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\*\*\* START OF THIS PROJECT GUTENBERG EBOOK FIVE OF MAXWELL'S PAPERS \*\*\*

Produced by Gordon Keener

This eBook includes 5 papers or speeches by James Clerk Maxwell.

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Foramen Centrale

Theory of Compound Colours

Poinsot's Theory

Address to the Mathematical

Introductory Lecture

On the Unequal Sensibility of the Foramen Centrale to Light of

different Colours.

James Clerk Maxwell

[From the \_Report of the British Association\_, 1856.]

When observing the spectrum formed by looking at a long ve rtical slit

through a simple prism, I noticed an elongated dark spot running up

and down in the blue, and following the motion of the eye as it moved

\_up and down\_ the spectrum, but refusing to pass out of the blue into

the other colours. It was plain that the spot belonged both to the

eye and to the blue part of the spectrum. The result to which I have

come is, that the appearance is due to the yellow spot on the retina,

commonly called the \_Foramen Centrale\_ of Soemmering. The most

convenient method of observing the spot is by presenting to the eye in

not too rapid succession, blue and yellow glasses, or, still better,

allowing blue and yellow papers to revolve slowly before the eye. In

this way the spot is seen in the blue. It fades rapidly, but is

renewed every time the yellow comes in to relieve the effect of the

blue. By using a Nicol's prism along with this apparatus, the brushes

of Haidinger are well seen in connexion with the spot, and the fact of

the brushes being the spot analysed by polarized light becomes

evident. If we look steadily at an object behind a series of bright

bars which move in front of it, we shall see a curious bending of the

bars as they come up to the place of the yellow spot. The part which

comes over the spot seems to start in advance of the rest of the bar,

and this would seem to indicate a greater rapidity of sensation at the

yellow spot than in the surrounding retina. But I find the experiment

difficult, and I hope for better results from more accurate observers.

On the Theory of Compound Colours with reference to Mixtures of

Blue and Yellow Light.

James Clerk Maxwell

[From the \_Report of the British Association\_, 1856.]

When we mix together blue and yellow paint, we obtain green paint.

This fact is well known to all who have handled colours; and it is

universally admitted that blue and yellow make green. Red, yellow,

and blue, being the primary colours among painters, green is regarded

as a secondary colour, arising from the mixture of blue and yellow.

Newton, however, found that the green of the spectrum was not the same

thing as the mixture of two colours of the spectrum, for such a

mixture could be separated by the prism, while the green of the

spectrum resisted further decomposition. But still it was believed

that yellow and blue would make a green, though not that of the

spectrum. As far as I am aware, the first experiment on the subject

is that of M. Plateau, who, before 1819, made a disc with alternate

sectors of prussian blue and gamboge, and observed that, when

spinning, the resultant tint was not green, but a neutral gray,

inclining sometimes to yellow or blue, but never to green.

Prof. J. D. Forbes of Edinburgh made similar experiments in 1849, with

the same result. Prof. Helmholtz of Konigsberg, to whom we owe the

most complete investigation on visible colour, has given the true

explanation of this phenomenon. The result of mixing two coloured

powders is not by any means the same as mixing the beams of light

which flow from each separately. In the latter case we receive all

the light which comes either from the one powder or the other. In the

former, much of the light coming from one powder falls on particles of

the other, and we receive only that portion which has escaped

absorption by one or other. Thus the light coming from a mixture of

blue and yellow powder, consists partly of light coming directly from

blue particles or yellow particles, and partly of light acted on by

both blue and yellow particles. This latter light is green, since the

blue stops the red, yellow, and orange, and the yellow stops the blue

and violet. I have made experiments on the mixture of blue and yellow

light--by rapid rotation, by combined reflexion and transmission, by

viewing them out of focus, in stripes, at a great distance, by

throwing the colours of the spectrum on a screen, and by receiving

them into the eye directly; and I have arranged a portable apparatus

by which any one may see the result of this or any other mixture of

the colours of the spectrum. In all these cases blue and yellow do

not make green. I have also made experiments on the mixture of

coloured powders. Those which I used principally were "mineral blue"

(from copper) and "chrome-yellow." Other blue and yellow pigments gave

curious results, but it was more difficult to make the mixtures, and

the greens were less uniform in tint. The mixtures of these colours

were made by weight, and were painted on discs of paper, which were

afterwards treated in the manner described in my paper "On Colour as

perceived by the Eye," in the \_Transactions of the Royal Society of

Edinburgh\_, Vol. XXI. Part 2. The visible effect of the colour is

estimated in terms of the standard-coloured papers:--vermilion (V),

ultramarine (U), and emerald-green (E). The accuracy of the results,

and their significance, can be best understood by referring to the

paper before mentioned. I shall denote mineral blue by B, and

chrome-yellow by Y; and B3 Y5 means a mixture of three parts blue and

five parts yellow.

Given Colour. Standard Colours. Coefficient

V. U. E. of brightness.

B8 , 100 = 2 36 7 ............ 45

B7 Y1, 100 = 1 18 17 ............ 37

B6 Y2, 100 = 4 11 34 ............ 49

B5 Y3, 100 = 9 5 40 ............ 54

B4 Y4, 100 = 15 1 40 ............ 56

B3 Y5, 100 = 22 - 2 44 ............ 64

B2 Y6, 100 = 35 -10 51 ............ 76

B1 Y7, 100 = 64 -19 64 ............ 109

Y8, 100 = 180 -27 124 ............ 277

The columns V, U, E give the proportions of the standard colours which

are equivalent to 100 of the given colour; and the sum of V, U, E

gives a coefficient, which gives a general idea of the brightness. It

will be seen that the first admixture of yellow \_diminishes\_ the

brightness of the blue. The negative values of U indicate that a

mixture of V, U, and E cannot be made equivalent to the given colour.

The experiments from which these results were taken had the negative

values transferred to the other side of the equation. They were all

made by means of the colour-top, and were verified by repetition at

different times. It may be necessary to remark, in conclusion, with

reference to the mode of registering visible colours in terms of three

arbitrary standard colours, that it proceeds upon that theory of three

primary elements in the sensation of colour, which treats the

investigation of the laws of visible colour as a branch of human

physiology, incapable of being deduced from the laws of light itself,

as set forth in physical optics. It takes advantage of the methods of

optics to study vision itself; and its appeal is not to physical

principles, but to our consciousness of our own sensations.

On an Instrument to illustrate Poinsot's Theory of Rotation.

James Clerk Maxwell

[From the \_Report of the British Association\_, 1856.]

In studying the rotation of a solid body according to Poinsot's

method, we have to consider the successive positions of the

instantaneous axis of rotation with reference both to directions fixed

in space and axes assumed in the moving body. The paths traced out by

the pole of this axis on the \_invariable plane\_ and on the \_central

ellipsoid\_ form interesting subjects of mathematical investigation.

But when we attempt to follow with our eye the motion of a rotating

body, we find it difficult to determine through what point of the

\_body\_ the instantaneous axis passes at any time,--and to determine its

path must be still more difficult. I have endeavoured to render

visible the path of the instantaneous axis, and to vary the

circumstances of motion, by means of a top of the same kind as that

used by Mr Elliot, to illustrate precession\*. The body of the

instrument is a hollow cone of wood, rising from a ring, 7 inches in

diameter and 1 inch thick. An iron axis, 8 inches long, screws into

the vertex of the cone. The lower extremity has a point of hard

steel, which rests in an agate cup, and forms the support of the

instrument. An iron nut, three ounces in weight, is made to screw on

the axis, and to be fixed at any point; and in the wooden ring are

screwed four bolts, of three ounces, working horizontally, and four

bolts, of one ounce, working vertically. On the upper part of the

axis is placed a disc of card, on which are drawn four concentric

rings. Each ring is divided into four quadrants, which are coloured

red, yellow, green, and blue. The spaces between the rings are white.

When the top is in motion, it is easy to see in which quadrant the

instantaneous axis is at any moment and the distance between it and

the axis of the instrument; and we observe,--1st. That the

instantaneous axis travels in a closed curve, and returns to its

original position in the body. 2ndly. That by working the vertical

bolts, we can make the axis of the instrument the centre of this

closed curve. It will then be one of the principal axes of inertia.

3rdly. That, by working the nut on the axis, we can make the order of

colours either red, yellow, green, blue, or the reverse. When the

order of colours is in the same direction as the rotation, it

indicates that the axis of the instrument is that of greatest moment

of inertia. 4thly. That if we screw the two pairs of opposite

horizontal bolts to different distances from the axis, the path of the

instantaneous pole will no longer be equidistant from the axis, but

will describe an ellipse, whose longer axis is in the direction of the

mean axis of the instrument. 5thly. That if we now make one of the

two horizontal axes less and the other greater than the vertical axis,

the instantaneous pole will separate from the axis of the instrument,

and the axis will incline more and more till the spinning can no

longer go on, on account of the obliquity. It is easy to see that, by

attending to the laws of motion, we may produce any of the above

effects at pleasure, and illustrate many different propositions by

means of the same instrument.

\* \_Transactions of the Royal Scottish Society of Arts\_, 1855.

Address to the Mathematical and Physical Sections of the British

Association.

James Clerk Maxwell

[From the \_British Association Report\_, Vol. XL.]

[Liverpool, \_September\_ 15, 1870.]

At several of the recent Meetings of the British Association the

varied and important business of the Mathematical and Physical Section

has been introduced by an Address, the subject of which has been left

to the selection of the President for the time being. The perplexing

duty of choosing a subject has not, however, fallen to me.

Professor Sylvester, the President of Section A at the Exeter Meeting,

gave us a noble vindication of pure mathematics by laying bare, as it

were, the very working of the mathematical mind, and setting before

us, not the array of symbols and brackets which form the armoury of

the mathematician, or the dry results which are only the monuments of

his conquests, but the mathematician himself, with all his human

faculties directed by his professional sagacity to the pursuit,

apprehension, and exhibition of that ideal harmony which he feels to

be the root of all knowledge, the fountain of all pleasure, and the

condition of all action. The mathematician has, above all things, an

eye for symmetry; and Professor Sylvester has not only recognized the

symmetry formed by the combination of his own subject with those of

the former Presidents, but has pointed out the duties of his successor

in the following characteristic note:--

"Mr Spottiswoode favoured the Section, in his opening Address, with a

combined history of the progress of Mathematics and Physics; Dr.

Tyndall's address was virtually on the limits of Physical Philosophy;

the one here in print," says Prof. Sylvester, "is an attempted faint

adumbration of the nature of Mathematical Science in the abstract.

What is wanting (like a fourth sphere resting on three others in

contact) to build up the Ideal Pyramid is a discourse on the Relation

of the two branches (Mathematics and Physics) to, their action and

reaction upon, one another, a magnificent theme, with which it is to

be hoped that some future President of Section A will crown the

edifice and make the Tetralogy (symbolizable by \_A+A'\_, \_A\_, \_A'\_,

\_AA'\_) complete."

The theme thus distinctly laid down for his successor by our late

President is indeed a magnificent one, far too magnificent for any

efforts of mine to realize. I have endeavoured to follow Mr

Spottiswoode, as with far-reaching vision he distinguishes the systems

of science into which phenomena, our knowledge of which is still in

the nebulous stage, are growing. I have been carried by the

penetrating insight and forcible expression of Dr Tyndall into that

sanctuary of minuteness and of power where molecules obey the laws of

their existence, clash together in fierce collision, or grapple in yet

more fierce embrace, building up in secret the forms of visible

things. I have been guided by Prof. Sylvester towards those serene

heights

"Where never creeps a cloud, or moves a wind,

Nor ever falls the least white star of snow,

Nor ever lowest roll of thunder moans,

Nor sound of human sorrow mounts to mar

Their sacred everlasting calm."

But who will lead me into that still more hidden and dimmer region

where Thought weds Fact, where the mental operation of the

mathematician and the physical action of the molecules are seen in

their true relation? Does not the way to it pass through the very den

of the metaphysician, strewed with the remains of former explorers,

and abhorred by every man of science? It would indeed be a foolhardy

adventure for me to take up the valuable time of the Section by

leading you into those speculations which require, as we know,

thousands of years even to shape themselves intelligibly.

But we are met as cultivators of mathematics and physics. In our

daily work we are led up to questions the same in kind with those of

metaphysics; and we approach them, not trusting to the native

penetrating power of our own minds, but trained by a long-continued

adjustment of our modes of thought to the facts of external nature.

As mathematicians, we perform certain mental operations on the symbols

of number or of quantity, and, by proceeding step by step from more

simple to more complex operations, we are enabled to express the same

thing in many different forms. The equivalence of these different

forms, though a necessary consequence of self-evident axioms, is not

always, to our minds, self-evident; but the mathematician, who by long

practice has acquired a familiarity with many of these forms, and has

become expert in the processes which lead from one to another, can

often transform a perplexing expression into another which explains

its meaning in more intelligible language.

As students of Physics we observe phenomena under varied

circumstances, and endeavour to deduce the laws of their relations.

Every natural phenomenon is, to our minds, the result of an infinitely

complex system of conditions. What we set ourselves to do is to

unravel these conditions, and by viewing the phenomenon in a way which

is in itself partial and imperfect, to piece out its features one by

one, beginning with that which strikes us first, and thus gradually

learning how to look at the whole phenomenon so as to obtain a

continually greater degree of clearness and distinctness. In this

process, the feature which presents itself most forcibly to the

untrained inquirer may not be that which is considered most

fundamental by the experienced man of science; for the success of any

physical investigation depends on the judicious selection of what is

to be observed as of primary importance, combined with a voluntary

abstraction of the mind from those features which, however attractive

they appear, we are not yet sufficiently advanced in science to

investigate with profit.

Intellectual processes of this kind have been going on since the first

formation of language, and are going on still. No doubt the feature

which strikes us first and most forcibly in any phenomenon, is the

pleasure or the pain which accompanies it, and the agreeable or

disagreeable results which follow after it. A theory of nature from

this point of view is embodied in many of our words and phrases, and

is by no means extinct even in our deliberate opinions.

It was a great step in science when men became convinced that, in

order to understand the nature of things, they must begin by asking,

not whether a thing is good or bad, noxious or beneficial, but of what

kind is it? and how much is there of it? Quality and Quantity were

then first recognized as the primary features to be observed in

scientific inquiry.

As science has been developed, the domain of quantity has everywhere

encroached on that of quality, till the process of scientific inquiry

seems to have become simply the measurement and registration of

quantities, combined with a mathematical discussion of the numbers

thus obtained. It is this scientific method of directing our

attention to those features of phenomena which may be regarded as

quantities which brings physical research under the influence of

mathematical reasoning. In the work of the Section we shall have

abundant examples of the successful application of this method to the

most recent conquests of science; but I wish at present to direct your

attention to some of the reciprocal effects of the progress of science

on those elementary conceptions which are sometimes thought to be

beyond the reach of change.

If the skill of the mathematician has enabled the experimentalist to

see that the quantities which he has measured are connected by

necessary relations, the discoveries of physics have revealed to the

mathematician new forms of quantities which he could never have

imagined for himself.

Of the methods by which the mathematician may make his labours most

useful to the student of nature, that which I think is at present most

important is the systematic classification of quantities.

The quantities which we study in mathematics and physics may be

classified in two different ways.

The student who wishes to master any particular science must make

himself familiar with the various kinds of quantities which belong to

that science. When he understands all the relations between these

quantities, he regards them as forming a connected system, and he

classes the whole system of quantities together as belonging to that

particular science. This classification is the most natural from a

physical point of view, and it is generally the first in order of

time.

But when the student has become acquainted with several different

sciences, he finds that the mathematical processes and trains of

reasoning in one science resemble those in another so much that his

knowledge of the one science may be made a most useful help in the

study of the other.

When he examines into the reason of this, he finds that in the two

sciences he has been dealing with systems of quantities, in which the

mathematical forms of the relations of the quantities are the same in

both systems, though the physical nature of the quantities may be

utterly different.

He is thus led to recognize a classification of quantities on a new

principle, according to which the physical nature of the quantity is

subordinated to its mathematical form. This is the point of view

which is characteristic of the mathematician; but it stands second to

the physical aspect in order of time, because the human mind, in order

to conceive of different kinds of quantities, must have them presented

to it by nature.

I do not here refer to the fact that all quantities, as such, are

subject to the rules of arithmetic and algebra, and are therefore

capable of being submitted to those dry calculations which represent,

to so many minds, their only idea of mathematics.

The human mind is seldom satisfied, and is certainly never exercising

its highest functions, when it is doing the work of a calculating

machine. What the man of science, whether he is a mathematician or a

physical inquirer, aims at is, to acquire and develope clear ideas of

the things he deals with. For this purpose he is willing to enter on

long calculations, and to be for a season a calculating machine, if he

can only at last make his ideas clearer.

But if he finds that clear ideas are not to be obtained by means of

processes the steps of which he is sure to forget before he has

reached the conclusion, it is much better that he should turn to

another method, and try to understand the subject by means of

well-chosen illustrations derived from subjects with which he is more

familiar.

We all know how much more popular the illustrative method of

exposition is found, than that in which bare processes of reasoning

and calculation form the principal subject of discourse.

Now a truly scientific illustration is a method to enable the mind to

grasp some conception or law in one branch of science, by placing

before it a conception or a law in a different branch of science, and

directing the mind to lay hold of that mathematical form which is

common to the corresponding ideas in the two sciences, leaving out of

account for the present the difference between the physical nature of

the real phenomena.

The correctness of such an illustration depends on whether the two

systems of ideas which are compared together are really analogous in

form, or whether, in other words, the corresponding physical

quantities really belong to the same mathematical class. When this

condition is fulfilled, the illustration is not only convenient for

teaching science in a pleasant and easy manner, but the recognition of

the formal analogy between the two systems of ideas leads to a

knowledge of both, more profound than could be obtained by studying

each system separately.

There are men who, when any relation or law, however complex, is put

before them in a symbolical form, can grasp its full meaning as a

relation among abstract quantities. Such men sometimes treat with

indifference the further statement that quantities actually exist in

nature which fulfil this relation. The mental image of the concrete

reality seems rather to disturb than to assist their contemplations.

But the great majority of mankind are utterly unable, without long

training, to retain in their minds the unembodied symbols of the pure

mathematician, so that, if science is ever to become popular, and yet

remain scientific, it must be by a profound study and a copious

application of those principles of the mathematical classification of

quantities which, as we have seen, lie at the root of every truly

scientific illustration.

There are, as I have said, some minds which can go on contemplating

with satisfaction pure quantities presented to the eye by symbols, and

to the mind in a form which none but mathematicians can conceive.

There are others who feel more enjoyment in following geometrical

forms, which they draw on paper, or build up in the empty space before

them.

Others, again, are not content unless they can project their whole

physical energies into the scene which they conjure up. They learn at

what a rate the planets rush through space, and they experience a

delightful feeling of exhilaration. They calculate the forces with

which the heavenly bodies pull at one another, and they feel their own

muscles straining with the effort.

To such men momentum, energy, mass are not mere abstract expressions

of the results of scientific inquiry. They are words of power, which

stir their souls like the memories of childhood.

For the sake of persons of these different types, scientific truth

should be presented in different forms, and should be regarded as

equally scientific whether it appears in the robust form and the vivid

colouring of a physical illustration, or in the tenuity and paleness

of a symbolical expression.

Time would fail me if I were to attempt to illustrate by examples the

scientific value of the classification of quantities. I shall only

mention the name of that important class of magnitudes having

direction in space which Hamilton has called vectors, and which form

the subject-matter of the Calculus of Quaternions, a branch of

mathematics which, when it shall have been thoroughly understood by

men of the illustrative type, and clothed by them with physical

imagery, will become, perhaps under some new name, a most powerful

method of communicating truly scientific knowledge to persons

apparently devoid of the calculating spirit.

The mutual action and reaction between the different departments of

human thought is so interesting to the student of scientific progress,

that, at the risk of still further encroaching on the valuable time of

the Section, I shall say a few words on a branch of physics which not

very long ago would have been considered rather a branch of

metaphysics. I mean the atomic theory, or, as it is now called, the

molecular theory of the constitution of bodies.

Not many years ago if we had been asked in what regions of physical

science the advance of discovery was least apparent, we should have

pointed to the hopelessly distant fixed stars on the one hand, and to

the inscrutable delicacy of the texture of material bodies on the

other.

Indeed, if we are to regard Comte as in any degree representing the

scientific opinion of his time, the research into what takes place

beyond our own solar system seemed then to be exceedingly unpromising,

if not altogether illusory.

The opinion that the bodies which we see and handle, which we can set

in motion or leave at rest, which we can break in pieces and destroy,

are composed of smaller bodies which we cannot see or handle, which

are always in motion, and which can neither be stopped nor broken in

pieces, nor in any way destroyed or deprived of the least of their

properties, was known by the name of the Atomic theory. It was

associated with the names of Democritus, Epicurus, and Lucretius, and

was commonly supposed to admit the existence only of atoms and void,

to the exclusion of any other basis of things from the universe.

In many physical reasonings and mathematical calculations we are

accustomed to argue as if such substances as air, water, or metal,

which appear to our senses uniform and continuous, were strictly and

mathematically uniform and continuous.

We know that we can divide a pint of water into many millions of

portions, each of which is as fully endowed with all the properties of

water as the whole pint was; and it seems only natural to conclude

that we might go on subdividing the water for ever, just as we can

never come to a limit in subdividing the space in which it is

contained. We have heard how Faraday divided a grain of gold into an

inconceivable number of separate particles, and we may see Dr Tyndall

produce from a mere suspicion of nitrite of butyle an immense cloud,

the minute visible portion of which is still cloud, and therefore must

contain many molecules of nitrite of butyle.

But evidence from different and independent sources is now crowding in

upon us which compels us to admit that if we could push the process of

subdivision still further we should come to a limit, because each

portion would then contain only one molecule, an individual body, one

and indivisible, unalterable by any power in nature.

Even in our ordinary experiments on very finely divided matter we find

that the substance is beginning to lose the properties which it

exhibits when in a large mass, and that effects depending on the

individual action of molecules are beginning to become prominent.

The study of these phenomena is at present the path which leads to the

development of molecular science.

That superficial tension of liquids which is called capillary

attraction is one of these phenomena. Another important class of

phenomena are those which are due to that motion of agitation by which

the molecules of a liquid or gas are continually working their way

from one place to another, and continually changing their course, like

people hustled in a crowd.

On this depends the rate of diffusion of gases and liquids through

each other, to the study of which, as one of the keys of molecular

science, that unwearied inquirer into nature's secrets, the late Prof.

Graham, devoted such arduous labour.

The rate of electrolytic conduction is, according to Wiedemann's

theory, influenced by the same cause; and the conduction of heat in

fluids depends probably on the same kind of action. In the case of

gases, a molecular theory has been developed by Clausius and others,

capable of mathematical treatment, and subjected to experimental

investigation; and by this theory nearly every known mechanical

property of gases has been explained on dynamical principles; so that

the properties of individual gaseous molecules are in a fair way to

become objects of scientific research.

Now Mr Stoney has pointed out[1] that the numerical results of

experiments on gases render it probable that the mean distance of

their particles at the ordinary temperature and pressure is a quantity

of the same order of magnitude as a millionth of a millimetre, and Sir

William Thomson has since[2] shewn, by several independent lines of

argument, drawn from phenomena so different in themselves as the

electrification of metals by contact, the tension of soap-bubbles, and

the friction of air, that in ordinary solids and liquids the average

distance between contiguous molecules is less than the

hundred-millionth, and greater than the two-thousand-millionth of a

centimetre.

[1] \_Phil. Mag.\_, Aug. 1868.

[2] \_Nature\_, March 31, 1870.

These, of course, are exceedingly rough estimates, for they are

derived from measurements some of which are still confessedly very

rough; but if at the present time, we can form even a rough plan for

arriving at results of this kind, we may hope that, as our means of

experimental inquiry become more accurate and more varied, our

conception of a molecule will become more definite, so that we may be

able at no distant period to estimate its weight with a greater degree

of precision.

A theory, which Sir W. Thomson has founded on Helmholtz's splendid

hydrodynamical theorems, seeks for the properties of molecules in the

ring vortices of a uniform, frictionless, incompressible fluid. Such

whirling rings may be seen when an experienced smoker sends out a

dexterous puff of smoke into the still air, but a more evanescent

phenomenon it is difficult to conceive. This evanescence is owing to

the viscosity of the air; but Helmholtz has shewn that in a perfect

fluid such a whirling ring, if once generated, would go on whirling

for ever, would always consist of the very same portion of the fluid

which was first set whirling, and could never be cut in two by any

natural cause. The generation of a ring-vortex is of course equally

beyond the power of natural causes, but once generated, it has the

properties of individuality, permanence in quantity, and

indestructibility. It is also the recipient of impulse and of energy,

which is all we can affirm of matter; and these ring-vortices are

capable of such varied connexions and knotted self-involutions, that

the properties of differently knotted vortices must be as different as

those of different kinds of molecules can be.

If a theory of this kind should be found, after conquering the

enormous mathematical difficulties of the subject, to represent in any

degree the actual properties of molecules, it will stand in a very

different scientific position from those theories of molecular action

which are formed by investing the molecule with an arbitrary system of

central forces invented expressly to account for the observed

phenomena.

In the vortex theory we have nothing arbitrary, no central forces or

occult properties of any other kind. We have nothing but matter and

motion, and when the vortex is once started its properties are all

determined from the original impetus, and no further assumptions are

possible.

Even in the present undeveloped state of the theory, the contemplation

of the individuality and indestructibility of a ring-vortex in a

perfect fluid cannot fail to disturb the commonly received opinion

that a molecule, in order to be permanent, must be a very hard body.

In fact one of the first conditions which a molecule must fulfil is,

apparently, inconsistent with its being a single hard body. We know

from those spectroscopic researches which have thrown so much light on

different branches of science, that a molecule can be set into a state

of internal vibration, in which it gives off to the surrounding medium

light of definite refrangibility--light, that is, of definite

wave-length and definite period of vibration. The fact that all the

molecules (say, of hydrogen) which we can procure for our experiments,

when agitated by heat or by the passage of an electric spark, vibrate

precisely in the same periodic time, or, to speak more accurately,

that their vibrations are composed of a system of simple vibrations

having always the same periods, is a very remarkable fact.

I must leave it to others to describe the progress of that splendid

series of spectroscopic discoveries by which the chemistry of the

heavenly bodies has been brought within the range of human inquiry. I

wish rather to direct your attention to the fact that, not only has

every molecule of terrestrial hydrogen the same system of periods of

free vibration, but that the spectroscopic examination of the light of

the sun and stars shews that, in regions the distance of which we can

only feebly imagine, there are molecules vibrating in as exact unison

with the molecules of terrestrial hydrogen as two tuning-forks tuned

to concert pitch, or two watches regulated to solar time.

Now this absolute equality in the magnitude of quantities, occurring

in all parts of the universe, is worth our consideration.

The dimensions of individual natural bodies are either quite

indeterminate, as in the case of planets, stones, trees, &c., or they

vary within moderate limits, as in the case of seeds, eggs, &c.; but

even in these cases small quantitative differences are met with which

do not interfere with the essential properties of the body.

Even crystals, which are so definite in geometrical form, are variable

with respect to their absolute dimensions.

Among the works of man we sometimes find a certain degree of

uniformity.

There is a uniformity among the different bullets which are cast in

the same mould, and the different copies of a book printed from the

same type.

If we examine the coins, or the weights and measures, of a civilized

country, we find a uniformity, which is produced by careful adjustment

to standards made and provided by the state. The degree of uniformity

of these national standards is a measure of that spirit of justice in

the nation which has enacted laws to regulate them and appointed

officers to test them.

This subject is one in which we, as a scientific body, take a warm

interest; and you are all aware of the vast amount of scientific work

which has been expended, and profitably expended, in providing weights

and measures for commercial and scientific purposes.

The earth has been measured as a basis for a permanent standard of

length, and every property of metals has been investigated to guard

against any alteration of the material standards when made. To weigh

or measure any thing with modern accuracy, requires a course of

experiment and calculation in which almost every branch of physics and

mathematics is brought into requisition.

Yet, after all, the dimensions of our earth and its time of rotation,

though, relatively to our present means of comparison, very permanent,

are not so by any physical necessity. The earth might contract by

cooling, or it might be enlarged by a layer of meteorites falling on

it, or its rate of revolution might slowly slacken, and yet it would

continue to be as much a planet as before.

But a molecule, say of hydrogen, if either its mass or its time of

vibration were to be altered in the least, would no longer be a

molecule of hydrogen.

If, then, we wish to obtain standards of length, time, and mass which

shall be absolutely permanent, we must seek them not in the

dimensions, or the motion, or the mass of our planet, but in the

wave-length, the period of vibration, and the absolute mass of these

imperishable and unalterable and perfectly similar molecules.

When we find that here, and in the starry heavens, there are

innumerable multitudes of little bodies of exactly the same mass, so

many, and no more, to the grain, and vibrating in exactly the same

time, so many times, and no more, in a second, and when we reflect

that no power in nature can now alter in the least either the mass or

the period of any one of them, we seem to have advanced along the path

of natural knowledge to one of those points at which we must accept

the guidance of that faith by which we understand that "that which is

seen was not made of things which do appear."

One of the most remarkable results of the progress of molecular

science is the light it has thrown on the nature of irreversible

processes--processes, that is, which always tend towards and never

away from a certain limiting state. Thus, if two gases be put into

the same vessel, they become mixed, and the mixture tends continually

to become more uniform. If two unequally heated portions of the same

gas are put into the vessel, something of the kind takes place, and

the whole tends to become of the same temperature. If two unequally

heated solid bodies be placed in contact, a continual approximation of

both to an intermediate temperature takes place.

In the case of the two gases, a separation may be effected by chemical

means; but in the other two cases the former state of things cannot be

restored by any natural process.

In the case of the conduction or diffusion of heat the process is not

only irreversible, but it involves the irreversible diminution of that

part of the whole stock of thermal energy which is capable of being

converted into mechanical work.

This is Thomson's theory of the irreversible dissipation of energy,

and it is equivalent to the doctrine of Clausius concerning the growth

of what he calls Entropy.

The irreversible character of this process is strikingly embodied in

Fourier's theory of the conduction of heat, where the formulae

themselves indicate, for all positive values of the time, a possible

solution which continually tends to the form of a uniform diffusion of

heat.

But if we attempt to ascend the stream of time by giving to its symbol

continually diminishing values, we are led up to a state of things in

which the formula has what is called a critical value; and if we

inquire into the state of things the instant before, we find that the

formula becomes absurd.

We thus arrive at the conception of a state of things which cannot be

conceived as the physical result of a previous state of things, and we

find that this critical condition actually existed at an epoch not in

the utmost depths of a past eternity, but separated from the present

time by a finite interval.

This idea of a beginning is one which the physical researches of

recent times have brought home to us, more than any observer of the

course of scientific thought in former times would have had reason to

expect.

But the mind of man is not, like Fourier's heated body, continually

settling down into an ultimate state of quiet uniformity, the

character of which we can already predict; it is rather like a tree,

shooting out branches which adapt themselves to the new aspects of the

sky towards which they climb, and roots which contort themselves among

the strange strata of the earth into which they delve. To us who

breathe only the spirit of our own age, and know only the

characteristics of contemporary thought, it is as impossible to

predict the general tone of the science of the future as it is to

anticipate the particular discoveries which it will make.

Physical research is continually revealing to us new features of

natural processes, and we are thus compelled to search for new forms

of thought appropriate to these features. Hence the importance of a

careful study of those relations between mathematics and Physics which

determine the conditions under which the ideas derived from one

department of physics may be safely used in forming ideas to be

employed in a new department.

The figure of speech or of thought by which we transfer the language

and ideas of a familiar science to one with which we are less

acquainted may be called Scientific Metaphor.

Thus the words Velocity, Momentum, Force, &c. have acquired certain

precise meanings in Elementary Dynamics. They are also employed in

the Dynamics of a Connected System in a sense which, though perfectly

analogous to the elementary sense, is wider and more general.

These generalized forms of elementary ideas may be called metaphorical

terms in the sense in which every abstract term is metaphorical. The

characteristic of a truly scientific system of metaphors is that each

term in its metaphorical use retains all the formal relations to the

other terms of the system which it had in its original use. The

method is then truly scientific--that is, not only a legitimate

product of science, but capable of generating science in its turn.

There are certain electrical phenomena, again, which are connected

together by relations of the same form as those which connect

dynamical phenomena. To apply to these the phrases of dynamics with

proper distinctions and provisional reservations is an example of a

metaphor of a bolder kind; but it is a legitimate metaphor if it

conveys a true idea of the electrical relations to those who have been

already trained in dynamics.

Suppose, then, that we have successfully introduced certain ideas

belonging to an elementary science by applying them metaphorically to

some new class of phenomena. It becomes an important philosophical

question to determine in what degree the applicability of the old

ideas to the new subject may be taken as evidence that the new

phenomena are physically similar to the old.

The best instances for the determination of this question are those in

which two different explanations have been given of the same thing.

The most celebrated case of this kind is that of the corpuscular and

the undulatory theories of light. Up to a certain point the phenomena

of light are equally well explained by both; beyond this point, one of

them fails.

To understand the true relation of these theories in that part of the

field where they seem equally applicable we must look at them in the

light which Hamilton has thrown upon them by his discovery that to

every brachistochrone problem there corresponds a problem of free

motion, involving different velocities and times, but resulting in the

same geometrical path. Professor Tait has written a very interesting

paper on this subject.

According to a theory of electricity which is making great progress in

Germany, two electrical particles act on one another directly at a

distance, but with a force which, according to Weber, depends on their

relative velocity, and according to a theory hinted at by Gauss, and

developed by Riemann, Lorenz, and Neumann, acts not instantaneously,

but after a time depending on the distance. The power with which this

theory, in the hands of these eminent men, explains every kind of

electrical phenomena must be studied in order to be appreciated.

Another theory of electricity, which I prefer, denies action at a

distance and attributes electric action to tensions and pressures in

an all-pervading medium, these stresses being the same in kind with

those familiar to engineers, and the medium being identical with that

in which light is supposed to be propagated.

Both these theories are found to explain not only the phenomena by the

aid of which they were originally constructed, but other phenomena,

which were not thought of or perhaps not known at the time; and both

have independently arrived at the same numerical result, which gives

the absolute velocity of light in terms of electrical quantities.

That theories apparently so fundamentally opposed should have so large

a field of truth common to both is a fact the philosophical importance

of which we cannot fully appreciate till we have reached a scientific

altitude from which the true relation between hypotheses so different

can be seen.

I shall only make one more remark on the relation between Mathematics

and Physics. In themselves, one is an operation of the mind, the

other is a dance of molecules. The molecules have laws of their own,

some of which we select as most intelligible to us and most amenable

to our calculation. We form a theory from these partial data, and we

ascribe any deviation of the actual phenomena from this theory to

disturbing causes. At the same time we confess that what we call

disturbing causes are simply those parts of the true circumstances

which we do not know or have neglected, and we endeavour in future to

take account of them. We thus acknowledge that the so-called

disturbance is a mere figment of the mind, not a fact of nature, and

that in natural action there is no disturbance.

But this is not the only way in which the harmony of the material with

the mental operation may be disturbed. The mind of the mathematician

is subject to many disturbing causes, such as fatigue, loss of memory,

and hasty conclusions; and it is found that, from these and other

causes, mathematicians make mistakes.

I am not prepared to deny that, to some mind of a higher order than

ours, each of these errors might be traced to the regular operation of

the laws of actual thinking; in fact we ourselves often do detect, not

only errors of calculation, but the causes of these errors. This,

however, by no means alters our conviction that they are errors, and

that one process of thought is right and another process wrong. I

One of the most profound mathematicians and thinkers of our time, the

late George Boole, when reflecting on the precise and almost

mathematical character of the laws of right thinking as compared with

the exceedingly perplexing though perhaps equally determinate laws of

actual and fallible thinking, was led to another of those points of

view from which Science seems to look out into a region beyond her own

domain.

"We must admit," he says, "that there exist laws" (of thought) "which

even the rigour of their mathematical forms does not preserve from

violation. We must ascribe to them an authority, the essence of which

does not consist in power, a supremacy which the analogy of the

inviolable order of the natural world in no way assists us to

comprehend."

Introductory Lecture on Experimental Physics.

James Clerk Maxwell

The University of Cambridge, in accordance with that law of its

evolution, by which, while maintaining the strictest continuity

between the successive phases of its history, it adapts itself with

more or less promptness to the requirements of the times, has lately

instituted a course of Experimental Physics. This course of study,

while it requires us to maintain in action all those powers of

attention and analysis which have been so long cultivated in the

University, calls on us to exercise our senses in observation, and our

hands in manipulation. The familiar apparatus of pen, ink, and paper

will no longer be sufficient for us, and we shall require more room

than that afforded by a seat at a desk, and a wider area than that of

the black board. We owe it to the munificence of our Chancellor,

that, whatever be the character in other respects of the experiments

which we hope hereafter to conduct, the material facilities for their

full development will be upon a scale which has not hitherto been

surpassed.

The main feature, therefore, of Experimental Physics at Cambridge is

the Devonshire Physical Laboratory, and I think it desirable that on

the present occasion, before we enter on the details of any special

study, we should consider by what means we, the University of

Cambridge, may, as a living body, appropriate and vitalise this new

organ, the outward shell of which we expect soon to rise before us.

The course of study at this University has always included Natural

Philosophy, as well as Pure Mathematics. To diffuse a sound knowledge

of Physics, and to imbue the minds of our students with correct

dynamical principles, have been long regarded as among our highest

functions, and very few of us can now place ourselves in the mental

condition in which even such philosophers as the great Descartes were

involved in the days before Newton had announced the true laws of the

motion of bodies. Indeed the cultivation and diffusion of sound

dynamical ideas has already effected a great change in the language

and thoughts even of those who make no pretensions to science, and we

are daily receiving fresh proofs that the popularisation of scientific

doctrines is producing as great an alteration in the mental state of

society as the material applications of science are effecting in its

outward life. Such indeed is the respect paid to science, that the

most absurd opinions may become current, provided they are expressed

in language, the sound of which recals some well-known scientific

phrase. If society is thus prepared to receive all kinds of

scientific doctrines, it is our part to provide for the diffusion and

cultivation, not only of true scientific principles, but of a spirit

of sound criticism, founded on an examination of the evidences on

which statements apparently scientific depend.

When we shall be able to employ in scientific education, not only the

trained attention of the student, and his familiarity with symbols,

but the keenness of his eye, the quickness of his ear, the delicacy of

his touch, and the adroitness of his fingers, we shall not only extend

our influence over a class of men who are not fond of cold

abstractions, but, by opening at once all the gateways of knowledge,

we shall ensure the association of the doctrines of science with those

elementary sensations which form the obscure background of all our

conscious thoughts, and which lend a vividness and relief to ideas,

which, when presented as mere abstract terms, are apt to fade entirely

from the memory.

In a course of Experimental Physics we may consider either the Physics

or the Experiments as the leading feature. We may either employ the

experiments to illustrate the phenomena of a particular branch of

Physics, or we may make some physical research in order to exemplify a

particular experimental method. In the order of time, we should

begin, in the Lecture Room, with a course of lectures on some branch

of Physics aided by experiments of illustration, and conclude, in the

Laboratory, with a course of experiments of research.

Let me say a few words on these two classes of

experiments,--Experiments of Illustration and Experiments of Research.

The aim of an experiment of illustration is to throw light upon some

scientific idea so that the student may be enabled to grasp it. The

circumstances of the experiment are so arranged that the phenomenon

which we wish to observe or to exhibit is brought into prominence,

instead of being obscured and entangled among other phenomena, as it

is when it occurs in the ordinary course of nature. To exhibit

illustrative experiments, to encourage others to make them, and to

cultivate in every way the ideas on which they throw light, forms an

important part of our duty. The simpler the materials of an

illustrative experiment, and the more familiar they are to the

student, the more thoroughly is he likely to acquire the idea which it

is meant to illustrate. The educational value of such experiments is

often inversely proportional to the complexity of the apparatus. The

student who uses home-made apparatus, which is always going wrong,

often learns more than one who has the use of carefully adjusted

instruments, to which he is apt to trust, and which he dares not take

to pieces.

It is very necessary that those who are trying to learn from books the

facts of physical science should be enabled by the help of a few

illustrative experiments to recognise these facts when they meet with

them out of doors. Science appears to us with a very different aspect

after we have found out that it is not in lecture rooms only, and by

means of the electric light projected on a screen, that we may witness

physical phenomena, but that we may find illustrations of the highest

doctrines of science in games and gymnastics, in travelling by land

and by water, in storms of the air and of the sea, and wherever there

is matter in motion.

This habit of recognising principles amid the endless variety of their

action can never degrade our sense of the sublimity of nature, or mar

our enjoyment of its beauty. On the contrary, it tends to rescue our

scientific ideas from that vague condition in which we too often leave

them, buried among the other products of a lazy credulity, and to

raise them into their proper position among the doctrines in which our

faith is so assured, that we are ready at all times to act on them.

Experiments of illustration may be of very different kinds. Some may

be adaptations of the commonest operations of ordinary life, others

may be carefully arranged exhibitions of some phenomenon which occurs

only under peculiar conditions. They all, however, agree in this,

that their aim is to present some phenomenon to the senses of the

student in such a way that he may associate with it the appropriate

scientific idea. When he has grasped this idea, the experiment which

illustrates it has served its purpose.

In an experiment of research, on the other hand, this is not the

principal aim. It is true that an experiment, in which the principal

aim is to see what happens under certain conditions, may be regarded

as an experiment of research by those who are not yet familiar with

the result, but in experimental researches, strictly so called, the

ultimate object is to measure something which we have already seen--to

obtain a numerical estimate of some magnitude.

Experiments of this class--those in which measurement of some kind is

involved, are the proper work of a Physical Laboratory. In every

experiment we have first to make our senses familiar with the

phenomenon, but we must not stop here, we must find out which of its

features are capable of measurement, and what measurements are

required in order to make a complete specification of the phenomenon.

We must then make these measurements, and deduce from them the result

which we require to find.

This characteristic of modern experiments--that they consist

principally of measurements,--is so prominent, that the opinion seems

to have got abroad, that in a few years all the great physical

constants will have been approximately estimated, and that the only

occupation which will then be left to men of science will be to carry

on these measurements to another place of decimals.

If this is really the state of things to which we are approaching, our

Laboratory may perhaps become celebrated as a place of conscientious

labour and consummate skill, but it will be out of place in the

University, and ought rather to be classed with the other great

workshops of our country, where equal ability is directed to more

useful ends.

But we have no right to think thus of the unsearchable riches of

creation, or of the untried fertility of those fresh minds into which

these riches will continue to be poured. It may possibly be true

that, in some of those fields of discovery which lie open to such

rough observations as can be made without artificial methods, the

great explorers of former times have appropriated most of what is

valuable, and that the gleanings which remain are sought after, rather

for their abstruseness, than for their intrinsic worth. But the

history of science shews that even during that phase of her progress

in which she devotes herself to improving the accuracy of the

numerical measurement of quantities with which she has long been

familiar, she is preparing the materials for the subjugation of new

regions, which would have remained unknown if she had been contented

with the rough methods of her early pioneers. I might bring forward

instances gathered from every branch of science, shewing how the

labour of careful measurement has been rewarded by the discovery of

new fields of research, and by the development of new scientific

ideas. But the history of the science of terrestrial magnetism

affords us a sufficient example of what may be done by Experiments in

Concert, such as we hope some day to perform in our Laboratory.

That celebrated traveller, Humboldt, was profoundly impressed with the

scientific value of a combined effort to be made by the observers of

all nations, to obtain accurate measurements of the magnetism of the

earth; and we owe it mainly to his enthusiasm for science, his great

reputation and his wide-spread influence, that not only private men of

science, but the governments of most of the civilised nations, our own

among the number, were induced to take part in the enterprise. But

the actual working out of the scheme, and the arrangements by which

the labours of the observers were so directed as to obtain the best

results, we owe to the great mathematician Gauss, working along with

Weber, the future founder of the science of electro-magnetic

measurement, in the magnetic observatory of Gottingen, and aided by

the skill of the instrument-maker Leyser. These men, however, did not

work alone. Numbers of scientific men joined the Magnetic Union,

learned the use of the new instruments and the new methods of reducing

the observations; and in every city of Europe you might see them, at

certain stated times, sitting, each in his cold wooden shed, with his

eye fixed at the telescope, his ear attentive to the clock, and his

pencil recording in his note-book the instantaneous position of the

suspended magnet.

Bacon's conception of "Experiments in concert" was thus realised, the

scattered forces of science were converted into a regular army, and

emulation and jealousy became out of place, for the results obtained

by any one observer were of no value till they were combined with

those of the others.

The increase in the accuracy and completeness of magnetic observations

which was obtained by the new method, opened up fields of research

which were hardly suspected to exist by those whose observations of

the magnetic needle had been conducted in a more primitive manner. We

must reserve for its proper place in our course any detailed

description of the disturbances to which the magnetism of our planet

is found to be subject. Some of these disturbances are periodic,

following the regular courses of the sun and moon. Others are sudden,

and are called magnetic storms, but, like the storms of the

atmosphere, they have their known seasons of frequency. The last and

the most mysterious of these magnetic changes is that secular

variation by which the whole character of the earth, as a great

magnet, is being slowly modified, while the magnetic poles creep on,

from century to century, along their winding track in the polar

regions.

We have thus learned that the interior of the earth is subject to the

influences of the heavenly bodies, but that besides this there is a

constantly progressive change going on, the cause of which is entirely

unknown. In each of the magnetic observatories throughout the world

an arrangement is at work, by means of which a suspended magnet

directs a ray of light on a preparred sheet of paper moved by

clockwork. On that paper the never-resting heart of the earth is now

tracing, in telegraphic symbols which will one day be interpreted, a

record of its pulsations and its flutterings, as well as of that slow

but mighty working which warns us that we must not suppose that the

inner history of our planet is ended.

But this great experimental research on Terrestrial Magnetism produced

lasting effects on the progress of science in general. I need only

mention one or two instances. The new methods of measuring forces

were successfully applied by Weber to the numerical determination of

all the phenomena of electricity, and very soon afterwards the

electric telegraph, by conferring a commercial value on exact

numerical measurements, contributed largely to the advancement, as

well as to the diffusion of scientific knowledge.

But it is not in these more modern branches of science alone that this

influence is felt. It is to Gauss, to the Magnetic Union, and to

magnetic observers in general, that we owe our deliverance from that

absurd method of estimating forces by a variable standard which

prevailed so long even among men of science. It was Gauss who first

based the practical measurement of magnetic force (and therefore of

every other force) on those long established principles, which, though

they are embodied in every dynamical equation, have been so generally

set aside, that these very equations, though correctly given in our

Cambridge textbooks, are usually explained there by assuming, in

addition to the variable standard of force, a variable, and therefore

illegal, standard of mass.

Such, then, were some of the scientific results which followed in this

case from bringing together mathematical power, experimental sagacity,

and manipulative skill, to direct and assist the labours of a body of

zealous observers. If therefore we desire, for our own advantage and

for the honour of our University, that the Devonshire Laboratory

should be successful, we must endeavour to maintain it in living union

with the other organs and faculties of our learned body. We shall

therefore first consider the relation in which we stand to those

mathematical studies which have so long flourished among us, which

deal with our own subjects, and which differ from our experimental

studies only in the mode in which they are presented to the mind.

There is no more powerful method for introducing knowledge into the

mind than that of presenting it in as many different ways as we can.

When the ideas, after entering through different gateways, effect a

junction in the citadel of the mind, the position they occupy becomes

impregnable. Opticians tell us that the mental combination of the

views of an object which we obtain from stations no further apart than

our two eyes is sufficient to produce in our minds an impression of

the solidity of the object seen; and we find that this impression is

produced even when we are aware that we are really looking at two flat

pictures placed in a stereoscope. It is therefore natural to expect

that the knowledge of physical science obtained by the combined use of

mathematical analysis and experimental research will be of a more

solid, available, and enduring kind than that possessed by the mere

mathematician or the mere experimenter.

But what will be the effect on the University, if men Pursuing that

course of reading which has produced so many distinguished Wranglers,

turn aside to work experiments? Will not their attendance at the

Laboratory count not merely as time withdrawn from their more

legitimate studies, but as the introduction of a disturbing element,

tainting their mathematical conceptions with material imagery, and

sapping their faith in the formulae of the textbook? Besides this, we

have already heard complaints of the undue extension of our studies,

and of the strain put upon our questionists by the weight of learning

which they try to carry with them into the Senate-House. If we now

ask them to get up their subjects not only by books and writing, but

at the same time by observation and manipulation, will they not break

down altogether? The Physical Laboratory, we are told, may perhaps be

useful to those who are going out in Natural Science, and who do

not take in Mathematics, but to attempt to combine both kinds of study

during the time of residence at the University is more than one mind

can bear.

No doubt there is some reason for this feeling. Many of us have

already overcome the initial difficulties of mathematical training.

When we now go on with our study, we feel that it requires exertion

and involves fatigue, but we are confident that if we only work hard

our progress will be certain.

Some of us, on the other hand, may have had some experience of the

routine of experimental work. As soon as we can read scales, observe

times, focus telescopes, and so on, this kind of work ceases to

require any great mental effort. We may perhaps tire our eyes and

weary our backs, but we do not greatly fatigue our minds.

It is not till we attempt to bring the theoretical part of our

training into contact with the practical that we begin to experience

the full effect of what Faraday has called "mental inertia"--not only

the difficulty of recognising, among the concrete objects before us,

the abstract relation which we have learned from books, but the

distracting pain of wrenching the mind away from the symbols to the

objects, and from the objects back to the symbols. This however is

the price we have to pay for new ideas.

But when we have overcome these difficulties, and successfully bridged

over the gulph between the abstract and the concrete, it is not a mere

piece of knowledge that we have obtained: we have acquired the

rudiment of a permanent mental endowment. When, by a repetition of

efforts of this kind, we have more fully developed the scientific

faculty, the exercise of this faculty in detecting scientific

principles in nature, and in directing practice by theory, is no

longer irksome, but becomes an unfailing source of enjoyment, to which

we return so often, that at last even our careless thoughts begin to

run in a scientific channel.

I quite admit that our mental energy is limited in quantity, and I

know that many zealous students try to do more than is good for them.

But the question about the introduction of experimental study is not

entirely one of quantity. It is to a great extent a question of

distribution of energy. Some distributions of energy, we know, are

more useful than others, because they are more available for those

purposes which we desire to accomplish.

Now in the case of study, a great part of our fatigue often arises,

not from those mental efforts by which we obtain the mastery of the

subject, but from those which are spent in recalling our wandering

thoughts; and these efforts of attention would be much less fatiguing

if the disturbing force of mental distraction could be removed.

This is the reason why a man whose soul is in his work always makes

more progress than one whose aim is something not immediately

connected with his occupation. In the latter case the very motive of

which he makes use to stimulate his flagging powers becomes the means

of distracting his mind from the work before him.

There may be some mathematicians who pursue their studies entirely for

their own sake. Most men, however, think that the chief use of

mathematics is found in the interpretation of nature. Now a man who

studies a piece of mathematics in order to understand some natural

phenomenon which he has seen, or to calculate the best arrangement of

some experiment which he means to make, is likely to meet with far

less distraction of mind than if his sole aim had been to sharpen his

mind for the successful practice of the Law, or to obtain a high place

in the Mathematical Tripos.

I have known men, who when they were at school, never could see the

good of mathematics, but who, when in after life they made this

discovery, not only became eminent as scientific engineers, but made

considerable progress in the study of abstract mathematics. If our

experimental course should help any of you to see the good of

mathematics, it will relieve us of much anxiety, for it will not only

ensure the success of your future studies, but it will make it much

less likely that they will prove injurious to your health.

But why should we labour to prove the advantage of practical science

to the University? Let us rather speak of the help which the

University may give to science, when men well trained in mathematics

and enjoying the advantages of a well-appointed Laboratory, shall

unite their efforts to carry out some experimental research which no

solitary worker could attempt.

At first it is probable that our principal experimental work must be

the illustration of particular branches of science, but as we go on we

must add to this the study of scientific methods, the same method

being sometimes illustrated by its application to researches belonging

to different branches of science.

We might even imagine a course of experimental study the arrangement

of which should be founded on a classification of methods, and not on

that of the objects of investigation. A combination of the two plans

seems to me better than either, and while we take every opportunity of

studying methods, we shall take care not to dissociate the method from

the scientific research to which it is applied, and to which it owes

its value.

We shall therefore arrange our lectures according to the

classification of the principal natural phenomena, such as heat,

electricity, magnetism and so on.

In the laboratory, on the other hand, the place of the different

instruments will be determined by a classification according to

methods, such as weighing and measuring, observations of time, optical

and electrical methods of observation, and so on.

The determination of the experiments to be performed at a particular

time must often depend upon the means we have at command, and in the

case of the more elaborate experiments, this may imply a long time of

preparation, during which the instruments, the methods, and the

observers themselves, are being gradually fitted for their work. When

we have thus brought together the requisites, both material and

intellectual, for a particular experiment, it may sometimes be

desirable that before the instruments are dismounted and the observers

dispersed, we should make some other experiment, requiring the same

method, but dealing perhaps with an entirely different class of

physical phenomena.

Our principal work, however, in the Laboratory must be to acquaint

ourselves with all kinds of scientific methods, to compare them, and

to estimate their value. It will, I think, be a result worthy of our

University, and more likely to be accomplished here than in any

private laboratory, if, by the free and full discussion of the

relative value of different scientific procedures, we succeed in

forming a school of scientific criticism, and in assisting the

development of the doctrine of method.

But admitting that a practical acquaintance with the methods of

Physical Science is an essential part of a mathematical and scientific

education, we may be asked whether we are not attributing too much

importance to science altogether as part of a liberal education.

Fortunately, there is no question here whether the University should

continue to be a place of liberal education, or should devote itself

to preparing young men for particular professions. Hence though some

of us may, I hope, see reason to make the pursuit of science the main

business of our lives, it must be one of our most constant aims to

maintain a living connexion between our work and the other liberal

studies of Cambridge, whether literary, philological, historical or

philosophical.

There is a narrow professional spirit which may grow up among men of

science, just as it does among men who practise any other special

business. But surely a University is the very place where we should

be able to overcome this tendency of men to become, as it were,

granulated into small worlds, which are all the more worldly for their

very smallness. We lose the advantage of having men of varied

pursuits collected into one body, if we do not endeavour to imbibe

some of the spirit even of those whose special branch of learning is

different from our own.

It is not so long ago since any man who devoted himself to geometry,

or to any science requiring continued application, was looked upon as

necessarily a misanthrope, who must have abandoned all human

interests, and betaken himself to abstractions so far removed from the

world of life and action that he has become insensible alike to the

attractions of pleasure and to the claims of duty.

In the present day, men of science are not looked upon with the same

awe or with the same suspicion. They are supposed to be in league

with the material spirit of the age, and to form a kind of advanced

Radical party among men of learning.

We are not here to defend literary and historical studies. We admit

that the proper study of mankind is man. But is the student of

science to be withdrawn from the study of man, or cut off from every

noble feeling, so long as he lives in intellectual fellowship with men

who have devoted their lives to the discovery of truth, and the

results of whose enquiries have impressed themselves on the ordinary

speech and way of thinking of men who never heard their names? Or is

the student of history and of man to omit from his consideration the

history of the origin and diffusion of those ideas which have produced

so great a difference between one age of the world and another?

It is true that the history of science is very different from the

science of history. We are not studying or attempting to study the

working of those blind forces which, we are told, are operating on

crowds of obscure people, shaking principalities and powers, and

compelling reasonable men to bring events to pass in an order laid

down by philosophers.

The men whose names are found in the history of science are not mere

hypothetical constituents of a crowd, to be reasoned upon only in

masses. We recognise them as men like ourselves, and their actions

and thoughts, being more free from the influence of passion, and

recorded more accurately than those of other men, are all the better

materials for the study of the calmer parts of human nature.

But the history of science is not restricted to the enumeration of

successful investigations. It has to tell of unsuccessful inquiries,

and to explain why some of the ablest men have failed to find the key

of knowledge, and how the reputation of others has only given a firmer

footing to the errors into which they fell.

The history of the development, whether normal or abnormal, of ideas

is of all subjects that in which we, as thinking men, take the deepest

interest. But when the action of the mind passes out of the

intellectual stage, in which truth and error are the alternatives,

into the more violently emotional states of anger and passion, malice

and envy, fury and madness; the student of science, though he is

obliged to recognise the powerful influence which these wild forces

have exercised on mankind, is perhaps in some measure disqualified

from pursuing the study of this part of human nature.

But then how few of us are capable of deriving profit from such

studies. We cannot enter into full sympathy with these lower phases

of our nature without losing some of that antipathy to them which is

our surest safeguard against a reversion to a meaner type, and we

gladly return to the company of those illustrious men who by aspiring

to noble ends, whether intellectual or practical, have risen above the

region of storms into a clearer atmosphere, where there is no

misrepresentation of opinion, nor ambiguity of expression, but where

one mind comes into closest contact with another at the point where

both approach nearest to the truth.

I propose to lecture during this term on Heat, and, as our facilities

for experimental work are not yet fully developed, I shall endeavour

to place before you the relative position and scientific connexion of

the different branches of the science, rather than to discuss the

details of experimental methods.

We shall begin with Thermometry, or the registration of temperatures,

and Calorimetry, or the measurement of quantities of heat. We shall

then go on to Thermodynamics, which investigates the relations between

the thermal properties of bodies and their other dynamical properties,

in so far as these relations may be traced without any assumption as

to the particular constitution of these bodies.

The principles of Thermodynamics throw great light on all the

phenomena of nature, and it is probable that many valuable

applications of these principles have yet to be made; but we shall

have to point out the limits of this science, and to shew that many

problems in nature, especially those in which the Dissipation of

Energy comes into play, are not capable of solution by the principles

of Thermodynamics alone, but that in order to understand them, we are

obliged to form some more definite theory of the constitution of

bodies.

Two theories of the constitution of bodies have struggled for victory

with various fortunes since the earliest ages of speculation: one is

the theory of a universal plenum, the other is that of atoms and void.

The theory of the plenum is associated with the doctrine of

mathematical continuity, and its mathematical methods are those of the

Differential Calculus, which is the appropriate expression of the

relations of continuous quantity.

The theory of atoms and void leads us to attach more importance to the

doctrines of integral numbers and definite proportions; but, in

applying dynamical principles to the motion of immense numbers of

atoms, the limitation of our faculties forces us to abandon the

attempt to express the exact history of each atom, and to be content

with estimating the average condition of a group of atoms large enough

to be visible. This method of dealing with groups of atoms, which I

may call the statistical method, and which in the present state of our

knowledge is the only available method of studying the properties of

real bodies, involves an abandonment of strict dynamical principles,

and an adoption of the mathematical methods belonging to the theory of

probability. It is probable that important results will be obtained

by the application of this method, which is as yet little known and is

not familiar to our minds. If the actual history of Science had been

different, and if the scientific doctrines most familiar to us had

been those which must be expressed in this way, it is possible that we

might have considered the existence of a certain kind of contingency a

self-evident truth, and treated the doctrine of philosophical

necessity as a mere sophism.

About the beginning of this century, the properties of bodies were

investigated by several distinguished French mathematicians on the

hypothesis that they are systems of molecules in equilibrium. The

somewhat unsatisfactory nature of the results of these investigations

produced, especially in this country, a reaction in favour of the

opposite method of treating bodies as if they were, so far at least as

our experiments are concerned, truly continuous. This method, in the

hands of Green, Stokes, and others, has led to results, the value of

which does not at all depend on what theory we adopt as to the

ultimate constitution of bodies.

One very important result of the investigation of the properties of

bodies on the hypothesis that they are truly continuous is that it

furnishes us with a test by which we can ascertain, by experiments on

a real body, to what degree of tenuity it must be reduced before it

begins to give evidence that its properties are no longer the same as

those of the body in mass. Investigations of this kind, combined with

a study of various phenomena of diffusion and of dissipation of

energy, have recently added greatly to the evidence in favour of the

hypothesis that bodies are systems of molecules in motion.

I hope to be able to lay before you in the course of the term some of

the evidence for the existence of molecules, considered as individual

bodies having definite properties. The molecule, as it is presented to

the scientific imagination, is a very different body from any of those

with which experience has hitherto made us acquainted.

In the first place its mass, and the other constants which define its

properties, are absolutely invariable; the individual molecule can

neither grow nor decay, but remains unchanged amid all the changes of

the bodies of which it may form a constituent.

In the second place it is not the only molecule of its kind, for there

are innumerable other molecules, whose constants are not

approximately, but absolutely identical with those of the first

molecule, and this whether they are found on the earth, in the sun, or

in the fixed stars.

By what process of evolution the philosophers of the future will

attempt to account for this identity in the properties of such a

multitude of bodies, each of them unchangeable in magnitude, and some

of them separated from others by distances which Astronomy attempts in

vain to measure, I cannot conjecture. My mind is limited in its power

of speculation, and I am forced to believe that these molecules must

have been made as they are from the beginning of their existence.

I also conclude that since none of the processes of nature, during

their varied action on different individual molecules, have produced,

in the course of ages, the slightest difference between the properties

of one molecule and those of another, the history of whose

combinations has been different, we cannot ascribe either their

existence or the identity of their properties to the operation of any

of those causes which we call natural.

Is it true then that our scientific speculations have really

penetrated beneath the visible appearance of things, which seem to be

subject to generation and corruption, and reached the entrance of that

world of order and perfection, which continues this day as it was

created, perfect in number and measure and weight?

We may be mistaken. No one has as yet seen or handled an individual

molecule, and our molecular hypothesis may, in its turn, be supplanted

by some new theory of the constitution of matter; but the idea of the

existence of unnumbered individual things, all alike and all

unchangeable, is one which cannot enter the human mind and remain

without fruit.

But what if these molecules, indestructible as they are, turn out to

be not substances themselves, but mere affections of some other

substance?

According to Sir W. Thomson's theory of Vortex Atoms, the substance of

which the molecule consists is a uniformly dense \_plenum\_, the

properties of which are those of a perfect fluid, the molecule itself

being nothing but a certain motion impressed on a portion of this

fluid, and this motion is shewn, by a theorem due to Helmholtz, to be

as indestructible as we believe a portion of matter to be.

If a theory of this kind is true, or even if it is conceivable, our

idea of matter may have been introduced into our minds through our

experience of those systems of vortices which we call bodies, but

which are not substances, but motions of a substance; and yet the idea

which we have thus acquired of matter, as a substance possessing

inertia, may be truly applicable to that fluid of which the vortices

are the motion, but of whose existence, apart from the vortical motion

of some of its parts, our experience gives us no evidence whatever.

It has been asserted that metaphysical speculation is a thing of the

past, and that physical science has extirpated it. The discussion of

the categories of existence, however, does not appear to be in danger

of coming to an end in our time, and the exercise of speculation

continues as fascinating to every fresh mind as it was in the days of

Thales.

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