ON THE NATURE

OF LIGHT

PROF. G. G. STOKES
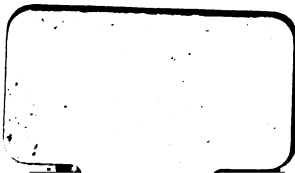
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BURNETT LECTURES.

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PREFACE.

THE lectures, of which the first course is contained in the present volume, originated in a new direction given to an old endowment. Mr John Burnett, merchant in Aberdeen, who died in 1784, bequeathed the greater part of his property to various charitable and pious objects. Among others, a portion of the property was vested in trustees for establishing prizes for the best and next best essay on the following subject:—

"That there is a Being, all-powerful, wise, and good, by whom everything exists; and particularly to obviate difficulties regarding the wisdom and goodness of the Deity; and this, in the first place, from considerations independent of written revelation,

S. *b*

and in the second place, from the revelation of the
Lord Jesus; and, from the whole, to point out the
inferences most necessary for, and useful to man-
kind."

These essays were to be competed for at
intervals of 40 years, and awards have been
made on two occasions since the original foun-
dation.

But it was thought that the production of
essays at such long intervals did not form a
satisfactory mode of utilizing the bequest of the
founder ; and in 1881 a new direction was given
to the foundation by an order of the Secretary
of State for the Home Department, in accord-
ance with the provisions of the Endowed Insti-
tutions (Scotland) Act of 1878.   By this order
it was provided that a Lecturer should be
appointed at intervals of 5 years, to hold office
for 3 years, the subject of the lectures being
determined by the following regulation :—

"The Trustees and assessors may prescribe as the
subject of the course of lectures that specified in the

codicil to the will of the testator, viz. [as above]. Or otherwise, the trustees may prescribe, as the subject of any course of lectures, recent researches (as at the date of the appointment) into any of the following branches of knowledge, viz :—

1. History, including the illustrations of the forms and effects of theistic doctrines among the older nations of the world.

2. Archaeology.

3. Physical science.

4. Natural science.

And they shall instruct the lecturer to have regard, in treating of the special subject prescribed, to the illustration afforded by it of the theme proposed by the testator, and that under such conditions or qualifications as they may prescribe."

The Trustees selected physical science as the subject of the first lectures under the new system, and they did me the honour of appointing me the first Burnett Lecturer. It has been arranged that four lectures are to be given each year of office, and that the subject of the complete course shall be Light, the subject being treated under three divisions, to which the lectures in each year are to be respectively devoted. The first

division is sufficiently explained by the title of
the present course; the subjects of the second
and third divisions will be found at the end of
the fourth lecture.

I am sensible of the difficulty of the task I
have attempted in this first year's course,
namely, while avoiding all mathematical details,
to give the audience some fair idea of the evi-
dence on which we accept the views respecting
the nature of light which are at present held, I
may almost say universally, in the scientific
world. I assume on the part of the reader a
knowledge of the rectilinear propagation of light
in the same medium, of the laws of reflection and
refraction, of the compound character and of the
decomposition of white light; but I have not
assumed that he is acquainted with the phe-
nomena of interference, or diffraction, or double
refraction, or polarization, though some acquain-
tance with these subjects will make the lectures
much more easy to follow. For the sake of
those persons to whom these things may be

nearly or entirely new, I have been obliged to enter into details which I cannot help fearing may be wearisome to those who have already a pretty good acquaintance with the subject; though I hope that even they may not find the weighing of the evidence wholly uninteresting or unprofitable.

G. G. STOKES.

CAMBRIDGE,
February 5, 1884.

CONTENTS.

LECTURES ON LIGHT.

FIRST COURSE.

ON THE NATURE OF LIGHT.
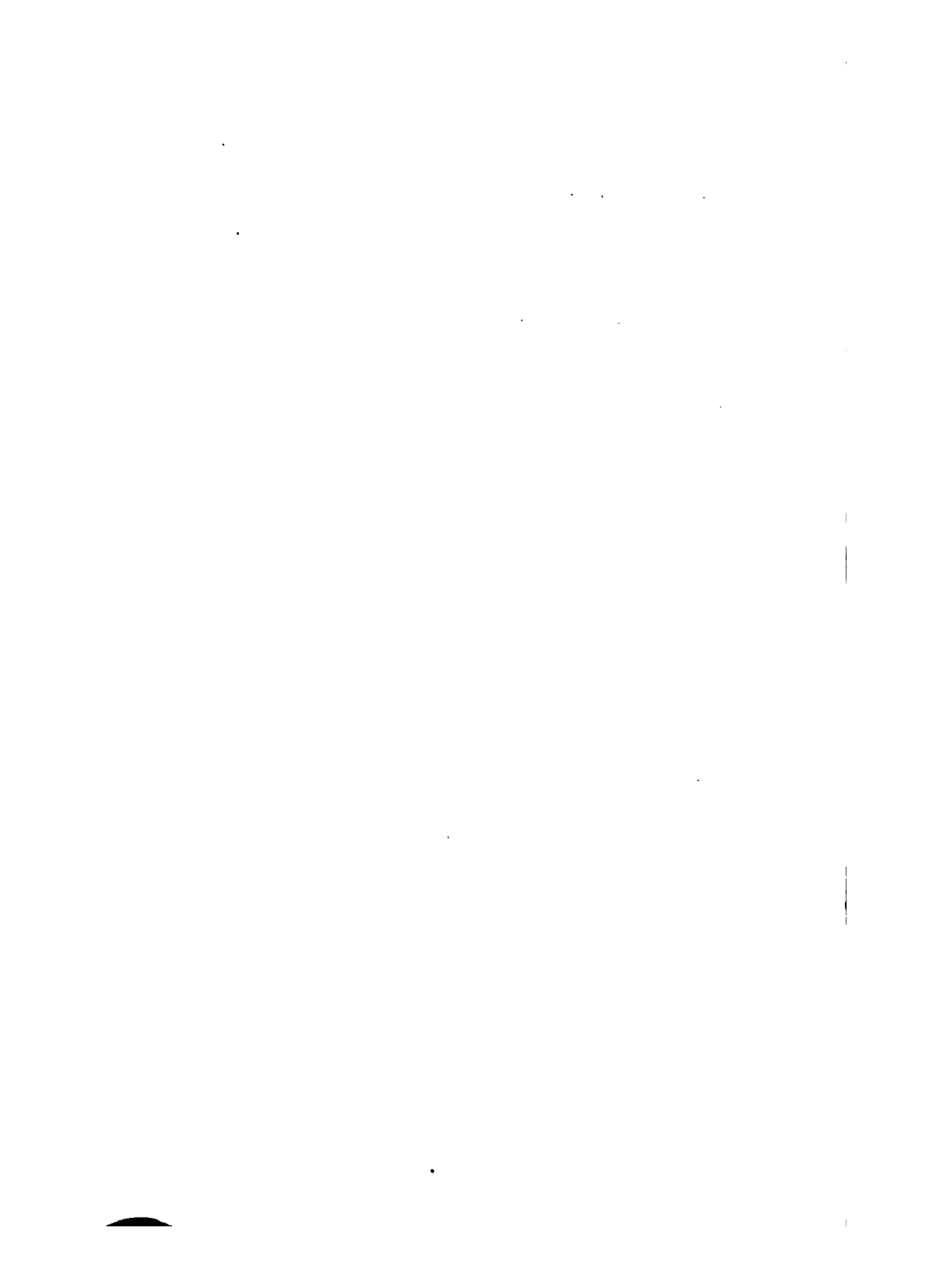
LECTURE I.

Bearing of the more obvious properties of Light on our view of its nature—Theories of emission and of undulations— Colours of thin plates—Newton's attempt to explain them on the theory of emission—Insufficiency of the explanation.

AMONG all our senses, there is none more wonderful than that of sight. It confers upon us, as Sir John Herschel has remarked, to a considerable extent the character of ubiquity. It is accordingly a matter of extreme interest to find out what we can as to the mode in which this end so important to our well-being is brought about; whether it be by investigating the properties of any agent external to ourselves which may be concerned in its accomplishment, or by seeking to penetrate some little way into that

S.   I

mysterious chain of sequence which connects the external agent with the sensation conveyed to our minds. It is true indeed that there is no prospect of our being able to bridge over the gulf which separates mind from matter; yet there are many things to indicate that that mysterious organ which we possess, the brain, has some intimate connexion with the operations of the mind, and we can do something towards tracing a connexion between the part of our bodies directly affected by the external agent and the brain ; a connexion of such a kind as to leave no doubt that it forms the means whereby the immediate action of the external agent is ultimately perceived by the mind ; though how the final conveyance takes place is a mystery we are not likely to fathom. But this does not prevent us from being able to trace some links in the chain of connexion, nor from recognizing the evidences of design which that portion which we can in a measure follow is calculated to impress upon us.

I have spoken of an external agent even though it was more especially the sense of sight that I had in view. We are so accustomed to the contemplation of an objective *something*, which we call Light, as the external agent by the action of which vision is in some way brought about, that we have a difficulty in conceiving how anyone could think otherwise. We

see the objects in a room, but it is matter of the commonest observation that though the objects may be there and our eyes may be there, we see them not if it be night, or if the room be closely shut up, until a lighted lamp or candle or something of the kind is introduced. We recognize the flame as the seat of some influence, to which we give the name of light, which is essential to vision. Yet obvious as this proposition appears to us, it is remarkable that it was not always so. At least one writer of acute intellect in ancient times held that it was something emanating from the eye, not something from without entering into it, that enabled us to perceive distant objects. This shows by what a slow and gradual progress our knowledge of physical science is built up. We are accustomed to vaunt of our knowledge in this 19th century; yet it may be that generations hence the scientific men of the day will wonder how we could have failed to perceive things which to them will appear quite obvious.

A self-luminous body then is, as such, a source of an influence which can be exerted at a distance. In this respect it by no means stands alone; two bodies at a distance from one another may for example influence each other through the attraction of gravitation, and other modes of influence might also be mentioned, but I will confine myself to gravitation.

Now between these two modes of influence there are great and striking differences. One of the most salient is this, that in the case of gravitation the influence is exerted independently of the interposition of matter of any kind, whereas in the case of light the influence is capable of being arrested by an extremely thin screen of matter of a suitable kind; for example, silver foil, or a film of Indian ink spread on glass.

The consideration of a screen leads us naturally to another fundamental and very obvious property of light. Suppose the screen pierced by one or more rather small apertures; how will the influence beyond the screen be distributed? We find that it is perceived only within the projections of the aperture or apertures made by straight lines drawn from the luminous body, which for simplicity I here suppose to be a point.

This indeed is not rigorously true, for about the boundary of the projection of the aperture, that is, near the projection of the edge of the aperture, there is a gradual, not sudden, passage from light within to darkness outside, accompanied by fluctuations of greater and less luminosity which I cannot now enter into particularly; and when the aperture is extremely small, the spreading out of the light which passes through it is by no means very small compared with

the breadth of the projection of the aperture itself. Nevertheless in ordinary cases the spreading out of the light is so small that we may disregard it, and say that light proceeds from a luminous point in all directions in straight lines until it is stopped by some obstacle. It will be understood that I am here speaking of light only as it passes in free space, or which comes to nearly the same thing, in air, and not from one medium into another.

When I speak of light proceeding *from* a luminous point, all I wish to express is that we recognize the luminous point as the seat or origin of a certain influence which is exerted, though with an intensity which diminishes with increasing distance, at all points from which a straight line can be drawn without interruption to the luminous point; I do not wish to imply the idea of motion, or propagation of any kind. The feature therefore that we are at present considering is common to light and gravitation.

A very important question now arises, Do these influences take time to travel, or does the influence exerted at any moment of time depend solely on the state and position of the two bodies, the influencing and the influenced, at that moment? The idea which we may be led to form as to the nature of light must depend most materially on the answer we have to give to this question as applied to light.

That light is propagated in time, cannot be inferred from ordinary observation. Thus when a landscape is illuminated at night by a flash of lightning or an explosion of gunpowder, the flash and the objects illuminated by it are seen, so far as our senses can decide, simultaneously, though in the latter case the light has to travel from the flash to the object illuminated, and from that to the eye, instead of coming straight from the flash. As far as ordinary observation goes, then, the question whether any time is occupied in the transmission is left an open one, and we can only say that *if* time is required for transmission, the rate of travelling must be enormously great.

But in the solar system we have distances to deal with compared with which the dimensions of the earth itself on which we dwell, let alone those of a landscape, sink into insignificance. It is conceivable that in travelling over those vast distances, if it does travel at all, light might occupy lengths of time which would not be insensibly small, and which possibly might be put in evidence by some celestial phenomenon. Thus if the light of the sun were emitted by flashes, the sun would take the place of the thunder-cloud in our supposed observation, and the planets that of the objects in the landscape. Though there are changes going on in the sun, as we

now know, there are none of such magnitude and suddenness as to be available for such an observation. Nevertheless it was by celestial observations, of a somewhat different kind, that the finite velocity of light was first revealed.

This was done as long ago as in 1676 by Roemer, who showed that an inequality in the times of occurrence of the eclipses of Jupiter's satellites, which he had observed, was simply explicable on the supposition that light is propagated with a finite velocity. The mean motion of a satellite round the planet being accurately known, from observations extending over a sufficient time, the times of successive eclipses can be calculated. It was found that the eclipses happened earlier or later than the calculated times, supposing the epoch so chosen as to make the accelerations and retardations balance on an average, according as the earth and Jupiter were on the same side of the sun or on different sides, and the differences in the apparent errors in the times of occurrence of the eclipses at different times of the year were proportional to the differences of the distances of Jupiter from the earth at those times. The greatest difference in that distance is evidently equal to the diameter of the earth's orbit (supposing for simplicity the orbits of the earth and Jupiter circular and in the same plane), and to this corresponds

the greatest difference in the apparent errors of the
times of occurrence, which amounts to about 16
minutes and a quarter. We learn therefore that if
this be the true reason of the observed inequality,
light takes 8 minutes and a few seconds to travel
over the distance from the earth to the sun. To ex-
press the velocity in miles per second, we require to
know the dimensions of the earth's orbit, or what
comes to the same the solar parallax, which, being
the ratio of the radii of the earth and the earth's
orbit, serves to express the latter in miles since the
former is accurately known. According to the value
of the sun's parallax which was accepted till recent
years, the velocity of light so determined came out
about 192,000 miles per second. More recent de-
terminations have reduced this to about 186,000 miles
per second.

For nearly 50 years after Roemer's discovery, no
other phenomenon was known which indicated that
the propagation of light was other than instan-
taneous; but then Bradley made the very remarkable
discovery of the aberration of light. It would
occupy too much time to go fully into this, and I
must content myself with giving you a general notion
of it.

Suppose a person out on a perfectly calm day
when rain was falling, and accordingly, on account of

the perfect calmness supposed, falling vertically. If
the person were at rest, he would deem the rain to be
falling vertically. But suppose he were carried hori-
zontally along with a motion so smooth that he was
unconscious of it. The rain though falling vertically
would *appear* to him to fall in a somewhat slanting
direction, as if it came from a point not exactly
in the zenith, but displaced from it towards the
point towards which the observer is being carried.
And if instead of falling vertically the rain be
falling in a slanting direction, it will appear to the
observer, unconscious of his own motion, to slant
differently; in fact, if we compound the velocity of
the falling rain with a velocity equal and opposite
to that of the observer, the direction of motion of
the rain will *appear* to be that of the resultant
velocity.

Now Bradley found that just the same thing takes
place with regard to light. The earth in revolving
round the sun moves at the rate of about 20 miles in
a second, towards a point in the heavens lying in the
plane of the ecliptic 90^0 in advance of the heliocentric
position of the earth, or rather what would be 90^0
if the earth's orbit were strictly circular. The light
which comes from any particular star appears to come
from a place deviating from the true place of the star
towards the point of the heavens towards which the

earth is moving. And as this point goes round the
ecliptic in the course of a year, the apparent place of
the star describes annually a closed curve, in fact, a
small ellipse, round the mean position of the star.
The *law* of the apparent displacement of the star is
found to be what it ought to be on the supposition
that the cause of it is what has been above explained,
and the coefficient of the displacement gives the ratio
of the velocity of light to that of the earth in its
orbit. We are thus furnished with a second means of
determining the velocity of light, and observation
will show whether the two do or do not agree. The
result is a remarkably close agreement when we
consider on the one hand the difficulty of fixing on
the exact moment of disappearance of a satellite
which enters the shadow of Jupiter, and on the other
the smallness of the displacement which constitutes
aberration, amounting at a maximum to only 20
seconds and a quarter of angle, about the angle
subtended by a six-inch rule at the distance of a
mile.

It is to be noted that the unit of length in terms
of which the velocity of light in both of these
methods is primarily expressed is the radius of the
earth's orbit, and in order to translate it into miles
per second we require to know the solar parallax.

While I am on this point I may mention that in

the year 1849 Fizeau, by an extremely ingenious combination of apparatus, succeeded in determining for the first time the velocity of light by direct experiment. The resulting value was confirmatory of the two astronomical determinations, but was not at first considered as capable of being put in competition with them for accuracy, and no wonder, since the whole interval of time to be measured in Fizeau's experiment, under the circumstances in which it was performed, amounted to only about the one twenty thousandth part of a second. Subsequent determinations, however, first by Foucault, by a different method, and afterwards by Cornu, by Michelson, and by the late Dr James Young and Prof. G. Forbes, by one or other of those methods modified, have rendered the determination so certain and accurate that it is probably quite equal in accuracy to the astronomical determinations. And since the experimental determination gives the velocity in terms of a known distance on the earth's surface, and accordingly in miles or kilometres per second, if we assume the experimental and either of the astronomical determinations as separately valid and sufficiently accurate, by comparing the two we can determine the radius of the earth's orbit, which fixes the scale of the whole solar system, in miles or kilometres. And as we know accurately the dimensions of the earth, we can thus

determine the solar parallax, by combining accurate
measurements of an astronomical phenomenon fre-
quently recurring or constantly going on with a
laboratory determination made once for all.

I have been tempted into a digression by the
interest of this subject, and I would now resume the
consideration of some of the elementary properties of
light, with a view of showing how we are gradually
led to the formation of a theory, now thoroughly
tested, as to the nature of Light itself.

The observations to which I have last alluded
show that light, whatever it may be, takes time to
travel, so that in speaking of light as an influence
that proceeds *from* a source of light there is no
longer occasion to exclude the idea of motion of some
sort. I have compared and contrasted two influences
of which I have spoken, light and gravitation, and in
this last point again it is a contrast with which we are
presented; or at least if the two are analogous the
analogy has never yet been established. After having
ascertained that light takes time to travel, and con-
templated that exceedingly curious phenomenon re-
lating to light, aberration, the question naturally
presents itself to the mind, Is there anything analo-
gous as regards gravitation? Does it like light take
time to travel, or is it an instantaneous influence?
Now the consequences which would follow as regards

the motions of the bodies of the solar system if gravitation like light took time to travel have been calculated, and it has been concluded that if the influence of gravitation takes time to travel, it at any rate travels incomparably more quickly than light, with a velocity accordingly which is incomparably greater than 186,000 miles per second.

I have reduced to a minimum the mention I have made of the fundamental properties of light, in order not to weary you by repeating what is to be found in every text book, and we may now attack the question, What notion are we to form of the intrinsic nature of this agent, so important to our well-being, so wonderful in the scale of magnitude of the quantities it brings before us, as we have seen already as regards the velocity with which it is propagated, and shall see later on in other respects?

There appear to be but two modes possible of conceiving of a *mechanical* influence emanating from an influencing body, travelling with a finite velocity, and ultimately influencing another body at a distance. A mechanical influence implies the action of matter of some kind, and this matter we may suppose to have been either darted forth, in the manner of a projectile, or to have previously existed in the space between the influencing and the influenced body, to have been disturbed by the influencing body, and

then to be successively agitated in successive portions, each portion being agitated by its predecessor, and in its turn yielding to its successor the disturbance so received. We have not far to go to find illustrations of both these kinds of action. Bullets exemplify the first; the progress of waves at the surface of water illustrates the second. A still better illustration, except that it is not visible, is afforded by the phenomena of sound.

Such accordingly are the ideas which lie at the base of the two theories as to the nature of light which for a long time divided the scientific world between them, the corpuscular theory, or theory of emission, and the theory of undulations.

Prima facie there is much to be said in favour of the theory of emission. It lends itself at once to the explanation of the rectilinear propagation of light, and the existence of rays and shadows. It falls in at once with the law of aberration. The laws of reflection and refraction admit of an easy explanation in accordance with it; at least if we except the existence of both reflection and refraction; for according to this theory we should rather have expected beforehand that light would have been *either* reflected or refracted, according to circumstances, not that incident light should have *divided* into a portion reflected and a portion refracted.

The theory of undulations on the other hand presents at the outset considerable difficulties. In the first place it requires us to suppose that the interplanetary and interstellar spaces are not, strictly speaking, a vacuum but a plenum ; that though destitute of *ponderable* matter they are filled with a substance of some kind, constituting what we call a *medium*, or vehicle of transmission of the supposed undulations. When I speak of this medium as a substance, or as material, I mean that it must possess that distinctive property of matter, inertia ; that is to say a finite time must be required to generate in a finite portion of it a finite velocity. The necessity of thus filling space with substance seems to have presented a serious difficulty to some minds. In the course of a conversation with Sir David Brewster, who had just returned from France, where he witnessed the celebrated experiment by which Foucault had just proved experimentally that light travels faster in air than in water, I asked him what his objection was to the theory of undulations, and I found he was staggered by the idea *in limine* of filling space with some substance merely in order that " that little twinkling star," as he expressed himself, should be able to send its light to us.

I cannot say that this particular difficulty is one which ever presented itself as such to my own mind.

To me the difficulty is rather that of conceiving such an influence as that of gravitation to extend across an absolute void. Such was the feeling of the great discoverer himself of universal gravitation. In a letter to Bentley, quoted by Faraday as falling in with his own views, Newton thus expressed himself :—

"That gravity should be innate, inherent and essential to matter, so that one body may act on another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws ; but whether this agent be material or immaterial, I have left to the consideration of my readers."

If the supposition that light consists in undulations obliges us to suppose that space is filled with some kind of substance, at least as far as the remotest star that our most powerful telescopes reveal to us, may it not be that that same substance forms, in some manner as yet unknown to us, the link of connexion whereby the sun is enabled to attract the earth, and keep it in its orbit ? It is true that notwithstanding the labours of various scientific men we are not in a

condition to give an explanation of gravitation, but
our inability to explain it by no means proves that it
is a primary property of matter, incapable of explana-
tion, or forbids us to suppose that it may in some way
be brought about through the intervention of that
same substance which we find necessary to assume
for the explanation of the phenomena of light on the
theory of undulations. And it is quite conceivable,
and we may now say even probable, that this same
substance has yet other offices to fill. Perhaps the
most remarkable of all the investigations of the late
Professor Clerk Maxwell is that in which he showed
that there is a certain velocity, numerically determin-
able by purely electrical experiments which can be
made and have been made in the laboratory, and
expressing the velocity of propagation of an electrical
state, which is identical with the velocity of light
within the limits of error of the experiments and
observations whereby the two have been determined.
Assuming for the moment, as a thing at the present
day resting on evidence quite overwhelming, that
light consists of undulations, we cannot fail to be
impressed by the multiplicity of purposes, all bearing
so intimately on our well-being, which it seems pro-
bable, or not unlikely, are fulfilled by one and the
same substance, endowed with properties which we
are only gradually learning.

I have ventured to allude for a moment to the present state of the theory of light, and I will now go back. The necessity of assuming the existence of some kind of substance in what we commonly speak of as a vacuum, does not appear to have been a serious preliminary difficulty in the way of the reception of the theory of undulations. A far more formidable difficulty appeared at first to be presented by the existence of rays and shadows. It was this that led Newton to adopt the theory of emission, though even he was led in the course of his researches on light to suppose that there was some sort of medium through which the particles of light moved, and in which they were capable in certain cases of exciting a sort of undulation. But the supposition of particles darted forth seemed to him necessary to account for shadows. If light consisted simply of an undulation propagated through a medium of some kind filling the interstellar spaces, as we know sound consists of an undulatory movement propagated in the air, how can we conceive of the existence of shadows, knowing as we do that sound passes freely round corners, and diverges after passing through apertures, though indeed it is true that the freedom is not absolute? Newton's contemporary Huygens was more bold, and adopted the theory of undulations pure and simple, rejecting altogether the notion of particles darted forth from

the luminous body, and travelling with the velocity of light. Huygens made a grand attempt to explain the existence of rays, the great stumbling-block at the threshold of the theory of undulations. The principle which goes by his name lies at the very foundation of the theory of undulations, and itself rests on a strictly mechanical basis, being in fact merely an application to the particular question under consideration of the general dynamical principle of the superposition of small disturbances. But this principle does not by itself alone suffice for the explanation of rays. It proves, or at least appears to prove, too much. It is as applicable to sound as, on the supposition that light consists in undulations, it is to light; and if Huygens's explanation of rays were complete there ought equally to be rays of sound, and sound ought to present the same sharp shadows as light.

Huygens attempted to get over this difficulty by entering on certain speculations as to the ultimate constitution of the *ether*, as we call the supposed medium which is the vehicle of light, and as to the mode of action, one on another, of the ultimate molecules of which he imagined it to consist. In this he abandoned the simplicity of the fundamental conceptions of the theory of undulations, and adopted a mode of reasoning not strictly allowable. For the transmission of regular undulations, of which the

period is arbitrary, at least within wide limits, requires us to suppose that the transmitting medium is either continuous or may be treated as such; that if it consist of ultimate molecules, or be otherwise heterogeneous, the number of intervals from molecule to molecule, or of deviations of one sign or another from an average homogeneity, shall be very great and as good as infinite within the length of a single undulation; and we have no right to extend to the medium treated as a whole, and regarded as continuous, a mode of communication of motion applicable only to the communication from one to another of a set of discrete molecules.

Accordingly, notwithstanding all that Huygens has done, the existence of rays and shadows, one of the most obvious properties of light, had received no satisfactory explanation on the theory of undulations such as it came from the hands of Huygens; and in this condition it remained for considerably more than a century. His explanation of the laws of reflection and refraction leaves nothing to be desired, except in so far as these laws involve the conception of rays. I cannot now speak of his discovery of the laws of double refraction in Iceland spar, because it belongs to a different branch altogether of the subject.

The theory of rays and shadows long remained in this unsatisfactory state; in fact, till quite the end of

the last century.   Newton's discovery of the com-
pound nature of white light showed that there must
be in light an element of some kind susceptible of
continuous variation.   Each theory, the corpuscular
and the undulatory, furnishes elements susceptible of
continuous variation.   What the element is, on the
theory of emission, has not, so far as I know, been
specified by the supporters of that theory, and diffi-
culties seem to attend whatever supposition in that
respect you can make.   The theory of undulations
presents one, and I may say but one, element which
might serve for the purpose, namely, wave length, or
what comes to the same thing, periodic time.   That
in fact periodic time must be the element variations
in which correspond to variations in refrangibility, is
clearly pointed out by other phenomena which I have
not as yet touched upon.   But this development of
the theory did not take place till the present century,
though some of the leading facts on which it is based
were known to and studied by Newton.   Accordingly
from the time of Newton till the end of the last
century, and even further, the theory of emission was
that chiefly in vogue with scientific men.   Various
causes probably contributed to this result.   The
rectilinear propagation of light at first sight looks
more like the motion of projectiles than the propa-
gation of undulations, which in cases of what are

undoubtedly undulations spread out much after being
laterally confined. The impetus given to the study
of the motion of particles under the action of known
forces by Newton's great discovery of universal gravi-
tation, turned the labours of men of science into that
channel rather than to a study of the propagation
of vibrations. The great weight again of Newton's
authority had doubtless its share in leading men to
follow the theory as to the nature of light which he
had taken up.

It is probably due to this preponderating influence
of the theory of emission that so little notice was
taken of the theory of the aberration of light. In the
explanation of the phenomenon which is contained in
the ordinary text-books of astronomy, which has
doubtless descended traditionally from that given in
earlier treatises, it is quietly assumed as a matter of
course that the rectilinear propagation of the light
coming from a heavenly body is not disturbed by the
motion of the earth. Did light consist of particles
darted forth, there is no reason to suppose that it
should ; in fact, to make such a supposition would
be to fly in the face of all we know respecting the
action of attracting forces, since any motion of the
attracting body does not enter into account. But on
the theory of undulations it is far otherwise. We
should naturally have been disposed to look on the

earth in its motion round the sun as ploughing its way through the ether. Now if light consist of undulations propagated through this ether, we might have expected that the ether being pushed by the earth out of its way, the course of the undulations which it carried would be affected, possibly in an irregular way, in case eddies were produced, and at any rate in a manner which there appears no reason *a priori* should be in conformity with the simple law of aberration.

Accordingly Dr Young, to whom mainly we owe the revival of the theory of undulations which took place about the beginning of this century, supposed that instead of the earth's pushing the ether out of its way, it allowed it to pass freely through its substance, 8000 miles though it be in thickness, far more freely than a grove of trees transmits the wind; and that in consequence of this perfect freedom of passage, the ether outside the earth's surface was not disturbed by the earth's motion, nor consequently the undulations passing through it.

Now startling as is this supposition, and contrary to all that we should have anticipated, we cannot say that it must be rejected. For we must remember that we have no direct evidence even of the existence of an ether; it is not directly recognizable by any of our senses; its properties may be, and doubtless are,

very different from those of ponderable matter, and
we must be content to learn them by degrees, as they
may be revealed by the study of the phenomena
which are referable to actions of the ether. Never-
theless we are not absolutely driven to accept Dr
Young's hypothesis; for there is as I have shown
another way in which the law of aberration may be
obtained; a way which though not free from diffi-
culties exempts us from the necessity of supposing
that the earth in its motion through the ether allows
the ether to pass through it with absolute freedom.

At this point it may be well to pause for a
moment and consider the probabilities in favour of
the two hypotheses. The existence of rays and
shadows seems perfectly simple according to the
theory of emissions; as far as we have gone it pre-
sents a serious difficulty on the theory of undulations.
The laws of reflection and refraction leave little
to choose between the two. In one respect indeed
the theory of undulations has the advantage; for it
indicates that there ought to be a partition of the
incident light into a portion reflected and a portion
refracted, whereas on the other we should rather have
expected that the light would have *either* been re-
flected or refracted. This advantage is however of
no great weight, for it would not be difficult to frame
plausible hypotheses in the theory of emission which

would lead to a partition. As regards aberration, the corpuscular theory has a decided advantage, for on it the explanation of the phenomenon is perfectly simple, whereas according to the theory of undulations all we can say is that it is not inexplicable.

The balance on the whole seems to lean towards the side of the corpuscular theory. And yet that theory is now altogether exploded, and the rival theory is established on so firm a basis that no one who has studied the subject can doubt that the second of the two modes of conception with which we started expresses the truth, and that light really consists of a change of state propagated from point to point in a medium existing between the luminous body and that which the light affects.

It may be said, If the former theory is now-a-days exploded, why dwell on it at all? Yet surely the subject is of more than purely historical interest. It teaches lessons for our future guidance in the pursuit of truth. It shows that we are not to expect to evolve the system of nature out of the depths of our inner consciousness, but to follow the painstaking inductive method of studying the phenomena presented to us, and be content gradually to learn new laws and properties of natural objects. It shows that we are not to be disheartened by some preliminary difficulties from giving a patient hearing to a hypo-

thesis of fair promise, assuming of course that those
difficulties are not of the nature of contradictions
between the results of observation or experiment and
conclusions certainly deducible from the hypothesis
on trial. It shows that we are not to attach undue
importance to great names, but to investigate in an
unbiased manner the facts which lie open to our
examination.

I now come to other classes of phenomena with
respect to the explanation of which there is the widest
possible difference between the two hitherto rival
theories respecting the nature of light. We are
all familiar with the vivid colours of soap bubbles,
colours which are exhibited in the case of transparent
solid plates or liquid films which are excessively thin,
or of very thin plates of air contained between two
surfaces of glass which are in contact, or almost in
contact, at one point. It is to Newton that we owe
the first investigation of the laws of these "colours of
thin plates" as they are called. By placing a convex
lens of small curvature in contact with a plane piece
of glass, we obtain a separating plate or film of air
the thickness of which vanishes at the point of contact,
and increases very slowly at first on receding from it.
In this case the colours are arranged in circles round
the point of contact, the rings forming annuli of
increasing diameter but decreasing width as we

recede from the centre outwards. The centre is dark ;
and when the incident light is white, on going out-
wards about seven alternations can be traced, after
which the field is sensibly of uniform illumination
and free from colour. The squares of the diameters
of the rings are found to increase in arithmetic
progression in passing from ring to ring.

When instead of using white light the system of
the rings is illuminated by the colours of a pure
spectrum, a vast number of rings is seen ; in fact, they
go on till from the increasing narrowness of the annuli
they become too fine to be seen. In this case the
colour naturally remains the same, being that of the
part of the spectrum that is used to illuminate the
glasses. The scale of the system changes greatly
with the colour, decreasing from the red to the violet.

The scale of the system depends very greatly
upon the curvature of the lens ; and to form the rings
on a large scale, which is convenient for examination,
it is necessary to use a lens of small curvature. The
thickness of the interposed plate of air where any ring
is formed can be obtained by an easy calculation
from the diameter of the ring and the radius of
curvature of the lens, both which can be measured.
When this is done it is found that a given ring is
always formed where the plate has a given thickness.

If the interposed medium be water instead of air,

or generally any transparent fluid, the scale of the
rings is diminished. It is found that the square of
the diameter of a given ring varies inversely as the
refractive index of the interposed liquid.

As my object is not to give a complete account of
the phenomena of the rings, much less a complete
explanation of the phenomena, I shall dwell no further
on the appearances which the rings present under
different circumstances, the features I have already de-
scribed being sufficient for my purpose, which is to give
a fair idea of the evidence on which rests that theory
as to the nature of light which we shall be led to adopt.

Newton endeavoured to account for the various
phenomena of thin plates by his celebrated theory of
fits of easy reflection and transmission. We have seen
that the existence of both reflection and refraction is a
difficulty in the theory of emission which has in some
way to be accounted for. It will not do to suppose
that there are two permanently distinct kinds of light,
of which one, when the light falls at a given inclination
on a given substance, is always reflected and the other
always transmitted, for if either the reflected or the
transmitted beam be allowed to fall at the same angle
on the same substance, it is divided into a reflected
and a transmitted beam. We must therefore suppose,
even independently of the phenomena of thin plates,
that the same particles of light are sometimes in a

condition to be reflected and sometimes in a condition
to be transmitted : and we have only further to postu-
late that these changes of state, whatever may be
their nature, take place with a regular periodicity in
order apparently to account for the phenomena of
thin plates, at least if we restrict ourselves to the case
of a perpendicular incidence on a plate of given kind,
such as a plate of air between two surfaces of glass.
For all the light which is transmitted by the first
surface must be in a fit of easy transmission which it
does not at once lose, so that if it falls immediately on
the second surface it is transmitted, and therefore the
central spot looks comparatively black. The same
must be the case where the distance between the
surfaces is equal to the length of a fit, or 2, 3, &c.
times the length of a fit; and for intermediate
thicknesses of the plate, corresponding to intermediate
distances from the point of contact, the effect will be
intermediate, and there will be more or less reflection,
which will be greatest for thicknesses exactly half
way between the critical thicknesses above mentioned.

But tempting as this theory at first sight appears,
though if it be true it must be left to subsequent
research to indicate what that periodic element in
relation to the particles of light can possibly be
which shows itself by an alternate capacity for
reflection and refraction, it fails completely to account

for the other features of the phenomenon, some of which I have mentioned. We should have expected beforehand that the length of a fit would have been independent of the angle of incidence ; and when we learn that to reconcile theory and observation we must *suppose* it to vary as the secant of the angle of inci- dence, we see no way of accounting for such a law.

Again, as to the effect of a change of medium, the most natural supposition to make would be that the constant element of periodicity which characterises light of any particular refrangibility is a constant periodic time. Now the explanation of the law of refraction according to the theory of emission requires us to suppose that light travels faster in refracting media than in vacuo, in the ratio of the refractive index to unity. We might have expected accordingly that the length of a fit in water would be greater than in air in that ratio. We have seen however that it is less in the inverse ratio. We are unable to frame any plausible hypothesis, on the theory of emission, why it should be so.

Accordingly the theory, if such it can be called, consists merely of a set of incoherent laws, not indicated beforehand by theory, not even falling in with it after they have been pointed out by obser- vation, and it has accordingly nothing about it which seems to bear the stamp of truth.

Nor is this all. Even as regards the formation of ·the rings at a perpendicular incidence, where at first sight the theory appears to be most successful, it leads to a conclusion which is belied by observation. According to this theory, the office of the first surface of the plate is solely one of sifting, by reflecting back those particles of light which are in a fit of reflection, and thereby preparing the way for the alternate transmission and reflection of the particles at the second surface. There should therefore be as much light reflected at the first surface as if the second glass were away altogether, and therefore the dark rings should be only comparatively dark. The experiment may be easily and successfully tried with homogeneous light, such as that of a spirit-lamp with salt on the wick, and it is found that the dark rings are decidedly darker; in fact as to sense apparently black.

LECTURE II.

Interference—Explanation of the colours of thin plates afforded by the theory of Undulations—Diffraction.

In my last lecture I pointed out the insufficiency of the theory of emission to account for the various phenomena of the colours of thin plates. Let us now see whether the theory of undulations lends itself to an explanation. In the first place the element of periodicity, instead of being wholly extraneous to the fundamental idea, and hardly if at all to be reconciled with it even when suggested by phenomena, is one naturally, almost inevitably, involved in the fundamental conception. I say, *almost* inevitably; for a set of undulations *might* consist of a succession of isolated pulses following one another in a wholly irregular manner; but the analogy of sound would make it far more likely *a priori* that they should form a series of regularly periodic disturbances; and the supposition that such is their character therefore harmonizes perfectly with what we should have expected beforehand.

Supposing then that the undulations we have to deal with are regularly periodic, can we on the undulatory theory give any account of the alternations of light and darkness which we observe in Newton's rings?

The explanation which this theory affords is based on a very simple and very general dynamical principle, of very wide application, called the principle of the superposition of small motions. For the sake of those who may not have much attended to dynamics, I will endeavour to give some idea of what this principle means.

Suppose a stone thrown into still water. It produces as we know a series of small waves, which spread out in the form of circles from the place of disturbance. What passes outwards is, not the material particles of the water, but a certain state of things. If we observe a minute floating body, it is seen to retain its average position, and merely to move very slightly backwards and forwards, up and down. So far, we have merely a visible illustration of the progress of an undulation, differing it is true from those belonging to sound or light in the fact that it is only in the neighbourhood of a particular surface that the disturbance is sensible, which does not however prevent it from forming a useful illustration for those to whom the conception may be new.

But now instead of a single stone, suppose that

there are two similar stones thrown in simultaneously
at a little distance apart. Each will give rise to a
series of circular waves which at first will be distinct,
each diverging from its own centre, but not yet
having reached one another. But presently the waves
from the one centre will invade the region occupied
by those from the other. What will then take place?
According to theory, each disturbance will find the
mass in which it is being propagated under as nearly
as possible the same conditions, so far as itself is
concerned, as if the other disturbance did not exist.
The consequence is that any particle of the mass will
be as much displaced from its mean position as if
the other disturbance did not exist, and its actual
place will therefore be found by compounding, as it
is called, the displacements due to the two dis-
turbances taken separately. Accordingly where a
ridge due to one of the disturbances coincides in
position with a ridge due to the other, we have an
elevation of double height, supposing we choose a
place where the intensities of the disturbances due to
the two sources are the same ; but where a ridge due
to one falls in with a trough due to the other, we
have neither elevation nor depression, but the water
is found at its natural level.

Now if there be any truth in the theory of undula-
tions, something of the same kind must take place

with light. If we have two sets of disturbances always exactly alike, then when the conditions are such that the two disturbances separately considered take place always in opposite directions, being super-posed they simply produce a disturbance *nil*, whereas when the directions agree they produce a disturbance of greater amount than either separately. And if light consist of such a disturbance in the ether, then in the former case we ought to have no light, whereas in the latter the light ought to be greater than if the one set of disturbances alone existed. And if the two sets of disturbances, while still in other respects alike, differed in amplitude in a constant ratio, the only difference that would make in the result would be that the light would not vanish at the minima.

But it must be particularly remarked that in order that this neutralization should always take place in the same way at the same place, it is essential that the two sets of disturbances should always agree as to their times of starting, or differ, if they do differ, by a perfectly constant quantity. For if one were half an undulation ahead of the other, that would make all the difference whether the two disturbances strengthened or neutralized each other: and if the relative times of starting varied irregularly a vast number of times in a second, we should have at a given place neutralization and cooperation succeeding

one another so rapidly that nothing but the mean effect would be perceived, and that, as may be shown, would be simply the sum of the mean effects taken separately. Now if the disturbances came from two independent sources, such as two different portions of a flame, the relative starting points of the two would be purely casual, and no fixed and permanent neutralization would be to be expected. And observation shows that none of those alternations of light and shade which on the theory of undulations we refer to interference are manifested unless the two interfering streams of light have come originally from the same source, having subsequently pursued slightly different paths.

Let us now see how these principles apply to the explanation of Newton's rings. If we consider a small portion of the thin plate of air by reflection from which they are seen, we perceive that there are two reflecting surfaces near one another, and consequently two reflected streams, one reflected from the upper surface of the plate of air, that is, internally in the upper glass, and the other reflected from the under surface of the plate of air, that is, from the upper surface of the under glass. These are it is true accompanied by other streams which have been reflected backwards and forwards internally in the plate, so that the total number of reflections in these

streams is 3, 5, 7, 9...; but these streams, being usually comparatively weak, may in a general explanation be left out of consideration. Now the second of these streams has had to travel a very little further than the first. In the simplest case, to which we may confine ourselves for the present, that of a perpendicular incidence, the excess of length of path of the second stream is evidently just double the thickness of the plate of air, and varies accordingly from point to point of the field of view, the thickness increasing slowly at first and afterwards rapidly as we recede from the point where the lenses are in contact. Close to the point of contact, the second stream is only imperceptibly behind the first, and the two might be expected to be in accordance, that is, to agitate the ether in the same direction, and therefore to strengthen one another. As we take a point further outwards from the point of contact, the retardation increases, and when it becomes half the length of a wave the two would be in opposition, that is, one would agitate the ether in a way just opposite to the other, and therefore, supposing the intensities of the two the same, as would nearly if not exactly be the case, they would neutralize each other, and darkness would be the result. And as the thickness of the plate of air between the two glasses varies as the square of the distance from the point of contact, the

law of increase of the rings for any one kind of light
as the order of the ring increases follows at once, and
moreover the law that under different circumstances
as to curvature of the glasses the same ring is always
formed where the thickness of the plate is the same.

But there is one marked point of contrast between
the results of theory, so far as I have yet explained it,
and observation, namely, that we have been led to
expect a maximum of brightness where the paths of
the two streams are the same, that is close to the
point of contact, or where they differ by one, two, or
a complete number of undulations, and a maximum
of darkness if not absolute blackness where the
difference is an odd number of half undulations.
This would give the places of light and shade exactly
reversed as compared with observation, which would
show one of two things, either that the theory must
be rejected, or that some circumstance has been over-
looked which, had it been taken into account, would
have made the theoretical result right in this respect.

Now the reflections which the two streams re-
spectively undergo take place under very different
circumstances. The first stream is reflected internally
in glass when the light, which had previously been
travelling in the glass of the upper lens, arrives at the
under surface of the glass, or upper surface of the
interposed plate of air, the second is reflected back

into air at the upper surface of the under lens. As
the two reflections take place under different, in some
respects opposite, circumstances, there seems nothing
unlikely *a priori* in the supposition that the signs of
the reflected vibration should be opposite in the two
cases. Dynamical analogies are not far to seek; for
example, we know that when sound travelling along
a pipe meets a closed end, it is reflected in such a
manner that condensation in the reflected answers to
condensation in the incident, and rarefaction to rare-
faction; but when it is reflected, as reflected it is,
from the open end of a tube, rarefaction answers to
condensation and condensation to rarefaction. And
this theoretical conclusion as to the opposition of
signs in the two cases, so probable from analogy, is
converted into a certainty by the application of a
simple dynamical law of great generality, which may
be called the law of reversion. Hence then the
occurrence and features of the rings at a perpendicu-
lar incidence, for any one kind of light, are on the
undulatory theory matters of pure prediction.

Next consider the effect of inclination. It follows
from a very easy calculation that the effect of the
thickness of the plate of air at any inclination in
producing retardation in the stream reflected from
the second surface of the plate, relatively to the
stream reflected from the first surface, is the same

as it would be at a perpendicular incidence for a
thickness smaller in the proportion of the cosine of
the inclination of the light while in the plate of air to
unity, or, which is the same thing in the case of a
lens with nearly parallel surfaces for the upper glass,
of the cosine of the angle of incidence on the first
surface to unity. Hence follows at once the simple
law of dilatation of the rings on increasing the angle
of incidence, namely, that the square of the diameter
of any ring varies as the secant of the inclination on
the first surface. Of this law, as we have seen, the
theory of emissions could give no account.

Again, the effect of the substitution of water for
air between the lenses in causing the rings to con-
tract, and the law of their contraction already men-
tioned, follow immediately on this theory from first
principles, since the explanation of refraction on the
theory of undulations necessitates the supposition
that light travels more slowly in refracting media
than in vacuum, in the proportion of the refractive
index to unity.

In short, the theory completely explains the phe-
nomena of the rings seen by reflection. And this is
true even as regards the more minute features, into
which I have refrained from entering. The New-
tonian "fit" at a perpendicular incidence expresses
half the length of a wave; and as the scale of the

rings decreases from the red to the blue, and accord-
ingly as the refrangibility of the light increases, we
learn that the variable element in light which corre-
sponds to a change of refrangibility and change of
colour must be the wave-length *in vacuo*, or, what
comes to nearly the same if not exactly the same
thing, the periodic time. The compound tints of the
rings are explained here as in all other optical phe-
nomena, whatever theory of light we may adopt, by
the superposition of the ring-systems corresponding
to the different kinds of light of which white light
consists, which are on different scales as to size ;
and the fact that not more than about seven rings are
seen with white light merely depends on the over-
lapping of the rings of high orders corresponding to
the different colours.

I have hitherto said nothing about the system of
rings in the transmitted light, which are complemen-
tary in character to the rings seen by reflection, but
far less vivid, resembling in fact a vivid system com-
plementary in character to the reflected system, over-
laid by a comparatively large quantity of uniform
white light. In nearly every respect the theory of
these is analogous to that of the reflected system, so
that whatever theory explains the one can hardly fail
to explain the other. There is just one feature the
explanation of which involves considerations into

which I have not entered, and to this I will confine
myself.

The transmitted system, according to the expla-
nation afforded by the theory of undulations, depends
on the interference of two streams of light, one
passing right through the plate of air comprised
between the lenses, and the other following it after
two reflections in air at the adjacent surfaces of
the glasses. These are associated with other por-
tions which have been reflected 4, 6 ... or any even
number of times; but these, being usually compara-
tively weak, may in a general explanation be disre-
garded.

Now when light is incident perpendicularly, or
nearly so, on the surface of crown glass, about 4 per
cent. of the incident light is reflected, and the rest
enters the glass. The intensity of light once reflected
in this manner being thus only the $\frac{1}{25}$th of the in-
tensity of the original, the intensity of light twice
reflected will be only the $\frac{1}{25}$th of that, or the $\frac{1}{625}$th of
the original. Now as the light which passes through
one surface loses only 4 per cent. by reflection, the
light which passes through the two surfaces of the
plate still retains the 0.96^2 or 0.9216 of its original
intensity; so that the twice reflected light has an
intensity only the $\frac{1}{676}$th of the direct, hardly more
than one-sixth per cent. How then, it might be said,

could so small an addition or subtraction of light be perceptible at all, and so produce in homogeneous light differences of intensity, and with white light changes of colour, which though not it is true by any means so striking as in the reflected system of rings are nevertheless very evident, and demand far greater differences of intensity than that?

The explanation of this paradox lies in a consideration of the relation between intensity and amplitude of vibration. If light consists in a disturbance of a subtile medium, or ether, then the greater be the disturbance, other circumstances being the same, the stronger must be the light. But supposing the amplitude of excursion of the ether to be doubled, trebled... is the intensity of the light doubled, trebled ... or if not, in what other proportion is it increased?

Now a number of different considerations lead decisively and independently to the same conclusion, namely, that the intensity is to be measured by the square of the amplitude of excursion. Hence if the amplitude of excursion to and fro of the ether is increased in the proportion of 1 to 2, 3..., the intensity of the light is increased in the proportion of 1 to 4, 9...

Suppose now that two disturbances from the same source, and following nearly the same path, have a, b for the coefficients of excursion due to them sepa-

rately; when acting together the coefficients of ex-
cursion will fluctuate between the limits $a + b$ and
$a - b$ according as the phases are in agreement or in
opposition. Hence whereas the intensities of the two
separately will be in the ratio of a^2 to b^2, the maximum
and minimum intensities due to the compound dis-
turbance will be proportional to $(a + b)^2$ and $(a - b)^2$.

Suppose now the first disturbance to be very
much greater than the second, then a will be very
much greater than b. Hence whereas the sum or
difference of the intensities of the two streams would
be got by adding or subtracting b^2 to or from a^2, the
maximum and minimum intensities of the compound
disturbance will be got very nearly by adding or sub-
tracting $2ab$, a quantity which is greater than the
former in the proportion of $2a$ to b. Thus with the
numerical values just mentioned, which are applicable
to Newton's rings at a perpendicular incidence, if we
take the coefficient of vibration and the intensity of
the stronger stream each for the unit of their respec-
tive kinds, the coefficient of vibration of the weaker
stream will be $\frac{1}{25}$ and the intensity $\frac{1}{625}$ nearly, where-
as the maximum and minimum intensities of the
compound stream will be $1 \pm \frac{2}{25}$ nearly, and the differ-
ence between them will be as great as $\frac{4}{25}$.

In the particular case of Newton's rings, the
weaker stream is not readily viewed apart; but in

certain experiments of diffraction, a subject that will be touched on by and by, the two streams lend themselves readily to separate observation, and the occurrence of distinct fringes of interference theoretically referable to an invisible agent—invisible because too faint to be seen—is not a little paradoxical in appearance.

It appears then that the fundamental hypotheses of the undulatory theory suffice to account in the most complete manner for the phenomena of Newton's rings, and the colours of thin plates in general, without making any fresh assumption whatsoever. It is to Dr Young that we owe the explanation of these colours on the theory of undulations, an explanation given at a time when any other theory than the corpuscular could hardly gain a hearing ; and to him also we owe the first direct experiment proving that certain fringes which connect themselves by numerical relations with Newton's rings are incontestably due to interference.

As introductory to this experiment, it will be convenient to mention another phenomenon which forces itself upon the notice of the observer simultaneously with that with which we are more immediately concerned, and which therefore it is desirable to be able at once to refer to its proper place, besides that at a more advanced stage of our study of the

subject it will be found to be of very great impor-
tance.

Suppose that the sun's light is reflected hori-
zontally into a darkened room, passing through a
very small hole in the shutter, or what is more con-
venient through a lens of short focus. Let the light
be allowed to fall on an opaque screen at the distance
say of a few feet from the luminous point, the screen
being terminated by a straight edge, suppose vertical.
Let the light passing the edge of the first screen be
received on a white vertical screen which we may
suppose a few feet further off from the luminous
point. According to geometrical optics, if we project
the edge of the opaque screen on to the receiving
screen, by straight lines drawn from the luminous
point, all to the illuminated, suppose the right-hand,
side of the projection we shall have uniform illumina-
tion, the same as if the opaque screen were away,
while all to the left of it we shall have darkness.
According to observation, there is no such abrupt
transition on the receiving screen from uniform dark-
ness to uniform brightness. The illumination in-
creases continuously, though rapidly. The illumi-
nation begins to be sensible before we reach the
geometrical shadow, or projection of the straight
edge, where it is still considerably feebler than the full
illumination at a distance on the right. On going in

a right-hand direction from the geometrical shadow the illumination rapidly increases, and actually becomes considerably greater than the full illumination at a distance. It then decreases to a minimum, increases to a maximum, and so on, the maxima and minima differing less and less, by excess and defect respectively, from the illumination at a distance, and gradually occurring in more rapid succession, till they become insensible. Three such bands can usually be traced before the illumination becomes sensibly uniform. With white light the scale of the bands, and of their distances from the geometrical shadow, changes as usual from colour to colour, decreasing from the red to the blue. The appearance is the same at all distances of the two screens from the luminous point, the scale, merely, of the system varying according to circumstances, and likewise the smallness demanded in the source of light in order that the bands may not be confused. From the constant character of these bands we readily recognise them in experiments in which they appear associated with other phenomena.

Suppose now that instead of the opaque screen extending indefinitely on one side, the light is intercepted by a narrow slip, a knitting-needle, or anything of the kind. The shadow is bounded on both sides externally by fringes of the character of those

just described, and which accordingly we must attri-
bute to the same cause, referring the right-hand set
to the light which passed to the right of the obstacle,
acting independently of that which passed to the left,
and similarly as regards the external fringes of the
left. But besides these the shadow itself is occupied
by another set of coloured fringes, finer usually than
the former, and unlike them of equal width through-
out. They may be viewed through an eye-lens; and
thereby magnified. If the screen be semi-transparent,
we may view them with the lens from behind, and
now if we take away the screen altogether, using
nothing but the lens, and receiving the light directly
into the eye, we see them as before. But now they
are much brighter, since the light is no longer scat-
tered in all directions by a screen, so that we can
afford to use a narrower source of light, suppose a
lens of shorter focus in the window; and having the
narrower source we may use a wider opaque slip
without the fringes getting confused. Under these
circumstances the system of internal fringes occupies
only the middle portion of the shadow, being well
separated from the external fringes on both sides.
The very centre of the shadow is bright for all the
colours of the spectrum, and the middle bright band
is accordingly white, except just at the edges, where
it is slightly reddish, but after that the bands right

and left soon become coloured, on account of the difference of scale for the different colours.

Now what account can we give of the formation of these internal fringes on the theory of undulations? We have seen that when light passes by an opaque screen, it does not at once become insensible within the limits of the geometrical shadow; a little light bends round the edge into the darkness, enough to produce a feeble and rapidly decreasing illumination on a screen placed to receive it. This "inflexion" of light is a fact, account for it as we will: and if light consists of undulations, the analogy of those undulations which we can directly examine would lead us to infer that light must be inflected into a shadow. Indeed the grand original difficulty which for so long a time prevented the reception of the theory of undulations was that of accounting for the comparative absence of inflexion; in other words, for the existence of rays and the sharpness of shadows. Admitting this small inflexion as a fact, we see that at any rate we can not be far wrong in supposing the bending to take place close to the edge of the obstacle. On this supposition, the length of path of the inflected stream in travelling from the luminous point to the point of the field, or focal plane of the eye-lens, where we seek the illumination, will equal the path from the luminous point to the edge of the

S. 4

obstacle, *plus* the distance from thence to the point in
the field. The difference of paths for the two in-
flected streams which reach the same point of the
field is accordingly easily found, especially as we are
only concerned with points lying but a very small
distance from the plane passing through the luminous
point and the middle of the obstacle, or say the
central plane. The difference of path vanishes at the
central plane, where accordingly we have a maximum
of brightness, and on receding in a lateral direction
from the central plane the difference of path changes
in proportion to the lateral distance, and accordingly
it is equal to 1, 2, 3... wave-lengths for a series of
equidistant straight lines parallel to that in which the
central plane cuts the field. These lines are what
ought according to theory to be the middle lines of
bright fringes, and lines midway between them ought
to be the middle lines of dark bands separating the
fringes. The theoretical breadth of a fringe, being
sensibly independent of the distance of the luminous
point, is connected with the wave-length by a very
simple formula, involving only the distance from the
opaque slip to the screen on which the light is received
and the breadth of the slip, both of which can easily be
measured. On the other hand, the fringes are actually
seen, and their breadths can be measured. Com-
paring the theoretical and observed breadth of a

fringe, we obtain the value of the quantity which if the theory be true expresses the length of a wave. On comparing this with the measure obtained from Newton's rings, we obtain identically the same value within the limits of errors of observation.

This numerical relation indicates that the alternation of light and dark in these two different phenomena is referable to a common cause, whatever that may be. Newton endeavoured to explain the rings which go by his name by the theory of fits of easy reflection and transmission. We have seen that this theory failed to account for the dilatation of the rings produced by increasing the angle of incidence, and for their contraction produced by the substitution of water for air as the interposed medium. Still, it gives correctly the law of increase of the radii of the rings at a perpendicular incidence as the order increases, and the law connecting the scale with the curvatures of the lenses. Now confining ourselves to the case of a perpendicular incidence, where the theory of fits is most successful, we may notice one radical difference between the explanations offered by the theory of fits and the theory of undulations. According to the former, the office of the first surface of the thin plate is simply one of sifting, and it is by reflection or non-reflection from the second surface that the rings are formed. According to the theory of undulations on

4—2

the other hand, there is no sifting at all : the light
reflected from and the light transmitted by the first
surface have both one and the other just the same
properties as the incident light, and it is by the
simultaneous working of the light reflected from the
upper and that reflected from the under surface that
the alternations of illumination are produced. Now
in the experiment of the internal fringes, there is
absolutely nothing to sift the light, and yet we get
alternations of light and darkness as in Newton's
rings, and what is more are conducted to the very
same measurable length, clearly representing some-
thing inherent in the nature of light, which according
to the theory of undulations is simply the length of a
wave, the conception of which is radically involved
in the fundamental points of the theory. We can
hardly refuse to admit that the alternations of light
and dark witnessed in these two phenomena are
really due to interference; to the simultaneous
working of two portions of light.

This conclusion was converted we may say into a
certainty by a celebrated experiment of Dr Young's
on the internal fringes. He showed that if a small
opaque screen were placed so as to intercept the light
going to fall on one side of the narrow slip, or else
which had already passed it, in either case the central
fringes disappeared as well as the external fringes on

the same side of the slip.   And if a plate of glass
were substituted for the opaque screen the internal
fringes disappeared as before, but now the external
fringes were seen on both sides.   It is clear therefore
that the internal fringes are really due to the joint
working of the two portions of light inflected in
passing the two edges, while as regards the external,
those which appear on the right and left are inde-
pendently produced by the light which passes on the
right and left respectively of the opaque slip.   The
disappearance of the internal fringes occasioned by
the interposition of a piece of glass on one side only
of the opaque slip is explained in the same way as
the non-exhibition of Newton's rings, when white
light is used, outside a very moderate number sur-
rounding the point of contact of the glasses.

After what precedes, the reality of the interference
of light might well be taken as established.   It is to
be noted however that in the theory of the last
experiment we have had to take for granted the in-
flexion of light.   It is true that the fundamental con-
ceptions of the theory of undulations lead us to expect
inflexion, the difficulty being rather to explain how there
is so little.   We have not however as yet seen how
the determination of the inflexion is to be brought
within the domain of theory, and to that extent we
have been working in a field not yet fully explored.

It is to Fresnel that we owe the first experiment of interference in which there is neither a thin plate, the first surface of which might according to Newton's views have exercised a sifting action on the light, nor anything unusual, such for example as inflexion, occurring in the progress of the light, but in which the two portions of light that interfere are simply regularly reflected or refracted as the case may be.

This object was accomplished by Fresnel in two ways, by reflection and by refraction : by a pair of interference mirrors, and by an interference prism, which is a prism with a very obtuse angle, such as 179^0. It will be sufficient to mention the former method.

Let two plane mirrors be procured, such as two pieces of plate glass, blackened on the back, each mirror being bounded by a straight edge. Let them be mounted so that the straight edges are close together, and the planes of the mirrors are nearly but not quite continuations of each other ; the planes of the faces making with each other a very obtuse angle, suppose only a fraction of a degree less than 180^0, the concavity being on the side of the reflecting faces. Great care must be taken that neither mirror juts out above the other where they meet along the straight edges, which may be ascertained by passing the finger lightly across the junction. Let the sun's rays be reflected into a darkened room, and brought

to a point, or what may be regarded as such, by a lens of short focus.   Let the light proceeding from the luminous point be received, say at the distance of a few feet, on the pair of mirrors.   According to geo- metrical optics, the light reflected from each mirror will proceed after reflection as if it came from the virtual image of the point in the mirror.   If each mirror be projected into space by lines drawn from the virtual image belonging to it, we see that there is a narrow wedge of space within which both reflected streams mix.   If the light be received on a screen there will be two illuminated areas corresponding to these two projections respectively, and a narrow more highly illuminated band where the two overlap, cor- responding in fact to a section, by the plane of the screen, of the wedge above spoken of.   If this doubly bright portion of the field be more carefully scru- tinized, by removing the screen and receiving the light directly into the eye through an eye-lens, we see the field marked on both sides by the usual external fringes at the boundary of an illuminated field.   But besides these we see, running parallel to these along the middle of the field, a set of sharply defined, and commonly much narrower fringes, which as usual are on a different scale for the different colours, coarser for the red, finer for the blue.   These are quite different in appearance from the compara-

tively vague external fringes which are seen at both sides of the field. And if any suspicion were entertained that after all these sharply defined fringes, said to be due to the interference of two regularly reflected streams, were really connected with the external fringes, their complete independence would be shovn by a very simple modification of the experimeit. Adjust the mirrors so that one shall jut out a very little above the other towards one end of the line of junction, and the other towards the other, this will be sufficient to make the line of intersection of the planes of the mirrors altogether oblique to the line of junction. The region of space within which the two streams mix remains as before, and the external fringes which bound it on the two sides, but the sharply defined fringes which before ran along the middle of the doubly bright region, being parallel to the line of intersection of the planes of the mirrors, are now altogether oblique to the line of junction, and accordingly inclined at a considerable angle to the external fringes. When the sharply defined fringes begin to run out of the doubly bright portion of the field, they and the external fringes begin to modify one another, but under the conditions which usually prevail, and at any rate are easily obtained, the former pursue for a long way their oblique course quite undisturbed.

It is proved therefore to absolute demonstration that the property of interference is one essentially belonging to light from its very nature. Two lights of the same kind, that is, of the same refrangibility, from the same source do really strengthen or oppose each other, in the latter case producing darkness if the intensities are equal, according as their lengths of path from the source to where they mix are the same or differ by a multiple of a certain length depending on the nature of the light, or as they differ by an odd multiple of half that constant. And if part of the path lies in some refracting medium, as water, instead of air, it is equivalent, so far as interference is concerned, to a path in air greater in the proportion of the refractive index to unity, and which accordingly would take exactly the same time to be travelled over according to the theory of undulations, since according to that theory the velocity of propagation in media is less than in vacuo in the proportion of unity to the refractive index.

This fundamental constant, which according to the undulatory theory expresses the length of a wave, may be determined more or less accurately by any of the instances of interference above mentioned ; though the progress of our knowledge has furnished us with methods of determining it of still greater exactness. It increases as we have seen, and that considerably,

something like in the proportion of 2 to 3, in passing from the blue to the red. As to its absolute value, it will be sufficient to say that for rays of mean refrangibility we may take it in round numbers at the $\frac{1}{50000}$th part of an inch.

We have seen with what admirable simplicity the theory of undulations explains the various phenomena, in all their details, which have been mentioned as referable to interference. And yet the grand original difficulty, that of explaining the existence of rays and shadows, has been left untouched, or received at most only a lame explanation. Yet if the theory be true it ought to be capable of accounting for these phenomena as well as for those, in some respects simpler in character, which have been so successfully referred to interference.

It is remarkable that it was not till the study of the theory of light had made great progress, subsequently to the revival of the undulatory theory about the beginning of the present century, that the elementary phenomena of rays and shadows received their full explanation; and yet, once that explanation is propounded, it is seen to involve nothing more than the very elements of the theory of undulations; to be in fact nothing more than might have been foreseen from the beginning had the human race been sufficiently acute. The history of the

explanation compared with our present knowledge affords a remarkable example of the manner in which we start on our course of investigation by mounting on the shoulders of our predecessors ; and so it may very likely be in the future that things that appear to us mysterious, and which we labour hard to explain, will to our successors seem so simple that they will wonder why we did not find them out.

I have mentioned the external fringes seen on the illuminated side of the geometrical projection of a straight edge bounding an opaque body exposed to light coming from a luminous point, which is taken as the point of projection. This phenomenon is modified in a great variety of ways according to the outline of the opaque body. It may for instance be a screen containing one or more apertures. Suppose the aperture circular. If the circle be moderately large, and the light be received on a screen beyond, we have a circular illuminated patch corresponding nearly with the projection of the hole, which is seen to be fringed within its boundary by fringes resembling the external fringes spoken of just now. If the size of the hole be diminished, or the distance of the receiving screen increased, which produces a similar effect, the fringes invade what had previously been the uniformly illuminated area corresponding to the projection of those portions of the hole which are at a little distance

from its edge; and after a very curious and complicated set of changes we are ultimately, when the hole is made very small, left with a circular patch of rather weak light on the screen, surrounded by a dark ring, followed by other rings alternately bright and dark, but of rapidly decreasing intensity, till they are lost in the dark shadow of the screen in which the hole is pierced. The central bright patch is much larger than the geometrical projection of the hole.

The continuity of the phenomenon connects the diffusion of the light which passes through the hole in the last case, and the comparative absence of diffusion in the first case, with the formation of coloured fringes and alternations of intensity about the boundary of shadows in general, and with the external fringes belonging to a straight edge in particular, and makes it probable that if we could explain these last the principles of the explanation would enable us also to explain the existence of rays, at least in so far as rays have a real existence at all.

Can we then explain the external fringes in the simple case of a straight edge? Newton made careful observations of the phenomenon; but guided as he was by ideas belonging to the corpuscular theory of light, he advanced no further towards an explanation than a few vague conjectures. Dr. Young, guided by the theory of undulations, was more successful. He

attributed these fringes to the interference of two
portions of light, one coming direct from the luminous
point, and one reflected at a grazing incidence at the
edge of the opaque screen, losing half an undulation
at the reflection.   This theory explained very well
the leading features of the fringes, showing them to
be hyperbolic in form, that is to say that a section of
the fringes, conceived as existing in space, made by a
plane passing through the luminous point and perpen-
dicular to the edge, is a system of hyperbolas having
for their common transverse axis the line joining the
luminous point with the edge, and having small con-
jugate axes, differing from one fringe to another of
the system.   It explained also the decreasing width
of the fringes as we recede from the shadow, and their
dilatation when the luminous point approaches nearer
to the opaque screen from the edge of which they
start.   It even gives very nearly the breadths of the
fringes and their distances from the geometrical
shadow.

Accordingly when Fresnel, many years later,
commenced his celebrated researches on diffraction, he
in the first instance adopted Young's theory as to the
cause of the formation of the external fringes.   In the
course however of his study he met with phenomena
of diffraction which did not fall in with Young's view,
and which at last opened his eyes to perceive the

grand principle which underlies the whole. We have seen that Huygens successfully explained the laws of reflection and refraction on the undulatory theory by introducing the principle that each element of the front of a wave may be regarded as the source of an elementary disturbance, and these disturbances must then be joined together. Now we have only to combine that principle with this other principle, that in so compounding them we must take due account of their respective phases, in order to account for the whole of the phenomena of diffraction, curious and complicated as they are. In other words, we have only to combine Huygens's principle with the principle of interference. These two principles again are nothing more than special applications of the general dynamical principle of the superposition of small motions; a principle which lies at the very basis of the theory of undulations, and of which the special applications just mentioned might have been foreseen.

The application of this principle to special cases, among others to the case of the external fringes, involves calculations of considerable complexity. Fresnel executed these calculations for the external fringes, and also made a series of most careful and accurate measurements of the positions of the fringes referred to the geometrical shadow under a variety of

circumstances.   The theoretical distances of the
several fringes from the geometrical shadow were a
matter of pure prediction ; for the only unknown
quantity involved in the theoretical expression, the
length of a wave, had been determined by Fresnel by
independent methods, some of them, as for example
that depending on the measurement of the fringes
produced by interference mirrors, not involving
diffraction at all, so that not a single arbitrary
constant was left, to be determined by some one
measurement of a fringe in some one particular case,
whereby an at least partial accordance between theory
and observation might have been brought about.
On the other hand the distances of the fringes from
the geometrical shadow in a variety of cases were
most carefully measured micrometrically, and the
comparison of the calculated and observed places
manifested a truly wonderful accordance, the average
error being only about the $\frac{1}{3000}$th part of an inch.

The distances calculated from the imperfect theory
of Dr. Young agreed nearly, but not exactly, with
those deduced from the complete theory of Fresnel : in
spite of the smallness of the difference, the measure-
ments were sufficient to discriminate between the two,
and the result was decisively in favour of the complete
theory as given by Fresnel.

As the geometrical shadow is not, like the fringes,

a visible object, it may not be superfluous to mention briefly the mode of referring the places of the fringes to the geometrical shadow. The fringes were formed by the cheeks of an aperture with parallel edges, one of which was moveable by a micrometer screw, by which means the breadth of the aperture could be very accurately measured. The cheeks were set a sufficient distance apart to prevent the fringes formed by the one affecting in any sensible way those formed by the other. We thence get from similar triangles, by the rule of three, the distance apart of the geometrical shadows at the focal plane of the eye-lens. Now the distance of any particular fringe, say the first dark fringe, on the right from the same fringe on the left can be measured by a micrometer moveable in the focal plane of the eye-lens. Half the excess of distance between the geometrical shadows in the focal plane over this gives the distance of the first minimum from the geometrical shadow.

The phenomena of diffraction may be varied indefinitely by varying the outline of the opaque body, or aperture or apertures pierced in an opaque screen, as well as the two distances concerned; and there is a large class of interesting appearances which may be seen by using a telescope with which a luminous point is viewed in focus, and covering the object-glass with a screen containing one or more apertures of any form

that may be chosen. In this case especially most curious and beautiful coloured patterns are produced, so strange and complicated that a person looking at them for the first time could never guess from the pattern what was the form of the aperture which produced it.

Besides the case of the internal fringes which was so carefully examined by Fresnel, a number of other instances in both classes, that is, without and with a lens or object-glass combined with the diffracting body, have been investigated theoretically, and the results compared with observation. The accordance is found to be absolutely complete even in the most minute particulars.

And now at last, as part and parcel of the complete theory of diffraction, we are able to explain the existence of rays; to show why it is that it is so nearly true that light proceeds in a straight course past bodies and through apertures.

The explanation may be given without entering into mathematical details. Suppose light coming from a luminous point which for simplicity's sake we may suppose to be at a practically infinite distance, an element of the sun's disk for example. Let it fall on a screen in which is a moderately small aperture, and consider the disturbance produced beyond the screen at a point, P, well outside the projection of the

aperture. Make P the centre of a set of concentric spheres with radii increasing by half the length of a wave. These will cut the plane of the aperture in a series of circular arcs, very close to one another in consequence of the extreme smallness of the wave length, and comprising between any two consecutive circles narrow slips of the aperture, such that two adjacent slips are very nearly equal in area. Now we have a right to regard each element of each slip as the source of an elementary disturbance, which reaches P after the lapse of a time proportional to its distance from P. Since corresponding points in consecutive slips differ by half a wave's length in their distance from P, and the elementary disturbances from them accordingly always reach P in opposite phases, so as to neutralize each other, we easily see that the total effect of one slip is very nearly indeed neutralized by that of its neighbour. This is still more nearly true if we take the disturbance produced by one slip and the mean of those produced by its two neighbours ; and in this way, by taking each alternate slip and the mean of its two neighbours, each slip gets counted once, and once only, except at the two ends, where, however, the length of the slips dwindles away to nothing. Hence there is no sensible disturbance, and therefore no sensible light, for a point P situated as we have supposed.

If now we take a point P situated well inside the projection of the boundary of the aperture, it may be shown by similar reasoning that the disturbance, and therefore the illumination, is sensibly the same as if the screen in which the aperture is pierced were away.

This conclusion rests, it will be seen, on the extreme smallness of the length of a wave, in consequence of which an aperture, unless extremely small, is cut a great number of times by a series of concentric spheres with radii increasing by half a wave's length.   There is no difference of explanation as regards light and as regards sound, save what depends on the difference of scale entailed by the difference of wave length.   Take as regards light the case of a small circular hole say the tenth of an inch in diameter, and of distances from the luminous point to the screen in which the hole is pierced, and from that again to the screen on which the light is received, of say 8 feet 4 inches, or 100 inches, each.   In this case, regarding the luminous patch on the screen as a whole, there would be no great diffusion of light, but the phenomena of diffraction would nevertheless be fairly pronounced.   There ought to be a corresponding case of diffraction for sound; but on what scale?   Take 50 inches as the length of a wave of sound, which would correspond to a musical note of moderate pitch.   Taking as before the $\frac{1}{50000}$th

part of an inch as the wave length for light, the length of the wave of sound will be two and a half million times as great as the wave length of light. Consequently to obtain the corresponding case of diffraction for sound, our "small" circular hole would be obliged to have a diameter of rather more than 4 miles, say 4 miles, and the distances from the source of sound to the hole through which it passes, and from that again to the place where the sound is listened to, would have to be 4000 miles each.

It is remarkable that the existence of rays, which formed the great stumbling-block in the way of the early reception of the theory of undulations, is now shown to belong to a class of phenomena, those of diffraction, the complete and marvellously simple explanation of which afforded by the theory of undulations now forms one of the great strongholds of that theory.

Before leaving this subject I will briefly mention two or three instances of diffraction which from their paradoxical character or their importance are deserving of notice.

Reverting to the case in which light from a luminous point passes through a circular aperture, and is received on a screen beyond, consider the brightness at a point just in the axis on the receiving screen. If the relation between the two distances already so

often mentioned and the diameter of the hole be such that the sum of the distances from the luminous point to the edge of the hole and from thence to the central point on the receiving screen exceeds the direct distance from the luminous point to the latter by just half a wave's length, theory shows that the illumination is actually four times as great as if the screen in which the hole is pierced were taken away altogether. Suppose now the hole be enlarged. We might have said at first sight, supposing we were ignorant of the theory, " Of course that must increase the illumination at the central point, or at any rate cannot diminish it." On the contrary it does diminish it; and if the hole be enlarged till its area is just double what it was in the first instance, the centre of the illuminated space on the screen is a black spot. This theoretical result is easily realized in experiment; only as the wave length varies from colour to colour, and the proper distance of the receiving screen varies with the wave length, when the screen is in adjustment for the brightest part of the spectrum it is not quite in adjustment for the fainter ends, so that the spot instead of being perfectly black is faintly purple.

Again, suppose a circular disk is exposed to radiation from a luminous point, and the shadow is received on a screen at some distance, or rather viewed directly through an eye-lens. According to

theory, the very centre of the shadow will be a bright point, as bright as if the disk were away. This strange result, again, can easily be verified experimentally, easily at least if we are not too ambitious as to the size of the disk; for the delicacy of the experiment increases with the size of the disk, and at the same time the total quantity of light that we have to work with decreases. In repeating the experiment I have seen without difficulty the central spot, with the system of rings round it well formed, in the centre of the shadow of a disk of about the size of a sovereign.

Among the class of diffraction phenomena in which a luminous point or line is viewed in focus through a telescope, and a screen with one or more apertures is placed in front of the object-glass, there is one case of very special interest. It is that in which a line of light is used, suppose an extremely narrow slit through which the sun's light is reflected horizontally, and a fine carefully ruled grating is placed opposite to the object-glass, the lines of the grating being parallel to the slit. The best results are obtained with gratings consisting of glass, or sometimes metal, on which fine parallel lines are ruled with a diamond point. It is requisite that the lines should be very accurately equidistant, and in fine gratings they are so close that several thousand

go to an inch.   If the grating thus constructed be ·
of metal, it can only of course be used for reflec-
tion.

Now on viewing through the telescope the light
transmitted through or reflected from such a grating,
a most remarkable appearance is presented. The
luminous line is seen through or by reflection from
the grating as if the ruled lines were away, and right
and left of it for some way the field is dark. But
then on both sides we get *pure* spectra, the blue ends
being nearest to the axis.   These are followed by a
second set of spectra, the blue ends of which overlap
the red ends of the former, and so on, the spectra as
we proceed overlapping one another more and more.
So pure are these spectra if the grating be a good one,
that they show the fixed lines of Fraunhofer to per-
fection.

The formation of these pure spectra can easily be
explained, and the formula expressing the deviation
for light of any one kind in the spectrum of any
order in terms of the interval of the grating, the
angle of incidence, and the wave length for that kind
of light obtained, from the general principles of the
theory of undulations.   But by mounting the grating
in the axis of a horizontal graduated circle which
carries the telescope, the deviations can be measured
with extreme precision.   The interval of the grating

is got by measuring the breadth of the ruled space, and dividing by the number of ruled lines less one. We thus have the means of determining the wave lengths, if the theory be true, for as many definite kinds of light marked by definite lines as we please; and by comparing the wave lengths measured by means of spectra of different orders, at different inclinations of the incident light to the grating, and by different gratings, we have a very sharp test of the truth of the formula deduced from theory. The accordance is complete; and that being so we are justified in the interpretation assigned to that measurable quantity which we call a wave's length, and obtain its value with great precision.

I may here perhaps mention that it has even been proposed to take the length of a wave of light of some particular kind, such for example as that belonging to one component of the double yellow line of a soda flame, as a natural standard to which a national standard of length might be referred in case of loss. The French refer their metre to the dimensions of the earth. The English refer their yard to the length of the seconds' pendulum. But supposing the earth to be slowly contracting by cooling, both these natural standards would be liable to be affected in the course of ages; and if such a catastrophe were to occur as the impact on the earth of some great

globe visiting our solar system, the dimensions of the earth and the value of gravity, and accordingly the length of the seconds' pendulum, would at once be affected to an unknown degree.   But the wave length of light of a given kind would remain unchanged, and the survivors of such a catastrophe might have recourse to it to recover the ancient standard of length.

LECTURE III.

Closer examination of the fundamental suppositions of the Theory of Undulations—Survey of the conclusions arrived at by a study of the phenomena of common light— Elementary facts of double refraction and polarization.

IT has been my aim in these lectures to endeavour to give you some definite idea of the evidence on the strength of which we assert that light consists in undulations propagated in a medium filling the interstellar spaces. To enable you to judge more fairly of the evidence, I have attempted to present the subject in an inductive rather than in a deductive form. Instead of starting with a number of hypotheses, originating you would not know how, and then showing how the conclusions following from them are in accordance with observed results, I have commenced with only the most fundamental conceptions of undulations, and of the conditions which we must suppose to obtain in order that they may exist, and have afterwards supplemented our original rather crude conceptions in the manner which a study of the phenomena showed to

be necessary. We have seen that not merely are the laws of reflection and refraction in agreement with the theory, but the curious and complicated phenomena of interference and diffraction are explained by it to the minutest particular. What strikes one most about the theory is what has been truly described as its *astounding simplicity*. This so carries on the face of it the stamp of truth, that to one who is familiar with the phenomena as well as the theory, an overwhelming conviction is produced that it is indeed true to nature. That being the case, it may be well now to examine the various hypotheses which must be made in some greater detail than has hitherto been done.

The fundamental hypothesis of the existence of a medium to which we give the name of ether, I have already mentioned. To account for undulations in this medium, we must attach to it the two radical conceptions of inertia and elasticity. First, a finite time must be required in order to generate in a finite portion of it a finite velocity by the action of a finite force. Secondly, a change of condition of some kind in the ether consequent on a relative displacement of its parts must call into play a force of restitution tending to restore it to its primitive condition. Thus in air condensations and rarefactions produce respectively an increase and a diminution of pressure,

so that any small portion of the air which has been
contracted or expanded tends to push out the sur-
rounding parts, or to be compressed by them, and so
to return to its primitive state ; and it is to the forces
of restitution thus called into play that the propaga-
tion of sound is due.   Naturally therefore those who
adopted the undulatory theory of light were led to
imagine the ether as possessing a similar kind of
elasticity.   There is however a whole class of phe-
nomena which I have not yet so much as named, and
which have no counterpart in sound, the study of
which has led us to conclude that the elasticity of
the ether is of an altogether different nature from
that of air.   The question of the relation between the
direction of vibration of the particles of ether and the
direction of propagation of the light is bound up
with that of the character of the elasticity by virtue
of which the undulations are propagated.   In the
explanation, however, of the ordinary phenomena of
interference and diffraction, we are not concerned
with the direction of vibration ; in interference for
instance, where we always have two streams of light
from the same source pursuing nearly the same paths,
and coming together either in the same direction, or
in directions very slightly inclined to each other,
whatever be the direction of vibration in one of the
interfering streams, the same, or very nearly indeed

the same, will be the direction in the other, and that is all that we are concerned with in the explanation. And the explanation of the ordinary phenomena of diffraction has, as we have seen, been resolved into the interference, not of two, but of an infinite number of infinitely small disturbances all coming originally from the same source, and following very nearly the same paths.

The mode of excitement of the undulations in the first instance is in some respects analogous to the mode of excitement of sound by a vibrating body, such as a bell, but in some respects distinctly different. A bell in exciting vibrations in the surrounding air acts as a continuous elastic body ; in explaining the mechanical action, we have nothing to do with speculations as to the molecular constitution of ponderable matter.   But the fact that the spectra of flames show bright lines depending on the nature of the chemical substances in the flame, shows that in the excitement of the ethereal vibrations we have in many cases, if not in all, to do with the vibrations of the constituent parts of the chemical molecules of which we have reason to believe that ponderable matter consists.

The explanation of refraction on the theory of undulations requires us to suppose that in refracting media, such as glass or water, the vibrations are

propagated more slowly than in what we call vacuum. In fact, theory shows that the sine of the angle of incidence must be to the sine of the angle of refraction in the ratio of the velocity of propagation in vacuo to the velocity of propagation in the medium. The question then arises, When light passes through water or air, what is the vibrating medium ?

At first sight we might say, of course the water or the glass itself. But this supposition will not bear examination. We know too much of the elasticity of water and glass to allow us to explain the result in that way. Water is sometimes spoken of as incompressible, but we know that it yields slightly to a compressive force. The amount of compressibility can be measured by hydrostatic means, and from thence the velocity of propagation of sound in water can be determined by calculation. It comes out about four times as great as that of sound in air, which agrees very well with the experimental measurement of the velocity which has been made in the Lake of Geneva.

Glass, and isotropic elastic solids in general, possess two distinct kinds of elasticity, one, by which they resist compression of volume, the other, by which they resist a distortion not involving alteration of volume, but opposed by the force by which the substance resists the gliding of one part over another,

a gliding which takes place freely in liquids. The two elastic constants of glass have been carefully determined, and from them can be calculated the two velocities of propagation of two kinds of disturbance which a large mass of glass would be capable of transmitting. These come out greater no doubt than, but still comparable with, the velocity of propagation of sound in air, and are therefore almost immeasurably smaller than the velocity required to account for the refraction of light on the supposition that it is the glass itself that vibrates. In fact, the velocity of light in vacuum is nearly 1,000,000 times as great as that of sound in air, and the velocity of light in glass would be equal to its velocity in air divided by the index of refraction of glass, or say about 600,000 times that of sound in air, so that it is altogether of a different order of magnitude.

Again, consider a gas or mixture of gases, such as air, in its relation to Light. Air can be rarefied till we have a near approach to an interstellar vacuum, or again can be condensed till its density becomes comparable with that of a liquid. Yet all through these changes there is a perfectly continuous change in its relations to light. It will not do therefore to say that light is propagated through air in one way, by one sort of mechanism, when the air is very rare, and by another when the air is very dense. But

when air is rare, and makes a near approach to what
we call vacuum, but which we must now conceive as
space filled with the luminiferous ether, it is clear that
it must be by the vibration of the ether that light is
propagated in it. We are led therefore to conclude,
from these considerations again, that when light is
propagated in a gas condensed till its density becomes
comparable with that of a liquid, and accordingly
in liquids also, and in solids like glass, which
behave with respect to light just as liquids, it is
ether existing in the interstices between the molecules
of the bodies, of which the vibrations constitute the
light that passes through them.

We might not perhaps have been disposed in the
first instance to suppose that such a solid material
as glass really had ether pervading it. But we must
beware of applying to the mysterious ether the gross
notions which we get from the study of ponderable
matter. The ether is a substance, if substance it may
be called, respecting the very existence of which our
senses give us no direct information : it is only
through the intellect, by studying the phenomena
which nature presents to us, and finding with what
admirable simplicity those of light are explained by
the supposition of the existence of an ether, that we
become convinced that there is such a thing. We
know that a magnet attracts iron through a piece of

glass ; and yet the magnetic influence is one which we can neither see nor feel : why then should not ether exist within glass, and be capable of vibrating within it?

It may readily be imagined, as more probable than the contrary, that the presence of the ponderable molecules interspersed through the ether, within the region of space which is enclosed by the surface of the glass, may have the effect of altering the velocity of propagation of the ethereal vibrations within it, and very probably diminish it. But what may be the precise mechanism by which this result is brought about we do not know. It is easy to frame plausible hypotheses which would account for the result, but it is quite another matter to establish a theory which will admit of, and which will sustain, cross-questioning in such a variety of ways that we become convinced of its truth.

It follows from Huygens's explanation of the law of refraction, which assumes nothing but what lies at the very foundation of the theory of undulations, that the ratio of the velocity of propagation of light in vacuo to its velocity in a medium, such as glass or water, must be equal to the index of refraction, and must therefore vary from one colour to another, increasing from the red to the violet, sometimes, as in the case of oil of cassia, as much as 6 per cent. and upwards within the limits of the visible spectrum. Now we know that in sound notes of different pitch are propa-

S.   6

gated with the same velocity, as also follows from theory, and it has been suggested that it is a difficulty in the way of the theory of undulations that the case must be so different with light. I cannot say that it appears as such to my own mind; for the case of the vibration of one of two mutually penetrating media, as for example glass and the ether, is so different from any that we have to deal with in the case of sound that we cannot argue from the one phenomenon to the other. If indeed it were established that the velocity of light in a vacuum differs from one colour to another, we should then have to allow that an analogy which might have been expected to hold good between the two phenomena does not really do so. But till very lately the observed phenomena which reveal a finite velocity of light, and the experiments which had been made to determine the velocity directly, all yielded a negative result as to any difference of velocity between one colour and another, so that the difference in the ratio above-mentioned was attributable to a difference of velocity in the refracting medium. I have said "till lately," because in a paper by the late Dr James Young and Professor George Forbes, presented to the Royal Society between two and three years ago, and printed in the Philosophical Transactions, in which the authors have determined experimentally the velocity of light by a method founded on that of Fizeau,

but presenting certain new features, it is mentioned as
a result of the observations that blue light seemed to
travel faster in air than red light. The method
employed is founded on the judgment of the observer
as to the equality of intensity of two points of light, or
artificial stars, seen simultaneously in the field of view.
It is difficult to see any possible source of instrumental
error which could have led to the above conclusion as
to a variation of velocity depending on the colour : on
the other hand, the conclusion rests on the judgment
of the eye of a single observer ; and till the question
has been further tested it would seem to be premature
to regard the difference as established. Should it be
confirmed by further observation, it will then be
necessary to consider whether the circumstance that
the light was propagated in a field of magnetic force,
namely the earth's magnetic force, may have had some-
thing to do with it. However even if the result of
further enquiry should be to show that the different
colours are propagated at different rates in a simple
vacuum free from disturbing influences, all we should
be entitled to say is that the expectation which we were
led to form beforehand from the analogy of sound,
and from the supposition that the forces whereby one
portion of ether acts directly on another are insensible
at finite distances, at distances even comparable with
the $\frac{1}{50000}$th part of an inch, has not been verified.

As regards the mode of perception, while there are analogies between sound and light there are at the same time notable differences.   In sound, the tympanum of the ear is thrown mechanically into vibration, and the nerves of hearing are mechanically affected, as a mechanical disturbance of a point on the surface of the body is made known by the sense of touch. But in light, just as we have seen reason to believe that it is the disturbance of the ultimate molecules, or of their constituent parts, by which the vibratory motion which constitutes light is in the first instance communicated from ponderable matter to the ether, so we have reason to think that when light is absorbed what takes place is that the disturbance of the ether is communicated, not to portions of matter regarded as forming portions of a continuous elastic body, but to the ultimate molecules of which matter consists, or to their constituent parts. It may be that temporary chemical changes are thereby produced in the ultimate filaments of the nerves of the retina, in which case the sense of sight would be more analogous to the sense of taste than to that of touch.

Corresponding theoretically to this difference is the fact that in light we have absolutely nothing answering to the sensation of harmony in sound. When two musical notes have their times of vibration in some simple ratio, we have concord when they are

sounded together; thus the ratio of 2 to 3 gives a fifth, that of 4 to 5 a major third &c., whereas when two notes sounded together have their times of vibration in no simple ratio, we have discord. But when two kinds of light, each of definite refrangibility, and accordingly of definite periodic time, are mixed together, there is no pleasing or disagreeable sensation depending on whether the periodic times are in some simple proportion or not.

And now before passing on to a totally different branch of the subject, the study of which leads us to believe that the properties of this mysterious ether must be very different from what we should have imagined beforehand, it may be well to pause and contemplate for a little the wonders with which our study of the phenomena up to the present point has shown that we are surrounded.

First, we learn to regard the interplanetary and interstellar spaces as no mere void, or empty space passed through by swift messengers in the shape of particles of light conveying information from distant worlds, but as filled with an ever present, all pervading substance, in which the ultimate particles of ponderable matter, including those of our own bodies, are continually as it were bathed, and yet of which our senses give us no direct cognizance.

Secondly, that whatever other important offices

this ether may fulfil, this one at any rate belongs to it, that it forms the medium of visual communication between ourselves and our fellow creatures, between ourselves and the various objects around us, between ourselves and distant worlds.

Thirdly, that this communication is carried on by tremors of some kind propagated through the ether with a velocity so enormous that for all practical purposes of communication on earth it may be deemed instantaneous. In fact, light would travel about seven and a half times round the whole earth in one second. But so rapid are these tremors that many hundreds of millions of millions take place in one second. Notwithstanding therefore the enormous rate of propagation, the lengths of the waves are excessively small, ranging about the $\frac{1}{50000}$th part of an inch.

It has been shown that it is this excessive smallness of wave length which enables light to be propagated so nearly in a straight course, in independent rays. Were it not for this, the formation of sharp images would be impossible. Were the lengths of the waves of light comparable with the lengths of the waves of sound, we should as regards the use of our eyes be nearly in the condition of a man who was all but blind; who could just distinguish light from darkness, or a gleam of red from a gleam of green, and no more. We should be in this condition if the

time of a vibration were anything like so great as the
one hundred millionth part of a second, in which case
the length of a wave of light would be comparable
with that of a wave of sound.

Fourthly, we learn that notwithstanding the
almost inconceivable shortness of the time of vibra-
tion, a variation in this periodic time is nevertheless
recognisable by our senses, and that it is to this cause
it is due that the face of nature does not present
to us simply light and shade, like a photograph, but
that we have that endless variety of colour which we
enjoy.

Fifthly, in the plan of an elastic medium con-
veying small vibrations, we have a mechanism of the
simplest possible kind having for result that rays of
light from objects all around cross each other's paths
in all sorts of ways without any mutual disturbance.

When we survey a varied landscape, each visible
point in it, however minute, may be regarded as an inde-
pendent source of light, from which the light proceeds
in all directions.   True, the objects are not in general
self-luminous ; they are seen by the light of the sun or
of the clouds which they irregularly reflect, but as
regards the behaviour of the pencils which proceed
from them they are as good as self-luminous. Well
then ; from each visible point, however minute, there
enters the eye every second a length of light of

about 186,000 miles, that is, light which would have
travelled that distance had not the eye been there to
catch it, this immense length being filled with undu-
lations of lengths ranging about 50,000 to the inch.
And if the landscape be contemplated by a multitude
of persons, from each visible point in it that vast
length of light, consisting of undulations of such
excessive minuteness, enters the eye of each spec-
tator every second of time; and all these various
streams of light, proceeding in all sorts of directions,
cross each other's paths in all sorts of ways without
the slightest mutual disturbance.

To one previously unacquainted with the subject,
these statements seem like the dreams of an enthu-
siast, or at best the speculations of some wild theorist,
and yet there is nothing in what I have stated
beyond the sober conclusions of scientific investiga-
tion, conclusions supported by an amount of evidence
altogether overwhelming. In saying this it is to be
remembered that the precise mode of disturbance of
the ether has been left an open question.

In studying this subject, one can hardly fail to be
struck with the combination of these two things:—
the importance of the ends, the simplicity of the
means. When I say the importance of the ends, I
use a form of expression which is commonly employed
as expressing design. And yet on that very account

we must be on our guard against too narrow a view. When we consider the subject of vision in its entirety, the construction of the recipient organ as well as the properties of the external agent which affects it, the evidence of design is such, it seems to me, as must to most minds be irresistible. Yet if I may judge of other men's minds by my own, it is rather in the construction of the recipient organ than in the properties of the agent that affects it, that the evidence of design is so strongly perceived. And the reason of this may be that we are here dealing with what more nearly resembles design as we know it in ourselves. Man takes the laws of matter as he finds them; the laws of cohesion, of the conversion of liquid into vapour, of the elasticity of gases and vapours, and so forth; and in subserviency to those laws he constructs a machine, a steam-engine for instance, or whatever it may be; but over the laws themselves he has absolutely no control. Now when we contemplate the structure of the eye, we think of it as an organ performing its functions in subserviency to laws definitely laid down, relating to the agent that acts upon it, laws which are not to be interfered with. We can it is true go but a little way towards explaining how it is that through the intervention of the eye the external agent acts upon the mind. Still, there are *some* steps of the process which we *are* able

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circumstances. The theoretical datum of four
several fringes from the geometrical limit was not a
matter of pure prediction; for the only measured
quantity involved in the theoretical results, the
length of a wave, had been determined by two very
independent methods, some of them a's no matter
that depending on the account of its fringes
produced by interference may be entirely
diffraction at all, so that we may safely
measurement of a fringe's own explanation
whereby an at least particular measurement
and observation might have been kept identical,
On the other hand the distance of its large fringes
the geometrical shadow is a value of ten measured
most carefully measured micron, and the
comparison of the calculated at hand places
manifested a truly wonderful series to accurate
error being only about the sixth part of a cell.

The distances calculated from experiment those
of Dr. Young agreed very little so nicely, with
those deduced from the complete hes of Fresnel; and
spite of the smallness of the effects, the measure-
ments were sufficient to discriminate between two competing
and the result was decisively in favour of the explanation
theory as given by Fresnel.

As the geometrical shadow is in fact in frequent

to follow, and these are sufficient to impress us strongly with the idea of design. The eye is a highly specialized organ, admirably adapted for the important function which it fulfils, but, so far as we can see, of no other use; and this very specialization tends to make the evidence of design simpler and more apparent. But when we come to the properties of the external agent which affects the eye, we begin to get out of our depth. These more nearly resemble those ultimate laws of matter over which man has no control; and to say that they were designed for certain important objects which we perceive to be accomplished in subserviency to them, seems to savour of presumption. It is but a limited insight that we can get into the system of nature; and to take the very case of the luminiferous ether, while as its name implies it is all important as regards vision, the present state of science enables us to say that it serves for one object of still more vital importance; we seem to touch upon another; and there may be others again of which we have no idea.

In the study of those phenomena of light which I have hitherto brought before you, we derived constant assistance from our knowledge of the theory of sound. I now come to a branch of the subject where the theory of sound fails us altogether, to a class of

optical phenomena which have nothing answering to them in sound. I refer to double refraction and polarization.

Of these phenomena, the former of which is so closely related to the latter, double refraction was the first to be discovered. It was in 1669 that Bartholinus published his account of the discovery of a strange and unusual refraction in Iceland spar. The subject was taken up and investigated with the keenest interest by Huygens, whom we must regard as the founder of the theory of undulations. It would not be in accordance with the object which I have had in view in these lectures to enter into details respecting the phenomenon, and I must content myself with mentioning a few of the more salient features.

Iceland spar stands we may say alone among minerals in at the same time possessing powerful double refraction and occurring in large clear crystalline masses. It was this circumstance which led to the discovery. The mineral cleaves very readily in three definite directions, so that a block obtained by cleavage is of the form of a parallelepiped. The three obtuse dihedral angles of this parallelepiped are all exactly equal, and are so turned that two opposite solid angles are contained by three equal obtuse plane angles, while each of the remaining six is contained by one obtuse and two acute. A *direction*

—not, observe, any special line—equally inclined to
the three edges which meet in one of the obtuse solid
angles is called the *axis* of the crystal. The crystal-
line structure, so far at least as it is revealed by the
cleavage planes, or I may add by the natural faces, is
symmetrical with respect to three planes and no
more, each of these being parallel to one of the edges
which meet in an obtuse solid angle, and perpendicu-
lar to the plane of the other two, and accordingly
being parallel to the axis of the crystal. Any one
of these planes is what Huygens called a principal
plane.

If an object near at hand be viewed through such
a block, as for example if the block be laid on a
printed page, two images of the object are seen, of
which one appears more elevated than the other when
both eyes are used, indicating a stronger refraction.
This image is called the ordinary, because it obeys
the ordinary law of refraction. That such is the
case was found by Bartholinus, and confirmed by
Huygens; and the accuracy of the ordinary law as
applicable to this pencil had stood the test of the
most refined measurements carried out by the most
improved modern methods, among which I may
specially mention the measurements made by Pro-
fessor Swan and Mr Glazebrook. But the rays
belonging to the other or *extraordinary*, as it is called,

image must obey some totally different law. If the eye and object be in the principal plane of the block, the extraordinary ray obeys the laws of ordinary refraction so far as this, that the refracted ray lies in the plane of incidence; but whereas in ordinary media there is no refraction at a perpendicular incidence, and at oblique incidences there *is* refraction, which goes on increasing as the angle of incidence increases, in the case we are now considering there *is* refraction at a perpendicular incidence, and at one particular oblique incidence a ray passes through *without* refraction. And if the block be turned round in the plane of its surface, so that the plane through the eye and the object, which I suppose perpendicular to the plane of the surface, is no longer a principal plane of the crystal, the refracted extraordinary ray does not so much as lie in the plane of incidence.

If a distant object be viewed through the block, there is no duplication, so that whether a ray passes through the block as ordinary or as extraordinary, the emergent ray to which it gives rise is parallel to the incident, just as in the case of ordinary refraction. This is true whatever be the inclination of the incident ray to the surface, and whatever be the plane of incidence. It depends however on the parallelism of the surfaces of incidence and emergence; and if these surfaces be inclined to each other, forming a prism,

then the duplication of a distant object is at once perceived. If a slit of light be viewed through such a prism, two spectra are seen, which are unequally deviated, and show in general unequal dispersion.

Now what notion are we to form of the cause of double refraction if we adopt the theory of undulations? According to the fundamental explanation of refraction given by Huygens, as there are two refracted rays, there must be two disturbances propagated within the crystal as the result of an elementary disturbance excited at a point of its surface, and these two must travel with different velocities. They will spread out from the centre of disturbance in two closed surfaces respectively, or it may be a surface with two sheets. For one of these, the velocity of propagation will be the same in all directions, and the surface will therefore be a sphere. This follows from the fact that the ordinary ray obeys the ordinary law of refraction. But as the other obeys some more complicated law, the surface for it must be other than a sphere, and the velocity of propagation must be different in different directions.

It has been already remarked that there are three planes of crystalline symmetry, and these, as might have been expected from the intimate relation of the optical properties to the crystalline structure, are also planes of optical symmetry; that is to say, all the

optical properties are symmetrical with respect to each of them. But there are other planes which are planes of optical, though not of crystalline, symmetry. Thus if a plate be cut perpendicular to the axis, the optical properties in it are symmetrical with respect to any plane through the axis; and if a plate be cut parallel to the axis, the plane perpendicular to the axis is a plane of optical symmetry as well as the plane passing through the axis, though the former cannot be, and the second is not in general, a plane of crystalline symmetry. In short, all the optical properties are symmetrical about the axis, the two poles of which are alike, so that the optical properties present the same degree of symmetry as an ellipsoid of revolution*. Accordingly the wave surface relating to the extraordinary ray must have thus much symmetry; and Huygens assumed for trial that it was a spheroid of revolution. As far as he could make out, the refraction in the direction of the axis appeared to be the same for the two rays, and he accordingly supposed that the sphere belonging to the ordinary ray, and the spheroid which he assumed

* According to Sir David Brewster, this is not altogether the case so far as relates to the properties of the light reflected from an artificial surface of the crystal. His observations appear never to have been published in detail, nor has anyone else, so far as I know, taken up the subject.

as the form for the extraordinary, touched one another in the axis. This equality of refraction along the axis we now know to be rigorously exact. The form of the wave surface being assumed, the refraction of the extraordinary ray followed at once from Huygens's construction; and the mode and amount of refraction were found to agree with the construction as near as the most accurate measurements made by Huygens could decide. Mr Glazebrook has recently executed a series of measurements of the refraction of the extraordinary ray in Iceland spar with all the exactitude of modern methods, guided by our increased knowledge of the subject; and the result is that no certain error of Huygens's construction can be detected.

So far however the laws of the extraordinary refraction in Iceland spar are merely empirical, based upon a happy guess as to the form of the extraordinary wave surface; it remains to be explained, if explain it we can, why there should be these two kinds of disturbance at all within the crystal, and why the form of the wave surface should be what we find it must be if the most fundamental principles of the wave theory are true, by which the form of the surface is connected with the observed refraction.

Huygens imagined that the ordinary ray was propagated by the vibration of the ether within the

crystal, while in the extraordinary the molecules of
the crystal took part as well as the ether. I have
already mentioned some of the strong objections
which exist to the supposition that the propagation
of light in media such as water or glass takes place
by vibrations of the ponderable matter; and similar
objections in good measure apply to the supposition
that both the molecules and the ether take part in it.
But be that as it may, Huygens himself, when he had
nearly concluded his researches, discovered a remark-
able phenomenon which ill accords with the supposi-
tion of his which I have just mentioned.

Suppose a second block of Iceland spar to be
placed on top of the first, so that the two are in the
same relative position which they would occupy if
they formed one larger block. Then neither of the
images, ordinary or extraordinary, seen through the
first block is split into two in passing through the
second block, but the ordinary of the first furnishes an
ordinary in the second, and nothing more, and simi-
larly the extraordinary an extraordinary. The same
is still the case if the second block be separated from
the first, or even inclined by turning in the principal
plane. But reverting for simplicity's sake to the first
relative position of the blocks, suppose that the second
is turned round the common normal to the two
adjacent surfaces. The moment the second block is

S.   7

turned from the primitive position, each of the images
which the first block furnishes is split into two by
the second block. The relative positions of the two
images forming the pair into which either of the
original images is split is just the same as if that
original image had been formed by common light,
but the intensities are different. When the second
block has been turned through only a small angle,
the ordinary of the first furnishes mainly an ordinary
in the second, but also a faint extraordinary, and
similarly the extraordinary of the first furnishes main-
ly an extraordinary in the second, but also a faint
ordinary. As the turning goes on, the faint images
get brighter, and the bright images get weaker, till
after turning through 45° the four are alike. As the
rotation continues, the pair of images that had been
the weaker get the brighter, and the pair that had
been the brighter get the fainter, till after turning
through 90° the pair of images that had at first
appropriated the whole of the light disappear alto-
gether, and the pair that began to spring into exis-
tence on first turning now alone are seen. In this
position, that is to say when the principal planes of
the two blocks are perpendicular to each other, the
image formed by light which suffered ordinary refrac-
tion in the first block furnishes nothing but an extra-
ordinary in the second, and the image formed by

light which suffered extraordinary refraction in the first furnishes nothing but an ordinary in the second. As the rotation goes on, the same series of changes are repeated, so that in a complete revolution the two pairs of images vanish alternately at every quarter of a revolution.

On account of the fundamental importance of this phenomenon, I must crave your indulgence for dwelling on it at some length, even though from its elementary character it must be familiar to those of you who have paid any attention to this branch of the subject.

Suppose that light coming directly from a luminous source, such as the flame of a lamp, is received on a screen with a circular hole. The screen will isolate a beam of light which, as I shall have occasion to deal with it only at short distances from the hole, I will call cylindrical. Let this beam be received, suppose perpendicularly, upon a block of Iceland spar bounded as usual by cleavage planes. In passing through the spar, it will be divided into two, an ordinary beam, which will pass straight on, and an extraordinary which will be deviated in a lateral direction in passing through the block, and will give rise to an emergent beam parallel to the incident, and accordingly parallel to the first emergent beam. If the hole in the screen be not too large to suit the thickness of the block, the two beams will

come out without overlapping, and may be examined
apart. They are found to be of sensibly equal in-
tensity whatever be the azimuth of the block around
its normal. Making abstraction of the small quantity
of light which goes elsewhere by reflection, we may
say that the whole of the incident light is divided
equally between the two beams.

Suppose now that we fix the block in any position,
say with its principal plane vertical, and place a second
screen with a circular hole to let pass the beam which
went straight through the first block, while it stops the
other. On examining this beam by a second block,
which is turned round, we find that it is divided into
two of unequal intensity, which vanish alternately at
every quarter of a revolution, the whole of the light
passing into an ordinary beam in the second rhomb
when its principal plane is vertical, and into an
extraordinary when it is horizontal. Hence whereas
a beam of common light is propagated in some
definite direction, but possesses no relation to space
in any other direction, so that there is no plane
passing through it which we can distinguish, merely
by the properties of the beam itself, from any other,
the beam we are now considering, namely that which
passed through the second screen, possesses properties
with respect to directions in space transverse to its
direction of propagation ; and if we knew nothing of

its history, but it were merely presented to us for examination, that would not hinder us from recognising, by means suppose of a rhomb of Iceland spar, the peculiar properties which it possesses, nor from fixing by observation alone the direction of those two rectangular planes, vertical and horizontal in the case supposed, with respect to which its properties are symmetrical, and in either of which if the principal plane of the examining rhomb be placed, an ordinary or an extraordinary beam, as the case may be, is alone produced in it. Light possessing this property, however it may have acquired it, is said to be polarized, and the plane with respect to which its properties are the same as are those of the ordinary ray in Iceland spar with respect to the principal plane, is called the plane of polarization. Its azimuth may be determined experimentally as being that of the principal plane of an examining rhomb when so turned as to transmit only an ordinary beam.

To go back now to the first arrangement, namely, that of a beam isolated by a first screen falling on a rhomb of Iceland spar, and then on a second screen provided like the first with a suitable hole, let this second screen be so placed as to transmit the beam which passed through the rhomb by extraordinary refraction, and stop the other, and then let the beam be presented for observation. On examining it with a

second rhomb we should find that it possessed iden-
tically the same properties as the beam obtained by
ordinary refraction in the first rhomb, save that the pro-
perties of this beam are related to the horizontal plane
precisely as were those of the former beam to the verti-
cal plane; and it is into the horizontal plane that the
principal plane of the examining rhomb must be turned
in order that nothing but an ordinary beam may be
transmitted through it.   Hence, according to our
former definition, we must say that the extraordinary
beam passing through Iceland spar is polarized in a
plane perpendicular to the plane of incidence.

More than a century elapsed after Huygens's
discovery of what we now call polarization before
it was known that polarized Light could be obtained
otherwise than by or as an accompaniment of double
refraction.   But in 1808 Malus made the very impor-
tant discovery that when Light is reflected from glass
at a certain angle, the reflected ray is wholly polarized;
and since the properties of the reflected ray are the
same with reference to the plane of incidence as are
those of the ordinary ray in Iceland spar with reference
to the principal plane of the crystal, we must in
accordance with our definition say that the reflected
light is polarized in the plane of incidence. The
light reflected at other inclinations possesses all the
properties of a mixture of common light with light

polarized in the plane of incidence, and may accordingly be said to be partially polarized in the plane of incidence. The transmitted light, whether the light be incident at the polarizing angle or the angle of incidence be arbitrary, is found to be partially polarized in a plane perpendicular to the plane of incidence.

Malus found that Light is thus polarized by reflection from transparent substances in general, from varnishes &c., but not from metals, the light reflected from which is found to be only partially polarized in the plane of incidence. The angle of incidence on transparent substances required for complete polarization Malus found to vary from one to another, though he did not discover according to what law, a law afterwards made out by Brewster, namely, that the polarizing angle is that for which the reflected and refracted rays are perpendicular to each other ; or, as it may be otherwise expressed, the tangent of the polarizing angle is equal to the index of refraction.

Malus's important discovery of the polarization of Light by reflection proves that polarization, whatever it may be, is something that may exist altogether independently of double refraction, and must therefore be something intimately bound up with the nature of Light in itself. The intimate connexion of double

refraction with polarization shows that we cannot hope to explain the former of these phenomena unless we can obtain some insight into the nature of what constitutes polarization.

LECTURE IV.

Phenomena presented on interposing a crystalline block or thin plate in the path of Polarized Light which is subsequently analyzed—Laws of Interference of Polarized Light— Theory of Transverse Vibrations—Conclusion.

WE have seen that when a beam of polarized light is examined by a rhomb of Iceland spar, it is divided into two of in general unequal intensity passing through the spar. As before, make abstraction of the small quantity of light which goes elsewhere by reflection, and call the intensity of the incident beam unity, and let us consider in the first instance the intensity of the beam which passes through the examining spar as ordinary. Let A be the azimuth of the principal plane of the examining rhomb referred to the plane of primitive polarization. Then the intensity, the relation of which to the angle A is the object of our search, must be such that it is equal to unity when A is nothing, decreases to nothing as A increases to $90°$, increases to unity again as A increases to $180°$, decreases again to nothing as A increases to

270°, and finally increases to unity as at first as A increases to 360°, having furthermore the same value for A negative as for A positive, and for 90° $-A$ negative as for 90° $-A$ positive. A very simple function possessing this property is $\cos^2 A$. If this be the intensity of the ordinary, since the rest of the light passes into the extraordinary, the intensity of the latter must be $1 - \cos^2 A$, or $\sin^2 A$, or the squared cosine of the angle between *its* plane of polarization and the plane of primitive polarization. Such was the law assumed by Malus, and called after his name. It has been verified by photometric measurements, and is of great importance with reference to the theory of what it is which constitutes polarization.

Suppose that light polarized in any way is subsequently analyzed, as it is called, whether by a thick block of Iceland spar furnished with screens so as to stop out one of the transmitted pencils, or by any of the more convenient methods more commonly employed. Let the analyzer be turned till the field of view is dark, the light falling upon the analyzer in that position being stifled, as in the case of a good tourmaline, or else sent elsewhere. If a block of Iceland spar be interposed between the polarizer and the analyzer, and turned round in its own plane, in general there is more or less restoration of light, there being only four azimuths of the block, separated by 90°, in a

complete turn in which the field remains dark as before the interposition of the block.

This restoration of light is very easily explained as a consequence of what has already been mentioned. The polarized light falling on the block is divided into two beams polarized respectively in and perpendicularly to its principal plane, which traverse the crystal independently though overlapping, and which emerge blended together. Each of these on entering the analyzer is again divided into two, polarized in rectangular planes, which are those of the plane of primitive polarization and the perpendicular plane, of which the latter alone is retained, and the two retained portions of each of the streams enter the eye together, and their illuminations are added together. A very simple application of Malus's law shows that if we take the intensity of the primitively polarized light for unity, and disregard the small loss by reflection, the intensity of the light entering the eye will be half the square of the sine of double the azimuth of the block, measured from one of the vanishing positions. This restoration of light in the dark field forms a very sensitive and easily applied test of the possession of double refraction by the substance interposed.

But an observation made by Arago about 1811 opened out quite a new field of research, remarkable

alike for the beauty of the phenomena, for the light they throw upon the nature of polarization, and for the information they afford us respecting the inmost structure of bodies. Arago found that when the interposed crystalline plate was very thin, as may easily be obtained with mica or sulphate of lime, the restored light was not as before white, but showed the most gorgeous colours, varying with the thickness of the plate, its nature, and the direction in which and amount by which it is inclined, if inclined it be.

The more powerfully doubly refracting be the substance interposed, the thinner as a rule is the interposed plate required to be in order to show these colours. But with a given substance, such for example as Iceland spar, the amount of double refraction varies immensely with the direction. Thus in Iceland spar, one of the most powerfully doubly refracting substances known, we have seen that in the direction of the axis the two rays are refracted alike. Accordingly if a plate of Iceland spar, even a thick plate, be cut perpendicular to the axis, and be interposed perpendicularly to the incident light in the dark field, a splendid system of coloured rings is seen, which are interrupted by a dark cross. The arrangement of coloured curves is still more remarkable in the case of biaxal crystals, but the simpler case of a thin crystalline plate is

better adapted to our present purpose, since in the other case there are too many things crowded at once upon the attention.

Let us revert then to the case of a thin crystalline plate interposed in the dark field, being held, as I will suppose, perpendicularly to the incident light. Even with this restriction, it would take too long to mention all the phenomena exhibited, and would be wearisome; nor is it advisable to treat the subject in this way, for in fact they have all been brought under the dominion of theory, and are best studied in connexion with it, except in so far as may be necessary to establish the theory in the first instance.

I shall restrict myself therefore to mentioning a few leading features of the phenomenon, premising that in the case of a doubly refracting plate in general, as in that of a block of Iceland spar, there are two rectangular directions in which the beams independently transmitted are respectively polarized, and that if the incident light is polarized, and the plane of polarization coincides with either of those directions, the whole of the light entering the crystal passes into the beam which is polarized in the plane of primitive polarization. The two rectangular direc-tions above mentioned have been named the *neutral axes* of the plate.

If the thin crystalline plate interposed be of uni-

form thickness, it is seen of a uniform colour; if the thickness vary irregularly, as in the case of a plate of selenite obtained by casual cleavage, the colours are arranged in patches, corresponding to the varying thickness. If the plate be turned in its own plane, there are four rectangular positions in which the field is left dark as at first, which are those in which one or other of the neutral axes lies in the plane of primitive polarization, and in which accordingly there is no bifurcation of the incident light on passing into the crystal.

If the analyzer be turned through 90°, so that the planes of polarization of the polarizer and analyzer are now coincident instead of perpendicular, and the field before the introduction of the crystal is at its maximum of brightness, on interposing the crystal-line plate the colours now seen are complementary in character to the former. They vanish altogether when either neutral axis comes into the plane of primitive polarization, and are less vivid than in the dark field except when the neutral axes are at an azimuth of 45° from the vanishing positions.

If the planes of polarization and analyzation be set at an arbitrary angle, and the crystal be turned in its own plane, there are eight positions in a com-plete revolution in which the colours disappear, giving place to white light of the same intensity as when the

plate is away. Between these critical positions, the colours have the character of those of the dark and of the bright field alternately. The critical positions are those in which one of the neutral axes lies in the plane of polarization or analyzation. Hence—and this is to be specially noticed—for the production of the colours it is essential that the polarized light we start with should be divided into two pencils in passing through the crystal, and that each of these again should be divided into two by the analyzer, of which one portion is retained.

If the crystalline plate be ground into the form of a slender wedge, the colours are arranged in bands parallel to the edge of the wedge, the bands for any colour being equidistant, and the scale larger for the red than for the blue. If the analyzer be set to give the dark and the bright field in succession, the tints of the wedge agree with those of the reflected system of Newton's rings in the former case, and the transmitted system in the latter.

The equidistance of the bands for any particular colour shows that the *law* of the order of the tints, as depending on the thickness of the plate, is the very same as in the case of Newton's rings, the *magnitude* of the thickness merely being very much greater than in that case. Nor is this all. If we know the doubly refracting energy of any particular

substance, suppose sulphate of lime, we can calculate the retardation of phase of one relatively to the other of the two rectangularly polarized pencils which a thin plate of the substance can independently transmit, in terms of the thickness of the plate, to which that retardation is proportional. Now Dr Young showed that when this is done the thickness of plate by which any particular tint is produced is just what it ought to be *on the supposition* that the colour is due to the interference of the two rectangularly polarized pencils which traversed the crystal independently. After all this we can hardly help supposing that the colours must in some way be due to interference. But if so, why are they not seen with common light, just as Newton's rings; why should it be necessary that the light should in some way be polarized, and the polarized light should in some way be analyzed, and that the crystalline plate should be interposed *between* the polarizer and the analyzer in order that any colours at all should be seen?

If we *assume* that the colours of crystalline plates in polarized light are due to interference, the laws of the interference of polarized light may be deduced from the observation of those colours without any experimental difficulty. But if it still be regarded as in any way an open question whether those colours

are due to interference, it becomes important to investigate those laws by means of experiments free from any such doubt.

Accordingly the series of researches by which Arago and Fresnel determined in a direct manner the laws of interference of polarized light must be regarded as making an epoch in the progress of the study of this branch of the subject. These experiments were made on the fringes of interference with which we had already become familiar as exhibited by common light; such fringes as those produced by the interference of two streams of light from the same source, such as two virtual images of a luminous point, or of two streams from the same luminous point, which after passing through two parallel extremely narrow apertures near one another diverged and mixed together. We have seen what a triumphant explanation the theory of undulations affords of the phenomena of interference and diffraction in the case of common light; and if we obtain the same fringes, with the appearance of which we are so familiar, in working with polarized instead of common light, we cannot refuse to admit that they too are due to interference.

The study of the interference of two streams of light polarized in the same way presents no experimental difficulty whatsoever. It is merely necessary

to use polarized light instead of common light in any of the ordinary experiments of .interference. On doing this the phenomena of interference are found to be absolutely the same with polarized as with common light. But to polarize two portions of light from the same source, and proceeding nearly along the same course, in two rectangular directions, and yet ensure a very near equality in the lengths of their paths, or rather equivalent paths in air, is a matter of very great nicety, so small is the difference of path that would suffice to prevent any exhibition of interference of the usual kind. Nevertheless by a series of ingeniously devised and carefully executed experiments Arago and Fresnel succeeded in establishing conclusively under what circumstances polarized light is, and under what it is not, capable of manifesting the usual phenomena of interference.

The result of this enquiry was summed up in five laws relating to the interference of polarized light, which were derived directly from observation. One of these has already been mentioned. Another is that when two streams of light from the same source are polarized in rectangular planes, they show no phenomena of interference, notwithstanding a near equality of paths. Another, that the mixed stream as in the last case may be analyzed without any phenomena of interference being thereby revealed.

Another that when two streams of light from the same source are polarized in rectangular planes, and afterwards analyzed, they *do* manifest the phenomena of interference *provided* the original source were one of polarized instead of common light. Lastly, in the phenomena of interference produced by rays which have experienced double refraction, the place of the fringes is not in all cases determined solely by the difference of equivalent paths; in certain cases it is necessary to alter the difference of paths by half an undulation. And the rule they gave for determining under what circumstances the half undulation should be added and under what circumstances not amounted to this:—when the planes of polarization and analyzation lie in the same quadrant made by the neutral axes of the crystalline plane, the character of the interference is determined simply by the difference of paths; but when they lie in adjacent quadrants, we must alter the difference of paths by half an undulation.

These five empirical laws embrace the necessity for a polarizer and for an analyzer in order that colours should be seen in a crystalline plate; and taken in conjunction with Malus's law, regarded at present merely as an empirical law, they enable us to calculate completely the colours of crystalline plates under all the varied conditions which may exist as

to thickness of the plate, doubly refracting energy of the substance of which it is formed, azimuth of the neutral axes relatively to the plane of primitive polarization, azimuth of the plane of analyzation; and for that we have no occasion to enter into any speculation at all as to what it is that constitutes polarization. Nay more; if we take the laws of double refraction as known empirically as the result of direct observation, we may even calculate completely the coloured rings and curves about the optic axis or axes of uniaxal or biaxal crystals, without entering into any speculation as to what the theoretical interpretation of polarization may be.

The question now arises, can we embrace the five laws of interference of polarized light, and Malus's law, in a theory which shall comprehend them all, and which shall be at least hopeful for the explanation of double refraction and of the polarization of light by reflection; though as these may depend on a knowledge of what is the actual state of things which we do not possess, we cannot demand of necessity that it *shall* lead to their explanation.

In applying Malus's law to the calculation of the colours of crystalline plates, we are led to contemplate an intensity which we may take as unity in the incident original polarized light as giving rise to intensities $\cos^2 A$ and $\sin^2 A$ belonging to light

polarized in a plane making an angle A with the plane of primitive polarization and in the perpendicular plane respectively, and these again as giving rise to polarized beams the intensities of which are obtained by a re-application of the very same law. Now in studying the interference of common light, we saw reason to conclude that for light of a given kind, that is, of a given refrangibility, the intensity is measured by the square of the coefficient of vibration, and consequently the coefficient of vibration by the square root of the intensity. It is true that I have not been able to lay before you the full evidence on which this conclusion is based, as it would have involved some considerations of too mathematical a nature to be suitable to the present lectures, so that I have been obliged to leave the result to be accepted in some measure on the strength of authority. Consequently we are led by pure observation, combined with so much of theory as belongs to the study of common light, to contemplate a beam of polarized light in which the coefficient of vibration may be taken as unity as being divided (as for example in passing through a crystalline lamina) into two polarized in rectangular planes, at azimuths of A and $90^0 - A$ to the plane of primitive polarization, the coefficients of vibration belonging to which are expressed by $\cos A$ and $\sin A$. But this is

identically the law according to which forces, or dis-
placements, or velocities in directions *perpendicular* to
that of propagation, and in or equally inclined to the
planes of polarization, would be resolved. We are
inevitably driven to the contemplation of a *something*
about polarized light which admits of composition
and resolution according to the above simple law.
This "something" can hardly be other than the
vibrations themselves, and we are thus led to conclude
that in polarized light the vibrations are rectilinear,
but instead of being in the direction of propagation,
as from the analogy of sound the vibrations of Light
might naturally have been expected to be, are *trans-
verse* to the direction of propagation.

When polarized light is obtained by ordinary
refraction through Iceland spar, or by reflection from
glass at the proper angle or incidence, everything is
symmetrical with respect to the plane of polarization.
We must suppose therefore that in polarized light
the vibrations are symmetrical with respect to the plane
of polarization. This leaves two alternatives open :
they may either be in the plane of polarization or
perpendicular to the plane of polarization. So far as
the explanation of the laws of interference of polarized
light is concerned, it is a matter of absolute in-
difference which alternative we adopt, and some
undulationists have adopted the one and some the

other.   The question can only be decided, if it can be
decided at all, by introducing more or less of dy-
namical considerations, and that introduces more or
less of speculation, since the dynamical nature of the
mutual action of ponderable matter and ether is in
great measure unknown to us.   Perhaps the argument
which introduces least speculation as to the dy-
namical nature of such actions is that derived from
diffraction at a considerable angle; though it is true
that even here we cannot produce that diffraction
without the intervention of ponderable matter.   The
result in this case, as I have elsewhere shown, is
decidedly in favour of the supposition that the vibra-
tions are perpendicular to the plane of polarization,
which is the alternative that was adopted by Fresnel,
and ultimately by Cauchy, though at first he adopted
the other; and it is the one for which, independently
of diffraction, there is I think the most to be said.
But as I have remarked the theory of transverse
vibrations taken by itself does not involve the decision
of this question.

If such be the nature of polarized light, what
notion are we to form as to the nature of common
light? Suppose light coming in some definite direc-
tion to fall on a screen with a hole, and to be
received at the other side on a block of Iceland spar.
Then if the hole be not too large, the two beams

which are produced within the spar will come out
separated from one another, and will show their
polarization in rectangular planes. This will still be
true however the screen may be moved about, so that
the light falls on different parts of the face of the
block. Now let the screen be removed altogether.
The light will still be decomposed into two beams
within the spar, giving rise on emergence to two
beams polarized in rectangular planes, but the beams
will be broad, and will mix on emergence. But the
mixture is identical with, or at any rate is undis-
tinguishable from, common light.* Now the two
disturbances in rectangular planes give rise by their
composition to a disturbance which is in the fronts of
the waves, but is in general elliptical, including the
extreme cases of circular and rectilinear vibrations,
but with the elements of the ellipse changing, as we
have every reason to expect, irregularly in all sorts of
ways a great number of times in a second. For
though the vibrations may be sensibly regular for
thousands or it may be myriads together (and the
phenomena of interference show that they must have

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* Abstraction is here made of the loss by reflection, which is not
quite the same for the ordinary and extraordinary, the higher refraction
of the former being accompanied by a slightly more copious reflection,
so that in the transmitted light there is a slight theoretical preponder-
ance of intensity in favour of the extraordinary. This is however so
small as to be barely sensible in refined experiments, and for our present
purpose it is best neglected.

a high degree or regularity) yet we should have no reason *a priori* to expect that they would remain regular for the fifty millions of millions or so of vibrations which must take place in the tenth of a second, the time for which an impression made on the retina is estimated to last.

Such being our notion of common light, the division of common light into two rectangularly polarized beams which follow different paths must be taken to imply that for some cause yet to be investigated the vibrations, which at first were in the fronts of the waves, but in other respects of any kind, are decomposed into two rectilinear vibrations in rectangular directions, which are propagated along different paths.

This fundamental conception as to the nature of polarized light, and its relation to common light, explains in the simplest manner the six laws relating to the interference of polarized light, and to the intensity of the polarized streams into which polarized light may be divided, to which I have just referred. The interference of light merely demands, so far as direction of vibration is concerned, that it should be as nearly as possible the same for the two interfering streams, a condition satisfied by two streams polarized in the same way. Again, the kinetic energy of a set of vibrations, to which for light of a given kind the intensity is theoretically proportional, is the sum of

the kinetic energies of the components in any two rectangular directions, irrespective of their difference of phase, and therefore no phenomena of interference ought to be visible in the mixture of two rectangularly polarized portions of light, even though they came originally from the same polarized source. Again, if common light be divided into two rectangularly polarized portions, which are afterwards subdivided in a similar manner, and a pair of these latter components which are polarized alike mix, having had but a small difference of path from the original source, they ought not to show any signs of interference. For as there is no fixed and permanent relation between the relative magnitudes or the relative starting-times of the first components, they are as good as if they came from two independent sources, in which case no phenomena of interference are either theoretically observable or experimentally observed, do what we will with the streams of light afterwards. But if the original source of light be one of polarized instead of common light, the case is altogether different. Then, whatever changes take place during the tenth of a second in the amplitude or starting-time of one of the first components, exactly the same take place in the other, and are carried on into the second components, of which therefore those which are polarized in the same way are in a condition

to interfere. And as to the circumstances under
which in this kind of interference the difference of
path must sometimes be imagined altered by half an
undulation, the matter is simple to the last degree on
the theory of transverse vibrations; it merely involves
attention to the sign of a geometrical projection; and
if we express the intensity of the mixed light, for the
case in which the planes of polarization and analyza-
tion lie in the same quadrant formed by the neutral
axes of a crystalline plate, by a formula in which the
symbols denote displacements or velocities or of an
ethereal particle instead of intensities, the formula
takes care of itself, and applies equally to the case in
which the planes of polarization and analyzation lie
in adjacent quadrants.

If polarized light be incident on a crystalline plate
of uniform thickness, and the emergent light be
viewed through an analyzer which is turned round,
then except in certain special cases which I need not
particularize, the light is not extinguished by the
analyzer however it be turned, but merely be-
comes, in general, alternately a maximum and a
minimum alternately at every quarter of a turn. So
far it agrees exactly with partially polarized light.
And yet the two are altogether different, and the
difference may be seen at a glance by viewing them
through a Nicol's prism capped by a plate of Iceland

spar cut perpendicular to the axis. The light we are now considering is called elliptically polarized light, and in contradistinction to it the light which I have hitherto called polarized has been denominated plane-polarized.

Elliptically polarized light may be obtained independently altogether of double refraction. The theory of transverse vibrations presents to the mind a very clear picture of what constitutes it. But as this is a matter of detail not involving any fresh principle, I forbear to enter further into it.

Being now armed with a definite theory as to the nature of polarized light, we are prepared to consider whether any explanation can be given of double refraction, as an accompaniment of which polarization was first discovered. Whether we are able or not to give a complete explanation of it, we might expect that the theory would at least so far fall in with it as to point hopefully towards an explanation.

The most salient feature of double refraction, interpreted by the aid of the theory which makes light to consist in undulations, and of that which specifies the nature of polarized light, is that when light falls on a crystalline plate or prism it gives rise to two kinds of disturbance within the crystal, which are propagated with different velocities, and in which the vibrations are rectilinear, and take place in planes

which are perpendicular to each other, or at least very approximately so, and of·which the directions are determined by lines fixed in the crystal.

Now we have not far to go to find a mechanical illustration of such a mode of action. Imagine an elastic rod terminated at one end, and extending indefinitely in the other direction. Let the rod be rectangular in section, the sides of the rectangle being unequal, so that the rod is stiffer to resist flexure in one of its principal planes than the other. Let this rod be joined on to a cylindrical rod forming a continuation of it which extends indefinitely. Conceive the compound rod as capable of propagating small transverse disturbances in which the axis of the rod suffers flexure. Imagine a small disturbance, suppose periodic, to be travelling in the cylindrical rod towards the junction. It will travel on without change of type even though the flexure of the axis be not in one plane. But to find what disturbance it excites in the rectangular rod, we must resolve the disturbance in the cylindrical rod into its components in the principal planes of the rectangular rod, and consider them separately. Each will give rise in the rectangular rod to a disturbance in its own plane, but the two will travel along the rod with different velocities. This illustrates the subdivision of a beam of common light falling on a block of Iceland spar into two beams

polarized in rectangular planes, which are propagated
in the spar with different velocities.   Again, suppose
the original disturbance in the cylindrical rod confined
to one plane.   If this be either of the principal planes
of the rectangular rod, the more slowly or the more
quickly travelling kind of disturbance, as the case
may be, will alone be excited in the latter ; and if the
plane of the original disturbance be any other, the
components into which we must resolve it in order to
find the disturbance excited in the rectangular rod
will in general be of unequal intensity, their squares
varying with the azimuth of the plane of the original
disturbance in accordance with Malus's law.   This
illustrates the subdivision of a beam of polarized light
incident on Iceland spar into two of unequal intensity
polarized in rectangular planes, and their alternate
disappearance at every quarter of a turn.   We see
with what perfect simplicity the theory of transverse
vibrations falls in with the elementary facts of polari-
zation discovered by Huygens, standing in marked
contrast in this respect with the conjecture by which
Huygens himself attempted to account for double
refraction.

    But can we go further, and account for, or
discover, from theory the laws of double refraction
and the accompanying polarization in different direc-
tions in doubly refracting crystals ?

It is to Fresnel we owe the first theoretical deduction of the laws of double refraction in Iceland spar, and the discovery of the beautiful laws of double refraction in biaxal crystals ; laws some of which had been previously known from observation, while in other respects theory served to correct what had been supposed to be the result of observation, but which more careful observation carried out in directions indicated by theory proved to have been incorrect. The generalization by which Fresnel passed from the laws of double refraction in uniaxal to those in biaxal crystals is one of the most splendid things which has been done in science. And yet the theory which guided him to the discovery of these laws is not one which is rigorous throughout, nor did Fresnel himself profess that it was, though in some reproductions of his theory contained in text-books it is presented as if it were, to the detriment of the student. Fresnel was a man of singular sagacity, endowed apparently with a mind of the inductive class, leading him often to the discovery of truth from conflicting or imperfect evidence. We may even say it is fortunate that Fresnel did not rigorously follow out to their conclusions the premises with which he started, for had he done so he would have missed the discovery of the elegant laws of double refraction. In fact, Cauchy and Neumann independently worked out the con-

clusions which rigorously follow from the state of things assumed by Fresnel ; but with all the squeezing which the arbitrary constants furnished by theory admit of, they were not able to obtain Fresnel's laws except as an approximation. Had this been all, it is possible that the more complicated laws expressed by their formulae might have fitted observation as well as the simpler and more elegant laws of Fresnel. But the theory is hampered by a third ray which cannot be got rid of, and which leads to conclusions at variance with observation. While admiring therefore the geometrical part of Fresnel's theory, we must reject the mechanical conditions which he supposed to exist in crystals, as not being in conformity with nature.

Fresnel's laws have been obtained in an extremely elegant manner, as the result of a rigorous theory, by Green, who starting with an assumed mechanical state inclusive of but more general than that assumed by Fresnel, and subsequently limiting the generality by a single condition which the phenomena of light would naturally lead us to introduce, arrived directly at Fresnel's laws. It was necessary however to suppose the vibrations of polarized light to be *in* the plane of polarization. In the same paper he showed however that by starting with a mechanical state still more general, limiting it as before, and introducing

two simple linear relations between the arbitrary
constants remaining, Fresnel's laws were again ob-
tained, but this time by supposing that in polarized
light the vibrations are *perpendicular to* the plane of
polarization. Almost simultaneously with Green
MacCullagh obtained equations of motion of the
ether in a crystal identical with those of Green in his
first theory, though by a method which does not seem
quite so satisfactory as that of Green. They led of
course to the same laws, and required the same
supposition as to the direction of the vibrations in
polarized light. Lamé has given a theory sub-
stantially agreeing with Green's first theory; and last
but not least we have the electro-magnetic theory of
Maxwell, which without any straining or assumption
of relations between constants leads directly to
Fresnel's laws.

It may seem strange that we should arrive at the
very same laws by such different theories; first by
one which is not a rigorous theory at all, and then by
others which are rigorous, but which differ among
themselves, even to such an extent that in one the ·
vibrations in polarized light must be assumed to be
in, in another perpendicular to the plane of polariza-
tion. In explanation of this it is to be observed, first,
that all these theories alike involve the idea of
transverse vibrations, and secondly that Fresnel's laws

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are really the simplest which can in any way suit the
phenomena. Fresnel's laws are embraced in an
elegant construction applied to an ellipsoid; and just
as an ellipsoid is the simplest generalization of a
sphere when we pass from what is alike in all directions
to what varies from one direction to another, so
Fresnel's laws are really the simplest that the nature
of the phenomenon, viewed in the light of the
theory of transverse vibrations, admits of. It is not
therefore so wonderful as at first sight might appear
that the same laws should be arrived at from theories
so different; and while the deduction of these laws is .
a strong confirmation of the truth of the theory of
transverse vibrations which is common to all the
methods, it is not itself alone to be taken as establish-
ing the truth of the supposition as to the mechanical
state of things in crystals which has been made in the
deduction of the laws.

I have mentioned Malus's discovery of the polari-
zation of light by reflection, and the question may
naturally arise, is this reconcileable with the theory of
transverse vibrations? To show that it is, I need
only refer to Fresnel's deduction of the intensity of
the light reflected from an isotropic transparent
medium, according as the vibrations are in or per-
pendicular to the plane of incidence. These formulae
were obtained by Fresnel with his wonted sagacity

from a process only partially complete, since we must either allow that a part only of the necessary equations of condition at the common boundary of the media are satisfied, or else that the mechanical state for which the conditions employed give the complete solution remains to be defined.

Fresnel's deduction of his laws for the intensity of reflected light was made on the supposition that in polarized light the vibrations are perpendicular to the plane of polarization. A slight difference of hypothesis as to the state of things would lead, by reasoning very similar to that of Fresnel, to the very same two formulae only with the directions of vibration to which they respectively apply interchanged. On the present supposition therefore the vibrations in polarized light must be supposed to be in the plane of polarization.

The polarization of light through the unequal absorption belonging to the two rectangularly polarized pencils within certain coloured doubly refracting crystals, such as tourmaline, readily falls in with the theory of transverse vibrations. For there can be no doubt that absorption in general consists in the expenditure of the incident ethereal vibrations in producing molecular agitation; and it is easily understood that the molecules may be more easily agitated by an ethereal vibration in one direction than in another.

The aim which I have proposed to myself in this first course of the Burnett lectures has been to lay before you, as impartially as I could, a summary of the evidence on which we accept the answer to the question, What is Light? given when we say, Light consists of undulations in a medium, called ether, pervading the interplanetary and interstellar spaces, and existing also within bodies formed of ponderable matter. Difference of refrangibility, with the accompanying difference of colour, depends upon a difference in the frequency of these undulations. The direction of vibration of the particles of the ether is transverse to the direction of propagation of the light, and accordingly (at any rate in the case of vacuum or an isotropic medium) the vibrations take place in the fronts of the waves, but in common light are not otherwise restricted, while in polarized light they are rectilinear, taking place in a direction which is symmetrical with respect to the plane of polarization.
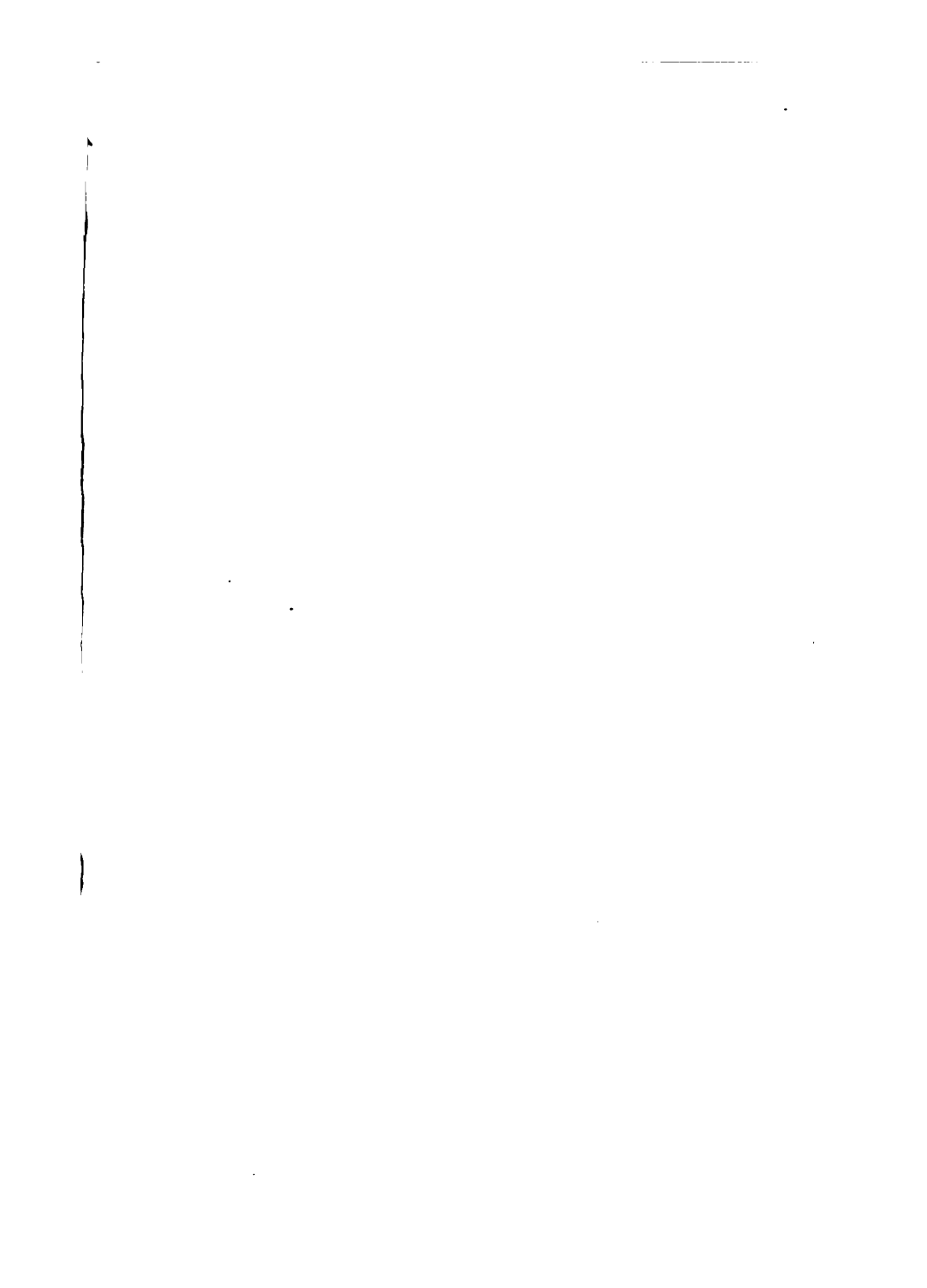
Naturally the full force of the evidence can be felt only by those who have well studied the subject. I hope however that I may have succeeded in showing, even to those who previously may have paid little attention to the matter, that at least there are powerful arguments in favour of the accepted answer. The inductive arrangement, which seemed best fitted for

the object I had in view, naturally led to a treatment in good measure historical, but I have not, I hope, neglected recent researches. I have endeavoured to discriminate, and to lead you to discriminate, between what is well established and what is still speculative, and have confined myself almost entirely to the former.

Should I be permitted to deliver courses of lectures again in the two following years, it is my intention, in accordance with a scheme communicated in outline to and approved by the Burnett Trustees, to devote next year's course to researches in which light has been used as a means of investigation, while the third year's course would be assigned to light considered in relation to its beneficial effects. While the different objects to be held in view in these lectures are more or less blended together, the second course would more especially relate to recent researches, while the third would naturally harmonize with the original intentions of the Founder of the Trust.

END OF FIRST COURSE.

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