YOUNG AND FRESNEL: A CASE-STUDY INVESTIGATING THE PROGRESS OF THE WAVE THEORY IN THE BEGINNING OF THE NINETEENTH CENTURY IN THE LIGHT OF ITS IMPLICATIONS TO THE HISTORY AND METHODOLOGY OF SCIENCE

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ABSTRACT

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This thesis aims to analyze how Thomas Young and Augustine Fresnel were successful in developing the wave theory of light at the beginning of the nineteenth century. The Methodology of Scientific Research Programmes will be used as the tool of analysis. Such a case-study will give the possibility to access the relative merits of the chosen methodology as well – it will help to point to the benefits of the method and to the parts which are open to further modification. That in turn will allow to discuss general issues appearing in the history and methodology of science, such as the applicability of a certain set of methodological rules to the process of theory-change throughout the flow of scientific development, the status of these rules as a guarantee of objectivity of scientific theories, and the possibility of scientific inquiry as objective process.

Keywords: Wave Theory of Light, Young, Fresnel, Methodology of Science.

ÖZ

YOUNG VE FRESNEL: BİLİM TARİHİ VE METODOLOJİSİNE ETKİLER IŞIĞINDA ON DOKUZUNCU YÜZYILIN BAŞINDA DALGA TEORİSİNDEKİ GELİŞMEYİ İNCELEYEN DURUM ÇALIŞMASI

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Bu tez, Thomas Young ve Augistive Fresnel'in, on dokuzuncu yüzyılın başındaki dalga ışık teorisinin gelişiminde ne kadar başarılı olduklarını incelemektedir. İnceleme aracı olarak, Bilimsel Araştırma Programların Metodolojisi kullanılacaktır. Bu durum çalışması, seçilmiş olan metodolojinin göreceli değerlerinin erişimine de olanak verecektir – çalışma, metodun faydalarına ve düzeltmeye açık noktalarına işaret etmesinde yardımcı olacaktır. Bu da, bir takım metodolojik kuralların teoriler değişimi sürecine bilimsel gelişimin boyunca uygulanabirliği, bu kuralların bilimsel teorilerin objektiflik garantisi olarak statüsü ve bilimsel araştırmanın objektif bir süreç olarak sürdürme olasılığı gibi bilim tarihi ve metodolojisinde çıkan genel meselelerini tartışmak için olanak sağlayacaktır.

Anahtar Kelimeler: Dalga Işık Teorisi, Young, Fresnel, Bilim Metodolojisi.

To My Dear Mother

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CHAPTER I

INTRODUCTION

Through the course of its development, scientific activity gives birth to a certain amount of examples, which in turn become quite interesting to investigate from the standpoint of the methodology of science. One of the most suitable candidate for such a methodological analysis would be the process of theory-change. It is found to be convenient since the examination of the shift from one scientific theory to another allows to track and understand the processes taking place during the change by the help of the chosen methodological "tools" of analysis. Moreover, such examination gives a chance to evaluate chosen methodological set of rules as well, test them "in action", so-to-speak. The shift of "paradigms" from Ptolemy's geocentric system to Copernicus's heliocentric picture of the universe, or shift from Newtonian mechanics to the General Theory of Relativity – these can be noticed as the most prominent examples used for such purpose. Still, there are other cases in the history of science which deserve close attention. One of these can be found in optics at the beginning of the nineteenth century, when the particle theory of light, developed by Isaac Newton, was abandoned in the favor of the wave theory of light. This case is fruitful from the methodological point of view not only as a bright example of the process of theory-change; a closer look at it uncovers an interesting fact in that switch.

When one reads a book in the history of the wave theory of light (also to be referred as "the wave theory" further), two scientists can be separated as major dedicators to this hypothesis – Thomas Young (1773 – 1829) and Augustine Fresnel (1788 – 1827). The former worked in England; the latter developed his hypothesis in France. There appears to be no concern at the first glance; but a closer examination would show that Young established his theory of light in the early 1800s, but the acceptance of the wave theory among scientists began only twenty years later, after Fresnel announced his hypothesis. If Young had successfully demonstrated his theory as valid, why then it was neglected for almost twenty years? Which factors played role in such a delay? What was there in Fresnel's formulation about the wave nature of light that allowed to convince the men of science in its validity; and how in that respect Young's work left incomplete? The current thesis would start its inquiry from this question. As the tool of analysis, the Methodology of Scientific Research Programmes developed by Imre Lakatos (Lakatos, 1978) would be incorporated; as has been stated above, it would also give the material to investigate the chosen methodology as well. That in turn would open the door to more general investigation in the methodology and philosophy of science. It would help to put an insight on the issues related to the objectivity of scientific inquiry and the progress in science, grasped by the help of methodological merits. Are there any valid methodological rules which can be applied to the context of discovery and the process of theory-change? Can these rules guarantee the objectivity of the scientific theories? Should the objectivity in science be traced in the retrospective manner, or there is a possibility to "predict" the path of its development? In general, is there such a thing as objectivity of scientific inquiry, or a viable answer to the skeptics would never be gained? A case-study would be much helpful in tracing a light on these questions.

The thesis is divided into six chapters. In Chapter I, the historical picture in the wave theory till the beginning of nineteenth century is discussed. It would enable to uncover theoretical and experimental background within the borders of which Young and Fresnel developed their theories. Chapter II covers the work of Young through his main manuscripts on optics. Chapter III describes how Fresnel built his hypothesis, which was rewarded with the Prize of Academy in Paris in 1819. Chapter IV summarizes the standpoint of Lakatos' Methodology of Scientific Research Programmes, and shows why this set of methodological tools is effective in analyzing the Young-Fresnel case. Chapter V illustrates the views of other authors in the methodology and philosophy of science who evaluated the case as well. In Chapter VI, certain conclusions are driven about the process of theory-change in optics at the beginning of the nineteenth century in particular, and about the methodology of science in general. An effort is made to answer the questions stated a few lines above.

CHAPTER II

HISTORICAL BACKGROUND: THE HERITAGE ACCOMODATED IN THE WAVE THEORY TILL 1800

Thomas Young and Augustine Fresnel can be underlined as the scientists with the greatest contribution to the development of the wave theory of light; but as the history of the wave hypothesis in optics shows neither of them was the founder. The acceptance of the wave theory might have begun in 1820s, but the roots of its birth go back to the seventeenth century. Thus, there are around two hundred years throughout which the hypothesis developed its theoretical and experimental grounds. These grounds were developed by the great thinkers of that time; among them the names of René Descartes, Robert Hooke, Francesco Grimaldi, Isaac Newton, Christiaan Huygens and Leonhard Euler should be given close attention as important dedicators to the wave theory. The contributions are described below in the chronological order, covering the period between the beginning of the seventeenth century and the end of the eighteenth century.

There are no historical records about the founder of the wave theory; such a judgment can be deduced since commentators in the history of optics do not mention any concrete name:

> Oddly enough it is not known who first suggested the wave theory of light; for none of the sixteenth or seventeenth century writers, Descartes, Grimaldi, Hooke or Huygens claim it as their own idea (Crew, 1930: 4).

It is worth to start the current point of examination with Descartes; there are parts in his hypothesis which are essential for the wave theory.

René Descartes (1596 – 1650) is a French philosopher who contributed a lot to the history of philosophy and science. Regarding physics, Descartes can be treated as the first thinker after the Middle Ages to write the history of this discipline properly enough (Whittaker, 1951: 4). He denied the concepts of "action at a distance" and empty space; having that in mind he, for the first time in the history of physics, gave definition to the concept of aether:

> Space is thus, in Descartes' view, *a plenum*, being occupied by a medium which, though imperceptible to the senses, is capable of transmitting force, and exerting effects on material bodies immensed in it – *the aether*, as it is called. […] Before Descartes, it [aether] had connoted merely the occupancy of some part in space: he was the first to bring the aether into science, by postulating it had mechanical properties (Whittaker, 1951: 6).

Descartes thought of space as a matter, consisting of small spherical particles (Whittaker, 1951: 8). He explained the colours of the spectrum accordingly: different colours of light are observed due to the different speed of movement of the spherical particles in space. The fastest particles give the sensation of red, and so forth in the spectrum till the slowest moving particles of blue (Whittaker, 1951: 9).

The laws of reflection and refraction have not escaped Descartes' attention either. The law of reflection, which holds that angle of an incidence is equal to the angle of reflection, was well known from the times of Greeks and posed no problem among physicists (Whittaker, 1951: 10). Descartes succeeded to formulate the law of refraction as well, but this formulation might be written as a dept to Willebrord Snell (1580 – 1626). Snell discovered the law experimentally but had not published the result. There is a historical evidence that Descartes had a chance to examine Snell's experimental notes (Whittaker, 1951: 10). In any case, Descartes's law of refraction appears in the form "$\sin i = \mu \sin r$", where "i" stands for the angle of incidence, "r" denotes to the angle of refraction, and "μ" – to the refractive index of the media (Whittaker, 1951: 10).

Descartes's deductions pushed optics to pass through the slack period of the Middle Ages. His considerations were crucial in changing the path of inquiry in optics to develop in the more organized pattern. Descartes made effort to describe "aether" concretely as a substance with certain mechanical characteristics. As a result, he directed his successors to work within the certain theoretical framework. Another important contributor to the wave theory of light was Robert Hooke (1635 – 1703). According to Whittaker, Hooke, in his *Micrographia* (1667), described a number of important observations which played their role in the development of the wave theory (Whittaker, 1951: 13). Firstly, Hooke observed the phenomenon of colours of thin plates, or the phenomenon of "Newton's Rings" as it was later called. The observation can be described as follows:

> The first was the observation of the iridescent colours which are seen when light falls on a thin layer or air between two glass plates or lenses, or on a thin film of any transparent substance (Whittaker, 1951: 13).

The rainbow of colours one sees on the soap bubble when the sun shines at it is actually the phenomenon of Newton's Rings. Secondly, Whittaker remarks that Hooke recorded the phenomenon of diffraction, or the phenomenon in which "light in air is not propagated exactly in straight lines, but that there is some illumination within the geometrical shadow of an opaque body" (Whittaker, 1951: 13).

In fact, the name "diffraction" came from the experiment of Francesco Grimaldi, who in 1665 observed the light, directed to a certain opaque body, to "bend" over it. Grimaldi called this bending of light within the geometrical shadow as "diffraction" (Whittaker, 1951: 14).

There is one more important deduction from Hooke's *Micrographia* that, according to Whittaker, is important to notice. Hooke understood light as forward and backward movement of light particles with very small amplitudes. He called such movement as "vibration", or "pulse". These light particles "vibrate" in the homogeneous medium with equal velocity. Hooke continued:

> …Every pulse or vibration of the luminous body will generate a Sphere, which will continually increase, and grow bigger. […] Whence it necessarily follows, that all the parts of these Spheres undulated through

> an Homogeneous medium (Hooke, 1667. Quoting from Whittaker, 1951:15).

In concordance with Whittaker, it may be argued that by the “Spheres” Hooke means wave-fronts, or “locus at any distant of a disturbance generated originally at a point” (Whittaker, 1951. 15). This fact is important since it was for the first time in optics when a wave-like motion of light was described:

> …One of the early opinions that light consists of a wave-motion came from Robert Hooke (1635 – 1703) who in his Micrographia (1665) insisted that the disturbance which produces light, whatever it may be, has three distinguishing features, namely; *it is vibrative; it is short; it is quick* (Crew, 1930: 4).

Important inferences in optics were also made by Isaac Newton (1642 – 1727). His understanding of elastic medium did not differ much from the mainstream conception of that time. As Whittaker remarks, Newton thought that

> all space is permeated by an elastic medium or aether, which is capable of propagating vibrations in the same way as the air propagates the vibrations of sound, but with far greater velocity” (Whittaker, 1951: 19).

Still, for Newton light and aether did not constitute for the same thing (Whittaker, 1951:19). Light would rather consist of rays or streams of corpuscules emitting from a luminous point (Whittaker, 1951:20). Newton draw his own connection between light and aether: “In any case, light and aether are capable of mutual interaction; aether is in fact the intermediary between light and ponderable matter” (Whittaker, 1951:19).

It can be seen that Newton did not choose waves to formulate his conception of the nature of light. He introduced the “corpuscules”; under his authority that theory, under the names of “corpuscular theory”, “particle theory”, or “emission theory” (each denotes to the same hypothesis), would receive a great amount of attention and would be developed by scientists till nineteenth century. In fact, as Crew notes, Newton himself was not a severe opponent of the wave theory of light. He was doubtful about the point that waves can move in straight lines; since in his set of theoretical beliefs the assumption that light should move in straight lines was indisputable, Newton went for the particle alternative. He had clearly stated it:

> To me, the fundamental supposition itself seems impossible, namely, that the waves or vibrations of any fluid can, like rays of light, be propagated in straight lines without a continual and very extravagant spreading and bending every way into the quiescent medium where they are terminated by it. I mistake if there be not both experiment and demonstration to the contrary (Newton, Phil. Trans. For Nov. 1672, Abstract I, p. 162. Quoting from Crew, 1930: 6).

There is one more thing to be discussed about Newton's work – the phenomenon of Newton's Rings. Newton performed a vast amount of observations in order to understand and explain the phenomenon. As the result, he proposes the theory of "fits of easy transmission and easy reflection", which can be described as follows:

> Every ray of light, in its passage through any refracting surface, is put into a certain transient constitution or state, which, in the progress of the ray, returns at equal intervals, and disposes the ray, at every return, to be easily transmitted through the next refracting surface, and, between the returns, to be easily reflected by it (Newton, Opticks, ii, prop. 12. Quoting from Whittaker, 1951: 21).

It should be added that "the interval between two consecutive dispositions to easy transmission or 'length of fit' varies, as he [Newton] found, with the colour, being greatest for the red light and least for violet" (Whittaker, 1951: 21). The phenomenon of the coloured rings appearing when the light is pointed to the two plates attached to each other with the layer of air between them would remain a complicated issue until the wave theory would bring its explanation by the help of the principle of interference.

If Robert Hooke was the first to describe the nature of light as waves spreading in the elastic medium, Christiaan Huygens (1629 – 1695) was the first in his attempt to advance the wave theory (Whittaker, 1951: 23). According to Whittaker, Huygens thought of light to consist "of disturbances, propagated with great velocity, in a highly elastic medium composed of very subtle matter" (Whittaker, 1951:24). It is crucial to provide the principle formulated by Huygens, which nowadays carries his name. This is how Whittaker describes it:

> Consider a wave-front, or locus of disturbance, as it exists at a definite instant t_o; then each surface-element of the wave-front may be regarded as the source of a secondary wave, which in a homogeneous isotropic medium will be propagated outwards, from the surface-element in the form of a sphere whose radius at any subsequent instant t is proportional to (t-t_o); and the wave-front which represents the whole disturbance at the instant t is simply the envelope of the secondary waves which arise from the various surface-elements of the original wave-front (Whittaker, 1951: 24).

This principle allowed Huygens to provide explanations to the phenomena of reflection and refraction operating with the concept of secondary waves constituting a wave-front at a certain moment of its propagation (Whittaker, 1951: 25). As would be illustrated later in details, without the Huygens' principle it would be hardly possible to give valid theoretical grounds to the phenomenon of diffraction:

> …What we are indebted to Huygens? The reply is, I think, that we owe to him a clear conception of the wavefront as the envelope of an infinite number of elementary waves… An idea which led, in later hands, directly to the explanation of the rectilinear propagation of light and of a host of other diffraction phenomena (Crew, 1930: 5).

The only thinker left to be mentioned here is Leonhard Euler (1707 – 1783). According to Whittaker, Euler became attracted to the wave theory by the fact that if light particles had been emanating from a source of light, that process would have caused the decrease in the mass of the source; but it is not the case when it comes to observation (Whittaker, 1951: 97). Emission of the waves do not produce such an effect, thus it appeared more reasonable for Euler to work on the wave theory.

As Whittaker remarks, Euler strongly believed in resemblance between light and sound. He thought that light waves propagate in the elastic aether just as sound waves spread in the air:

> he [Euler] insisted strongly on the resemblance between light and sound; the whole of the space through which the heavenly bodies move is filled with a subtle matter, the aether, and light consists in vibrations of this aether; ‘light is in the aether the same thing as sound in air’ (Whittaker, 1951: 98).

Moreover, Crew makes the same point by saying that

> …The great Euler (1707 – 1783) poses as the champion of the wave theory of light. He bases everything upon the analogy between light and sound. He has no conception either of interference or of traverse waves; but he was too keen a thinker to deceive himself into the belief that he had made any real contribution to the subject of optics” (Crew, 1930: 7).

What then was accomplished concerning the wave theory of light in the period of two hundred years before Young?

The concept of aether was well elaborated; its mechanical characteristics of elasticity were described by the each thinker mentioned above. Light was supposed to be a “disturbance”, “vibration”, or “pulse”, spreading from the source of light with the great (but finite) velocity in the aether. The phenomena of diffraction and Newton’s Rings were observed; but none of them was successfully explained by the wave theory. On the other hand, the laws of reflection and refraction were explained by the means of the wave theory, owing to Huygens. Huygens made another gift for his successors – the Huygens’ principle, which would later help Fresnel a lot. Nevertheless, theoretically and experimentally, that was not so much in order to call the wave theory as mature scientific “block” to replace its competent, particle theory of light. It is not to say that the emission theory succeeded to account for all optical phenomena described at that time; even if the particle theory had explanations for the phenomena of reflection, refraction or the colours of the thin plates, the phenomenon of diffraction, for instance, was “new” for the both candidates. A lot of work should have be done for the wave theory to stand as a viable alternative uncovering the nature of light. The great amount of it would be performed by Thomas Young and Augustine Fresnel.

CHAPTER III

THOMAS YOUNG AND THE PRINCIPLE OF INTERFERENCE

Thomas Young was born in Milverton, England, in 1773. He grew up as a very talented child, and knew six languages by his teen age. He might be considered as "one of the great linguists of the nineteenth century" (Rothman, 2003: 13). At the age of nineteen Young became a medical doctor; he continued his education in Edinburgh and Göttingen, "where he wrote a dissertation on the human production of sound" (Rothman, 2003: 13). In 1800, "he completed his medical studies at Cambridge, and settled as practising physician in London" (Crew, 1900: 77). It was in 1800 when Young composed his paper, "Outlines of Experiments and Inquiries Respecting Sound and Light", on the nature of sound and submitted it to the Royal Society. The paper includes a section called "Of the Analogy Between Light and Sound", which is the starting point of investigation in this Chapter.

In the "Of the Analogy Between Light and Sound" section of his "Outlines of Experiments and Inquiries Respecting Sound and Light", Young mentions Newton and the emission theory of light, claiming it to be "universally admitted in this country, and but little opposed in others" (Young, 1800: 125). By "others" Young means Euler and Huygens, and their theories of vibrations in the aether. He justly notes that the hypotheses of these thinkers were not "sufficiently powerful and liable to be attacked on many weak sides" (Young, 1800: 125). Young then presents his own considerations to show that the wave theory is not that hopeless.

Firstly, Young expresses his thoughts about aether. He claims that existence of the elastic medium is undeniable, as was clearly showed by Euler. Then he supposes that, on the resemblance with sound in the air, elastic aether protects the waves of light from divergence:

> …Sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small… (Young, 1800:126).

Secondly, Young claims that the suggestion about colours of light occurring due to the difference in vibration frequency might also be deduced from the resemblance between light and sound:

> …It is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ pipes. […] It appears, from the accurate analysis of the phenomena which Newton has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of an uniform blast, from organ-pipes which are different multiples of the same length (Young, 1800: 129).

The remark about the resemblance issue should be left here. It was not only Young who used analogy between light and other phenomena. It might be supposed that resemblance helped thinkers to understand and grasp the process of wave propagation, since it was not something visible. In any case, the quotations provided below might clarify that point:

> Every pulse or vibration of the luminous body will generate a Sphere, which will continually increase, and grow bigger, just after the same manner (though indefinitely swifter) as the waves or rings on the surface of the water do swell into bigger and bigger circles about a point of it, where by the sinking of a stone the motion has begun (Hooke, *Micrographia*, p. 55. Quoting from Whittaker: 14).

Newton also draw the analogy between light and sound:

> …If by any means those [aether vibrations] of unequal bignesses be separated from one another, the largest beget a Sensation of a Red colour, the least or shortest of a deep Violet, and the intermediate ones, of intermediate colours; much after the manner that bodies, according to their several sizes, shapes and motions, excite vibrations in the Air of various

> bignesses, which, according to those bignesses, make several Tones of Sound (Newton, Phil. Trans., 7, 1672: 5088).

Huygens used the resemblance between light and sound as well:

> If, in addition, light requires time for its passage, it will then follow that this motion is impressed upon the matter gradually, and hence is propagated, as that of sound, by surfaces and spherical waves. I call these waves because of their resemblance to those which are formed when one throws a pebble into water and which represent gradual propagation in circles, although produced by a different cause and confined to a plane surface (Huygens, 1690: 11. Quoting from Crew, 1900).

Young continued his investigations in optics; he wrote a number of papers in this field, “The Bakerian Lecture: on the Theory of Light and Colours” (1802a), “An Account of Some Cases of the Production of Colours, not Hitherto Described” (1802b), and “The Bakerian Lecture: Experiments and Calculations Relative to Physical Optics” (1804), all of which were published in the *Philosophical Transactions*. The papers are described below, accordingly.

In his 1802a, Young tries to formulate the fundamentals of the theory in optics he would like to advocate. He states that a closer examination of Newton’s works show that the wave theory does not seem to contradict the Newton’s opinion:

> A more extensive examination of Newton’s various writings has shown me that he was in reality the first that suggested such a theory as I shall endeavor to maintain; that his own opinions varied less from this theory than is now almost supposed; and that variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works… (Young, 1802a: 48)

Following that way of thought, Young composes a number of hypotheses in the favour of the wave theory, which would “coincide more nearly with Newton’s own opinions” (Young, 1802a: 14).

The first hypothesis comprised by Young states that the universe is filled up with highly elastic ether (Young, 1802a: 14). As it was remarked above, Newton defined the aether in almost the same way. Young confirms that fact once again by quoting Newton:

> …It is to be supposed that there is an ethereal medium, much of the same constitution with air, but far rarer, and more strongly elastic” (Birch, Hist. of R.S., vol. iii, p.249, 1675. Reference and quoting from Young, 1802a: 15).

The second hypothesis Young put forward declares that light is nothing else but undulations excited in the ether (Young, 1801: 16). Young finds the same assumption in Newton:

> Were I to assume an hypothesis, is should be this, if propounded more generally, so as not to determine what light is further than that it is something or other capable of exciting vibrations in the ether; for thus it will become so general and comprehensive of other hypotheses as to leave little room for new ones to be invented (Birch. Vol. III, p.249, 1675. Reference and quoting from Young, 1802a: 16).

It is worth to mention the third hypothesis before continuing with Young’s considerations. It states that “the sensation of different colours depends on the different frequency of vibrations excited by light in the retina” (Young, 1802a: 18). He does not forget to furnish the hypothesis with Newton’s words:

> …The agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the ether of various depths or bignesses, which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white color; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red color; the least, or shortest, of a deep violet, and the intermediate ones of intermediate colors… (Newton, Phil. Trans. Vol. VII, p.5088. Reference and quoting from Young, 1802a: 18)

After defining the hypotheses, Young proceeds to describe the set of propositions, which would constitute the theory in optics he would defend. Taking the historical background presented above into consideration and aiming to trace how it was possibly expanded by Young, the most important of them can be introduced as follows.

Proposition II states that

> *An undulation conceived to originate from the vibration of a single particle must expand through a homogeneous medium in a spherical form, but with different quantities of motion in different parts*" (Young, 1802a: 23).

There are a couple of facts which can be extracted from this proposition. First of all, it should be understood that the aether, according to Young, consists of moving particles, the vibration of which produce undulations, or waves. Secondly, the spherical path of movement and the difference in motion in the various parts of the sphere much resembles Huygens' wave theory. Actually Young would state that Huygens succeeded to explain this complicated matter: "The theory of Huygens, indeed, explains the circumstance in a manner tolerably satisfactory" (Young, 1802a: 24).

Proposition VIII is one of the most important assumptions not only in this paper, but in the history of the wave theory as well. Formulated by Young, it would initiate the history of interference – the principle which gave the victory to the wave theory of light. Here is the original version of it:

> "*When two undulations, from different origins, coincide either perfectly or very nearly in direction, their joint effect is a combination of the motions belonging to each*" (Young, 1802a: 34).

A remark on the principle of interference should be included here in order to understand what Young means by his proposition. In its modern sense, when two beams of light (waves of light) interfere, the crests and troughs (see Figure 2.1 for details) of the waves correlate in a special way; in fact, they *superpose* on each other either constructively or destructively. In the points where crest meets crest, the waves interfere constructively; in the points where crest meets trough, there is a destructive interference. If one examines the process of interference on the observational screen, it would be seen that the points of constructive interference produce bright fringes, and those of destructive interference – dark ones. The process allows the possibility of putting two portions of light together and observe the darkness as a result. It should be also mentioned that interference is the principal characteristic of a wave; particles do not have the capacity to interfere.

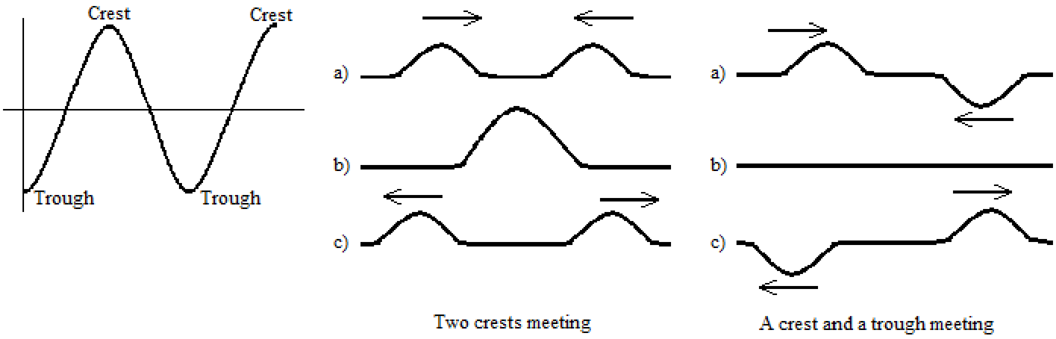


Figure 2.1. Constructive and destructive interference. Baierlein, 1993.

Now, returning to the Young's proposition about interference. He talks about two waves from *different sources*, which, falling *in the same or "nearly" the same direction*, combine their motions. Since Young left the proposition without any experimental grounds, it is hard to judge whether he understood the principle of interference as it is known today. To what extent Young's proposition was successful is the point of further investigation.

Before continuing with the next paper, it is worth to summarize Young's 1802a work. It can be seen that his theoretical grounds were not much different from the background accumulated before him. The elastic aether, its mechanical properties, the nature and structure of light and colours – all these can be found in Hooke, Newton and Huygens. The principle of interference Young brought as a new theoretical aspect of the wave theory should be treated as a remarkable development in its history. It should be traced how this theoretical tool gained power in the hands of its discoverer.

In his 1802b, Young examines how the principle of interference, defined as proposition in his 1802a, can produce an explanation to the phenomenon of the colours of thin plates, or Newton’s Rings as was stated in Chapter I. He modifies the definition, which in this paper occurs as follows:

> Wherever two portions of the same light arrive at the eye by different routes, either exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any multiple of a certain length, and least intense in the intermediate state of the interfering portions; and this length is different for light of different colors” (Young, 1802b: 387).

It might be supposed that by the “length” Young means different frequencies of light, which are different for every color in the spectrum. Young would call the principle defined above as “the general law of interference” (Young, 1802b: 392), and would try to explain the phenomenon of the thin plates in accordance with this law. He provides the reader with several observations; the one performed with two pieces of flat glass is represented here.

One of the Young’s observations of the colours of thin plates involved looking at the two pieces of flat glass with water layer between them in front of the candle-light (Young, 1802b: 390). He realized that the fringes observed in this case are much larger than those produced by the plates with the air among them. Young explains that it happens as the

> light transmitted through the water, moving in it with a velocity different from that of the light passing the interstices filled only with air, the two portions would interfere with each other and produce effects of color according to the general law (Young, 1802b: 391).

From the observation with the two pieces of flat glass Young makes a very important deduction about the speed of light in different media, which nowadays proved to be the case:

> In applying the general law of interference […], I must confess that it is impossible to avoid another supposition, which is a part of the undulatory theory – that is, that the velocity of light is the greater the rarer the medium…” (Young, 1802b: 392).

There might be no definite experimental results, but the suggestion, as the history of optics has shown, got the jackpot: in 1850, the experiment of Foucault and Fizeau demonstrated that the speed of light in water is less than that in the air (Whittaker, 1951: 127).
There is one more work by Young left to be described. In his 1804, Young takes a try to demonstrate the phenomenon of diffraction, incorporating the "general" law of interference, with two experiments, "which may be repeated with great ease, whenever the sun shines, and without any other apparatus than is at hand to every one" (Young, 1804: 2).

The first experiment is made with the portion of light, directed from the small hole in the window-shutter to the "slip of card, about one-thirtieth of an inch in breadth" (Young, 1804: 2). The shadow of the card is then observed on the wall, or on the other cards placed at the various distances. Young detects colourful fringes on the both sides of the shadow. Moreover, there are fringes observed within the shadow as well. Young explains that "these fringes [fringes within the shadow] were the joint effects of the portions of light passing on each side of the slip of card, and inflected, or rather diffracted, into the shadow" (Young, 1804:2).

The second experiment Young describes is very similar to that of Grimaldi, who, as it was stated in Chapter I, observed the fringes within the shadow of an object when light is directed on it. Young states that such an observation can be achieved by the same apparatus as was used in the first experiment, with only difference that the card, the shadow of which is examined, should have rectangular edges (Young, 1804: 3). Young supposes that "These fringes are also the joint effect of the light which is inflected directly towards the shadow, from the each of the two outlines of the object" (Young, 1804: 3).

Young then introduces the table of experimental results, consisting of four entries. Two of them were performed by Young, the other two were taken from Newton. These two results were taken into consideration for the purpose of comparison, as

Young claims (Young, 1804: 4). The table 2.2. provides two entries to satisfy Young's aim.

Table 2.1. Young's observations on diffraction. All values are in inch. Young, 1804.

Table II. Obs. 3. N.

Breadth of the hair		$\frac{1}{280}$
Distance of the hair from the aperture		144
Distances of the scale from the aperture	150	252
Breadths of the shadow	$\frac{1}{54}$	$\frac{1}{9}$
Breadth between the second pair of bright lines	$\frac{2}{47}$	$\frac{4}{17}$
Interval of disappearance, or half the difference of the paths	.0000151	.0000173
Breadth between the third pair of bright lines	$\frac{4}{73}$	$\frac{3}{10}$
Interval of disappearance, ¼ of the difference	.0000130	.0000143

Table III. Exper. 3

Breadth of the object	434
Distance of the object from the aperture	125
Distance of the wall from the aperture	250
Distance of the second pair of dark lines from each other	1.167
Interval of disappearance, 1/3 of the difference	.0000149

It should be noted here that there are no results related to the second experiment described a few lines above; Young did not give any reason on their absence in the paper. It is also quite interesting why Young took Newton's results to compare with his own. It might be supposed that examination of Newton's experimental results appeared suitable for Young's point of investigation: the aim of the observation appeared to be tested by the same parameters, or there was a possibility showed up to Young to interpret Newton's results in the way suitable for his demonstration. Still, in such a case the certain amount of interpretation is inescapable, which in turn would reduce the overall objectivity content of the result.

It was all to be extracted from Young's works in optics. It can be said that Young brought a lot of "new" theoretical data to the wave theory. His introduction of the principle of interference and further construction of explanations of diffraction and Newton's Rings on its grounds constituted a huge step forward in the development of the wave theory. But all that was still not enough to establish it as the one correct alternative explaining the nature of light. Notwithstanding such a great theoretical work, men of science had not paid much attention to the wave theory. The change started only after Fresnel was awarded for the Academy Prize in Paris fifteen years after Young's paper was published in 1804. It would be even more interesting to understand why such a delay took place.

CHAPTER IV

AUGUSTINE FRESNEL AND THE CELEBRATED ACADEMY PRIZE

Jean Augustine Fresnel (1788 – 1827) was born in Normandy in 1788 (Crew, 1900: 156). He graduated from the Ecole Polytechnique, where he was studying civil engineering. After the graduation he worked as a civil engineer, and turned his attention to science only a couple of years later. Fresnel did not start his investigation from optics; he firstly tried to develop a theory in chemistry related to the cheap production of soda, but problems with obtaining immediate results from such an activity might then bring him to optics (Buchwald, 1989: 113). There exists an explanation for such a behavior: "Fresnel's letters clearly reveal that he was extremely eager to make a discovery of almost any kind" (Buchwald, 1989: 113). After unsuccessful trials in the field of engineering or chemistry and ambitions remained unsatisfied, Fresnel started to pay close attention to optics. In any case, following Buchwald's way of argument, it may be stated that Fresnel's prior interest after the graduation from Ecole was not optics.

There is no certain information about how Fresnel got interested in the wave theory of light:

> Certainly by 1814 he saw it – in its assimilation of both light and heat to vibrations in an ether – a solution to the more damaging, in his view, objections to contemporary theory" (Buchwald, 1989: 116).

It might be supposed that Fresnel heard and was aware of the "analogies" made at the time by various scientists:

> I tell you I am strongly tempted to believe in the vibrations of a particular fluid for the transmission of light and heat. One would explain the uniformity of the speed of light as one explains that of sound; and one might perhaps see, in the derangement of the fluid's equilibrium, the cause of electric phenomena (Fresnel, Oeuvres, 2: 821 – 22. Quoting from Buchwald, 1989: 116).

Starting from 1814, Fresnel wrote to Ampere, which initiated long and friendly conversations between them. Around the same year he met with François Arago, French politician and scientist, and by that time he might become more acquainted with the wave theory (Buchwald, 1989: 117). Arago had his own investigations in optics; he believed the wave theory to be the only acceptable hypothesis in explaining the nature of light. He became attracted to Fresnel's interest in optics and started close correspondence with him. These conversations with Arago was to some extent helpful for Fresnel to get acquainted with the stage of development of the wave theory (Buchwald, 1989: 117).

Fresnel started his investigation from observations of a shadow coming from an obstacle, and the fringes formed by it.

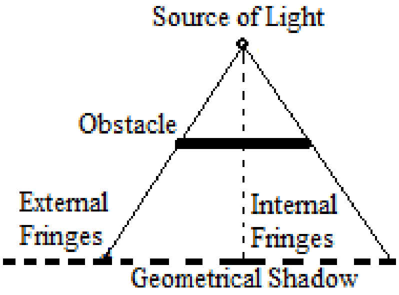


Figure 3.1. External and internal fringes formed by diffraction from an obstacle.

As can be seen from the figure 3.1., the fringes formed by diffraction of light from an obstacle (or, as has been stated before, "bending" of light over the edges of an obstacle) are figuratively divided to internal and external in respect to the geometrical shadow of an object on the observational screen. In his first examinations, Fresnel firstly concentrated on external fringes, but then noticed that, when the light is completely blocked, internal fringes totally disappear, which led him to conclude that rays striking from the sides of an object were the cause for the fringe pattern:

> For a long time I stopped at the external fringes, which are the easiest to observe, without bothering about the internal fringes. These latter are the ones that finally led me to an explanation of the phenomenon. I had already many times glued a small square of black paper to one side of an iron wire that I used in my experiments, and I had always seen the fringes inside the shadow disappear opposite this paper; but I was seeking only its influence on the external fringes, and I shut my eyes to the remarkable consequence that this phenomenon was leading me to. It struck me as soon as I occupied myself with the internal fringes, and I at once had the following thought: since intercepting the light from one side of the wire makes the internal fringes disappear, the occurrence of the rays that arrive from both sides is therefore necessary to produce them (Fresnel 1815a: 16 -17. Quoting from Buchwald 1989: 119).

It is still not the principle of interference; but Fresnel's method of delicate abstraction from observations appears promising. Fresnel continued his studies; till 1819 he obtained enough material to comprise a paper on diffraction, which covers all his studies on the subject performed so far. The year of 1819 appeared to be very important for Fresnel and for the history of the wave theory. In 1819, Parisian Academy announces a prize on diffraction, requesting for a theory well-supported by experimental results. Fresnel submitted his "Memoir on the Diffraction of Light" to the Academy. After the Committee examined Fresnel's paper in detail, they had no other choice but to give the prize to its author. As will be illustrated below, Fresnel succeeded to unify the theoretical background of the wave theory, namely the law of interference and Huygens's principle and support it by precise experimental results. Fresnel's Memoir on Diffraction obtained the Prize, and wave theory had to be accepted as the viable hypothesis explaining the nature of light. Thus, it would be crucial to describe Fresnel's Celebrated paper in detail.

As has been already stated, in his Memoir on Diffraction Fresnel examines the phenomenon of diffraction and postulates his hypothesis explaining it. His work may be divided into two parts. Firstly, Fresnel tries to demonstrate how corpuscular theory and the wave hypothesis developed by Young are inconsistent to provide satisfactory explanations to the phenomenon of diffraction. Afterwards, he states his own definitions of the principle of interference and that of Huygens, to combine them, and introduce his own hypothesis covering the phenomenon of diffraction. The details of Fresnel's work are provided as follows.

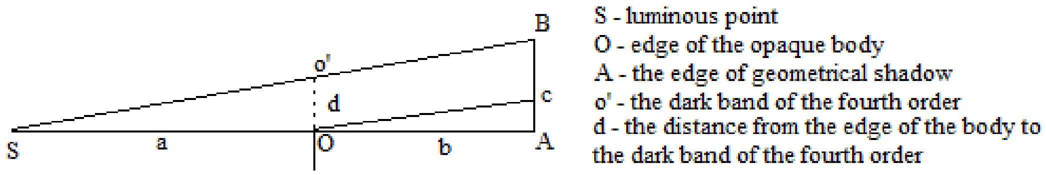
Fresnel starts his investigation with the examination of the fringes, produced by an obstacle. Figure 3.1. might again be useful to understand that observation. Following the particle theory of light, the formation of exterior fringes should be explained by "forces" (sometimes also called as "repulsive forces"), emanating from the edges of an obstacle and affecting particles of light in such a way for them to bend the edges of the opaque object within the geometrical shadow. Fresnel would attack this explanation and show that it is inconsistent with observational results he would obtain. He provides the details of his observations. Fresnel uses a lens with 2 mm focus placed between the eye and opaque body (in order to observe fringes with greater accuracy as he states). He then measures the distance between the edge of the shadow and the first fringe with the micrometer. That distance occurs to be less than 0,015 mm, which allows him to count the first fringe to be at the very edge of the shadow. Fresnel claims that if one moves the obstacle towards the source of light, the fringes observed with the lens become larger in size. Following the way of thought of corpuscular theory, it should be proposed that such increase in size occurs as the forces emanating from the edges of the obstacle become more intense. But

> This is impossible, for the intensity of this force can evidently depend only upon the distance at which the light corpuscle passes the opaque body, upon the size and form of the surface of this body, upon its density, mass, or nature; and [by hypothesis] these all remain constant (Fresnel, 1819. Quoting from Crew, 1900: 83).

Fresnel uses the interesting technique to show the inconsistency of the particle theory with experimental results – he assumes that he works within the framework of corpuscularists and deduces a formula to calculate the distance from the edge of a shadow to the fourth dark fringe. Further, he tests the formula, and comprises the table to compare theoretical and observational results. The formula is obtained in accordance with the figure 3.2., and takes the following form:

$$Ao' = br + \frac{d(a+b)}{a} \ ,$$

where Ao' is the distance between the edge of the geometrical shadow and the dark band of the fourth order, and br=AC (Fresnel, 1819. Quoting from Crew, 1900: 84).



S - luminous point
O - edge of the opaque body
A - the edge of geometrical shadow
o' - the dark band of the fourth order
d - the distance from the edge of the body to the dark band of the fourth order

Figure 3.2. Observation of the dark fringe of the fourth order. Crew, 1900.

The table 3.3., comparing theoretical and experimental results, is provided accordingly. It shows that in some observations the difference between theory and experiment occurs to be striking.

Table 3.1. Comparing theory and experiment, the fourth dark band. Crew, 1900.

No. of Observation	Distance of luminous point from opaque body	Distance of opaque body from micrometer	Distance between the edge of geometrical shadow and darkest point of fourth band		Difference
			Observed	Computed from formula $br + \frac{d(a+b)}{a}$	
	m.	m.	m.	mm.	mm.
1	0.1000	0.7985	5.96	-----	-----
2	0.510	1.005	3.84	3.32	-0.52
3	1.011	0.996	3.12	2.81	-0.31
4	2.008	0.999	2.71	2.57	-0.14
5	3.018	1.003	2.56	2.49	-0.07
6	4.507	1.018	2.49	2.46	-0.03
7	6.007	0.999	2.40	-----	-----

Fresnel would continue his attack on the corpuscular theory. He would claim that if the spreading of light from a small aperture is occurring due to the action of forces coming from the edges of that aperture, then mass, density, surface, etc. of the edges should have its effect on the light passing through. Fresnel performs two experiments to demonstrate that this is not the case.

In the first experiment, a beam of light passes through two steel plates brought very close together. A part of the edge of each plate is rounded, whereas the other one is sharpened instead; the edges were arranged in such a way that the sharp edge of one plate corresponds to the round one of another plate (Fresnel, 1819. Quoting from Crew, 1900: 97). Arrangement of this sort is made to trace the difference in fringe pattern depending on the shape of the edge. But, as Fresnel had expected, there was no difference detected. Even when the material of the plate has been changed, there was no variation of fringes.

In the second experiment, Fresnel took an unsilvered mirror, covered with a sheet of paper painted with India ink, and made a slit with the width of 1.17 mm. He further took two massive copper cylinders of the same size and placed them nearby, leaving the space of 1.17 mm between them. Both mirror and cylinders were placed in front of the source of light at a distance of 4.015 m; the fringes were observed accordingly. Fresnel provided these numbers while describing the experiment (Fresnel, 1819. Reference from Crew, 1900: 98); it might be argued that Fresnel kept the width of the slit and the distance between cylinders equal and put them at the same distance to the light source to eliminate possible deviation of the results which might occur due to these parameters. The distance between the first two dark bands appeared to be the same in both cases; the distance between two fringes of the second order occurred to be the same in both cases. The apparatus of the experiment can be seen in the figure 3.6.

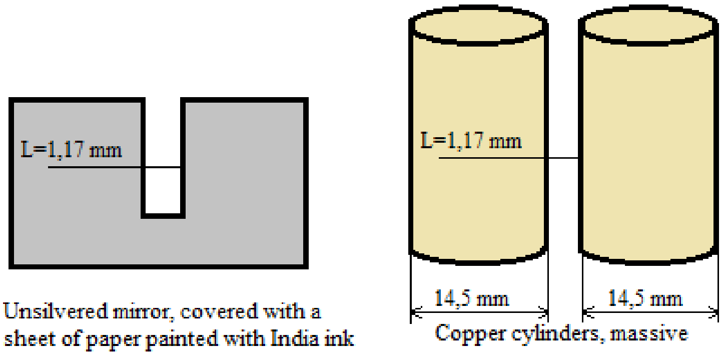


Figure 3.3. Fresnel's experiment with mirror and copper cylinders.

By these experiments Fresnel demonstrates that the difference in material, mass, density, shape etc of an object, which edges form the slit, does not produce any effect in the formation of fringes. It can be seen that the experiments provided are enough accurate to exclude a chance for mistakes in observations. Fresnel concludes that emission theory is not much successful in explaining phenomena of diffraction:

> It is therefore certain that the phenomena of diffraction do not at all depend upon the nature, the mass, or the shape of the body which intercepts the light, but only upon the size of the intercepting body or upon the size of the aperture through which it passes. We must, therefore, reject any hypothesis which assigns these phenomena to attractive and repulsive forces whose action extends to a distance from the body as great as that at which rays are inflected (Fresnel, 1819. Quoting from Crew, 1900: 98 - 99).

Whereas the corpuscular theory of light faces the problem with explaining the phenomenon of diffraction, the wave theory, according to Fresnel, can provide a solution. He proposes that introducing the law of interference would help to remove the inconsistency between theory and experiment:

> Introducing the principle of interference, however, we are able to predict not only the variation in size of the exterior fringes when the screen is made to approach or recede from the luminous point, but also the curved path of the bright and dark bands. The law of interference, or the mutual

> influence of rays of light, is an immediate consequence of the wave-theory; not only so, but it is proved or confirmed by so many different experiments that it is really one of the best-established principles in optics (Fresnel, 1819. Quoting from Crew, 1900:87).

Fresnel mentions Grimaldi as the first scientist who observed "the effect which rays of light produce upon one another" (Fresnel, 1819. Quoting from Crew, 1900: 87). Fresnel describes two experiments; one is organized to observe fringes when light passes through two small slits (also called as the "two-slit experiment") and the other one resembling the first, but performed with two mirrors. Here is how Fresnel describes the first one:

> Brighter and sharper fringes may be produced by cutting two parallel slits close together in a piece of cardboard or a sheet of metal, and placing the screen thus prepared in front of the luminous point. We may then observe, by use of a magnifying-glass between the opaque body and the eye, that the shadow is filled with a large number of very sharp-colored fringes so long as the light shines through both openings at the same time, but these disappear whenever the light is cut off from one of the slits (Fresnel, 1819. Quoting from Crew, 1900: 88).

In the second experiment, two beams of light, reflecting from two mirrors placed at a very acute angle to each other, meet and produce fringes of the same kind as those from the two-slit experiment, but even brighter and sharper. Fresnel remarks that only the principle of interference is able to explain such experiment, since the usage of reflected light from the mirrors excludes any action of "repulsive forces" coming from edges of any object, and the delicacy in its performance would not leave a place for accidental data. He also states that if one would take one mirror away the fringes would disappear, which proves once again that there is no effect produced by forces emanating from the edges of the mirrors, but only the correlation of beams of light (Fresnel, 1819. Quoting from Crew: 89).

Fresnel proposes that the same explanation should be deduced for diffraction of light from obstacle/slit. He confesses that initially he deduced a different hypothesis explaining the phenomenon; he originally thought that the fringes produced by diffraction of light from an obstacle/slit were formed due to the two rays – the one coming directly from the source and the other coming from the edges of an obstacle or slit. The Figure 3.4. demonstrates that way of thought.

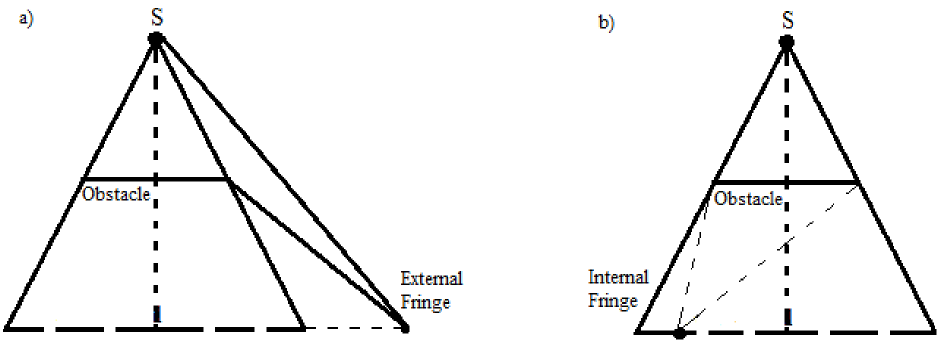


Figure 3.4. Fresnel's initial explanation of diffraction. a) The formation of external fringe is determined by two rays: one coming from the source and the other – from the edge of an obstacle; b) The formation of internal fringe is determined by two rays coming from the edges of an obstacle.

Fresnel claims that such explanation of fringes formation was also advocated by Young (Fresnel, 1819. Quoting from Crew, 1900: 89). Unfortunately, a closer examination of this hypothesis demonstrated its invalidity in respect to experimental results. Fresnel would like to examine this theory in order to track its inconsistencies:

> The first explanation which occurs to one is that these fringes are produced by the interference of direct rays with those which are reflected at the edge of the opaque body, while the interior fringes result from the combined action of rays inflected into the shadow from the two sides of opaque body, these inflected rays having their origin either at the surface or at points indefinitely near it. This appears to be the opinion of Mr. Young, and it was at first my own opinion; but a closer examination of the phenomena convinced me of its falsity. Nevertheless, I propose to follow it to its logical conclusion and to state the formula which I have derived in order to facilitate comparison of this theory with that which I offer as a substitute (Fresnel, 1819. Quoting from Crew, 1900: 89).

The details of Fresnel's initial theory can be provided as follows. Fresnel uses the same technique of test as he did in the case of the corpuscular theory: he deduces the formula to calculate the position of the fringes, and then, by testing it, tries to reveal the inconsistency. The formula for this test would take the following form:

$$x = \sqrt{\frac{2n\lambda b(a+b)}{a}},$$

where x is the distance from fringe in question to the edge of geometrical shadow; a – the distance from the luminous point to the opaque body; b – the distance from the opaque body to its geometrical shadow; λ – the length of the wave, or "the distance between two points in the ether where vibrations of the same kind are occurring at the same time and the same sense" (Fresnel, 1819. Quoting from Crew, 1900: 90); and n is the variable permitting the calculation of different positions of the fringes (first dark one, second dark one, and so forth).

By organizing the experiment on the diffraction from an obstacle, Fresnel observes certain inconsistencies between the results calculated by the formula and those obtained by experiment. For instance, Fresnel perceives that the first dark fringe calculated by the formula deduced, occurs to be bright in experiment. In other words, where the dark fringe is predicted theoretically the observation of bright band is recorded:

> In general, the position of the dark and bright bands deduced from [this] formula is almost exactly the inverse of that determined by experiment" (Fresnel, 1819. Quoting from Crew, 1900: 91).

In the case with internal fringes, the calculation of the positions of the bands distant from shadow edges is quite verified by experiment, but in area very close to the edge of the shadow the difference between theoretical and observational results is detected:

> So long as the extreme fringes are sufficiently distant from the edges of the shadow, [this] formula agrees fairly well with experiment; but when they approach very near or pass beyond the edges, one detects a slight difference between their actual position and that deduced from the

> formula. In general, the calculated values are always a little larger than the observed (Fresnel, 1819. Quoting from Crew, 1900: 92).

Fresnel still keeps the possibility that such disagreement between theory and observation may occur due to the defects in experimental tools, or inaccuracy of measurements. Thus he designs and performs another experiment to demonstrate the inadequacy of this hypothesis.

Fresnel took a copper plate with the shape shown in the Figure 3.5., and placed it in the dark room about four meters from the luminous point. Then he examined the fringes formed by the plate with magnifying glass. Fresnel observed that the fringes produced by the lower part CDFE are much brighter and purer than those produced by the higher part, ABDC. He then concludes:

> If now, the only inflected light were that which grazed the edges of the opaque bodies, the fringes of the upper part ought to be sharper and ought to show purer colors than those of the lower part; for the first are produced by the meeting of the two systems of waves which have their centres upon the edges AC and BD, while the others are formed by the meeting of four systems of waves having their origin at the edges C'E', CE, DF, DF'; and this would necessarily diminish the difference of intensity between the dark and bright bands, in the case of homogeneous light, or the purity of colors, in the case of white light, because the fringes produced by the rays reflected and inflected at C'E' and DF would not exactly coincide with those produced by the meeting of rays coming from CE and D'F'. Now experiment shows, as I have just said, that exactly the reverse of this is true (Fresnel, 1819. Quoting from Crew, 1900: 95).

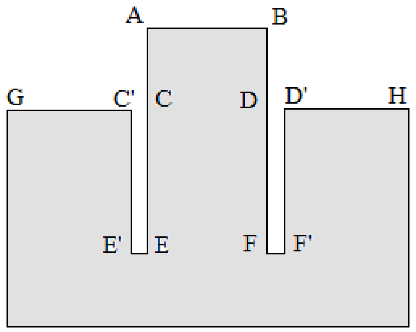


Figure 3.5. Copper plate used in Fresnel's experiment. Crew, 1900.

Having demonstrated that neither his original hypothesis, explaining the appearance of diffraction fringes as due to the light coming from the edges of an obstacle/slit, nor corpuscular theory can provide satisfactory explanation to the phenomena of diffraction, Fresnel passes to his advanced theory.

Fresnel claims that grasping and expressing the superposition of waves (and the process of how a wavefront behaves when it meets an obstacle) in terms of pure analytical mechanics would be a quite complicated matter. What Fresnel offers is rather

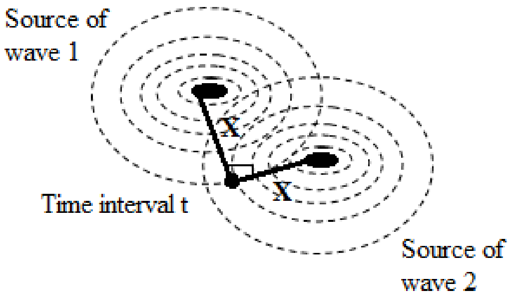
> … To compute the relative intensities at different points of the wave-front only after it has gone a large number of wave-lengths beyond the screen. Thus the positions at which we study the waves are always to be regarded as separated from the screen by a distance which is very considerable compared with the length of a light-wave (Fresnel, 1819. Quoting from Crew, 1900: 100).

In other words, the effect of the waves produce on each other when they correlate would be evaluated by calculating their intensities at the points of investigation. Fresnel makes further assumption – the disturbances of light waves should be treated as acting in a sequence. There is no reason for trying to understand the behavior of a single light wave in isolation, if one tries to explain the correlation of waves and their mutual effect on each other.

In the light of such assumptions, Fresnel defines the principle of interference:

> *Given the intensities and relative positions of any number of trains of light-waves of the same length and travelling in the same direction, to determine the intensity of the vibrations produced by the meeting of these different trains of waves, that is, the oscillatory velocity of the ether particles* (Fresnel, 1819. Quoting from Crew, 1900: 101).

It can be said that, according to Fresnel, one should grasp the process interference of the waves through their correlating intensities (by calculating resultant intensity or velocity). Thus, Fresnel would introduce the formula for calculating the resultant velocity. Figure 3.7. would be helpful in grasping Fresnel's denotations.



x - the distance which series of waves travel to a particle in the ether from the source of the wave (starting point of motion).

t - time interval, needed for a series of waves to reach the point in question.

u, u' - velocities of the waves.

Sources are placed by a quarter of a wavelength to each other (i.e. differing in phase by 90°).

Figure 3.6. The scheme of interference of two waves.

The formula which Fresnel introduces can be expanded as follows:

$$u = a\sin\left[2\pi\left(t - \frac{x}{\lambda}\right)\right]$$

$$u' = a'\sin\left[2\pi\left(\frac{t - x + \frac{\lambda}{4}}{\lambda}\right)\right] = -a\cos\left[2\pi\left(t - \frac{x}{\lambda}\right)\right]$$

$$a = A\cos i\,,$$

$$a' = A\sin i\,,$$

$$U = u + u'\,,$$

where a and a' are intensities of the waves coming from the sources, A – the resultant intensity, and U – the resultant velocity.

Substitution of these would lead to:

$$U = A\sin\left[2\pi\left(t - \frac{x}{\lambda}\right) - i\right],$$

where U is the resultative velocity of the wave resultant from the interference of two waves. As it can be seen from the figure 3.7., Fresnel took certain time interval and position of the waves to deduce his formula, but he claims that it can be successfully used for any position and can be called as general:

> The solution of this particular case for waves differing by a quarter of a wave-length suffices to solve all other cases. In fact, whatever the number of the trains of waves, and whatever be the intervals which separate them, we can always substitute for each of them its components referred to two reference points which are common to each train of waves and which are distant from each other by a quarter of a wave-length; then adding or subtracting, according to sign, the intensities of the components referred to the same point, we may reduce the whole motion to that of two trains of waves separated by the distance of a quarter of a wave-length… (Fresnel, 1819. Quoting from Crew, 1900: 105)

After reaching the general formula for calculating resultant velocity, Fresnel introduces his definition of Huygens's principle in order to describe how all these theoretical considerations are to be applied to the phenomena of diffraction. He also calls this principle as "the principle of superposition of small portions", and formulates it as follows:

> *The vibrations at each point in the wave-front may be considered as the sum of the elementary motions which at any one instant are sent to that point from all parts of this same wave in any one of its previous positions, each of these parts acting independently the one of the other* (Fresnel, 1819. Quoting from Crew, 1900: 108).

Fresnel talks about "elementary motions" or "disturbances", which constitute for light vibrations. The principle can be understood in the following way: any point at the wavefront should be treated as the sum of "the motions" of light particles in the ether occurring simultaneously and reaching this point.

At this stage Fresnel makes a couple of theoretical assumptions to clarify his "principle of superposition":

a) All vibrations in the ether constituting a light wave have the same characteristics (intensity, velocity, etc);
b) These disturbances are infinite in number;
c) Disturbances occur simultaneously;
d) These "elementary motions" are continuous and take place in the single spherical plane;
e) The last assumption goes as follows:

> The velocities impressed upon the particles are all directed in the same sense, perpendicular to the surface of the sphere, and, besides, that they are proportional to the compression, and in such a way that the particles have no retrograde motion. I have thus reconstructed a primary wave out of partial [*secondary*] disturbance. We may, therefore, say that the vibrations at each point in the wave-front can be looked upon as the resultant of all secondary displacements which reach it at the same instant from all parts of this same wave in some previous position, each of these parts acting independently one of the other (Fresnel, 1819. Quoting from Crew, 1900: 109).

According to Fresnel, every point (or "vibration") on the wave-front includes all "elementary motions" of ether particles coming to the direction of that point from its previous positions. These "disturbances" or elementary motions of the particles of the ether constitute a light wave; they all occur in the same plane, have the same physical characteristics, and are infinite in number. In other words, having such characteristics constitute this motion to a uniform wave of light. In fact, if one would try to imagine the process Fresnel describes as the sum of infinite number of vibrations occurring at the same moment of propagation of the wave-front, it would be seen that Huygens and Fresnel talk about the same process, but by applying different terminology.

When a wave spreads from the source, one can speak of its uniformity; but when the wavefront meets an obstacle or passes through an aperture, the intensity changes in accordance with its obliteration from the edges of obstacle or aperture.

Fresnel claims that the incorporation of the principle of interference together with his understanding of Huygens' principle would remove the inconsistency in his original hypothesis, namely the problem of disagreement between theoretical and observational results (see p. 23 for details). The Figure 3.8. demonstrates how Huygens' principle would modify the theoretical explanation of diffraction - there are more than two rays (CP and DP in the Figure 3.8.) which take the role on the formation of the fringe at the point P.

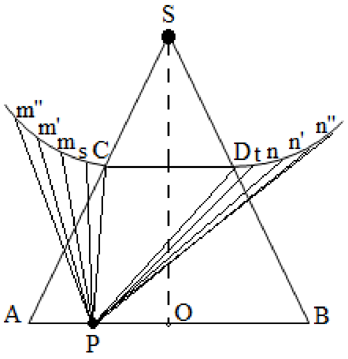
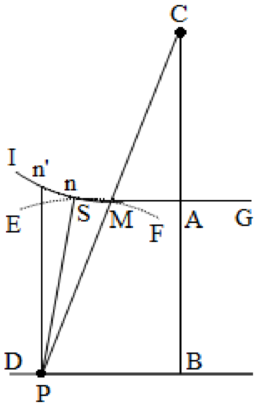


Figure 3.7. Incorporation of the Huygens's principle to the phenomenon of diffraction of light. Crew, 1900. CD stands for obstacle, P – for the fringe in question.

Now to return to Fresnel's development of his advanced hypothesis. Trying to unite the law of interference with the Huygens's principle, he deduces the formulas for calculating the sizes of the fringes formed by the wave of light passing through obstacle or aperture. In order to perform all that,

> we propose to apply to the principle of Huygens the method which we have already explained for computing the resultant of any number of trains of waves when their intensities and relative positions [*phases*] are given (Fresnel, 1819. Quoting from Crew, 1900: 118).

As has been aimed in his definition of the principle of interference (see p. 30 for details), Fresnel starts with the deduction of the formula for calculating the resultative intensity. The Figure 3.9. would be useful in understanding the development of the formula.



The obstacle AG is large enough, so no light falls from the edge G.

Aim: to find resultant intensity at P.

Figure 3.8. Observation of diffraction from an obstacle. Crew, 1900.

From the center C (source of light) Fresnel draws the circle AMI, which would be partly crossed by the obstacle AG. From this position of the wave Fresnel would calculate the resultative intensity. He would state that from this position of the wave the calculation is the simplest, since all parts of the wave have the same intensity. Moreover, none of the secondary waves are affected by the obstacle; they pass freely in all directions (Fresnel, 1819. Reference from Crew, 1900: 119).

In order to find intensity at P, Fresnel takes one special segment on the circle AMI, claiming it to be enough for calculation (as far as it can be deduced from the paper it is the arc n’nM). Here is how the formula being developed:

If nn'=dz – element of the primary wave; z – distance from nn' to M; and nS – distance between circles AMI and EMF (a circle described around P); then

$nS = \frac{1}{2}\frac{z^2(a+b)}{ab}$, where a=CA, b=AB.

Fresnel introduces the formula:

$dz\cos\left(\pi\frac{z^2(a+b)}{ab\lambda}\right)$, for the wave leaving M;

$dz\sin\left(\pi\frac{z^2(a+b)}{ab\lambda}\right)$, for the wave displaced $\frac{1}{4}\lambda$ apart from M.

It is clear that the formula encloses two sections – the wave coming from M straightly on CP, and another wave displaced $\frac{1}{4}\lambda$ from the preceding wave.

Combining all the elements together would give:

Intensity of vibration= $\sqrt{\left[\int dz\cos\left(\pi\frac{z^2(a+b)}{ab\lambda}\right)\right]^2+\left[\int dz\sin\left(\pi\frac{z^2(a+b)}{ab\lambda}\right)\right]^2}$

By “intensity of vibration” Fresnel means the speed of particle in the ether during oscillation (Fresnel, 1819. Quoting from Crew, 1900: 120). He uses integrals (because he takes the total sum of all similar components from other elements, in accordance with Huygens’s Principle). Fresnel also deduces a formula for the “intensity of sensation”, or the intensity of light (Fresnel, 1819. Quoting from Crew, 1900: 120), which takes the following form:

Intensity of sensation= $\left[\int dz\cos\left(\pi\frac{z^2(a+b)}{ab\lambda}\right)\right]^2+\left[\int dz\sin\left(\pi\frac{z^2(a+b)}{ab\lambda}\right)\right]^2$

In such a way Fresnel provides the formula for calculating resultant intensity of light in diffracting condition. It can be seen that he uses integrals, for which he became famous for. Fresnel further comprises a table for the values of integrals in order to be able to calculate the intensity at P in every position. The table 3.10. represents some of the results provided by Fresnel. To obtain the results provided in the table, Fresnel substituted integrals of original formula by using the limits of partial integration, “which are taken so close together that we can neglect the square of half of the arc included between them” (Fresnel, 1819. Quoting from Crew, 1900: 121). Thus the formula reached a less complicated form; the substitution arrived at $\int dv\cos q\upsilon^2$ and $\int dv\sin q\upsilon^2$. Fresnel calculates the values of integrals from 0.10 to 5.50 limit, inclusively.

Table 3.2. Table of the numerical values of the integrals. Crew, 1900.

Limits of Integrals	$\int dv\cos q\upsilon^2$	$\int dv\sin q\upsilon^2$	Limits of Integrals	$\int dv\cos q\upsilon^2$	$\int dv\sin q\upsilon^2$
From $\upsilon = 0^q$			From $\upsilon = 0^q$		
to $\upsilon = 0^q.10$	0.0999	0.0006	4.60	0.5674	0.5158
to υ=0.20	0.1999	0.0042	4.70	0.4917	0.5668
0.30	0.2993	0.0140	4.80	0.4340	0.4965
0.40	0.3974	0.0332	4.90	0.5003	0.4347
0.50	0.4923	0.0644	5.00	0.5638	0.4987
0.60	0.5811	0.1101	5.10	0.5000	0.5620
0.70	0.6597	0.1716	5.20	0.4390	0.4966
0.80	0.7230	0.2487	5.30	0.5078	0.4401
0.90	0.7651	0.3391	5.40	0.5573	0.5136
1.00	0.7803	0.4376	5.50	0.4785	0.5533

The only matter left to do then in order to calculate the intensity at any position of P is "to take from the table the values of $\int dv\cos q\upsilon^2$ and $\int dv\sin q\upsilon^2$, using the value of υ as an argument, then add to each $\frac{1}{2}$, and finally take the sum of their squares" (Fresnel, 1819. Quoting from Crew, 1900: 124).

There are two sub-steps Fresnel mentions here to reach the final formula. The first sub-step is to calculate the values of υ "corresponding to maxima and minima, i.e., the brightest and darkest points in the respective bright and dark bands" (Fresnel, 1819. Quoting from Crew, 1900: 124). These values of υ are necessary for their later use in the calculations. In the second sub-step Fresnel substitutes the "simplified" integrals with the value of q equivalent to $\frac{\pi}{2}$.

As the result of the vast amount of effort, Fresnel reaches the formula of the form

$$x = n\sqrt{\frac{\frac{1}{2}(a+b)b\lambda}{a}},$$

where “x” denotes to the distance of the fringe under examination from the opaque screen, and “n” is “the value of υ which yields a maximum or minimum value for integrals” (Fresnel, 1819. Quoting from Crew, 1900: 126). That is where the results of the first sub-step to be applied. Fresnel finally obtains the theory which is ready to be tested by experiment.

Fresnel performs a vast amount of experiments to test the formula; he presents the results in the two tables, one of which identifies the positions of dark fringes of the red light, and the other determines the positions of “maxima and minima in the fringes produced by narrow aperture” (Fresnel, 1819. Quoting from Crew, 1900: 135) The tables 3.11. and 3.12. below represent the results accordingly.

Table 3.3. Comparing theoretical and experimental results on the exterior fringes of red-light of wave-length 0.000638 mm. Crew, 1900.

Number of Observation	Distance of luminous point from opaque screen, a	Distance of opaque body from micrometer, b	Order of dark band	Distance from darkest point in each band to edge of geometrical shadow		Difference
				Observed	Computed	
	m.	m.		mm.	mm.	
1	0.1000	0.7985	1	2.84	2.83	-1
			2	4.14	4.14	0
			3	5.14	5.13	-1
			4	5.96	5.96	0
			5	6.68	6.68	0
10	1.011	2.010	1	2.59	2.59	0
			2	3.79	3.79	0
			3	4.68	4.69	+1
			4	5.45	5.45	0
			5	6.10	6.11	+1
20	3.018	0.253	1	0.54	0.55	+1
			2	0.80	0.81	+1
			3	1.00	1.00	0
			4	1.16	1.16	0
			5	1.31	1.31	0

Table 3.4. Comparing theory with experiment regarding of positions of maxima and minima in the fringes produced by a narrow aperture. Crew, 1900.

Number of bright or dark bands counted from middle	Approximate value of υ counted from edge of aperture	Corresponding intensity	Value of υ corresponding to maxima or minima	Distance of maxima or minima from projection of centre of aperture		Difference
				Observed	Computed	
First Observation						
	m.	m.	mm.			
	a=2.010;	b=0.617;	c=0.50;	tabulatory value of c=1.288		
				mm.	mm.	mm.
1. Minimum	+0.812 +0.912 +1.012	0.03495 0.01645 0.03406	+0.913	0.79	0.77	+0.02
2. Minimum	+2.412 +2.512 +2.612	0.00238 0.00235 0.00541	+2.463	1.58	1.58	0.00
Fifth Observation						
	m.	m.	mm.			
	a=2.010;	b=0.492;	c=1.50;	tabulatory value of c=4.224		
				mm.	mm.	mm.
1. Maximum	-1.300 -1.200 -1.100	2.7289 3.0466 2.9780	-1.168	0.42	0.43	-0.01

The results are striking; no result gives a difference between theory and experiment of more than 0,02 mm. It can be stated that integrals, constructed on the correlation of Huygens principle with the law of interference, have the ability to give satisfactory results for every special case. Fresnel claims:

> Our theory rests upon a hypothesis which is at once so simple and so inherently probable, and which besides has been so strikingly verified by many varied experiments, that one can scarcely doubt the truth of the fundamental principle (Fresnel, 1819. Quoting from Crew, 1900: 136).

He also adds:

> I have now applied the principle of Huygens to the three general classes of phenomena in which diffraction occurs […] Comparing observations with the predictions of the theory, I have shown that it suffices to explain the most diverse phenomena, and that the general expression for the intensity of light derived from it gives us a faithful picture of the phenomena… (Fresnel, 1819. Quoting from Crew, 1900: 138)

It should be stated that Fresnel performed a huge amount of work in his memoir of diffraction. His method of deduction, testing and demonstrating a certain theoretical claim deserves applause. Examination of Fresnel's paper give certain grounds to analyze it and the works of Young; the silhouette of evaluation can be sketched from now on.

CHAPTER V

WHY METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES?

Imre Lakatos (1922 – 1974) is one of most well-known philosophers of science of the twentieth century. Throughout the course of his work at the London School of Economics, he contributed a lot to the philosophy and methodology of science. One of the most remarkable manuscripts Lakatos produced is, undeniably, the Methodology of Scientific Research Programmes (or MSRP as it is often abbreviated), published in 1978 after his sudden death. Instead of the dogmatic set of methodological "laws", this work offers a new, dynamic standpoint to the way a philosopher of science can analyze and trace the processes occurring within the process of scientific development. In this Chapter, which is fully devoted to MSRP, the methodology and the reasons for choosing it as the tool of analysis will be discussed.

Considering generally, Lakatos advocates for the objective, rational view of the scientific growth. According to Lakatos, there is a continuity of the scientific progress; such continuity is provided by research programmes – series of hypotheses that are changing in the process of scientific development (Lakatos, 1978: 47). Every research programme has a certain hard core – set of bold basic statements, accepted to be true by convention. For example, the hard core of the wave theory would be a statement like "Light is wave, propagating in all directions in the elastic medium – aether". All explanations and predictions of a theory should be coming out of (and support) the hard core. Hard core is "surrounded" by the "protective belt" of auxiliary hypotheses – additional assumptions and theories which help to form a complete research programme (Lakatos, 1978: 48). In the case of the wave theory, the protective belt might be constructed from Huygens's principle and the principle of interference.

Notwithstanding the fact that the concept of the hard core, surrounded by the protective belt of auxiliary hypotheses takes the central part of Lakatos's methodology, the heuristic power of the research programmes can be traced by the notions of positive and negative heuristic. It can be accomplished that, when evaluating two research programmes in the process of theory-change, negative and positive heuristic should be applied as the main tools of such evaluation. Application of these methodological principles would allow one to see whether a theory in question constitutes for progressive or degenerating problem-shift, which in turn would point to the reasons of certain theory-choice.

Negative heuristic is a principle which makes the hard core "irrefutable" by possible falsifiers. By the help of the negative heuristic scientists deal with anomalies, try to solve and corroborate them in the framework of the hard core (Lakatos 1978: 48). By application of this heuristic, scientists purpose to increase predictive power of a programme, trying to solve the problems and corroborate anomalies step by step. Lakatos makes an important deduction from the process of applying the negative heuristic – a research programme should bring about theoretical progress, and only as a result it possesses the empirical progress. Each step, each anomaly's solution may not immediately increase the empirical content of a programme; but it should still be a step to modify theoretical basis in such a way to achieve higher empirical success. As Lakatos points out, empirical progress is intermittent. (Lakatos 1978: 49).

Whereas negative heuristic helps to strengthen the hard core of a programme, positive heuristic works around the protective belt – it shows certain tips on how to organize and modify the belt in such a way to put all the auxiliary hypotheses in the progressive order. It is called as "positive" since it advises in which way a scientist can change, modify or reconstruct the set of auxiliary assumptions without looking at anomalies and problems a programme is faced with. (Lakatos 1978: 50). One can also define positive heuristic as an order of internal theoretical work within the certain research programme – a scientist does not get lost among anomalies and instead tries to internally develop its theoretical basis. By what tools exactly would a scientist succeed to make such an order? Lakatos would claim that this process is possible through developing models – set of initial conditions by sophistication and change of which one would be able to "play" with the protective belt until s/he reaches well-corroborated, progressive content:

> The positive heuristic sets out a programme which lists a chain of ever more complicated models simulating reality: the scientist's attention is riveted on building his models following instructions which are laid down in the positive part of his programme (Lakatos 1978: 50).

Important aspect to be taken from the positive heuristic is that, according to Lakatos, it constitutes for the "relative autonomy of theoretical science": by using the positive heuristic, scientists, not counter-evidence attacking from the outside, determinate in which way they should proceed and develop a programme. (Lakatos, 1978: 52). It is purely internal and theoretical process; that is why it is called as autonomy. Moreover, such internal process allows one to rationalize the process of scientific discovery to certain extent. (Lakatos 1978: 52).

It should be noted that the term "corroboration", preserved from Popper's terminology, also plays an important part in Lakatos's position. It is the main guide when one tries to reduce the degree of conventionalism in scientific theories – since there is never a theory standing alone against a severe test, but a series of theories, together with all helpful auxiliary hypotheses, one should explain novel predictions, understand and try to entail anomalies; if there is no possibility of such entailment, one should find and modify the set according to the degree of corroborative content. Faced with a certain task or a problem, one should test and examine each part in a theory set and see which members increase overall positive value of it, and which do not. If necessary, each constituent can be tested in such a way. Well-corroborated set of scientific propositions should be chosen to develop the scientific discipline.

Why then can MSRP be employed as a viable tool of methodological analysis? There are plenty of options MSRP offers in order to construct a comprehensive investigation in the philosophy of science.

The first feature is the way MSRP incorporates the continuity of scientific development. There is never a theory in isolation, standing as bearing of knowledge or being preferred to some other theory. There are always series of theories, comprising continuity in the course of scientific development. Science grows from one series of hypotheses changing to another series; thus it is impossible to reject one scientific set of theories by simple falsifying evidence. Moreover, it seems to be offering a good starting point to resolve Duhem's problem – rejection of a theory by single conflicting evidence is a viable position, since no theory stands alone for explanation of certain phenomena. It is much more preferable, and actually that is how scientists act in their practice, to find a puzzling part inconsistent with the whole picture and modify it to achieve "more corroborated" content (if that is possible, of course).

There is another fruitful fact "hidden" in the concept of continuity. Understanding the development in science as the process of dynamical "flow" from one set of beliefs to another allows to detect most of the parts in the "tiny network" of scientific activity. When the process of theory-change is in question, having such an opportunity in hand becomes crucial. One can see how the research programme starts its construction, how it turns to the mature stage, and how, by what reasons it loses a battle to a new-coming candidate. This way of monitoring is possible because MSRP allows to perform a retrospective analysis of the process of theory-change in particular and scientific growth in general. It is the application of retrospective analysis, which enables to investigate and understand development in science till its current state. For instance, without the option of retrospective analysis it would be hard to give grounds and rationally track why there was a delay in acceptance of the wave theory at the beginning of the nineteenth century.

The second beneficial feature of MSRP is that it correctly explains that scientists do not merely work on anomaly solution; otherwise, instead of scientific activity it would turn to nothing else but to trial-and-error procedure. As a result, such activity will lose its objectivity content, i.e. there would be no possibility to evaluate it as scientific/unscientific knowledge. There is an internal process of theory production, and that points out to the importance of positive heuristic (which, as Lakatos specifies, shows the way of scientist's theoretical work independent of external factors). Detailed examination of the works of Young and Fresnel demonstrates the "internal autonomy of theoretical science" at work. It might not be so clearly traced from Young's papers; but Fresnel's manuscript uncovers how a scientist can develop his set of hypotheses from a small set of theoretical assumptions to the powerful theory by gradually solving the puzzles in front of him.

The power of the positive heuristic would then constitute an important part of "mature science":

> Mature science consists of research programmes in which not only novel facts but, in an important sense, also novel auxiliary theories, are anticipated; mature science – unlike pedestrian trial-and-error – has "heuristic power". Let us remember that in the positive heuristic of a powerful programme there is, right at the start, a general outline of how to build the protective belts: this heuristic power generates the autonomy of theoretical science (Lakatos, 1978: 88).

MSRP bears still another advantage – it enables to trace the level of innovation in the process of theory-change. By granting the process of internal analysis of a certain scientific activity, MSRP helps to trace the light on the degree of heuristic power of that process. That in turn gives the possibility of tracing what part of a theory, with its theoretical grounds and their experimental support, was successful to contribute to its scientific value, and which one, one the contrary, was the weak link in the chain. In other words, it enables to understand how theoretical and empirical power of a theory brought it to a certain success. For instance, Young succeeded to envelope his deductions into the concrete set of theoretical assumptions; but the lack of empirical success in the support of these assumptions may stand as a possible reason for the neglect of his wave theory of light.

With all its plausible features, the methodology of scientific research programmes stand as a viable tool of analysis. The applicability of this method to the Young-

Fresnel case is to be discussed in the last Chapter of the thesis; it would reveal whether this set of evaluative criteria is helpful in understanding the reasons of the delay in the acceptance of the wave theory of light at the beginning of the nineteenth century. The case-study might point to the pitfalls of the methodology, if any.

CHAPTER VI

FURTHER EVALUATIONS ON THE YOUNG-FRESNEL CASE

Although the issue of delay in the acceptance of the wave theory in the beginning of nineteenth century is not much popular among commentators in the history of science, there is a number of valuable papers which evaluate that period. John Worrall paid a great amount of attention to that topic; this Chapter would begin with the representation of his papers.

In his 1976, John Worrall investigates Young's developments in the nineteenth century optics. Young's definition of interference, obtained as a result of his "famous" experiments, should have acted like "crucial experiment" in the favour of the wave theory. But, the theory-change in optics from emission theory to its wave alternative occurred only in 1820 – 30s. If all Young's "discoveries" in optics were documented before 1807, why such a delay in the process of scientific "revolution"? Worrall, by providing an analysis of this historical case from the standpoint of MSRP, tries to demonstrate that by the utilization of Lakatos' methodology one can provide a purely "internal" picture of the story and explicitly point to the factors that produced an obstacle for Young's developments to overthrow the corpuscular research programme.

William Whewell, a historical commentator who described Young's accomplishments in optics, provided several reasons which can be counted as the causes of Young's failure. The causes, which will be dealt with below, are purely external in character ("external" meaning not depending on Young's theoretical work on formulating his hypothesis); these should be mentioned briefly in order to understand Worrall's reason for later rejecting them. Firstly, Whewell mentions "Newton-worship" factor – it is claimed that the power of Newtonian corpuscules was so strong that no scientist, at least in England, was eager to accept the "revolution" in optics (Worrall, 1976: 110). Secondly, Whewell calls Young's inability to present his ideas clearly and concretely as another possible reason for scientists to ignore his theory (Worrall, 1976: 110). And lastly, it is claimed that the reviews of Young's papers, made by Henry Brougham in Edinburgh Review after their publication, declined to certain extent Young's victory – "…we can only hardly doubt that these Edinburgh reviews had their effect in confirming the general disposition to reject the undulatory theory…" (Quoting Whewell, Worrall, 1976: 111).

Worrall would of course reject these explanations for being purely external. To the first he would claim that there was a lot of work done after Newton published his Opticks in 1707, and none of the scientists, pointing out to the inconsistencies of corpuscular theory, was neglected because of Newton's "worship" if their objections were illustratively reasonable. (Worrall, 1976: 114). Apart from this, Newton himself was never a committed corpuscularist (he would rather believe that the nature of light, consisting from particle emission, is the most reliable description). After all, if Newton's reputation was that unshakeable, why has the picture "suddenly" changed towards 1820?

Concerning the second explanation, Worrall responses that Young was actually quite clear in his claims:

> In fact, those aspects of his work which are generally alleged to constitute his primary achievement and to have established the wave theory's superiority – namely the qualitative aspects of his 'crucial' experimental results and his principle of interference – are on the whole presented with admirable clarity. And in fact Young's accounts of the qualitative aspects of his experimental results (namely the appearance of interference fringes in certain circumstances and their disappearance in others) seem to have been clearly understood by corpuscularists like Biot and Brougham, both of whom report them accurately (Worrall, 1976: 115).

To the third explanation Worrall would respond that under-representation of Young's arguments by Edinburgh Reviews also appears quite implausible. For, even if such reviews were popular and spread widely in England, why was

Young's theory neglected in France, where these reviews were not published? In any case, no matter how strong an objection formulated in these reviews may be, this factor cannot postpone the development of the theory in question for twenty years (Worrall, 1976: 116).

After rejecting these "traditional" explanations of why Young's studies were neglected among scientists, Worrall invites his reader to analyze Young's work in optics from the standpoint of MSRP. If such an analysis will enable to trace actual turning point of Young's hypothesis, and show its predictive power, it may give a clue for further investigation for historians of science. They would have to find another explanation of why such an important work was underestimated. But if the utilization of Lakatos' methodology would explicitly illustrate why Young's work was abandoned, there might be no further need for any kind of external/internal explanation.

The wave theory of light, or series of theories as a research programme, had its hardcore (light is the wave disturbance in the aether) and positive heuristic (reduce explanation of all phenomena to the ordinary mechanics of ether, "without invoking any force not already made available by theoretical mechanics" (Worrall, 1976: 136). Worrall notes that the wave programme existed before Young, even the phenomenon of interference he became famous for was previously described by Newton, with only difference that Newton explained the interference of water tides by gravity forces, whereas Young applied elasticity of aether to the waves of light (Worrall, 1976: 137). Thus, Young was not an inventor of the wave programme; he was operating within existing series of theories. The main point of examination then is to see whether Young turned this programme to be progressive or degenerated problem-shift. Starting from the first definition of interference, Worrall describes developments of Young's claims.

Young's first definition of interference states that (as has been stated before in Chapter I) "Whenever two undulations, from different origins, coincide either perfectly or very nearly in Direction, their joint effect is a Combination of the

Motions belonging to each" (Young, 1802a. Quoting from Worrall, 1976: 138). Worrall underlines that Young's restrictions (rays coming nearly "in one direction") act as the main restrictive factor in his discovery. It seems like Young was faced with certain difficulties in formulating the principle and started to incorporate restrictions to his theory in an ad-hoc way. In fact, as has been stated above (see p. 15), the interference of two waves of light should not necessarily depend the position of the sources of light, or on the direction of the rays. For instance, the phenomenon of diffraction from an obstacle/slit does not involve two separate beams of light coming "in one direction", but the fringes which are observed within and out of the geometrical shadow are appear due to the process of interference. Nevertheless, Young, by making modifications on his definition of interference, appeared to get lost in ad-hoc considerations. These modifications are provided by Worrall accordingly.

Young's second definition of interference takes the form of law and declares that

> …wherever two portions of the same light arrive at the eye by different routes, either exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any multiple of a certain length, and least intense in the intermediate state of the interfering portions; and this length is different for light of different colours (Young, 1802b. Quoting from Worrall, 1976: 139).

In his third definition Young claims that

> In order that the effects of the two portions of light may be thus combined [to produce 'the alternate union and extinction of colours'] it is necessary that they be derived from the same origin…" (Young, 1807: 464. Quoting from Worrall, 1976: 140).

The last definition of interference Young formulated is provided by Worrall as follows:

> …When two equal portions of light, in circumstances exactly similar, have been separated and coincide again, in nearly the same direction, they will either co-operate, or destroy each other, accordingly as the difference of the times, occupied in their separate paths, is an even or an odd multiple of

> a certain half interval… (Young, 1817: 287. Quoting from Worrall, 1976: 142).

Worrall then concludes:

> Thus at each stage Young modified his claims precisely so as to reconcile his principle of interference with known refutations, without providing the principle with extra content which would be exposed to possible refutation. Some modifications were actual logical weakenings; others consisted essentially of weakening the previous version so that it no longer said anything about those cases in which it had been refuted and then adding to it correct descriptions of the previously refuting instances. The various versions of Young's principle do then form a classic case of a 'degenerating problemshift' (Worrall, 1976: 142).

There is another ad-hoc way of thought Young followed in his wave hypothesis, as it was traced by Worrall. According to Worrall's investigation of Young's work on interference, it might be supposed that Young's theoretical considerations were extracted from experiments in an ad-hoc manner – his experiments actually did not come "out of the theory" and his theoretical claims were made "to fit" certain experimental result:

> It cannot then be frequently cited qualitative features of either of Young's two famous experimental results which provide the 'novel facts' which the methodology of research programmes requires Young's theory to predict if it is to constitute progress. For not only were these features already known, they were not predicted by Young's original theory and subsequent versions only dealt with them in an ad hoc way (Worrall, 1976: 147).

Worrall provides further details to support this point. He states that in order for a theory to make a certain prediction (that would be counted in the favor of that theory), it should come out of that theory. It means that, for example, a scientist should act like "Light is a wave spreading in the elastic aether; if I take two portions of light, point them at the screen with two slits, I would observe dark and light fringes as the waves coming from these slits interfere in accordance with my theoretical consideration, with the first dark fringe to be observed at this point on the screen, followed by light fringe at that point". Then the scientist organizes an experiment in order to test the precision of his theoretical prediction. But in the case of Young, Worrall mentions that

> The initial condition or auxiliary hypothesis concerned is arrived at, not on the basis of some independent experimental technique, but by first looking at the experimental result and then working backwards to find some assumption which when fed into the theory gives the already known result. This is a consistency proof rather than a prediction! (Worrall, 1976: 150)

Worrall insists that the case of "consistency proof" was the actual state of affairs with Young's diffraction experiments.

Regarding the two-slit experiment, or formation of the fringes from the light passing through two small slits, Worrall declares that there is no certainty on whether Young actually did perform the experiment. First of all, he never describes double-slit experiment in his papers, merely stating that it is "the simplest case of interference" (Worrall, 1976: 153). Moreover, Young speaks of the slits as of "small pinholes", and this fact, as Worrall notes, is quite confusing since it is very hard to observe fringes on the screen using round holes. Besides, "Young gives no numerical details on this experiment, whether about setting up the experiment or about its results" (Worrall, 1976: 153). If Young wished to demonstrate how exactly he performed the double-slit test, and how exactly can others repeat it, he would have paid more attention to details:

> …The dearth of details in Young's account [on double-slit experiment] make it seem unlikely that Young ever did successfully perform it, and certain that he did not give sufficient information about the conditions of the experiment to ensure its repeatability by others (Worrall, 1976: 155).

Worrall concludes that the internal analysis provided in terms of MSRP successfully demonstrates Young's failure to champion the wave theory:

> The analysis provided by the methodology of scientific research programmes thus yields, I claim, a completely 'internal' explanation of the reception of Young's work. […] Although there is no denying that Young had some intelligent and suggestive ideas, his work neither established the truth of the wave theory nor its superiority over its rival. Thus, there is, on this account, no need to invoke external factors like 'Newton worship' to explain Young's alleged neglect (Worrall, 1976: 161).

Tony Rothman, in his *Everything's Relative: and Other Fables from Science and Technology,* published in 2003, would also attack Young's deductions in the wave theory. He would claim that the concept of interference was not a new term in physics at that time; it was widely used in explanations of anomalous tides of water and in the phenomena of sound. Rothman stresses on the point that Young, taking the route of analogy between light and sound, failed to develop that assumption and employ it to the phenomena of light:

> At the dawn of the nineteenth century, interference in water and sound waves was accepted. Young, reasoning by analogy, intended to prove that interference of light explained everything Newton could not (Rothman, 2003: 16).

Moreover, Rothman is in agreement with Worrall about his doubts of Young performing the double-slit experiment:

> In the Bakerian Lecture [Young, 1801] one can appreciate Young's gracious writing, and you will find there a detailed explanation of Newton's rings in terms of interference… You will also find Young's precise values for the wavelengths of light inferred from Newton's own measurements: extreme red, "37640 undulations in an inch"; extreme violet, "59750 undulations in an inch". But you will not find the double-slit experiment. Neither will you find it in his lecture of 1802, where he enunciates very clearly the "law" of interference, nor in the Bakerian lecture of 1803, in which he recounts his repetition of Grimaldi's experiments and reports extremely precise results (Rothman, 2003: 17).

Rothman continues by stating that the observation of the interference of two waves Young became famous for was not actually denoting to the phenomenon of light. In his *A Course of Lectures on Natural Philosophy and the Mechanical Arts* (1807), Young describes the apparatus, by the means of which the behavior of waves can be observed. Here is how Rothman continues:

> Today this apparatus is termed a ripple tank, and in it students create water waves and observe their behavior. Young goes on to describe the interference pattern produced by two circular waves that have been created near each other, as you might do by dropping two stones into a pond. It is the diagram of *this* interference pattern that he publishes in the *Lectures*. In other words, Young's famous illustration of interference, reproduced in

> textbooks worldwide, is not of light at all but of water! (Rothman, 2003:18)

Generally, the main point of Rothman's argument is that Young, by drawing analogy between light and sound, did not succeed to get out of that way of thinking:

> Young begins his exposition with an analogy to sound and water, then passes to the realm of what happens to be a concrete experiment. It is difficult to pinpoint where the analogy is to break off – indeed, whether he intends it to (Rothman, 2003: 19).

Henry Crew also evaluates Young's studies. While editing *The Wave Theory of Light: Memoirs of Huygens, Young, and Fresnel* (1900), he states:

> After all, it must be confessed, even by his most ardent admirers, that Young's style is, in general, far from clear. Whether this is in any way connected with his lack of mathematical training, or whether it is due to the fact that his own clear intuitions bridged most of the gaps in his written work, it is difficult to say; but in any event many of his papers are obscure, and few of them are read (Crew, 1900: 78).

He also adds that Young's

> scientific style left much to be desired; his haphazard education and his lack of strenuous mathematical training led him in later life to make this remark, 'When I was a boy, I thought myself a man; now that I am a man, I find myself a boy' (Crew, 1930: 10).

Still, Crew thinks that Young's work is important and should not be underestimated. He was still the first scientist who introduced the principle of interference, and made other contributions to the wave theory:

> [To this evidence] Young added the fundamental and crucial experiment of shutting off the light which grazes either side of the wire and proving that under these circumstances the fringes behind the wire disappear. This simple but tremendously important fact that two rays of light incident upon a single point can be added together to produce darkness at that point is, as I see it, *the one outstanding optical discovery which the world owes to Thomas Young* (Crew, 1930: 7)

John Mollon, in his article “The Origins of the Concept of Interference”, thinks that it was Young who, thinking by analogy between light and water waves, successfully applied the principle of superposition of light waves:

> That leap was made by Thomas Young, and it was only in 1801 that the concept of interference emerged as an explanatory principle applicable equally to the interaction of tides, to the beats of sound of nearly the same frequency, and to the colours of thin films. This principle – he himself called it a general law (Young 1802c) – has proved to be the most powerful of Young’s several legacies to science and scholarship (Mollon, 2002: 808).

Mollon makes an interesting point – he states that it is important to differentiate between Young’s work on interference and his wave theory of light, and claims that Young himself sought for others to understand that distinction:

> It is important to distinguish between, on the one hand, the concept of interference, which follows analytically from the linear superposition of waves, and, on the other, Young’s particular version of the wave theory of light. He himself encouraged his audience to make this distinction (Mollon, 2002: 812).

Mollon agrees on the point that Young was not an accurate experimentalist. People that were close to Young admitted that it was Young’s “style of inquiry”:

> …He was afterwards accustomed to say, that at no period of his life was he particularly fond of repeating experiments, or even of very frequently attempting to originate new ones; considering that, however necessary to the advancement of science, they demanded a great sacrifice of time, and that when the fact was once established, that time was better employed in considering the purposes to which it might be applied, or the principles which might tend to elucidate (Gurney, 1831. Quoting from Mollon, 2002: 814).

Mollon believes in the fact that Young performed the double-slit experiment. Still, he notes:

> Frustratingly, he [Young] never published a systematic experimental paper using the two-slit arrangement, and he did not sharpen up the definition of what it meant to say, that the light must be derived ‘from the same origin’. Nevertheless, he clearly judged that his vibration theory of light was

> strengthened by the quantitative coincidence of values derived from Newton’s measurements of thin films and his own measurements of interference pattern (Mollon, 2002:815).

Mollon concludes that notwithstanding certain incompleteness of Young’s theory, it should not be ignored: “So Young’s wave theory was thus very much a transitional theory. It is his ‘general law of interference’ that has stood the test of time…” (Mollon, 2002: 816). Young should have his place in the history of optics as the moderator of the principle of interference.

Two more Worrall’s papers are to be described; they examine Fresnel’s study in optics.

In the article, called “Fresnel, Poisson and the White Spot: The Role of Successful Predictions in the Acceptance of Scientific Theories” (1989), Worrall would like to show that Fresnel’s wave theory of light, formulated in his Prize Memoir, was accepted and became successful among physicists not because of the “crucial experiment”, made by the suggestion of Poisson (member of the Committee), but rather because of its different method of empirical support:

> I shall argue in what follows that this story is historically incorrect. But my purpose is not simply to pour some factual cold water on an appealing story. The story has often been cited as an important illustration of a general methodological thesis: the thesis that favourable novel evidence – evidence first discovered only as a result of testing some already articulated theory – carries greater weight in support of that theory than does favourable, but already known, evidence. The main aim of the present paper is to show that the real history of the reception of Fresnel’s wave theory of diffraction supports, not this “novel facts count more” view, but a rather different account of empirical support (Worrall, 1989: 138).

In order to demonstrate his point, Worrall firstly tries to uncover several illusions about Fresnel winning the Academy Prize by examining direct historical sources of that period. The first “illusion” that Worrall discusses is the misinterpretation of the “competition” among scientists applying for the Prize. There were actually only two applicants, and since Fresnel’s “rival” had obvious mistakes in explanations of the optical phenomena (not only on diffraction, but on more simple terms like reflection and refraction), the Committee had "no other choice" than to give award to Fresnel:

> It is easy to form the impression from recent accounts that the prize was a highly competitive affair with a long list of entrants, that it was always unlikely that Fresnel would win and that he therefore needed something as dramatic as the unexpected success of the white spot prediction derived by Poisson. In fact there were just two competitors. There seems to be no record of who Fresnel's rival was – the candidates were officially anonymous, each memoir being identified by an epigram and being referred to by the commissioners as 'number one' and 'number two'. […] As Verdet remarked '[Number one's] work was not for an instant put in the balance with that of Fresnel'. The competition facing Fresnel could hardly have been less stiff (Worrall, 1989: 140).

The second illusion, which Worrall discusses, is misinterpretation of the fact that "the white spot", observed as a necessary consequence of Fresnel's theory, was accepted among scientists as the turning point from corpuscular theory to its wave alternative, due to the fact that this experiment showed the superiority of Fresnel's hypothesis. Even members of the Committee, judging manuscripts on diffraction (namely Biot, Poisson and Laplace), never got fully convinced in the wave theory, not even to talk about the scientific community of that time. Thus, one cannot claim for sure that white-spot experiment can be counted as crucial experiment:

> In fact, the report [about the Academy Prize] does its best to ignore basic questions concerning the nature of light: despite Fresnel's own strong emphasis on general theory, and his wholehearted commitment to the wave theory, the report manages to avoid any discussion of this and indeed the word 'wave' occurs nowhere in it. Fresnel's theory is given a rather severe positivistic reinterpretation… There was no instant conversion to Fresnel's wave theory even among prize commissioners, let alone of the scientific communities in Britain and France more generally (Worrall, 1989: 140).

Still, there should be something in Fresnel's thesis, which left members of the Committee no other choice than to award his work. If not to take the white-spot experiment on account, there should be other factors contributing to the judges' full convincement about the power of Fresnel's work. Worrall would claim that examining how his memoir affected commissioners would allow to see that such "crucial experiment" had no impact at all.

First of all, Worrall emphasizes the point that Fresnel's memoir on diffraction, as can be traced from the commissioners' report, occurred to be much impressive not because of its explanations on the grounds of the wave theory or some other reason, but because of remarkable experimental "devices" Fresnel used in the course of his work:

> Fresnel's memoir after all records his invention of a new method of observing and measuring diffraction fringes. Earlier investigators had observed the fringes indirectly, either by casting them on a white screen or by viewing them from behind a plate of unpolished glass. Fresnel discovered that this was unnecessary and that the fringes could be viewed directly, in mid-air so to speak, using a simple magnifying glass. This direct method led immediately to greatly enhanced visibility of the fringes and allowed them to be observed much closer to the diffracting object itself than had hitherto been possible. Moreover, using an instrument of his own construction, which allowed the position of the lens to be nicely adjusted via a micrometer gauge, Fresnel had measured the distances between fringes with greatly increased precision. Almost one half of the prize commission's report is taken up with Fresnel's new observational method and its advantages – before any mention at all is made of any account which might codify the observational results achieved via the method (Worrall, 1989: 142).

Worrall underlies that the Committee, being devoted corpuscularists, paid a few attention to the Fresnel's wave theory, talking more about his integrals and incorporation of Huygens' principle. The white-spot experiment, according to the report, did not have different destiny – only two sentences were written about it, describing that Poisson suggestion about white spot being observed in the center of the shadow of an opaque circular screen, was successfully demonstrated by experiment (Worrall, 1989: 144). They seem to be more impressed with Fresnel's explanation of the phenomena already known at that time than with certain novel predictions.

After examination of the report and the situation with the white-spot, Worrall passes to more general implications and conclusions. Worrall rightly states that utilization of the strict normative merits in the philosophy of science, namely as the "logic of empirical support" to guarantee rationality of scientific theories, is too bold to trace the actual scientific activity (in this case, Fresnel's hypothesis on diffraction). The issue is then to try to find such merits which would not only extract objectivity content, but also demonstrate that actual works of scientists are not "chaotic presuppositions" but are the components of real scientific network:

> A major problem for this 'logic of support' tradition is that of exactly how this normative enterprise is meant to mesh with the descriptive details of the history of science. Without going into details, it is clear that, just as a normative theory of goodness would be in bad trouble if a large number of generally recognized saints turned out to be evil according to its criterion, so a normative theory of science would be in bad trouble if such notables as Fresnel, Arago and Poisson turned out to be judging theories unscientifically. The aim then is to construct a 'logic of empirical support' which both seems a priori plausible and captures the judgments of most prestigious scientists. Or, if this logic fails to capture some particular judgment of that kind, it should provide a convincing and historically well-supported account of why the judgment went awry (Worrall, 1989: 147).

As a possible solution to that issue, Worrall proposes an account developed by him and Elie Zahar – "heuristic account" of empirical support. According to this thesis, it is actually not principal to uncover whether some fact, explanation or prediction was stated before or after theory formation, whether certain evidence was novel or was already standing as a part of a theory. It is not crucial since if a theory is formed in such a way that to entail certain evidence, then testing of that evidence will produce no harm for the theory under consideration. Examination of a hypothesis based on the strict formal rules, on the other hand, would show no other way than to take such hypothesis aside as it failed to "pass the severe test". This clearly shows that a good test should rather explore theory's construction than merely make use of purely logical norms.

Worrall, by the example of Fresnel's theory of diffraction, shows that it is really does not matter whether a certain prediction is already entailed in a theory or occurs as "novel" afterwards, at least it does not change its success.

The last paper to be presented here is Worrall's *"Heuristic Power" and the "Logic of Scientific Discovery": Why the Methodology of Scientific Research Programmes is Less Than Half the Story*, published in 2002. In this paper, John Worrall invites his reader to explore his analysis of actual examples from the history of science to show how Lakatos's MSRP, namely his notion of positive heuristic, can be further developed and complemented to achieve a more valuable "methodological tool". Worrall tries to show that the process of discovery can be reconstructed from the particular analysis of the formation of scientific theory. In other words, Worrall tries to demonstrate, no matter how Popper would reject it, that the "logic of discovery" can be preserved and actually traced from the history of science, and positive heuristic can guarantee certain amount of objectivity.

The episode Worrall takes from the history of science is Fresnel's wave theory. It is described in detail here. It has much to clarify not only about how MSRP can be advanced, but also about Fresnel's possible way of thinking as well. In this episode, Worrall tries to illustrate that the 'hard core' of Fresnel's theory, or as Worrall calls it "the general wave theory", can be clearly reconstructed by "the method of deduction from phenomena" (which by definition would enable to trace rationale of the positive heuristic). According to Worrall, such a reconstruction, provided a few lines below, may be considerable since it helps to see that the modified version of positive heuristic can be useful not only to strengthen "the protective belt", but can be utilized for the hard core as well, which as a result would articulate Lakatos's original account:

> For Lakatos, a "positive heuristic" gives guidance for the articulation of specific theories only within the context of a given scientific research programme… And there is no suggestion in Lakatos that the invention of "core" theories is anything other than a matter of logically unanalysable Popper-style conjecture. I shall show that, not only did Fresnel infer specific theories from the data, plus the general wave theory, he *inferred the general wave theory* (i.e. the *hard core* of his programme itself) *"from the phenomena"* (Worrall, 2002: 93).

Thus, it is useful to look closer at the hard core of Fresnel's hypothesis. Worrall's reconstruction of Fresnel's way of comprising his hard core can be presented in the following way:

> The inference that Fresnel used to argue for the wave theory of light can be reconstructed as follows:
>
> (i) Background knowledge (in the form of the "mechanical philosophy") entails that the physical world consists of matter in motion.
>
> (ii) Hence light in particular consists of either matter in motion or motion through matter.
>
> (iii) If light consisted of bits of matter in overall motion, then the emission of particles from a luminous source would form either (a) a more or less continuous stream or (b) a succession of discrete particles.
>
> (iv) Possibility (a) is ruled out by the fact that two beams can cross each other, at right angles say, without either being affected beyond the point of crossing (if the two beams were two streams there would surely be a good deal of interesting action where they crossed which would modify the beams in their further progress).
>
> (v) No such problem need arise on possibility (b). This sort of "non-superposition" could be explained by assuming that the particles of light follow one another at great distances, hence making the probability of any collision between particles in beams that cross one another very small. However, at least in Fresnel's opinion, this possibility too was ruled out in clear cut way by well-established experimental results – principally those concerning diffraction of light.
>
> (vi) It follows therefore from (i) to (v) that light must consist of motion through matter.
>
> (vii) It is also part of background knowledge that light has a finite velocity (Huygens explicitly refers to Roemer as having established this); hence there must be a material medium intervening between source and receptor to carry the motion making up the light in the finite time-interval between emission and absorption. (the "luminiferous aether" is hence inferred not conjectured!)

> (viii) All sorts of optical phenomena exhibit periodicities – properties that recur at regular spatial and temporal intervals: notably the phenomena of Newton's rings and various interference effects. (This premise, firmly emphasised by Fresnel, is missing from Huygens who really held a "disturbance", rather than a wave, theory of light.) Again the periodicity of light was part of commonly accepted background knowledge (accepted by Newton, for example, who, to explain this, conjectured that his "parts" of light revolve with given periods as they move along).
>
> (ix) Hence light consists of regular, periodic oscillations transmitted from point to point in the ether (Worrall, 2002: 94-5).

The argument shows more or less clearly Worrall's standpoint: how, step by step, formation of a certain theory's hard core, in this case Fresnel's wave theory, can be re-established. Worrall's incorporation of the method of "deduction from phenomena" in this particular case would mean the correlation of knowledge in hand (i.e. already accomplished, "background" knowledge) into the hard core of the theory, which in turn gives grounds for the demonstration of its objectivity, or, as Worrall would state, can "indeed be argued to on the basis of material that is taken to be already known rather than merely conjectured" (Worrall: 2002, 95). It might be supposed that Worrall offers the possible solution to the problem of conventionalism in Lakatos's conception of the hard core (which, according to Lakatos, should be accepted as "true" by convention) by illustrating how it can be "inferred", not "merely conjectured", from the background knowledge.

As a conclusion, some clarifications, described by Worrall, should be discussed. First of all, Worrall would claim that Fresnel, or any other scientist, does not have construct his hard core in the manner described above. But the fact that it is able to trace many fragile details in the process of discovery should be noticed:

> …I am not claiming that Fresnel (or any other great scientist) actually first discovered any of his theories (first arrived at any of them in his own mind) by consciously going through a detailed argument of the kind articulated here. […] But the fact that there is such a detailed argument to be articulated, the fact that its premises are widely known and fairly widely, if not always universally, accepted is crucial. It explains what would otherwise be the entirely mysterious process of theory-creation; it

> explains what would otherwise be the entirely mysterious prevalence of simultaneous discovery or near simultaneous discovery in the history of science; and it explains what I think is the inescapable feeling for anyone who studies the history of science carefully that even the greatest scientists save science only a relatively few years… (Worrall, 2002: 96)

Secondly, Worrall rightly clarifies that the method of “deduction from phenomena” does not in any way provide a proof for the theory under consideration. Neither this method guarantees the universal “truth” of the theory, since it can be clearly seen that every theory from the history of science, no matter how much successful, becomes rejected in the favor of another one. Worrall would claim that notwithstanding the fact that a certain hypothesis can be rationally reconstructed, this hypothesis still has a chance to be rejected some time as long as it involves theoretical judgments and claims. In general, Worrall does not insist on the point that the method of “deduction from phenomena” is absolutely valid; it surely has certain problems, such as the status of background knowledge in the “objective” logical chain of theory’s argumentation, or applicability of this method to other cases in the history of science. Worrall only tries to point out to the fact that such method is worth of further consideration.

In concluding this Chapter, it can be said that the case of Young and Fresnel was not actually much deficient for evaluative attempts. Much useful information is gathered to be used; it appears that Young’s studies are better clarified since they received more attention from commentators. The issue is that Young was not evaluated positively at the most of the time; it is the issue of the succeeding Chapter to understand why.

CHAPTER VII

CONCLUSION: WHO MADE THE REVOLUTION IN OPTICS IN THE NINETEENTH CENTURY?

The last section of the current thesis aims at combining the information into the unified piece of analysis. It can be said that enough material is gathered to trace the light into the reasons of the delay occurred in the acceptance of the wave theory at the beginning of the nineteenth century.

It can be stated with certainty that neither Young, nor Fresnel stood at the roots of the birth of the wave theory. Looking from the standpoint of MSRP, the research programme came to the hands of these scientists with a certain hard core, which can be defined as "Light is a wave, propagating in the elastic medium – aether". Huygens' principle was developed; both Young and Fresnel incorporated it in the set of assumptions of the protective belt. The phenomena of diffraction and Newton's rings were observed, offering the alternatives for empirical test. Thus it is necessary to understand how, in hands of Young and Fresnel, the wave research programme was developed in a progressive problem-shift, or was it developed at all. It is also vital to detect which factors, according to the Methodology of Scientific Research Programmes, made the acceptance of the programme to be postponed.

To take Young's work in optics; it is hard to claim that Young succeeded to turn the wave theory into a progressive problem-shift. Starting with certain theoretical grounds, i.e. the hard core of the wave theory provided above, he was unable to serve them with satisfactory empirical support. Investigation of his papers demonstrates that Young did not try to develop his theoretical basis, he did not occupy himself with arranging experiments and taking record of their results in order to improve his theoretical grounds. Four papers of Young were examined; from these four papers only one table of experimental results was obtained, the half of the which was filled with Newton’s results. It is surely not enough for the theory which aims to make the change in optics. One would not be able to point at the certain experimental technique or method, which was unique to Young, and which could be helpful to moderate and push his hypothesis further. All that shows that the functions of positive and negative heuristics described in MSRP have not received their realization. Young’s theory in optics did not succeed to accumulate its heuristic power, it was not convincible enough to impress the scientific environment of that time. Thus, there is nothing extraordinary in the fact that Young’s deductions were ignored. There is no need to search for external explanations of the reasons of this neglect, and as Worrall (1976) demonstrated in any case they have no grounds.

Nevertheless, it should not be declared that Young’s hypothesis should go to the trash can. Young might have started his inquiry from analogy between light and sound, but as was stated in Chapter I he was not the only one who was using the analogy reasoning. Notwithstanding that way of thought, Young was the first to formulate and introduce the principle of interference in optics, not in the field of hydraulics or sound. Young might be unsuccessful to develop that principle into the competitive tool to stand as a part of “mature science”; still, it should be admitted as Young’s remarkable contribution to the wave theory. Moreover, while covering the phenomenon of Newton’s rings (Young, 1802b), Young correctly “foresees” that the velocity of light in different media is different. That fact should not be neglected either.

What is the heritage then Young’s theory of light left in the history of optics? Young’s considerations on the wave theory might not be taking the central place in the wave research programme, they can stand as a set of “intelligent suggestions” – a series of theoretical assumptions which projected the path of development of that programme. Revising one of the valuable features of MSRP (p. 40), it can be said that Young’s hypothesis was more successful on theoretical level, suffering a lack of empirical support. Namely, Young was the first who formulated the wave theory in the way that made it to be heuristically advantageous, which in turn led to its the progress later.

The work of Fresnel, on the other hand, can be treated as a brilliant example of the formation of "mature science". Fresnel started his investigation in optics from the same point as Young did – not much work was performed in the field of the wave theory in the years between the publications of Young and Fresnel's memoir on diffraction. In his paper Fresnel explicitly demonstrates his method of inquiry and the route of development of his hypothesis he followed. By concentrating on the phenomenon of diffraction, Fresnel succeeds not only to achieve a valid explanation of it, but also demonstrates how other existing explanations are inconsistent with the hypothesis he defends. The hard core of Fresnel's theory does not undergo much modification and can be defined in the same way, that "light is a wave spreading in all-pervading medium – ether". Fresnel clearly postulates his protective belt of additional assumptions – the principle of interference and Huygens' principle. One can clearly see the positive heuristic at work – as Fresnel himself declares, he firstly arrived at the different explanation of diffraction, but, after detecting the inconsistency between his theory and the experiments he performs, modifies the assumptions in the protective belt, and achieves very precise results. Fresnel's technique of testing a theory is also quite remarkable: he firstly composes some theoretical grounds for explanation, deduces a formula and makes a list of theoretical results; then he performs observations and records the results obtained by the experiment. The comparative analysis of theoretical and empirical data gained by the means of this technique appears convincing enough to demonstrate the failure or success of the conjecture in question.

The integrals incorporated into the Fresnel's hypothesis should be mentioned as well. The use of mathematics throughout the work brings certain organization and structure to Fresnel's theory. It becomes testable and adds an amount of "objectivity" to his account. Thus, the use of integrals, together with technique of the test, might be included to the heuristic tools of Fresnel's wave theory.

Fresnel's theory can be treated as successful on the both levels of innovation – it succeeded to built a set of theoretical grounds supported by experimental results of high precision to constitute for a hypothesis with heuristic power. Together with incorporation of the novel fact on the diffraction of light, this factor underlies the main difference between Young and Fresnel. By the means of MSRP it is possible to demonstrate the failure of Young and the success of Fresnel. Where Young gets confused among his theoretical formulations, Fresnel consolidates his protective belt of assumptions; where Young persists with vague descriptions of certain experiments, Fresnel develops his techniques to increase the demonstrative power of his claims; where Young is satisfied with a few experimental results, Fresnel works hard to increase the empirical power of his theory. The incorporation of MSRP reveals that Young's neglect among scientists is not a surprise at all.

The case-study investigating the delay of the acceptance of the wave theory has succeeded to trace the light on the applicability of MSRP to this particular example from the history of science. It can be seen that this method of analysis has many effective features which help to evaluate the activity of scientists. MSRP enables to trace the value of a certain theory and its role in the course of scientific development.

Nevertheless, the Methodology of Scientific Research Programmes has a couple of points which are open to discussion and can be modified to increase its effectiveness. First of all, there is a question of reliability of Lakatos' account. Lakatos states that

> Scientists, [on the other hand], are very skeptical even of their best theories. Newton's is the most powerful theory science has yet produced, but Newton himself never believed that bodies attract each other at a distance. So no degree of commitment to beliefs makes them knowledge. […] The cognitive value of a theory has nothing to do with its psychological influence on people's minds. Belief, commitment, understanding are states of the human mind. But the objective, scientific value of a theory is independent of the human mind which creates it or

> understands it. Its scientific value depends only on what objective support these conjectures have in facts (Lakatos, 1978: 1)

Lakatos claims that there is no place for commitment in the knowledge-making process. One cannot call a commitment to certain belief as knowledge, and Newton may be skeptic about his hypothesis, but how scientists can work on to produce "knowledge" without personal commitment to certain belief or set of beliefs? There is a slight possibility that scientist, explaining certain phenomenon, does not have the strong belief that it is, or should be, actually the case. In the light of that claim the concept of the hard core, which should be accepted as true by convention, reappears as a problem. The statement "being true by convention", synonymous to "being true by mutual agreement", can be re-written as "being true by mutual commitment of scientists to the certain set of theoretical propositions". No matter how bold and crude these propositions are, putting a methodology on the basis of a "convention" and claiming that personal commitment of scientists should not be considered in any case, appears inconsistent.

A short remark should be made here on the point of the conventionalism in the account of MSRP. In order to reduce the degree of conventionalism in MSRP and achieve more "rationalized" concept of the hard core, Worrall (2002), by the example with Fresnel's hypothesis, proposed that the hard core of a programme can be actually inferred from the background knowledge, thus leaving no space for the "truth by convention". The analysis of the Young-Fresnel case demonstrates that "inference from the background knowledge" might not represent the actual state of affairs. It can be traced that Young and Fresnel *accepted* the background knowledge after studying what was done in the wave theory so far and obtaining all necessary information; they did not produced inferences. In other words, these scientists "chose" to accept the hard core to be "true" by convention.

Another discussable point in MSRP is the relation between hard core and protective belt: there is no clarification of how exactly these two are related to each other and who decides which conjecture goes to which of the two. In the case of Newton the picture may be more or less clear, but, for example, the case of Fresnel becomes much more difficult to classify. It can be agreed that Fresnel's indestructible basis is the claim that "light is a wave propagating in elastic medium – aether". Following MSRP, it could be accepted as Fresnel's hard core. At the beginning of his inquiry, there were no auxiliary hypotheses which would construct the protective belt of the hard core. Through the course of the development of his theory, Fresnel arrives at his "protective belt" consisting of the principle of interference and Huygens' principle. By deducing the formula on the basis of these principles, Fresnel unifies them in such a way that these postulates become fundamentals for his theory. Should then they be also treated as the part of the hard core? This point is also open to discussion.

The concept of novel prediction in MSRP leaves a space for discussion as well. Lakatos would state that

> in a progressive research programme, theory leads to the discovery of hitherto unknown novel facts. In degenerating programmes, however, theories are fabricated only in order to accommodate known facts (Lakatos, 1978: 5).

Following this line of thought would classify Fresnel's account on diffraction as "degenerating", but is that actually the case? Should a successful proposition, deduced as a consequence of a certain theory and passed the empirical test with very high degree of precision, be treated as novel only if it is new in time? The case of Fresnel shows that it should not necessarily. The phenomenon of diffraction was not new in the time Fresnel wrote his memoir; in fact Fresnel mentions the experiments of Grimaldi to show that he is well aware of that fact. Still, it should be stated that Fresnel's explanation of diffraction can be no doubtfully treated as novel prediction. It is novel in the sense that theoretical results received a great degree of confirmation in experiment, which in turn increased the total corroborative content of the theory. It shows that there should be no necessity for the "novel" to be new in time; the classification of novelty should be rather performed in accordance with the degree of corroborative content. Worrall (1989), who advocates for this understanding of novel facts, brings the proper correction to this point of MSRP.

These are the points in MSRP revealed by the case-study, which are suitable for further investigation and can be further moderated to achieve more efficient tool of analysis. In any way, the aim should not be the invention of the perfect method; it should rather be in finding a set of evaluative criteria which would allow to conduct a valuable examination and help to understand a certain period from the history of science.

It can be now stated that the most of tasks of the current study have been completed. The only challenge left to accomplish is to try to find possible answers posed in the Introduction Chapter of this thesis. Methodology of Scientific Research Programmes demonstrates that there is a chance for the rational reconstruction of the process of the theory-change. By "rational" it is meant that a set of formal methodological merits is very helpful to track the objectivity content of the process in question. It helps to understand how one series of theories replace another series, and points to the factors which made one series to fail, and another to succeed. The term "reconstruction" is also vital: the level of objectivity and success, and the analysis itself can be conducted only retrospectively. That is one of the features MSRP stands for. As long as the methodology of science would deal with evaluation of the scientific activity, including that of theory-change, it would need the access to the complete picture of the development of the certain scientific programme – i.e. how it came to the scene or its background, how it was developed, and how it was transformed to another programme. Taking the current condition of scientific development would not give the complete piece for analysis, since there is no certainty about how exactly it would be developed and transformed.

The question of whether these methodological rules can stand as a guarantee of objectivity of the scientific theories may also be answered from the standpoint of retrospective analysis. As long as a certain part from the history of science is concerned, and the main goal of scientific activity is to accumulate objective content by means of the theories it produces, it is possible to record the path of objectivity from one programme to another via the chosen set of rules. It is possible by monitoring which pieces can be treated seriously as contributions to science, and which ones go the can of mere "trial-and-error". Again, there is no possibility to warrant this path for the future, since it is not certain how the activity in science would evolve.

Coming to the last and the trickiest question, it should be said that skeptics have a powerful argument in hand, which takes its roots in the doubt of the possibility of the scientific inquiry as such. Skeptical argument about objectivity and possibility of scientific inquiry persists due to the constant change in the domain of science. Sooner or later, every programme is replaced by another one; but each is claiming to cover the objective picture of phenomena in nature. If a programme accepted as "true" at one period of time fails to be "true" at another, there is no such thing as objectivity in science. But if the aim of science is to accumulate its objective content, then the process of scientific inquiry itself is meaningless. The problem of scientific inquiry goes back to the times of Plato, who in his *Meno* states the doubt about it. That problem is sometimes called as Meno's Paradox, and can be formulated as follows:

> The [Meno's] paradox is commonly formulated as a dilemma: either you know what you are searching for (in trying to solve a problem or acquire new knowledge) or you do not. If you do know, you already have it, whence inquiry is not possible. And if you do not know, you would not recognize it even if you stumbled on it accidentally; hence, again, inquiry is impossible, pointless (Nickles, 1980: 6).

The Paradox in its sharpest form is frightening; it looks like there is no possible solution for it, and science should be accused of being busy with the conduct which has no meaning. The possible way out of the paradox is to demonstrate that "you can know what you are looking for without already having it" (Nickles, 1980: 6). Methodology of science might be utilized as a tool to guide the inquiry to the right place it was "looking for".

It should be clarified here that scientific inquiry is a process, which is possible under certain conditions. One of the main aims of scientific inquiry is to obtain knowledge about phenomena in question, in a special way. One tries to obtain knowledge about the external world from the prism of certain theoretical basis, and this condition is inevitable, since otherwise there would be no valuable content in that inquiry. The methodological rules play their role in that process as well. These merits are ready to help in tracing the path of scientific activity from less complicated to its mature level, uncovering the reasons why scientists might have built their theories in a certain way, etc. It can be said that the choice of proper set of methodological criteria can give grounds to the process of scientific development and guide the path of inquiry.

But to what extent are these methodological merits useable and applicable to the actual case of scientific research? For example, did Fresnel, working on the certain problem of diffraction, have such criteria in mind in order to direct his inquiry in a certain way? These questions point to the fact that the process of scientific inquiry is not a static, law-like process. It cannot be fully analyzed only by the help of methodological rules. There is certain dynamics in the process of scientific inquiry, and especially in the process of discovery. Of course, it is not to rationally reject methodological considerations, without them the process of evaluation would be hardly possible, and without evaluation one would not be able to see how and toward what path the scientific inquiry is directed. Of course, every scientist deals with problem-solving, directs his research and the path of inquiry according to the needs of his/her "conjectural hypothesis", but there are other factors, such as personal interests, socio-political circumstances, support from environment, etc., which do influence the route of inquiry to some extent, and which, being individual (most of the time) for each case, can be hardly packed into a certain law-like generalization.

It should be deduced then that methodological merits are useful to understand how and in what direction the development of scientific inquiry occurs, but they have limits of application since scientific inquiry is a dynamic process, where each example has its own special circumstances. That process can be evaluated, but can hardly be controlled.

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APPENDICES

A. TEZ FOTOKOPİSİ İZİN FORMU

ENSTİTÜ

Fen Bilimleri Enstitüsü ☐

Sosyal Bilimler Enstitüsü ☐

Uygulamalı Matematik Enstitüsü ☐

Enformatik Enstitüsü ☐

Deniz Bilimleri Enstitüsü ☐

YAZARIN

Soyadı : Kulandina
Adı : Yevgeniya
Bölümü : Felsefe

TEZİN ADI : YOUNG AND FRESNEL: A CASE-STUDY INVESTIGATING THE PROGRESS OF THE WAVE THEORY IN THE BEGINNING OF THE NINETEENTH CENTURY IN THE LIGHT OF ITS IMPLICATIONS TO THE HISTORY AND METHODOLOGY OF SCIENCE

TEZİN TÜRÜ : Yüksek Lisans ☐ Doktora ☐

1. Tezimin tamamından kaynak gösterilmek şartıyla fotokopi alınabilir. ☐
2. Tezimin içindekiler sayfası, özet, indeks sayfalarından ve/veya bir bölümünden kaynak gösterilmek şartıyla fotokopi alınabilir. ☐
3. Tezimden bir (1) yıl süreyle fotokopi alınamaz. ☐

TEZİN KÜTÜPHANEYE TESLİM TARİHİ: