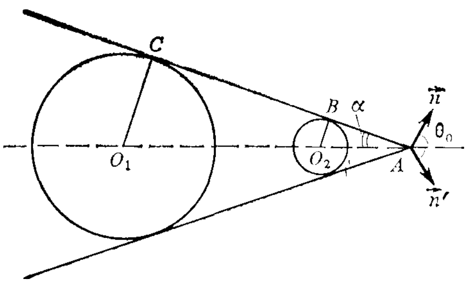


Fig. 41.

points. If and only if the velocity v is greater than the phase velocity of light in the medium, i.e., $v > c/n$, then the spherical wave fronts with the centres in the different points of the charge trajectory (Fig. 41) possess a common envelope: a cone with the apex A coinciding with the instantaneous position of the charge. Note that it is essential for the considered picture that a discrete charge rather than a flow of charges, i.e., a current, moves. The enveloping cone can be characterized by the normals O_1C and O_2B which define an angle θ_0 with the vector of the particle velocity. It is easy to show that

$$\cos \theta_0 = \frac{c}{nv}.$$

Indeed, suppose it takes the particle the time Δt to travel the distance O_2A. Then the radius of the wavefront with the centre O_2 is equal to $c\Delta t/n$ when the charge passes point A. Evidently, $O_2A = v\Delta t$ $(\sin\ \alpha = \cos\theta_0)$ and $\sin\ \alpha = O_2B/O_2A$. Hence

$$\sin\left(\frac{\pi}{2} - \theta_0\right) = \cos\theta_0 = \frac{c}{nv}.$$

This result, the inference on the predominant forward direction of the radiation, was one of the most essential corollaries of the Tamm-Frank hypothesis. It also indicated a method of direct quantitative verification of the hypothesis in experiment: in was required to determine the angle θ_0 and compare its magnitude with the one calculated from the formula above. The experiment was set up, and it produced results in good keeping with the calculations. (We have considered only the elementary theory of the phenomenon. In reality the refractive index n depends on the frequency, and therefore the radiation with different frequencies is emitted at somewhat different angles. However, the range of Δn is small for the frequencies in the visible spectrum, and therefore the difference between the angles is also relatively small, so we can instead use an average angle θ_{av}.)

Besides the inference on the predominant direction of the radiation, analysis of the hypothesis led to some other important conclusions. For instance, it followed from the condition $v > c/n$ that only fast electrons should emit light. This conclusion agreed with the experimental data that x-ray radiation, the energy of whose quanta is too small to make the electrons move with a velocity $v > c/n$, does not cause this sort of fluorescence. In addition, the hypothesis showed that the electron's radiation is proportional to its path length, and is therefore inversely proportional to the density of liquid. At the same time, the number of electrons interacting with gamma quanta is approximately proportional to the density of liquid. Therefore the total intensity of emission should not strongly depend on the density of liquid, which was observed experimentally.

However, the qualitative consideration of the hypothesis did not make it possible to calculate a very essential characteristic of the emission, its intensity. Recalling this period, Frank emphasized that this made the suggested qualitative picture of the phenomenon very vulnerable. He wrote:

> "I happened to share these thoughts with some theorists who began to show interest in Cherenkov's experiments (especially when the predominant direction of the radiation was established), but nobody was sympathetic ... Tamm even suggested to publish a paper before any further detailed analysis. However, this would have been too premature. The problem of the intensity had not yet been considered, and the very possibility of such radiation became immediately doubtful. Tamm related to Mandelshtam the qualitative picture allowing to interpret the radiation. Mandelshtam's remark was as follows: the electron does not emit in uniform and rectilinear motion."

(Possibly, the reader unacquainted with the phenomenon revealed by Cherenkov will think that the assumption on the constant velocity of the electron is strange because an emitting electron releases energy and therefore brakes. This is undoubtedly true, but it has only to be taken into account in a strict calculation. However, the essential point is that in this particular case the electron brakes because it emits rather than emits because it brakes, as it happens in bremsstrahlung.)

The situation was curious. The qualitative picture of the phenomenon explained almost every feature of the radiation, but it was regarded, mildly speaking, with caution because the idea it was built on seemed erroneous. However, the explanation of the effect given by Tamm and Frank had a direct analogy in acoustics: when a shell moves at supersonic speed through air, it emanates the so-called 'Mach wave', whose properties are very much like those of the radiation discovered by Cherenkov. Then what is the reason why the Tamm-Frank hypothesis was regarded as doubtful? Tamm answered this question thus:

> "I think that we have here an instructive example of a situation not uncommon in science, the progress of which is often hampered by an uncritical application of inherently sound physical principles to phenomena lying outside of the range of validity of these principles.
>
> "For many decades all young physicists were taught that light (and electromagnetic waves in general) can be produced only by *nonuniform* motions of electric charges. When proving this theorem one has—whether explicitly or implicitly—to make use

> of the fact that super-light velocities are forbidden by the theory of relativity (according to this theory no material body can ever attain the velocity of light). Still, for a very long time the theorem was considered to have an unrestricted validity.
>
> "So much so that I. Frank and I, even after having worked out a mathematically correct theory of Vavilov-Cherenkov radiation, tried in some at present incomprehensible way to reconcile it with the maxim about the indispensability of acceleration of charges. And only on the very next day after our first talk on our theory in the Colloquium of our Institute we perceived the simple truth: the limiting velocity for material bodies is the velocity of light *in vacuo* (denoted by c) whereas a charge moving *in a medium* with a constant velocity v will radiate under the condition $v > c'(\omega)$, the quantity $c'(\omega)$ depending on the properties of the medium. If $c'(\omega) < c$, then this condition may very well be realized without violating the theory of relativity ($c' < v < c$)."

(Here $c'(\omega)$ is the phase velocity depending on frequency: $c'(\omega) = c/n(\omega)$.)

Consequently, when the hypothesis on the mechanism of this radiation was put forth, its authors had to overcome the stereotypes ingrained in the minds of most physicists. It is curious that one of these stereotypes, the conclusion that if the velocity of light in vacuum is the maximum possible in vacuum, then the velocity of light in a medium is the maximum possible in the medium, took shape very rapidly: the discovery of the liquid luminescence produced by gamma rays was made less than 30 years after the formulation of the special theory of relativity!

In the long run, Tamm and Frank succeeded in calculating the total intensity of the radiation. This calculation allowed them to explain why the luminescence was blue. In 1937, three years after Cherenkov and Vavilov's first publications, Tamm and Frank published their paper "Coherent Light Emission of a Fast Electron in a Medium", in which they presented a quantitative theory of the phenomenon rightfully called the Vavilov-Cherenkov (Čerenkov) phenomenon. However, the story of this most interesting physical effect did not come to an end then. The research in the area of the velocities greater than the phase velocity of light in media was successfully continued in both theory and experiment, but I am going to tell about it a bit later.

We shall have to come back, to the period prior to the discovery of the Vavilov-Cherenkov phenomenon, and consider the historical paradoxes related to it.

First, about the experimental aspect of the problem. The radiation revealed by Cherenkov is universal, i.e., it does not depend on the nature of liquid. Since the time of the discovery of radioactivity, numerous experiments were conducted where different liquids were bombarded by gamma radiation. Then why nobody prior to Cherenkov noticed this peculiar luminescence? The scientific literature published before 1934 shows that the blue luminescence of liquids was, in fact, marked by investigators more than once. It was observed, in particular, by Pierre and

Marie Curie at the very outset of the 20th century. During the period from 1926 to 1929, the French physicist L. Mallet studied this luminescence and even determined its spectrum. However, prior to 1934 it came to nobody's mind to single out the observed phenomenon from a number of the effects traditionally related to luminescence. Precisely this historical circumstance gave rise to the title of this chapter: *A Long Overdue Discovery*. Only Vavilov's profound knowledge of the properties of luminescence allowed the scientists to see the peculiarity of the blue radiation of liquids. Therefore, it is not always that observation of a phenomenon leads to its 'discovery', it can only occur if the researcher comprehends the experimental situation in all its details. The story of the Vavilov-Cherenkov effect once again proves that discoveries in the area of experimental physics are practically never accidental.

No less instructive are the events related to the theory of the effect. I have to admit that I used the quotation from Tamm to answer a question other than Tamm himself answered. The quotation above was preceded by the following lines:

> "You see that the mechanism of this radiation is very simple. The phenomenon could be easily predicted proceeding from classical electrodynamics many decades before it was actually revealed. So why was this discovery so late?"

But there was no actual delay in the discovery. However, in the late 1950s, i.e., during the period when Tamm wrote these lines, no one of the creators of the theory of the Vavilov-Cherenkov effect knew that the phenomenon of the radiation of a super-light charge had been theoretically predicted almost 50 years before the Tamm-Frank theory was offered. Here is one more interesting quotation:

> "The question now suggests itself, what is the state of things when $v > u$ (u is the phase velocity of light in a medium.—*Author*)? It is clear, in the first place, that there can be no disturbance at all in front of the moving charge (at a point, for simplicity). Next, considering that the spherical waves emitted by the charge in its motion along z-axis travel at speed u, the locus of their fronts is a conical surface whose apex is at the charge itself, whose axis is that of z, and whose semiangle θ is given by
>
> $$\sin\theta = u/v."$$

Don't you think that something is mixed up, and we turned again to Tamm and Frank's paper? No, the author of these lines is the English physicist and electrical engineer Oliver Heaviside (1850-1925), a man of exceptional talent and very involved life. It was written in the late 1880s, during the period when the Maxwell theory of electromagnetic field (i.e., classical electrodynamics mentioned by Tamm) was yet perceived as a scientific novelty. There was a stroke of genius in Heaviside's consideration of the motion of a point charge: the electron had not then been discovered, and the concept of a point charge seemed a pure abstraction. It is no less important that Heaviside, naturally unaware of the restrictions imposed by the special theory of relativity on the velocity of material bodies, only regarded a charge in a dielectric:

"... I should remark that this is not in any way an account of what would happen if a charge were impelled to move through the ether (in vacuum in the modern terms. — *Author*) at a speed several times that of light, about which I know nothing; but an account of what would happen in Maxwell's theory of the dielectric kept true under the circumstances, and if I have not misinterpreted it."

Heaviside's insight is all the more amazing: many years later, in 1904, the well-known German physicist Arnold Sommerfeld (1868-1951) considered an effect appearing in the motion of a charge with a super-light velocity in vacuum, i.e., tried to solve a problem which is absurd from the viewpoint of the special theory of relativity. It is only natural that very soon, after Einstein's work on the electrodynamics of moving bodies appeared, Sommerfeld's investigations were forgotten. However, Sommerfeld's work was brought to the attention of Tamm and Frank by the Soviet physicist Abram Ioffe (1880-1960) before they completed the development of the Vavilov-Cherenkov effect theory. The fact that the problem of motion of super-light charged particles in a medium had been considered by Heaviside only surfaced in 1974. It was practically simultaneously indicated by the Soviet physicist Alexei Tyapkin and the English physicist T. R. Kaiser. But why had Heaviside's correct conclusions been consigned to oblivion?

Several ideas can be offered in this connection. Firstly, Heaviside's work did not acquire due credit in world of science during his lifetime because of some reasons far from science. Secondly, he did not succeed in complete solution of the problem of motion of a super-light particle. For one, he could not calculate the total intensity of the radiation. This, in its turn, did not allow him to estimate the possibility to observe the effect in experiment. However, even if the intensity of the radiation had been calculated in the early 1890s, an experimental substantiation of the predicted effect would have been infeasible all the same, because neither the electron nor the phenomenon of radioactivity, as a source of fast charged particles, had been discovered then. Nothing was also known on cosmic rays, which could bring about the Vavilov-Cherenkov radiation too. Therefore, Heaviside's work at the moment of its appearance could be regarded at best as a curious corollary of Maxwell's electrodynamics and unpromising from the viewpoint of experimental verification. The history of science shows that such theoretical discoveries made long before their application to the analysis of real situations were soon forgotten. Alas, the problem of 'timely' discoveries is not at all far-fetched, and the fate of Heaviside's work is one more substantiation of the inexorable laws of the history of science.

* * *

Now let us return to the Vavilov-Cherenkov phenomenon theory offered by Tamm and Frank. They made clear certain details in the papers following the first publication in 1937. In 1940 the Soviet physicist Vitaly Ginsburg (born 1916) considered this phenomenon from the quantum standpoint. Step by step, it was found that not only the Vavilov-Cherenkov phenomenon but also other curious effects are related to the motion of super-light particles. A theoretical development of the problem of motion of fast particles in a medium gave rise to a scientific trend, the optics of super-light particles.

During the first years after the discovery and explanation of the Vavilov-Cherenkov phenomenon, it seemed that the probability of its practical application was very scarce. However, the rapid advance of the electronic technology made people reconsider this. Based on the Vavilov-Cherenkov phenomenon, special detectors of fast charged particles were designed, which could both register the particles and give the data on their velocities. Much research has been done in the high-energy physics using the Cherenkov detectors. In particular, the application of the counters of this type allowed scientists to discover a new elementary particle, the antiproton. Thus the effect related to the motion of super-light particles began to 'work' in physics.

The merits of the scientists participating in the research of the mysterious blue radiation and the development of the theory of this phenomenon received a high esteem. In 1946 Vavilov, Tamm, Frank, and Cherenkov were awarded the USSR State Prize. In 1958 Cherenkov, Tamm, and Frank became the first Soviet physicists among the winners of the Nobel Prize. According to the statute of the Nobel Prizes, they are only conferred on living scientists. Unfortunately, Vavilov, who played a key role in the study and explanation of this phenomenon, died in 1951.

Chapter 9

In Quest of Precision

Over three hundred years have passed since the time of the first determination of the velocity of light. It is an immense period for the history of science. Many physical problems stopped being interesting for scientists during these three centuries and came over from the pages of scientific journals to those of textbooks. New problems came to the forefront. And what about the velocity of light? Is its story over? Modern physics has a definite answer to that: no, the story of the velocity of light has not yet been completed! This is evidenced by new work carried out during the past years.

A sharp increase in the accuracy of measuring the velocity of electromagnetic waves occurred after the Second World War. Apart from the threat to the very existence of humanity, the research conducted for military purposes brought numerous purely scientific important results. One of them is the development of the super-high-frequency (SHF) technology. Both generators and receivers of radiation with wavelength from one metre to several millimetres were produced. Scientists succeeded in performing very accurate, and what is most essential, independent measurement of super-high frequency ν and the wavelength λ of the same radiation. This allowed them to determine the velocity of propagation of SHF radiation by the simple formula

$$c = \lambda\nu.$$

Why is this technique of determining the velocity of light so convenient? The point is that wavelengths of the order of 1 cm can be determined within very fine accuracy by applying interferometric methods. This is very difficult at greater wavelengths. Just imagine the dimensions of an interferometer operating at wavelengths of hundreds of metres! When the wavelength is comparable to the characteristic dimensions of the instruments employed in the experiments, the diffraction phenomena begin to play a role, and it is very hard to take them into account. It is relatively easy to introduce corrections related to wave diffraction into the results of measurements while using radiation of the shortwave region of the SHF range, and there are no appreciable problems in the determination of frequency in the SHF range.

However, one should not think that it was too easy to measure the value of c by just applying new technology. Every scientist working in this field aimed at measuring ν and λ as accurately as possible in order to obtain the value of c as accurate as possible, and the work at the limit of accuracy is always complicated.

A certain stage in measuring c within the SHF range was marked by the work of the American scientist K. D. Froome, whose results were published in 1958. Froome applied a four-horn interferometer to determine the wavelength. His SHF generator operated at a frequency of 72.006 GHz (1 GHz, or one gigahertz, equals 10^9 Hz), and the wavelength amounted to $\lambda \simeq$

4.2 mm. Basically, Froome succeeded because he used the original methods to take into account the diffraction phenomena. He obtained the following result:

$$c = 299{,}792.50 \pm 0.1 \text{ km/s}.$$

This value of c was considered to be the most accurate for a long period.

In order to better the accuracy of the determination of c, it was required to produce new methods that would make it possible to conduct the measurements in the range of greater frequencies and, correspondingly, shorter wavelengths. A possibility to develop such methods came into being when lasers, or optical quantum generators, were constructed.

Laser radiation possesses a number of unique peculiarities. Firstly, it is essentially more monochromatic than the radiation of other sources of light. This means that the range of frequencies, where the greatest part of laser radiation energy is observed, is small as compared with the spectral intervals characteristic of other sources. Secondly, lasers generate radiation that is easily focussed in one specific direction. Finally, the radiation of many lasers is very powerful. As to the problem of the determination of c, of greatest importance is the first property of laser radiation, its monochromaticity, because the range of frequencies $\Delta\nu$, in which the generation occurs, determines the limit of accuracy in frequency measurements. The first lasers possessed such values of $\Delta\nu$, which did not allow scientists to hope for a further enhancement in the accuracy of the determination of c. In particular the problem was that one and the same laser at different external conditions, for instance, at different temperatures and modes of adjustment could generate at various frequencies. Although the deviation of frequency amounted to only one millionth part (10^{-6}) of the frequency itself, any attempts to improve Froome's result were clearly doomed to failure because Froome's accuracy was $\simeq 4 \times 10^{-7}$!

Consequently, before setting about the determination of the frequency, it was necessary to attain its better stability. The latest achievements in electronics, spectroscopy, and technology were applied to solve this problem, and the laser frequency stability was brought to a fantastic value of 10^{-11}! Using the radiation of such great stability, one could tackle the problem of the determination of the velocity of light. (I should only add that the data on the stability given here only refer to the apparatuses applied to determine c. At present, thanks to the effort of Soviet physicists, the laser frequency stability amounts to 10^{-15}.)

The process of measurements is always, either directly or indirectly, reduced to comparing an unknown value with a reference measure. Until recently, the metre was the one redefined in 1960 as the length equal to 1,650,763.73 wavelengths in vacuo of the radiation corresponding to the transition between the levels $2p_{10}$ and $5d_5$ of the isotope ^{86}Kr. This definition abrogated the platinum-iridium metre bar as the standard of length. The reference second is until now a unit of time defined as the duration of 9,192,631,770 periods of the radiation corresponding to the transition between two hyperfine levels of the ground state of the isotope ^{133}Cs. Therefore, the determination of c is reduced to comparing the radiation wavelength of a laser and that of atoms ^{86}Kr, and of the radiation frequencies of the same laser and the atoms of ^{133}Cs. Most difficult is to measure laser radiation frequency.

A direct comparison between the reference frequency and that of laser is practically infeasible. Therefore physicists suggested two indirect methods of determining frequency. The first consists in using modulated laser radiation. The gist of the method is the transformation of laser radiation with frequency ν into radiation characterized by frequencies $\nu_1 = \nu + f$ and $\nu_2 = \nu - f$ $(f \ll \nu)$. Frequency f is well-known and is set by an SHF generator. Two waves with frequencies ν_1 and ν_2 are passed through a special device, a Fabry-Perot interferometer. They pass through the interferometer without any essential attenuation only if a certain relationship between ν_1, ν_2, the length of the interferometer baseline, and the velocity of light holds true. If the length of the interferometer is determined for two positions of its mirrors at which the radiation at the two frequencies passes through it with minimum attenuation, then it is possible to find the frequency ν through a rather simple mathematical procedure. Applying this method, a group of American physicists from the National Bureau of Standards succeeded in determining the radiation frequency of a helium-neon laser: $\nu = 473{,}612{,}166 \pm 29$ MHz. It can be readily calculated that the accuracy in the determination of frequency in these experiments amounted to $\Delta\nu/\nu \simeq 6 \times 10^{-8}$.

The second method is closer to the idea of direct comparison of laser frequency with a reference frequency. It consists in that the radiation with a well-known frequency is multiplied by means of special converters, i.e., is transformed into radiation with a frequency an integer times greater than the initial one. The same converter can also 'mix' the frequencies, i.e., transform radiation with frequencies ν_1 and ν_2 into the

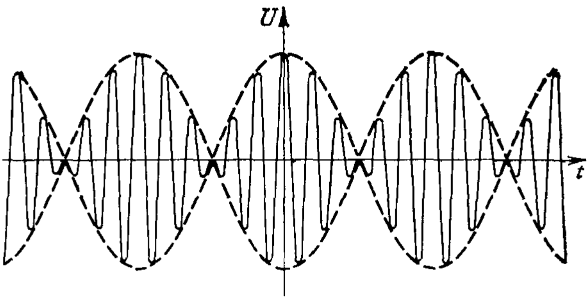


Fig. 42. The beat.

radiation $\nu_3 = \nu_1 + \nu_2$. Both possibilities are used in real experiments. First, a sufficiently large and fixed frequency from one source, for instance, a laser working in the infrared region is 'multiplied', and then it is 'mixed' with a controlled frequency of a klystron, a generator of the SHF range. The resultant radiation of high frequency is directed to a special receiver, and the laser radiation is directed to the same receiver as well. The superposition of the two waves with slightly different frequencies results in a signal which is called a beat. Figure 42 shows the dependence of signal U on time t when the beat occurs. If the difference in the frequencies of the two superimposed oscillations becomes less, the periods of the envelope of the signal and its high-frequency ‘filling’ differ more. One can eliminate the beat by regulating the frequency of the klystron. The elimination of the beat occurs when the frequencies of the two oscillations coincide precisely. Since the radiation frequency of the klystron and an auxiliary laser is known with high accuracy, the method of ‘zero beat’ makes it possible to find the frequency of the studied laser radiation.

The main experimental problem impeding the implementation of this method is that the efficiency of the elements transforming the radiation of a given frequency into the radiation of a greater frequency drastically decreases when the frequencies approach those of visible light. Therefore in reality it is only possible to conduct measurements of this kind in the near infrared. Thus another group of experimenters in the American National Bureau of Standards succeeded in measuring the infrared radiation frequency of a He-Ne laser ($\lambda = 3.39$ μm):

$$\nu = 88{,}376.181{,}627 \pm 0.000{,}050 \text{ GHz}.$$

This method to measure frequency is very complicated from the technical viewpoint. Is there any hidden danger of unaccounted errors in the determination of ν? One experiment answered this question. The frequency of a laser of another type, operating on carbon dioxide, was determined by a similar method in the British National Physical Laboratory. It was equal to $\nu' = 32{,}176.079{,}482 \pm 0.000{,}028$ GHz ($\lambda' = 9.3$ μm).

Naturally, it is infeasible to compare the frequencies obtained by the American and English physicists. However, it is possible to judge the *correctness of the technique* by the results of the calculation of the velocity of light (the methods of accurate measurements of radiation wavelengths were developed much earlier than the methods of measuring frequencies and therefore the determination of λ is considered to be a common procedure performed by researchers with approximately the same accuracy). According to the US National Bureau of Standards:

$$c = 299{,}792.4574 \pm 0.0011 \text{ km/s};$$

according to the British National Physical Laboratory:

$$c = 299{,}792.4590 \pm 0.0008 \text{ km/s}.$$

The accuracy in the determination of c was improved practically a hundred-fold with respect to Froome's experiments! At the same time, both results coincide within the accuracy of the conducted measurements. Note that the method of the determination of frequencies by means of modulating laser radiation produces the value $c = 299{,}792.462 \pm 0.018$ km/s.

This result, within the accuracy of measurements, *is in agreement with the data quoted* above although the relative error is greater.

Therefore the accuracy in the determination of c, obtained to date, amounts to $\simeq 3 \cdot 10^{-9}$. The situation is interesting: now the accuracy of the determination of c is limited by the indeterminacy of the standard of length, the radiation *wavelength of the isotope* ^{86}Kr.

This situation made scientists to approach the selection of the basic units of measurements, the units of length and time, anew. There were different suggestions to improve the definition of the unit of length, the metre. In particular, a question was posed: if the metre cannot be determined within the required accuracy, then isn't it better to avoid it at all? It is possible to select as standard, for instance, a certain frequency and ... the velocity of light, and find length through calculations. This was the path chosen by scientists in the development of a new definition of the metre.

It should be noted that the change in the determination of the metre is remarkable because the 'old' definition, based on the effect of radiation of the isotope ^{86}Kr, had been adopted at the 9th General Conference on Measures and Weights in 1960, and in 1983, barely 23 years later, it was reconsidered! Don't the scientists change the basic units of measurements too often? The answer is no, they don't: the introduction of a new determination of the metre was not a whim but it was necessary because the indeterminacy of the old definition (to be more accurate, the indeterminacy of the standard produced on its basis) impeded the progress in some areas of science. This concerns not only the problem of the determination of the velocity of light. For instance, the insufficiency in the standard metre did not make it possible to improve the results of many other measurements in astronomy and geodesy. Therefore this short 'life-span' in the definition of the metre was not accidental, but evidenced an extremely rapid development of physics, metrology, and engineering to date, in the period of scientific and technological revolution.

A discussion of the details of the new definition of the metre occupied several years, and it was accepted in October 1983, at the 17th General Conference on Measures and Weights. In compliance with the new definition,

> "Le mètre est la longueur dur trajet parcouru dans le vide par la lumière pendant une durée de 1/299,792,458 de seconde."

French is the definitive language of the International System of measurements (SI) and so any English translation must be considered with care, but the following is expected to be the internationally agreed English version:

> "the metre is the length of the path travelled by light in vacuum during a time interval of 1/299,792,458 of a second."

Let us try and analyse this definition in some detail. First of all, let us pose a question: why is it that the definition of the metre mentions light rather than electromagnetic waves in general? It should be emphasized that metrologists never employ arbitrary terms. In the meanwhile, the concept of light, i.e., the electromagnetic radiation in the region from infrared to ultraviolet parts of the spectrum, is essentially less wide than the concept of the 'electromagnetic radiation' in general. Then what is the meaning of this limitation in the definition of the metre? The scientists intended to give the metre a definition such that it would not be reconsidered for as long as possible. This required to use the concepts with the minimum chances of their further change or improvement. In our case, above all, the point is that vacuum is a nondispersive medium for electromagnetic waves. Although it follows from the Maxwell theory that electromagnetic waves of any wavelength propagate with the same velocity, only experiment can provide a final inference on the validity of this conclusion.

(The problem of dispersion of electromagnetic waves in vacuum is closely related to the problem of zero rest mass of the photon (see Conclusion). Therefore the selection of the visible range for the definition of the metre is often explained in scientific literature by the possibility to neglect the photon rest mass, if any, while maintaining the required accuracy in the standard metre. It should be remarked that both considerations are practically equivalent.)

According to modern concepts, if there is any dispersion of electromagnetic radiation in vacuum, the consequences of such a discovery will be minimum for metrology if the notion of light is used in its basic definitions. Indeed, as it has been mentioned above, the precise measurement of the velocity of propagation of radio waves or of the electromagnetic radiation of even greater wavelengths is complicated. The improvement in the accuracy of experiments and the application of short-wave length gamma radiation may lead to peculiarities related to the suggested quantum nature of space-time. Therefore metrologists decided to avoid the 'extreme' regions of the electromagnetic spectrum and used the notion of 'light' in the definition of the metre.

Two more points are interesting in the new definition of the metre. Firstly, it mentions the 'path travelled by light'. This means that the group velocity is implied. Although we know that the phase and group velocities of light are the same in vacuum because there is no dispersion there, the implicit use of the concept of the group velocity is advantageous: there is no need in this case to specify which shape of the wavefront is meant (flat, spherical, etc.). This problem is then related to the creation of the standard metre. Secondly, the selection of the time interval Δt in the definition of the metre requires elucidation. It seems that it would have been expedient, while changing the definition in principle, to choose a magnitude of the time interval being most convenient for memorizing and calculation (for instance, $\Delta t =$ 1/300,000,000 s) which corresponds to a 'definition' of $c = 300{,}000{,}000$ m/s. Probably, it would have been worth doing from 'aesthetic' considerations. However, there would have been then certain complication with the description and analysis of the results of precise measurements carried out before the introduction of the new definition of the metre. Besides, the problem of convenience of the calculations is not as acute in the era of computers as it was before. That is why metrologists selected the value of Δt cited above, which corresponds to the value of c recommended in 1975 by the 12th General Conference on Measures and Weights. When this value of Δt is employed, the results of the preceding measurements are not liable for reviewing.

We have discussed certain fundamental problems related to the new definition of the metre. However, not only the new definition per se is essential in metrology, it is no less important to indicate the technique of implementation of the *standard* metre in practice, so that the standards produced in different countries would be identical within a given accuracy. Two techniques to produce a standard metre were recommended. The first one consists in the determination of the path travelled in vacuum by light, whose velocity is taken to be $c = 299{,}792{,}458$ m/s during the time $\Delta t = 1/299{,}792{,}458$ s; therefore the reference metre in this case is produced directly on the basis of the definition. Because the recommendation is meant for practical production of a standard, it is specified that the wavefront of the light should be flat. The second technique to produce the reference metre proceeds from the determination of the wavelength of a flat light wave with a frequency ν, the wavelength being calculated from the relationship $\lambda = c/\nu$. The second technique was employed to produce the standard metre in the USSR.

To simplify the production of identical standard metres, requirements were also worked out for the lasers by means of which a reference metre can be implemented. These requirements have been formulated for a number of different lasers aiming at getting standard metres of various accuracy. In order to demonstrate how detailed the specifications are, let me quote them for the He-Ne laser providing a possibility to carry out measurements with the best accuracy known to date. The frequency of He-Ne laser is stabilized by means of a technique which was named the method of 'saturated absorption'. There is no possibility to describe the essence of this method here; suffice it to say that it employs the effect of absorption of radiation by methane molecules (CH_4) put into a special cell. The 'working' transition is that between the energy levels designated ν_3 [P (7)] for the component F_2^2. These symbols are hardly known to the general reader, but they accurately define the energy levels in methane molecules used in the method of saturated absorption. The value of frequency 88,376,181,608 ± 18 kHz is ascribed to this transition. It is also indicated in the specifications that the pressure of methane in the cell should not exceed 3 Pa, while the density of the radiation flux at the output should not be greater than 10^4 Wt/m^2. Specified also is the allowed curvature radius of the radiation wavefront ($\geqslant 1$ m) at the minimum reflection factor of the mirror located near the methane cell ($r = 0.95$). It should be added to the above that the processed results of measurement should be corrected for radiation diffraction, nonideal nature of vacuum, etc. Therefore, despite the outward simplicity of the new definition of the metre, the scientists engaged in the production of the respective standard are confronted by many technical problems. Nevertheless, there is no doubt that they will be solved successfully. The achievements of science and technology in the years to come will help decrease the indeterminacy in the standard metre. However, the definition of this value will hardly be reconsidered in the near future.

* * *

The fixation of the value of the velocity of light after the formulation of the new definition of the metre does not mean yet the end of the story of this fundamental constant. There are numerous other problems related to the 'greatest velocity'. Many of the problems covered in this book have not found a definite answer as yet.

This eloquently evidenced by the titles of the papers on the pages of modern scientific journals: for instance, *Eighth Velocity of Light* or *Is the Speed of Light Independent of the Velocity of the Source*?

Physicists continue to investigate the problem of whether the velocity of light is constant in time. There have been no indications that the value of *c* can change in time as yet, but physics cannot take anything for granted and neglect such a possibility. New measurements of the velocity of light will give much more data for the cognition of nature, which is inexhaustible in its infinite varieties.

Conclusion

The story of the velocity of light has come to an end. It was not too long because out of the numerous experiments on the determination of *c* I picked only those whose results were significant both for finding more accurate magnitudes of the velocity of light and for shaping our physical picture of the world.

The chain of events related to the story of the velocity of light has traversed many sections of the physical science. Roemer's and Bradley's astronomical observations proved the velocity of light to be finite, while the estimates produced on the basis of these observations made scientists familiar with new values of cosmic scale. The first 'terrestrial' experiments on the determination of the velocity of light solved the argument which lasted for a century and a half between the proponents of the wave and the corpuscular theories of light in favour of the former. The experiments on measuring the electromagnetic constant provided physicists with an important evidence on the relationship between optical and electromagnetic phenomena. The measurements of the velocity of light in different media helped strengthen the positions of the general theory of wave processes, where the concepts of phase and group velocity of light are most essential. The experiments in the area of optics of moving bodies played an important role in reviewing the properties of space and time and fortified the positions of the special theory of relativity. The measurement of the velocity of propagation of electromagnetic waves in various regions of the spectrum gave strong substantiation for the correctness of our fundamental views of the surrounding world. And finally, the latest measurements of the value of c make physicists once again tackle the problems of metrology, without which any scientific investigation of nature is futile. Astronomy, optics, electromagnetism, metrology, and the special theory of relativity are among the areas which are concerned with such a seemingly particular problem as the determination of the velocity of light. And yet, the velocity of light would not merit this detailed discussion if the problems related to this fundamental constant were not still of importance to science. Let us consider at least some of these problems.

Recall once again the second postulate of the special theory of relativity:

> The velocity of light in vacuum is the same in all inertial frames of reference in all directions and depends neither on the velocity of the source nor on the velocity of the observer.

We have already considered (see Chapter 6) the experiments proving the independence of the velocity of light of the motion of both the source and the observer. *Let us try and see how we* should understand the conditions of the validity of the second postulate. For instance, what does 'the velocity of light in vacuum' mean? The question seems at first glance to be idle: we can remove from a space every molecule, atom, and particle and obtain vacuum. However, in order to attain vacuum in the Einstein's sense of this word, it is not sufficient just to eliminate from a volume of space every atom, molecule, and particle, it is necessary also to get rid of the gravitational field. But there is no way of 'screening' out a gravitational field. So what is the sense of the second postulate if this condition of it cannot be valid. Is it possible to support the validity of the special theory of relativity at all? Is there any theory within whose framework the propagation of light in gravitational fields could be considered strictly? Let me first answer the last question. The answer is yes, there is such a theory. It is called the Einstein's general theory of relativity. Even a brief consideration of the basic principles of the general theory of relativity is far beyond this book, and therefore I shall limit myself with the formulation of the overall inference concerning the propagation of light in gravitational fields as given by Einstein himself:

> "... in general, rays of light are propagated curvilinearly in gravitational fields.
>
> "In two respects this result is of great importance.
>
> "In the first place, it can be compared with the reality.
>
> "Although a detailed examination of the question shows that the curvature of the light rays required by the general theory of relativity is only exceedingly small for the gravitational fields at our disposal in practice, its estimated magnitude for light rays passing the sun at grazing incidence

> is nevertheless 1.7 seconds of arc. This ought to manifest itself in the following way. As seen from the earth certain fixed stars appear to be in the neighbourhood of the sun, and are thus capable of observation during a total eclipse of the sun. At such times, these stars ought to appear to be displaced outwards from the sun by an amount indicated above, as compared with their apparent position in the sky when the sun is situated at another part of the heavens. The examination of correctness or otherwise of this deduction is a problem of the great importance, the early solution of which is to be expected of astronomers.
>
> "In the second place our result shows that, according to the general theory of relativity, the law of the constancy of the velocity of light *in vacuo*, which constitutes one of the two fundamental assumptions in the special theory of relativity and to which we have already frequently referred, cannot claim any unlimited validity. A curvature of rays of light can only take place when the velocity of propagation of light varies with position."

Then we can find in the same work by Einstein the answer to the question we posed above:

> "Now we might think that as a consequence of this, the special theory of relativity and with it the whole theory of relativity would be laid in the dust. But in reality this is the case. We can only conclude that the special theory of relativity cannot claim an unlimited domain of validity; its results hold only so long as we are able to disre-

gard the influence of gravitational fields on the phenomena (e.g., light)."

Therefore the analysis of light phenomena in gravitational fields could provide, on the one hand, a proof of the validity of the general theory, of relativity and, on the other hand, show the limited validity of the special theory of relativity.

The excerpts quoted above have been taken from Einstein's paper "On the Special and General Theory of Relativity" written in 1917. Only two years later, an English expedition headed by the physicist Arthur Stanley Eddington (1882-1944) carried out observations of stars during a total solar eclipse. These observations supported the prediction of the general theory of relativity. However, the accuracy of the observations was not good enough, and therefore similar observations continued to be a topical problem.

A considerable improvement in the accuracy of measurements was achieved when scientists began measuring the curvature of radio waves rather than light waves, using the radiation from quasars, bright radio sources. During the period from 1969 to 1976, the experiments in the region of radio waves were performed 12 times. The result was that the physicists proved that the observed curvature of the electromagnetic radiation in the solar gravitational field deviates from the value calculated on the basis of the general theory of relativity by no more than 1%.

To date astronomers discuss the features of a more complicated phenomenon, the gravitational lens effect. This effect has long since been predicted: for instance, the light travelling to the Earth from a quasar may deviate from a straight line by the gravitational field of a massive body and several images of the quasar may thus be produced in a telescope. According to the relevant theory, the quantity and the position of the images depend on the shape of the massive body which is called a gravitational lens. The picture observed through the telescope is also naturally defined by the position of the lens with respect to the quasar.

In 1980 a group of American astronomers discovered that the image of the quasar designated Q1115 + 080 consists of three close components. The images had very similar spectra, from which it was concluded that if the images were created by different objects, they were very close together. The probability of three similar quasars being so close to each other is infinitesimal. Therefore scientists believe that the 'triple' image in this case is the result of a gravitational lens. Nevertheless, the hypothesis requires verification. A verification is simple. We know that the brightness of most quasars in the visible range is variable. So brightness of each image over time should naturally vary in the same way. However, *the changes need not coincide*. Since the light for each image moves along its own path, any change in the brightness of an image may either lag or be ahead of the respective changes in the brightness of the other images. Consequently, if long-term measurements of the variations in the image brightness showed that they would occur simultaneously or with a certain constant delay in time, then the doubts in the discovery of a gravitational lens would be dispelled.

The general theory of relativity predicted one more effect related to the action of gravitation on light: a delay of an electromagnetic impulse in a strong gravitational field. This effect, which is 'akin' to the phenomenon of light ray curvature, was comparatively recently substantiated in experiments conducted 'at home', within the Solar system. The magnitude of the effect can be judged from the following example. According to the theory, the delay in the electromagnetic impulse emitted from Mars to the Earth at the moment of conjunction (i.e., when Mars, the Sun, and the Earth are approximately in the straight line) should amount to 2×10^{-4} s.

The experiment was conducted according to the following scheme. A powerful impulse of UHF radiation was directed, by means of the antenna of a terrestrial telescope, to an artificial satellite orbiting Mars. A retransmitter mounted on the satellite amplified the signal and sent it back to the radiotelescope. The sensitive equipment connected to the radiotelescope made it possible to measure the time of propagation of the signal to the satellite and back within the accuracy allowing scientists to reveal the effect of delay. The greatest accuracy was reached within the framework of the American project Viking. In a series of measurements carried out in 1979, the prediction of the general theory of relativity was corroborated within the accuracy of 0.2%.

It is clear from the quoted examples that the experiments and observations directly related to the problem of the velocity of light are considered even to date as the most essential methods of verification of such a fundamental physical theory as the general theory of relativity.

And now the final example. It concerns the problem of the photon rest mass. In the quantum theory of radiation, the photon rest mass is considered to be zero. However, this assumption is only a postulate of the theory or, in other words, a result of the generalization of experimental facts. In the meantime, not a single real physical experiment can prove with absolute accuracy that any magnitude, including the photon rest mass, is equal to zero with absolute accuracy. Physicists have to make do with the following type of assumption: "It follows from experiment that a photon rest mass m_{ph} amounts to no more than 10^{-n} of the electron rest mass m_{e}." If physicists ever get experimental results showing that $m_{\mathrm{ph}} \neq 0$, then they will have to reconsider many of the generally accepted theories. The rest mass of the photon is so fundamental that scientists strive to set off the upper limit of the possible value of m_{ph}, i.e., to increase n as much as possible.

However, what is the connection between the problem of the photon rest mass and the subject matter of our discussion, the velocity of light? The relationship proves to be a direct one. Recall that one of the most essential inferences of the Maxwell theory was that vacuum is a nondispersive medium. This conclusion remains valid within a more general theory, the quantum electrodynamics, in which it is assumed $m_{\mathrm{ph}} = 0$. From the viewpoint of quantum electrodynamics, the assumption of a nonzero photon rest mass would lead to that the velocity of light in vacuum would not be a fundamental constant but would depend on the energy of the photon. (The velocity of light in this case would not remain the maximum possible velocity of propagation of signals. However, even in this 'modified' theory, there should be a certain maximum velocity.) One of the results, for instance, would be that the velocity of blue light would be greater than that of red light.

However, the experiments carried out until present did not reveal any noticeable dispersion of electromagnetic radiation in vacuum. This makes it possible to estimate the upper limit of the photon rest mass. A group of Soviet physicists headed by Mandelshtam and Papaleksi showed that the difference between the velocity of radio waves with $\lambda = 300$ m and that of visible light does not exceed 5×10^{-4}. It follows from these measurements that

$$m_{ph} \leqslant 6.7 \times 10^{-43} \text{ g}.$$

This means that the mass of a photon cannot exceed 10^{-15} of the mass of the electron! And still, despite the fact that the analysis of measurements of the velocity of propagation of electromagnetic waves produced such a low magnitude, physicists try to employ other observations and measurements in order to decrease the upper limit of m_{ph}. Without going into details, let me tell the reader that the best estimate of m_{ph} known to date was obtained from an analysis of astronomical data and yielded

$$m_{ph} \leqslant 3 \times 10^{-60} \text{ g}.$$

It is interesting that an unbounded decrease of the upper limit of the photon rest mass makes no sense from the viewpoint of modern concepts. The point is that the effects produced by a non-zero photon mass can only be registered while carrying out experiments or observations in space dimensions exceeding the value of

$$\Delta_{ph} = \frac{h}{m_{ph}c}.$$

The time elapsed since the start of the Universe, according to modern concepts, is great but is finite, the space within which such observations can be performed in principle is given by $c\Delta t_U$, where Δt_U is the lifetime of the Universe. If we equate this value to Λ_{ph}, we can find the 'limit' value of m_{ph}: it follows that if the photon rest mass were less than 10^{-66} g, then we would be unable to find out whether it is nonzero photon mass. Therefore, the value of 10^{-66} g is the limit to which physicists intend to decrease the upper boundary of m_{ph}. It is curious that if m_{ph} were 10^{-66} g, the difference between the velocity of longwave radiation ($\lambda \simeq 10^3$ m) and that of gamma rays ($\lambda \simeq 10^{-17}$ m) would be

$$\frac{c\,(10^{-17}\ \text{m}) - c\,(10^3\ \text{m})}{c\,(10^3\ \text{m})} \simeq 10^{-22}!$$

It would nigh on impossible to measure such a tiny deviation.

* * *

The problems concerning the velocity of light are still intriguing and thought-provoking for scientists. The three centuries of the story of the fundamental constant c clearly demonstrated its relationships with the essential problems of physics. These relationships become ever more profound as science advanced and show themselves in many ways. This old constant is bound to bring us more news.

Other Books in This Series

M. KAGANOV, V. TSUKERNIK

The Nature of Magnetism

This popular-science physics book, dealing with precisely what its title suggests, exposes the reader to a fascinating interplay of classical, statistical, and quantum approaches to matter.

The book describes microscopic sources of magnetic field and explains why not all macroscopic bodies produce magnetic fields. The question why different bodies behave differently in response to an applied magnetic field is considered and ferromagnetism and antiferromagnetism are explained in detail. The study of magnetic structures and other interesting problems are also discussed.

L. TARASOV, A. TARASOVA

Discussions on Refraction of Light

The book treats refraction of light from two points of view. Firstly, it describes the many, and sometimes quite surprising, forms that the refraction of light can assume. Secondly, the book traces the long and difficult path mankind covered before it came to understand some of the mysteries of Nature. Recent inventions such as the laser and the transmission of images over optic fibres are also dealt with. The book was first published in Russian in 1982, and has now come out in English. We are certain it will be well received by those who are interested in the history of science and the present development of theoretical optics and its related technology as well as by all lovers of Nature.

SCIENCE FOR EVERYONE

The constant denoting the velocity of light in vacuum is encountered in every branch of physics, and this universality brings out the unity of the physical world. Although it is now over three hundred years since the constant was first defined, it yielded to scientific assaults only slowly, revealing as it did unexpectedly new phenomena. Its universality and the surprises that it threw up make any attempt to relate how the velocity of light came to be measured a minor history of physics. This book therefore explains the requisite science against the background of the historical personalities involved. Intended for teachers and school pupils.

ИЗДАТЕЛЬСТВО · МИР ·