SOME UNRECOGNIZED

LAWS OF NATURE

‘Willst du in’s Unendliche schreiten,
Geh’ nur im Endlichen nach allen Seiten.’

‘Willst du dich am Ganzen erquicken,
So musst du das Ganze im Kleinsten erblicken.’

GOETHE.

SOME UNRECOGNIZED
LAWS OF NATURE

AN INQUIRY INTO

THE CAUSES OF PHYSICAL PHENOMENA

WITH SPECIAL REFERENCE TO GRAVITATION

BY

IGNATIUS SINGER

AND

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WITH ILLUSTRATIONS

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PREFACE

AT last, after years of patient plodding in dim regions, where the footprints are few and the pitfalls many, the time has arrived when we are enabled to place before the world of science the first-fruits of our explorations.

We do so with mixed feelings of confidence and fear. With confidence, because we feel sure that we have struck a mine containing a rich vein of scientific truth. With fear, because the specimens we have brought with us might possibly not fairly represent the richness of the mine itself. In that case the fault would have to be ascribed entirely to ourselves. Not, indeed, that we could plead guilty to a lack of industry so much as to lack of ability and light to discriminate with that exacting precision, required by science, which, difficult of attainment under ordinary conditions, is almost impossible in exploring those hidden and all but trackless recesses of nature where so many abler men have failed for want of guiding finger-posts.

We have brought with us plenty of ore; but, before its value can finally be assessed, it will have to undergo the necessary refining process of criticism and verification. What this value will be after the unavoidable dross has been carefully separated from the more valuable portions, time and patience alone can decide. But, taking the most unfavourable view, if nothing but the four cardinal principles, on which all our reasoning is based, should survive—and concerning these we entertain no doubt—they should be a sufficient justification for our venturing before the public with this work. It has been an honest endeavour, and the book now before the public is what our utmost efforts could make it. Would that its quality were in proportion to the labour bestowed on it!

We have nothing to add in the nature of explanatory notices; for all we had to say or could say, or at least all we have deemed it necessary—and, possibly our readers may think, more than was judicious—to say, we have said in the book itself. Yet are we glad of the time-honoured custom which confers on authors the privilege of saying a few commendatory or apologetic words, as the case may be, on presenting their works to the public. In the present case it is not for the sake of mere conventionality that we appeal to the reader's patience and indulgence. No doubt the former will be taxed at times to the utmost straining point. For not only has it been our misfortune to have to criticize the works, and question the conclusions, of the greatest philosophers known to science, but it is our painful duty to impeach beforehand the tribunal before which new theories are generally tried. This tribunal is what is generally called the 'Mind' (writ large). And we will take this opportunity to briefly state our case against it.

'Whatever is contrary to reason,' said a well-known writer, 'is absurd.' If this maxim be accepted, the reader is bound to find many absurdities in these pages; hence we are anxious to discredit it as it stands. It would be more correct to say that the 'Mind' (or 'reason') regards as absurd anything that does not agree with itself; i.e. with accepted conceptions. But accepted conceptions are not necessarily true; and, therefore, absurdity is not necessarily synonymous with untruth. The announcement of the heliocentric theory was regarded as 'absurd,' and the doctrine of a negative weight as 'reasonable.' Yet of these the former has been found to be true and the latter false. The former seemed absurd for no other reason than because it conflicted with accepted theories; and the latter seemed reasonable because it was in harmony with the then current belief that, by fire, spirits were expelled from bodies, leaving behind the mere *calx*. In neither case had *the facts* bearing on it anything to do with the judgement. In the court of human reason facts will always prove or disprove whatever *the Mind*, the chief arbiter, approves or disapproves of. And this Mind—we crave indulgence, patient reader!—is but the accumulated experience of the past; a bundle of more or less coherent CONCEPTIONS, *some* of which *may* be true, many of which are certainly false.

In submitting, therefore, what is here set forth to the judgement of your *conceptions*, we pray you to remember that our chief accusation is against *them* (the conceptions), and therefore these should be banished from the judgement seat and ordered into the dock to take their place at the side of the theories here propounded.

We would urge the reader to remember that all philosophy (therefore including all scientific theories) deals with the *interpretations* of facts, and not with the facts themselves. The facts may be true, and the interpretations false. Now, in the present work, we do not question the truth of any known facts, i.e. of any actually observed phenomena, but merely the correctness of their *interpretations*.

Whenever, therefore, the reader feels inclined to lay the book down on account of what may seem to him to be an absurdity, we would urge on him the necessity to reflect for a moment as to why some statement seems absurd. In not a few instances he will find, perhaps to his own surprise, that the absurdity of a statement is simply due to its being contrary to some accepted notion, which the reader has . . . or, we should rather say, which has taken hold of the reader's mind because it was already in possession of the minds of his forefathers. On examining such conceptions, the merest reflection will often reveal that they are based neither on fact nor on theory, but have been handed down with the language from the remote past. Thus a mere meaningless phrase is often regarded as the most obvious fact, simply because we have been familiar with it from childhood, and have never thought of questioning it. What could seem a more obvious fact than the existence of *matter*? and yet, once examined, what could be more hypothetical?

It is against this tyranny of words and rooted ideas that we appeal to the reader's protection. Of facts we are not afraid. If, in every case where our conclusions disagree with current theories, the facts themselves be appealed to, we shall rest content, whatever may be the verdict. Errors there may be in the book—are, in fact, sure to be; for who, of mortals, has ever written accounts of hitherto unexplored regions without committing errors of detail? But as to the chief points—the four primary principles of PERSISTENCE, RESISTANCE, RECIPROCITY, and EQUALIZATION—we have no misgivings whatever.

But, if these principles be admitted, then it is clear that present theories would have to be reconstructed. All the hypothetical entities, by which at present an explanation of physical phenomena is attempted, would have to be discarded; and any new explanation would have to be based on the *realities* of Persistence, Resistance, Reciprocity, and Equalization.

To show the *possibility* of such an explanation of natural phenomena was one—though not the sole—object of the chapters on Heat, Electricity, Magnetism, &c. That is to say, whilst our object was to establish principles only, without entering into their detailed application to particular phenomena, these have merely been introduced by way of illustration or explanation. It is here where errors are most likely to have crept in. Our explanations of specific phenomena may in parts be wrong; but such errors would in no case vitiate the truth of the principles themselves. Principles may be true without their application in particular instances being correct. To give but one instance: attraction and repulsion may be true principles, but the explanation that hydrogen is repelled by our globe may be wrong nevertheless.

Such errors of detail are almost sure to have crept in, notwithstanding that our aim has been to deal with principles only. But, if the latter be true, the correction of the former is sure to follow.

As our object has been truth, and truth only, we invite criticism—the more severe, the better for the object we have in view. Our request is merely that the reader may not push the theories here presented hastily aside, for no other reason than because they are new and, therefore, come before him as perfect strangers.

We are indebted to Mr. F. Swann, B.A., B.Sc., who was kind enough to read our manuscript, for many valuable suggestions and corrections; and to Mr. T. Whitaker for his assistance in correcting the proofs. Our warm thanks are also due to the publisher, Mr. John Murray, for useful hints in preparing the work for the press, as well as for his liberal assistance in revising the proofs. Nor must we omit to express our acknowledgement to the press-reader for his painstaking and searching vigilance, which has enabled him to discover and to correct so many slips of grammar as almost to justify in us the hope that none are left in the book.

Postscript by L. H. Berens.

Many years have slipped away since my friend, Mr. Singer, and myself commenced our investigations into certain phenomena, the intimate relation of which to those treated of in the present work I, at least, did not suspect. Of the road we have travelled together I need here say nothing, since it is alluded to in the present work and will be further disclosed in the larger one we hope shortly to publish. But I would take this opportunity to state, in justice both to my friend and to myself, that the discovery, or recognition, of the principles enunciated—in so far as they are new, of course—as well as their detailed application—in short, the science as well as the philosophy of the work—is exclusively due to Mr. Singer. It has been my privilege to have accompanied him on his explorations, and to have assisted him in his labours, carried out as they have been under many disadvantages. More than this I do not and cannot claim.

LEWIS H. BERENS.

ILKLEY,
February 26, 1897.

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BOOK I

ON METHODS OF INQUIRY

CHAPTER I

INTRODUCTION

The confirmation of theories relies on the compact adaptation of their parts, by which, like those of an arc or dome, they mutually sustain each other, and form a coherent whole.—BACON.

THE object of this essay will, perhaps, be best described as an inversion of the task which Sir Isaac Newton set himself in his famous work. That is, whereas Newton's object was 'only to trace out the *quantity* and *properties* of this force [of gravity] from the phenomena and to apply these in a mathematical way . . . and to avoid all questions about the *nature* or *quality* of this force,' our present object is to trace out the *nature* and *quality* (to use Newton's own expression) of this force and to avoid all quantitative determinations. In short, while Newton's labours were directed solely towards the ascertainment of the *vis gravitatis*, the present essay is to be devoted exclusively to the ascertainment of the *causa gravitatis*.

From this it will be seen at once that, although the phenomena under consideration are the same in both cases, the explanations sought are of an entirely different character, necessitating on our part an inquiry in its nature entirely different from that pursued by Sir Isaac Newton.

The difference between the two kinds of investigation may be aptly illustrated by the following analogy. Let us assume the phenomena to be investigated to be those produced by a running train pursuing a certain course at more or less regular intervals. In that case Newton's investigations would be restricted to the ascertainment of velocity, the path described, and the force that would be requisite to divert the said train from its path, without any reference whatever as to the causes to which the motion itself is due, or as to what retains the train in its irregularly regular path. Our object, on the other hand, would be to discover the causes of the motion itself, the conditions of acceleration, retardation, and direction, without entering into quantitative determinations of either force or rate of motion. In the one case it is a question of measuring relative *magnitudes*, in the other of determining *qualities*. Hence, while Newton achieved his object by the cultivation of mathematics, we shall have to devote ourselves to the study of physics.

We should mention, however, that our conclusions concerning gravitation were reached while engaged in an inquiry undertaken for an altogether different end, about which a few words may here find a fitting place.

It is now some years since we set ourselves the task of subjecting biological phenomena to the methods which have yielded such marvellous results in the physical sciences. The object we then had in view was the study of man, in the widest sense of the term. Dissatisfied with the ambiguities and subtleties of metaphysical dialectics, which in all matters appertaining to man are still made to do duty for science, but which, instead of explaining anything, only succeed in further mystifying an already obscure subject, we proposed to ourselves the attempt of applying to these problems what is known as the physical method of inquiry. It was fortunate, perhaps, that we were ignorant of the difficulties and dangers which were before us, or else we might well have shrunk from the task. We may say, however, that the greatest difficulties were not in the facts we have essayed to investigate, but in the general conceptions concerning them. It was no small matter to rid the mind of its preconceived notions, deep-rooted both in thought and speech, having their origin in the remote past. It is in connexion therewith that we had to fight the hardest battles; but we have, as we have every reason to believe, eventually succeeded—not, however, until we had managed to get rid of nearly the whole of current terminology together with all those conceptions due to an anthropocentric view of nature; a subject of which we shall have to speak more fully in a subsequent chapter.

This once accomplished, our task became comparatively easy. Instead of pondering on terms and phrases, we set about the investigation of phenomena. We collected and collated facts, arranging them according to their similarities, and regardless of conventional classifications. It was while engaged in this task, groping our way step by step from the simpler phenomena to their more complex manifestations, generalizing as we went along, and grouping all related phenomena under such generalizations, carefully verifying each step before taking the next, that we have reached a generalization which at the time seemed to embrace all the phenomena of biology. It contained the promise of the light we were in search of; to wit, the principle which would make possible a uniform and harmonious explanation of all organic phenomena. However, before accepting it as a fact and allowing ourselves to make deductions from it, we thought it necessary to verify our induction (or 'law') by every possible means. On the provisional assumption, therefore, that the said induction was true, we made sundry deductions, which were in many cases (to us at least) of a surprising character. But in every such case, where the conclusions were contrary to what we believed to be the fact, we were able, either by research or by experiment, to verify the conclusion.

Nor was this all. Not only did the manifestations of organisms, from the lowest to the highest, conform to this principle, but even after a mental analysis of the organism, and resolving the parts into their elementary constituents, i.e. the chemical elements, did we find the said law to hold good. Thus we passed from the organic kingdom into the inorganic, from 'living' organisms to 'dead' matter, without having lost trace of this principle. It thus became evident that we had to do with a primal quality of matter—a general law of nature.

Here new means of verification suggested themselves, and were followed up. We applied the law in every way we could think of, made deductions from it, and verified the same by suitable experiments. In this manner we could, by its aid, satisfactorily explain to ourselves the phenomena of heat, light, sound, electricity, magnetism, chemical changes, and many other mysteries with which our sciences still abound; and that without taking refuge in the convenient but deceptive fictions of 'ethers,' 'forces,' or other like imponderabilities.

After numerous experiments for the purpose of verifying sundry deductions concerning physical phenomena, we resolved on a final experiment—an *experimentum crucis*. This was to consist in applying our induction to the explanation of the phenomena of gravitation. We were guided in this choice by the fact that gravitation comprises the most universal and at the same time most mysterious of phenomena. To solve these should therefore admit of extensive verifications, both by experiment and observation. For our said generalization did not consist in the discovery of any new 'force,' but revealed the *cause of forces* (so-called). Hence, if true, this should include also the 'force' of gravitation; and, moreover, should also teach us how to increase or diminish any such 'force'; which, if applied to gravity, leads to unexpected conclusions, as the reader will readily perceive. For in that case we should be able artificially to increase or diminish the gravity—or weight—of bodies without adding to or subtracting from their quantity. Surprising as such a conclusion is, it was by no means the most startling of those with which we have met; and we found little difficulty in verifying it by actual experiment. In what manner, will be stated in the proper place.

Having thus satisfied ourselves that the law did hold good regarding terrestrial gravitation—in addition to numerous verifications in other branches of science—we might have rested satisfied, were it not in the nature of the human mind always to urge on, and after each new discovery to propose to itself the question—And what is beyond? Thus it was that we applied our theory to interstellar gravitation; with what success these pages will reveal.

This, then, is a brief outline of the circumstances which have led us to study the causes of gravitation. We do not pretend to offer solutions of all the problems of astronomy. Indeed, our object here is not to solve any astronomical problems at all, but merely to supply the key by which we believe such solutions may be obtained by those competent to deal with them. Neither of the present authors is either a mathematician or an astronomer; and nothing was further from their minds, when years ago they started on their investigations, than to approach the subject of gravitation, still less to grapple with astronomical problems. In applying our induction to gravitation our object was merely to verify the law. But just as one may become familiar with the properties of a lever without understanding the intricate mechanism of a complex machine; so the principles underlying the most complex phenomena of nature may be comprehended by studying them in their simplest manifestations.

Thus it will be seen that the present essay is an offshoot of our larger and more general labours. These labours, which will show the route by which we have arrived at our induction, we hope to be able to publish shortly. Our reasons for publishing this essay separately and in advance of our chief work are—firstly, because the subject of gravitation would not very well come within the scope of that work; and, secondly, because the principle can be applied to gravitation independently of the manner in which it was discovered.

We would also mention here that most of the facts and experiments we shall cite in these pages, as if the generalizations had been made from them, were really subsequent verifications or deductions from the general induction; so that, instead of being data from which to generalize, they were, in truth, *a posteriori* verifications.

And now a few words as to the method we shall adopt in these pages in placing our results before the reader. From what has already been said it is clear that, although adhering in form to the inductive method, we shall not here reproduce the original process by which we have arrived at our conclusions. We shall make use of a road by which we *might have* arrived at the same results, but which was not the route along which we have actually been travelling. For these reasons we shall not always follow a path which in a case of original research is followed as a matter of necessity. That is, while in our original studies we were ignorant of what was coming, and were therefore unable to anticipate—save by conjecture, which mostly turned out to be erroneous—in the present case, knowing beforehand what we are going to prove, we shall arrange our matter in a manner we think most suitable to place our conclusions before the reader. This is not a very desirable procedure. It is always dangerous for a writer to know beforehand at what conclusions he will arrive; for in that case the method of generalization and induction becomes a mere matter of form, and cannot be accepted as evidence of independent and unprejudiced inquiry, as when he undertakes the investigation of phenomena concerning which he has as yet no opinion at all. But as we are dealing with physical phenomena in the physical method, where conclusions may be tested by experiment or observation, that is not such a serious matter as when the proof of a conclusion is only to be found in a definition purposely framed in order to lead to preconceived results.

CHAPTER II

ON METHODS OF INQUIRY AND SOURCES OF ERROR

In almost every act of our perceiving faculties, observation and inference are intimately blended. What we are said to observe is usually a compound result, of which one-tenth may be observation, and the remaining nine-tenths inference.—MILL.

Wherefore if we would philosophize in earnest, and give ourselves to the search after all the truths we are capable of knowing, we must, in the first place, lay aside our prejudices; in other words, we must take care scrupulously to withhold our assent from the opinions we have formerly admitted, until upon new examination we discover that they are true.—DESCARTES.

THE first duty of every inquirer into nature is to acquaint himself with the properties and peculiarities of the instruments by the aid of which he is about to make his observations. This should apply, in even a far greater degree, to man himself; that is, before attempting to interpret phenomena, we should be acquainted with the manner in which we become conscious of them, and the psychical processes that take place when they are interpreted. This subject forms the main part of the work alluded to in the opening chapter; but all that can be done here is briefly to state some of the principal conclusions there arrived at.

The most important of these is that the physical and mental constitution of the observer are among the factors to which are due the phenomena he observes. Let us explain this. When a person becomes conscious of a phenomenon, he is simply conscious of some sensation; and the object (more correctly, the conception thereof), or phenomenon, to which this sensation is due is in every case an inference from the sensation. For instance, a person seeing a distant object might say at once, 'It is a man.' But all he can be sure of is that he has received an impression—or group of sensations—similar to those which previous experience has taught him to associate with the presence of a man. He may in all cases be sure of his sensations, but not as regards his inferences concerning the nature of the object to which these sensations are due. Thus an object which at a distance looked like a man may turn out to be a post or the stump of a tree, which in its outlines resembled those of a human being, and therefore called up the idea of a man. But, whether far from or near to the object, the observer is conscious of his sensations only; and in each case the object, or *the cause* of the sensations, is inferred by referring the group of sensations to prior similar sensations. Whenever new impressions are received similar to some previous impressions—or sensations—they are at once associated with objects or circumstances to which those prior impressions have been traced. The smell of apples will call up the idea of apples, although there are other substances (e.g. oenanthic ether) which might produce the same effect. So as regards any of the sense organs or any other object. To this fact are due the frequent deceptions to which the sense organs are liable. It has been our common experience, and we doubt not also that of others, in hastily glancing at a thing resembling something with which we were familiar, that the image of the latter was at once present in our mind; and only a closer examination disclosed the error. Thus, for instance, persons resembling in some general feature others with whom we are acquainted would at once remind us of the respective persons, even though the resemblance be very slight. Or reading the syllable 'Glad—,' for instance, on some poster, made us mentally complete the word Gladstone; though on inspection it proved to be some other word commencing with the same syllable. A remarkable instance of an error due to this identical cause, and shared by thousands of people at the same time, is mentioned by Dr. Carpenter in his *Mental Physiology*, of which we shall avail ourselves as an apt illustration of the point we are aiming at.

When the Crystal Palace was on fire the onlooking crowd were excited by seeing, as they thought, a chimpanzee on the top of the roof in the midst of the flames, trying to make his escape. Later on, however, it was discovered that the supposed chimpanzee was a fluttering rag, and that the animal in question was safely stored somewhere else. This incident will serve us to illustrate the point we wish to emphasize. Had the crowd not known that a chimpanzee had been in the building, and, moreover, had they never seen a chimpanzee, it is obvious that the fluttering rag could never have called up in their minds the idea of such an animal. They might still have mistaken it for a living creature, but most assuredly not for a chimpanzee. It might have been a man or a woman, or any animal they had seen and of which they knew that it could climb to the top of a roof. In other words, the rag was one factor, and their own prior ideas, or 'conceptions,' constituted another factor in producing the new idea of the supposed presence of a chimpanzee.

Without entering further into the details of this subject, we may state the general conclusion as follows:—

New sensations are referred to and compared with prior similar sensations, and attributed to like causes as those to which those prior similar sensations have been traced.

It is easy to see how several individuals, looking at a like object, such for instance as the fluttering rag among the flames, would arrive at different conclusions as to the nature of the object, if each were to record independently his opinion, without having prior knowledge of the opinions of others. But, so soon as such opinion has been given, the testimony of the others could no longer be regarded as independent evidence. The mere suggestion that it was a chimpanzee would have been enough for many people, who otherwise might never have thought of that animal, to recognize it at once plainly in the outlines of the fluttering rag, especially when such folk were in a state of excitement. But it would be difficult for any individual to decide for himself whether, under such circumstances, the sensations on which his conclusion was based were produced by the object he was looking at, or were evoked by his 'imagination'—i.e. *a reproduction of some prior sensations.*

The general law just stated holds good in all cases of ratiocination. New phenomena are always interpreted by attributing them to causes like those to which similar phenomena have been previously attributed. To restate the same thing in other words:—New sensations are compared with and comprised under prior similar sensations, and interpreted accordingly. It will be plain, on even a superficial consideration, that this is the necessary and only possible mental process; and from this it will be evident what an important part already existing conceptions play in making new observations, as well as in interpreting them.

The reader knows how, when sitting in a moving train, the objects on either side of the line seem to move past him. These objects simply produce on him the sensation of motion, and he at once jumps to the conclusion that these objects are actually moving. Nay, we are wrong; he does not jump to this conclusion, but only marvels at the deception. But he knows that it is a deception only because of his prior knowledge that the trees and houses are fixed, and the train in which he is sitting is in motion. If that other object happens to be a train on an adjacent line, he is not so sure, and has to make additional observations before he can decide which of the trains is moving. Furthermore, if both trains were moving, he would have to make further observations before he could decide in which direction either of the trains is moving. For, needless to say, a train moving in the same direction as the one in which we are sitting might seem to move in an opposite direction, if its speed be greater or less than that of the train in which we ourselves are travelling.

In the case of the relative motions of sun and earth there is no such prior knowledge that would at once enable one to conclude which of the two bodies is in motion; and hence people arrived at the natural conclusion, in accordance with the indications of the senses, viz. that the sun was moving from east to west. How many other erroneous conclusions were due to this one wrong conception of a moving sun is now matter of history. All we would point out here is the general conclusion that all these errors were due, not to defective observation, but *to prior false conceptions*, which presented the observed phenomena to the mind in the light of what was already known, or supposed to be known, and prevented it from conceiving them in any other light. Indeed, so great a part do these prior conceptions play in all mental operations that in most cases it is impossible to tell how much of what we are conscious is really due to actual observation, and how much to mere interpretation.

As the important bearing of these considerations on inquiries like the one before us is too self-evident to need more than pointing out, we need not apologize to the reader for giving yet another striking example for the purpose of emphasizing this and other like sources of error. We shall again make use of an error of the past; not, however, for want of modern examples, but because it will carry greater conviction to the reader's mind than if, at the present stage, we were to cite similar errors still accepted as well-proven facts. As we proceed we shall have occasion to point out several such errors and misconceptions now currently believed, and to which alone we attribute the great difficulties which beset the solution of the problems of gravitation, as well as of many other problems.

The object of the illustration to be given is to show :—

1. That observations and measurements can never be relied on as being accurate.

2. That any conclusions based on such observations can never be accepted as valid beyond the range of observation (in other words, that a generalization based on partial observation can never safely be made into a universal induction).

3. That the interpretation of such observations is in every case more dependent on our prior conceptions than on the observations from which the conclusions are supposed to be drawn.

We shall then consider by what process the errors due to these causes may be guarded against; in other words, we shall try to devise what has been phrased as a 'test of truth.'

Let us consider, then, the following experiment:—We suspend two plumb-lines at a convenient distance, and then measure their distances from each other at both ends. The most delicate measurements at present possible would demonstrate—as far as this is possible by direct observation—that the two lines are parallel to each other. By the aid of the abstract axiom that parallel lines, if extended indefinitely, would never meet, we would draw the inevitable inference that two such plumb-lines, if indefinitely extended, would never meet. This conclusion would seem both obvious and inevitable; yet the student of to-day knows it to be false. But his knowledge is not due to *direct* observation, but to his acquaintance with the fact that the earth is round, and that plumb-lines at any part of the earth are at right angles to the horizon.

But let us suppose this experiment to have been made at a time when the earth was supposed to be a flat expanse. We should not have to go so very far back. Let us further suppose that some one in those days asserted that plumb-lines were not exactly parallel, but convergent; that this convergence increased with the intervening distance; and that at a little more than six thousand miles apart two plumb-lines, instead of being parallel, would actually be at right angles to each other; and, further, that any two plumb-lines, at whatever distance apart, would cut each other, if extended downwards, at a distance of about four thousand miles.

Would such a declaration made in those days before a College of Professors not have been at once rejected as the most flagrant absurdity ever propounded? We are almost afraid to contemplate the fate of a man in those days daring enough to propound so monstrous a proposition. Let us suppose, however, that one of the then philosophers deemed the propounder of such a preposterous theory worthy of a serious confutation, and what would his reply have been? It would, in all probability, have been somewhat as follows:— 'You assert that plumb-lines converge more the further apart they are. This plumb-line which you here see is exactly at right angles to the water-level. The latter, as you know, is everywhere the same.' (His 'everywhere,' of course, would be the result of but limited observation.) 'Now, if what you say were true, then at some distance from here plumb-lines should be inclined towards the horizon, or the water-level, which has never yet been observed. I have made the experiment in Athens and in Crete, in the Levant and in Italy, and have everywhere found plumb-lines to be exactly at right angles with the horizon. Furthermore, your theory is contradicted by the universal experience that bodies fall towards the earth in a straight line. If what you say be true, that plumb-lines six thousand miles distant from here form a right angle with this line, then the weight at the end of the string, instead of falling towards the ground, would pull the string in a horizontal direction [1], which is contrary to universal experience. If, therefore, you will not be counselled

[1] This would be a necessary conclusion of a man who supposed the earth to be flat; since on no other supposition could he picture to himself two plumb-lines to be at right angles to each other.

by me and abandon this mad theory of yours, we shall have to recommend you to the tender care of some kind and competent person.'

In all this we have supposed our philosopher to be imbued with the notion that the earth was flat; and with such conception in his mind no other reply to such a theory would be possible. That his general conception concerning the world around him might be wrong would certainly not dawn upon him: and even if the daring propounder of such new theory had insisted that he had arrived at his conclusion by actually measuring plumb-lines by some new and delicate instrument—something of the nature of the zenith sector—which enabled him to discover that plumb-lines were divergent upwards and convergent downwards; and that this convergence increased at a regular ratio with increased intervening distance; the philosopher would doubt the accuracy of the instrument, rather than question his own conceptions. *The fact is that we never question our conceptions, for the simple reason that they would not be ours if we doubted them.*

To the man who conceived the earth as a flat expanse nothing could be more conclusive than that plumb-lines were strictly parallel. He could prove it—no, he could *demonstrate it by actual experiment*; a phrase with which our readers must be very familiar. But, notwithstanding such 'direct and positive evidence,' the student of to-day disbelieves this conclusion; and that not because he has any 'direct evidence' to the contrary, but because it conflicts with the now established fact that our earth is a sphere. His evidence is not due to direct observation, but is circumstantial, depending on a concatenation of inferences.

As every one knows, such illustrations might be quoted *ad infinitum*. Instances might be found on almost every page of the history of the sciences; and in most cases it might be shown that the error was based on *actual observation*, 'verified by experiment'—as the favourite phrase runs—whilst the discovery of the error was in almost every case based on circumstantial or inferential evidence. The 'negative weight' of phlogiston may here be mentioned as another case in point. In fact, our language, both that of common parlance and of science, bears evidence of the manner in which ideas have originated. In common parlance lime is still *burnt*, and in technical language bodies are *calcined*; whilst the term *calx*—itself doubtless a metaphor or analogy—has been applied by the earlier chemists in the sense of cinder to any solid residue that was left after the 'spirits' had been expelled by fire[1]. 'Spirits of wine,' 'spirits of salt,' 'spirits of hartshorn,' &c., remind us of the conceptions our forefathers had concerning these substances. In Bacon we can read about the properties of all substances being due to their 'spirits'; some of them subtle, quick, and penetrating; others coarse, slow, and sluggish. And just as man was actuated by his 'soul' or 'spirit,' so the bodies of the universe owed their manifestations to the 'spirits' residing in them.

The human mind has not yet quite emancipated itself from the idea of these 'spirits.' It is their names only that have been changed. The disembodied, impalpable 'spirits' have been changed into impalpable 'forces.' Beyond this change of name there is hardly any difference of conception. The 'forces' still 'reside,' or are 'inherent,' in the bodies. Without these 'forces' matter is 'inert'; and whatever happens is attributed to the effects these 'forces' produce *on* or *in* matter. It is these fundamental conceptions we shall have to combat before we can place our proofs before the reader. In fact, this is all we have to do, as our theory of gravitation would then at once be self-evident as soon as stated; just as the proposition that plumb-lines are not parallel becomes a self-evident conclusion as soon as the fact is comprehended that the earth is a sphere. But of this more further on. At present we will content ourselves with summarizing the results of these considerations, and laying down a few rules for our future guidance.

Firstly. The absurdity of any proposition is in itself no proof that the proposition is untrue. Anything contrary to our general conceptions necessarily appears an absurdity. Absurdity simply means inconceivability; and *anything contrary to, or incompatible with, our accepted notions is for that very reason*—and sole reason—*inconceivable*.

Secondly. Perfect agreement of a theory with our con-

[1] Thus all oxides or 'dephlogisticated metals' were *calces*, and were regarded as such even after Lavoisier had demonstrated their combination with 'air,' and shown that they gained in weight by just so much as the weight of 'air' with which they combined.

ceptions is no proof of its truth. This, of course, is the converse of the preceding statement. Since new phenomena are always interpreted in the light of prior conceptions, such agreement with general conceptions—and this is all that is meant when a theory is said to conform to reason—is a necessary result. Nor can the senses be trusted in such matters, since they are both imperfect and deceptive; and, like our processes of reasoning, are dominated by our preconceived notions[1]. Theories are generally verified by referring them to 'facts'; but these 'facts' consist either in observation or in our prior conceptions, both of which are untrustworthy. To be sure, then, of our 'facts,' we would have to be sure both that we have observed correctly and that our ideas are correct.

The conclusions we have arrived at are sweeping, and can be summed up in a few words:—

1. Our old conceptions, which include all past knowledge, cannot be implicitly relied on.

2. Our senses are untrustworthy, being both defective and deceptive; and therefore—

3. A generalization is to be regarded as approximately true, and in respect of those observations, only, from which the generalization has been made, and should not be accepted as valid beyond that range. Thus, for instance, it would be sufficiently correct to say that two plumb-lines near each other are parallel, and to determine the parallelism of two walls by means of plumb-lines. But it would manifestly be wrong to infer from an experiment made by suspending a few yards of string that plumb-lines are *absolutely* parallel, and hence, if indefinitely extended, would never meet. For, obvious as such a conclusion must have been at one time, we now have good reason to believe that, at the comparatively small distance of four thousand miles, such lines would actually touch each other.

4. From this it will be seen how dangerous it is to reason from analogy. Yet the whole present conception of gravita-

[1] The former illustration of the chimpanzee affords an example of this. A still more striking illustration is afforded by the persistent lying of our senses even when their indications are known to be untrue. Thus trees and houses distinctly move past us when looked at from a moving train, although we know the objects to be fixed and stationary; and that simply because the phenomenon produces on us the sensation of objects in motion.

tion, and, indeed, the fundamental laws of Sir Isaac Newton as far as they apply to the *quality* of gravitation, are based entirely on analogy, as will presently appear. A body on earth falls to the ground: this is observation. Body and earth attract each other: this is an obvious and necessary inference, but still an inference only. That '*All* bodies attract each other' is a universal induction based on this partial observation, *not* borne out by facts.

That 'all terrestrial bodies are attracted by our globe' is a truth. But that 'all terrestrial bodies attract each other' is a theory which *may* be true. As far as the facts warrant it, the statement should be as follows:—Some bodies *attract*, others *repel*, and yet others seem neither to attract nor to repel each other. Which statement might be supplemented by the additional observation that bodies seemingly inert towards each other may, under some conditions, attract, and under other conditions repel, each other.

This is a true statement of what we know concerning terrestrial objects. And if we are to follow Newton's second rule of reasoning, and assign to the same natural effects, 'as far as possible,' the same causes, we would have to extend this statement to celestial bodies. And this statement is certainly more in accordance with observed celestial phenomena than the hard-and-fast universal doctrine that all bodies attract each other. But of this we shall have to speak again.

By the above four points it would almost seem as if we had cut all ground from under our feet. If our conceptions are to be called in question, our senses to be declared cheats, our observations unreliable, and our reasoning by analogy altogether inadmissible, it may well be asked what standing-ground there is left for the philosopher to start from. This brings us face to face with the old problem of the test of truth, which we shall now proceed to consider.

CHAPTER III

METHODS OF VERIFICATION

What is Truth, was an Enquiry many Ages since; and it being that which all Mankind either do, or pretend to search after, it cannot but be worth our while carefully to examine wherein it consists; and so acquaint ourselves with the Nature of it, as to observe how the Mind distinguishes it from Falsehood.—LOCKE.

SEEING, then, what an important part prior conceptions play in the formation of new ideas, our first object ought to be to revise our beliefs before approaching the solution of any new problem. Prior to any such contemplated inquiry it is necessary, therefore, to make sure that our general views of the universe are correct; or, to put it more precisely, that those principles, inductions, and theories on which our knowledge is based are true.

To explain anything means to make it intelligible to the understanding. And we have shown that it is possible to give an explanation of phenomena agreeable to the understanding; one which would seem so self-evident as to be mistaken for direct observation (*vide* apparent motion of the sun) and yet be far from the truth. On the other hand, we have seen that a truth may seem an absurdity because incompatible with what we believe to be true, and rejected on that account.

Touching the explanation of the causes of gravitation, what is really required—i.e. what would be the general criterion of such explanation—would be to refer the phenomena of gravitation to such known causes as would be capable of producing the phenomena, and which at the same time would conform to our general knowledge of the universe.

In the physical sciences no theory or principle is to be discovered on which to base any such explanation. The only prevalent idea which would seem applicable is that of 'force'—an unexplained and inexplicable abstraction. When the idea of 'force' was still held in its original form—i.e. as 'spirits'—it was a something which the mind could comprehend. In that form it was simply an anthropomorphism: an invisible thing or being which tossed or moved things about. There can be no question but that the idea of 'force' was the product of analogy; the invisible 'spirits' or 'forces' having been supposed to act on bodies in a manner analogous to that in which man can act on things around him. And it is just this anthropomorphic idea of 'force' which is underlying those theories of gravitation which have hitherto been advanced. That of Le Sage, for instance, has, instead of 'spirits,' little atoms which knock against the bodies and keep them in their places. It was the same with the ancient theory of the earth resting on the back of an elephant, and the latter on a tortoise. This explanation was very satisfactory as far as the then current idea of a necessary support for the earth was concerned; but it failed to explain what supported the tortoise: just as the above theory of Le Sage fails to explain what keeps his hypothetical atoms in action, knocking at the right time and in the right direction.

The same may be said of the ether theory, of which Herr Isenkrahe is the latest expounder. Here, again, in place of explanation, we have mere substitution. If it be the action of an ether which keeps the bodies in their places, an explanation is still wanted to account for the action of the ether. But this latter demand could easily be satisfied. All that is needed would be to attribute to this ether any properties the theory may require; make it elastic or rigid, soft or hard, sluggish or active, according to the needs of the theory, without fearing that any of the assumed properties might be found by experience to be contrary to facts. For this ether is conveniently conceived as a substance so subtle as to put an actual examination of its physical properties beyond the range of possibility.

But, whilst rejecting all such theories as insufficient or unsatisfactory, they all serve to confirm what we have just said, viz. that explanation, as at present understood, does not necessarily mean to refer phenomena to their true causes —this may be true incidentally—but to explain them to the understanding; i.e. *to assign causes in agreement with our general conceptions.* Both the atomic theory of Le Sage and the ether theories, from Leibnitz to Isenkrahe, are based on the assumption of some coercive 'force,' analogous to human power, by which to account for the phenomena of gravitation. All these endeavours agree in this one point, that, without even attempting to discover the true causes, the framers of these theories simply tried to discover known causes of other phenomena which might be capable of producing the phenomena under consideration. In other words, they have searched, not for the *actual*, but for *efficient* causes—causes which *might* produce the phenomena.

Starting from this standpoint, there is but one idea in our modern physical sciences which seems at all applicable; and that is the anthropomorphic idea of 'force.' And, let us say it at once, it is this very conception of 'force' which has, from its antiquity, such a strong hold on the human mind as to incapacitate the latter from seeking a solution in any other direction. The form in which the question involuntarily obtrudes itself on the mind is always, What is the *nature* of the 'force' of gravity? the 'force' being assumed as a matter of course.

But, put in this form, gravitation is not the only problem at present insolvable. The phenomena of electricity are, in like manner, attributed to the *electric force*; and here, too, it seems impossible to account for the *nature of the force*. So, too, with the magnetic, chemical, &c., 'forces.' All these are inconceivable and incomprehensible, because the only 'force' the human mind can conceive of is that of pushing or pulling; i.e. 'forces' having some analogy to human actions.

All the phenomena of electricity, magnetism, chemical changes, light, heat, sound, &c., require as much explaining, and in the same sense, as do those of gravitation. The assumption of 'forces' does not explain the causes of any manifestations, except where these latter can be traced to kinetic or dynamic causes; i.e. pressure, pull, or push. But even in such cases the explanation does not go beyond the proximate causes. A body, for instance, attached to one end of a lever, and raised by a weight attached to the other end, is explained by showing the descent of the one to be the cause of the raising of the other. The relative weight and relative length of the arms of the lever are ascertained, and their relations, at first deduced from the phenomenon, are then explained under mechanics. In truth, however, this mechanical problem resolves itself into one of gravitation, which remains unexplained.

All our knowledge of physical phenomena is quantitative. Of *causes* nothing is known beyond theories; and, as these have in each instance been framed to meet the necessities of each separate group of phenomena, there is the greatest possible discordance between them. Each particular branch of science has its own theories and its own peculiar terminology; and all these theories agree in this one point, that they all aim at referring phenomena to mechanical causes. Whichever way we may turn, we are everywhere confronted with this one fundamental conception of 'force'; and beyond it the physical sciences have not as yet been able to penetrate. Nor has this mechanical theory yet been successful in accounting for any phenomena except those which clearly fall under the heading mechanics; nearly all of which [1] really resolve themselves into problems of gravitation.

The problem before us must then be approached by first examining our conceptions; notably by inquiring into the correctness of this fundamental conception of 'force,' which now dominates the mind, as the conception of 'spirits,' or of a stationary flat earth, dominated the minds of our forefathers.

But how is this to be accomplished? We have already seen in what manner subsequent knowledge has been instrumental in modifying prior conceptions. A new discovery, or supposed discovery, when conflicting with such conceptions, is at first generally rejected as absurd. But when such facts accumulate and are confirmed, then doubt commences to arise concerning the correctness of such older views. Investigations are made, and old opinions, hitherto held to be self-evident facts, are overhauled and slowly but gradually modified, and are eventually supplanted by new theories. The mental process in such cases

[1] We might say *all*, inasmuch as we shall show the causes of electricity, magnetism, &c., to be identical with those of gravitation.

consists in harmonizing old conceptions with new facts. This points out to us the road along which we should travel, and brings us to the point towards which we were aiming in this chapter, viz. *the test of truth.*

Our former and sweeping conclusions would seem to indicate that certainty of knowledge concerning anything is altogether unattainable. This, however, is not so; in reality it all depends on the meanings given to the words 'knowledge' and 'certainty.'

The incontestable fact, which follows as a necessity from our very constitution [1], that what we observe are our sensations, and that our knowledge consists merely of subjective conceptions concerning the causes of these sensations, is in a degree fatal to any hope of our ever being able to ascertain any 'positive,' 'absolute,' or 'objective' fact—which seems to have been the object of philosophy from the earliest times. But, paradoxical as it may sound, it is this very discovery, this conclusion that 'absolute truths' are unattainable, which will help us to find a test of the correctness of our knowledge. For this conclusion has simply revealed that absolute truths are of a kind with the philosopher's stone or the universal solvent of the ancients.

The very facts of our constitution and organization exclude the possibility of our ever attaining to anything such as 'absolute truths.' But little reflection is needed to see that the phrase itself is an absurdity, a thing unthinkable. Whatever we sensate, cognize, or know, consists always of comparisons, the referring of one thing to another—an endless process of comparisons. Save by way of comparison we know nothing of size, weight, number, colour, temperature, &c. Our actual as well as our potential knowledge may be expressed in the following formulae:—A is larger than B; C heavier than D; and so on. And if there were no other objects producing in us the same kind of sensations, but in different degrees, we could have no knowledge of such qualities at all.

We can be conscious of an external world only when that external world can evoke in us sensations. It is these sensations which form the groundwork of our knowledge; and the

[1] Dealt with more fully in the manuscript work alluded to and shortly to be published.

only means whereby we can represent them to ourselves, or express them to others, consists in a comparison of these sensations with each other. Where sensations are wanting, or discrimination between sensations, knowledge is impossible. Nor can we possibly convey to others an idea of things sensated by ourselves unless the individual to be instructed has himself felt corresponding sensations. By no possible effort, for instance, could we convey to a blind man an idea of colour.

If, then, all our information is derived from our sensations, and *consists in comparisons of these sensations with each other*, then it must be equally clear that all our knowledge can be of relations only. (We should point out, perhaps, that we do not include here in the term knowledge the products of the imagination, whether true or false, based on knowledge directly due to the information of the senses.) Whether speaking of objects or of phenomena, it is always a question of relations to other objects or phenomena. And, if our knowledge of these *relations* is correct, this is all we need aim at; at any rate, it is all we can possibly attain to.

We compare the sizes of several objects, and in doing so we fix upon one, quite arbitrarily, as a unit measure for the others. It is quite immaterial which object we select for this purpose; all we need concern ourselves about is that the relative sizes should be correctly ascertained and expressed in terms of the arbitrary unit. As our standard of length we may select a yard, rod, or metre; as our standard of volume, a cubic foot, a bushel, or a litre; as our standard of weight, the pound, ounce, or gramme. And if we can secure uniformity of standard, and then correctly ascertain the relation all other objects bear to this standard, there is no need to trouble our heads about whether there is in the universe an 'absolute' measure of length, weight, or volume. For, even supposing such a thing conceivable, it is difficult to see in what manner this could help us to solve any of the problems in which mankind are interested.

Speaking of absolute weights and measures will no doubt sound ridiculous to most of our readers; and it may be objected that no one ever proposed such an absurdity; that this is not what is meant by 'absolute truths.' But substitute for these any other idea, and the case will not in the least be altered. Substitute, for instance, for measures, qualities or states. There are many natural philosophers who have attempted to find the 'absolute zero' of temperature, and have actually fixed it at 273° below the present centigrade zero[1]. But this would only mean the substitution of a lower temperature for that of congealing water as the standard zero, and would effect no greater alteration than if the gramme were substituted for the pound.

Take yet another case. In measuring inclines and declivities the level of stationary water has been fixed upon as a standard; and the spirit level, which is used as the standard instrument, simply indicates the plane that would be formed by a sheet of water[2]. Let the reader try to imagine for one moment an 'absolute level' in the universe, or an 'absolute' *up* or *down*, and he cannot fail to see at once that the idea of 'absolute truths' is an absolute absurdity[3].

But, so soon as this is recognized, another truth of far more practical importance at once becomes manifest, viz. that our knowledge must for ever be confined to a knowledge of *relations*. This puts the whole problem in a new and more promising light, as the ascertainment of relations is well within our reach.

[1] It is worth while noticing here, in passing, the reasoning on which this supposed 'absolute zero' is based. Gases are found to contract in volume by about $\frac{1}{273}$ for each degree of temperature lowered; and the conclusion is made with confidence that, if we could reach a temperature of 273° below zero, the gas would be reduced to nothingness. On that supposition, of course, it is safe to assume that a lower degree of temperature would be impossible, as there would be no substance left to experiment on. The argument is on all fours with that of the plumb-lines, without having as much warranty as the latter. Water contracts, too, when cooled, and expands when heated; but when it reaches 4° it no longer contracts, but commences to expand and is converted into a solid; and at 100° is converted from a liquid into gas, thus affording another striking example how dangerous it is to base universal inductions on very limited observation.

[2] For practical purposes water, in a limited area, may be regarded as forming a plane.

[3] As instances of 'absolute' truths are generally brought forward certain maxims or axioms of philosophy, called 'self-evident truths'; such as 'the part is lesser than the whole.' But such are necessary truths only because of the suppositions. In nature there is no such thing as a 'necessary truth,' if by it be meant, as is generally the case, that it must be so because we cannot conceive it otherwise. The reason why 'parallel lines can never meet' is because we conceive them so, and because so soon as lines approach towards each other we no longer call them parallel. On analysis it will be found that all *necessary truths* (so called) consist of concise statements of certain conceptions; that is, of definitions which in every particular detail agree with themselves, and nothing else. In each case the necessity lies in the conception, and that merely because the opposite would be contrary to supposition.

Absolute knowledge, then, as far as the present essay is concerned, will have no other meaning than an absolutely correct statement of the several relations. Thus, when it is said that A is four times the volume of B, this would be *absolutely* true if the volume of A were absolutely and accurately four times that of B. But, as such absolute correctness is impossible of attainment, we cannot even know whether our statement of relations is *absolutely* correct. Nor is correctness of such a high degree necessary, since, even if attainable, it would be inappreciable by the human mind. All that can be hoped for, and all that is needed for all practical purposes, is a close approximation to truth[1].

Under this view the problem of certainty of our beliefs, and the possibility of a criterion whereby to ascertain whether our conclusions are correct, assume an altogether new aspect. It is no longer a question of anything in the 'absolute'—whatever this phrase may mean—but whether certain relations have been accurately ascertained and correctly stated. Whether our conceptions of the universe, our graphic mental representations of the phenomena, are actually true or not, we do not know, and probably never can know (excepting, perhaps, of some few phenomena within our immediate reach). Nor is any such knowledge necessary; for all practical purposes are achieved so long as our conceptions correctly represent the *relations* between the several phenomena.

Thus at one time the phenomena of day and night were explained on the supposition that the sun moved from east to west. At the present time the same phenomena are attributed to the diurnal motion of the earth round her own axis. Now, so far as these phenomena by themselves are concerned, it is immaterial which of the two suppositions be accepted; for the time between appearance and disappearance of the sun could still be measured with the same precision on either of the two suppositions. The same may be said in respect of the seasons. In the one case it might

[1] In the construction of a delicate balance, for instance, it is stipulated that it should indicate one ten-thousandth part of its capacity, which is found to be sufficient for all practical purposes. It would be useless, even if attainable, to express the weight of a body weighing several tons to fractions of a grain, as such minute accuracy would be altogether inappreciable by the mind, and valueless in practice.

be assumed that it was the sun that moved south and north, as was at one time believed; or, according to current belief, our earth may be conceived as moving round a stationary sun with her axis inclined at a certain angle; as either theory would account for the phenomena of the seasons.

So long as such theories are strictly confined to the particular group of phenomena to which they relate, no inconvenience can arise therefrom. But the case is very different when theories are compared with each other and found to be mutually exclusive. Thus the supposition of the ancients that the sun moved round the earth in a certain path well agreed with the phenomena of variable nights and days and the variable seasons, but could not be harmonized with other astronomical observations.

It is under such circumstances that a revision of old theories becomes necessary. When, for instance, one group of facts is explained by a theory which well agrees with the particular group of phenomena with which it deals, the theory is generally accepted as conclusive. But, so soon as another group of equally well-proven facts is found to disagree with, or to contradict, such theory, it is then only that the latter is questioned and a new explanation is sought that would embrace the larger group of phenomena. Thus, as soon as the spherical shape of the earth had been recognized, a large number of former conceptions and theories had to be modified or altogether abandoned.

From these considerations we may draw a general conclusion which will serve us as a finger-post towards the object of our search, viz. the test of truth—

The greater the number of facts explained by one and the same theory, without involving any contradiction, the more trustworthy is such theory.

This conclusion will enable us to understand why of two such conclusions—as (1) plumb-lines are parallel; (2) plumb-lines are convergent—we accept the latter, though based on a long chain of inferences, as against the former, which is the result of actual observation. The greater credibility of the circumstantial evidence is not due to its indirectness, but to its greater agreement with a long series of other observations, and because it does not involve such contradictions as the first conclusion.

The element of credibility, then, depends entirely on harmony, the theory harmonizing and explaining the greatest number of facts being the most reliable. From which it follows that a theory comprising all known facts and explaining them consonantly with each other would be the most perfect *for the time being*. Should any new facts be discovered that can at once be ranged under the old theory, without necessitating any strain, and well agreeing with all other facts already comprised under it, this would be additional confirmation—not of *the truth* of the theory, but that it correctly states *the relations* of all known facts. Should, however, such newly discovered facts disagree with the theory itself, or with any of the assumptions on which such theory is built, then a revision of all the facts would at once become necessary, and the theory would have to be corrected or modified, as may be necessary, in order that all known facts might be conceived under one general induction.

Briefly stated, then, the only criterion of the truth of our beliefs consists *in unity of conception of all known facts.* That is, whatever may be our general conception of the universe, there should be a perfect harmony between all known phenomena. Whereby is meant that the *relations* between all known facts shall be truly stated, each to all and all to each; no matter whether the general conception itself be true or not.

This requires explaining, and we shall do so by an illustration which we trust will be both clear and conclusive. Instead of speaking of phenomena generally, let us confine ourselves to observations of the earth's surface. Say that we had to prepare a map of the globe. The object to be aimed at would be, of course, to represent correctly on this chart the relative distances and positions of different places. In that case it would not be necessary that the chart should be a miniature facsimile of the earth. Indeed, the surface of our globe, or portions of it, might be mapped out on a flat sheet, without in any way impairing its usefulness. Let the different places on the earth's surface represent the observed phenomena, or isolated facts. Let us further suppose that no such map is as yet in existence; and then see in what manner such a map would be prepared and its correctness tested and verified.

Suppose that at first we had to determine the relative positions of two places only, say those of London and Edinburgh. In that case it would be quite sufficient to measure the distance which intervenes between these two places, and mark them on our sheet of paper at a corresponding distance reduced to scale. In such case the position or direction of the two places would be quite immaterial. But so soon as we have to add to our map a third place—say Manchester—the process of map-making becomes a little more complicated. We should now have to add direction to distance; viz. to note in which direction we should have to travel, from either Edinburgh or London, in order to reach Manchester. For if we were to mark Manchester at the proper distance from Edinburgh, whilst neglecting its position relatively to London, we should find ourselves wrong if we attempted to reach Manchester from London by following the indications of the map. Evidently the position of Manchester would have to be laid down relatively to both places already noted.

Say that we now had to add a fourth place, and that, having fixed its position by compass and triangulation from Manchester, it were afterwards discovered that its true position relatively to the other places did not agree with the indications of the chart. In such case, if it be found that the triangulations from Manchester were correct, it would become necessary to redetermine the positions of the other places, until our map, as far as it goes, is found to agree in all its details with actual observation.

This is the actual process of map-making; and the greater the number of places marked down on it, and the relative positions of these several places tested and found correct, the greater will be the reliability in the correctness of such a chart.

It may be that a new continent is discovered, and a place—say New York—is marked down relatively to Liverpool; and, by-and-by, another place—say Adelaide—is marked down on the map relatively to New York. In course of time, however, a sailor wishing to reach Colombo from Sydney, taking this map for his guide, finds himself landed in a wrong place. Here, then, is a discrepancy, again necessitating a resurvey. In such case it is necessarily doubtful whether the error was committed in the later or the earlier surveys. In actual experience it has mostly been found that the older surveys were the least reliable; and that partly on account of imperfect instruments and defective methods, but mostly because the means of verification were few. The more places marked on such a chart, the sooner is an error likely to be detected; for, even if the survey be correct relatively to one or two places, it could not, unless exactly accurate, be correct relatively to other places.

To sum up: The correctness of such a chart would depend entirely and solely on the accuracy of relative distances and positions of the places marked on it, and not on the actual resemblance of the chart to the globe itself. The best maps of to-day are accurate in a very high degree, yet not one of them even remotely resembles the actual shape of the earth. A flat sheet map is as useful as a globe; and, even if the globe could be made an accurate facsimile of the earth, its usefulness would thereby be in no way increased. Nor would it matter if the earth itself were conceived as a flat expanse, were it not that such conception would conflict with other theories or known facts.

This, then, is what we meant by saying that, provided the relations of phenomena—by which is meant the sequence of events, causes and effects, &c.—are correctly stated, embracing all known facts, it is immaterial under what particular theory they are conceived. But so soon as such theory, or mental conception, conflicts with some known facts, or any other accepted theory, the general conception would have to be modified accordingly.

Thus, to give from the physical sciences an analogous case to the foregoing illustration of the map, it is essential to know that a certain weight of oxygen will combine with a certain determinate weight of hydrogen, sulphur, or iron, under what conditions these combinations would take place, as well as the nature of the resultant products. But whether the fact of this combination be conceived as being due to 'affinity'—a 'reciprocal love'—or to 'basicity' and 'acidity,' all of which are purely mental conceptions, is immaterial, so long as such conceptions do not involve any contradiction of fact, and do not clash with other ideas.

It is worth while noticing here that in the preparation of the earlier charts the fancies of man, or his fanciful conceptions of the earth's surface, played a prominent part. But, as geographical knowledge became extended, these fanciful representations have gradually yielded to others more and more approximating to actuality; and, although we may not yet possess absolutely perfect charts, we are making them more exact by every successive addition. It might further be noticed that it is the common experience of topographers that, in order to keep pace with the increasing demand of accuracy, it is from time to time necessary to resurvey portions of the earth's surface, so that our knowledge of its features shall be abreast of the requirements of the day.

What is true of the study of the earth's surface is true, word for word, in respect of all other phenomena. In each case it is necessary to prepare such a chart; and, in order to ensure its accuracy, the periodical resurvey of the several groups of facts, and a redetermination of their relations towards each other, are indispensable. Every actually observed fact becomes a point in our scientific chart; and to determine and redetermine its exact relations to all other facts is as necessary to science as the periodical resurveys are to chart-making.

In this wise only can the accuracy of our knowledge be tested and verified. The test of the truth of knowledge is supplied by this knowledge itself; and is, therefore, as progressive, if we may use the term, as the latter. For just as there can be no 'absolute knowledge,' so there can be no 'absolute test.' Our theories and observations can only be verified by comparing them with each other; a process to be repeated from time to time, as new discoveries are being made. And if all known facts can conformably be conceived under one all-embracing theory, and the reciprocal relations of phenomena—causes and effects, conditions and results, antecedents and consequents, relative magnitudes, &c.—are correctly stated, this would be at any time the utmost possible test of truth—for the time being.

The theory may even in that case be as far from the truth as the sheet map differs in shape from a sphere; so that the test of truth would in any case only apply to relations. In this respect our knowledge would more nearly approximate to actual truth the greater the number of facts which have thus been ascertained and classified. *In all this we suppose, of course, that the various facts of nature could be compared with each other, no matter to what particular branch of science they belong.*

But at this point an obvious difficulty is encountered. In the making of a geographical chart, distances and direction are the only two elements to be determined; while in general science the relations are manifold. A still greater obstacle is that each field of knowledge has its own peculiar terminology, which would seem to make such a uniform conception impossible. How, it may be asked, are the facts of astronomy, for instance, to be compared with those of chemistry? In what manner are we to determine the relations between the weight of oxygen and the circulation of the blood? What connexion is there between embryology and pneumatics? As currently conceived and taught, these are distinct groups of phenomena, having nothing in common with each other—and that notwithstanding the accepted axiom of the unity of nature and universal law of causation. But even leaving out of consideration fields of knowledge so widely apart as astronomy and physiology, and confining our attention to what are called the physical sciences, we still find the greatest possible differences in both language and conception. We have hydraulics and hydrostatics, mechanics, dynamics and pneumatics, electricity and magnetism, light, sound, and heat, and so forth; each separate branch of knowledge having its own distinctive theory and peculiar terminology not applicable to the others. It is like the confusion at the Tower of Babel. The workers in the different fields of knowledge cannot understand each other, therefore cannot commune with each other for the purpose of seeing whether the conclusions deduced from one group of facts agree with the conclusions derived in other fields of inquiry.

The reason of this discrepancy and confusion of thought and language is not far to seek. The various discoveries have not been made by the same persons, nor at the same time or in the same places. And as each discoverer necessarily conceives his new discovery in accordance with his own prior conceptions, and according to the circumstances and conditions under which the discovery has been made; and as these conceptions and conditions necessarily differ in almost every instance, evoking in each investigator different ideas; these independent discoveries give rise to as many independent theories, and, in due course of time, to as many independent sciences.

One man discovered near Magnesia an iron ore which attracted particles of iron; hence the term magnetism. Another man in Greece made the discovery that amber ('electron'), when rubbed, attracted light particles of matter, which gave rise to the name, and subsequently to the science, of electricity. To Dr. Galvani's discovery is due the science of galvanism; and to that of Volta, voltaic electricity. The close relation of the different phenomena embraced under electricity, magnetism, and galvanism is now well known, yet are they still treated as separate and distinct sciences; and it required the labours of a Faraday to demonstrate their close relations, if not their identity.

As we proceed we shall have no difficulty in convincing the reader that the relations between these phenomena are not closer than those which exist between the phenomena of any of the physical sciences; that the phenomena of hydraulics, electricity, gravitation, &c., are as closely related with each other as those of magnetism and electricity; that they can all be expressed in identical terms, and compared with each other both quantitatively and qualitatively. But, before this can be done, it is obvious that we require a uniform terminology—a uniform conception of the universe.

As we have said before, it is not new facts we have to present to the reader, but new conceptions concerning old facts: conceptions which shall be in harmony with all hitherto ascertained facts. It is not the latter we are about to assail, but the current *theories* concerning them. Each particular group of phenomena has at present its own theory, which outside that group of manifestations is meaningless; or, where an attempt to apply it to other related phenomena is possible, is at once involved in contradictions, of which many examples will be given as we proceed.

What we propose to do, therefore, is to reconsider some of the principal facts of the physical sciences; to state them with the greatest possible precision, without any reference whatever to accepted explanations; to classify them according to their marks of similarity and dissimilarity, and to make generalizations accordingly; to throw these again into higher generalizations, and so on in succession, until we have reached an induction which shall embrace all phenomena of the physical sciences (for the sake of brevity, organic phenomena will be omitted in the present work): and thus may we be enabled to comprehend the phenomena of what are now considered as distinct sciences under one all-embracing theory. On the degree of success in this direction will depend our success in proving our theory of gravitation. In this wise shall we be able to test the accuracy of current conceptions, and to correct them when necessary in accordance with present-day knowledge; without which correction any theory, however plausible, would have to be regarded as a mere hypothesis, and worthless for the purposes of practical science.

CHAPTER IV

THE BASIC PRINCIPLE OF THE CURRENT THEORY OF GRAVITATION

It is always safe and philosophic to distinguish, as much as is in our power, fact from theory; the experience of past ages is sufficient to show us the wisdom of such a course; and considering the constant tendency of the mind to rest on an assumption, and, when it answers every purpose, to forget that it is an assumption, we ought to remember that it, in such cases, becomes a prejudice, and inevitably interferes, more or less, with a clear-sighted judgment.—Faraday.

After a somewhat lengthy, though necessary, disquisition concerning methods of inquiry and verification, we will return to the consideration of those theories of Newton which bear on the subject of this essay—i. e. the *cause* or *quality* of gravitation—and show that his theories do not in all cases accord with present-day knowledge.

One of these is that 'attraction is proportional to mass.' This is now a universally accepted theorem in connexion with gravitation, and may be said to be the basic conception in the light of which all phenomena of gravitation are interpreted. To the modern mind it is in respect of gravitation what the idea of a flat earth, or the geocentric conception, was to the ancients. Everything is viewed from this standpoint; and every phenomenon in any way connected with gravitation is explained in accordance with this fundamental conception. It is, in fact, the primal theory, and the test of every subsidiary theory[1]. An explanation of any particular

[1] Sir G. B. Airey, for instance, in his admirable Ipswich Lectures, after having explained how the different densities of planets are determined, says: 'This estimation rests absolutely on the theory of universal gravitation.' This is not quite true, however; the correctness of these conclusions does not depend merely on the correctness of the assumption of the *universality* of gravitation, but also, and perhaps chiefly, on the assumption that 'attraction is proportional to mass.' For, if it could be shown that the

manifestation relating to gravitation which would not agree with this fundamental conception is not even admitted by the mind. And, even should any such explanation suggest itself, it is at once rejected as untenable so soon as it is perceived that it would not conform to this fundamental conception.

If, for instance, it is discovered that a larger planet performs less work, proportional to its volume and distance, than a smaller planet, the inference that the larger planet is of a lesser density is at once seized upon as an obvious and inevitable conclusion—the necessity arising from the acceptance of the aforementioned fundamental conception. If the attraction between any two bodies is found to vary, as when it is found that the attraction between a body on our earth and the earth itself varies in different parts of the globe—in which instance, of course, we are bound to assume the 'mass' to remain the same—then explanations are still sought in one direction only; i.e. how can this be harmonized with the 'undoubted fact'—the 'fact' itself being assumed as beyond question—that 'attraction is proportional to mass'? An explanation is sought for 'the seeming exception to the universal law'; and the kind of explanation sought for is naturally the one which would agree with the fundamental conception. In the present instance the exception is attributed to a difference of the centrifugal force near the poles and the equator respectively; the greater or lesser distance from the centre of attraction at different parts; and, when these strictly mechanical, and therefore rational, causes are still found to be insufficient to account for the variation, recourse is had to the imagination. The interior of the earth is conceived as a fluid consisting of different strata, each stratum having a different density and an obliquity of its own. We do not say that these explanations may not be the true ones, but merely wish to point out that they are not in accordance with sound reason-

reciprocal attraction between any two bodies can be increased or diminished without altering their relative 'mass,' the deductions concerning the 'mass' the several planets are supposed to contain would be worthless. As far as observation goes, the conclusion is justified that this attractive force does vary; inasmuch as sun and earth, if their relative positions be due to attraction, attract each other at different times in variable degrees; they being sometimes nearer each other and sometimes further apart. This inference seems all the more probable, though not conclusive in itself, because these variations in relative distance are periodical and regular.

ing, and certainly not with the requirements of experimental science.

In our former illustration of the map we have shown that when a new place is added thereto, and it is found that, while it is correctly represented relatively to one or two places, it is not correct relatively to other places, it remains an open question whether the error be due to the new triangulations or to the former measurements; and that in such a case a resurvey would be necessary until the source of error has been discovered. In the case of map-making there is no room for fanciful inventions to compensate for bad triangulations or errors of measurement; there, no 'negative distance' can be assumed in order to evade the necessity of a resurvey. Any discrepancy is at once accepted as positive evidence of error, although it leaves undecided where or when the error has been committed. The same method ought to be adopted in all other branches of knowledge. This, however, is not the case; and that because, when theories depend entirely on abstract reasoning, it is not the assemblage of facts but the mind which is the ultimate criterion as to whether the conclusions arrived at are right or wrong.

'All bodies have spirits, which are expelled by heat,' reasoned our predecessors; 'therefore a burnt substance, or *calx*, is less in mass than before it was burnt.' But the 'calx' of metals was found to be heavier; hence the obvious and necessary conclusion, amounting to 'positive proof' and 'experimental verification'—*because of the above general conception*—that the substance expelled had a 'negative weight.' It will be hardly necessary to point the lesson of this illustration.

The theorem that 'attraction is proportional to mass' is just such another fundamental conception, to which all explanations of phenomena in any way connected with gravitation are referred. It is imperative, therefore, that we should make sure of its truth before we accept it as the basis for further inquiry.

Now we shall have no difficulty in showing that there was no warranty for accepting this theorem as true even in Newton's time; and that subsequent discoveries have not only failed to confirm his assumption, but tend rather in the opposite direction. We shall show—

1. That the term 'mass' conveys no distinct idea, and that as regards different kinds of matter we have no knowledge of what constitutes 'mass.'

2. That the doctrine of attraction being proportional to mass is based on the fallacious argument by analogy.

3. That it is contradicted by facts.

4. That the evidence advanced in support of it is based on a fallacy known in logic as reasoning in a circle.

5. That the experiments of Newton bearing thereon are inconclusive.

In the first place, what is 'mass'? Newton defined it as 'the quantity of matter which bodies contain'; which is really no definition at all, but a mere substitution of one ambiguous and meaningless phrase for another[1]. If we try to trace this idea of 'mass,' it is easy to see that it is another instance of analogy, and was suggested by certain previously observed facts. In taking, for instance, a quantity of a compressible substance, say wool or feathers, its weight is found to be the same when compressed into one-half, a quarter, or a tenth of its former volume. It has also been observed that when thus compressed the parts composing the mass are packed more closely together, and that in consequence the interstices have been reduced. Hence the (seemingly) obvious conclusion that a *heavier* or a *harder* body owes these qualities to greater *density*. But such conclusions seem obvious only because they are agreeable to our understanding—i. e. are in conformity with our prior conceptions. Not knowing anything of the causes to which weight and hardness, generally, are due; and knowing that, volume for volume, compressed matter *of the same kind* is harder and heavier than when not compressed; *density* is at once assumed as the true cause of weight or hardness. When, however, we come to compare substances of different kinds, such as steel, lead, diamond, and mercury, it is difficult to reconcile these conceptions with each other and with the facts. In what respect, for instance, can we attribute the greater weight of mercury as compared with that of lead to greater 'density'; or the greater hardness of the diamond as compared with mercury? To say that the weight is due to the particles being closer to each other, and

[1] And was given by Newton as such. By mass he merely meant weight, as he was careful to point out.

the greater hardness to a stronger 'cohesion' of the particles, is merely substituting words for explanations, without conveying distinct ideas to the understanding.

Let us pursue this subject further. We place a certain quantity of gold in one pan of a balance, and a quantity of iron sufficient to counterpoise the gold in the other, and then say the two are of equal weight; and, according to Newton's supposition, both contain an 'equal quantity of matter.' But, unless we know what constitutes 'matter' or 'mass,' it is impossible to say whether this conclusion is true or not. The experiment does not in the least enlighten us on this point; it simply shows that the quantity of iron in the one pan performs, *under these conditions*, an equal amount of work to the gold in the other pan. That is, x units of gold are attracted towards our globe with an intensity equal to the attraction of y units of iron. To know whether the quantity of 'mass' is the same in both cases, we should have to find some independent standard of comparison. But so soon as we look for such an independent standard the analogy falls to pieces; for we then discover that the two quantities of matter which are *equivalent* (not equal, but equivalent) as regards the attraction of the earth are equivalent in no other respect; that, having different specific volumes, even their relative weights are not the same when the experiment is made in different media. Even *in vacuo* their relative weights are not the same for different altitudes, or at different temperatures; but they are equivalents only under precisely the same conditions, which latter include media, temperature, altitude, &c.

Since Newton's time an important discovery has been made, which has disclosed that the different elements combine with each other in fixed proportions; and chemists would certainly be more inclined to accept these combining proportions as a measure of 'mass,' in preference to that of weight. But here, too, equivalent combining proportions do not correspond to either equivalent of weight or equivalent of volume. And, what is more, the chemical combining proportions of substances differ at varying temperatures; as will be shown in a subsequent chapter[1].

In short, from whatever point we may approach the problem,

[1] See Book iv, chapter v.

all reasoning and all experiment lead to the conclusion that any two quantities of different kinds of matter are *equivalent* to each other only in certain respects, and under certain conditions; and that in no two cases are the equivalents of quantity the same, whatever standard of comparison we may employ. Equivalents of volume are not equivalents of weight, of solubility, of chemical combining proportions, of acidity or basicity, and so forth. There is not even a correspondence between weight and chemical equivalents. Thus 39 grammes of potassium, one of the lightest of known metals, combine with 8 grammes of oxygen; while the equivalent proportions of, say, silver and magnesium, both of which are heavier than potassium, are, reduced to the same standard of comparison, 108 to 8 and 12 to 8 respectively. Substances are equivalents in all cases in respect of certain 'properties' and *under identical conditions* only; and as we do not know that matter consists of atoms—i. e. ultimate particles—we cannot tell, of course, whether the number of particles are the same to which are due the equivalent volumes, equivalent combining proportions, or equivalent weights.

In fine, we have no means whatever of determining what constitutes *mass*; and therefore we cannot say for certain, even on this ground alone, that 'attraction is proportional to mass[1].' All that may be said with certainty is, that, *all other conditions being equal*, two ounces of gold will be of double value to one ounce; and two equivalents of iron will (again with the proviso of *caeteris paribus*) be double the value of one equivalent of iron. Always remembering, however, that equivalents in one respect do not correspond to equivalents in other respects.

Coming now to Newton's supposed evidence concerning this theorem, we have one body of evidence deduced from celestial phenomena, and another from his experiments. We shall deal with the former first, and show that the supposed proof is the most perfect illustration of arguing in a circle that it is possible to conceive of.

Values of 'density' are given to the sun and planets pro-

[1] Equality of mass is inferred by Newton from equality of work done, he having assumed gravity to be a force 'which admits of neither intension nor remission.' We shall show by experiment that the gravity of a body may be varied without varying the quantity of matter.

portional to the relative effects these bodies exert on each other. But, instead of being a proof of the theory that 'attraction is proportional to mass,' these several 'densities,' and, in fact, the assumption of 'variable density itself,' are but inferences from the above theory. The results of observation merely show that the perturbations caused by the different planets, when reduced to the same distance, vary in magnitude, and that these perturbations do not bear any fixed relation to volume. According to the already accepted theory that 'attraction is proportional to mass,' this discrepancy between volume and work done is necessarily attributed (agreeably to this basic theory) to difference in 'density'; i. e. it is an inference necessitated by the primal assumption.

We now come to Newton's experiments, of which we may remark that, both in execution and interpretation, they fall far short of the requirements of modern physical methods.

The experiments he adduced consisted in suspending wooden boxes of equal size, filling one with wood, and placing in another 'an equal weight of gold,' or other substance; that is, he first weighed out on his balance a quantity of gold equivalent to the quantity of wood in the other. His pendulum experiment was, therefore, only another method of weighing; and, having previously, by means of a balance, ascertained that he had taken quantities which under like conditions would perform an equal amount of work, his pendulum experiment could prove no more than if he had reweighed the same substances with each other in another balance. But the weighing of two such pre-adjusted quantities and finding them of equal weight—after having taken the trouble to make them so—is no proof of anything, except that his weighings were correct. For to suspend such quantities subsequently in pendulums and to cause them to swing is—in a sense—only another manner of weighing; and, supposing the weights to be equal, and the length of the pendulums and the resistance of the air to be the same; and supposing further that *the centre of gravity of each quantity of matter coincided with the centre of oscillation*, then it is self-evident that they would swing together *in the same locality*[1].

[1] Though not necessarily in other localities. It is well known that the same pendulum makes different numbers of swingings in different parts of

It is this last condition, which we have italicized, which makes the experiments worthless for the purpose for which they have been made. For if we place a lump of gold and an equivalent weight of wood in two boxes, each being suspended from the same height, they would not swing together unless their respective centres of oscillation were in both cases equidistant from the centre of suspension. Newton does not inform us in what manner he ascertained the centre of oscillation of each. His own words are—'I suspended an equal weight of gold (as exactly as I could) in the *centre of oscillation of the other.*' But he does not say in what manner he found this centre of oscillation. Presumably in a similar manner as in weighing out 'equal masses'; i. e. by raising or lowering the gold until the pendulums did swing together. But, unless the centre of oscillation of two substances of equal weight could be determined by any other and independent means, such experiments can count for nothing. They prove neither equality of 'mass,' nor that 'attraction is proportional to mass.'

Let us consider now some of the facts that bear on this subject. Bodies falling through a resisting medium do not fall together, even when of equal weight, if the resisting surface be unequal. Thus, for instance, an ounce of gold will fall quicker through air than an ounce of wood; or an ounce of gold in the lump will fall more quickly than an ounce of gold-leaf. If, instead of air, another medium be substituted, say water or oil, these differences would vary in every instance. And, if the experiment be made *in vacuo*, then a feather would fall in the same time as a lump of gold. In each case we can ascertain *equivalents*, but not equality of 'mass.' But from none of these experiments can we adduce that 'attraction is proportional to mass.'

If we look at other phenomena, as magnetic or electric attraction, we can still find no corroboration of this doctrine. A glass rod, when tested, seems to exert no attraction at all; but when rubbed can attract small particles, which attraction can be increased up to a certain limit by increased friction.

our globe. According to the theory of gravitation we are about to prove, two pendulums of the same length and weight, but consisting of different substances, which swing together in one place, would not swing together in every part of the globe; which is directly opposed to Newton's theory of 'mass.'

Still more significant is it that if an excited glass rod be brought near one that has not been excited it at first attracts and then repels it: the 'mass' remaining the same in both instances. The same is true of either permanent or temporary magnets; their attractive powers can be increased or diminished without increasing or diminishing their 'mass.'

Newton got rid of the difficulty presented by the phenomena of magnetism by a dialectic fence; a form of self-deception due to a fundamental psychic law to which repeated allusions have been made, and with which we shall deal more fully in the work mentioned[1]. His third rule of reasoning runs—'The qualities of bodies *which admit neither intension nor remission of degrees*, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.' Behind this we can again clearly see lurking the theorem that 'attraction is proportional to mass,' and that for the same 'mass' it remains always the same. His assumption, necessitated by the acceptance of this fundamental conception as true, was that gravity was different in kind to magnetic attraction; and he drew, therefore, the distinction between the two kinds of phenomena by implying that, whilst magnetism admitted of 'intension and remission,' the force of gravity of any body remained constant. We shall show, however, that this is not so, and adduce experiments to demonstrate that we can actually increase or diminish the weight of a body without either increasing or decreasing its quantity.

Of course a larger quantity of steel, if magnetized to the same extent, would manifest a greater power of attraction than a lesser quantity. Quantity of matter undoubtedly must be a factor in the phenomena of gravitation, as it is in every other respect; but this is not the same as saying that 'attraction is proportional to mass.'

Conclusions. The object of this present chapter was a twofold one. Firstly, to show that one of the cardinal doctrines of gravitation, that 'attraction is proportional to

[1] And to which, it might be added, the present authors may—notwithstanding this caution—also have fallen victims in places. Our quarrel is not with Sir Isaac Newton, but with human frailty.

mass,' remains unproven, and, therefore, cannot serve as a basis for further reasoning. Secondly, and chiefly, to point out the fallacy of this kind of reasoning. We have shown that the supposed proof deduced in favour of this theorem from celestial phenomena is based on reasoning in a circle; that the idea of the theorem was suggested by analogy; and that, since all experiments and observations have been interpreted in the light of this fundamental conception, the supposed harmony between theory and fact cannot be accepted as evidence of the truth of the former.

Any theory deduced from phenomena and framed purposely to fit in with such phenomena is bound to be in agreement with the latter. In that case the supposed proof is not an *a posteriori* verification, but merely a reversal of the original process of deduction. When, for instance, from certain phenomena of electricity we infer the existence of two kinds of electricities, or of electric fluids, the phenomena of electricity cannot then be regarded as evidence of the truth of the theory. Or, when the hardness of the diamond is attributed to the 'force of cohesion,' the hardness of the diamond cannot subsequently be adduced as evidence of the truth of the 'theory of cohesion.' Such proof is entirely one of dialectics, and merely proves that the theory agrees with itself. In fact, it amounts to no more than if we say four is double the quantity of two, and then prove that twice two are four.

Having thus discussed at some length the principal sources of error, and arrived at the conclusion that, beyond the range of experiment, the only test of the correctness of our reasoning can be found in the harmonious agreement of our several conceptions, our first duty will be to revise our common conceptions concerning physical phenomena, with a view to reclassification, and the adoption of a uniform terminology which shall enable us to apply such test. For this purpose we shall study the general 'properties' (so called) of matter, in order to arrive at higher and higher inductions, until we have reached a general principle which shall embrace all known phenomena, and which, therefore, can be verified independently of the phenomena, or assumptions, from which the induction itself has been deduced.

If we succeed in establishing such a general principle, as we confidently hope that we shall, it will not be difficult to offer an explanation of the mysteries of gravitation in harmony with all other known physical facts, and without any assumption of ethers, 'forces,' or such imaginary media.

For brevity's sake, however, we shall not include in the present essay organic phenomena, although originally the principle itself has been discovered in connexion with the study of the latter. Nor shall we be able to exhaust, within the limits of this book, the whole domain of even physical phenomena. But a sufficient number of facts will be dealt with, and experiments adduced, to achieve our contemplated object. In what manner this is to be done will be disclosed in the chapters that are to follow.

BOOK II

FIRST PRINCIPLES

CHAPTER I

PERSISTENCE

It must be kept constantly in view that in science, those who speak of explaining any phenomenon mean (or should mean) pointing out not some more familiar, but merely some more general phenomenon, of which it is a partial exemplification; or some laws of causation which produce it by their joint or successive action, and from which, therefore, its conditions may be determined deductively. Every such operation brings us a step nearer towards answering the question . . . What are the fewest assumptions, which being granted, the order of nature as it exists would be the result? What are the fewest general propositions from which all the uniformities existing in nature could be deduced?—J. S. MILL.

THE task before us is to consider the physical qualities or properties of matter; or rather to reconsider them with the double purpose in view: firstly, to ascertain whether current doctrines are correct; and, secondly, for the purpose of revising our conceptions so as to bring them into harmony with present-day knowledge. We shall concern ourselves, however, with what may be termed the *primal* qualities of matter only; i.e. such qualities as belong to matter itself, and are not merely incidental to form, position, or states of aggregation [1].

[1] As *primal* qualities we regard all such qualities (or properties) as are indestructible, and are independent of form, position, or states of aggregation, as, for instance, the weight of a body, in contradistinction to *incidental* qualities, which are peculiar to certain forms or states of aggregation, and which are destroyed when the combination is broken up; e.g. the properties of a lever.

We will begin by considering Newton's first law of motion:—

'Law I. Every body perseveres in its state of rest, or of uniform motion *in a right line*, unless it is compelled to change that state by forces impressed thereon.'

In an investigation which was originally entirely confined to organic phenomena, we have, by successive stages, reached the following generalizations:—(1) All organisms have a tendency to preserve their structure, organs, functions, habits, and dispositions unchanged. (2) That, when this tendency is successfully counteracted by conditions, any new impressions received in consequence are subject to the same law, and there is a tendency to perpetuate the newly acquired dispositions, or changes, in like manner.

These conclusions were arrived at quite independently of Newton's laws. In fact, not until the conclusion had been written down have we been reminded of its analogy to Newton's first law of motion. It was this analogy, or sameness, of a conclusion deduced from organic phenomena to a law derived from the study of physical phenomena, which first suggested to our mind that what we had found was in all probability a principle which was universal; i.e. a quality which belonged to all matter and under all conditions. Later on, as a result of further investigation in this direction, we were able to formulate the same law in more general terms, which included both our own generalization and Sir Isaac Newton's first law of motion, as follows:—

All matter tends to persevere in whatever state it may happen to be and to resist change.

We shall first explain in a few words the meanings we attach to the principal terms in this proposition, and then endeavour to establish the universality of it. In the first place, we have substituted 'matter' for 'body' in Newton's wording, on the one hand, and for 'organism' in our first statement, on the other; because, as will be shown further on, 'the tendency to persevere' of both 'bodies' and 'organisms' is due to the fact that this tendency is a primal quality of matter, of which both 'bodies' and 'organisms' are composed. This statement, of course, will require proof, which shall presently be given. We have further substituted the word *state* for 'state of rest or of uniform motion in a right line,' a phrase used by Newton, on the one hand, and a parallel phrase in our own generalization, on the other, so as to make the proposition more universal and inclusive of both. By 'state' we mean any quality, property, tendency, or disposition of whatsoever nature, be it that of motion or rest, of temperature, of expansion or contraction, or of any other kind.

In like manner we propose to substitute the phrase 'law of persistence' for that of 'law of inertia,' as being more general than the latter.

It now remains to prove the truth of the proposition in this extended form. In this we shall not strictly follow the inductive method, and that because of the fact, previously pointed out, that we are conscious beforehand of what we are going to prove. Our method will, therefore, be more didactic than inductive; i. e. we shall lay down our propositions first and prove them subsequently.

If, therefore, the principle of persistence as above stated be true, i. e. if the tendency to persist in any given state be a primal quality of matter, it would follow that the resistance to change, already so well known in mechanics under the name of inertia, should be manifested by all bodies and under all circumstances of impending change. The verification of this conclusion is easy; indeed, it might almost be said to need no proof, since it is evident to everybody that a certain expenditure of force (using the term in a general sense), however small, is always requisite before a sensible change can be produced in any body. And this is all we mean by the law of persistence; viz. that every body offers a certain amount of resistance to any change, and the *quantity* of this persistence we call the resistance of that body[1].

And now let us consider a few of the phenomena bearing on this point. For instance, we press against a brick resting on the table, attempting to move it, but it remains in its position. We press against it more heavily, and yet more heavily, until at last it yields, and a change of position takes

[1] By persistence and resistance we mean, as will be seen, one and the same thing; but it will be useful to retain both terms: the former to denote merely the *quality* of bodies to persist, and the latter when reference is made to the *quantity* of this persistence. As, for example, 'All bodies are endowed with *persistence*, but the *resistance* which different bodies offer to the same agency varies.'

place. The amount of energy that was required to change the state of rest of that brick to a state of motion we call, under those given conditions, its total resistance to motion. This resistance may be due to several causes; as, for example, gravitation, friction, or perhaps even adhesion to the body on which it rests. It will not be necessary here to discriminate between these several causes.

If, instead of a brick thus resting on a table, we select an object suspended by a thread, a certain amount of energy will still be required before the body can be moved out of its position. With a light body this resistance may be so infinitesimally small as to be altogether imperceptible: a pith ball, for instance, would yield to the least air-current. We can soon convince ourselves, however, of the truth of the law by increasing the bulk or weight of the suspended body, in which case its increased resistance at once becomes manifest, inasmuch as a proportionally greater amount of force would then be required to move the larger mass of matter out of position.

Let us now take a railway truck resting on level rails for our experiment. That, too, as is well known, will offer a certain resistance before it can be moved. But, once this resistance has been overcome and the truck is set in motion, a much smaller force than that which was originally necessary to translate the state of rest to a state of motion would be sufficient to keep it in motion; i. e. a force just sufficient to overcome friction, &c., would now suffice to keep the already moving truck in motion, and every additional power in the same direction would produce a sensible effect that would manifest itself in accelerated speed.

But, once in motion, the truck cannot be stopped by the mere withdrawal of the force by which it is kept in motion—i. e. if we ignore friction and gravitation. This is too well known to need proof. For whenever the motion exceeds the retarding energy of friction and gravitation a railway train will run on after the steam has been turned off, or the engines have been reversed and brakes applied. In other words, the truck, which originally required a certain amount of force before its state of rest could be changed to a state of motion, would now require the expenditure of force to change its state of motion to one of rest: it will resist a change in the one case as in the other, and the amount of external force necessary to effect a change of state in a body is in each case *proportional to its resistance*. In this general sense the axiom is self-evident, inasmuch as we make the amount of resistance offered its own measure.

Let us pursue this subject further. A body set in motion requires a certain amount of force before it can be brought to rest; therefore, by supposition, the amount of force necessary to effect this would be the measure of its resistance. We shall now endeavour to show that the resistance of a moving body is equal to the force which was required to set it in motion. There are various ways of convincing ourselves of this. For instance, a railway truck might require several men to set it in motion, after which one man might be sufficient to maintain its speed. But, if the force by which the truck is now kept in motion were withdrawn, the truck would not come instantly to a standstill; nor would a force sufficient to keep up its motion on level rails indefinitely be sufficient to effect a stoppage if exerted in an opposite direction. As is well known, it could be experimentally proved that, all circumstances taken into consideration, the force required to arrest such a moving body would be equal to the total amount of energy which was at first necessary to impart the motion to it [1].

Let us analyze this force. At first one man tries to move the truck, but fails. Another man comes to his assistance, with no better result. The assistance of a third man is called in, and then the truck is set in motion, and can now be kept in motion by one man. Now it would manifestly be erroneous to suppose that the first two men produced no effect at all on the truck, for that would be to suppose that the third man might have set it in motion without the assistance of the other two, which would have been impossible. We are bound to assume, therefore, that the first two men did produce an effect, though not a *sensible* one. For let us assume the total resistance of the truck to be expressed by 100, and the total force exerted by the two men by 80; then, inasmuch as their strength did not exceed the amount of resistance, no *sensible* effect

[1] It would perhaps be more correct to substitute 'the acquired persistence' for the phrase 'the total amount of energy, &c.,' and that because we have strong reasons to believe that time plays an important part in the phenomenon. But of this we may speak more fully in a future chapter.

could result from their exertions. But that they did produce some effect is evident from the fact that, whereas at first a force equal to 100 would have been required to neutralize the resistance of the truck, now an additional force of only 20 would be sufficient to effect the same result.

This may be more conveniently illustrated by using a balance and weights. Say that 100 grammes be placed into one pan of a delicate balance, and 1 gramme into the other pan, with the object of moving the beam in the opposite direction. In ordinary parlance we should probably say that the 1 gramme had produced no effect at all; which statement, however, would be true only if it be said that it produced no *sensible* effect. For evidently one-hundredth part of the resistance to be overcome has been neutralized by the 1 gramme, and a proportionally less weight would now be sufficient to cause the beam to turn on its centre.

In changing, therefore, the state of rest of a body to one of motion, we may divide the force or energy thus expended into *latent* and *sensible*; the latent force being that amount of energy[1] just sufficient to neutralize exactly the resistance of a body without effecting any sensible motion, and the remainder would be sensible energy.

Assuming now that in our former illustration the energy, strength, or power of the first two men was just sufficient to neutralize the resistance of the truck, and that the additional push of the third man resulted in sensible motion, then (ignoring loss by friction, &c.) a power greater than that which imparted visible motion would be required to restore the truck to its former state. In other words, the latent force would now manifest itself as sensible energy; and the force necessary to bring such a moving body to a standstill would have to be equal to the total force (latent + sensible) originally required to set it in motion.

But for the deductions we shall make from this principle, there would be no necessity to dwell at such length on such well-known facts. But the truth is that we are purposely

[1] Here, as everywhere else, unless otherwise stated, we use the terms force and energy synonymously, and in the sense of power or strength, and shall use either of these indiscriminately as the one or the other may happen to be most convenient.

making use of well-known mechanical laws whereby to illustrate a principle which is universal. It is well known, for instance, that the impact of a projectile is equal to velocity × weight; where weight may be regarded as representing the resistance, or *latent* force, and the velocity the *sensible* force which was originally necessary to give to the body its motion [1].

Assuming, then, that to set a body in motion a total force equal to 100 was required, of which 60 became latent and 40 produced sensible motion, then it follows that after having spent its sensible energy of 40 the body would not yet come to rest, but be capable of overcoming an additional resistance of 60.

This principle may be stated in Newton's words, viz. 'a body maintains every new state it acquires'; using, however, the term 'state' in a wider sense than was done by Newton. After having changed, therefore, the state of a body, the resistance offered to a return to its former state would be equal to the original resistance offered to the acquisition of the new state. Or in yet other terms, latent energy may become sensible, and sensible energy latent, according to circumstances; but in each case the total specific resistance of any one body under like conditions would remain the same.

The gyroscope affords a ready means of illustrating this experimentally in several ways. One is based on the fact that in a revolving body the axis of revolution remains parallel to itself. Having set our gyroscope in motion, a certain amount of force is required to bend its axis out of position; but, having done so, it will retain every such new position; and—provided the speed be maintained—the force required to restore the axis to its former position would be just equal to the force that was previously necessary to move it out of it.

If, instead of states of rest and motion, we consider thermal

[1] Thus an amount of force or energy which could project a body weighing 1 ounce with a considerable velocity would not be enough to move a cannon-ball weighing several pounds. A portion of the energy required to eject such a body from the cannon's mouth must, therefore, be regarded as having been necessary to overcome the 'gravity,' 'inertia,' or resistance of the ball, and the remainder only as having produced sensible motion. But the amount of resistance such a projectile can overcome would be equal to the total amount of energy, i.e. velocity × weight.

phenomena, we shall still find the principle of persistence to hold good. It will be found that substances resist a change of temperature just as they do a change of motion. Indeed, change of temperature might be regarded as a change of motion; only that in this case the motion would be that of the particles relatively to each other, and not that of the mass compared to the relative position of other objects. But the best proof of this is to be found in the *difference* of resistance to changes of temperature in different substances.

If equal weights of water and mercury be placed under like conditions in front of a fire, the latter will in the same interval of time become much hotter than the water; which shows that the resistance of water to a change of temperature is greater than that of mercury. But, if water takes longer to heat than mercury, it likewise takes longer to cool. Indeed the relative increase or decrease of temperature in water is both ways proportional to that of mercury. Taking, then, the resistance of mercury, or of any other substance, as unity, the relative resistance of other substances to change of temperature might be expressed in terms of this unit. In fact this has been done, only under another name. Instead of calling this quality of matter 'specific heat,' we shall call it, in conformity with analogous phenomena in mechanics, *specific resistance.*

On comparing these two classes of phenomena they will be found in every respect analogous to each other. Thus, while water acquires a higher temperature less rapidly than mercury, it is equally true that its 'latency' is correspondingly greater. For if a pound of water and a pound of mercury be both heated to the same temperature and then transferred to two calorimeters the water will give off a greater quantity of heat than the mercury. And if the temperature of a substance be increased until it effects a change of state (a translation or transmutation), as from solid to liquid, or from liquid to solid—analogous to a change of state in respect of relative rest or motion—the amount of heat thus becoming latent, that is the heat expended without producing a sensible increase of temperature, becomes again sensible when the process is reversed.

This same principle of resistance is met with in magnetism, electricity, light, solubility, chemical affinities; in fact, in respect of every agency or quality. Thus when we place two poised magnets within each other's field of influence and allow them to acquire a state of rest, and then turn one of them out of position, a sensible time will elapse before the other will follow the movement. In chemistry the relative affinities of different substances towards the same agent vary considerably. Thus, while barium is precipitated by sulphuric acid almost instantaneously, strontium will form a precipitate with the same reagent only after the lapse of a considerable time. The same holds good of solubility; for in taking two substances, both of which dissolve in the same menstruum, the solution of one takes longer than that of the other. In respect of light this resistance to change is best demonstrated by our own organs of sight. Very minute objects, not readily perceptible, can be seen by the steady application of the eyes, at first faintly, and presently more and more distinctly. And that the vision remains for some time after the object has been removed—known as 'persistence of vision'—is too well known to need any proof. But what is perhaps more to the point is that this persistence of light is proportional to its intensity: just as the energy or time necessary to translate bodies from a state of motion to that of rest is proportional to the force which was necessary to produce the motion.

With the various explanations offered of these several phenomena we are not here concerned. What we wished to show was that *persistence* is a universal property of all matter, that *all bodies tend to persevere in whatever state they may happen to be, to retain every newly acquired state, and to resist change*[1]: a change from rest to motion as much as from motion to rest; from a lower to a higher temperature as much as from a higher to a lower; from a solid to a liquid or gas as much as the reverse; and so forth. In each and every case a resistance is offered to change; and—

Unless there be a sufficient external force to overcome this resistance to change, the body would continue in whatever state it may happen to be.

To recapitulate—by *persistence* we designate that tendency

[1] In biology this same law of persistence is met with under such different disguises as heredity, instinct, force of habit, persistence of vision, bias, prejudice, &c., all of which manifestations may be traced to the tendency of matter to continue in its state, be it that of rest, motion, grouping, or function.

of all matter to remain in any given state even after the conditions are altered (i. e. a quality); while the term *resistance* is used to denote the *intensity* of this persistence (i. e. a quantity). This affords us a convenient and general conception under which to comprise a large number of different phenomena, which, if alluded to by special terms, would not admit of comparison. Instead of speaking, therefore, of inertia in respect of mechanical reactions, latency in respect of heat, resistance of electricity, or relative solubility of bodies, or the time required for precipitating various substances by the same reagent, we can always speak of the *relative resistance* offered by several bodies towards a certain agency. Groups of phenomena, no matter how different they may be in their manifestations, may nevertheless possess points of similarity and be due to the operations of the same principle. And it is precisely by comparing these points of similarity that we can lay hold of the principle itself. Hence the necessity for such uniform terminology. Such terms as impact, inertia, latency, conductivity, &c., do not call up simple ideas or principles, but complex phenomena or doctrines concerning them, without suggesting to the mind any possible relationship between these several manifestations. But all these qualities may be comprehended under the term persistence or resistance: terms which have no special reference to any particular group of phenomena, but at once call up in the mind a principle, reminding us of the universal tendency of all matter to persist in any given state. And this may at once afford the means of explaining many phenomena on one and the same supposition.

Under the term persistence may be comprehended all those phenomena we have been discussing. It is the same as when the particular modes of locomotion, such as walking, running, riding, driving, sailing, flying, &c., were expressed by the general term *progression*. The value of the substitution of such general terms for particular or special terms must be apparent, since it is the necessary first step, we might say the *conditio sine qua non*, of generalization.

CHAPTER II

RELATIVE RESISTANCE

To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal and directed to contrary parts.—NEWTON.

ANOTHER important proposition of Sir Isaac Newton, of which not sufficient notice has been taken by physicists, is that *All reactions are mutual and directed to contrary parts*, which it will be convenient to call the *law of reciprocity*. This follows directly from the conclusions of the last chapter; and the range of this proposition must necessarily be co-extensive with that of the 'law of inertia,' which henceforth we shall call the law of persistence.

If every body possesses the tendency to persevere in whatever state it may happen to be, then a change in that state can be effected by external agencies only. The resistance of that body would have to be overcome. But, since all bodies are subject to the law of persistence, it follows that no body can overcome the resistance of any other body without itself being modified to a corresponding degree by the persistence of the body with which it is reacting; that is, by neutralizing a proportional amount of its own persistence. In other words, if one body, *A*, causes in another body, *B*, a change of state, then by so doing it has at the same time changed its own state to a corresponding degree and in an opposite direction. And this conclusion is verified by all known facts. If a cold body should cool a hotter body, it will itself be heated to a corresponding degree; if a body in motion should impart motion to another body, it will itself lose a corresponding degree of its own motion; if an acid should neutralize an alkali, it will itself be neutralized to a proportional degree; and so forth. In every case the reaction will be mutual and directed to contrary parts.

Several conclusions of the utmost importance follow therefrom, which we shall now consider seriatim. In the first place, body undergoing any change whatever must necessarily be in reaction with another body or bodies. Or in other words, in every reaction at least two bodies are concerned, not *acting on*, but *reacting with*, each other. As a matter of fact, in every reaction more than two bodies are concerned, directly or indirectly. But it will be convenient in most cases to consider but two bodies in respect of each other, without taking notice of the simultaneous changes caused by and in other surrounding conditions. In some cases this reciprocity of the reaction is manifest, as when hot and cold water are mixed, in which case the result will be a proportional mean temperature. So likewise, if a moving ball overtakes another similar ball moving in the same direction but at a lesser speed, whatever the latter may gain in speed will be lost by the former.

In many cases, however, the reciprocity of the reaction is not so obvious; and the fact is, therefore, likely to be overlooked. We fire a cannon-ball, for instance. The velocity of this cannon-ball will gradually diminish, and finally the projectile will come to a state of rest. In such a case it is common to say that the ball has 'spent its force.' But what has become of it? In passing through the atmosphere it had to overcome the resistance of the latter, and in doing so has gradually lost its own 'impressed force'—to make use of Newton's expression—until the latter has been entirely neutralized; when, being no longer able to overcome the further resistance of the atmosphere, it comes to rest. It has 'spent its force,' therefore, in modifying some other body or bodies—in the supposed case the atmosphere—and in so doing has itself become modified to a corresponding degree. The reaction was reciprocal.

If the medium through which the cannon-ball is projected be less dense (i. e. have a lesser resistance) than the atmosphere, then the ball will be able to penetrate further; if denser (i. e. of a greater resistance), it will come sooner to a state of rest. But in each case it will be able to penetrate until it has overcome an amount of resistance equal to its own energy or persistence, i. e. the impulse that has been imparted to it. If, on the other hand, a cannon-ball could be projected in this manner into a space in which there is no resistance at all—which means that if we could assume the unthinkable, that a cannon-ball could be thus set in motion in a void in which there is no other body with which to react, or which could modify its tendency—then we could not conceive of anything lessening its velocity or modifying its course. Such an experiment, of course, is impossible; and therefore the bare assumption of it, according to our own contention in the preceding book, is inadmissible. But there are nevertheless ample means by which to verify this. The mere fact, that the distance to which the same projectile can be impelled by a like force increases as the resistance of the medium through which it passes is lessened, is sufficient to establish this law. A pendulum in a vacuum will oscillate for several hours as compared with the short duration of its oscillations in the atmosphere.

A cannon-ball, therefore, does react with other bodies even when not striking any particular object. Even ignoring gravitation, in consequence of which it reacts with the earth, it reacts with the medium through which it passes; the latter neutralizing its motive power, and thus slowly changing its state from one of motion to one of rest.

Let us now take two bodies of different temperatures, and bring them near each other. We shall again ignore all other objects around, and consider only what takes place between these two bodies. As the reader knows, the colder will become warmer and the warmer will be cooled, until both are of the same temperature. At that point this reaction between the two bodies would cease; and that because neither would any longer be in a condition to change the temperature of the other. If these two bodies were now placed in an atmosphere which remained constant, then it is evident that the temperature of these two bodies would remain constant also.

From all of which follows—

1. *That bodies can react with each other only when there is a difference of state in respect of any quality or tendency.*

Thus two bodies at rest cannot change each other's state of rest. Nor can two bodies moving with equal velocities in the same line and the same direction affect each other's motion. Nor can two bodies of equal temperature affect each other's thermal states[1]. In each case there must be a difference of degree, or direction, or of states, before a reaction can take place.

2. *The extent or intensity of the reaction will be proportional to this difference, and will cease altogether when relative equalization has been reached.*

Thus when a moving body impinges on a body at rest the reaction will be complete when the total amount of resistance of the one or both has been neutralized. If the body at rest offer a greater resistance than the body impinging on it can overcome, the latter will come to rest, or—under conditions—will have its line of motion modified. If, on the other hand, the resistance of the obstructing body be less than the impact of the projectile, the latter will have motion imparted to it, the former losing so much of its own velocity. If an acid be allowed to act on a base, the reaction will proceed until all the acid or the whole of the base is neutralized, when the reaction will again cease.

Thus, then, the change in any body or substance can only take place by an interchange or interaction with other bodies or substances, and in each case will result in reciprocal modification. In the absence, therefore, of such modifying agencies no change could take place at all. If, therefore, all bodies in the universe could be conceived as being of the same temperature, for instance, there could be no thermal changes; and if all bodies could be conceived as being at rest there could be no motion.

Conclusions. The results of this and the preceding chapter may be summed up in the form of a series of generalizations.

1. *All bodies tend to persevere in whatever state they may happen to be, and to resist change.*

2. *Two bodies in different states, in respect of either degree or direction, when opposed to each other, will mutually modify each other until their relative resistance has reached equivalent degrees, after which no further modification can take place.*

3. *Every reaction, therefore, can be conceived of as a process*

[1] We mean, of course, by mere radiation, or transmission of heat, and except changes of temperature due to chemical action, in which latter case the increase of temperature would be the same for both reacting substances.

of reciprocal modification, equalization, or adaptation to conditions.

Assuming the tendencies of two such bodies to be opposed to each other, then, each endeavouring to persevere in its own state, it follows that neither of them can so remain. Hence—

4. *The reciprocal modification will be inversely as the total resistance of each.*

5. *Each reaction being due to reciprocal modification of states, it follows that the greater the degree of difference of the opposing tendencies, the more intense will be the reaction; the latter decreasing gradually in proportion as equalization is approached, and ceasing altogether when this point has been reached.* Hence—

6. *No two bodies can react with each other in respect of any quality or tendency which both possess in equivalent degrees.*

Thus one body cannot change the thermal state of another body if both are of a corresponding temperature. Nor can one body at rest cause motion in another body at rest after both are in adjustment with each other; and so forth.

7. *The degree of resistance in respect of any agency is different in various bodies; and different in the same body in respect of different agencies.*

This is true if under resistance be conceived all qualities of matter, such as weight, hardness, solubility, conductivity, &c. For instance, the hardness of a body is the resistance it offers to the penetration of other bodies; its solubility, the resistance it offers to the modifying action of solvents; its specific or latent heat, the resistance it offers to change of temperature, or to a change of state from solid to liquid, and vice versa. Under this point of view water may offer a greater resistance to thermal changes than copper, and a lesser resistance to penetration. This, then, is the sense of the above generalization, which simply expresses well-known facts in more general language.

8. *Two bodies may be in a reciprocal state of equilibrium with each other in respect of one or more qualities or tendencies, without necessarily being in equilibrium in other respects.*

Thus, a quantity of copper in one pan of a balance and a corresponding weight of water in another may be said to be in equilibrium in respect of weight, but not necessarily in respect of their thermal states. The one may be cold and the other hot; therefore, while equipoising each other on the balance, they might still react with each other by a reciprocal modification of their thermal states.

9. *Since, then, every modification of any body is due to a simultaneous modification of another body or bodies, no change at all can take place unless the body opposes or is opposed by another body in a different degree in respect of any state or tendency.*

From which follows—

(a) *That bodies in equilibrium with each other cannot modify each other.*

(b) *That every reaction may be conceived of as a process of reciprocal equalization.*

CHAPTER III

EQUALIZATION

In nature, nothing is at rest, nothing is amorphous; the simplest particle of that which men in their blindness are pleased to call 'brute matter' is a vast aggregate of molecular mechanisms performing complicated movements of immense rapidity, and sensitively adjusting themselves to every change in the surrounding world.—HUXLEY.

OUR endeavour to construct a terminology which shall be more universal than current scientific terms has so far resulted in the introduction of two new terms: persistence and resistance. If this were merely substitution of one term for another, the advantage, if any, would be very trivial indeed. As a matter of fact, however, we have achieved far more than the mere substitution of new terms for old ones, since the new terms are more inclusive than those they are intended to replace. What we have done is that we have generalized a large number of facts and made more general inductions; and the terms persistence and relative resistance are merely the verbal statements of these inductions.

Since we have hitherto introduced no new facts, nor questioned old ones or the doctrines concerning them, no special proof was so far necessary, but merely explanations were needed of the meanings with which these new terms are to be used in this essay. Our 'persistence' is intended to supersede the term 'inertia,' but is used in a more universal sense. The two terms are by no means synonymous. *Inertia* (= *Trägheit*) does not convey a correct idea of the law for which it is supposed to stand. To speak of the tendency of a body in motion as being due to 'inertia,' or *Trägheit*, involves a contradiction of terms; and for this reason alone, even if there were not the more weighty ground that misleading terms are apt to beget wrong conceptions, a change of terminology is desirable. We consider that the term 'persistence' more correctly expresses the idea. It simply affirms a tendency to continue any given state, without necessarily suggesting either rest or motion, inertness or activity. That this persistence (or 'inertia') is universal, is obvious; and its rejection would involve the rejection of the law of universal causation. For it is the most universal of human experiences that in order to change the state of any body some effort (causation) is necessary; and persistence, in the sense in which we employ the term, simply affirms that *in the absence of such external modifying agencies no change would occur in the state of any body or substance.*

Resistance we have defined as the quantitative expression of persistence, the latter term merely denoting a quality. By the resistance of a body we merely express the extent to which it can resist the modifying influence of another body; and it is, therefore, the measure of the effort that would be required to overcome the persistence. The advantage of such clearer and more universal statements of general principles lies in the fact that they aid the understanding and assist in the formation of clearer conceptions of the phenomena around us, often leading to new and important conclusions. Such terms as inertia, impact, latent heat, &c., call up in the mind certain specific groups of phenomena, but do not embody general principles or suggest new ideas, or lead to the detection of any errors in accepted theories. In short, they are neither fertilizing nor aiding the critical faculties. It is different, however, when such phenomena are generalized and expressed in universal propositions; the chief value of such universal propositions lies in their fertility—their suggestiveness, and the manifold deductions which at once are sprung, so to speak, on the reflective mind. Whether such propositions be true or not, they alike possess this advantage of fertility, and thereby supply the means of testing their own validity. For, if untrue, or not strictly true, such generalizations are bound to lead to conclusions which will detect whatever error there may be in them. In fact, a slight error becomes by continued deductions correspondingly magnified. It may be laid down as a safe rule of philosophy that an error in any generalization will be the sooner detected the more universal it is, inasmuch as it affords vaster fields of verification, and hence the greater liability of leading to wrong conclusions if not correctly stated.

The third law of Newton, that 'to every action there is always opposed an equal reaction: or that the mutual actions of two bodies upon each other are always equal and directed to contrary parts,' has already suggested several other generalizations, which we have enumerated in the preceding chapter. But, well considered, this law and its corollaries suggest something else to the mind, which suggestion it will be well for us to follow up. It is this, that *bodies modify each other*. What we should particularly inquire into, therefore, is as to the why and the wherefore of this reciprocal modification. But to this end we must approach the phenomena themselves; taking with us our reasoning faculties, but making the utmost endeavour to leave behind all our former notions, theories, or explanations concerning the phenomena we are about to investigate.

Let us watch, then, a series of reactions of such a nature as to be well within our reach of observation. Here are two balls rolling towards each other until they meet: we notice that they modify each other's speed and direction. Then we take a ball and set it in motion, and send after it another ball with a greater velocity. The latter presently overtakes the former, and again an interaction takes place—an interchange of velocities; the first ball having its velocity increased by the second, while the latter will have sustained a corresponding loss of its own velocity. We make a third experiment, sending this time both balls in the same direction and with equal velocities; and then, provided this equality of velocity and direction be maintained, the two balls will have no modifying influence on each other. The same happens when two balls are at rest. While, therefore, bodies do not modify each other in respect of rest, direction, or motion, when both are of equal states—as when both are at rest or are moving in the same direction with equal velocities—we find that the extent of their reciprocal modification in respect of direction or velocity is all the greater the more they differ from each other in either respect. Thus the modification of direction of two moving balls when meeting will be all the greater, the greater the angle formed by their respective paths; or the increase or decrease of their velocities respectively will be the greater, the greater the difference of their respective velocities.

In some of the above experiments we saw two balls modifying each other's direction or velocity; while in some of the experiments no such modification took place. First of all let us note that, whenever the direction or the velocity of one of the balls was altered by the second, the latter experienced a change at the same time, but of an opposite character. Thus, when the speed of the ball *A* was increased by *B*, that of *B* was decreased. Well-known experiments, which it is needless to repeat here, have shown that the gain in velocity of the one corresponds to the loss of velocity in the other: all of which facts are confirmatory of Newton's third law.

We have further noticed that, in those instances in which the two balls had a modifying influence on each other, there was some difference of velocity or direction between the two. For when both were in states of equality, as when both were at rest or when both moved in the same direction and with equal velocities—whether touching each other or not—no such reciprocal modification took place. They only affected each other when in different states, and *while* such difference existed. For, when one ball imparts motion to (or accelerates the velocity of) the other, the reciprocal modification has its limits so soon as an equalization of states or tendencies has taken place. One such ball may transfer its motion to another; in which case the said ball will lose in velocity what the other has gained, and may have its motion arrested. However, the phenomenon of a moving ball coming to rest by striking against another while the latter is moving forward does not clearly explain the principle of action. What actually takes place in such a case is an *interchange* of velocities; and that whether both balls still continue to move after impact, or whether one of the balls should come to rest. What really happens in such a case is the same in principle as when two balls moving from opposite directions strike against each other; in which case each ball, in its tendency to continue in its course, sends the other ball in an opposite direction; and that with a force inversely proportional to their respective powers of persistence. Thus, when both balls are moving in the same direction but with unequal velocities, the hinder one moving faster and striking against the one in front of it, the latter is impelled forward and the former backward. Now, according to their respective velocities and quantities of matter, this backward push of the hinder ball may result merely in a lesser velocity, in being arrested altogether, or in being sent backward. In either case the gain in velocity of the one is equal to the loss of velocity in the other. Whichever may happen, therefore, the principle is a process of equalization; and in its simplest form may be thus stated:—If a ball moving with a certain velocity strike against another ball moving in front of it with a lesser velocity, both balls having the same kind and quantity of matter, their respective speeds would be modified to such an extent until both move with equal velocities. (Of course, as just explained, this interchange of velocities may result in bringing one of the balls to a standstill, or sending it in an opposite direction. In the latter case the equation would have to be represented by regarding the backward motion as a *minus* velocity.)

From this we see that when both balls are already in equivalent states they do not modify each other; and that when not in equivalent states the extent of their modification is proportional to this difference of states, and has its limits as soon as equalization has taken place.

The principle will be found to hold good in each and every case where the phenomena are at all accessible to observation or experiment. Let us take, for instance, two bodies of unequal temperatures and bring them into contact with each other, and we shall see the same principle exemplified. They will modify each other's thermal states until their respective temperatures are equivalent, after which point no further modification will take place in respect of temperature. Nor will two bodies of the same temperature alter each other's thermal states [1].

Or, again, we may take two springs of unequal elasticity, compress them, and allow them to press against each other; we shall find that, in their endeavour to expand, they will press against each other unequally, each tending to push back

[1] Of course we have here in mind the imparting of heat by radiation, and do not include substances which would act chemically on each other. In the latter case it would be a case of generation of 'heat,' the cause of which we do not know yet: but of this more in a subsequent chapter.

the other. But, neither being able to expand completely, both will expand inversely proportionally to their relative strength or power of expansion; that is, they will modify each other until the expansive force of the one will exactly neutralize that of the other.

(This power of reciprocal modification of bodies has in all cases its limits; and when that state is reached we shall say that both are *equivalent* to each other, be it in respect of motion, of pressure, of temperature, or of chemical affinities. And the extent to which any two bodies can modify each other respectively will be the measure of their relative resistance. When, therefore, two bodies in reaction with each other have reached the maximum of modification, that is, when each can successfully resist the modifying tendency of the other, we shall call that state one of relative equilibrium.)

In short, wherever observation is possible, we can trace all changes to interactions of bodies; and the interactions of bodies to some difference in respect of some state, tendency, or quality. In many cases this may not be quite so obvious as in the cases above instanced; yet, by following up the course of events, it is often possible to convince ourselves of the truth of the above conclusion. If we see the waters of a river running towards the sea, we may not be able to see without reflection that this reaction is due to such a tendency of equalization between two bodies. But it is different when we take two vessels in which the water stands at different levels, and then establish communication between these two vessels. The water will then be seen flowing from the vessel of the higher level into the other vessel where the level is lower; and this flow will continue until the levels will be equal in both, if the fluid in both be the same; or until the pressures are equal, if the liquids in the respective vessels be of different densities (in which case the levels will be *equivalents*). For the present we will leave out of consideration the influence of gravitation, as if we knew nothing whatever about this force, and merely note the fact that, if free to interact, the masses of liquids in different vessels will tend to establish equilibrium of levels.

With this knowledge in our mind we can follow the course of the river and see it flow from the higher level to the lower level of the ocean. And if we could imagine the level of the ocean raised to the height of the river's source this downward flow of the latter would, of course, be arrested, in the same manner as the flow of water from one tank to the other is arrested as soon as the levels in both are equal.

The same holds good of air-currents, which we must ascribe to difference of pressures, no matter to what causes these differences may be due. This, too, may be verified experimentally. We may take two vessels, in one of which the air has been compressed to a greater extent than in the other. If communication between these two vessels be now established, the compressed air of the one will flow into the other of lesser density (causing an air-current, or 'wind') until equilibrium of pressures has been established. Or we may vary the experiment as follows:—Take a vessel and compress air in it. If an opening be now made in this vessel, the air will rush out with a force proportional to the difference of pressure inside the vessel and that of the outside atmosphere. But if this vessel were placed inside another vessel in which the air is kept at the same pressure, and communication were established between the two, no such rushing out of air would take place.

In each case two things are manifest: (1) that bodies do not modify each other when already of equivalent states (in respect of any quality or tendency); (2) that any reciprocal modification, when observed, ceases as soon as relative equilibrium is established. We thus arrive at two important conclusions as to the causes of the interaction of bodies and the extent to which interacting bodies can modify each other. The (proximate) *cause* of interaction, or reciprocal modification, of any two bodies, or systems of bodies, is a difference in state, tendency, or quality; and the *extent* of this reciprocal modification is conditioned by the degree of difference. This last conclusion is verified by the fact that any reaction (or extent of modification) is the greater, the greater the difference, diminishes as equalization proceeds, and ceases altogether when equilibrium is established.

Moreover, by altering the qualities or tendencies of any two bodies, we can make them capable of reacting with each other. On the other hand, we can make any two bodies 'inert' to each other (that is, incapable of reciprocal modification) in respect of any quality by previously making them equal in respect of such quality. Thus by making two bodies of equal temperature before bringing them into contact we shall make it impossible for these to alter each other's thermal states. On the other hand, we can confer on bodies the power of modifying each other's thermal states by previously imparting to the one a temperature above or below that of the other.

Now any agency which is capable of producing a phenomenon, or that will occasion an increase or diminution of a manifestation, and that in proportion as that agency varies, must be held to be the cause thereof. But, apart from this philosophic axiom, we have seen that in every case where observation is possible all reactions can be traced to an interaction of bodies, consisting of reciprocal equalization of some tendency or quality; and that the reactions cease entirely when modification has proceeded so far that both bodies are in equivalent states.

This view disposes of the necessity for assuming 'forces' or 'energies' as separate entities to account for phenomena; as under it these can be explained without any such assumption. We need only bear in mind that *every* reaction is mutual; that is, that any change in any body under observation is accompanied by a corresponding change in some other body, and that this change is due to an interaction of such two bodies in respect of some state (quality or tendency) which they possess in an unequal degree.

This latter conclusion follows as a philosophical necessity from Newton's third law, which, as we have seen, is borne out by observation and experiment. But, though this law is universally accepted as true, its full import and meaning are completely overlooked in physical philosophy; being overshadowed by certain deep-rooted notions concerning the causes of phenomena, which notions may be attributed to the dualistic conception of the universe—a subject of which we shall treat more fully in the next chapter but one. We are too prone to regard a change observed in any one body as being an isolated and complete phenomenon; forgetting that it is not a separate event, but an essential part of the endless succession of events, which not only has its antecedent and consequent, *but is accompanied by a* *simultaneous change in some other body.* The mind is not puzzled for an explanation of a phenomenon when both interacting bodies are in view. Nobody seeks for an explanation why a quantity of cold water becomes hotter when in contact with a hot body, or vice versa. It is only when the complementary part of a phenomenon is out of view that such explanations are searched for. But, then again, we do not remember Newton's second rule of philosophy, to which we have assented in our schooldays with so much approval, viz. that 'to the same natural effects we must, as far as possible, assign the same causes.' We do not try to explain such phenomena by attributing them to causes like those to which other like phenomena have been traced, but prefer to turn to our imagination for an answer. It is the 'mind' which is appealed to for a solution. But, alas! this 'mind' has originated in the remote and ignorant past, and can see things only through the ancestral spectacles, of which we are the unfortunate (as well as fortunate) inheritors. Our ancestors explained phenomena on two assumptions: a passive and inert matter—a body without passions; and an active principle—or passions without body. When, therefore, but part of a phenomenon was observed—a change in a body without seeing the concurrent change of another body—such change was at once laid to the charge of this active principle, variously called at different times soul, spirit, force, or energy.

As we proceed we shall find, not only that any such assumptions to account for the various phenomena can be dispensed with, but that many vexed problems of the physical sciences, which under the present dualistic theory defy explanation, will easily yield up their secrets when examined in the light of inductions derived from observation, and not from inherited ideas.

CHAPTER IV

THE GENERAL TENDENCY OF NATURE

LET us recapitulate the various steps of our reasoning. From Newton's first law we have been led to the law of persistence; in his third law we have found expressed the law of reciprocity; which in its turn has now led to a third and important law, which henceforth we shall refer to as the law of equalization.

But there is another obvious conclusion which directly follows from the above generalization, which demands our attention. It is this: If the law of persistence be true, and, what seems to us a necessary corollary, if it also be true that the tendency of bodies in different states is towards equalization, then the conclusion seems inevitable that the tendency of nature is towards equilibrium: a conclusion which seems to be contrary to experience, and which is opposed to currently accepted notions.

There are obvious objections to this conclusion that the general tendency of nature is towards equilibrium. In the first place it may be asked, If the tendency of nature be towards equilibrium, then how comes it that two or more bodies which already are in equilibrium do not remain so? What is it that disturbs them out of their equilibrium, if you deny 'forces' on the one hand, and affirm the universal tendency towards equilibrium on the other? As a further objection, on purely theoretical grounds, it might be urged that if this universal tendency towards equilibrium were a fact; if all phenomena were simply due to this reciprocal equalization of states; and if after equalization has taken place the further reaction between such bodies ceases:[1] then

[1] We purposely put here the objection in words which do not correctly

there should be a general and perceptible decrease of activity—a greater and greater approach towards universal stagnation. Instead of which we have nothing but change following upon change, a succession of events, each new change being the cause of myriads of other changes.

The question now to be decided is, Which of the two views is more in accordance with the facts? There can be no question of this tendency to equalization, any more than it could be doubted that changes are continuously occurring. The question then is, Have we to do with two opposing principles, or is the one tendency merely the result of the other?

We unhesitatingly affirm the latter alternative. A direct proof of either conclusion is, of course, out of question; but we can examine and verify or disprove the data on which the two conclusions respectively are based.

Be it understood that we are not now thinking of the general *aspects* of nature, but of *tendencies*. Of course there can be no question as to the constant changes around us. They are the most universal of all phenomena; so much so that nature might be defined as a succession of changes. But then the question we are now considering is, not as to what actually does happen, but rather the why thereof—the causes of these constant changes. And the answer to this question is as plain as universal; for in each case, where observation is possible, we do find *reciprocal* modification only; and, where two bodies are alike both in kind and degree, such reciprocal modification does not take place. Moreover, when two bodies are acting on each other, the reaction is the more powerful in proportion as those states in respect of which they are reacting with each other are farther apart; and, in proportion as equalization between two such bodies is approached, so the intensity of the reaction diminishes. Two bodies of unequal temperature will react with each other more violently in proportion as the difference of their thermal states is greater. At first the one body will be cooled and the other heated at a much greater rate

represent our generalization, so as to make the objection more forcible. We did not say that after equalization between two bodies they are incapable of further reaction; but merely that they cannot react while in that state of equilibrium.

than when their thermal states more nearly approach to equilibrium[1].

This rate of decrease in a reaction is capable of ascertainment and of mathematical expression; but with such quantitative statements we are not here concerned. Sufficient for our purpose is the universal and indisputable fact that the intensity of a reaction between two bodies (or their reciprocal modifications) is the greater, the greater the degree of difference between them in respect of which they are reacting with each other.

To this we may add two corollaries, both of which are equally universal facts: firstly, that the reaction goes on in an ever diminishing ratio as this difference more nearly approaches towards zero (ceasing altogether when this point is reached); and, secondly, that two bodies alike in any quality cannot modify each other in respect of such quality. Two bodies in equilibrium cannot react with each other; but if the state of one or both be changed, so as to disturb this equilibrium, then a reaction can again take place between such two bodies; which reaction will again result in an equalization.

But, it may be objected, if the tendency of nature is towards equilibrium, how is it that we observe nothing but an incessant series of changes? The answer is, that it is this very tendency to equalization which, being due to the universal persistence of bodies, prevents the attainment of universal equilibrium *on account of the unequal resistance of bodies*, as will presently be shown.

We shall be able to illustrate this by an interesting experiment. Take two magnets, which we will call *A* and *B*, both delicately poised, or suspended by threads, just within each other's magnetic field, and allow both to come to rest. Now move magnet *A* round an arc of 90° and retain it in this position. It will then be observed that the other magnet does not instantly follow the motions of the first, but that a sensible time will elapse before it begins to move out

[1] If a thermometer be immersed in a hot liquid, the mercury at first rises suddenly, with a gradual and visibly decreasing velocity; and the last few degrees the mercury creeps up very slowly. The same thing can be observed with a pendulum, the arcs described by the latter decreasing in each successive swinging; and the rate of decrease becomes less and less, until the final swingings are scarcely perceptible.

of its position. At first it will begin to move very slowly, the speed gradually increasing until its pole is nearest to the opposite pole of the other magnet. It will not come there to rest, however, but proceed further in the same direction (by virtue of its persistence), its speed now gradually diminishing until it comes to a momentary rest beyond the point towards which it was attracted. Presently it will return towards the pole of the other magnet, its speed again increasing until just opposite the point of attraction; again proceed beyond with a decreasing speed; and keep on thus oscillating, describing lesser and lesser arcs, until it will come to rest with its pole nearest the opposite pole of the other magnet held in position. Here we see the persistence of the magnet manifesting itself, at first by retaining momentarily its original position, and then by persisting in its motion by moving beyond the point of attraction[1].

Let us now somewhat vary this experiment. After both magnets have been allowed to come to rest, let one of them be moved again out of position, as before, and then instantly released again, so as to be free to move, and then note what will happen. The magnet A, thus moved out of its adjustment with magnet B, will return towards its former position, being attracted by the magnet B. A sensible time, however, will elapse before it arrives at its former position, and then, by virtue of its persistence (persisting in its motion), pass *beyond* the point aimed at. But in the meantime the magnet B will have made a motion in the direction in which A had originally been moved; so that, when A arrives opposite where formerly the magnet B was, B is no longer there, having gone—if we may make use of the metaphor—in search of A. But A will now be attracted in the direction where B actually is at the moment; and B will likewise be attracted in the direction where A is. Both will turn towards each other, with the result that both will have left their respective positions by the time the other has arrived at the point towards which they were each in its own turn attracted[2].

[1] In the above description we purposely leave out of consideration the influence of the earth's magnetism, which would modify the point of attraction towards which the magnets are tending.

[2] If the magnets be sufficiently near, so as to be able to fly against each other, so that their poles can touch, then, of course, equilibrium is established almost immediately. But the further apart they are, provided they are

Here we see clearly that this persistence, or the small fraction of time which elapses before the magnets obey the impulse due to a change of conditions, is the cause of the delay in the re-establishment of their relative equilibrium.

If one of the magnets be forcibly retained in position, the equilibrium is re-established much sooner; and that because the point of attraction for the moving magnet remains constant. But, if both magnets are free to move, this coming to rest takes much longer, for reasons already stated. And if, instead of two magnets, we employ several, all suspended in the same plane and within each other's magnetic field, these 'searchings for each other' last correspondingly longer. For these experiments we have used bar magnets $6'' \times \frac{1}{2}'' \times \frac{1}{8}''$; and with four such magnets, disposed at the four corners of a square, these oscillations have lasted for several hours, and that though originally one only was momentarily moved out of position. Now we need only multiply these magnets in imagination to infinity in order to see that the duration of these reactions would last for a corresponding period. For, by the time that the last in the series has been modified in consequence of some change in the first, the first, second, third, &c., would no longer be in position, and would necessitate another adjustment. To put it briefly, *the establishment of relative equilibrium between any two bodies results at the same time in disturbing already existing adjustments relatively to other bodies.*

For instance: Let a hot body be brought into a room in which we will suppose all objects to be of equivalent temperatures; and let such body be brought near another body colder than itself. There will be an exchange of temperature between these two bodies, the colder body being heated and thereby approaching nearer to equilibrium with the heated body. But in that case its equilibrium in respect of its temperature relatively to the other bodies in the room, including the atmosphere, would be disturbed. And, if we remember the differences of resistance (or, in current phraseology, the different conductivities) of the different bodies, it will at once be seen that such a slight disturbance would, as

still within each other's magnetic field, the more distinctly will this persistence, as well as the acceleration and retardation of their speed, be observable, and the longer will it take for them to become quiescent.

in the case of the magnets, result in a large number of disturbances, necessitating an almost infinite series of thermal readjustments, even within such a limited area.

Here, for instance, is a piece of furniture composed of wood, glass, and metal, all of which we will suppose to be of a temperature corresponding to that of the surrounding air. Let the latter be heated, then the metal, offering the least resistance, would be the first to be heated, and thereby become hotter than the wood or glass with which it is in contact, and, therefore, be cooled by the latter. But, being cooled, it will in its turn cool the air, and then again be hotter than the wood or glass, and so on. Now, such myriads of changes take place around us in every second, of all of which the senses give us no information at all. Yet are we sure of their existence, and can demonstrate the same in the same way as every other scientific theory can be demonstrated; i.e. by demonstrating the properties, or tendencies, of bodies in unequal temperature, and then reasoning from the data.

Now, if bodies offered no resistance at all, then a change of temperature in any one place, from whatever causes, would as instantly be diffused throughout the universe; which we know not to be true. Or if the resistance in all bodies were equal, irrespectively of distance, then the rate of diffusion would be equal, and the temperature of bodies throughout the universe would rise or fall together. This, too, we know to be contrary to experience. But Persistence, as well as Unequal Resistance, being a fact, it follows of necessity from reasoning, and is verified by observation, that *each adjustment results in a disturbance of already existing adjustments*, and, since bodies in different states tend to modify each other, necessitates new readjustments.

Take the atmosphere, for instance. It would be very difficult, nay, it would be impossible, to conceive it under this view as ever being in a state of equilibrium. Being in contact with the warmer or colder surface of the earth in its lowest layers, and exposed to the rays of the sun, or other influences, in another direction—quite apart from the many changes of organisms, burning fires, &c.—it can never attain in any respect to a state of equilibrium; more especially when we bear in mind the sluggish nature of atmospheric air. Even when apparently perfectly calm, we need only gently puff some smoke into the air and watch its unequal diffusion to be convinced of the inequalities in the perfectly transparent and seemingly homogeneous atmosphere.

From all of which it is manifest, not only that the doctrine of the tendency of nature being towards equilibrium is not opposed to the facts of continuous changes, but that the latter follow of necessity from this very tendency.

. . . .

Having explained our meaning of the terms equalization and equilibrium, we will now explain the meaning of some other terms which it will be necessary to employ. Equalization having been explained to be the condition of bodies in perfect adjustment with each other, we shall call the opposite state a 'State of Excitation.'

Two magnets, for instance, in adjustment with each other we should describe as being in relative equilibrium with each other. But when this adjustment has been disturbed, and they are in the act of readjustment, or equalization, we shall designate this state by the term Excitation. And when, from some cause or other, they are prevented from readjusting themselves we shall say of such bodies that they are in a 'State of Coercion.'

To recapitulate:

1. Two suspended magnets seeking each other's opposite poles are in a State of Reaction or Excitation.

2. After having adjusted their relative positions they are in a State of Relative Equilibrium.

3. If their adjustment is disturbed and obstacles are interposed which prevent their readjustment, they are in a State of Coercion.

Of the results which follow from these three states we shall treat more fully in a future chapter. Only two remarks concerning these states will be made at present.

Firstly. That, inasmuch as there are inequalities of every kind all around us, it follows that all matter must necessarily be in a constant state of excitation, and that this degree of excitation in any one body may be more or less relatively to other bodies. In other words, the tendency to equilibrium results merely in relative changes of states of excitation. For instance, if a body, *A*, approaches nearer to equalization to a body *B*, it may at the same time be further removed from equalization relatively to other bodies. If, for instance, a piece of cold iron placed on a hot stove reaches the same temperature as the stove, it will be nearer to equilibrium therewith, but further from it in respect of the surrounding atmosphere or other objects. In other words, as already pointed out, an approach towards equilibrium relatively to one body results in a greater departure from equilibrium relatively to other bodies. Hence all matter must necessarily be in a constant state of excitation.

The second point here to be mentioned is that all bodies whose resistance is insufficient to overcome the opposing resistance are in a state of coercion. Two magnets, for instance, lying on the table without being in adjustment exert a pull on each other just the same as when freely suspended. But, while in the latter case they possess sufficient power to overcome the torsion of the string and the resistance of the air, when lying flat on the table they cannot overcome the resistance of friction and gravity, hence are in a state of coercion.

It is needless to point out that such states of coercion are states of excitation; inasmuch as the tendency to equalization remains, and the obstructed bodies are pressing against or otherwise reacting with the obstructing medium.

Another term already introduced, requiring some further explanation, is that of Equivalents. When we say that two bodies are in equilibrium with each other, we simply mean that their relative resistances are proportional and balance each other; but not that they are *equal* in any sense whatever.

Take, for instance, a **U**-tube, and pour water into one limb and oil into the other until both equally balance each other. Then, supposing the water to be specifically heavier by one-tenth than the oil, the column of the former will be proportionally less. As the temperature increases or diminishes—the coefficients of expansion being unequal—the relative proportions of water and oil which would thus balance each other would vary accordingly. Thus a change of temperature in one or both would necessitate a readjustment, not only of thermal states, but also of relative heights. When both fluids just balance each other, the two columns of oil and water should meet in the lowest part of the tube; but, when the water predominates, the oil will stand higher than the bottom of the limb. But in either case we would consider the two substances to be in equilibrium, whatever their relative mass or specific gravity may be.

Thus, too, as regards resistance, to illustrate which we may again take two springs pressing against each other in opposite directions. Say that the elastic force of the one is 100, and that of the other 10; then the latter would be compressed by the former nine-tenths as against one-tenth. So too with attraction. Two bodies attracting each other, the one with a force of 100 and the other with a force of 10, the stronger body would be pulled one-tenth and the lesser nine-tenths out of their respective positions.

Adjustment or equilibrium of any two or more bodies, therefore, simply means *an equation* of relative resistance; and it is to this tendency to equalization that all the phenomena of the universe can be traced.

To summarize: persistence is demonstrated; so is the unequal resistance of bodies. No less certain is it, in all cases where experiment or observation is at all accessible, that the reactions of any two bodies cease at the point at which their relative resistances are equally balanced. (Of this we shall speak more fully further on.) Under this view we can state the law of causation in more suggestive terms, as follows:—

The state of each body is due to the totality of its conditions. Each change of conditions necessitates an alteration in the state of any body; but, such body being in turn part of the conditions of other bodies (either near or far), any change in it involves a change of conditions of all those bodies within its own sphere of influence, necessitating readjustments in all such bodies. These latter again, by changing their states, would in their turn change the conditions of other bodies in adjustment with them; and so on in endless series.

The whole play of nature may, then, be described as an endless succession of changes; each new change disturbing existing adjustments, necessitating fresh readjustments.

If this view be correct, then we should be able to deduce the whole play of nature from these four principles:—persistence, unequal resistance, reciprocity, and tendency to equalization.

CHAPTER V

ON 'FORCES'

No one has yet been found of so great constancy and sternness of mind, as to have determined and set it as a task to himself, utterly to abolish common theories and conceptions, and to apply afresh to particulars an Intellect cleared and levelled. And so that human Reason which we possess is a kind of farrago and gathering of much credulity, much chance, and the boyish conceptions which we first imbibed.—BACON.

LET us remember that the object we are aiming at is to prepare a scientific chart, as explained in a former chapter. It will be well, therefore, to pause for a moment and to compare the results of our surveys, after the manner of topographers; always remembering, however, that our fixed points are to be inductions, or general propositions, which shall enable us to reach any of the lesser points (subsidiary laws, or isolated facts) with certainty and precision; and which shall at the same time serve as starting points for our future (mental) triangulations.

The chief points we have so far marked down on our sheet are *persistence*, *resistance*, *reciprocity*, and *equalization*—four great, fundamental, and universal laws of matter. These laws are not mere theoretical deductions from metaphysical propositions, but may be regarded as general verbal statements of what is actually to be observed everywhere around us. Nor is any subtle reasoning or ingenious apparatus required to demonstrate their truth. As is to be expected of any *universal* law, each one of the above-named principles can be seen at work in every single phenomenon within our reach.

Persistence and resistance—which are merely different names for one and the same thing—are the most universal facts of all human experience, forming, indeed, the basis of all physical knowledge. For, whatever man may intend to do

—whether it be the moving of an object, the changing of the character or quality of a substance, or any other physical act—he always starts on the assumption that he will encounter some sort of opposition to the premeditated change, and at once calculates the extent, as well as the kind, of opposition he will have to overcome. Nobody, however ignorant, expects to be able to effect any tangible or visible change without exertion of some kind. 'Spontaneous' or uncaused changes or transformations are justly regarded as superstitions, and dismissed as belonging to a world totally different from the one with which we are acquainted.

Persistence and resistance, moreover, are the only means by which we become conscious of an external world, and even of our own existence: and we might as well doubt the one as the other. But, if persistence and resistance are facts, then reciprocity and equalization must equally be so; and that because the four are not distinct phenomena, but merely four distinct statements of one and the same identical principle, according to the point of view from which we may regard its manifestations. For when we are simply watching the resistance of *one* body to another we see PERSISTENCE. But if our attention is directed, not merely to the one body to be changed, but also to the other body by which this change is to be effected, then we see the same persistence in both bodies, and this double persistence then becomes—*in our mind*—RECIPROCITY; and the result of this reciprocal resistance and modification becomes—again in our mind—EQUALIZATION.

From this it will be seen that, although we have four laws, all four refer to the same identical manifestation; the difference being merely due to different mental conceptions of one and the same fact, according to the point of view from which it is regarded.

This fact of persistence, with its corollaries, must henceforth be to us what a base line is to a surveyor, viz., starting point and standard of reference. We do not mean thereby, however, that any conclusions derived from this premise shall therefore be accepted as necessarily true. This would be a return to the worthless metaphysical method of making a premise both starting point and final test. In no case and on no account can we dispense with the necessary *a posteriori* verification. All we mean is that, believing the aforementioned principles to be true, we must accept them as such, and draw our inferences with all the confidence with which a careful surveyor will calculate distances or areas on the supposition that the measurement of his base line was accurate. These inductions are to serve us as finger-posts to be relied on until their indications are contradicted by facts. But where the contradiction merely emanates from previous conceptions or theories we must not allow these to carry any weight; at any rate no greater authority is to be given them than to our inductions; and that notwithstanding the great tenacity with which old theories cling to us, tending to clog the 'reasoning ducts' (if the crude metaphor will be pardoned) of the mind.

Disregarding, therefore, current theories, we shall attempt to deduce explanations of phenomena from the above generalizations, and then compare our deductions with actual facts for purposes of verification. By thus deriving our explanations of different phenomena from one and the same set of principles, we shall achieve our immediate object of building up a harmonious conception of the universe; while each subsequent verification of our deductions will be additional confirmation of the truth of our basic principles.

The importance of generalizing known facts and of expressing such generalizations in universal terms, so as to make them inclusive of all known facts, will be seen from the conclusions to which the above-named generalizations lead, and in the modified conceptions they suggest. Bearing strictly in mind, not merely that to every action there is opposed a reaction, but that *every* change is due to an *interaction* of bodies, we shall no longer be liable to use such blundering language as to speak of 'forces' acting *on* matter, and being the cause of phenomena, when but little reflection will show that these 'forces'—or rather the manifestations that have suggested their idea—are *results*, and not *causes*, of the respective phenomena.

But before proceeding further it may be advisable to amend still further our terminology, so as to exclude as far as possible all such terms now current as denote or connote ideas which may or may not be true, but which in any case obtrude themselves on the mind as if they represented facts, and often pass current as such. Such a term now in common use is 'property,' which signifies an attribute, a something *peculiar to* and *inherent in* a body or substance, quite apart from other bodies or substances. Such a conception, however, does not accord with present-day knowledge. Iron in itself has no properties at all that we can know of. We only know of iron by its effects on our senses, or the phenomena to which it gives rise in its interactions with other bodies. Hence, instead of speaking of the 'properties' of iron, we should speak of its *dispositions* towards other substances—a term which suggests relativity; and, instead of speaking of the effects of oxygen *on* iron, we shall speak of the dispositions which oxygen and iron have *for each other*, so as to express the relativity and reciprocity of all such reactions. For, apart from its relations towards other substances, iron can manifest no 'properties' at all; but as soon as we bring it into contact with any other body or substance we simply prove the *relative* qualities of the two substances compared with each other. The oxygen may as truly be said to have been *ferridized* by the iron as that the iron has been *oxidized* by the oxygen. What takes place between the two substances is a reciprocal modification, which, according to our terminology, we would call an equalization of the tendencies of iron and oxygen. But when speaking of the effects which oxygen has *on* iron we attribute to the former a peculiar power inherent in it—the oxidizing power—as if the matter changed by the oxygen were merely passive, and the oxygen alone the active agent. This, indeed, was substantially the view held when the terms were coined, but cannot be held to represent correctly modern ideas concerning the process. In any case the idea of regarding one of two reacting bodies as active, and the other as passive, disagrees with the third law of Newton; hence we should have to abandon either the said law or the above view. We doubt not for one moment as to the reader's agreeing to reject the latter.

This view of one body acting on another, while the latter remains perfectly passive, may be traced to that fertile source of error, the anthropocentric view of nature and the anthropomorphic interpretation of every phenomenon.

'Force' is another term the underlying idea of which is an *active* agency with a *passive* object as its corollary. 'Force' is, in fact, a modern representative of the ancient idea of 'spirits'—the fanciful creations of the infantile imagination of man to account for phenomena. And this necessity of assuming 'spirits' (i.e. invisible beings) arose in consequence of another erroneous view—an error which has survived to the present day—viz. that matter is inert and passive: a view still held, not merely by the ignorant masses, but often preached by professors of science who believe in Newton's third law and sneer at the superstitions of the vulgar.

'Matter' was 'dead'; hence of itself it could do nothing. If, notwithstanding this, 'dead' matter was seen to move and give rise to most remarkable phenomena, this was convincing proof that a *somebody*—differing from man himself chiefly, if not merely, in being invisible—was the active cause thereof. This theory of gnomes, fairies, spirits, &c., is still to be found amongst many races, and in a modified form in the whole human family, with perhaps but very few individual exceptions. This anthropomorphic theory to account for phenomena has been handed down from generation to generation, and is still taught as modern science in all our universities, with the only difference that the invisible some*bodies* (spirits) have become invisible some*things* ('forces'). In still more recent times the idea of 'forces' (in the sense of *vires*) has also been found to be incompatible with current views of nature, and another change has been made, without any essential difference. The change is a mere verbal one, the substitution of one name for another. 'Force' in the sense of causation is now ridiculed by advanced thinkers, and the term 'energy' is put in its place, often boastfully, as representing one of the great achievements of modern thought. The term may be a new one, but the underlying idea is still the same. Instead of disembodied 'spirits,' we have abstractions; and, although the modern conception of 'energy' is more refined than the cruder conception of 'spirits,' the evolution of the one from the other is clearly traceable. It is in vain to pretend that the modern conception of 'energy,' as a distinct entity, is the product of modern science; the facts of to-day could never have led to the current theory of 'energy' had it not been for the inherited fundamental conception of a 'somebody' or a 'something,' outside of matter and apart from it, being the cause of the various manifestations.

This modern idea of 'energy' will be further discussed in a later chapter; here we would only point out the broad fact that the word simply stands in place of an unknown cause. Phenomena are supposed to be explained by saying that they have been produced by this or that form of 'energy'; and the only mystery which still embarrasses physicists is the nature of this invisible 'energy.' Just as at one time life and the phenomena of life were explained by attributing them to the 'vital force'—and even with modern writers the question is not as to what are due organic phenomena, but as to what is life—so likewise in physical phenomena whatever happens is attributed to 'energy,' an independent, though inseparable, existence of which is affirmed with the greatest certainty. Physicists are at present chiefly occupied in studying the nature of 'energy,' its different forms and transformations. The problem is not as to the causes, proximate or ultimate, of phenomena, but as to the nature of this 'energy.'

From the point of view of our generalizations, notably that *all reactions are mutual*, there is no such mental necessity to assume 'forces,' or 'energy,' in order to account for phenomena. Indeed, we shall presently see that the different 'forms of energy' are *results*, and not *causes*, of the phenomena. Persistence, resistance, reciprocity, and the tendency to equalization given, then all phenomena, including all the different 'forms of energy,' so called, can be accounted for. An analysis of a few actual phenomena will help to make our point clear.

Take a steam engine, for instance. The essential parts of an engine are a cylinder and a movable piston; the latter dividing the former into two chambers. In one of these chambers is introduced an expansive fluid, such as steam, having a greater pressure than the air in the opposite chamber. The steam expands until the pressure on both sides of the piston is equal. Were we to introduce this steam into a chamber without such a movable piston, the steam would rush in until the pressure in the chamber was the same as in the boiler. But if to the expansive force of the steam be opposed a movable piston, which, though an obstruction, is incapable of entirely counteracting its expansion, the piston will be pushed forward until the resistance of the air on the other side of it will arrest its further progress.

Here clearly a process of equalization of pressures on the two sides of the piston has taken place. Of this we can be all the more certain because the intensity with which the piston is pushed forward does not depend on the absolute pressure in the boiler, but on the *difference* of pressure between the steam and the atmosphere on the other side of the piston. Experimental verification of this may be obtained in various ways. For let steam of equal pressure be introduced simultaneously on both sides of the piston, and the latter will not move at all. Or let both chambers be simultaneously exhausted, and again the piston will retain its position. But, if we exhaust one of the chambers only, the piston will be forced in the direction of the rarefied space. In each case the power with which the piston is moved will be proportional to the *difference of pressures.*

The action of the steam engine is generally attributed to 'heat'; and calculations are based as to the quantity of work to be got out of a heat engine on the supposed mechanical equivalent of heat—a subject which we shall discuss more fully later on. Here we would only point out an error in reasoning which consists in generalizing proximate causes, instead of endeavouring to lay hold of principles. In the case of the steam engine, for instance, it is the *agency* which is taken as the basis of generalization, while the *principle* on which the action of the agency depends is entirely overlooked. There is not a single instance in which 'heat' *per se* can be shown to result in work; it always depends on the manner in which this 'heat' is utilized. If, for instance, we have a closed cylinder divided by a movable piston and apply 'heat' to both parts of it, the piston will not be moved. But, if 'heat' be applied to one part only, then the piston will be pushed in the opposite direction; and that because the resistance, or expansive force, of the one volume of air would be increased relatively to the resistance of the volume of air on the other side of the piston. In other words, 'heat' must be utilized in such a manner as to create a *difference.* But in that case 'heat' is merely the agency; whereas the true cause of the motion of the piston is a disturbed equilibrium and the consequent tendency to equalization of the pressures in the two chambers.

The practice of regarding the visible agencies by which certain phenomena are evoked as their causes is due to the all too common fallacy known in logic as the *cum hoc*, or *post hoc, ergo propter hoc* form of reasoning. Because 'heat' is the agency by which a steam engine is set in motion, the work done is ascribed to 'heat' itself, and not, as it should be, to the *conditions* produced by 'heat.' A simple illustration in chemistry will at once demonstrate the fallacy of such reasoning. By bringing together nitrobenzol, a metal, and an acid, the nitrobenzol is converted into amidobenzol, a substance materially different in character from the original substance. Now, notwithstanding the fact that it might experimentally be shown that this conversion is due to the presence of the acid and metal, it would be hasty to regard either or both of these two substances as the true cause of this conversion. As a matter of fact it is known to chemists that they are not; since neither the metal nor the acid enters into the composition of the new substance. The change is effected by the substitution of two atoms of hydrogen for the oxygen in the nitro group; and the office of the acid and metal is simply to supply the hydrogen, which, by displacing the oxygen, is the true cause of the transformation. While, therefore, the acid and the metal produced *the conditions* for the production of the phenomenon, they were not the *cause* thereof.

So likewise with 'heat' in the case of the steam engine. It is an agency capable—if properly employed—of producing the necessary conditions, without necessarily being the actual cause of the resulting phenomenon.

As a proof that 'heat' (considered as 'energy') is the cause of work, the mechanical equivalent of heat is generally appealed to. (But in like manner might it be shown that the quantity of nitrobenzol converted into amidobenzol bears a fixed relation to the quantity of metal which has been oxidized.) The mechanical equivalent of heat, however, merely demonstrates the extent to which a disturbed thermal equilibrium can, in its turn, disturb what we may term a mechanical equilibrium. But, if it be correct to speak of a mechanical equivalent of heat, we would also have to speak, as a philosophical necessity, of a mechanical equivalent of cold. For, not only can work be done by cooling as well as by heating (i. e. by abstracting 'heat' as well as by generating or transmitting it), but it can be shown that the amount of work in the one case would be, degree for degree, equal to the amount of work in the other, as we shall presently show.

But first we would like to guard ourselves against being misunderstood on the one hand, and against the charge of misrepresentation, or of putting false constructions on certain advanced theories, on the other. As regards the first, the reader will notice that we use the terms 'heat' and 'cold' as synonymous with heating and cooling; and that because we find it impossible to separate in our mind 'heat' as an entity. To us 'heat' means simply temperature; i. e. *a certain state of matter*, the cause of which is hitherto unexplained, but for which we hope to be able to offer an explanation in a subsequent chapter. True, we can measure 'heat' (or temperatures); but so we can acidity, or hardness, or depth of colour. And if, therefore, we have to admit the existence of 'heat' as a distinct entity, and not merely as a state or quality of matter, we shall also have to admit acidity, hardness, and colour to be indestructible entities.

And now as to the second point. In order to dispel what we consider to be a serious error in modern physical philosophy, we are about to employ the *reductio ad absurdum*; not, however, out of any disrespect to those who hold these theories, but simply because of the acknowledged efficiency of the method. We are anxious, therefore, to show that we are representing fairly the theories we are combating, and are not putting a false interpretation thereon. What we desire to point out is, that 'heat' is regarded as a distinct entity, and not merely as a state of matter; that 'the power of doing work' is attributed to this entity, and not merely to differences in the thermal states of bodies; that, in short, the term 'heat' as currently employed is not used as a relative term, merely denoting a higher temperature as compared with lower ones, but is regarded as a *substance*—'subtle and rarefied to a proportion,' as Lord Bacon would have described it—differing from matter only in having neither extension nor weight, but yet being of a fixed determinate quantity.

The lucid style of Professor Tait, so able an expounder of modern physical science, leaves no room for doubt as to the correctness of the above statement. According to him, 'heat' is a distinct entity, representing for each thermal unit so much work; and which if abstracted from a body would leave the latter absolutely dead and inert. 'It would be impossible,' he says, 'to make it [a body] any colder than the absolute zero of temperature just stated as 274° C. under the freezing point of water. Otherwise an engine could be constructed which would give more work from a quantity of heat than its dynamical equivalent. And this engine would work by taking heat from a body already more than totally deprived of heat[1]!' Which *reductio ad absurdum* is supposed to be a complete demonstration that the amount of 'heat-energy,' or capacity for doing work, of any body depends on the number of heat units which are (or shall we say 'reside'?) in a body; and that after a body has given off all its 'heat,' and has been reduced to the 'absolute zero of temperature' (the absolute maximum of 'cold'?), there is no longer in it any capacity for doing work. Clearly our author is thinking of 'heat' as a 'property,' and not as a *relative quality*; while forgetting all about Newton's third law, mentioned and endorsed by him in another part of the same volume, viz. that to every action there is always opposed an equal reaction, and that in every reaction at least two bodies are concerned. Such grave errors are only possible by hunting after the absolute, and forgetting all about the relativity of phenomena as well as of our knowledge concerning them.

Having found the theory contrary to reason, let us now test it by a reference to facts. Let us take a quantity of water, for instance, and fill a vessel with it. Then let us abstract 'heat' from it; by which process, if the above-stated view be correct, we are abstracting from it with every unit of 'heat' so much capacity for doing work. Let us continue this reducing of its 'energy' until the water freezes and bursts the vessel—which may be a strong iron shell capable of withstanding many tons of pressure. In what equivalents are we to express this, in equivalents of 'heat,' or equivalents of 'cold'?

[1] *Recent Advances in Physical Science* (third edition), p. 125.

Or take yet another case. We place a metal bar between two bodies and heat it; in expanding it will press these two bodies asunder. Result: work. We now push the ends of the heated bar through two walls and secure them on the other side, and then allow the bar to cool. Result: work; and in each case the work will be proportional to the *difference* of temperatures.

Yet another experiment with the thermo-electric pile. The same amount of work can be produced either by heating or by cooling one side of the pile; and in each case the result will again be proportional to *the difference of temperatures at the two ends of the pile*, and will not at all depend on the total amount of 'heat' *per se*. To convince ourselves of this we need only heat or cool both ends of the pile simultaneously.

In speaking, therefore, of the mechanical equivalent of heat, it must be distinctly understood that it can be true only if stated for *degrees of difference* of temperature (not for the total amount of 'heat' *per se*); and that it is quite immaterial whether this difference has been brought about by heating or by cooling, by the 'infusion' or by the 'abstraction' of 'heat,' by the combustion of a fuel or by a freezing mixture.

The theory here set forth concerning the causes of phenomena is essentially different from current theories. Under the latter point of view there is an 'energy' apart and distinct from matter, which acts on the latter. Thus 'heat' is regarded as one form of 'energy'; and the amount of work to be got out of any body is supposed to depend on the absolute quantity of heat-units that are contained in that body (vide above quotation from Tait, parallel passages to which are to be found in the works of other physicists). Whereas we hold that the amount of work to be got out of any body depends, not on any supposed absolute quantity of 'heat-units' in any one body, but merely on the difference of temperatures between two reacting bodies [1].

[1] We cannot conceive that a body having been cooled down to 273° C. could not be cooled down still further. There may be technical difficulties in continuing the cooling; just as there are technical difficulties in increasing temperatures beyond a certain point. But surely our inability to do certain things does not prove the impossibility of such a thing in nature. Nor is this contended, since on that assumption the 'absolute zero' would have to be assumed as considerably higher than 273° C., viz. at the minimum temperature actually obtained. The fixing of the 'absolute zero' at – 273° C.

What can be proved, however, is that work can be got from 'heat-energy' where there is a *difference* of temperature; that it can only be got in quantities proportional to this difference; and, lastly, that in no case can we obtain work out of a body, whatever its temperature (or number of 'heat units'), save in interaction with another body, when the available 'energy' will again be proportional to the *difference* between the temperatures of two such bodies.

Let us test this conclusion. It is held that a body cooled down to 273° C. below zero would no longer be capable of performing any work; and that because there would be 'no energy left in it.' Suppose, that, instead of a body cooled down to the 'absolute zero,' we heated a body to the utmost extent we are capable of doing, and what work could be got out of that body? By itself neither of these bodies could perform any work; to do so they would have to react with other bodies. Suppose, then, that we brought our cold body into contact with another body 'as devoid of energy' as itself; and that the hot body was brought into contact with a body 'as replete with energy as itself'; i.e. so that in each case both bodies shall be of exactly the same temperature; and it will be found that the hot bodies have as little capacity for doing work as the cold bodies; and therefore would have to be pronounced to be as devoid of 'energy' as the others—i.e. if we accepted Tait's definition of 'energy' as 'the power of doing work.'

Before the 'heat' of the one can be utilized for the purpose of doing work, it would have to react with a body colder than itself. But in that case it is no longer the one body that does all the work: the effect would be due to a reaction in which both reacting bodies are equally concerned. Say that the temperature of our hot body was 10,000° C.; then it is clear that the amount of work to be got out of it would be all the greater the colder the body with which it is allowed to react. And, if there were really such a thing in the universe as an 'absolute zero of temperature,' then the maximum amount of work to be got out of the 'heat' of any body would be obtained by allowing it to react with

is based on the theory that at that temperature a body would practically be annihilated—a conclusion which in a previous chapter we have shown to be unwarranted by the facts. (Cf. p. 23, note.)

such a body 'devoid of heat'—i. e. by allowing a *something* to react with a *nothing*[1].

But we have shown that the work performed by a lowering of temperature (*caeteris paribus*) is the same, degree for degree, as by increasing a temperature; and that in case of equal temperatures no work is obtainable at all, no matter what the thermal states of the bodies may be. Newton's law, that to every action there is opposed an equal reaction, or that the reaction between bodies is mutual, &c., is as true of phenomena due to thermal states as of every other group of phenomena. Speaking of two reacting bodies as 'cold' and 'hot' respectively, it is obvious that the cold body has as much share in the resultant reaction as the hot: just as the power of attraction between two bodies is equal, whether we measure the attraction exerted by the body A on the body B, or that of the body B on the body A.

If, instead of thermal phenomena, we direct our attention to any other kind of manifestations, the same principles will be found to hold good. In each case a phenomenon will be traceable to an interaction of two or more bodies, consisting in a process of reciprocal equalization. And wherever there is equilibrium between two bodies in respect of any quality, state, or tendency, there will be no such interaction, and hence no changes.

[1] We say 'a *something* with a *nothing*,' because 'the capacity for doing work' depends, according to the current theory, on the amount of 'energy' in the body, while the body itself is supposed to have nothing to do with the matter. It is this 'energy' which is said to do the work, and, by the above supposition, all the 'energy' was possessed by the one body.

CHAPTER VI

ON 'FORCES' (*continued*)

It is evident, therefore, that our physical views are very doubtful; and I think good would result from an endeavour to shake ourselves loose from such preconceptions as are contained in them, that we may contemplate for a time the force as much as possible in its purity.—FARADAY.

So far, then, as the phenomena above discussed are concerned, the 'forces' (or, if preferred, different '*forms* of energy') must be regarded as *results*, and not as *causes*, of those manifestations in connexion with which they are observed. We may accept the term 'energy' as defined by Tait, as 'the power of doing work'; but not in the sense of being the *cause* of the work done. The cause of this power lies in the different states of the reacting bodies, and the consequent tendency to equalization. Thus the work performed by a steam engine is due to the difference of pressure between boiler and atmosphere; or, what practically amounts to the same thing, to the difference of temperature between generator and condenser. It is owing to this *difference* that there is a power of doing work; and the available 'energy'—or 'power of doing work'—depends on this difference of states, and hence is *a result*, and not *a cause*, of the phenomenon. In short, it is not a something called 'heat energy' to which the action of the steam engine is due, but it is the tendency to equalization between bodies in different states: in the case of the steam engine to the equalization of different pressures or of different temperatures.

We shall now analyze in this light those phenomena connected with the idea of 'force' or 'energy,' and contrast the current theory of an independent 'energy' with the view here set forth. We shall carefully note how far the two theories agree with each other, and in what respects they differ; and, where the two theories differ from each other, we shall appeal to the facts and to such inductions as are accepted as true whichever theory be held. In any such case of divergence we shall not content ourselves with merely establishing a claim for our own theory, but shall in every such case endeavour to trace out the error of the rival theory. This will of necessity involve us to a certain degree in controversy; which, however, is not introduced in these pages from a controversial spirit, but merely from necessity and in the interests of truth.

In our analysis we shall confine ourselves principally to the steam engine, as being a convenient object whereby to illustrate a general principle. The steam engine depends for its motive power on the expansive force of the steam, which, in its endeavour to expand, pushes the obstructing piston. Or we might say that part of the motion of the steam is transferred to the piston, from which again it may be transferred to other objects—some mechanical arrangement, for instance. But the motion of the steam itself, or its tendency to expand, depends, as we have seen, not on its absolute temperature[1], or elasticity, but on its difference of temperature or pressure as compared with some other body.

The essential conditions of a steam engine are two chambers in one of which the pressure is greater than in the other, having a movable object (the piston) interposed. In ordinary steam engines the two chambers are the boiler and the atmospheric air, or boiler and condenser. The two halves of the cylinder may respectively be regarded as forming part of these two chambers. To what causes this inequality of pressure is due, need not concern us here. As is well known, this may be brought about in various ways, but always with the result that whenever the equilibrium in the two chambers is disturbed there will be this tendency to equalization; and, if not resisted by an obstacle the resistance of which is greater than the expansive power of the steam is capable of overcoming, equalization will forthwith take place. Let it be noted also that this equaliza-

[1] We use here the term 'absolute' in a conventional sense, meaning the full temperature of a body as indicated by some instrument, and not the 'absolute temperature' above discussed.

tion would take place—and even more readily and completely—if the piston were not interposed. We say more completely, because in the absence of the piston the pressure at the end of the reaction would be the same in both vessels; while in presence of the piston this may not always be the case. In the following diagram (Fig. 1) let *A B* represent a cylinder and *c* a movable but closely fitting piston; and let the pressure in *A* be greater than in *B*. Then the piston

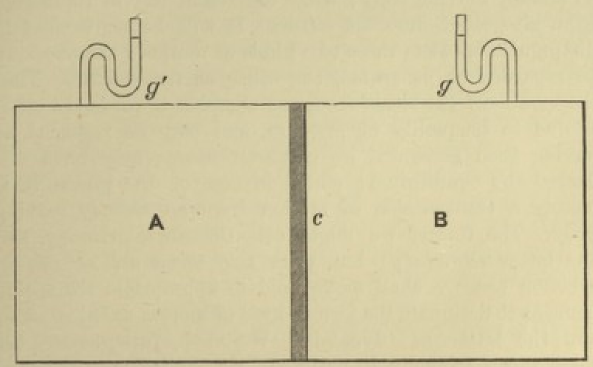

FIG. 1.

c would be pushed towards *B* with a power proportional to this difference, *minus* the resistance offered by the weight and friction of the piston. At the end of the reaction there would, therefore, still be a slightly greater pressure in *A* than in *B*, as could be demonstrated by mercurial or water gauges *g*, *g′*. If free communication between the two chambers were now established, a further quantity of air or steam, as the case may be, would pass from *A* to *B* until the gauges would indicate equal pressures.

The piston, therefore, has really no direct share in the whole reaction, inasmuch as its presence is neither a necessary condition of the disturbed equilibrium, nor is it essential to the subsequent equalization. It is there either by accident or design; but in either case it is passive and acts merely as an *obstructive*. It is to this latter accident of position that its motion is due: not being able to prevent entirely the expansion of the steam, it is pushed forward by the latter.

Now here we have clearly two distinct causes of motion. The motion of the steam from A to B takes place in consequence of a disturbed equilibrium (or a difference of pressures) in the two chambers A and B; whereas the motion of the piston is due to a *transmission* of motion from the steam to the piston. The piston, therefore, derives its motion from an already moving body; while the steam derives its motion from altogether different causes. It will be convenient to distinguish between these two kinds of motion: the one may be regarded as *generated*, the other as *transmitted*. Thus the motion of the steam from one chamber towards the other is due to inequality of pressure, and may be regarded as having been generated by whatever causes may have disturbed the equilibrium; whilst in case of the piston it is merely a transmission of motion from an already moving body. The former we might call, therefore, *primary*, and the latter *secondary*; but, since two terms are already in common use, we shall make bold to appropriate them, and henceforth designate the former kind of motion as 'kinematic,' and the latter as 'dynamic.' We shall, furthermore, use these terms whereby to designate all similar reactions, and that whether they result in visible motion or not. But more of this further on; at present we shall restrict ourselves to actual motions, as affording the most striking illustrations of a general principle. By kinematic *action* (in general) or *motion* (in particular) we shall henceforth mean all those changes, actions, motions, or any other manifestations, which are the direct result of the equalization between two bodies in reciprocal reaction with each other. But when such equalization is obstructed by a third body, and hence motion or some other change is transmitted to this by the kinematic action of the former—in short, whenever change is *transmitted*—we shall designate such motion or change as dynamic.

There would be, perhaps, no necessity to make this distinction were it not that we have to combat a prevalent and grave error, with which we intend to deal presently. But first let us further acquaint ourselves with the phenomenon we have chosen as an object-lesson. We have already shown that equalization between the two chambers would take place more readily and also more completely in the absence of any such obstruction as the interposed piston in the above illustration. In the absence of any such third body the equalization at the end of the reaction would be complete: the loss of *A* would reappear as gain in *B*. But, when such a piston is interposed, part (*and part only*) of this pressure would be utilized in moving the piston, and only the remainder of the expansive power of the steam would be discoverable in the increased pressure in the other chamber. (We are assuming, of course, that both chambers are closed.) When the piston comes to rest, there would still be a difference of pressure in the two chambers, though not so great as before. The increase of pressure in the one chamber would not now represent the full loss of pressure of the other. Nor would the motion of the piston (or 'the work done') represent by itself the full loss sustained by the pressure of the steam. To estimate the exact loss of the latter by the changes produced, all the resultant changes would have to be taken into account; in other words, we should have to add the increase of pressure in the opposite chamber *and* the resistance (or 'work equivalent') of the piston which the expanding steam had to overcome.

The conclusion we wish to draw from this is that the motion of the piston by itself can in no case fully represent the power lost by the steam; any more than does the gain of pressure in the opposite chamber. The fact that the piston moved must be taken as a positive proof that the power of the steam was greater than the resistance of the piston; and that because the former more than neutralized the resistance of the latter, since it actually changed the state of the piston from one of rest to one of motion, which it could not have done had its power of resistance not been greater than that of the piston[1]. Suppose, for instance, that the

[1] Further evidence that the resistance of the piston when pushed forward must have been less than the pressure behind it may be adduced as follows. The piston of a steam engine is pushed forward only when the pressure on the opposite side is less than the pressure in the boiler; and will only be pushed forward until the pressure on the other side *plus* the resistance of the piston itself exactly counterbalance the expansive power of the steam. But the piston in moving forward increases the space in which the steam is imprisoned, and decreases that of the other chamber. Or the steam in the boiler, in pushing the piston forward, at the same time *compresses* the air in

resistance of the piston had been exactly equal to the power of the steam; then it is clear that, since the two opposing powers exactly balance each other, no motion could result. At that point, if we increased the pressure of the one or the other, a corresponding motion would result; either a forward motion of the piston by the steam, or a greater compression of the latter by the former.

As the conclusion to which these considerations lead is of great importance, and is contradictory to a notable theory currently accepted as fact, it will be well to recapitulate our argument, so as to make sure of having made clear our meaning.

1. When steam under pressure is acting on the piston of an engine, it is only because the latter is in the way of the expanding steam. It must be understood that the steam is expanding because its pressure is greater than that in another chamber, and, therefore, is moving towards that chamber in the act of equalization (kinematic motion).

2. The intensity or power of this motion depends on the difference of states between the two reacting bodies.

3. The greater, therefore, this difference, the greater will be the intensity of the reaction; hence a correspondingly greater resistance would be necessary to prevent this equalization from taking place.

4. If an obstructing piston can offer a resistance equal to the power of the steam (which power will depend on its greater pressure as compared with that on the other side of the piston) then equalization could not take place at all, and the steam would be retained in a state of coercion. For before equalization between the two chambers could take place the resistance of the obstructing piston would have to be overcome. But, this being (by supposition) equal to or greater than the power of the steam, the latter—*the steam*—could not move in the direction of the rarefied chamber. (For be it remembered it is the steam that tends to move towards a certain determinate spot, i. e. towards the chamber

the other chamber; hence is doing double work: moves the piston and compresses the air. Where the arrangement is such as here supposed, viz. closed chambers on either side, this is demonstrable; but the principle is the same, though not quite so obvious, when the opposite chamber is connected with the atmosphere. In the former case the air is compressed; in the latter a volume of air is set in motion.

of lesser density. The pressure on the piston is merely accidental, because the latter happens to be in the way. It must also be remembered that the steam presses on all sides of the cylinder equally, provided the external pressure be equal all round; and that, therefore, if the piston be moved, it is only because that part of the cylinder offers the least resistance. It would be easy to construct a cylinder in which the weight of the piston shall offer greater resistance than the fixed sides; in which case the steam would break through the walls of the cylinder without moving the piston.)

If, therefore, the piston does move forward, this is evidence that partial equalization between the two chambers has taken place; and, therefore, is conclusive evidence that the resistance of the piston was not sufficient to prevent entirely this equalization from taking place, but merely partially obstructed it.

5. The extent of this obstruction would be exactly equal to the resistance of the piston; and in overcoming this resistance a proportional amount of the power of the steam would be neutralized.

6. To estimate, therefore, the force, power, or energy (using these terms synonymously) of the steam by the results, we should have to ascertain both the total resistance offered by the piston *and the extent to which equalization has taken place.* These two items added together would be exactly equal to the original force, power, or energy of the steam.

7. Of these two effects that part of the power of the steam which has been utilized to overcome the resistance of the piston we designate as its dynamical effect; and that part which has resulted in equalization with the other chamber as its kinematic effect.

Trusting that we have made clear the meaning in which we shall henceforth employ these terms, we may proceed to the consideration of the error above alluded to, which we shall show to be due to a confusion of ideas owing to the employment of vague phraseology. In a philosophic sense the term 'work' is (or *should be*) used to denote the actual pressure exerted by the imprisoned steam. In common parlance, however, this term is employed to denote the amount of useful labour performed, or of service rendered to man. Now it will be obvious that the two things are not the same. If a body presses against another body, then in a philosophic sense the pressure exerted is the measure of 'work' performed by that body; but such 'work' need not necessarily be of any service to mankind. In the case of steam engines, however, the *work done* is always estimated by the utility of such 'work,' as measured by its industrial value. But from the above considerations it will be clear that we obtain such useful 'work' by only partially obstructing certain processes of equalization. The body we wish to be moved must be placed in the way of an already moving body, or of a body about to be moved. But if such obstructing body should offer a resistance equal to or greater than the expanding steam can overcome it will not be moved at all. Under such conditions our object would not be achieved, but 'work' would be performed all the same—indeed, under such conditions *the steam would exert its maximum power on the obstructing piston.*

What we are about to combat is the doctrine known as 'the dissipation of energy,' advanced by Sir William Thomson (now Lord Kelvin) and since accepted by leading physicists [1]. Because the theoretical amount of 'work' cannot be obtained from any generator, the conclusion is drawn that some 'energy' is *dissipated.* The term ought to be *lost*; but, inasmuch as this would ill accord with the theory of the conservation of energy, the word 'lost' has no doubt been considered objectionable, hence the substitution of the other term.

We are not quite sure that much consolation is to be derived from this substitution of one term for another, since the conclusions to which the doctrine leads are in either case alarming. If we admit a loss of 'energy,' owing to our using it up in our factories without being able to renew it, it is clear that we are drifting towards bankruptcy as regards this particular commodity. But, if 'energy,' instead of being lost, is merely *dissipated* then. . . . But we had better let the

[1] We may say, however, that we are combating this doctrine as a preliminary to an attack on the doctrine of 'energy' itself, as currently taught. We are choosing this roundabout method because the theory of an independent 'energy' derives its greatest support from the supposed harmony of its various applications and subsidiary theories. Our object, therefore, is to show first that the several deductions from the doctrine of 'energy' neither agree with the facts nor are logically compatible with the parent conception of 'energy.'

defenders of the 'energy' theory themselves state the 'inevitable conclusions' to which this doctrine has led them.

'Joule,' says Balfour Stewart[1], 'proved the *law* [?] according to which work may be changed into heat; and Thomson and others, that, according to which heat may be changed into work. Now it occurred to Thomson that there was a very important and significant difference between these two laws, consisting in the fact that, while you can with the greatest ease transform work into heat, you can by no method in your power transform all the heat back again into work. In fact, the process is not a reversible one; *and the consequence is that the mechanical energy of the universe is becoming every day more and more changed into heat*. . . . Now, if this process goes on, and always in one direction, there can be no doubt about the issue. *The mechanical energy of the universe will be more and more transformed into universally diffused heat, until the universe will no longer be a fit abode for living beings.*'

The same view is endorsed by Professor Tait in these words:—

'Thus the energy of the universe is, on the whole, constantly passing from higher to lower (*sic!*) forms[2], and therefore the possibility of transformation is becoming smaller and smaller, so that after the lapse of sufficient time all higher (*sic!*) forms of energy must have passed from the physical universe, and we can imagine nothing as remaining, except those lower forms *which are incapable*, so far as we yet know, *of any further transformation*[3].'

The italics are ours. In plain English what Professor Tait tells us means that 'energy'—which is 'a power of doing work'—is of a fixed and definite quantity, and indestructible; but that this indestructible 'energy' may be deteriorated ('pass from higher to lower forms'), the deterioration consisting in and being measured by the decrease of this 'power of doing work.' Now in common parlance and in common-sense reasoning, when a thing has been used up we generally speak of it as having been consumed, even though what has been consumed be merely a quality. If, therefore, 'energy' be merely 'a power of doing work,' and this power can become less and less until it is equal to *nil*, then it is clear that 'energy' is consumable; and no amount of dialectics, or substitution of terms, can get us out of the difficulty. Of course, 'energy' having been proclaimed as of a fixed quantity and indestructible, its ultimate destruction could not well be

[1] *The Conservation of Energy*, pp. 141, 142. (The italics are ours.)
[2] Are we to understand from this that 'energy,' though indestructible, may yet be deteriorated?
[3] *Recent Advances in Physical Science*, p. 20.

admitted. The fact is that the two views or theories are irreconcilably contradictory, and the discrepancy cannot be bridged over by words. A word must have a meaning; and, where this is wanting, the deficiency is not made good by following the advice of Mephistopheles—

> 'Denn eben wo Begriffe fehlen,
> Da stellt ein Wort zur rechten Zeit sich ein.'

Now 'energy' we are told is not matter, nor yet a mere quality of matter, but is a distinct thing, a something of the existence of which we only know by its 'power of doing work.' But what is 'work'? and in what does this 'power of doing work' consist? Professor Tait does not leave us in doubt as to what he had in his mind, though it is more than questionable whether he himself was conscious of it. He says[1]: 'We have seen that *change* is essential to the existence of phenomena such as we observe; and, that this change may take place, it is necessary that there should be constant transformations of energy.' Just so; no doubt 'change is essential to the existence of phenomena.' But whence the necessity that there should be 'constant transformations of energy'? Surely this necessity does not lie in the theory of 'energy' itself? Were it not for our empirical knowledge of the fact, the necessity for these changes in the production of phenomena could never have been deduced from the theory of 'energy' itself. Yet, if the doctrine of 'energy' were a true induction, we should be able to deduce from it all the particulars which fall under such a generalization—that is, if 'energy' explained anything at all; instead of which the facts are so contradictory that it has become necessary to explain the theory by the facts, instead of deducing the latter from the former.

Let us consider this statement for a moment. Matter is held to be 'dead'; a passive something which cannot change itself, or of itself do anything at all[2]; everything that happens is attributed to 'forces'—or, since this word has lately been outlawed, to 'energy.' All changes, therefore, are supposed to be effected by 'energy'; but that changes may

[1] *Recent Advances in Physical Science*, p. 20.

[2] 'The only real things in the physical universe are matter and energy, and of these matter is merely passive.'—Stewart and Tait, *The Unseen Universe*, p. 116.

take place it is necessary, we are told, that this 'energy' itself should be changed—or 'transformed'—as is clear from the above sentence. But what is it, we may ask, that changes 'energy'? Before 'energy' itself can effect a change, it must itself be changed; 'it is necessary that there should be constant transformations of energy.' So that, instead of accounting for phenomena—or changes—by attributing them to a something called 'energy,' we are actually told that 'energy' itself requires to be transformed. Is this not merely substituting a more difficult problem for a simpler one? It clearly reminds us of the theory that the earth was supported by an elephant, who in his turn rested on the back of a self-supporting tortoise. Clearly this necessity of a support for the earth was due, not to any observations, but merely to the incapacity of the human mind to conceive a body floating in space. So soon as it was realized, not that bodies *can* float but that they actually *do* float in space, it was recognized that it would be just as easy to conceive a self-supporting earth floating in space, 'carrying itself,' as a self-supporting tortoise carrying, in addition to itself, a giant mammoth with a monster globe on its back.

So it is with this 'energy.' If we are to attribute every change to the action of this 'energy,' well and good. The explanation, even if not true, is comprehensible, though not necessarily understood. It is simply saying that an observed phenomenon is due to a something about which we know nothing. But if this theory has to be amended by another; if the same trouble arises with this 'energy' as with the problems for which 'energy' is to account; and if, furthermore, we have to attribute to this 'energy' something contrary to what it is conceived to be; if, in short, 'energy' is to be considered as a definite quantity and indestructible, and yet liable to be 'dissipated': then we believe that the contradictions are sufficiently great to warrant a revision of the grounds of our belief in such theories.

If 'energy' itself must be changed before it can change 'matter,' we shall not have solved an old problem, but merely added a new one to it. But we have already seen that phenomena can be explained without any such assumption. The law of reciprocity and the law of equalization are clearly sufficient to account for all phenomena without the necessity of going beyond actual observation. We can dispense, therefore, with the theory of 'energy'; whereas, as may be seen from the above quotation, the laws of reciprocity and equalization cannot be dispensed with. For in substituting 'active energy' in place of 'passive matter' the necessity for *different states* and reciprocal modification is still felt. We need not, therefore, substitute a something which is unknown for a something which is. And, just as the elephant and tortoise have been knocked from under the earth without in the least altering its position in space, so shall we be able to dispense with this second and immaterial entity, and still leave the phenomena to take place as before; with the additional advantage that we shall have something which is accessible to investigation, instead of having to deal with a fiction of the human mind, whose supposed properties have to be assumed in accordance with the necessities of the phenomena to be explained by it.

But let us proceed. 'Now when all the energy of the universe,' continues Professor Tait, 'has taken the final form of heat so diffused as to produce uniform temperature, it will obviously (*sic!*) be impossible to make any use of this heat for further transformation.' So, manifestly, it is not only matter which is supposed to be 'inert' and to require some active principle to change it, but 'energy' itself seems to be in the same predicament. We shall not be able, or, as Tait puts it, 'It will obviously be impossible to make any use of this heat for further transformation'; and that because if all 'energy' is of the same kind, i.e. 'uniformly diffused heat,' it will itself not be able to change itself, and hence unable to produce other changes; whereas 'We have seen that change is essential to the existence of phenomena such as we observe.' But what becomes, then, of this 'energy' as an active principle? If its action merely depends on change and transformations, there is no necessity to assume its existence; for an assumption it is pure and simple. There is not a scintilla of evidence of its physical existence. We say this notwithstanding the positive assertion of our most eminent physicists to the contrary. It is a purely theoretical deduction, and, therefore, entirely a matter of reasoning, which on closer examination cannot be sustained by either reason or argument. It all turns on the evidence as to whether phenomena could be accounted for without this assumption. If they can be thus explained, then, to put it in its mildest form, there is no longer any necessity for the assumption.

In passing we might draw attention to a curious tendency of the human mind to adhere to whatever notions may have entered it, and, whenever such notions are found to disagree with subsequent observation, to attempt to alter the phenomena—or rather to represent them differently than actually observed—so as to make them fit in with already accepted theories. The true course, however, in such a case would be to endeavour to alter our conceptions so as to make them fit in with external observations, instead of attempting to modify the facts. What Messrs. Thomson, Tait, Stewart, and others have seen are clearly the laws of equalization and reciprocity. Truly there can be no phenomena without changes; and where all bodies are in equivalent states—or, as Tait puts it, when 'heat is so diffused as to produce uniform temperature'—they will no longer be able to effect changes in each other, because, as Newton said over two hundred years ago, 'to every action there is always opposed an equal reaction: or the mutual action of two bodies upon each other is always equal, and directed to contrary parts.'

But we have stronger arguments against this doctrine of a distinct 'energy.' For we shall be able to trace the idea of it, as well as the necessity for the idea, to certain erroneous conceptions, and show that the whole doctrine of 'energy,' with all its astounding and contradictory corollaries, has not been deduced from any facts at all, but is begotten of those conceptions which have come down to us in unbroken succession from our primitive and ignorant ancestors. The facts have merely been distorted in order to make them fit into the mould of the prejudiced human mind. But of this we shall treat in the succeeding chapter; for the present we shall deal with the error which has given rise to the doctrine of the dissipation of energy.

We have distinguished between two kinds of changes, *primary* and *secondary*, or *kinematic* and *dynamic*. We have seen that primary or kinematic changes are due to inequalities of states, and result in the reciprocal modification of the reacting bodies. Confining ourselves at present, for the sake of clearness of expression, to those changes which result in visible motion[1], we may state the problem thus:—Primary or kinematic motion is due to an inequality of states of two bodies; while secondary or dynamic motion is due to a transference of motion from an already moving body to another body that obstructs its motion.

When, therefore, Thomson, Tait, and others, speak of a 'conversion of energy,' they are simply estimating *kinematic motion* by its *dynamical results*. That is, not the actual dynamic work which such kinematic power *could* produce, but the amount of service which is rendered by such change to man in his industrial operations, i.e. the labour performed in the service of man. To convince ourselves of this, we need only look at the means by which such determinations of 'work' or 'energy' are made. A dynamometer in its simplest form is a weight which is raised by the power whose 'energy' is to be determined. Let the source of 'energy' be a boiler; and the contrivance by which the power of the boiler is being tested a steam engine connected with a winch and weight. Let this weight of 1,000 pounds be raised one foot high in one minute; then the power of the steam in the boiler, or its 'energy,' is put down as equal to 1,000 foot-pounds per minute. (To this, of course, is to be added the friction of engine and mechanism, which we may, however, assume to be included in the weight of the body, so as to simplify our explanation.) Now this by no means represents the total power of the boiler; for this reason, that if the weight (plus friction, &c.) be exactly equal to the power of resistance which the steam in the boiler can overcome there could be no visible result at all. To obtain *mechanical* results, i. e. to get some labour performed, the resistance of the obstructing body must be *less* than the power we wish to utilize. But, if the resistance of such obstructing body be greater, then the boiler would exert its *maximum* power on that body, but the latter would not be moved at all. For when the body acted on does move it is evident that equalization is taking place between the active body (in the supposed case, the steam) with a body other than the one it is pushing (in the supposed case, with the volume of air with which it is equalizing).

[1] We shall deal with other phenomena in a subsequent part, and show that the principle is the same in all phenomena.

This can be demonstrated by the following simple but conclusive experiment. In Fig. 2, *A* is a glass tube bent **U**-shape as shown. Its shorter limb is closed by a dead-weight valve *b*, which is made cup-shaped for the introduction of weights. The valve is ground on to its seating so as to close the tube perfectly, and can be raised without any friction. Let us now pour some water into the tube at *a*, until it escapes at *b*, and the level becomes constant. Say that the level is then at *a′*; then the column of water in the longer limb would exactly counterbalance the water, plus the weight of the valve, in the shorter. If ever so little water be now added to *a*, its pressure would be greater than in *b*; the valve would be raised, and a corresponding quantity of water would escape, after which the valve would again close. Now it is well known that *if the bore of such a tube be uniform and equal to the area of the valve* any additional weight on the valve would sustain an equal weight of water in the opposite column. Let us place 100 grammes of mercury into the valve and introduce an additional 100 grammes of water into *a*, and the two weights will exactly balance each other. The quantity of water in *a* would exert its maximum pressure on the valve *b*, and vice versa; but, the pressures being exactly equal, the valve would not be raised at all. To raise the latter we should have to introduce a quantity of water in *a*, whose weight would be in excess of that of the valve; in which case either the extra quantity of water would escape, or, if the arrangement be made such that the water could not escape, the valve would be raised to a corresponding height.

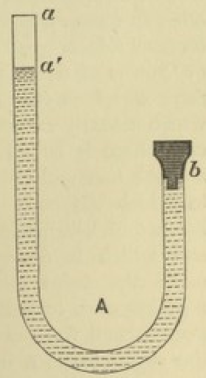

FIG. 2.

In the above illustration we have not to deal with any so-called 'transformations of energy'; it is merely a question of the balancing of weights. Yet, if the weight of the column of liquid in *a* were to be determined by the weight and distance travelled by *b*, there would be a discrepancy, a loss of gravity, and hence, by parity of reasoning, a 'dissipation of gravity.'

The error is due to a confusion of the philosophic meaning of the term 'work' with its conventional meaning of industrial labour.

Pressure is 'work,' and so is motion. When, therefore, one body has been raised or moved out of position by another body, this is evidence that the latter has been stronger, more powerful, than the body which it has moved out of position. But this fact is so well known that it is no wonder that it should escape notice. When the beam of our balance is exactly equipoised, we very naturally conclude that the weight in either pan is equal to that of the other; and, when one pan exerts a greater pull than the other, that the weight on that side is greater. Yet, when estimating the efficiency, or 'power of doing work,' of a body, we do not measure *the force which is just necessary to arrest its motion*, but that which actually causes it to move a certain distance; in which case there is bound to be a discrepancy, an apparent loss; and the loss will be all the greater, the greater the distance to which the body has been moved, and that because the balance of kinematic power—which resulted in equalization, and which is disregarded—would be all the greater. To make the point quite clear by an analogy, suppose that a grocer were to retail a hundred pounds of sugar in single pounds, and were always to make the pan which contained the sugar to be a little lower than the one in which were the weights, a loss of sugar would be the inevitable result. But this loss, be it remembered, would merely be a financial loss to the grocer, and not a real loss of sugar.

Of the fallacy of the doctrine here combated we have, however, even more positive proof than the foregoing considerations, conclusive though these seem to us in themselves. For not only have we succeeded—to our own satisfaction, at any rate—in disproving the doctrine of the dissipation of energy by such reasoning and experiments as have just been given, but we were able to find the source of the error on which this doctrine is based, and which we shall now submit to the judgement of our readers. We shall do this at some length, firstly because the error is as fundamental and far-reaching as it seems to be deep-rooted in the minds of modern physicists; and, secondly, because it affords a splendid object-lesson in illustration of our contention in the opening chapters as to the extent to which conceptions can dominate the mind and make even observation itself unreliable. We shall endeavour to show that none of these conclusions have been deduced from facts and observations, but from theories; and that the whole doctrine of 'energy' and its subsidiary 'laws' and inferences are based on a series of theories which may be traced back to a false conception as their fountain-head.

Thus the conclusion that in course of time all 'energy' will be 'dissipated' is based on Sir William Thomson's conclusion that 'while mechanical energy can readily be converted into heat energy, heat energy cannot be reconverted completely into mechanical energy.' But it is doubtful whether Sir William Thomson himself would have reached this conclusion had he not already accepted as a fact Joule's conclusions regarding the 'mechanical equivalent of heat' as stated by the latter. But Joule himself has based his reasoning on an assumption, as will now be shown. Joule accepted the foot-pound-time unit whereby to measure mechanical effects. What he did was to allow a weight to fall a certain height and thereby impart motion to a certain mechanism moving in water; he then estimated the increase of temperature in the latter, and expressed this increase of temperature in foot-pounds. Now it will be clear from pure reasoning, based on our general knowledge of nature, that a certain cause will produce a certain definite result; that under like conditions the effect produced by a certain cause would be the same; and, therefore, it is clear that the one could be expressed in terms of the other. But it is equally evident that if our unit measure be wrong, being assumed either larger or smaller than it actually should be, the result must equally be wrong, notwithstanding that the underlying principle be perfectly correct. We may measure a ton of water with a litre, and express the weight by its equivalent volume; but, if in thus measuring the water we do not take care that our measure shall always be full, the volume-equivalent would be too great and the weight-equivalent too small. And this is practically the error Joule has committed, owing to his assumption that a certain weight descending a certain height in a certain time is the measure of its mechanical power. The fallacy of this follows from the foregoing considerations, as will presently be shown.

But let us look first at the manner of Joule's experiments and his method of interpretation. The descending weight, by causing some paddles to revolve, agitated the water, and thereby caused an increase of temperature in the same. This increase of temperature he has ascribed to the mechanical effect of the descending weight. But where he committed the error was that he assumed *the whole power* of the descending weight to have been expended in driving his mechanism; which, however, is not true, as will presently be manifest. The mechanism itself offered a certain resistance which had to be overcome by the weight attached to it before the latter could descend. Had this weight been just equal to the resistance of the mechanical contrivance, the latter would not have been moved at all; yet *pressure would have been produced, consequently also work.* To produce motion in the mechanism *the weight had to be greater than the resistance of the latter.* And as Joule's mechanical contrivance has been set in motion, and the weight did descend, we possess conclusive proof that the power of the weight was greater than the mechanical effect in his paddles; and hence *greater than the corresponding thermal effect.* But *by how much greater* Joule never determined, or even thought of determining, since he assumed that *the whole effect of the descending weight* had been utilized in driving his mechanism. On reflection, however, it must be obvious that this could not have been the case; for when the weight had descended to the distance at which Joule severed it from his mechanism there was still power (or 'energy') left in the weight—i. e. the tendency to continue with a power equal to the velocity it had acquired at that point.

To see more clearly the point we are endeavouring to elucidate, we may simplify the apparatus. A mechanical contrivance may always be regarded as a load, or resistance, and may be represented by an equivalent dead weight. Let P, a weight attached to one end of a rope passing over a pulley, represent the weight employed by Joule, and W, another weight attached to the other end of the rope, represent the resistance of Joule's mechanism. We shall then have the simple contrivance of a cord passing over a pulley with a weight attached at each end.

In what follows it will be convenient to assume the pulley to have no friction and the cord to have no weight.

If P and W were of equal weight, then it is clear that they would balance each other, and hence remain stationary. By pulling P downwards W would ascend; but in withdrawing the pull the weights would again remain in whatever position they may have been at the time of withdrawal—*save for the important and not to be ignored fact that both bodies would have the tendency to continue to move in their respective directions.*

Assume the weights of W and P to be ten pounds respectively, and let us add one pound to P; the latter would then commence to descend, and in doing so increase its velocity.

W has been raised then by a power greater than itself.

We have shown above that when kinematic power is transmitted to a third body, resulting in a motion of the latter, the former must always be greater than the resistance of the latter. And the greater the distance travelled, or the velocity of the motion of such third body, the greater will be the excess of the kinematic power over the dynamic effect. Thus, to revert again to our pulley and weights, eleven pounds at P would cause the ten pounds at W to ascend with a certain velocity; but if we doubled the weight of P, without altering that of W, the velocity would be greater. In the one case the descent of P would be due to a weight of

$$11-10 \text{ lbs.} = 1 \text{ lb.}$$

In the second case we should have

$$P = (20-10 \text{ lbs.}) = 10 \text{ lbs.}$$

or a surplus of ten pounds as against one pound in the former supposition.

What is evident from these experiments is that part of the power of the descending weight, P, has been neutralized by the resistance of W; or, in Joule's experiments, by the resistance of his mechanism. And it is precisely this part of the work which corresponds to the thermal effects observed by Joule.

Joule, therefore, has estimated the thermal effects produced by direct thermo-metrical measurements; but he did not do the same for his mechanical unit. He *assumed* the latter to be entirely converted into 'heat,' and then calculated the value of the mechanical unit by dividing it by the number of his thermal units. What Joule determined experimentally was that which was known before, i.e. that friction increases temperature. And from this increase of temperature he calculated the mechanical equivalent of the power which he employed to drive his contrivance—assuming, as a matter of course, that the whole of the power employed was thus utilized.

The fallacy will become glaring if we try to reverse the process. Let us suppose, then, that it had occurred to Joule to determine what is called the mechanical equivalent of heat by the reverse process. Instead of estimating the 'heat' produced by friction, and then dividing the assumed mechanical power by the number of heat units—as Joule did—he might have estimated the mechanical effect produced by 'heat,' and then have calculated the heat equivalent of work by dividing the calculated thermal units (which no doubt he would have assumed as being all converted into mechanical effect) by the resultant work expressed in foot-pounds. Manifestly he would, in the latter case, have given too high a heat value to the mechanical effects; just as in his actual reasoning he has given too high a mechanical value to his heat units. Equally clear is it that, if this process had been adopted for determining the mechanical equivalent of heat, the process would not be a reversible one without sustaining a loss. Only in this case it would not be a loss of mechanical power, but of 'heat.' On this supposition all 'heat energy' could be readily converted into 'mechanical energy,' while the latter could never be completely reconverted into 'heat energy,' for the obvious reasons above given.

Let us see now what would be 'the inevitable conclusion' to which a philosopher, who accepted the above determinations as final, would be pushed [1]. It would run somewhat as follows:—

[1] There can be no doubt that it was purely accidental that the latter process has not been adopted by Joule. For, if he assumed that the two 'forms of energy' have a fixed relation to each other, it is obvious that it should be immaterial which way their relative values are determined; and it could only have been a question as to which method first occurred to the philosopher, or was more easy of execution. The relative equivalents of two chemical substances, say those of calcium and oxygen, could be determined either by determining the weight of oxygen absorbed by a known quantity of calcium, or by determining the weight of calcium that would be oxidized by a known quantity of oxygen. If a chemist made the double experiment, but obtained discordant results, he would regard this as positive proof that an error had been committed somewhere; he may doubt his weighings, or

'Joule proved the law according to which heat may be changed into work; and others that according to which work may be changed into heat. Now there is a very important and significant difference between these two laws, consisting in the fact that, while you can with the greatest ease transform heat into work, you can by no method in your power transform all the work back again into heat. In fact, the process is not a reversible one; *and the consequence is that the heat energy of the universe is becoming every day more and more changed into mechanical energy.* Now if this process goes on, and always in one direction, there can be no doubt about the issue. *The heat energy of the universe will be more and more transformed into mechanical energy, until we shall have a universe devoid of heat and replete with mechanical power*[1]*!*'

We are anxious to have it understood that we are not writing thus in a spirit of levity, any more than did Euclid when he tried to prove the truth of a proposition by applying to its opposite the *reductio ad absurdum*. We have merely reversed Joule's experiments, following in all other respects his own methods of experimenting and reasoning. What we wish to show thereby is that 'heat' can no more be completely converted into mechanical motion than mechanical motion can be completely converted into 'heat'; i. e. under such conditions and by such methods as were employed by Joule.

Thomson's error consisted in confounding the common or industrial meaning of the term 'work' with its philosophic meaning; but the two are neither comparable nor commensurable. Thomson's error was due to his acceptance of Joule's conclusions. Joule, in his turn, arrived at his fallacious conclusions by the double error, firstly of accepting the foot-pound-time unit as a reliable standard whereby to measure mechanical effects; and, secondly, by assuming that the whole of the mechanical power of his motor was converted into 'heat.' And both these philosophers have shared in the fundamental error that 'energy' is a *something* which is transferred from one body to another.

the purity of his substances, or may look for some other error. But in no case would the explanation that one or the other of the constituents is being 'dissipated' be regarded as satisfactory, or even as admissible—*save when some pre-existing theory demands such an explanation.* To regard certain quantities as equivalents where the process is not a reversible one only shows the extent of absurdity into which the mind may be allured rather than give up already accepted and cherished theories.

[1] We will not undertake to say whether in this instance, too, it would be regarded as a conversion from 'higher' to 'lower' forms, or whether from 'lower' to 'higher,' as this would depend on the mental state of the writer. Why 'heat' should be regarded as a *lower* form of 'energy' than 'work,' it is impossible for us to conceive, unless we are to attribute it to the anthropomorphic tendency to regard all as 'higher' which happens to be more profitable to ourselves.

Thus do we see how fertile an erroneous conception is in producing error. And it also illustrates the pertinacity with which ideas that have once gained admittance into the human mind can urge their nine points of law, and thereby refuse admittance to most obvious facts. Verily, as Lord Bacon expressed it:—

'The human intellect, in those things which have once pleased it, (either because these have been received and believed, or because they delight,) draws also all other things to vote with and consent to these—and though the weight and multitude of contrary instances be the greater, yet either it does not observe them, or despises them, or draws distinctions, and so removes and rejects them—not without great and pernicious prejudice—in order that the authority of those previous conclusions may remain unshaken.'

CHAPTER VII

'MATTER' *AND* 'ENERGY'

There is, then, a spontaneous tendency of the intellect to account to itself for all cases of causation by assimilating them to the intentional acts of voluntary agents like itself. This is the instinctive philosophy of the human mind in its earliest stages, before it has become familiar with any other invariable sequences than those between its own volitions or those of other human beings and their voluntary acts. As the notion of fixed laws of succession among external phenomena gradually establishes itself, the propensity to refer a phenomena to voluntary agency slowly gives way before it. The suggestions, however, of daily life continuing to be more powerful than those of scientific thought, the original instinctive philosophy maintains its ground in the mind, underneath the growths obtained by cultivation, and keeps up a constant resistance to their throwing their roots deep into the soil.—J. S. Mill.

'A fundamental error,' says J. S. Mill, in his *System of Logic*, is seldom expelled from philosophy by a single victory. It retreats slowly, defends every inch of ground, and often, after it has been driven from the open country, retains a footing in some remote fastness.' There is a truism expressed in the above sentence, but it is doubtful whether the metaphor is a happy one. Such fundamental errors, if they retain a footing at all, never retreat to a 'remote fastness,' but remain lodged in the inmost recesses of the human mind; still more correct would it be to say that such *fundamental* errors constitute *the* mind; or, at least, are an essential and integral part thereof. For that which is called 'the mind' consists simply of our beliefs, our common knowledge, in the light of which every new experience is viewed and interpreted. While still in possession of the mind, such fundamental errors do not retreat at all; for an error is simply a false belief, rooted in our conceptions, and can exist nowhere else; and to make it *retreat* would mean to abandon it altogether. But such is seldom the case, as such deep-seated errors more often merely change their outward form and reappear under a new guise.

The fundamental error to which we must attribute all those subsidiary errors we have been combating in the preceding chapters can be traced to the dualistic conception of the Universe; and this dualistic conception itself could be shown to be a necessary result of those physical conditions and methods through which what is called the human mind has been developed. As has been said in the opening chapters, every new sensation is referred to prior similar sensations, and interpreted accordingly. Necessarily, the first knowledge of man was of an active and conscious self *and* of an external nature. What he learned of the latter was interpreted according to his knowledge (ideas) of the former. Man's knowledge of the world he lived in must necessarily have commenced in the knowledge of his own sensations, his own doings, and his own motions: a process of development we can see repeated in every child. Hence it is a necessary, and we might add an inevitable, result that the early conceptions of the universe should be anthropomorphic, as well as anthropocentric. A little reflection will show that anthropomorphism must have preceded even anthropocentric ideas: for, as the latter imply reflections as to the causes of the universe and the destiny of man, such ideas could only have originated at a comparatively advanced state of human development. Anthropomorphism, on the other hand, must be regarded as coeval with the dawn of human consciousness; and that because man can only interpret things in accordance with already acquired knowledge; and the knowledge of self—that is, of his own sensations, motives, and purposes—must necessarily have been amongst the first of his ideas. From that dawn forward, everything observed in external nature would naturally and necessarily be interpreted according to such anthropomorphic conceptions as such primitive men would possess. This unconscious anthropomorphism would—again naturally and necessarily—be the ruling method of interpretation through countless generations—i.e. until the human mind has developed to that high order at which it is capable of abstract reasoning. But by that time such anthropomorphic conceptions would have crystallized into fixed ideas, and have become firmly rooted in human speech. When commencing—so to speak—to reflect on the universe, man does not, as indeed he cannot, suddenly divest himself of all his notions. He thinks in the language of which he has been the inheritor; and this language, replete with errors, forms the stock of his ideas. To put it briefly, the ideas of any generation are the result of the thought of all those that have preceded it. The most general of our conceptions are those which have been handed down to us from generation to generation;

'Weh dir dass du ein Enkel bist!'

for we are inheritors, not only of the accumulated experience of the countless generations that have preceded us, but also of their errors. In the course of generations, many theories have been propounded, accepted, and exploded; but those theories which were evolved unconsciously in the very infancy of the human race have never been questioned, but have come down to us unimpaired and still claim dominion over the human mind. Such a fundamental theory is the dualistic and anthropomorphic conception of the universe, which regards 'matter' as 'dead,' tossed about and acted on by a something that is supposed to be the very opposite of 'matter.' This something is as firmly believed in—with but very few individual exceptions—to-day as it was by the Cave-men of ages ago. Substantially it is still the same, though from time to time it has changed its outward form, and has been known under different *aliases*.

Its latest *alias* is that of 'energy,' 'an inseparable though distinct entity'; and as it is necessary to dislodge this fundamental error of dualism, if we are to harmonize our conceptions with our present-day knowledge of the universe, it is imperative that we should combat this theory from all sides, so as to extirpate it completely from the deep-rooted hold it has acquired during all these ages. For with such an error in the mind, under whatever guise, it is impossible to see the plainest facts as they are. Men are in the habit of examining their instruments, of making corrections for refraction, aberration, &c.; but they are apt to forget that their own mental spectacles are made of the crudest of materials, which distort everything. We are wont to speak of 'physical science' as something distinct and apart from 'mental phenomena'; it must not be forgotten, however, that our knowledge of an external world consists chiefly in our views, interpretations, and theories concerning the causes of those sensations which form the basis of our knowledge of a physical universe, and, therefore, is more psychological than physical. *Interpretation* has always a far greater share in what constitutes our knowledge of nature than *observation*. As Mill expresses it, 'What we are said to observe is usually a compound result, of which one-tenth may be observation, and the remaining nine-tenths inference.' Indeed, all our observations can be reduced to a comparatively few sensations: sensations of colour, of shape, of size, of weight, taste, and smell; all the rest is interpretation. Even our distinctions between bodies or substances depend on these few sensations. Bodies are ranked as similar, or identical, if they agree in those characteristics by which they become manifest to us: hence in early classifications bodies were ranked as similar which subsequently have been found to be distinct from each other; and others which are identical have been classified under different heads. To a savage a hard substance is a 'stone,' and a combustible is 'wood.' He can see no difference between different kinds of 'stones,' or stones and metals, until he may have some use for them, and then finds differences in qualities. But we need not go to savages for such illustrations. It is not long since only one tartaric acid was known, whereas at present chemists distinguish three kinds. And in the mass of organic compounds which modern chemistry has brought to light new distinctions between what were considered identical substances are daily being discovered.

A true knowledge of physical science could, therefore, in our opinion, only be attained by a careful regard to the psychical processes through which such knowledge can alone be acquired. It is not sufficient to overhaul the phenomena and to know them by new names. It is useless trying to 'harmonize nature,' as is so often said; nature harmonizes already: what we have to do is to bring our conceptions into harmony with ascertained facts. To this end we must overhaul our conceptions, and modify them as may be found necessary; instead of which, it is more often that the mental conceptions are accepted as facts, and made into a kind of Procrustean bed on which the facts are stretched and hacked about in order to make them fit in with our inherited ideas.

No doubt a vast body of actual facts can be quoted in connexion with such theories as the conservation, dissipation, or transmission of 'energy.' But it is equally easy to show that these facts have been mutilated and distorted in order to make them fit in with the theory of 'energy'—the latter itself being a mental necessity, due to the dualistic point of view. That which can be appealed to in support of the conservation of energy can be shown to be the law of equalization, a process going on everywhere around us; and, if by 'conservation of energy' were simply meant the equation of *states* (i.e. of any quality or tendency) between two bodies, no exception could be taken to the theory. But the doctrine of the 'conservation of energy' means more than this: it affirms the existence of a something for which there is not an iota of evidence. The 'transmission of energy' has also been explained above in accordance with principles which are demonstrable, and which do not require the assumption of any such hypothetical entity; whilst the 'dissipation of energy'—a doctrine which, if it could be shown to be true, would be a direct negation of the 'conservation of energy'—has been shown to be based on what we must call a gross error of both observation and interpretation.

The theory of 'energy' has not been deduced from facts, and our knowledge of it is not due to physical causes at all. The origin of 'energy' is to be traced in the evolution of the human mind only, and not in physical phenomena. It is a theory by which an attempt has been made to account for physical phenomena—in accordance with the dualistic conception—and not a deduction made from physical facts. For since ghosts and spirits had to be abandoned, and the 'forces' (*vires*) have been found to be untenable, this dualism had once more to assume a new guise and reappear under a new *alias*; hence it is known to modern philosophers in its latest dress as 'energy.' The change is not a great one: it is simply the substitution of a Greek word for its Latin equivalent.

Unfortunately, as already pointed out, this dualistic conception of nature is a great obstacle in our road; hence the necessity for this long digression. But in order to combat the doctrine of 'energy' as currently taught, and to expose all the many fallacies of physical science which spring from this most fertile source of error, this discussion is unavoidable. While leading physicists still entertain such ideas as '*dead* matter' tossed about by an '*active* something'—two hypotheses which have come down to us from the early dawn of human consciousness and have been accepted without question—a clear understanding of the simplest facts is impossible. To show that this necessity for a separate entity is due to the (often unconscious) dualistic conception of the universe, it will be sufficient to quote two leading exponents of modern scientific thought.

Du Bois Reymond, in the often quoted passage of his famous address, *Ueber die Grenzen des Naturerkennens*, said[1]—'It is absolutely and for ever inconceivable that a number of carbon, hydrogen, nitrogen, and oxygen atoms should be otherwise than indifferent as to their own position and motion, past, present, or future.' It is strange how any man who has spent his life in a laboratory could make a statement so contrary to facts as the above. The only possible explanation is that, at the time when this strange sentence was composed, the metaphysician had entirely obliterated the man of science. From a metaphysical point of view it is easy to arrive at the conclusion that the elements *should* be indifferent; but to the man in the laboratory it is—or should be—a well-known fact that they *are not*; that not only are iron, phosphorus, sulphur, nitrogen, &c., not indifferent, but are most particular and precise in every respect. They are sensitive to every change of conditions, are particular with what bodies they combine, under what conditions, in what proportions, &c. In fact, all our modern chemistry is based on the fact that the atoms—the *dead* atoms of Professor Du Bois Reymond—are most particular, and not indifferent. And so well known is this even to Professor Du Bois Reymond himself that when he is in doubt as to the constituents of a solution he would prefer to make his interrogation through the medium

[1] 'Es ist eben durchaus und für immer unbegreiflich, dass es einer Anzahl von Kohlenstoff-, Wasserstoff-, Stickstoff-, Sauerstoff- u. s. w. Atomen nicht sollte gleichgültig sein, wie sie liegen und sich bewegen, wie sie lagen und sich bewegten, wie sie liegen und sich bewegen werden.' The above translation is from Tyndall's *Fragments of Science*.

of such 'dead' atoms, rather than trust to his own sense of taste, sight, or smell. The fact, then, is that, whether matter be regarded as 'dead' or 'alive,' it certainly is not 'indifferent.'

The same fundamental idea is contained in the following utterance of Professors Stewart and Tait :—'To reduce matters to order (*sic !*), we may confidently assert that the only reasonable and defensible alternative to our hypothesis [i. e. of energy] (or, at least, something similar to it) is the stupendous pair of assumptions that visible matter is *eternal*, and that IT IS ALIVE [1].' This statement has the advantage over the former in that it plainly implies why the assumption of 'energy' is less 'stupendous' than the other two assumptions. Here we have anthropomorphism in its naked simplicity. 'Matter' is 'dead'; therefore, of itself can do nothing. But in that case 'energy' must be 'alive.' This, indeed, was the idea of those who first conceived the hypothesis in its pristine form. But here again we may point to the fact that 'visible matter' does act and change; and if it be argued that, since 'visible matter' is 'dead,' *therefore* there is a necessity to assume a something outside of it, in order to account for its manifestations, then, manifestly, such a necessity is purely a mental one—one of conception. Professors Stewart and Tait may find it difficult to conceive of matter as being 'alive'; but then the question is, Why should it be conceived of as alive? and, secondly, What is it that constitutes 'life'? It is again purely a matter of conception. If activity constitutes 'life,' then matter can be shown to be active. But if 'life' be regarded as consciousness, or activity with a set predetermined purpose, then clearly we are confronted with anthropomorphism in its crudest form.

The truth is that the whole conception of 'matter' *and* 'force' is false. Both are hypotheses whereby to account in a crude way for the phenomena of external nature. We have dealt already with one of these conceptions—that of 'force' or 'energy'—and we shall now deal with the conception of 'matter,' and show it also to be a mere hypothesis.

Dr. Johnson, when discussing Bishop Berkeley's theory of the non-existence of 'matter,' kicked against a stone, saying, 'I refute it thus.' What was it, it may be asked, that his

[1] *Unseen Universe.* (Preface to the second edition.—The italics and capitals are the authors' own.)

foot encountered? 'Matter'? Surely not; for 'matter' is but a hypothesis, a theory, or an explanation of a phenomenon. His foot simply encountered *resistance*; and anything which offers resistance is called a body, and is said to be composed of 'matter.' Before his foot reached the stone, it passed through the air, which also offered resistance; but this resistance was so slight that the coarse senses gave no indication thereof. In the air there was, we may assume, some aqueous vapour, of which the senses felt even less, hence could not serve the worthy doctor so well as the solid stone. But that same moisture in the form of a hard icicle would have served admirably the doctor's purpose.

Dr. Johnson's foot encountered 'resistance,' and felt a sensation: all the rest is mere inference, theory, and hypothesis. The hypothesis is that the stone—the body which arrested the forward motion of his foot—consisted of inert matter which was *endowed* with certain properties, such as extension, hardness, colour, &c. A skilled metaphysician could subtract all these 'properties' from the body and still have a balance left, a sort of crude material having neither weight, hardness, colour, nor extension; and this residual figment[1] (we can think of no other term by which to call it) was the 'matter'—a sort of raw material—of which bodies were supposed to be composed. Now, when once the mind can conceive of a thing void of those qualities which constitute it and through which alone we can have a knowledge of any existence, it becomes an absolute mental necessity to invent a second and complementary entity, by which, from which, and through which alone such matter can acquire the several qualities by which it is known. Both are figments of the human mind, and can have no concern at all with *physical* science in the true sense.

'Matter' and 'force' are the two complementary parts of the dualistic hypotheses to account for phenomena. They are both hypotheses, and of the existence of either we know

[1] 'A mind just entering on the subject may consider it difficult to think of the powers of matter independent of a separate something to be called *the matter*, but it is certainly far more difficult, and indeed impossible, to think of or imagine that *matter* independent of the powers. Now the powers we know and recognize in every phenomenon of the creation, the abstract matter in none; why then assume the existence of that of which we are ignorant, which we cannot conceive, and for which there is no philosophical necessity?' —*Faraday*.

absolutely nothing. We know only of sensations; and 'matter' and 'force' are the explanations by which it is attempted to account for these sensations. We know only of phenomena: the causes of these are mere matter of speculation. What we can investigate are relative resistance and relative magnitudes. This is the province of physical science. How we conceive these things is purely conventional. We can still speak of 'bodies' or of 'forces,' as a matter of convenience; but we must build no theories on such fictions; still less can they be accepted as established facts wherefrom to make deductions. As we have said before, it is immaterial whether such theories are true or not, provided they serve the purpose of discourse and do not conflict with known facts. This limit, however, has been overstepped; and to-day 'energy' is put forth as the cardinal pillar of modern scientific thought. But, instead of assisting in the study of nature, it positively obscures most obvious facts, and makes explanation impossible, where otherwise—as will hereafter be shown—most vexed problems could yield a most simple and satisfactory explanation.

The dual conception which is at the bottom of all this confusion must, then, be banished from philosophic thought. But which, it may be asked, is to be abolished, 'matter' or 'energy'? Our answer is that as a mere question of terms it is immaterial which of the two be retained; but as a question of conception both would have to be abandoned, since each represents one half of the truth only, and is complementary to the other. 'Matter' is resistance, and so is 'energy'; for we have seen that bodies modify each other, and in so doing both offer resistance [1]. The process is reciprocal: and it is this reciprocal modification which has given rise to the idea of 'energy' or 'force,' from the mental necessity just explained. But, having established the fact that bodies in different states tend to equalize, there is no longer any mental necessity to assume some living 'essence,' 'spirits,' or 'forces,' behind 'matter' in order to account for its

[1] In a subsequent part (Book IV, chap. iv) it is proposed to give new meanings to the terms 'matter' and 'force' by using them synonymously with persistence and resistance as previously defined. This would make it possible to retain both terms (as in any case it would be difficult to banish them from our language) without causing any confusion, or involving contradiction.

changes. We must yield our minds to the force of the fact that bodies of different states *can* and *do* modify each other (or, to express the same thing in the old terminology, 'We know matter by its properties, and one of the properties of matter is that bodies in different states modify each other'), and then all difficulties as to whether 'matter' is 'dead' or 'alive' will at once vanish. It is quite immaterial whether we can conceive why this should be so, or not: the fact is that *it is so*. At one time it was equally difficult to conceive the earth floating in space; but crude conceptions had to yield to the logic of facts.

Of two separate existences, or principles, we have no evidence whatever. What our senses encounter is resistance—different degrees of resistance. As to the kind of resistance, that is again purely a matter of conception, since it depends on the kind of sensation through which we become conscious of phenomena. Of this we shall speak more fully in a subsequent part, where it will be shown that the so-called 'transformations of energy,' or what is also called 'the interconvertibility of forces,' are more a physiological, or psychological, than a physical problem.

The aim of all science, and more particularly of physical science, is to explain phenomena. But mere physical methods can never yield an explanation; they only supply the data for this purpose. Measurements of temperature can give no possible explanation of the causes of 'heat,' any more than measurements of relative weights of bodies, or relative motions, can throw any light on the causes of gravitation. To gain a knowledge of causes we must have recourse to ratiocination; and in this process psychology plays an all-important part. Our only possible hope of explaining causes—even proximate causes—lies in true inductions. To this end we must generalize our *facts*, and abandon all theories however deep-rooted and cherished they may be. It is from such generalizations of facts that we have reached the inductions of universal persistence or variable resistance, of the tendency to equalization and of the law of reciprocity. In neither of these generalizations have we allowed any theories, conceptions, or preconceived notions to have any share. They simply express generalizations of facts, verifiable at will. It may be doubted whether the language in which we have given expression to these generalizations is the best possible; but we may at least claim that we have not allowed any prejudices to creep into these statements, nor have they been so expressed as to lead to any predetermined conclusions. As has already been seen, these generalizations explain in a simple manner, and without the assumption of any hypothetical entity, all those facts and phenomena which are generally invoked in support of the hypothesis of 'energy,' and without which hypothesis these facts are generally supposed to be inexplicable.

In vulgar parlance we may still speak of 'matter' and 'force,' or 'matter' and 'energy,' and that because in any case speech cannot be changed so easily. In this we are limited by the necessities which govern our means of communication with each other. We have inherited our language, and with it all the errors of our ancestors. But, while we cannot change language at will, we may alter our conceptions so as to bring them into conformity with current knowledge. We have knowledge of persistence, of resistance, of tendency to equalization, and reciprocal modification. In addition to this we can study the modifications which bodies undergo by their reciprocal reactions under varying conditions, to which investigation may be given the name of Phenomenology.

CHAPTER VIII

THE CONSERVATION OF ENERGY

In matters of evidence, as in all other human things, we neither require, nor can attain, the absolute. We must hold even our strongest convictions with an opening left in our minds for the reception of facts which contradict them; and only when we have taken this precaution have we earned the right to act upon our convictions with complete confidence when no such contradiction appears.—J. S. MILL.

AFTER having denied—if not actually disproved—the existence of 'energy' as a separate entity, it may seem a waste of space and time to discuss any of its subsidiary doctrines. It is very much like a certain historian who, after having stated that there were no snakes in Ireland, is said to have assured his readers that those that were there were not poisonous. The doctrine of the 'conservation of energy,' however, demands our attention on account of the facts which underlie it, and which are generally appealed to as conclusive evidence of the theory itself. Now, as we are not denying the facts, but merely the theory which is supposed to account for them, and which theory (by the convenient reversal of the argument) is supposed to be established by the very facts to account for which it has been framed, it seems to us expedient to show that the facts can be well explained (and even better explained) without such theory, and that the theory itself is not borne out by the facts, as is generally supposed; that, in truth, the theory has not been deduced from any facts at all, but followed as a mental necessity from prior assumptions. We shall show, first, that the facts relied on to prove the doctrine of the 'conservation of energy' lend no support to the hypothetical entity of 'energy'; secondly, that the facts can be explained without recourse to any such hypothesis; thirdly, that the existence of such an entity is but an illogical inference from the facts which have led to the doctrine of conservation, and not an inevitable conclusion;

and, fourthly, that in so far as the doctrine affirms a fixed quantity of 'energy' in the universe (using for the nonce this term in the sense of 'power of doing work,' independently of the manner in which this power is conceived) we have no evidence whatever to support such an idea.

What is it really that is designated by the 'conservation of energy'? Using current phraseology, whenever 'energy' disappears in one form it reappears in some other form. Transcribed into our new terminology, we should express the same thing by saying that any change in one body is accompanied by a corresponding but opposite change in another body. But when any one body changes it will thereby approach nearer to equilibrium relatively to one body, and at the same time be removed further from equilibrium relatively to some other body. And, as we have shown that the power of reaction between any two bodies is proportionate to the difference of states, it is obvious that whatever a body may lose in quality relatively to one body it will gain relatively to some other body or bodies. And this is all there is of fact to support this doctrine of the 'conservation of energy.' For, needless to repeat, that which is called 'energy' consists in the reciprocal modifying powers of bodies, which have been attributed to some mysterious agency, or entity, in conformity with dualistic and anthropomorphic conceptions.

The *power of doing work* (using the phrase in its widest possible sense, implying modifiability) does not reside *in* any body or *outside* any body, but depends on the *relativity* of two bodies, and is always, in the words of Newton, 'mutual, and directed to contrary parts.' It is this law of reciprocity and of equalization which has been staring physicists everywhere in the face, and which, interpreted in the light of the dualistic conception, has been twisted and distorted into supposed proofs of the existence of a hypothetical entity. If by 'energy' be simply meant force, power, intensity, extent of reciprocal modifiability, &c., the term may still be retained as a convenient phrase. In that sense we might also speak of the 'conservation of energy,' implying by the phrase that any change is at the same time a condition for subsequent changes; that each change is the *effect* of prior and the *cause* of subsequent changes; and that between these changes, or between causes and effects, there are certain fixed relations [1].

In this sense and to this extent can we accept the doctrine of the conservation of energy as a convenient, and perhaps also as a useful, phrase. But further than this we cannot go. We cannot accept such a conclusion, inference, deduction, theory, doctrine, or whatever else we may please to call it, as an induction wherefrom to make unlimited deductions.

According to the view unfolded in these pages, the source of 'the power of doing work' (or briefly, of 'energy,' and in this sense we shall henceforth use the term) depends on the difference of states which exists between the different bodies in the universe. As the *cause* of this 'power of doing work,' owing to such differences, we assign *the tendency of nature towards equilibrium*. As to the why and wherefore of this tendency we offer no opinion, but simply accept the fact as we find it. To it can be traced all phenomena; and without it—that is, if we tried to deny this tendency—all basis of physical science would be thrown to the winds. But for the acceptance (whether conscious or unconscious) of this tendency to equilibration, we should have no reason to suppose that two quantities of matter, which when placed in the pans of

[1] 'Energy' in the sense of 'available power' is, according to our view, due to a disturbed equilibrium, and in that sense may be said to be an effect rather than a cause, since the power is not available until an equilibrium is disturbed. The dynamical changes, however, which take place in a third (incidentally obstructing) body owing to the subsequent equalization of the bodies whose equilibrium has been disturbed may be regarded as an effect thereof. So that the 'available source of power' may be said to be the *effect* of a disturbed equilibrium, and the *cause* of dynamic changes. But in speaking of this 'available source of power' (not of 'energy,' however, in the sense of a distinct entity) a distinction is to be made between primary (or kinematic) and secondary (or dynamic) reactions. Every disturbed equilibrium may be utilized as a source of a power, which would, therefore, be the *result* of the disturbed equilibrium, and not its cause. In its turn, however, the equalization may be a *cause* of other disturbances, and thereby create new sources of *available power*. In short, as previously remarked, every equalization results in the simultaneous disturbance of other equilibria, and in this sense each change may be said to be the *effect* of prior and *cause* of subsequent changes. When we say, therefore, that 'energy' is in every case an effect rather than a cause we have in our mind the supposed entity called 'energy,' which is merely a theoretical deduction from the phenomena, not to be confounded with the idea of available source of power. Water does not fall from a higher to a lower level because it is pushed by 'energy'; but is capable of performing work because it *is* falling. Nor does its velocity depend 'on the quantity of *energy* by which it is moved,' but rather the *available power* will depend on its velocity. In short, the 'force,' 'energy,' or 'power of doing work' is in each case on this side of the phenomenon, so to speak, and not behind it.

a balance equipoise each other, were of equal weights; and such terms as equivalents would be devoid of all meaning. Any two bodies not in equilibrium with each other tend to equalize; and any third body that happens to obstruct the free process of this equalization is itself acted on in consequence, and modified accordingly, as before explained. Under this view the 'power of doing work,' or the 'energy,' of the universe would depend on the degree of difference (or disparity) there existed between the several bodies filling space.

Now it is quite true that with each approach to relative equilibrium of any two or more bodies there is a corresponding disturbance of already existing relative equilibria. But whether, in the myriads of changes (the regular sequence of events) which are taking place in every second of time, the one set of reactions exactly balances the other; or whether the totality of differences, and hence the total 'energy,' or 'power of doing work,' in the universe, remains the same, we have no means to ascertain, or even to conjecture.

In one respect, however, there is a material and essential difference between the conclusions to which the view here set forth would lead as compared with those based on the existence of a hypothetical 'energy.' The latter view postulates a definite and fixed quantity of that *something* called 'energy,' which, therefore, can never be destroyed (though it may be 'dissipated'). Under the former view 'energy,' in the sense of the 'power of doing work,' may be conceived of alike as being destroyed, created, or regenerated—according as the matter in the universe is conceived of as in universal equilibrium or as having its equilibrium disturbed. When an equilibrium is disturbed, *energy*, in the sense of an *available* source of power, is created; when an equilibrium is restored, 'energy' is destroyed, consumed, lost, or dissipated. If in restoring such disturbed equilibrium another equilibrium is disturbed in consequence, we have a transmission or transformation of 'energy,' as the case may be—and then we shall have also 'conservation of energy,' or, in the phrase of the late Professor Huxley, a 'persistence of force.' While, therefore, the theory of 'energy,' as currently taught, postulates *a fixed and unalterable quantity* of this entity, the view here set forth admits neither of an affirmative nor of a negative as to the invariability of the source of power in the universe.

Giving free rein to our imagination for a moment, we need only conceive of all matter in the universe in reciprocal agreement. Then, whatever may be the thermal or other states of the universe, there could be no further 'energy.' Each change being due to the interaction of bodies in seeking equalization, there could be no change where all bodies are already in equilibrium. All matter being of one and the same temperature, no one body could change the thermal state of another; and, since we should have to include in this 'All' the particles of matter filling space, there could be no loss of heat by radiation. And, since the atmosphere—which is only another form of matter—would be of the same temperature as the rest of the universe, there could be neither cooling nor heating. In such a universe as here imagined, before changes could at all take place some portions would have to become hotter or colder, or in some other way have their relative resistance increased or diminished, so as to be able to cause a change, or readjustment, in other bodies. Let us imagine in one part of such equipoised universe such a change to take place; then the equilibrium of all matter in the universe, one to each and each to all, would be disturbed. In the one case, therefore, logic leads us to the conclusion of the possible annihilation of all 'energy'; and in the other to the deduction that a slight disturbance may be the cause of the creation of 'energy' to an infinite degree. (It is to be noted that in the foregoing we are continually using the term 'energy' in the sense of *available source of power*, which results whenever the equilibrium of any two bodies is disturbed.)

It is in this point where our conclusions differ from the current doctrine of the 'conservation of energy.' It is not a conflict of facts which is involved, but a *difference of conception*. It is only when deductions are made from the two conceptions, respectively, that vastly different conclusions are reached; and the source of this discrepancy lies solely in the form in which the facts are generalized.

It is currently believed that it is a something called 'energy' which moves or otherwise changes bodies. From the point of view of our theory, however, it is not 'energy' which is the cause of motion or other changes, but it is these changes which are the sources of energy in the sense of *available power*, and not as a *something*. Energy itself, as commonly understood, is but a fiction of the imagination, invented to stand in place of an unknown cause. Not knowing to what motion is due, and attributing it to some unknown power in analogy to human exertions, 'energy' has been assumed as being a cause thereof. And, seeing also that each change begets new changes, it is easy to see how this doctrine of the conservation of energy has gained ground in the human mind. But under the point of view set forth in the previous chapters no such assumption of a hypothetical entity is necessary; indeed there is no longer any room for it. The doctrine of persistence and of unequal resistance in various bodies is all-sufficient to explain all the phenomena bearing on this doctrine. The 'persistence of force[1]' is simply due to the fact that a disturbance in one body causes an *equivalent* disturbance in another body, and so on in endless succession; but this is quite different from the assumption of a fixed quantity of 'energy.'

Energy being due, according to our showing, to a want of adjustment, unless it could be shown that this amount of disturbance of equilibrium throughout the universe is always the same, the conclusion of a fixed quantity of 'energy,' or source of energy, cannot be sustained. Each disturbance of an equilibrium is followed by an endless series of other disturbances. When one body is disturbed out of its adjustments, the reaction is not complete when that particular body has again come to rest; nor have we any grounds to suppose that the totality of changes caused by such a disturbance will be exactly equal to the power required to produce it in the first instance.

Such a view seems to us incompatible with the general conception of the law of causation, according to which each effect becomes in turn a cause of new effects; and in this

[1] Professor Huxley's equivalent phrase for 'conservation of energy.' By this phrase Professor Huxley—as he himself explained—simply meant to imply that 'force' cannot be destroyed, but is merely changed into some other form. This is certainly a far more philosophic statement than the doctrine of the 'conservation of energy,' inasmuch as it involves no assumption of a separate entity, but merely expresses a fact of common observation.

light each disturbance would have to be regarded as the source of an endless sequence of events. In our artificial experiments, or limited observations, we take notice of one or two bodies only, and hence we see finality of any reaction. We admit steam into an engine, and see the latter move for a certain time, until equilibrium is again established. But we only take notice of engine and boiler, and do not follow, even in imagination, the other effects which the burning of the fuel, the thermal and mechanical disturbances of the air, &c., have produced; still less do we contemplate that endless series of effects which must spring from the same source. Indeed, if we allow ourselves to speculate in this direction, we cannot conceive of any act or disturbance to be lost in this universe. Nor is it quite correct to speak of the sequence of events as a '*chain* of causation,' according to which the sequence of events (cause and effect) is conceived as a series of alternate links. Nature is not quite so simple as this. Each effect is due to more than one solitary cause, and certainly produces more than one solitary effect. Each effect is due to a concatenation of causes, and becomes in its turn the cause of a multiplicity of effects. We believe it was Sir (then Mr.) W. R. Grove who said, and said very truly, that if by raising one leg we shift the centre of gravity of our body we at the same time shift the centre of gravity, to a proportionate degree, of the earth, and in so doing disturb the relations of all bodies to the earth. By again restoring the centre of gravity of our own body, we do not thereby restore the former state, since in the meantime myriads of other changes must have taken place in consequence of our action. Highly imaginative as this may seem, we can actually demonstrate the truth of this statement by a few magnets.

Let us take a series of magnets, as in a former experiment, and suspend them just within each other's field of influence. By moving one of these magnets out of position, all the other magnets will in succession begin to oscillate. By restoring the first magnet to its former position, we shall not, however, have restored the other magnets to their prior states, as these will continue to oscillate. Now we know that magnets have a fixed relation relatively to our globe, known as 'polarity.' We also know that this polarity of our earth changes, and that magnets assume different positions at various times in consequence of such changes. The earth, then, may be conceived of as a large magnet which continually tends to place itself in adjustment with our small magnets, just as the latter tend to adjust themselves to the varying changes of terrestrial magnetism, though in inverse proportion. Having, therefore, created a disturbance in a series of magnets by temporarily removing one of them out of position, we have changed the relations of the remaining magnets, each to all and all to each; and, having changed their positions relatively to the magnetic meridian of the earth, we must also have changed, *proportionately*, the direction of the magnetic meridian of the earth; and, by inference, the relations of every other magnet on the globe relatively to the latter.

But now follows an important consideration bearing on the subject of the 'conservation of energy.' The reason why the disturbed magnets did not resume their former position at the moment when the first magnet had been restored is that *they persevered in their acquired states.* For instance, if we take two magnets and place them in position as here shown (Fig. 3), A being supposed to be fixed and B free to

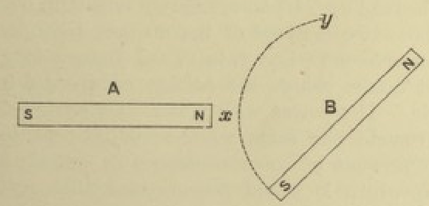

FIG. 3.

rotate in a horizontal plane, then B and A will attract each other in direction of the dotted line. A being fixed, B will, therefore, move with its S pole towards x. It will be noted that its motion is accelerative, and that it will have acquired its maximum speed when just opposite the N pole of the magnet A. But, though this was the point towards which it has been attracted, it will not come to rest there, but will continue beyond it with *a velocity equal to that it had acquired at that point.*

The magnet, be it borne in mind, was attracted only as far as x, but moved beyond it *in virtue of its persistence*. In the first part of its journey it had been *impelled*; in the second part it moved *against* an impelling force, merely because of its tendency to continue in its acquired state. It is also to be noted that the intensity with which such a magnet would pass beyond the point of attraction would depend entirely and solely on the velocity which it had acquired when just opposite the point of attraction.

Here it seems to us we have a distinctly greater effect than the extent of the original disturbance; and presently we shall show that, through such tendency to continue, there is an excess of power, which, under certain conditions, may be utilized for the performance of work. We will leave out of consideration the question as to how many other magnets might have been disturbed in consequence of it, but merely note the fact that the attraction between A and B resulted in a motion of the latter beyond the point towards which it had been attracted. For the magnet B was attracted towards the point x, and up to that point only; beyond that it moved in virtue of its acquired persistence. There was no impelling force that pushed it beyond that point; quite the contrary: it moved beyond *against* an oppositely impelling power.

Now these two motions of the magnet, from its original position, as shown in Fig. 3, to x, and from x to y, are due to distinct causes, which are neither comparable nor commensurable. The power with which it is attracted towards x would remain the same whether adjustment took place slowly on account of some resistance or quickly; but the force with which it would pass beyond that point would depend on the velocity it had acquired when arriving at x. Thus, if the magnet be allowed to move towards x in slow successive stages, its velocity may be so slight as not to be able to move much beyond the point of attraction.

It may be objected that in arresting the motion of the magnet at different points some of its 'energy' that was necessary to carry it to y had been spent. We substitute, therefore, for this experiment another one. We dip a glass tube open at both ends into water, and it will be noted that the level inside the tube is equal to that outside it, the water having risen in the tube as the latter has been pushed down. We then repeat the experiment, this time closing the upper end of the tube with our thumb prior to immersing it, so that the imprisoned air shall prevent the water from entering the tube. The surface of the water immediately below the tube will now be at a different level, so that, when we withdraw our thumb, the water will rush into the tube, and consequently will arrive at the level of the surrounding water with a certain velocity. If our reasoning be true, then the water in the tube should rise beyond this level; and this is actually the case. With a tube, three-eighths of an inch in bore, which has been immersed in this manner to a depth of 5 inches, the water rose 2½ inches beyond the outer level; immersed to a depth of 10 inches, it rose 4½ inches; and with an immersion of 15 inches it rose 5½ inches. To obtain these results the thumb must be withdrawn suddenly; for, if the air be allowed to escape but slowly and gradually, the motion of the water will be correspondingly slow, and consequently it will not rise much beyond the level in the outer vessel.

No doubt there is a relation between the impelling force and the subsequently manifested force due to persistence; nevertheless the two are distinct from each other. In our experiments with the magnets the attraction was between *A* and *B*, while the resultant motion of *B* was very nearly double the distance. When arrived opposite *x* (the point of attraction) it could still overcome resistance—*a resistance equal in power to the force which would have been required to send the magnet from* x *to* y.

From all of which we conclude that there is a distinct gain of power due to this persistence. The experiment just described, which shows that water may rise in a tube above the level of the column by which it is pressed upward, is to our mind a conclusive proof of our contention, since this extra height to which the water rises might be utilized in its return fall to create a new disturbance. And this is actually being done in that ingenious contrivance known as the hydraulic ram, the action of which depends on the same principle as the experiment above described [1].

[1] The hydraulic ram may be conceived as a U-tube in which the delivery column is higher than the inflow column. The impact of the descending column compresses the air in the interposed air-chamber, and, the back pressure being prevented by an interposed valve, the column of water is thereby raised. Of course, not all of the water flowing down the descending tube is thus raised, as some of it escapes at a lower level; but this escape

Yet another conclusion follows from these considerations. According to current explanations, when a body is thrown upwards and has reached its maximum ascent, it is said that at that point the 'energy' which has been expended in throwing it upwards has been converted into 'potential energy,' which will again manifest itself in its descent as 'dynamic energy.' 'And calculation applied to this case,' says Professor Tait[1], 'shows that at every stage, whether of the ascent or of the descent, the sum of the potential and the kinetic energies remains precisely the same, except in so far as it is modified by the resistance of the air.' That this, however, is not true can be proved by any ordinary balance. For let the beam of the balance be pulled down on one side by a certain weight, and let an equal weight be placed ever so gently in the opposite pan, then the beam will not merely assume its horizontal position, but descend now on the other side as if the added weight were heavier than the original one: our own explanation of which is that the downward and upward moving pans both tend to continue in their acquired motions, the one in an upward and the other in a downward direction. This oscillating of the balance is clearly due to this tendency to continue in the direction of the acquired motion, and is as true of the ascending body as of the descending one: consequently the one must for the time being be 'lighter' and the other 'heavier' relatively to each other; in other words, it cannot be true that 'at every stage, whether of the ascent or of the descent, the sum of the potential and the kinetic energies remains precisely the same.' For, while the bodies are moving, another factor, besides gravitation, is at play, viz. *persistence*. This is proved by the fact that if the balance be equally loaded in both pans while the latter are steadied, and be then raised, such oscillation will not take place. On the other hand, the higher one end of the beam and the pan attached thereto before the counterpoising weight be added: in other words, the greater the distance through which this weight has to fall to equilibrium, the greater will be the extent of these oscillations; and, as the descending pan tends to pass below the point

is a necessity of the mechanical contrivance only, so as to make its action continuous.

[1] *Recent Advances in Physical Science*, p. 19.

of equilibrium, so does the ascending pan tend to continue to ascend above it. If further evidence in support of our contention against the truth of Professor Tait's dictum be required, we would refer the reader to the extensive and carefully executed ordnance experiments, none of which have hitherto conformed to theoretical calculations[1]; the reasons for which will be apparent from what has just been said.

From the point of view here unfolded, 'energy,' in the sense of source of power, may be conceived of both as being created and destroyed, and the 'energy' or power required to do either the one or the other will in all cases be *equivalent*, but not necessarily *equal*, to the result.

One objection will at once occur to the reader's mind, and that is the old but effective *reductio ad absurdum*, that, if this be true, perpetual motion should be possible. Our reply to this is, that we do have perpetual motion in the universe; that when we have pushed a body, or in some other way changed its state, the resultant effects of our action do not stop there, but continue to produce endless other changes beyond our power of utilizing them, and beyond our range of observation; that, in short, there will flow from any such action, however slight, an endless sequence of events, changing (*to a proportionate degree*) the whole course of nature. In short, any such action becomes a factor—infinitesimal though

[1] See Spon's *Dictionary of Engineering*, div. v, article on Gunnery. The extensive and carefully executed ordnance experiments, none of which conform to the requirements of the theory contained in Professor Tait's dictum, fail to lend any support whatever to the theory that 'at every stage, whether of the ascent or of the descent, the sum of the potential and the kinetic energies remains precisely the same.' The fact is that they cannot be the same, for the conditions to which is due the ascent of a body are essentially distinct from those which cause its descent. In sending a projectile upwards the force applied is dynamic—or an 'impressed force,' as Newton called it—which is at its maximum at the moment when it leaves the hand which throws it upwards, and which diminishes, not merely in consequence of the resistance of the air, but also in consequence of the backward pull of the earth. A projectile thrown upwards would be 'resisted,' therefore, even in a vacuum. But in its descent the conditions are entirely different. It is no longer a dynamic or impressed force by which it is actuated, but by kinematic causes—in the previously defined sense of the term kinematic—and, the impelling force being active, the velocity will be accelerative. Could we conceive a tunnel passing through the earth, so that the falling body might proceed unopposed as far as the point of attraction, it would at that point have acquired its maximum velocity. The distinction between kinematic and dynamic or primary and secondary forces is essential; they have absolutely nothing in common, and do not admit of comparison. Of course from the point of view of the theory of 'energy' it is different. But then this is a mere hypothesis against which we have been able to marshal many grave objections, and without any discoverable evidence in support of it.

it may be—in the subsequent play of the universe. But the impossibility of the MECHANICAL *perpetuum mobile* could hardly be better demonstrated than by deductions from the very generalizations from which this present theory is deduced. For, inasmuch as 'energy'—i.e. the power of doing work —is but a result of bodies in process of equalization, it follows that an *available* source of power will diminish in proportion as this equalization proceeds. Hence to have a perpetual source of power we should as perpetually have to cause a *local* disturbance.

But, though the equalization of the particular bodies which serve us as a source of power be complete, we cannot say that it has produced no other effects; that, after we have burned a pound of coal in a boiler and have caused a steam engine to move, this is the sum total of all the effects. It is certainly far more reasonable to suppose that, whatever cause of disturbance may be thought of, it will be followed by an endless succession of effects; each change being followed by other changes *ad infinitum*.

If the doctrine of the conservation of energy as currently taught were true, then we should be able to nullify certain acts; that is, after we have created a disturbance and restored local equilibrium, such action should have no further effect than if it had never taken place at all, a conclusion which is to our mind as inconceivable as it is incompatible with the now universally accepted axiom of universal causation. And, though the latter is but an axiom, it is based on such wide experience that we cannot but believe that each effect will in its turn become a source of subsequent effects without an end.

Our contention is, in short, that there can be no cause without an adequate effect, though such effect might not always be manifest to our senses. To which must be added that each effect would in its turn become a condition of subsequent effects. Or, to express the same thing more emphatically, there can be no change without being the cause of subsequent changes, since both cause and effect are but changes consequent on prior changes.

When we throw a lighted match into a barrel of gunpowder and blast a rock, bringing down an avalanche of stone and earth, burying a whole village, and diverting the course of a river into new channels and into new regions, causing destruction, death, and migration of men and animals in distant parts, with all other attendant changes that may follow from such action: it is easy to *assume* that all this 'energy' was 'locked up' in the gunpowder as 'potential energy,' and that the muscular effort necessary to rub the match was merely the unlocking of this imprisoned 'energy.' But where is the *evidence* that the effect has just equalled the cause as far as 'energy' is concerned? It is different from the point of view of disturbing an equilibrium; for a slight disturbance in one part of a system of bodies would or might be followed by a disturbance of equilibrium of the whole, and be further followed by an endless succession of other disturbances outside that particular system of bodies. But in no case have we the means of proving that the amount of 'energy' liberated throughout the universe by any such disturbance would be just equal to the amount of 'energy' required in the first instance to produce these changes.

To sum up: our theory agrees with all the facts on which the doctrine of the conservation of energy is based, but leads to different conclusions. The facts, we need hardly insist, give greater support to this new view than to those notions or theories from which this doctrine of the conservation of energy has been deduced. The chief argument by which this latter doctrine is supported is that of the mechanical equivalent of heat. But (quite apart from the error underlying this generalization, previously pointed out) there is no instance in which it could not be demonstrated that the mechanical equivalent is proportional to *difference* of temperature, and not to 'heat' absolutely. And, if we compare the relative conclusions which are reached respectively from the two points of view, we believe the reader will agree with us that the conclusions here arrived at are more in harmony with universal experience than the doctrine of a separate entity of 'energy' in a fixed quantity.

All we affirm positively is, that what is called 'energy' is due to a difference of states, and hence can be created locally, for human purposes, by disturbing an existing equilibrium[1]. The 'energy,' or source of power, thus created

[1] It may here be objected that to disturb an equilibrium postulates 'energy,' an objection which has been made to us by some friends. It involves

would be kinematic (or primary), in the sense heretofore explained, which, by interposing a third body, could be turned into an available source of power, whereby the kinematic (primary) power would thus be converted into dynamic (secondary). We can also affirm that these local changes will be followed by other changes beyond our range of observation; and in so doing the bodies affected will be brought *nearer to* relative equilibrium with some bodies, and *further from* it relatively to other bodies. Whether these two kinds of changes produced throughout the universe would exactly balance each other, we have no means of ascertaining.

While, therefore, arguing that the doctrine of a fixed quantity of 'energy' in the universe is 'not proven,' we are not prepared to meet such a universal affirmative by a universal negative. We have ourselves pointed out the dangers of such a course; hence are not prepared on any consideration to enter the realms of the absolute.

effort, *work*, or *power*, certainly, but not a hypothetical entity. Our contention has been that each change is followed by other changes, inasmuch as an adjustment of a body *A* with another body *B* results of necessity in disturbing the equilibrium of both *A* and *B* with other bodies. All we mean by 'energy can be created *locally*' (note the qualification !) is that, if we desire to have work performed by *A* and *B*, we must disturb their relative equilibrium. The expenditure of power will not merely be equal, but under conditions may be greater (theoretically) than that to be gained. We do not mean that the hypothetical something called 'energy' can be *created*. Our contention is that it does not exist, and that all mechanical power is due to the equalization of a disturbed equilibrium. Such disturbance we can cause *locally*, and create a source of *available power*. On the other hand, we may '*expend energy*' without having created an *available* source of power, if it does not result in disturbing an equilibrium.

BOOK III

PHENOMENOLOGY, OR 'THE INTERCONVERTIBILITY OF FORCES'

CHAPTER I

INTRODUCTORY

I have long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their action.—FARADAY.

IF our contention against the hypothesis of a separate entity of 'energy' be valid (as we believe it to be), the necessity for an altogether different conception of phenomenology becomes manifest. The facts, of course, remain the same, no matter by what theory they are explained; but, inasmuch as reasoning is always based on mental concepts rather than on facts, it is necessary to modify the former in accordance with more recent information, and that because the mind is only too liable to be misled by false theories, or wrong terms to which are attached meanings no longer tenable.

We can no longer refer to changes which result in different manifestations—as, for instance, where heat results from motion, or electricity from heat—as 'transformations of energy,' or an 'interconvertibility of forces'; because the idea of 'force' or 'energy' (save in the sense of power, strength, or intensity) has to be abandoned as a no longer tenable hypothesis. All phenomena are henceforth to be conceived of as being due to an interaction and reciprocal modification of bodies. Looked at in this light, it is no longer a 'transformation of energy' or a 'conversion of force,' but merely a change of manifestation; and what has been called the 'interconvertibility of forces' resolves itself into a change of manifestations[1].

Under this view the problem becomes a comparatively simple one, for thus it is no longer a change of 'force,' or a transformation of 'energy,' but *a change of sensations*. Instead of defining imaginary 'forces,' we shall be able to study the phenomena themselves, and trace the diversity of manifestations to the difference in the quality of bodies or a difference in conditions. When wax, ether, gunpowder, and iron, for instance, are placed on a hot stove, we shall behold a variety of phenomena caused by the same agency. Under this new view, the explanation of this diversity of manifestations would no longer have to be sought for in the kind of transformations of supposed forces, but in the changes which different substances undergo when exposed to like conditions, on account of their different qualities. Thus viewed, it is evident that the source of the idea of different 'forces' is traceable to the different modes in which we ourselves become cognizant of the different manifestations. In short, the study of the various 'forces of nature' would resolve itself into a study of phenomenology.

But phenomenology, again, has reference to the *subject* rather than to the *object*: it depends on the sensations we receive; the particular groups of sense-organs through which these sensations are conveyed; and the intermediary agency through which we are informed that certain bodies have undergone certain changes. Thus, for instance, the rise of the mercurial column of a thermometer informs us that a thermal change has taken place in a certain body; the electroscope informs us of electrical or magnetic changes, of which, without such intermediary instrument, our senses might not become cognizant.

That it is not a question of different 'forces,' but of different sensations and conceptions, can be shown in various ways. We will take a tuning-fork, for instance, and cause it to

[1] Or, perhaps, still better, a change of sensations.

vibrate. Of this fact we might become aware in many ways. Our auricular nerve might become excited, then it would be *sound;* we may see the vibrations, in which case it would be *motion;* or we might connect the vibrating fork with a very sensitive electroscope, in which case we should have *electricity*. There can be no question that the temperature of the vibrating fork is slightly increased, which possibly might be sufficient to deflect the needle connected with a thermopile, in which case we should have *heat*.

Now, manifestly, all these different sensations are traceable to one and the same exciting cause; i. e. to the stroke which has caused the vibrations of the tuning-fork. If we except the solitary sensation of vision, all the other modes through which we have become conscious of the vibration of the fork may be said to have come to us indirectly through different agencies. Thus the sensation called sound was caused by the vibration imparted to the air by the vibrating fork, which, in its turn, has acted on the drum of our ear, and has set up a disturbance (sensation) in the auricular nerve. As regards electricity and heat the mediateness through the various instruments is apparent. There are a host of other instances in which certain changes in bodies are inferred from the effects they produce on third bodies; as when we test the acidity or basicity of a body by noting the effects it produces on litmus or other indicators, or when the chemist tests for certain substances by means of reagents, and so forth. In many such cases it is beyond our senses to recognize the presence of certain bodies or the state they are in. We then bring a third body into requisition, and note the effects produced in that third body, in order to infer from it either the nature of certain substances or the states they are in.

Now, in many cases it depends entirely on the nature of the third body, or intermediary agency, through which we become informed of the particular state of a body, as to whether one 'kind of force' or another is inferred. In the illustration given, supposing we could become conscious of the vibrations of a tuning-fork through an electroscope only, it would unquestionably be ascribed to *electricity*. Or suppose a thermometer or a thermopile were the only means by which we could become informed of the activity of the vibrating fork, then the phenomenon would be ascribed to *heat.* In short, the 'nature of the force' would depend in each case either on the particular sensation or the particular instrument through which we become cognizant of the disturbance, and in each case on the particular associations that are called up in the mind.

Heat, light, sound, electricity, magnetism, and all other kinds of manifestations can be due only to disturbances of bodies, i.e. to the endless modifications to which all bodies are subject; and the different manifestations of such disturbances must be traced, not to different 'forces,' but—

Firstly, to the different modes in which various bodies may be affected by the same agency, on account of the varying conditions of the several bodies.

Secondly, to the different sensations or agencies through which we become cognizant of such changes.

We take, for instance, a copper and a zinc rod, immerse them in dilute acid, connect them with silver wires, insert in one place a piece of platinum wire, in another twist the silver or copper wire into a helix and insert into it a bar of iron. In the acid a disturbance will be set up, by which the equilibrium of the whole series of connected bodies will be disturbed. The piece of iron will now be able to attract small particles of iron, and thus indicate *magnetism.* The platinum wire will become hot to the touch, showing the presence of *heat.* If this heat be sufficiently great to affect the retina, or to decompose a silver salt, we shall have *light.* And by deflecting a magnetic needle inserted in the circuit we shall have evidence of *electricity.* In the cell some of the zinc will be found to have combined with the acid, which again indicates *chemical 'force'* or '*energy.*' Thus the disturbance created by the reciprocal action of an acid and two metals may cause a disturbance in other bodies within reach, but manifest itself differently according to the different natures of those bodies and the conditions under which such disturbance has been communicated to them.

It is in this direction that a solution of that problem currently known as the transformation or interconvertibility of so-called 'forces' has to be looked for. The problem is, Why does the same disturbance originating from the chemical interaction of two metals and an acid cause an increase of temperature in platinum, a deflection of the magnet, and confer on a bar of iron the power of attracting iron, while leaving silver or copper seemingly unaffected?

For according to our reasoning all these different manifestations can be due only to an interaction of the various bodies, in accordance with the tendency to equalization as heretofore explained, and not to any supposed 'immaterial matter' or 'subtle imponderability.' Under the view set forth in these pages, all these manifestations can be conceived of as different states of such bodies; and of such states we know of three only, viz. the states of relative equilibrium, relative excitation, and coercion.

If our reasoning has been correct, we should be able to trace all these various phenomena to one or other of these three states. It is no longer a question of what is heat, or what is the nature of heat, but, to what is the increase of temperature of bodies due? And so likewise, to what causes is that particular state of iron which confers on it the power to attract other pieces of iron due? And so forth.

We shall now endeavour to trace these various phenomena to the variable resistances of the several bodies—since either of the three foregoing states can, according to the results of our previous investigations, be traceable to nothing else.

From this summary review it will be apparent that both method of inquiry and direction of thought must be entirely different from those the reader is familiar with; and but little reflection will show that a new criterion is equally necessary. What we mean by this is that the criterion currently applied to scientific reasoning would not be applicable in the present case, in so far as the theories and explanations of phenomena to be put forth in the succeeding chapters are based on a system of thought entirely differing from that underlying the present scientific fabric. It is not merely a question of giving new explanations of certain specific phenomena, but of establishing an entirely new groundwork for scientific philosophy; the particular instances being merely selected as illustrations of how this new system can be applied to the explanation of already familiar phenomena.

If, therefore, our accounts or explanations of certain thermal, electrical, or magnetic phenomena should differ from current explanations and theories, we submit that they are not to be rejected on that account, nor to be judged in the light of already accepted theories or supposed principles, since we call these in question, and therefore they must be regarded in a sense as being *sub judice*.

We should apologize for repeatedly urging the necessity for such an obvious precaution, but we have the sad experience of history to warn us that fundamentally new theories are always judged in the light of older theories, and (at first at least) accepted or rejected in proportion as the new theory agrees with or differs from accepted notions. If the principles of persistence, resistance, the tendency to equalization, and, above all others, the principle of reciprocity—i. e. that every reaction is mutual, and hence that every modification of a body is caused by and due to a corresponding modification in some other body—be true, then it is obvious that our whole scientific fabric has to be reconstructed. It is not sufficient to accept these principles in the abstract; if they are true, we must accept also the deductions to which they lead, and build our philosophy of phenomena accordingly. For be it remembered that *it is not a question of facts, but of interpretations*. If, therefore, we should assert that the deflection of a magnetic needle inserted in a circuit is due to the interaction of two bodies in different states of excitation, and not to some entity called 'electricity,' the difference is not as to matters of fact, but as to correctness of interpretation. In such case all that we should be required to show is that our explanation *agrees with the facts*; and, should we succeed in this, then, no matter how incompatible such explanation may be with current theories, our contention, we submit, would be proved.

On the other hand, if we should succeed in showing that the supposed two kinds of 'electricities' are identical in kind, differing only in degree; or, to speak more correctly, that there is no 'electricity' (in the sense of an entity) at all, but that the *manifestations* ascribed to two 'electricities' differ from each other in degree only, and not in kind: this should be sufficient to prove the hypothetical character of the current theory.

We may of course commit (as no doubt we shall) many errors as to minor details, and shall be glad to see such errors corrected by other workers. But in our own mind we have no doubt on the two main points: firstly, that the fundamental principles of persistence, resistance, reciprocity, and equalization are as true as they are universal, and are at the bottom of all phenomena whatever; secondly, that the main pillars of the present scientific fabric, i. e. of assumed 'forces' or 'forms of energy,' are illusory, and all theories based on these false and misleading. In no single instance are these fundamentals of the present scientific fabric based on universally true facts; quite the contrary, they are based for the most part on unphilosophic and unwarranted assumptions. It is not true, as is so commonly supposed, that, on cooling, all gases contract uniformly $\frac{1}{273}$ of their volume for each degree Centigrade, as the coefficients of expansion and contraction for different gases vary. But that is not all. The coefficient of the same gas varies at different temperatures. *In fact, there is not a single gas known which contracts or expands uniformly* $\frac{1}{273}$ *of its volume per degree at different temperatures.* The variations are slight, but nevertheless real and appreciable, as is well known to chemists. This irregularity is attributed to the fact that the gases we are acquainted with are not '*perfect* gases' (whatever that may mean), and that the law is true only of a 'perfect gas'! Such a gas, however, has yet to be discovered.

The current theory of 'light' requires a 'perfect elastic'; a body unknown, which therefore had to be supplied by the imagination in the shape of the luminiferous ether.

The current theory of 'heat' is illustrated (*sic!*) by Carnot's 'perfect engine'; an instrument unknown in the world of facts.

To these perfections Sir William Thomson (now Lord Kelvin) has added a 'perfect fluid.' And thus it might be shown how the whole of the present scientific fabric is built up on assumptions and hypotheses.

The fundamental assumption responsible for all this is a prevalent idea of the uniformity of nature: a supposition which at one time seemed obvious, but which the perfection of instruments should long ago have dispelled. There is nothing uniform or regular in nature save the first principles which underlie all phenomena. But identity of principle is not the same thing as uniformity of phenomena. The principles will always be the same, and this may be said also of their mode of action. But the resultant phenomena depend on conditions and collocations; and, as no two sets of conditions can be precisely the same, variability of manifestation is a far more necessary deduction than the supposed uniformity. And this deduction is, in truth, verified by extensive observation. But the habit of reducing everything to rigid mathematical formulae has necessitated the assumption of a rigid uniformity in nature nowhere discoverable outside mathematical treatises.

That there is law and order in nature there can be no doubt. But that merely means, or should mean, that there is no effect without an adequate cause; and that geometrical angularity (if we may use this barbarism) and arithmetical precision, required by the mathematician to prove the truth of his own formulae, are nowhere discoverable.

We do not intend to inveigh against mathematics, or to underestimate the value of that science in almost all branches of human activity; yet we cannot help thinking that, when attempting to accommodate physical phenomena to the rigid and inflexible requirements of arithmetic, mathematicians have overstepped the limits of their usefulness. Mathematics, like logic, is a useful handmaid in unravelling the mysteries of nature. But neither of these *auxiliaries* of science can, of itself, establish a fact in nature. Physical laws deduced in such manner may be true of 'perfect gases,' 'perfect fluids,' 'perfect elastics,' 'perfect solids,' 'perfect engines'—in short, of a 'perfect world,' in accordance with mathematical requirements; but they are not true of the world around us. Whenever, therefore, our conclusions should happen to disagree with certain accepted 'physical laws,' we would request the reader to bear in mind that we are trying to explain nature as she is, and not as she ought or is supposed to be.

CHAPTER II

ACCELERATION AND RETARDATION

SINCE acceleration and retardation play an important part in every reaction, and should be taken note of whenever a phenomenon is being interpreted, it will be convenient to discuss here the causes to which acceleration and retardation are due.

To begin with acceleration: this is directly due to the persistence of bodies. Let motion be imparted to a body, then, if not met by some obstacle, it will continue to move with a uniform velocity. If, therefore, to a moving body a new impulse be given in the same direction in which it is already moving, then such impulse would increase the velocity of that body. Hence it follows that, if a body be impelled by whatever causes in a certain direction, then, while the impelling causes are still active, its velocity will increase, and that even though the impelling agency may be at the same time diminishing.

Say, for instance, that a piston in a steam engine, or a projectile within a cannon, be pushed forward by the expansion of a fluid. Then we have to note two things: firstly, that the expanding fluid continues to push the body in front of it while it is expanding; secondly, that, as the imprisoned elastic fluid is pushing the piston or projectile forward, so its own power diminishes, because in the greater space its compression, or state of coercion, is becoming less and less.

To make this quite clear, let A in Fig. 4 be a cylinder, and P a closely fitting movable piston. When the piston is just at the mouth of A, the chamber C being filled with atmospheric air, the pressure on both sides of the piston will, of course, be equal. By pushing the piston P into the cylinder, the air in the air chamber *C* will be compressed, and in consequence thereof the pressure on that side of the piston will be greater than on the side communicating with the external atmosphere. The further we push this piston into the cylinder, the greater will be the difference in pressure on the two sides, and the greater the resistance to the force by which the piston is pushed inwards.

Here the imprisoned and compressed air acts as a retarding agent, and its power of opposition becomes greater as the compression is being increased.

If the pressure on the piston be now removed, the tendency of the imprisoned air to place itself in equilibrium with the outside atmosphere—or, in common parlance, the tendency of the compressed air to expand—will cause it to press on the piston and push the same forward. Now it is evident that

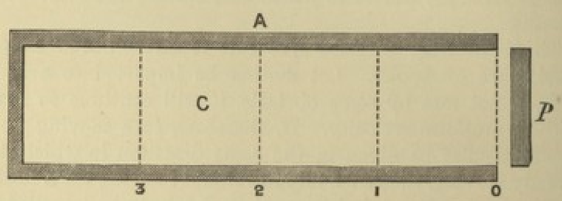

FIG 4.

this push will be all the greater, the greater the compression of the air. It will be greater when the piston has been pushed three-fourths into the cylinder than when pushed only halfway, because in the former case the compression will be greater. So likewise if the piston has been pushed down say as far as 3 in the above illustration, and, on the air expanding, the piston has been pushed back to 2, the pressure in the chamber *C* will have decreased, *yet the velocity of the piston in travelling from* 2 *to* 1 *will be greater than in travelling from* 3 *to* 2, *and from* 1 *to* 0 *greater than from* 2 *to* 1. So that the piston will have acquired its greatest velocity when at 0 (i.e. at the mouth of the cylinder), where the impelling force behind the piston is reduced to *nil*; and instead of coming to rest at the point of equilibrium, where the air pressure on both sides is equal, the piston or projectile will actually be at its maximum power at this point.

The cause of this is to be found in the law of persistence, of which law the phenomenon under consideration is at the same time a splendid illustration. For when the piston is pushed by the expanding air from 3 to 2 it has acquired at this point a certain velocity, and thence would tend to continue with that velocity, even though the impelling force were at this point suddenly withdrawn. But though the volume of the air chamber has been doubled by the piston moving from 3 to 2, and the pressure of the imprisoned air has in consequence been reduced to one half, there is still force left in it to push the piston forward, *which force is superadded to the already acquired velocity of the piston.* In consequence of this the piston will arrive at 1 with a greater velocity than it had at the point 2, while the expansive force of the imprisoned air at this moment will be correspondingly less. But, the air still expanding, though at a much lesser rate, it still exerts a push on the piston; and as the piston has already a certain acquired velocity, in which it tends to persevere, this extra push, however small, will add to the velocity of the piston, and so forth, until equality of pressure on both sides of the piston is established. At this point the impelling force would have reached its *zero*, while the velocity of the piston would be at its *maximum.*

From this follows the *law of acceleration*:—

The result of any impelling force, power, or agency will be intensified while that impelling agency is still active, even though the latter may at the same time be diminishing; the effect being at its maximum at the moment when the impelling cause is just at zero.

The reverse of this reasoning would lead to the causes of retardation. In pushing the piston into the mouth of the cylinder, we compress the air in the chamber *C.* By this compression the resistance of the air is increased until the latter is equal to the power or push we exert on the piston. We may assume the pressure exerted on the piston to be uniform all the way, yet, the resistance increasing at a geometric ratio, *the effect* of the push would decrease in like proportion, until there is equilibrium between the power pushing the piston and the resistance of the compressed air on the other side.

Needless to say that, when the piston, on being pushed forward by the expanding air, has reached the mouth of the cylinder, it will not come to rest there—although at this point the *status quo ante* would have been re-established, inasmuch as the piston would have arrived at the original point where the pressure was equal on either side—but will tend to move forward, and that with a velocity and power it did not possess at any prior point, while there was still pressure behind it.

This law of acceleration and retardation is most familiar in connexion with falling bodies, but has an important bearing not to be overlooked at any time in interpreting phenomena. We have said, for instance, in a former part of this work, that the intensity of every reaction will be proportional to the difference of states of the reacting bodies; that, the greater the difference of states between any two bodies, the stronger will be the tendency to equalization, and the more intense the reaction; and that, as equalization proceeds, so the intensity of the reaction, or the tendency to equalization, will diminish. Now this is absolutely true; yet in actual experiment the effect may often seem to be the contrary. But on analysis we shall find this divergence from the law to be due to a composition of causes. In the above illustration, for instance, we have seen the velocity of the piston to increase the nearer the two pressures of the atmosphere on either side approached to equilibrium. But then it should be remembered that when speaking of the effects of the law of equalization we are referring to the two bodies in actual reaction with each other, and not to the effect that such reaction may incidentally have on a third body. In the case of the piston moved forward by the compressed air in chamber C, the *primary reaction* is between the air imprisoned in the chamber and the outside atmosphere. The piston P had no direct share in this reaction, and its being acted on is merely due to the fact of its being so conditioned as to obstruct the free equalization of these two bodies of air.

It is quite true, therefore, that the reaction between these two volumes of air will be the more intense the greater the difference in their relative states (in this instance states of compression), and that the intensity of the reaction diminishes as equalization proceeds and equilibrium is approached. This expansion of the air is due to *primary* causes (kinematic action, as previously explained); while the motion imparted to the piston is *secondary*, or dynamical. Now the former is diminishing while the latter is increasing, *the effect on P being cumulative on account of the persistence of bodies.*

Let us now apply these principles to falling bodies. We have not yet reached any definite conclusions concerning the causes which make bodies tend towards some point within our globe. But from the general principles adduced we may infer that this is due to some difference of states between the body falling and some other body or bodies within our earth with which these bodies are in reaction, and which in consequence mutually attract each other[1]. Applying the laws above discussed, it may be inferred that in proportion as two bodies are approaching each other the nearer they will be to equilibrium; therefore the reaction, or the force of attraction, should diminish. Actual observation, however, informs us that the velocity of falling bodies is accelerative. But this will now be seen to be no contradiction of the above conclusion, since the increased velocity is due, not to intensified action on the part of the impelling agency, but to the persistence of bodies which makes the effect cumulative.

In interpreting phenomena in order to verify any such conclusion, it must be borne in mind, therefore, that, while the experimenter is watching intently to see the operations of *one law*, several are active at the same time, which often neutralize each other, and sometimes entirely mask the operations of the particular law the observer is intent on verifying. Many apparent contradictions that may occur to the reader while perusing the chapters that are to follow will, on reflection, be found to be due to such composition of causes. And, as it would swell this essay to undue proportions if we were to dwell in every instance on these collateral conditions when illustrating a certain law by some specific phenomenon, we have thought it advisable to discuss such general disturbing elements in the present chapter, but shall not be able, for reasons just assigned, to point out all such disturbing causes in every instance. In illustrating

[1] Further on we shall show that all bodies in different states of excitation, if free to move, have a tendency to fly bodily against each other; and that certain electrical and magnetic phenomena are simply manifestations of bodies acting in accordance with this law.

a law we are bound to notice the effects of that particular law only, and to ignore all other conditions and collateral effects due to an unavoidable concatenation of causes.

Let us now apply this reasoning to the action of a pendulum. It is commonly stated that a pendulum once set in motion would oscillate for ever, in consequence of the motion imparted to it, were it not for the retarding action of atmosphere and friction. Whether a pendulum set swinging *in vacuo* and having no friction would actually keep on swinging for ever, we will not discuss here. That it would keep on swinging for ever if there were no retardation whatever must of course be admitted as a philosophical necessity of the axiom. But the matter of doubt in our mind is as to whether the downward pull of gravitation would not have to be regarded as another retarding element, besides friction and the resistance of the air. It is certain that a body thrown upwards *in vacuo* would be retarded by the downward pull of gravitation. But the extent of this retarding effect is so far yet an undetermined quantity. But, be that as it may, what we wish to point out in connexion with the pendulum is that the explanation of its action is far more complex than that usually given; and in dealing with it here our object is to show the difference between the explanations of phenomena to be deduced from the principles established in these pages and those deduced from the theory of the hypothetical entity of 'energy.'

We shall not impart motion to our pendulum by striking it; we shall merely move it out of the perpendicular, say 30°, and then withdraw our hand. Just at the moment of withdrawal, the bob attached to the string will, in virtue of its persistence, tend to remain there; but, gravity acting on it, it will be pulled downwards towards the ground in a vertical direction. But, being pulled towards the point of suspension by a string, and not being able to move further away from that point, the downward tendency of the bob would result in a motion describing an arc (according to the principles explained by Newton, whereby a rectilinear motion of a body is converted into a curvilinear motion by a centripetal force) until the suspended weight has reached its lowest point. Let it be noted—and with a pendulum about 20 feet long it is distinctly visible—that the motion of the pendulum is accelerative, reaching its maximum velocity when at the lowest point, whence it moves forward, describing the second and upward half of the arc, *in virtue of its acquired velocity.*

Thus analyzing the phenomenon, we find that the downward half and the upward half of the oscillations of a pendulum are due to two distinct causes, *the former, or downward half of the motion of the pendulum, being due to gravitation, and the upward half to persistence, or acquired velocity.*

These two causes are as distinct from each other as they can be, and are not commensurable; and that notwithstanding that the acquired velocity, which causes the pendulum to move upwards, is the direct result of the power of gravitation which caused its descent in the first half.

Let us study the phenomenon further. The pendulum, continuing beyond the lowest point, in virtue of its acquired velocity, will move forwards and upwards, the extent of the second half of the arc described depending (*a*) on the acquired velocity when at the lowest point, (*b*) on the resistance to be overcome. Here the point is to be considered whether gravitation is not an element of resistance, tending to arrest the upward motion of the pendulum. Clearly the upward motion of the pendulum is a motion *against* gravitation; and, if it be contended that this has no retarding effect, then a body thrown upwards in a vacuum by ever so minute a force should fly upwards indefinitely, until arrested by the top of the vacuum chamber.

But, this apart, the velocity of the pendulum, which in the downward half of its journey was accelerative, is now decreasing until it has reached a point where its motion is arrested. Could we conceive it possible at the moment when the body thus comes to rest, when at its highest point, that the power of gravitation were suddenly arrested, annihilated, or suspended, then we are bound to conclude that the body would remain there at rest in virtue of its persistence. But, gravitation still acting on it, it is again drawn towards the lowest point it can reach. The second swinging of the pendulum, therefore, is not, as is commonly represented, a mere continuation of the original motion imparted to it, but is a distinct reaction, though due to identical causes as the first swinging.

Let us summarize this: A pendulum is moved out of the vertical. It is pulled by gravitation towards the lowest point. In consequence of being fastened to a centre, the downward fall of the pendulum is converted into a curvilinear motion. In virtue of the acquired velocity the weight, when arrived at this point, tends to continue its motion in a horizontal direction, which again is converted by the suspending string into a curvilinear and upward motion, until the force due to the acquired velocity is neutralized by the resistance overcome (in which resistance we include that due to gravitation). At this point the double reaction is complete, and the subsequent repetition of the oscillations is due, not to the original motion imparted to the pendulum, but to the new conditions that have thus been created. By the acquired velocity the pendulum was able to overcome, in addition to the resistance caused by air and friction, the downward pull of gravitation, and it has come to rest in a position where it is unsupported while acted on by gravity.

We do not think that anybody is likely to find fault with this analysis, or object to the explanation here given; though it might be urged that nothing is gained by this new explanation, nor anything added thereby to our knowledge of the action of the pendulum. As far as this particular instrument is concerned that may be true; but it is different when we apply such knowledge as inductions for philosophic purposes. For then the conclusions reached severally from the principles of persistence and reciprocity, and from the hypothetical entity of a something called 'energy,' are often diametrically opposed to each other. In the case of the pendulum illustration just given, the explanation is obvious because the phenomena explained are visible and tangible; and because while reading these explanations the reader can mentally behold the actual phenomena described. This, however, is not always possible. When dealing with certain thermal, electrical, or magnetic phenomena, for instance, the causes of which as well as the mode of action are entirely unknown, the reader will have nothing to fall back upon as an immediate and ready criterion save the theories and hypotheses with which his mind had become so saturated as to be often incapable, save by a special effort, to distinguish them from facts. It is for the exercise of this special effort and mental analysis that we appeal to the reader in every instance where our explanations may have a jarring effect or conflict with accepted theories. We would have the reader bear in mind that such jarring effects of a statement may often be due to its being contrary to his mental constitution (the system here expounded being so entirely different from that he is accustomed to) without, on that account, being necessarily incompatible with actual facts.

CHAPTER III

HEAT[1]

Now from this first vintage the form or true definition of heat (i. e. *of heat relative to the universe, not merely to man's senses*) *is briefly this: heat is motion, expansive, restrained, and struggling through the lesser parts* (*of a body*). *Expansion is modified: though expanding in every direction, yet it somewhat tends upwards. And that struggling through the parts is also modified: it may not be at all sluggish, but vigorous and of some impetuosity. But as regards the practical side, the thing is the same. For this is the account of it. If in any natural body you can excite the motion of self-dilation or expansion; and also can so repress that motion, and turn it upon itself, that that dilation proceed unequally, partly taking place, partly being repressed, without doubt you will generate heat.*—BACON.

IN strict logic, it is not the 'nature of heat' we are about to investigate, but the causes to which thermal changes are due. We set out on this inquiry with the conviction that it is merely a manifestation of certain conditions; and, therefore, that to know the causes to which a change of temperature is due we need only compare the various conditions under which the temperature of a body is either increased or diminished with those conditions which leave the thermal state of a body unaffected. The inquiry being thus narrowed down to a comparison of conditions, we shall be able to apply the rules of induction as laid down by Mill in the following words:

'*First canon. If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree is the cause* (*or effect*) *of the given phenomenon.*

'*Second canon. If an instance in which the phenomenon under investigation occurs, and an instance in which it*

[1] We still use the term heat as a heading, though thermal states, or thermal changes, would be more appropriate. We do so, however, in deference to accepted habits of thought, so as to establish a continuity, so to speak, between current conceptions and those which are to supersede them.

does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.

'*Third canon. If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.*

'*Fourth canon. Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.*

'*Fifth canon. Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation.*'

To the above five canons we might add another useful rule, as follows: Any cause, agency, or condition, which may occasion an increase or diminution of any quality, must be regarded as the cause of that quality. From this point of view the discovery of the causes of phenomena currently called the different 'forces,' or different 'forms of energy,' is entirely removed from the domain of mysticism and speculation, and is brought within the practical field of physical methods, inasmuch as the causes of these different states can be traced experimentally, by comparing the conditions under which any such manifestations appear with those which fail to produce them.

As our first experiment we will take an electric battery, and insert in the circuit silver, copper, iron, and platinum wires. Of these we shall note the platinum to become heated. If the current be increased, the iron, too, may become hot; and, if the current be very great and the conducting wires very thin, even silver and copper may become sufficiently heated to be melted.

Shall we say, then, that heat is due to electricity? To this question we would have to answer No! since electricity does not always result in the production of the phenomenon. Nor can we attribute it to the presence of particular bodies, such as platinum or iron, since the phenomenon may appear with silver alone, while in other instances the electrically excited silver may fail to give rise to the particular manifestation. We want to lay hold of a cause or agency which is always present when the phenomenon is produced, and in the absence of which the phenomenon never appears. If we can lay hold of such a cause or condition, we shall be entitled to the belief that we have discovered the true conditions of the particular manifestation, and by suitable experiments shall be able to raise our belief to certainty.

The next step in our reasoning is to inquire, in what respect do platinum, iron, and silver differ from each other under the influence of electric excitation? And, secondly, in what respect are the conditions of the silver relatively to the electric current varying when in one of two experiments the silver remains thermally unaffected and in the second becomes heated?

The obvious answer to the first question that will occur to the observer will be the different conductivities of the above-named metals. In respect of an electric current, the platinum differs from the iron in its greater resistance; and the iron differs from the silver in the same respect. So that we may provisionally conclude that the heating effect of the electric current in these bodies is due to their greater resistance. Following up this clue we make different experiments, which lead us to attribute the heating effect to the greater resistance of the platinum and iron. But, inasmuch as silver, the resistance of which is so much less, may also be heated under certain conditions, we cannot yet have reached the ultimate or true cause. By further experimentation and reasoning, which we shall not dwell on at length, we have arrived at the generalization that heat is due to coerced states of bodies; that is, when a body is excited and cannot satisfy its tendency to equalization this state results in an increase of temperature.

Briefly, then, we concluded that the 'generation of heat,' or, more correctly, increase of temperature, is due to States or Acts of Coercion.

The foregoing experiments seem at once to confirm this theory; at least, there seems to be no disparity between the conditions under which the different metals have been heated and the causes here assigned to the phenomena. For if a body possess great resistance there is bound to be coercion, inasmuch as such body endeavours to resist a change of state. Now, when an electric current is passed along good conductors, the excitation received by such a conductor would as quickly be passed on to other bodies, and there would be a 'diffusion' or a 'transference' of excitation. In other words, the good conductor would as quickly return to relative equilibrium as it had been excited. But, supposing that the circuit consisted altogether of thin silver wire, and that this excitation could not be conducted away, then even a good conductor might be excited to a degree at which it would become heated.

We do not offer this as evidence of the theory, but merely as showing that it would not be incompatible with the supposed explanation. For proof we must look to experiments suggested by deductions made from this generalization. We can think of many ways of coercing bodies; i.e. of preventing them from satisfying their natural tendency to equalization; and, if our generalization be true, then in every such case there should be a manifestation of heat.

Let us take a couple of magnets. As is well known, if a magnet be free to move it will assume a certain definite position relatively to another magnet. We poise a magnet so that it can revolve freely in a horizontal plane and bring near to it one pole of another magnet. By way of distinction we will call the former the armature, and the latter the field-magnet. If the field-magnet be revolved near the armature, so that its opposite poles are alternately brought nearer to the latter, then the armature will always follow the motions of the field-magnet; that is, its like pole moving from and its unlike pole towards the pole of the field-magnet nearest to it. We continue to revolve this field-magnet, varying the speed, and find the armature to follow its motions. If, therefore, we prevented the armature from moving, but continued the revolutions of the field-magnet, the former, according to our reasoning, would be in a coerced state; inasmuch as its natural tendency would be to follow the motions of the field-magnet, but is prevented from doing so.

Now, if the above conclusion be correct, there should be an increase of the temperature of the coerced magnet. And this is actually the case. We take two bar magnets and place them parallel to each other, with a delicate thermometer between their further poles, so that the bulb of the thermometer is touched on either side by one of the magnets, and then revolve our inducing magnet. If this is done with even a moderate speed, just turning the magnet in the hand without any mechanical appliance, the mercury of the thermometer will rise. Of course, the increase of temperature would depend on the velocity given to the inducing field-magnet, and also on the temperature of the air and other conditions under which the experiment is made, and that because these conditions influence the rate of diffusion. For it is to be borne in mind that, as soon as the temperature of the magnet rises above that of the atmosphere and surrounding objects, equalization will begin to take place; and, unless the rate of increase of temperature exceeds the rate of diffusion, a slight increase of temperature may easily escape detection. But if the experiment just described be carried out with ordinary care and skill, using a delicate thermometer [1], there will be no difficulty in producing an increase of temperature of several degrees. And with a great velocity, and under conditions which would prevent loss by radiation, this heating might be increased to such an extent as to heat the magnet to redness—that is, if the magnet were so conditioned that the excitation it received could not be conducted away and thus diffused to other objects. Such overheating is often observed in dynamos, when the resistance of the conducting wires is great and the rate of generation high.

Our first experiment, therefore, is a most satisfactory confirmation of our conclusion that the manifestation of increased temperature is due to a state of coercion, produced by the resistance which prevents a body from satisfying a certain tendency to equalization. In passing it may also be noted that it is immaterial whether this coercion be due to internal resistance or to external causes. Indeed, the distinction between external and internal resistance is more artificial (or subjective) than real; for what is called internal resistance may be considered as the resistance which one molecule offers to the modifying tendency of another molecule. But philosophically one molecule of iron is as much an

[1] We have employed one graduated in tenths of degrees, and one degree of which corresponded to 16 mm. of mercurial thread.

external body to another molecule of iron within the same mass as two bodies which are separated from each other.

Before passing on to another experiment we will also note that when these excited magnets are placed within helices of wires, and an electroscope is inserted in the circuit, the needle of the electroscope is deflected; and what we have just now identified as 'heat' would thus manifest itself as 'electricity.' Had we, therefore, allowed the excited magnet to revolve according to its tendency, we should have seen nothing but 'magnetism'; but, having been coerced into rest, this 'magnetism' manifests itself as 'heat'; and, having by means of wires conducted this excitation to an electroscope, what we have just now identified as 'heat' has all at once become 'electricity.'

But from the foregoing experiments it is clear that if the magnetic needle of the electroscope had been forcibly retained in its position, so that its deflection had been made impossible, it would have become hotter (and possibly a thermo-pile might have made the increase of temperature apparent); in which case, instead of 'electricity,' we should again have the presence of 'heat.'

It was not, therefore, three different 'forces,' nor three distinct 'kinds of energy,' which we have witnessed, but simply three kinds of manifestation, indicative of the three states of matter: to wit, relative equilibrium, relative excitation, and states of coercion. The revolving inducing magnet excited the stationary magnet; the latter, not being free to place itself in adjustment with the altering conditions of the revolving magnet, and not being able to conduct away the excitation thus received (i.e. not being able to equalize itself either with the magnet or other bodies), became relatively more highly excited, and was thus in a state of coercion. As soon as it is connected with a good conductor, this excitation is carried away, and equalization proceeds; and, if this equalization can take place as fast as the excitation is received, the magnet will not be heated, but may become a source of 'heat' or a source of 'electricity,' a source of 'magnetism,' a source of 'light,' or a source of mechanical power, according to the nature of the obstacle or resistance we interpose to intercept this process of equalization.

This is the fundamental principle of dynamos, as, indeed, of all mechanical contrivances. If both field-magnet and armature of a dynamo were free to rotate, it would no longer be a source of power, either in the shape of 'electricity,' 'heat,' 'light,' or 'motion.' But, so soon as an obstacle is placed in the way of this free equalization, power can be made available for mechanical purposes. In Fig. 5 is represented the simplest form of a magneto-dynamic machine. For simplicity's sake we take two permanent horseshoe magnets. The two magnets are so mounted that each can revolve freely and independently of each other. The illustration shows them in their normal position, viz., with their opposite poles near each other. If the magnet *A* be now turned round on its vertical axis, the magnet *B* would perform a corresponding motion. If in the road of *B* be placed a body, *x*, the resistance of which would not be sufficient to arrest its motion, then this body would be pushed forward; and thus direct work would be obtained out of one of the magnets by moving the other. But, supposing that the obstruction be greater than the tendency of the magnet to revolve could overcome, then, if the magnet *A* be revolved, *B* would be in a state of intercepted equalization, or, in our adopted terminology, in a coerced state. Conducting this excitation to another similar arrangement, we shall be able, by a suitable contrivance, to make a distant magnet revolve and to perform work. There is no difference in principle between the first mode or second mode of obtaining work from such a source. In each case the work done will be due to a partially intercepted process of equalization. Yet in the first case the work would, according to current conceptions, be ascribed to 'magnetism,' and in the second to 'electricity.'

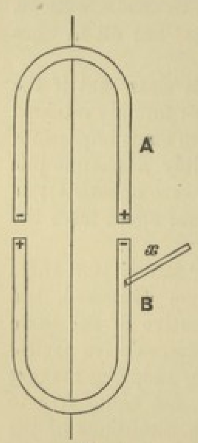

FIG. 5.

Nor is this all. We might cause our inducing magnet to revolve by hand, by a descending weight, or by expanding steam; and according to the nature of the original disturbance there would be 'a conversion of muscular, gravitation, or heat-energy into electric energy.'

Power derived from any other source is due to the same principle. If we push a volume of air into a vessel whence it cannot escape, we literally coerce it into a smaller volume; and, if the air cannot escape through some opening as fast as it is being pushed in, there will be a source of power, which might be utilized in various ways: just as in the case of a coerced magnet.

But let us return to our experiments to verify our former conclusion that the manifestation of 'heat' is due to states of coercion. Air thus compressed in a vessel becomes heated. Of course, if retained there, this heat will be diffused, in accordance with the tendency to equalization, until it is of the same temperature as surrounding bodies. Here, too, the extent of heating in consequence of compression will depend, as in the case of magnets, on the rate with which this compression is proceeded with relatively to the rate of diffusion. It is not difficult to compress air in a vessel at so slow a rate that diffusion (or equalization) shall take place as fast as the increase of temperature[1]; in which case the senses would give no indication of heating. But if the compression be proceeded with at such a rate that the rise of temperature shall be greater than the simultaneous diffusion, or radiation, there may be an increase of temperature sufficient to ignite easily combustible bodies. If an electroscope be connected with such a vessel, this compression may be shown to be capable of manifesting itself again as 'electricity'; and, once we have 'electricity,' we can, by our former experiments, obtain also from the same source all other manifestations.

But it may be said that after the vessel of compressed air has cooled down to normal temperature the air still remains in a state of coercion without giving any evidence of generating 'heat.' Our own belief is that all bodies while in states of coercion are constant sources of heating, but that the diffusion is equal to the rate of generation, and hence no sensible increase of temperature can take place. The proof or disproof of this statement is not beyond experimental verification, but requires further investigation. Of water it is known

[1] Or, if surrounded by cooling media, the temperature may even be lowered at the same time. Yet, notwithstanding this, the heating consequent on compression would be a fact, though not ordinarily perceptible under such conditions.

that it will freeze more readily if exposed to an external cold in an exhausted vessel than when under pressure. But this may possibly be due to causes (as, for instance, the bad conducting power of the air) other than the simultaneous slight generation of heat due to the state of coercion. On the other hand, almost all bodies we are able to think of are, when in states of coercion, good protectors against cold, amongst which may be classed snow and ice, which properties we are inclined to attribute to the fact of their being in states of molecular coërcion. Certain it is that under the act of coercion bodies do become heated. Thus, when bodies are compressed (and under circumstances when expanded) beyond their normal state, a rise of temperature becomes manifest. The doubtful point is only as to whether this generation of heat is continuous while the state of coercion lasts, or whether it only takes place during the act of coercing. From reasoning we are inclined to the belief that the former is the case, though, as we have said, the point needs further investigation [1]. But as to the fact of bodies becoming heated while being coerced, by whatever means, there can be no doubt whatever. That pressure, whether of gases or of solids, results in an increase of temperature, is common knowledge. In case of steel-springs or indiarubber, there is an increase of temperature both when they are compressed or stretched beyond their normal state. Gases when in the act of attenuation beyond the state that is normal to them, we have no

[1] All experimental evidence points to the fact that heat is generated during the act of coercion only, and not during states of coercion. If nevertheless we hesitate to accept this as final, it is because our investigations have given us ample reason not to pronounce a thing impossible because our senses fail to detect changes which, from general principles, we are led to believe to take place. The different thermal states of bodies require explaining after the existence of heat as an entity has been rejected. We purposely put the case thus doubtfully in order to induce further investigations. We must confess, however, that, whilst abstract reasoning would lead us to believe that bodies evolve some heat while in states of coercion, experiments point in the opposite direction; viz. that this evolution of heat takes place only during acts of coercion. But, if we consider that in this world of changes the conditions cannot be the same in any two consecutive seconds of time, then it becomes clear that bodies not free to adjust themselves to the ever-varying conditions must of necessity be exposed to constant acts of coercion. Thus, for instance, a magnet would be subjected to acts of coercion with every change of the magnetic meridian; and so likewise every other body in proportion to its degree of resistance. These passing but constant *acts of coercion* may in each instance be very slight, yet sufficient to account for all the phenomena of different thermal states, even if it should finally be settled that heat is evolved only during acts of coercion.

doubt in our own mind, would also be found to become *momentarily* heated just at *the moment* of expansion; but, equalization of the particles being thereby greatly facilitated, and this resulting in the opposite action of cooling, the phenomenon may thereby be masked.

A third source of 'heat' is when bodies in motion are suddenly arrested by striking against an obstacle: which is again a case of coercion. 'Heat' has been said to be 'a mode of motion.' But here we have 'heat' manifested through arrested motion. True, the explanation is that the motion through space now manifests itself as 'molecular motion.' But the peculiarity of the phenomenon is that the body does not manifest this 'heat' while falling, but only when its motion is arrested. And, again, if 'heat' were due to motion, then equal weights of different substances falling from equal heights should either manifest the same increase of temperature, or it should be the greater the more free this molecular motion. This, however, is not borne out by common experience. Experiments on this point would be desirable; for instance as to whether the heating effects of equal weights of different substances, such as water, lead, platinum, &c., if allowed to fall from equal heights, would be the same. The specific heat of these bodies being known, the relative increase of temperature due to the fall of such bodies could thus be determined. We must confess, however, that we are unable to suggest the means whereby such data could be obtained with sufficient accuracy.

One thing, however, is as certain as it is obvious, that in any circumstances 'heat' is the *result* and not the *cause* of thermal phenomena; since 'heat' (in the sense of an entity) is but a hypothesis to account for a phenomenon not yet traced to its causes. It is merely the familiar and discredited *caloric* under a new *alias*.

Again, in case of friction the heating will be proportional, not to the velocity of motion or pressure, but to the *amount of resistance*. This is well known to machine-users, who are careful that their spindles shall be smooth and well oiled. In a revolving spindle, with equal weights and velocities, there will be less heating in the presence of oil than in its absence. Pressure and motion are in each case the same, but not the resistance. Nor could Dr. Joule have obtained identical results when substituting mercury or iron for water, as would appear from his statement that '. . . it was therefore necessary to make allowance for the quantity of force expended in producing these effects'—i.e. of vibration and sound, to which he attributed any disparity in the results he obtained.

If, instead of his paddles, Joule had used an arrangement of a spring fixed with one end to a revolving shaft and with its other end pressing inside a ring, then he would have found that as this ring was composed of iron, copper, glass, or wood, so the results obtained by him, by measuring the increase of temperature, would have varied in each case. In other words, a smooth ring with a smooth spring pressing against it would not have resulted in the same amount of 'heat' as with two roughened surfaces, even though the amount of 'work' expended were the same.

Instead, therefore, of assuming 'heat' as an entity, we shall henceforth know it merely as a state or quality of bodies, which can be augmented or diminished, and which is due to states of coercion, i.e. when bodies are prevented from satisfying their natural tendencies[1].

[1] The phenomena of evaporation under varying pressures, and the consequent variations of temperature, deserve attention in the light of the above explanations, of which they may be regarded as other sources of evidence. So likewise the gradual decrease of temperature with pressure in the atmosphere as we are ascending higher and higher will at once become intelligible if we bear in mind that temperature is a result of pressure, and hence we should naturally expect to find a higher temperature where the pressure is greater. And, if we have reasoned correctly from our experiments and principles, we might even be able to assign a cause for the internal heat of our globe; but to this we may have occasion to refer again.

CHAPTER IV

MOLECULAR PHYSICS

Of absolute rest nature gives us no evidence; all matter, as far as we can ascertain, is ever in movement, not merely in masses as with the planetary spheres, but also molecularly, or throughout its most intimate structure; thus every alteration of temperature produces a molecular change throughout the whole substance heated or cooled; slow chemical or electrical actions, actions of light or invisible radiant forces, are always at play, so that as a fact we cannot predicate of any portion of matter that it is absolutely at rest.—W. R. GROVE.

BEFORE continuing our study of thermal phenomena, we shall have to make a short digression on molecular physics; and shall resume our subject in the next chapter. Pressure[1], we have said, results in increased temperature; and that because pressure always results when two bodies oppose each other, whereby the natural tendency of both is intercepted, and a state results which we have designated as a state of coercion.

To deduce from this why or how gases in a greater state of density result in an increase of temperature, we must briefly consider how the molecules in a body are affected by change of conditions. To this end we must form some conception of a molecule. There are two things we have to bear in mind in connexion therewith; firstly, that the conception would have to be a true one, that is, in accordance with facts; secondly, that the molecule is not visible to us, hence it would seem difficult to adduce its true deportment with anything like certainty. The difficulty, however, of forming a true conception of molecular physics is not so great as would

[1] The word resistance or coercion would here be more appropriate as being more general. But it could be shown that resistance, coercion, friction, &c., can all be resolved into pressure; and so we use this term, not because we think it philosophically the most suitable, but because of its more graphic suggestions as to the nature of an invisible phenomenon.

appear at first sight. We would again merely have to clear the mind of those conceptions which the term molecule calls up, and look more closely at the facts rather than dwell on accepted ideas, in order to admit light into darkness.

As currently conceived, the molecule is a hypothetical entity, with which we have here no concern whatever. What we understand by the term molecule is simply a small particle of matter—an infinitesimally small body which forms a fraction of a larger mass. By a molecule we mean—

Any body, or mass of matter, which in respect of any other body, or mass of matter, behaves like one body.

When we heat a mass of copper, *part* of the mass is heated first, and the remaining portion receives its heat by conduction. We conceive, therefore, such a mass of copper to consist of innumerable smaller masses, which react with each other in the same manner as would two distinct masses of copper. Any portion of such mass, therefore, which behaves in respect of another similar mass within one and the same solid as one body, would be a molecule according to our conception.

The definition is a vague one, and made so purposely, in order to leave it undecided whether the mass, shape, and condition of these particles remain unalterable, or whether these supposed molecules are variable both as regards quantity and shape. As just said, by it we simply mean a body; and in that sense we may regard the sun or earth—or, for that matter, our whole solar system—as a single molecule, in the same sense as we would the smallest particle of matter the mind can conceive of.

Under this point of view the study of molecular physics need no longer remain merely speculative, but can be made the subject of experiment and observation. We can study the behaviour of a mass of copper of sufficient size to be easily observable, and may deduce from it that under like conditions the smallest particle of copper, even the hypothetical molecule, would behave in the same manner. Whatever may be the specific resistance of copper to heat or electricity will be the same in its smallest particles. If, therefore, we study such qualities of bodies as are essential, and not merely incidental to shape or aggregation, as explained in an earlier part, then we shall at the same time have studied the qualities of the molecules of such substances.

Take, for instance, a magnet. Two magnets attract each other by their unlike poles. By cutting a magnet in two we shall have two such magnets which will behave similarly. These again may be divided; and again we should have the same phenomena. Thus we can go on dividing the magnets into smaller and smaller parts, with the result that we should have a large number of magnets, smaller in size than the original, but otherwise each part behaving exactly as did the mass before division. There is no reason to suppose, therefore, that with a still further subdivision our particles of matter would change those qualities which constitute the distinguishing characteristics of the substance.

There is, then, no difficulty whatever in inferring the effects certain conditions would produce on the particles composing a solid mass from the behaviour of the mass itself—provided we are always careful to make sure that any characteristic manifestation of a larger mass is not a quality merely incidental to shape or aggregation, but is, in truth, a characteristic of the substance itself. If, therefore, we find that a magnet tends to assume certain positions relatively to other magnets, we must assume that the particles of matter of which that magnet is composed possess a like tendency both relatively to each other and relatively to other bodies. And if we find in the larger magnet that, where such adjustment to altered conditions cannot take place it results in an increase of temperature, we shall conclude the same to take place when the particles composing a mass are similarly hindered in their free adjustments.

There is no difficulty in proving the truth of this conclusion. It is easy to make a large magnet composed of very small particles. Steel filings enclosed in a glass tube would admirably serve our purpose. We have used by preference needle-points about one-eighth of an inch long and all of equal size, so as to be able to watch their deportment. As is well known, a glass tube thus filled may be magnetized, and thus have conferred on it the properties of an ordinary bar magnet. If such a magnetized tube be suspended horizontally and another magnet brought near it, it will turn to or recede from the latter, according as one or the other pole is brought near it.

But let us place this magnetized tube so as to be prevented from moving bodily, and then approach it with one of the poles of a powerful bar magnet. It will then be noticed that the small bits of steel inside the tube tend to turn towards this magnet with their opposite poles. Some of them, those on the surface of the mass, will actually do so; but most of them, i.e. those packed between others, will not be able to turn round, partly for want of room, and partly from want of power to overcome resistance of friction, gravity, and the influence of the other minute magnets which attract them in opposite directions. These small magnets within the tube would, therefore, be under similar conditions as when a magnet is revolved in front of another magnet which is prevented from adjusting itself to the motions of the former. There will be pressure, coercion, the evolution of heat, &c.

Now we need only conceive an ordinary magnet to consist of innumerable small particles closely packed together, so as to form a solitary mass, in order to be able to conceive a reaction between the molecules within that mass analogous to the behaviour of the mass itself.

So likewise with other substances and in respect of other qualities. By heating one end of a copper wire the whole mass is not heated all at once. The rise of temperature proceeds gradually from end to end, thus showing that there is resistance in the copper to thermal changes. We may conceive, therefore, this heating process in the first instance as a process of equalization between the source of heat and those particles of the mass nearest thereto; then a similar process of equalization to proceed between those heated particles and the adjacent colder particles; and so on. Thus a new branch of physical science will be opened, which will enable us to think of reactions taking place between two bodies identical in substance. Thus copper can react with copper, iron with iron, and so forth; and to this end it is not necessary that there should be two distinct masses of the same substance, but one and the same mass may be conceived as consisting of an aggregation of an infinite number of smaller masses in constant reaction with each other.

There is, in truth, no such thing as 'dead' or 'inert' matter, save in the crudest anthropomorphic sense. Outside the window where we are writing is an iron lamp-post, which to our dull senses seems 'dead' indeed. Yet have we the means of knowing for a certainty that within that mass of seemingly dead iron there is a seething activity between the particles which compose it, which, could we see it, would defy description even in the most general terms—and presently we shall be able to tell the reader how he may actually obtain a glimpse of these maelstroms, to excite his admiration beyond anything the imagination could conceive.

But how do we know that these activities do take place within that lamp-post? The rays of the sun fall on it, warming the external parts of that side which is turned towards the sun. This heat cannot directly and simultaneously warm all the particles; the remaining mass can only be warmed by conduction from particle to particle. In the meantime there are gusts of wind, now striking this part, now that, more forcibly, and delicate thermometers, placed at different heights close to the post, would show how the temperature is constantly varying, now being slightly higher here, now there, and so on, the different mercurial threads constantly dancing up and down.

Now to *see* all this actually we need only substitute a liquid for a solid. A glass of clear water will look as 'dead' and motionless as a lamp-post; yet is there a continuous circulation, myriads of currents—literally myriads—flowing at the same time in all possible directions. By placing in that clear water some very fine solid particles—say some precipitate—the specific gravity of which is the same as that of the liquid, this will at once become visible. But a still more magnificent spectacle will be seen if some naphthol be dissolved in alcohol (of course, many other substances would serve the same purpose) and a drop of this clear solution be placed under a one-inch power microscope on an uncovered slide—the presence of particles of dust would by no means be a disadvantage. We shall not attempt to describe the activities of these 'dead' substances: but, if the reader will take the trouble to make such a simple experiment, molecular interactions will be made visible to him. We do not say, of course, that the particles of iron in the solid mass float about in a like manner; nor do we care to affirm positively that the particles move or rotate each in its respective position, though we incline strongly to the belief that the latter is the case in many solids. But in any case the *tendency* to move and rotate is there, and would necessarily produce the corresponding effects. Expansion and contraction, however, are bound to go on during these changes of temperature, so that we may infer, and even demonstrate, positive molecular motion of some kind[1].

Thus both by reasoning from known facts, as well as by actual demonstration, may we convince ourselves of the truth of intermolecular activities; and if we conceive these molecules in the only light in which we can possibly conceive them, i.e. as small masses of matter, we shall be bound to conclude that their motions, activities, and manifestations are due to the same causes and are governed by the same principles as those of larger masses. These motions we must ascribe to their reciprocal attractions in consequence of being in different states of excitation, though complicated in many respects by local conditions. Thus, in a glass of water the warmer particles at the bottom of the vessel will be displaced by the colder and heavier particles at the surface, which is entirely due to the attractive force exerted by our earth. This is one of the local disturbances which is ever present. Again, a particle in its ascent may meet with another particle moving in an opposite direction, causing a mutual deflection; which is another such local disturbance. And there are, no doubt, many others of which we are entirely ignorant.

Having premised these general facts, let us endeavour now to deduce theoretically what should be the behaviour of homogeneous substances under varying conditions, and then see whether such theoretical deductions are verifiable by experiment and observation.

[1] Our mental conception of molecular activities is derived from the actual observation of a large number of small magnets (such as are used as watch pendants) ranged side by side in several rows, and then noting their behaviour when a magnet or a piece of iron is moved about in their vicinity. Such a group of magnets represented to our mind a solid, and the individual magnets the molecules. If these needles had not been mounted, but had been closely packed together, their *tendency* to move under the influence of a magnet would be the same, although no longer able to satisfy this tendency. This, then, is our mental conception of the molecules within any mass, and is based on the following philosophic axioms:—(1) Whatever (primal) quality may be discovered to belong to any particle of matter is a quality of all matter without exception, differing merely in degree; (2) Whatever (primal) quality is possessed by a mass belongs in an equal degree to the minutest parts of that same mass; in short, the quality we observe in connexion with large masses of matter is the aggregate result of the qualities possessed by its constituent particles. With this explanation the reader should have no difficulty in being able to follow mentally the phenomena we have in our mind when speaking of molecular actions.

The only recognized circulation in liquids under the influence of thermal changes ('convection') is the one which is due to gravitation; i.e. the displacement of hot by colder and heavier particles. When heated, bodies expand and become specifically lighter; and, when cooled, contract, becoming specifically heavier; so that the heavier particle is continually displacing the lighter, resulting in the well-known circulation of liquids when heated from below. But the molecular interactions we are thinking of are distinct from these, and would take place in the absence of such a third body as our earth, which exerts a disturbing influence on the regular process of equalization which would take place

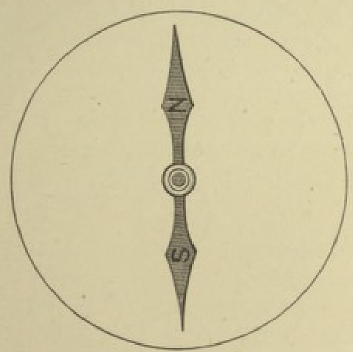

FIG. 6.

between two reacting particles: just as the earth's magnetism is a disturbing element when we are experimenting with magnets in our laboratories. To get a mental representation of the nature of molecular interactions we shall again turn to our magnets.

For this purpose we have employed very light needles, two inches long and delicately poised on agate bearings. Fig. 6 is a full-sized representation of the needle as mounted on brass disks.

Six or more such magnets are arranged in the form of a circle, or in any other grouping, but so that they shall be well within each other's magnetic field, without being too near. The following diagram, Fig. 7, represents the arrangement we have made use of.

A is a round cardboard disk, divided into 360° as shown, resting on a board, *B*, also divided into 360°. The disk *A* is so adjusted that the line passing from *N* through the centre is in the magnetic meridian. The board *B* is fixed, and the circular disk carrying the magnets is movable. In this manner the latter can be revolved, and by comparing the two scales the angle of rotation out of the magnetic meridian can

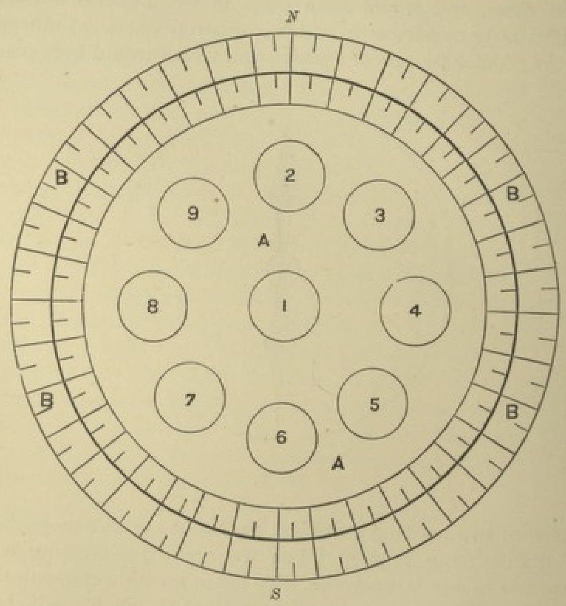

Fig. 7.

be readily ascertained. The positions of the magnets are marked by the circles 1 to 9. By this arrangement the position of each magnet relatively to the magnetic meridian when in the various positions could at once be ascertained by inspection, as shown in the diagram.

First, we placed one of the magnets in the centre (marked 1), and the circular disk *A* was turned with its *N* to the magnetic north. If a second magnet be now placed into any of the circles which are not on the meridian, the central magnet at once deviates from its meridional position, according to the position of the second magnet. The same happens with the addition of every magnet; there is always a disturbance in the system, and a readjustment in accordance with the modified conditions. If now the disk *A* be turned to the right or left ever so slightly, all the magnets again alter their directions; for the effect would be the same as if the magnetic meridian had been shifted. But what is to be particularly noted in this experiment is that the extent and direction of the alterations in position are not the same for all the magnets in the system; and that because the consequent adjustments are not only relatively to the magnetic meridian, but also relatively to each other. Each change relatively to the meridian disturbs also their adjustments with each other; but the extent and direction of these readjustments will be determined by their positions relatively to each other as well as relatively to the magnetic meridian.

Still more striking results are obtained if a piece of iron, or a permanent magnet, be brought near to the system, at first from one side, then from the other, now above, now underneath, now nearer to and now further from the arrangement. In each case the extent of modification, as well as the direction of each of the poised magnets, differs.

Or, again, we may place one of these magnets at such a distance from a bar magnet that the latter exerts no visible influence on it, and then place a second magnet between the two. As the bar magnet is now revolved, the intermediary magnet will change its position, and in consequence thereof the furthest magnet will perform a corresponding movement. But, supposing this latter magnet to be part of a system of several such magnets, but that itself only has been within the reach of the external magnet, then the change in position in this one magnet would result in a change in that of every other magnet in the system—a change which is by no means very simple to express. Let the system consist of three magnets, a, b, and c; then, the position of a being changed, both b and c would follow the direction of a; but thereby the equilibrium of b and c would be disturbed, and, b and c trying to adjust themselves with each other relatively to their new positions, the equilibrium of *a* relatively to both *b* and *c* would be destroyed. There would be an oscillation to and fro, each new position of any one of the magnets creating a new disturbance and necessitating new readjustments throughout the whole system.

In this wise the manner in which the molecules must affect each other can be made visible in a most instructive manner; and we would strongly urge on our readers to procure half a dozen of these little magnets (such as are used for watch pendants might serve the purpose) in order to see these reactions for themselves. For, simple as the whole thing is, it is impossible to convey in words even an approximate idea of the amount of instruction to be obtained from such few bits of magnetized iron.

The behaviour of such magnets may be taken to represent the behaviour of the particles of which they are composed. This is demonstrable of particles so small that the human eye can but just recognize them. On this point we have satisfied ourselves by detaching such particles from a natural lodestone by means of a file, and each was found to be distinctly magnetic. And there is no reason to suppose that in case of further subdivision the same result would not hold good, if the particles could be examined separately. Furthermore, we must assume the same law to hold good for all matter, and not merely for what are called 'magnetic' bodies, though in a variable degree. We do not mean thereby that all bodies are necessarily 'magnetic'[1]. Magnetism is but one peculiar manifestation of a general law—i.e. the tendency of all matter to relative equilibration; just as heat, light, electricity, gravity, &c., are manifestations of the same law, though in consequence of varying conditions giving rise to different phenomena. Because iron, nickel, &c., manifest this quality in a very striking degree, whilst other bodies will manifest similar qualities under exceptional conditions only (thus when rubbed most substances will attract small particles of matter), it would be a mistake to conclude such a quality to belong exclusively to certain bodies only. In all such cases we prefer to think that particular qualities are possessed

[1] Since writing the above we have found good reason to modify this view; and further on we prove by experiment that such substances as shellac, paper, india-rubber, &c., are all 'magnetic' *if tested under proper conditions.*

by some bodies in a much higher degree than by others; and, even where we fail to demonstrate this, that the particular quality (if a *primary* quality) is possessed by that body in so feeble a degree as to be imperceptible under ordinary conditions. We do not deny the influence of terrestrial gravity on hydrogen gas, nor attribute to it a negative weight, because we always find it receding from the earth; nor do we deny the possibility of liquefying carbon, or volatilizing platinum, because we are unable to provide the conditions by which this might be effected. So likewise in respect of the mutual attraction of bodies, which in the case of iron is called magnetism. Further on, it will be shown that this magnetism is identical with every other form of attraction, inasmuch as it is due to identical causes. It is for this reason that we have adopted the general term 'states of excitation,' so as to be able to comprehend all these different manifestations under one and the same conception.

In a previous chapter we have shown how one and the same source of excitation may be transformed into any or all of these different manifestations; from which we have drawn the inference—by no means a new one—that the different manifestations depend on the different characteristics of the matter affected, and the different sensations these produce in ourselves. Thus we may take a copper wire and connect one end of it with, say, a voltaic battery, wind this copper wire into a helix, and place a bar of iron into it; continue the wire and interpose a piece of platinum; then introduce in the circuit a saline solution, and so on; and, starting with chemical decomposition, we shall have in turn magnetism, heat, light, chemical force, and so forth: innumerable illustrations of which have been supplied by Faraday.

Thus may we form a mental image as to how a change of conditions in one mass of matter may, in consequence of a disturbed equilibrium and the subsequent tendency to equalization, necessitate corresponding changes (readjustments) in other bodies. And according as the resistance of the molecules of such bodies is greater or less, hence according as the molecules of such masses are free to adjust themselves to the altered conditions or are thrown into states of coercion, so should we get different manifestations, which would be called by such distinctive names as magnetism, motion, heat, electricity, &c., according to the sensations evoked in our sensorium or the manner (instrumental or otherwise) by which we are made aware of such manifestations.

This is the light in which we view what are called the 'different forces' or 'different forms of energy,' and is the point of view from which we intend to approach the solution of these problems. All these distinctions we regard as being subjective, and, therefore, they can be solved only by the double process of analyzing the phenomena as well as the mental processes by which these are interpreted. As regards the phenomena we shall find these to be due to identical causes, and shall be able to trace the differences in manifestations to the several bodies affected possessing the identical qualities in different degrees, and producing in us different groups of sensations.

After this necessary digression we shall return to the consideration of the phenomena of heat, viewed from the standpoint as being due to coerced states; and subsequently investigate the phenomena of electricity, magnetism, &c., in a like manner. Before doing so, however, we would here summarize, in a series of propositions, the leading principles, derived from all the considerations hitherto made, which are to serve us as guides in our subsequent reasonings.

We must recognize—

1. The relativity of all bodies to each other and to varying conditions.

2. That the 'conditions' of any one body are determined by the conditions of all other bodies (in the widest sense); and in a narrower sense by the conditions of those bodies which are within its 'field of influence,' and hence can directly act on it.

3. That, every body being thus conditioned by the state of all other bodies, there is a continuous reciprocal adjustment and readjustment; and according as one body, *A*, gains (under these incessant changes of conditions) preponderance over another body, *B* (or the reverse), so one becomes *positive*[1] to the other (or vice versa).

[1] By 'positive' we always mean the body of higher relative state of excitation, and that whether this excitation manifests itself as heat, motion, magnetism, or electricity. Thus in respect of thermal states a body would be 'positive' to one of lower temperature, and 'negative' to one of a higher temperature. By 'positive' and 'negative,' therefore, we merely designate the state of

4. We conceive, then, all matter in states of relative excitation —not as *dead* or *inert*, but as sensitive to every change, ever adapting or tending to adapt itself to varying conditions.

5. We distinguish between distinct forms (or kinds) of matter according to their different degrees of resistance to varying changes, their modifiability, or modifying power, relatively to certain other bodies or states.

6. According, therefore, as the molecular resistance of a body is greater or less, and according as it can equalize itself more or less readily with other bodies in different states, and according to the manner in which we become aware of such resulting changes, so shall we characterize the changes (due to identical causes) as thermal, magnetic, electric, &c. In other words, the cause of the manifestation will always be the same, though the manifestations may vary. And, instead of attributing such distinct manifestations to magnetic, electric, or thermal 'forces,' we shall have magnetic, thermal, or electric *phenomena*, produced by identical causes; the distinctive manifestations being due either to the varying conditions or to our various modes of observation.

a body *relatively* to another body, and not that it is different in kind. The same state of the same body would thus be at the same time either 'negative' or 'positive,' according to the state of the body with which it is compared, in the same way as when we speak of a body being larger or smaller.

CHAPTER V

HEAT (*continued*)

ONCE the mind has familiarized itself with the idea that bodies, even of the same substance, if in different states of excitation, tend to equalize; then it is no longer possible to think of matter as inert or dead. Nor can we conceive the molecules within any mass of matter to be quiescent for a single moment. For, even if we assumed the atoms to be of a definite shape and in all respects exactly alike, there would still remain one important difference, viz. the difference due to position.

Take a bar of copper, for instance. The exterior particles cannot be in the same position, nor exposed to the same conditions as the interior ones. The former are in contact with the atmosphere, with water, or some other ambient substance, while the inner particles are surrounded by like particles. One side of this copper may be exposed to heat, light, electricity, magnetism; and the other side to different disturbing agencies. Equalization would proceed from both sides towards the centre, the interior particles being chilled from the one side and heated from the other. In such a case, it is evident that no two particles of that mass could at any moment be in exactly the same state of excitation.

Perfectly homogeneous masses, then, cannot exist; for, however small a particle of matter we may conceive of, such a particle would have 'sides,' and could be exposed to opposing conditions from opposite directions, whereby one part would be affected differently from another part.

That such molecular interactions within a mass of the same matter can take place, we have illustrated above with magnets. In those experiments we had the same kind of matter and the same properties in each magnet. Yet with every change of conditions each behaved differently both as regards direction and degree. Here clearly these differences of behaviour were solely due to differences of position. The absence of magnetism near the centres of bar magnets affords another illustration of this. For, if a magnet be there cut in two, the resulting ends would at once be strongly magnetic, while each of the two halves would have a neutral zone in parts where formerly they were magnetic. This, along with the experiments above adduced, is clear demonstration that such change in the properties of the molecules is due to difference of relative positions.

Taking now a glass of water, its mass cannot be 'homogeneous' as regards the properties of its particles. Some of these will be nearest the containing glass; others, at the surface, will be in contact with the air; still others, at the bottom of the vessel, would have to sustain the pressure of the supernatant molecules. But, being thus exposed to different conditions, they would be in constant reaction one with the other, floating to and fro. Thus such a mass of matter contains within itself the elements of producing changes; and that not because their tendency is to change, but *because of the tendency to equalization.*

Let us follow out this process by again magnifying, this time in imagination, the molecules, and see to what conclusions these reflections may lead.

Let us imagine a number of small magnets, say about two or three millimetres long and of a corresponding thickness, to float in this water, or any other liquid of such specific gravity that such minute magnets could freely move in it; we should then be able to notice many interesting phenomena, notably those of crystallization. For, assuming all these magnets to be of equal size and equal strength, it is evident that their groupings would become symmetrical. Furthermore, let us assume a number of such magnets suspended in a suitable liquid to have adjusted themselves, forming so many symmetrical groups of 'crystals'; and let us further assume a ray of light, or some other source of excitation, to fall upon one of these groups, or even on one individual magnet of such a group, thereby throwing it into a higher state of excitation. Then its magnetic force would, of course, be increased, necessitating a new local readjustment. But one group, thus disturbed, may disturb the equilibrium of adjacent groups, and through these that of the others throughout the mass; evoking a motion and a readjustment throughout, until equilibrium is re-established. Suppose now a powerful magnet to be brought near, this would drive all these little floating magnets with a suddenness and great intensity in different directions, the one down, the other up, each endeavouring to fly towards the strong magnet. Then any two opposing magnets would press against each other, and all the more strongly the greater the intensity; and to sever them a proportional force would be required: hence a *solid* would result.

Let us assume now that the magnets in the liquid are so numerous as to prevent their free motion, and yet the external conditions to be continually changing. In that case the magnets, which before such disturbance were in a state of equilibrium, would now have this equilibrium disturbed; and, inasmuch as a readjustment in accordance with the now altered conditions would be impossible, the former state of equalization would be converted into a state of coercion. That is, whereas both before and after this change their arrangement and groupings may remain to all appearances the same, an important change has nevertheless taken place. For before such disturbance their groupings were due to a tendency in that direction, and they would have resisted with a certain force any attempt to alter their groupings; but after such disturbance their tendency is another way, and their groupings will now be due, not to their tendencies, but to their coerced conditions.

If now we could take this coerced mass and transfer it to a larger vessel, where the magnets would be free to move, they would at once readjust themselves in accordance with their tendencies. And, the state of coercion giving way to a state of equilibrium, we should expect, according to our theory, a depression of temperature to take place: since the state of coercion is always a state of intercepted and suspended reaction, and hence a continuous state of excitation. In this state there is, in common language, a 'potential force,' which potentiality will remain until equalization has taken place.

Thus, as already pointed out, air compressed in a vessel will press on the sides of that vessel, constituting a 'potential source of force.' But, if the external atmospheric pressure were increased until the pressures inside and outside the vessel were equal, this 'potential force,' which is due to *the tendency to equalization of matter in different states*, would be destroyed.

To return now to our glass of water. What we have noticed with our imaginary magnets actually takes place in the water. Its particles are all sensitive to every change, however slight; and, being free to move, adjust themselves to every altered condition. But, as water is cooled down, so it loses its power of adjustment; it becomes more *sluggish*; and hence, if the cooling be too rapid, the particles will not be able to adjust themselves so readily as at higher temperatures. Normally, it reaches its maximum sluggishness at about 4° C.; but this is by no means to be taken as its actual maximum density; and presently it will be shown that water may be made to assume a greater density than that of 4° C. When it has reached such a low state, at which it can no longer adjust itself freely—that is, when the particles are no longer able to overcome each other's resistance—the particles are thrown into partial states of coercion. And, if the temperature be now rapidly lowered, the difference of states between the innermost and outermost particles would be considerable, whereas their power of adjustment would be but very slight. The particles would press against each other, form a solid, and, in consequence of their now coerced states, cause an immersed thermometer to rise.

But if, on the other hand, the temperature be lowered very, very slowly—that is, at a rate corresponding to the decreased energy of the particles, so that adjustment can keep pace with the altered conditions—the temperature of water may be lowered many degrees below zero without causing it to assume the solid state, or causing the thermometer to rise. But at such a low temperature the particles will have become correspondingly more sluggish, and the least disturbance—a slight tapping at the sides of the vessel, or a particle of matter thrown into it, or a sound which may set the air in vibration—would suddenly transform the dense and sluggish liquid into a hard solid, and thereby impart a high excitation to an immersed thermometer, causing the mercury of the latter to rise [1].

This heat of congelation might possibly be sufficient to remelt the solid ice that has thus been formed, were it not that the heat thus generated is quickly diffused on account of the necessarily great cold of the surrounding atmosphere in such experiments. But, even after this momentary heat has been diffused, the frozen mass still remains in a state of excitation; it still generates heat, but the latter is in most cases (not always) diffused as fast as generated; so that the water remains in its solid state. The particles, not having sufficient power to adjust themselves, simply press against each other, and the greater the cold the greater this pressure [2].

It sounds very paradoxical, but none the less is it a fact, that the colder the ice the more heat does it generate; and that this cold may become so intense as to create in this wise—by increasing the state of coercion—sufficient heat to melt the frozen mass. And still more paradoxical is it that, in melting, the readjustment may be sufficiently sudden to produce a cold so intense as to refreeze the melting mass.

This fact makes it easy to determine whether a mass of ice is melting under the influence of external or internal heat. When a lump of ice is brought into a warm atmosphere, or thrown into hot water, it retains its hardness, liquefaction taking place on its exterior surface only. This is due to the fact that the melting of the exterior causes a depression of temperature, protecting the remaining mass of ice against the influence of heat, in a similar manner as the escaping vapours of solid CO_2 protect the latter when placed inside a hot crucible. But when the melting is due to interior excitation the whole mass of ice becomes soft and rotten. Ice, in nature, assumes the spongy form when there is what is known as a general

[1] Such sudden congelations have often come under our notice in the laboratory while operating on different substances. This happened on several occasions, more particularly with a melt of aniline in sulphuric acid during the process of making small quantities of sulphanilic acid. The melt was still hot and a perfectly clear liquid, but, as soon as the flask was slightly tilted, the whole mass suddenly congealed. Analogous phenomena with supersaturated saline solutions will readily occur to the reader.

[2] This may explain why snow and ice can serve as protectors against cold. The protection is due to the internal heat caused by the coerced state of the particles of compressed air or frozen water.

thaw; but under the influence of the sun the icicle from which the water is dropping retains its crystalline hardness[1].

By way of proof, we would direct attention to some well-known natural phenomena which seem to be anomalous, but which are quite in accordance with the general principles here advanced. Ice and snow are known to be good protectors against cold. Melting ice causes a depression of temperature sufficient to regelate two bits of ice, even when immersed in warm water! Whilst freezing water actually sends the thermometer up several degrees!

But we would direct attention to another phenomenon observable almost every winter. When snow is on the ground in a hard frozen state, the temperature does not feel so cold as when the snow begins to melt. But that is scarcely the point to which we wish to draw attention. There is often a hard frost, snow and ice everywhere covering the ground; the sun may be shining all the while without being able to thaw the frost. But it often happens that the rays of the sun do not penetrate the mists and clouds for days, yet there seems to be a sudden thaw, and again as sudden a frost, perhaps even harder than the first, making the streets impassable on account of the smoothness of the ice. Now it would be very difficult to attribute these alternate frosts and thaws entirely to the influence of the sun. On such a supposition it would be difficult to explain—that is, if we are to attribute the temperature of our climate entirely and solely to the influence of the sun—why there should be a hard frost between, say, the 10th and 20th of December, a gradual thaw the next day or two, and then again a sudden and hard frost. But on the above theory these phenomena admit of a rational and simple explanation. The frost itself is a source of heat, because the frozen particles are all in a state of coercion; while the melting of the ice and snow, and the consequent readjustment of the particles, may be the cause of another frost. The phenomena are analogous, nay, in respect of the operating causes are identical, with the production of heat and cold by first compressing air and then allowing it to expand.

[1] On many occasions have we seen such a piece of 'rotten' ice in quite a spongy state, when brought into a warm room, become hard and crystalline while the outside was melting.

Heat, therefore, must not be regarded as a kind of entity, which must always come from without. Like 'force,' heat is a result of the different states of matter, merely indicative of relative states of excitation or coercion—which latter always results from *arrested* motion or intercepted equalization[1]. Thus even in the absence of an external heat-giving body, like the sun, it is possible to conceive of a mass of matter alternately heating and cooling without any external source of heat or cold. When caustic soda is dissolved in water, there is no *external* source of heat; and, when a salt, such as sodium or ammonium nitrite, is dissolved in water, there is no external source of cold. Yet by the one means can ice be converted into steam, and, by the other, steam into ice. In the one case the state of excitation of the bodies is increased, in the other it is decreased; hence we get in the one case heat, and in the other cold. And it is worthy of notice that in either case the degree of change of temperature is altogether independent of the state of the surrounding atmosphere. Thus, for instance, if eight parts of crystallized sulphate of soda be mixed with five parts of hydrochloric acid, the temperature of the resulting mixture will be 30° C. less than the temperature of the constituents before mixing; whereas, if snow or pounded ice be mixed with crystallized calcium chloride, there is a depression of 40° C. That is, the degree of decrease in state of excitation is altogether independent of external temperature; it entirely depending on the degree of excitation, or the extent to which equalization takes place between the substances thus brought into contact.

This difference of depression of temperature in the above two cases is also easily explained on the above theory. For, when sodium sulphate is dissolved in hydrochloric acid, one of the substances employed—the hydrochloric acid—is already

[1] The late Professor Tyndall held that heat was 'a mode of motion.' It is not quite clear whether the phrase is to be understood that heat and motion are identical, or that motion is the cause of heat. But whichever view be taken would be wrong, inasmuch as heat is due to arrested motion (i. e. coercion) and is not manifested where free adjustment (i. e. motion) is possible. Thus dynamos are often heated to an alarming extent; but, if the field-magnet were free to rotate and to follow the motions of the armature, such would not be the case. In falling bodies, too, heat is only manifested when such body is suddenly arrested; and even in that case the amount of heat generated will vary as the bodies striking against each other are harder. A quantity of water or mercury falling from a certain height would not generate as much heat as would a like quantity of, say, steel falling from an equal height.

a liquid; while in the latter case both the snow and the calcium chloride are in a state of coercion. We should, therefore, have expected, theoretically, a greater depression of temperature in the latter case than in the former.

If, in accordance with the current teachings of science, it be assumed that heat is a form of 'energy'; that there is a fixed quantity of 'energy' in the universe; and that this 'energy' is gradually being converted into the form of heat; and, further, if it be granted that at one time all the planets were in a high state of incandescence, but have cooled, and that the sun and other fixed stars are now undergoing a like cooling process: then it is difficult to conceive what has become of all the heat during this *universal* process of cooling, to say nothing of the supposed gradual and simultaneous conversion of other forms of 'energy' into 'heat energy.' The two theories, of the cooling of the celestial orbs, and the tendency of other 'forms of energy' to be constantly converted into 'heat energy,' are irreconcilable with each other, and the latter with observed facts. If there were such a tendency of mechanical energy being converted into heat in the universe, it is impossible to believe that such a universal tendency should leave no trace whereby to detect it. But, putting aside these mutually exclusive theories, and reflecting merely on what seems to be a rational inference, that the planets have once been in a high state of incandescence, but have cooled: it is difficult to conceive what has become and is becoming of all this heat, or whence it all originated. The idea of an incandescent universe gradually cooling down is not compatible with the idea of the eternity of the universe: for, in that case, we should have to assume a very hot beginning and a very cold ending. It is also opposed to the law of reciprocity—viz. that every reaction is mutual. If the whole universe once consisted of a nebulous mass in a high state of incandescence, and has partly cooled down, other parts must have been correspondingly heated. But one body could not give off heat to another body so as to make the latter many more times hotter than itself. It would obviously be absurd to assume, for instance, that the great heat of our sun is due to the cooling of the planets. Yet what are we to assume has become of all the heat which our earth has given off in passing from its state of incandescence to its present state? It is quite in accordance with both experience and reason to infer that, whatever heat our earth receives from the sun, the latter loses. This interchange is in accordance with the law of reciprocal equalization. But it is impossible to imagine that a pound of water at 50° C. could transfer its heat to another pound of water of an equal temperature, adding the whole of its own 'heat'[1] to that of the other, so that the latter should be converted into steam, and the former into ice. Yet would we have to suppose some such absurdity in order to account for the loss of the heat by the planets[2]. Still more surprising and contradictory does the problem become when we assume the sun (and, by presumption, all other stars) to be cooling concurrently. For, in that case, what becomes of all the heat? and what becomes of the universal conservation of energy?

Such a supposition, that one body may heat another body over and above its own temperature, would truly be in accordance with the axiom that the tendency of nature is to change. But there is absolutely nothing in experience or reason to justify such an axiom; while its alternative, put forth in these pages, that the tendency of nature is towards equilibrium, is entirely against any such supposition.

Under the theory of excitation, coercion, and equalization, each of which propositions can be abundantly verified both by experiment and observation, a rational explanation of such problems is both possible and easy. We can conceive of a large mass in a high state of excitation approaching nearer and nearer to equilibrium; its particles moving to and fro in accordance with the law of equalization, mutually adjusting themselves to each other, while at the same time disturbing already existing adjustments; and that in this double process of adjusting and disturbing—building and destroying—the process of equalization on the whole exceeds that of the dis-

[1] We are aware that here, as elsewhere, we are speaking of 'heat' as if it meant the same as the now abandoned 'caloric.' The reason for this is our inability to see any essential difference between the two. To say that one is 'energy' and the other a 'substance' is no greater a distinction than that of spirit and matter, so long as energy is spoken of as a *thing* which has existence *per se*.

[2] The explanation that the heat is converted into motion is obviously inadmissible, since we have seen that motion may result in the annihilation of heat (moving magnets or melting ice, for instance), while arrested motion may generate heat. Furthermore, such an explanation is conflicting with the conception that heat itself is a mode of motion.

turbances, and thereby the aggregate state of excitation is gradually lessened. But it should be remembered that, while this process of equalization is proceeding, the *rate* of equalization decreases in proportion—and at a geometric ratio—as equilibrium is approached. The reaction proceeds more slowly, but still goes on until equalization, or at least relative equalization, of part of the mass is established: the result of all of which is—cooling!

But this cooling and condensing can reach a degree at which adjustment can no longer take place so freely, at which point the resulting disturbances of the double process of building and destroying may exceed the total amount of the equalizations. There is cooling and heating going on at the same time; in the one case the cooling exceeding the heating, in the other the heating exceeding the cooling: in the former case a sun may be converted into a planet, in the latter a planet into a sun, in endless and endless succession.

Of course, while such changes are taking place in consequence of purely *internal* causes, equalization is also proceeding with other masses of matter. The hot sun is warming our earth, and our cold earth is cooling the sun. But not all the heat of our earth is due to the sun. There is heat generated here, on and in our earth, in which the sun need not necessarily have any direct share; though, of course, it is impossible to separate entirely any two bodies in this universe, however far apart. But of this more further on.

Conclusion. We have thus disposed of one 'form of energy,' by showing it to be due to coerced states. Heat—that is, an increase of temperature—is the result of friction, pressure, arrested motion, or whenever particles of matter are prevented from satisfying their natural tendencies. It is, therefore, far more correct to say that heat is due to arrested motion than to say that heat is a mode of motion. But even this statement is true only when the arresting of the motion is coercive.

CHAPTER VI

ELECTRICITY

Mankind in all ages have had a strong propensity to conclude that wherever there is a name there must be a distinguishable separate entity corresponding to the name; and every complex idea which the mind has formed for itself by operating upon its conceptions of individual things was considered to have an outward objective reality answering to it. —J. S. MILL.

IF heat[1] be merely regarded as a relative *state* of matter, as explained in the preceding chapter, the problem presents no difficulties either in mental conception or in devising experiments for verification. All the trouble arose from verbal confusion due to false conceptions. The mind is so constituted that in reasoning it does not compare facts, but ideas. The facts are expressed in certain terms; and, according as such terms denote or connote ideas different from what the facts themselves would warrant, so the mind is led astray—being led on by the well-known principle of association of ideas—by ideas suggested by the words, until the facts are left all on one side, while the suggested ideas usurp their place. At that stage explanation no longer means a harmonizing of our conceptions with observed facts, but rather adapting and interpreting the facts in harmony with our ideas.

Heat was an entity, one of the children of those fertile parents 'force' and 'matter'—the dualistic conception of the universe. 'Force' and 'matter'—the metamorphs of body and soul—are the twin theories whereby the mind attempted to account for whatever it could not grasp or understand. Heat was a part of this 'force,' a peculiar form

[1] By heat we merely mean relative temperature; and a body to us is 'hot' or 'cold' according as it is compared with a body colder or hotter than itself. While we can conceive, therefore, of a *coldest* body among any number of bodies, we cannot conceive of a body void of temperature.

of it, one of the children of the great Pan so beautifully personified by Greek imagery, and so clumsily dressed up in philosophic garb by modern science. Thermal phenomena had their ready explanation in being ascribed to the appearance or disappearance of heat—heat itself being accepted as a veritable entity. Put Vulcan in the place of heat, and we have at once before us the rich imagery of juvenile imagination.

No wonder that our greatest philosophers should have tried in vain to discover the undiscoverable. Without pausing to reflect whether there was any evidence to warrant a belief in the existence of the supposed entity, they bravely set out in search of 'heat' (a kind of Holy Grail), being urged on by the *ignis fatuus* of the inherited mental concept that it was this *something* called 'heat' which was responsible for all thermal manifestations. Yet it requires but little analysis to see that 'heat' is merely a name, a noun adjective turned into an abstract noun substantive, and that beyond this grammatical derivation there is not a scintilla of evidence to prove the existence of 'an outward objective reality' answering to this mental concept.

We now approach 'electricity,' and on reflection we find this hypothetical entity to be even more shadowy than its elder sister (or brother), 'heat.' For, whilst our senses can at any rate feel this 'heat,' we have no evidence at all of anything which might rightly be termed 'electricity.' We never see or feel it either directly or indirectly; it is the purest assumption it is possible to conceive of. Let us consider for one moment in what manner we become aware of what is attributed to 'electricity.' We rub a piece of sealing-wax, or a glass rod, and we observe mutual attraction or repulsion. These phenomena are said to be due to 'frictional electricity.' Why? we do not know, because there are other attractions and repulsions which do not in any way suggest the presence of 'electricity,' or would be admitted as being due to 'it.' Or we may immerse two metal plates in a liquid and connect them with a 'galvanoscope'; that is, we place a magnetic needle near to the connecting wires. The magnetic needle is deflected, which again is regarded as having been produced by this hypothetical 'it'—to wit, 'electricity.' In the cell we have chemical decomposition; in the magnetic needle we have either attraction or repulsion. The only novel feature in the process is that the magnet has been deflected by a copper wire undergoing at its terminals chemical decomposition. Had this deflection of the magnet been caused by a piece of iron, it would have been attributed to 'magnetism'; but, this having been accomplished by other means, not previously observed or yet understood, a new personage, or a new 'it,' is required, and this is supposed to be 'electricity.'

Other evidences of this new 'ity' are the heating of a platinum wire, the decomposition of water, the magnetization of a piece of steel, or the mutual repulsion of two gold leaves. In short, we may start with heat, friction, chemical decomposition, magnets, or motion—as when a copper disk is revolved in front of magnets or masses of iron; and at the other end we shall observe heat, magnetism, chemical decomposition, or mechanical motion. In no instance is 'electricity' to be seen, but it is always *inferred* as being the occult cause of these phenomena. And it is worth while noting here that any of these phenomena can be produced by means which would not be attributed to this peculiar entity.

If we inquire into the origin of this 'electricity,' we shall find it to be due to the merest accident. A certain Thales happened to discover that when amber (electron) was rubbed it could attract small particles of matter; hence 'electricity.' Had he made the same discovery with glass, we should probably have now 'vitricity' instead of 'electricity.' Indeed, we had, and still have, other 'ities' or 'isms'; to wit, 'galvanism,' 'voltaism,' 'frictional electricity,' 'magneto-electricity,' and 'electro-magnetism.' All these 'ities' and 'isms' have now been traced to the same entity, which is making its appearance under different guises; and the problem to-day is, not to explain the causes of the phenomena, but to discover 'the nature of electricity'; the phenomena themselves being supposed to be explained by attributing them to this mystic entity[1].

We, however, will not trouble ourselves about this nymph, naiad, or kobold, but simply study the phenomena them-

[1] We are aware of the objection that nobody now regards electricity as a *thing*; but it is talked and reasoned about in all scientific books we are acquainted with as if it were; and the error is not the less grave because committed unconsciously. The same remark applies to the double fluid theory.

selves and note the several conditions under which they occur.

When a roll of sulphur, a crystal, or almost any brittle solid is broken near a gold-leaf electroscope, the latter indicates 'electricity.' Or rather we should say that it indicates nothing of the kind, that we merely see the gold leaves fly apart (we do not even know that they repel each other, since their flying apart may be due to attraction in opposite directions), the 'electricity' being in our mind a ready explanation of the phenomenon. This is one of the evidences of 'electricity.'

We rub a piece of sealing-wax and bring it near another piece; they fly against and then recede from each other. This is all we observe; the rest is supposition and theory. The phenomenon is again attributed to 'electricity'; but, there being a double phenomenon of attraction and repulsion, the difficulty is forthwith solved by creating two 'electricities,' a 'negative' and a 'positive' one. And, wonderful! the theory which has been created expressly to account for certain phenomena agrees so remarkably well with the facts that this 'agreement between fact and theory' is subsequently offered as evidence of the truth of the latter. How truly did Mephistopheles say—

'Mit Worten lässt sich trefflich streiten,
Mit Worten ein System bereiten,
An Worte lässt sich trefflich glauben,
Von einem Wort lässt sich kein Iota rauben.'

The same phenomenon and similar phenomena are produced with glass rods; and, as an excited glass rod will attract excited resin but repel excited glass, these two substances are supposed to give rise to different 'electricities,' which are distinguished as 'vitreous' and 'resinous.' Further on we shall adduce experiments in favour of the identity of these 'two kinds of electricity,' and show that there is merely a difference of degree and not of kind.

Other conditions under which so-called 'electrical' phenomena are produced are the simultaneous heating and cooling in different parts of the same body; or the heating or cooling of the junction of two dissimilar metals; or the immersion of two metals in the same liquid, or of two pieces of the same metal in different liquids; and so forth. We may thus exhaust all the known means of producing this subtle 'agent,' 'fluid,' 'force,' or 'form of energy,' and throughout we shall have to employ as sources heat, friction, chemical action, mechanical motion, &c., and shall see in turn the manifestation of heat, motion, chemical action, &c.; but never 'electricity'—the latter being in each and every case the hypothetical entity to account for the phenomena.

The supposed 'electricity,' therefore, can be produced by any of the known changes which bodies undergo; but is by no means the solitary manifestation that is produced by any of the above-named conditions. When a body is rubbed, it is said to have been 'electrified'; but we have more positive proof of its having been heated during the process; and, considering that it can now attract particles of matter, we might also say that it has been 'magnetized'—were it not that this term has already been appropriated for the phenomena of attraction manifested by certain specific substances only.

Let us further consider this form of 'electric' generation. A body is rubbed and is found capable of exciting a gold-leaf electroscope, or of deflecting a magnetic needle. But the same might be done by heating or cooling. A body rubbed has been heated, and may thus serve as a source of heat; and if such heated body be brought near one end of a copper wire, or the face of a thermopile, this heat may be utilized in producing the well-known 'thermo-electric' phenomena But the same result may be achieved if the body in question were heated by a spirit-lamp, instead of by friction; so that it would by no means be a straining of language to say that the supposed phenomenon was due to the increase of temperature, rather than to the friction: the latter being merely the means by which the body in question had been converted into a source of heating[1].

[1] A body heated in the ordinary way does not produce 'electric' phenomena as when heated by friction. The reason of this will be made clear further on, but may here be briefly stated thus: electric excitation depends on the relative molecular adjustment of two dissimilar substances, or on the process of equalization between two excited bodies of different degrees of resistance. In case of friction (the bodies being dissimilar) there are always two bodies concerned; but in case of simple heating that is not so. If, however, the heated body be brought near a thermopile, we again obtain 'electric' phenomena, because two dissimilar metals are heated simultaneously, and, being of variable resistance, unequally.

Still more simple would the problem become by saying that the body has been raised to a higher state of excitation; for in attributing the phenomena to different states of excitation it at once becomes manifest that it is indifferent by what means this higher state of excitation is brought about, whether by heating, friction, chemical action, or such permanent sources of excitation as magnets.

But it is not only in case of friction that 'heat' is generated. When a body is forcibly severed, or subjected to chemical action, a change of temperature is demonstrable, as well as an 'electric current.' And it is well known that a depression of temperature may cause 'electric' phenomena, just as increase of temperature, though the direction will be opposite to the other.

Of course, we shall approach the solution of these problems in the same way as all other problems; viz. in the light of those general principles we have adduced.

In interpreting the phenomena to be described we shall again be guided by two philosophic axioms, which have been the guiding principle of all our former reasoning; firstly, that whatever *primal* quality is possessed by any one body or substance is possessed by all other bodies, though not in equal degree; secondly, that whatever condition will cause an augmentation or diminution of any phenomenon, whereas the absence of such condition is always attended by the non-appearance of such phenomenon, is to be held the cause thereof.

We shall also hold that any manifestation exhibited by larger masses of matter would be the same in smaller masses (at a correspondingly reduced rate, of course); that is, we shall deduce the tendencies and behaviour of the molecules from the tendencies and behaviour exhibited by their larger aggregates.

We have already learnt the causes to which are due thermal manifestations. We also know that such thermal changes may, by suitable arrangements, be transmitted by means of conductors to more or less distant objects, and there cause a magnet to be deflected or a platinum wire to be heated; and that certain bodies when rubbed are heated and will attract light particles. Taking all these phenomena into account, and by a long process of reasoning and experiment, which we shall not detail here, but which will be made sufficiently clear by the experiments that are to follow, we arrived at the conclusion that bodies in different states of excitation, if free to move and uninfluenced by tendencies in other directions, would bodily fly against each other. But if prevented from doing so, either by being fixed in place or by being pulled at the same time with an equal force in opposite directions, then such bodies, if free to rotate *in situ*, would turn towards those other bodies that side which is attracted most strongly, viz. that point in respect of which the difference in state is greater. But if they can do neither the one nor the other, being hindered therein by other bodies, they will press against such bodies, and then there will result a state of coercion.

This we hold to be true of large masses of matter as well as of the molecules within any given mass. We shall now illustrate this, again employing magnets for our purpose, and then show these principles to be true of all other substances (including liquids and gases) without exception. Then we shall show that the phenomena of 'electricity' are due to the equalization of bodies in different states of excitation; and that what are considered the two 'electricities' differ from each other in degree only, and not in kind. Furthermore, we shall show that, as there is no *absolute* temperature, so there is no *absolute* 'electric' state; but that the 'electricity' of a body (like its temperature) is relative only; and, whatever its state of excitation may be, it would be *positive* to a body in a lower state of excitation, *negative* to a body in a higher state of excitation, and *indifferent* to a body in an equivalent state of excitation.

CHAPTER VII

ELECTRICITY (*continued*)

The long and constant persuasion that all the forces of nature are mutually dependent, having one common origin, or rather being different manifestations of one fundamental power, has made me often think upon the possibility of establishing, by experiment, a connexion between gravity and electricity, and so introducing the former into the group, the chain of which, including also magnetism, chemical force and heat, binds so many and such varied exhibitions of force together by common relations. . . . In searching for some principle on which an experimental inquiry after the identification or relation of the two forces could be founded, it seemed that if such a relation existed there must be something in gravity which would correspond to the dual or antithetical nature of the forms of force in electricity and magnetism.—FARADAY.

As has been said in the preceding chapter, the unknown cause of certain as yet unexplained phenomena is supposed to be an agency, or entity, to which has been given the name of 'electricity.' Whether it be called an agency, a fluid, a force, or a 'form of energy,' the fundamental idea concerning 'it' is the same. 'Electricity' is conceived as a something having an existence, inseparable perhaps from matter, yet distinct from it; while the bodies affected are supposed to be *acted on* by this hypothetical entity. Such a conception, however, is incompatible with the view of nature derived from a closer study of phenomena. It is incompatible with the law of reciprocity as stated by Newton and extended by ourselves. If every reaction is mutual, then it is no longer possible to conceive of one body as being exclusively active and the other as exclusively passive: under this view both reacting bodies must be conceived as being active and passive at the same time, and the resulting modification as mutual. Our contention is that 'electrical' phenomena form no exception; that these, too, are due to the interactions of bodies; and that the group of phenomena comprised under the special designation 'electrical' is the result of such mutual modifications, and falls under the universal law of equalization.

The points we shall principally endeavour to make clear in this chapter are:—

1. That 'electrical' phenomena can be obtained only from bodies in unequal states;

2. That, whatever may be the state of excitation of any two bodies, no 'electrical' phenomena can be evoked by their mutual action if both are in equivalent states of excitation; and hence

3. That there are no 'two electricities,' any more than there are 'heat' and 'cold,' but that any difference is merely one of degree and not of kind.

While elucidating these points, we shall also, in passing, endeavour to explain many hitherto unsolved problems of 'electrical' phenomena, thereby showing how easily these simple principles of excitation, equalization, &c., are capable of explaining the most mysterious phenomena, and that without any violence to the understanding or the necessity for any fanciful assumptions.

Let us try to prove, first, that 'electricity' is due to difference of states. As is well known, 'electrical' phenomena can be obtained either from two different metals, when exposed to like changes; or from the same metal, when subjected at the same time to different conditions. Two dissimilar metals, such as zinc and copper, or bismuth and antimony, if joined and exposed to the same temperature, manifest no 'electricity' at all; but if taken into a hotter or a colder room, while connected with a delicate galvanoscope, a deflection of the needle takes place. More than this: if such an arrangement be taken first into a colder and then into a warmer atmosphere, a deflection of the needle will take place in each instance, but in opposite directions. The experiment may be well performed with the thermo-pile, which will exhibit this phenomenon on merely carrying the instrument, while connected with the galvanoscope, from one room into another having a different temperature.

The explanation of the phenomenon is very simple. Bismuth and antimony (or copper and zinc) offer unequal resistance to thermal changes; or, in current phraseology, possess different degrees of conductivity. As a result thereof the better conductor will be heated or cooled more quickly than the other, hence will be hotter or colder, as the case may be, than the other body. But, being of unequal temperatures (or in unequal states of excitation), a reaction—a process of equalization—will take place between two such bodies. Under ordinary conditions we should know nothing of this process between two such metals; and only when some resistance is interposed, as when a magnetic needle is inserted in the circuit, are we made aware that some change has taken place in the conditions of zinc and copper, or bismuth and antimony, by such change of temperature.

The 'electric current' which is set up between the two metals is due, therefore, to their being in unequal states of excitation, and the 'electricity' manifested is simply the sensible indication of the subsequent process of equalization.

When two metals are simultaneously subjected to the chemical action of an acid, the same explanation would hold good, inasmuch as the 'electricity' depends on the unequal affinities of the two metals for the acid. In proof of this we may mention the fact that the quantitative electrical result depends on the greater difference of affinity which subsists between the two metals. Thus the 'potentials' derived from the use of zinc and silver would be equal to the sum of 'potentials' to be derived from two separate couples of zinc and iron and iron and silver [1].

The conclusion that 'electricity' is due to the equalization of two bodies in unequal states of excitation is easily verifiable in different ways. Firstly, it may be proved by showing that different metals in equivalent states of excitation do not exhibit 'electrical' phenomena. This is proved by the fact that, while under equal thermal conditions, two metals do not give rise to 'electrical' phenomena. When both are exposed to a change of temperature, however, or when both are immersed in the same fluid, they are no

[1] We make this statement on the authority of Urbanitzky (vide *Electricity in the Service of Man*, p. 99), who gives the following table on the authority of Volta :—

				Difference value.
Between	zinc	and	lead	5
,,	lead	,,	tin	1
,,	tin	,,	iron	3
,,	iron	,,	copper	2
,,	copper	,,	silver	1
,,	zinc	,,	,,	12 (= 5 + 1 + 3 + 2 + 1)
,,	,,	,,	iron	9 (= 5 + 1 + 3)
,,	lead	,,	copper	6 (= 1 + 3 + 2)

longer under like conditions, because of their unequal resistance. Secondly, this may be proved by showing that similar metals can produce 'electricity' if in different states[1]. Thus, if copper be alternately cooled and heated, or cooled and heated at the same time at the two ends, 'electrical' phenomena will result. Thirdly, when two pieces of the same metal are immersed in different substances, as for instance acids and alkalies, we again obtain 'electricity.'

In each and every case where 'electricity' is manifested, this difference of states of the two reacting bodies is clearly demonstrable; while wherever precaution is taken to have both bodies in equivalent states of excitation no 'electric current' is discoverable, and that whether the elements be of the same or of dissimilar substances. But other and more conclusive proof of this conclusion is possible; for it can be shown that bodies in a highly 'electrified' state cannot give rise to 'electrical' phenomena with other bodies in an equal state of 'electrification': just as two bodies of equal temperature could not modify each other's thermal states.

This is well known to electricians, who always calculate the available 'electric force' from what they call 'difference of potentials': meaning thereby the different degrees of excitability of two metals[2].

In support of these conclusions we will describe some experiments made with an electrical machine. As the construction of the machine is of consequence in the interpretation of the experiments, we will first give a description of its principal features. It consists of two vulcanite drums, one mounted concentrically within the other on a common spindle; but, whilst the outer drum is fixed to the spindle and revolves with it, the inner smaller drum has a sleeve in its centre through which the spindle passes loosely, and when the machine is at work the two drums are made to revolve in opposite directions. Brass conductors on either side of the drums communicate by little brushes of fine brass wire (which, however, do not actually touch the drums) with both drums. The conductor of the interior is metallically

[1] '*Effect of First Immersion.*—It is hardly possible to have the two wires of the same metal, even platinum, so exactly alike that they shall not produce a current in consequence of their difference.'—*Faraday.*

[2] See Urbanitzky, loc. cit., p. 96 (English edition by R. Wormell).

connected with the conductor of the exterior drum on each side, but the two pairs are not connected with each other. The vulcanite drums have the usual metallic strips pasted on, as shown in the illustration (Fig. 8).

From this description it is clear that the conditions in both pairs of conductors on either side are the same, and that, if our reasoning is correct, the 'electricity' in both should be identical both in kind and intensity; and this was actually the case, as will be seen from the following experiments.

1. A Leyden jar could be charged equally from either conductor, no matter from what part of the brass conductors connexion was made.

2. Two jars could be charged simultaneously by connecting

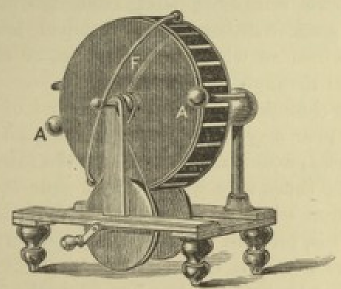

FIG. 8.

them separately one to each of the conductors of the machine. If the leading wires were of equal length and thickness, the two jars were equally charged (as judged by the subsequent discharges). The two inner or the two outer coatings would give no sparks when connected by a discharger, but sparks could be obtained by connecting the outer coating of one jar with the inner coating of another.

3. When the leading wires were of unequal length, or of unequal thickness, and the machine received only three turns of the handle (corresponding to about ten revolutions of the drums), so that the jars should not be fully charged, the jar which had most copper interposed between itself and the machine gave a much smaller spark than the other. That this was due to interposed resistance, and not to either difference in currents or different capacities of the jars, we have ascer-

tained by first changing the jars and then changing the wires. In each case whichever jar was connected to the longer wire, no matter to which side of the machine, received the lesser charge.

4. We then made the following series of experiments: Wires (cotton-covered) of different thickness were attached to each of the conductors of the machine, and their ends brought to within sparking distance. The sparks passed from the thin to the thick wire. The wires were now interchanged, with the same result; viz. the spark passed again from the thin to the thick wire, showing that the 'current' in both conductors of the machine was the same, and that the 'reversal of the current' was due to a change of the wires. (This is easily explained if we remember that from the same source of 'electricity' greater intensity is obtained from thin than from thick wires, as when water, steam, or compressed air are discharged through thin tubes.)

5. Without changing the wires, if the end of the thin wire was brought near any of the longitudinal parts of the thick wire, the sparks passed from the *thin* to the *thick* wire; but, if the end of the thick wire was brought near the longitudinal parts of the thin wire, the sparks passed from the *thick* to the *thin* wire. This, too, can be explained by different intensities, since it is well known that the discharge of conductors is more intense at their pointed ends. Thus the 'current' could be 'reversed' *ad libitum*, without any change of wires, speed, or direction of rotation, by simply approaching alternately the *side* of one wire with the *end* of the other[1].

6. To one of the wires we attached in succession carbon, zinc, iron, and a magnet. The sparks always passed from the free copper wire to any of the above-named bodies, no matter

[1] With the apparatus here described there is not the least difficulty in observing the direction of the spark. It always passes from that side of the generator where the resistance of the attached wire is least. And, considering that every electrician admits now that the power obtainable from any electric source depends on what is called 'difference of potentials,' it is clear that of two wires fed from the same source, but unequal in resistance, the one of least resistance will have at its end a higher potential than the other, and consequently the current is bound to pass from the wire of higher to the wire of lower potential. Analogous results might be obtained from a head of water, if the same be allowed to run out from a tank through a branched pipe, the two branches being either of unequal length or unequal bore. In either case the volumes and velocities discharged at the two ends would be unequal.

to which of the wires, or to whichever side of the machine, they were attached.

Now concerning the cause of these phenomena, or 'the source of electricity,' we have two dissimilar substances, metal and vulcanite, rubbing against each other; but, inasmuch as the resistance of each is different, they are differently excited: a point of importance to which we shall revert further on. Equally obvious is it, from the construction of the instrument, that, if there be one kind of electricity only, this machine should give identical results from either of its two conductors, which the above experiments amply verify. For whenever we have obtained different results this could be shown to be

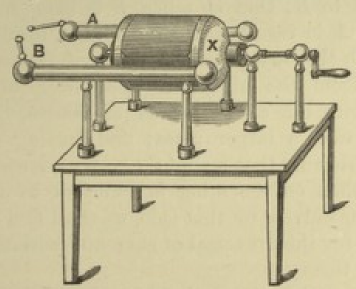

FIG. 9.

due to a difference in the 'ducts' or 'conductors,' which supplied either different volumes or different intensities. In this machine, therefore, there are no 'two electricities,' nor can either of the conductors be called a 'prime conductor,' since both are equal in every respect.

But it is otherwise with machines of different constructions; as in machines, for instance, where one of the conductors gets its excitation direct from a revolving glass disk or cylinder, whilst the other has interposed, between itself and the glass, leather or silk coated with amalgam; in which case the currents are unequal *because of the interposed resistance*, or the different degrees of excitation which the two conductors respectively receive.

Fig. 9 represents such a machine, the conductor *B* being in connexion with the rubber and amalgam, giving off 'negative electricity,' while *A* is supposed to yield a 'positive' current. After the foregoing experiments we shall find no difficulty in accounting for this difference of current, by merely attributing this to difference of degrees, either in volume or intensity. We have not ourselves experimented with this particular form of machine, but derive our information concerning the phenomena it produces from an excellent article in Watts' *Dictionary of Chemistry*[1]. The current, it is there stated, passes *normally* from *A* to *B*. Considering that amalgam and silk are interposed between *B* and the revolving cylinder, we might have inferred this from the previous experiments.

But it is only *normally*, we are told, that the 'current' passes from *A* to *B*; that is, when *B* is connected with the earth, whilst *A* is insulated. But, if *B* is insulated and *A* connected with the earth, then 'the current passes from *B* to *A*'—clearly from the higher to the lower excited body. 'Connexion with the earth' means, of course, the diffusion of excitation over a larger mass; and, having seen in the previous experiments that a slightly larger quantity of copper on one side than on the other is sufficient to diminish the intensity of the current on that side, we shall find no difficulty in accounting for this 'reversal of currents' without the aid of a double-fluid theory.

The current explanation is that of two electricities, different in kind; whereas our contention is that there is merely difference of degrees. For the sake of greater clearness we shall here put the two explanations side by side, so as to enable the reader to compare each with the phenomena and to decide for himself which of the two explanations is more in accordance with observed facts. To guard against possible misrepresentations of current theories, we shall state these in the words of the carefully written article above referred to.

Old Theory.	*New Theory.*
'On turning the cylinder, the glass acquires positive electricity, the cushion and the brass conductor attached to it negative electricity, and the positive charge of the glass is transferred to the prime conductor.'	On turning the cylinder, both the glass and cushion are excited, but the former more than the latter. From the excited glass both conductors are excited, but *B* less than *A*, on account of the interposed resistance in the shape of the rubber.

[1] New edition, London, 1883, vol. ii, p. 379.

Old Theory.	*New Theory.*
'If both conductors are insulated, the charge on each of them soon reaches its maximum; that is to say, the transfer of $+E$ to the glass, and of $-E$ to the cushion, soon becomes so great as the machine is capable of effecting.'	If both conductors are insulated, both will soon reach their maximum charge, a state at which diffusion (equalization with the air) is equal to rate of generation; hence the state of excitation will remain constant, and both will be excited to the same degree. Equally true is it that the conductor A will reach its maximum of excitation before B.
'But if the negative conductor is connected with the ground by a brass chain or wire, the $-E$ developed on the cushion is at once carried away, and an equal quantity of $+E$ is transferred to the glass cylinder, and thence to the prime conductor, which will then give off its positive charge in long bright sparks to any conducting body brought near it.'	But if the negative conductor is connected with the earth its excitation is thereby lowered much below that of the positive conductor, &c.
'If, on the other hand, the positive conductor be connected with the ground, and the negative conductor insulated, the $+E$ is continuously carried away to the earth, and an equal quantity of $-E$ is transferred from the prime conductor to the glass cylinder, and thence to the negative conductor, which then acquires a high negative charge, and will also give bright sparks to any conductor presented to it.'	But if the positive conductor be connected with the earth, and the negative conductor be insulated, the reverse process will take place; the excitation of the former would be transferred to the earth, so that what was formerly the lesser excited will now be in a higher state of excitation, resulting in a reversal of currents.

From the above it will be seen that the current assumption is of two kinds of 'electricities'; but, though the two-fluid theory has its rival in what is called the single-fluid theory, it is still the dominant conception. Assuming that two 'electricities' are generated by the machine, and that the 'electric' spark is due to the union of the two 'electricities,' there are two difficulties which present themselves to our minds. Firstly, the union of two 'electricities' would suggest their affinity for each other—their tendency to combine. But, if so, then why, being generated together, should they separate first in order to recombine? We are told that the positive 'electricity' is driven in one direction and the negative in another, for no other purpose, as it would seem, than to rush against each other and to reunite.

The second difficulty is why the 'positive electricity' should in one case rush to one side, and in the other case to the other side. If the separation of the two 'electricities' be a fact, then such separation must be due to some difference of the two bodies which could single them out. But why in one case the 'positive electricity' should rush into the glass, and in the other into the cushion, simply because one or the other of the 'conductors' is connected with the earth, is not apparent. But, on the other hand, if we regard that part of the machine connected with the earth as consisting of the conductor *and* earth, and the other side, which is insulated, as consisting of the metallic conductor only, then we should have on the latter side a much smaller mass to electrify than on the other, and hence this part would be in a much higher state of excitation. The process may be well illustrated by the flow of water. Let water flow from some common source into two tanks in equal quantities; and let one tank have a hole where the water can rush out almost as fast as the inflow. In that case one of the tanks would fill faster than the other; and, if communication be established between these two tanks, there would be a flow of water from the 'insulated' tank to the one 'connected' with the earth; and, if some mechanical arrangement were interposed, this 'current' could be utilized for doing work. By stopping up the hole of one tank and making a corresponding hole in the other, we should also reverse the 'current.' In the case of an electrical machine both conductors may be said to receive excitation from a common source; but, whilst one of the conductors is connected with the earth, the excitation on that side will be passed on (diffused) almost as quickly as received; and hence the other conductor will be constantly in a higher state of excitation (positive) and capable of exciting the former.

But even stronger evidence may be adduced to establish our three-fold contention, viz.—

1. That 'electricity' is due to the equalization between two bodies in different degrees of excitation;

2. That the excitation of such bodies differs in degree only and not in kind (i.e. that there are not two 'electricities');

3. That the 'electric' state is one of relativity only, and that 'electricity' as an entity, or a distinct and independent 'form of energy,' has no existence.

The first point is proved by the fact that, in every case where electrical phenomena are produced, this *difference of excitation* is demonstrable. To the proofs already given we may add that, if *dissimilar* substances are rubbed against each other, both are electrified and can react *with each other* electrically.

1. Connect the two bodies to be experimented on with a delicate galvanoscope, and rub them against each other. The effect will, of course, be the same as when dissimilar bodies are exposed to the influence of heating. This will prove their reaction with each other.

2. Take two similar bodies, and proceed in like manner, when there will be no deflection. But, if the two rubbed pieces are separately brought near a gold-leaf electroscope, both will cause a diversion of the leaves; showing that both bodies had been excited, but, being excited equally, cannot react with each other.

3. Rub a body, say wood, first against glass and then against rubber, as above. In each case, if both bodies are connected and a galvanoscope be inserted, there will be a deflection of the needle, but in one case the wood will be *negative* and in the other *positive*—according as it is more or less excited than the body rubbed against. These experiments prove conclusively the above three points. Two dissimilar bodies rubbed against each other are not on that account to be considered as having been subjected to the same conditions. In the one case a *soft* body is rubbed against a hard one; in the other case a *hard* body is rubbed against a soft one. For hard and soft we may substitute greater or lesser resistance. Whilst, therefore, two bodies thus rubbed against each other are both being excited, their respective degrees of excitation would be different; and hence the resultant phenomena of electricity. This will also explain why the same body may be electrified 'vitreously' or 'resinously' according as it is rubbed against a body of greater or lesser resistance.

We will quote again from the above essay (*loc. cit.* p. 376):—

'The kind of electricity which any given substance acquires by friction is not always the same, *but varies according to the nature of the substance against which it is rubbed.* Thus woollen cloth becomes vitreously electrified when rubbed against wax or resin, resinously when rubbed against glass; glass itself becomes resinous when rubbed with a cat-skin, vitreous when rubbed with cloth. The following table, taken from De la Rive, gives a list of substances, each of which becomes vitreously electrified when rubbed with either of those which follow it; resinously, when rubbed with either of those which precede it.'

And then follows this table:—

The skin of a cat.	Wood.
Diamond.	Sealing-wax.
Flannel.	Colophony.
Ivory.	Amber.
Rock-crystal.	Sulphur.
Wool [1].	Caoutchouc.
Glass.	Gutta-percha.
Cotton.	Prepared paper.
Linen cloth.	Collodion.
White silk.	Gun-cotton.
The dry hand.	

In connexion herewith we will quote (still from the same article) 'the general result which was deduced by Coulomb from his very numerous and exact experiments on the subject':—

'When the surfaces of two bodies are rubbed together, that whose component parts recede least from each other, or are least disturbed from their natural position of repose, appear, in consequence, more disposed to assume the vitreous electricity; this tendency augments if the surface experiences a transient compression. Reciprocally, that surface whose particles deviate most from their ordinary position, by the violence of the other, or by any cause whatever, is, for that reason, more disposed to take the resinous condition. This tendency increases if the surface undergoes a real dilation. *The stronger this opposition of circumstances, the more energetic is the development of electricity on the two surfaces.* It grows feebler in proportion as their state becomes more similar. *Perfect equality would nullify the phenomena,* provided it could exist.' (The italics are ours.)

The reader is requested to compare these conclusions with our own generalizations, and he will not fail to see how far more in accordance with the facts are the latter, which may be thus summarized:—

1. *If two substances are rubbed against each other, the one of greater resistance will receive the higher excitation, and hence be positive to the other.*

[1] Why wool and flannel should be classed separately is not quite clear, since flannel itself is made of wool. But probably the experimenter has used a 'wool' different from that of which the flannel was made, having probably a greater resistance, unless the texture of the fabric itself is accountable for these different results, which is highly probable.

2. *If two dissimilar bodies be brought into contact with an excited body, they will abstract excitation from the latter inversely proportionally, and become temporarily excited directly proportionally, to their relative resistance.*

3. *Hence the body of lesser resistance will conduct 'electricity' more readily, whilst the body of greater resistance will be 'electrified' (temporarily) in a higher degree.*

The same is true if, instead of being rubbed against each other, two dissimilar bodies are simultaneously acted on by a third body either thermally, chemically, or mechanically.

And now a few words to trace the origin of the two-fluid theory. An excited glass rod will repel another excited glass rod. So, likewise, will an excited resin rod repel another excited resin rod. But excited glass and excited resin will attract each other. Whilst, therefore, resin and resin, and glass and glass, give rise to identical phenomena, resin when reacting with glass gives rise to opposite results, since the two attract each other. In the one case, when like substances are employed, two excited bodies repel each other; but in the other case, when glass and resin, two dissimilar substances, are employed, two excited bodies attract each other. These were the phenomena which required explaining; and a theory had to be found that would explain these seemingly contradictory results. The theory was made to fit the problem. 'Both are electrified, but each has a different kind of electricity.'

The theory having thus been deduced from the phenomena themselves; or, to speak more correctly, having been invented on purpose to fit the phenomena, the latter are subsequently invoked as proof of the theory. It is not true, however, that glass and glass always repel each other; an excited glass rod if brought near another one that has not been excited, suspended horizontally by a thread of unspun silk, will attract the latter. And, as we have seen above, 'glass itself becomes resinous' if rubbed against a body of higher resistance than itself; clearly showing that the manifestation is due, not to difference of kind, but to difference of degrees.

If, when approaching the solution of such problems, Newton's law, that *every reaction is mutual*, were borne in mind, and the explanation of such phenomena were sought in general principles, such theories could never find entrance into the mind, still less meet with universal acceptance.

As another proof of the 'two electricities' the phenomena of the gold-leaf electroscope are appealed to. As a matter of fact, however, these phenomena have nothing to do with the explanation, since the experimenter approached this instrument with a fixed theory in his mind; hence, no matter what may be the manifestations of the electroscope, they are explained in the light of the already accepted theory. If an excited body increases the divergence of the leaves, it is at once concluded that 'the electricity of that body is of the same kind as that in the electroscope'; and, if the divergence is decreased, then it is 'a demonstration that the electricity is of the opposite kind.' A mind thus possessed by a theory is, indeed, no longer a competent observer even, since the facts themselves are likely to be twisted into agreement with the theory. This is shown in the case of the electric machine. For, when it is found that the positive and negative poles may be changed by connecting either the one or the other of the 'conductors' with the earth, the theory of the 'two electricities' at once obtrudes itself, and 'drives the positive electricity one way and the negative the other.'

As a matter of fact the electroscope gives no indications at all of different 'electricities.' When an excited body is brought near it, the leaves either diverge or converge; and these phenomena are equally well explained by saying that, when a body in a higher state of excitation than the electroscope is brought near the latter, 'positive electricity' will be transferred from that body to the electroscope; and, if a body in a lesser state of excitation be brought near it, 'electricity' will pass from the instrument to the body. In the one case the leaves will diverge, and in the other converge. In other words, the divergence of the gold leaves will be proportional to their degrees of excitation. If, therefore, we approach the instrument with a body in a lesser state of excitation, the equalization between it and the body would take place, with the result that the leaves will partly or wholly collapse.

But this is only another possible explanation of these phenomena, though not necessarily a perfectly true one. The gold-leaf electroscope does not consist of metal only; there is the glass jar which takes part in the phenomenon. When we bring an excited body near the instrument—it is not necessary to touch it—the air is locally 'electrified'; and this air 'electrifies' the glass as well as the brass cap. The excited glass rod might be brought nearer to the glass than to the brass cap, so that the former would be nearer to the exciting body than the latter. The result would be, therefore, that both glass and metal would be 'electrified,' but 'electrified' unequally, and hence a reaction would ensue between glass and metal. The two gold leaves, being equally excited, would not react one with the other; but each would react with the glass, and be drawn in opposite directions towards the glass. The gold leaves do not really repel each other (as will be more fully explained in a subsequent chapter), but are attracted by the glass in opposite directions.

Thus viewed, the gold-leaf electroscope, consisting as it does of glass and metal, will be found to be analogous to an electric machine. Indeed, the principle of the instrument is not yet understood, and no attempt has been made to explain it, because it was and is being regarded as a mere passive witness, whose sole office seems to be to bear testimony to an accepted theory.

That neither the phenomena of the electric machine nor those of the gold-leaf electroscope are really good or reliable evidence of the theory which they are supposed to prove is shown by the fact that we are utilizing the identical phenomena to prove our own theory. Agreement, therefore, between theory and facts is not in itself conclusive evidence; least of all when the theory has been deduced from the very phenomena which are offered as confirmatory evidence of such theory. Whilst we are quite prepared, therefore, to admit that the circumstance of our own theory being capable of affording a satisfactory explanation of these phenomena is, therefore, not necessarily conclusive proof of its truth; yet would we point out one important and essential difference between our own theory of 'electric' phenomena and that to which it is opposed. It is this: while the current theory has been suggested by the very phenomena to be explained, ours has been deduced from general principles, derived from altogether different phenomena, and shown to be true in a variety of totally different circumstances. In this case, therefore, every 'electrical' phenomenon which conforms to the deduction is a true *a posteriori* verification. And that the laws of 'electricity' can be expressed in identical terms with the laws which govern the phenomena of heat, hydraulics, pneumatics, &c., showing the identity of causation as well as identity of principle, is, in our humble opinion, the strongest proof of a theory approaching to truth which physical science is capable of producing. The reader will note that in dealing with the various phenomena we do not introduce a distinct set of laws or principles for each separate group of phenomena, as is the case in current physical science, but throughout derive our explanations of the various phenomena from the two chief principles of variable resistance and tendency to equalization.

Conclusions. 'Electricity,' then, regarded as an entity, is as chimerical as its sister or brother, 'heat.' It is an assumed cause of unexplained and ill-understood phenomena. As promised, we have not tried to discover the nature of 'electricity'—since we did not admit the assumed entity—but have paid more attention to the conditions and collocations under which those phenomena occur which are generally attributed to this supposed agency. Again, we have come to the conclusion that 'it' is neither an agency nor a cause, but a *result* of certain conditions; the conditions being unequal states of excitation, and the 'electricity' being merely inferred as present whenever the equalization of two such bodies in unequal states is intercepted by a third body which thereby undergoes some modification. This modification is always the indicator of the supposed 'electric current,' and may consist in increase of temperature, chemical changes, magnetism, or mechanical motion: just as the causes may be either of these. 'Electricity' is never observed, but always inferred.

'Electricity,' therefore, like every other 'form of energy,' so called, is but the manifestation of the process of equalization which takes place between bodies in different states of excitation.

CHAPTER VIII

CONDUCTION

All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phenomena are produced. . . . Every body appears to discharge; but the possession of this capability in a greater *or* smaller *degree in different bodies, makes them better or worse conductors, worse or better insulators; and both* induction *and* conduction *appear to be the same in their principle and action, except that in the latter an effect common to both is raised to the higher degree, whereas in the former it occurs, in the best cases, in only an almost insensible quantity.*—Faraday.

Before entering into an analysis of the phenomena of conduction, we will briefly consider, theoretically, the manner in which equalization of two bodies, in respect of any quality, can take place.

It is obvious that when one end of a metal bar is exposed to a source of heat the whole bar is not heated all at once. And, inasmuch as only part of the bar is exposed to the heating effects of the lamp, the remainder becomes heated only by 'conduction.' Now, what is this 'conduction'? It simply means *the heating of one particle by another.* But it also means the reverse; i.e. the simultaneous cooling of one particle by another. We may conceive our bar to consist of a series of layers or disks; indeed, we could substitute a number of disks, closely packed, for our bar. We heat the first disk by exposing it to the flame of a lamp. It will become hotter than the next disk, and therefore heat the latter, which, in its turn, will at the same time cool the former. To prove this more conclusively, we may heat such a bar to redness, and then place against one of its ends another bar, which, for the better demonstration of our principle, we will make of wax. At the point of contact the red iron will at once be darkened, while the wax will melt; thus showing that the former has been cooled at the same time as the latter has been heated. The reader cannot fail to be reminded here of the 'double currents' in connexion with electrical phenomena, which gave rise to the theory of two fluids. The 'double currents' (if this term be used figuratively only to express the *reciprocity* of the reaction) can be proved, but not the fluids.

The same process must, of course, take place in the solid bar. Each successive layer of metal offers a certain resistance to a change of its thermal state; and, as the preceding hotter layer raises its temperature, so the latter cools the former. The law of reciprocity is as true of intermolecular reactions as it is of distinct bodies: 'the reaction is always mutual, and directed to contrary parts.'

With this principle clearly in our mind, we shall endeavour to follow, mentally—that is *by an entirely imaginative supposition*—the process of equalization between two or more bodies. We here use the term 'body' in the same sense as we have previously defined the term 'molecule': i.e. any mass of matter which in respect of any other mass of matter behaves like one body. It is immaterial, therefore, whether our bodies are small or large, or whether they are close to each other—i.e. forming part of one and the same solid—or further apart. When apart from each other, we have to consider, however, anything that may be intervening. What we shall here say, therefore, is to be understood to apply to two or more contiguous molecules of the same mass, as to two or three contiguous larger bodies separated from each other by *space*, but not by any intervening substance —using the term 'contiguous' in the sense as employed by Faraday[1].

(The reader is not likely, we hope, to fall into the error of supposing that the *alternate* cooling and heating, as set forth in the following illustrative suppositions, is meant to represent an actual process. We have adopted this analytic method merely because, though not strictly true, it enables us to explain more clearly the *principle* of the actual process.)

[1] 'I mean by contiguous particles those which are next to each other, not that there is no space between them.'—*Faraday.*

Let there be three bodies, A, B, and C, represented by the three cubes in the following diagram (Fig. 10); and let A be in a higher state of excitation than either B or C. In that case

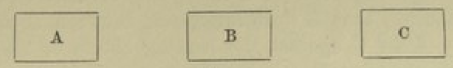

FIG. 10.

A will tend to equalize itself with both B and C; but, B being interposed between A and C, and B and C being in relative equilibrium, C would not be modified by A, nor A by C; B shielding, so to speak, A and C against their mutual modifying tendencies. Before C could be affected by the higher state of A, the latter would first have to overcome the resistance of B.

To simplify the explanation of the problem, we shall assume the higher excitation of A to manifest itself as heat. We will suppose A to have a temperature of 100° C., and B and C to be at zero. We shall also assume throughout this explanation that A, B, and C are the only three bodies concerned in the reaction—a purely hypothetical assumption, but one which is thinkable—and that they are altogether uninfluenced by any outside agencies. In that case the mutual reaction would consist in reciprocal heating and cooling.

Restating, then, our case, A could not heat C until it has heated B over and above the temperature of C; not until B has been made hotter than C, could the latter be heated by the former. Thus, then, at first, C is practically shielded by B from the heating effects of A. But B offers a certain resistance to A, and before any heat from A could reach C this resistance would have to be overcome; or, in plain words, B would have to be heated before C could receive any increase of temperature from A.

It is obvious that, the lesser the resistance of B, the sooner would the heat from A reach C; and, if we removed B—i. e. the intervening resistance—altogether, then A and C would mutually react at once—that is, always providing that no other body or substance were interposed after the removal of B.

In ordinary parlance it would be said that B has conducted the heat from A to C; that is, provided that we wanted to have C heated by A; and the more readily this heating took place—i. e. the lesser the resistance offered—so would B be pronounced as a better or a less good 'conductor' of heat. But, supposing that B were interposed between A and C in order to *shield* C against the heating effects of A, then it would be regarded as a protector or an *insulator*; and, in proportion as it would answer the *intended* purpose, so would it be regarded as a good or bad *insulator* of heat.

From all of which it is clear that 'conductors' and 'insulators' are purely subjective conceptions. B might be pronounced as a very good *insulator* or as an indifferent *conductor*, according as we wanted to heat C or to keep it cool, and according as B performed either office to our satisfaction.

Conduction, then, simply means the reciprocal modification of two bodies through the medium of a third body. But thus viewed the term 'conduction' will at once strike the reader as most inappropriate, since it hinders rather than conducts. Conduction is another anthropomorphic idea, suggesting the idea of conveying or leading, as if without the aid of such a conductor such interaction could not take place at all. But if we consider that each 'conductor,' so called, interposed between two bodies, offers resistance, and that resistance means hindrance or obstruction, it will be seen how incompatible are the actual facts with the idea of conduction. We may provide *ducts* between two bodies separated from each other by highly resisting bodies; but such ducts are not the same as conductors or leaders. A duct might be established between two bodies by wholly removing any interposed resistance or obstruction; or by substituting channels of lesser resistance for the more highly resisting matter that may intervene. But this view will at once suggest to the mind a set of ideas altogether different from those suggested by the term 'conduction.' There is a good deal in a name, notwithstanding the utterance of the great bard; for a name is a stamp which gives currency to a thought, and we must be careful, therefore (in philosophy at least), that we are not using a wrong stamp, thereby setting into circulation some false coin.

Let us return to our three cubes. We have assumed A to be of a temperature of 100° and *B* and *C* each to stand at 0°. We will further assume these three cubes to be of the same metal and of equal weight. *B* and *C* are in equilibrium; *B* and *A* are not. Hence a reaction will take place between the two latter bodies, which will continue until both are of the same temperature; this, if there were not a third body present, would be 50°. The diagram in Fig. 11 represents the supposititious states of the three bodies at the end of this reaction; *assuming* here purposely, for the sake of simplicity of explanation, that the temperature of *C* is not changed until equalization between *A* and *B* is complete.

A	*B*	*C*
50° C	50° C	0° C

Fig. 11

At first *B* and *C* were in relative equilibrium, while *B* and *A* were in reaction with each other. In the present diagram *A* and *B* are represented in equilibrium, and *B* and *C* in states of difference. A reaction would in such a case ensue between *B* and *C*, at the end of which both would stand at 25° C.; thus—

A	*B*	*C*
50° C	25° C	25° C

Fig. 12.

We now have once more *B* and *C* in equilibrium, and *B* and *A* in states of difference. Repeating the same process of equalization between *A* and *B*, at the end of the reaction we should have

A	*B*	*C*
37·5° C	37·5° C	25° C

Fig. 13.

Representing the process of equalization in this manner, we should get in succession—

	A	*B*	*C*
At commencement .	100	0	0
After 1st stage . .	50	50	0
" 2nd " . .	50	25	25
" 3rd " . .	37·5	37·5	25
" 4th " . .	37·5	31·25	31·25
" 5th " . .	34·375	34·375	31·25
" 6th " . .	34·375	32·8125	32·8125
" 7th " . .	33·59375	33·59375	32·8125
" 8th " . .	33·59375	33·203125	33·203125
" 9th " . .	33·3984375	33·3984375	33·203125
" 10th " . .	33·3984375	33·30078125	33·30078125

and so on until equalization is established throughout the series.

It is here assumed, of course, that the three bodies are alone concerned in the reaction, uninfluenced by external conditions. It is further to be remarked that this reaction does not alternate in the manner here represented. *C*, for instance, does not remain unaffected until equalization between *A* and *B* has been completed, but begins to react with the latter as soon as the resistance of *B* has been overcome and its state is different from that of *C*. In other words, as soon as the resistance of the middle body is overcome, it at once begins to react with *C*, and henceforth is in uninterrupted reaction with both *A* and *C*; and all three bodies are concurrently in incessant interaction until all three have reached relative states of equilibrium.

But there is one thing in the above table which is sure to strike the reader; viz. that, as equalization proceeds, the *difference of states*, to which the reactions are due, is gradually diminishing, approaching more and more towards zero. And, as the intensity of a reaction depends, *caeteris paribus*, on the degree of difference, this intensity of the reaction must diminish proportionally; and this is true universally. It is true of electric batteries; it is true of two heads of water; it is true of compressed air expanding; it is true, even, of the rise of the mercurial thread of a thermometer. If a thermometer be dipped into boiling water, the mercury rises with a sudden rush, but its velocity gradually and visibly diminishes, and the last few degrees it creeps along quite slowly. It is true also of the swingings of a pendulum. If a pendulum be moved from the vertical through an angle of say 45°, then the successive arcs it will describe will become less, but the *rate of decrease* will also diminish, approaching more and more towards zero.

These comparisons of different phenomena are of great interest, inasmuch as they show that, however different the manifestations, they all obey the same laws, and the quantitative or mathematical expression of one would serve alike for all. This, more perhaps than anything else, seems to our mind to establish identity of causation, and to show that reactions—i. e. phenomena—are due to difference of states, and not to distinct 'forces.' There are seeming exceptions, and many points of importance and supreme interest, connected therewith; but, in order not to break our present argument, we shall deal with these further on.

From the law of gradual decrease in the intensity of reactions as equilibrium is approached we shall be able to deduce explanations of certain phenomena connected with 'electricity,' more especially with 'voltaic electricity.' But first we shall further consider the conducting and insulating qualities of bodies. We shall again make use of our former illustration; but, instead of having only one body intervening between two other bodies, we shall interpose a series of such connecting bodies, or 'conductors.'

In the following diagram we have six such bodies, B^1 to B^6, interposed between A and C.

A	B^1	B^2	B^3	B^4	B^5	B^6	C
100° C	0° C	0° C	0° C	0° C	0° C	0° C	0° C

FIG. 14.

The initial temperature of A is again supposed to be 100° C., and that of the rest is supposed to stand at 0° C. It is also assumed that all the bodies throughout are the same in quality and quantity[1]. The process of equalization would commence, of course, between A and B^1, and proceed in the direction of C. For the sake of simplicity of explanation, it will again be convenient to assume that no reaction takes place between B^1 and B^2 until equalization between A and B^1 respectively is complete; and so on throughout the series. By an inspection of the diagram it will be seen that there is a difference of 100° between A and B^1; but at the end of the first reaction, when both A and B^1 stand at 50°, there will be a difference of 50° only between B^1 and B^2. The intensity as well as the extent of the reaction between B^1 and B^2 will, therefore, be greatly diminished as compared with the reaction which took place between B^1 and A. B^2 having equalized itself with B^1, both these would stand at 25° C.; so that when the reaction reaches as far as B^3 it would be feebler still, diminishing as it proceeds from A towards C. And if C were very far from A; that is, if the quantity of matter interposed between C and A were comparatively great—or, what would practically amount to the same thing, of considerable specific resistance—C might be left practically unaffected by the higher state of A. The following table is intended to illustrate this gradual diminution of the reaction:—

	A	B^1	B^2	B^3	B^4	B^5	B^6	C
At commencement	100	0	0	0	0	0	0	0
After 1st stage . .	50	50	0	0	0	0	0	0
,, 2nd ,, . .	50	25	25	0	0	0	0	0
,, 3rd ,, . .	50	25	12·5	12·5	0	0	0	0
,, 4th ,, . .	50	25	12·5	6·25	6·25	0	0	0
,, 5th ,, . .	50	25	12·5	6·25	3·125	3·125	0	0
,, 6th ,, . .	50	25	12·5	6·25	3·125	1·5625	1·5625	0

We may assume, of course, the connecting bodies B^1 to B^6 to be of a much lesser mass than either A or C. The principle

[1] Whenever we speak of quantity or *mass*, we simply mean equal, double, treble, &c., quantities of the *same* substance. Thus, two pounds of iron are double the quantity or mass of one pound of iron. Our statement in an earlier part of this work, that we cannot know what constitutes equality of mass, only applies when different substances are compared with each other; and in all such cases we shall always speak of *equivalent* weight, quantity, or mass.

of the gradual diminution of the reaction proportionally to the total resistance of the intervening matter, as here illustrated, would still hold good. Only in that case, the resistance of the connecting bodies (we purposely abstain from using the word 'conductors') being so much less, the change effected in *C* by *A* and in *A* by *C* would be proportionately greater.

The point of interest in the above table is in the fact that by the time the reaction reaches *C* the difference has sunk from 100° and 0° to about 1·5° and 0°. Now we must always remember that every reaction is mutual; that one body can heat another body just in proportion as it is itself being cooled by the latter. Hence the reaction between any two adjacent bodies will always be proportional to their difference of states: which in case of thermal reactions would be proportional to difference of temperature. Glancing at the last line of the above table, it will be seen that this difference of temperature between any two of the adjacent bodies is considerably less than it was between *A* and B^1 at the start, and less between any two adjacent bodies as we move from the left to the right in the last line. The reaction, therefore, is a gradually diminishing one. *C* is still at 0° C.; but the body B^6, to which it is contiguous, is only about 1·5° C. warmer than itself; and, at the end of the reaction with *C*, B^6 will be but very little colder than B^5. The decrease of temperature in B^5 would be still less; so that the reciprocal heating and cooling, as it proceeds from *A* to *C*, and from *C* back to *A*, would be gradually diminishing.

By the aid of the principle illustrated in the above diagram we shall now be able to show that all the phenomena of electricity are due to the operations of the principle of resistance, or the law of persistence. This will also enable us to show that many of the current explanations of these phenomena are mere empirical generalizations, which do not correctly represent what is taking place; furthermore, that the conceptions based on such explanations are wrong, and consequently useless and misleading when used for the purpose of further deductions.

1. It is currently believed that, when the circuit in batteries is broken, no 'electricity' is evolved.

This, however, is not true; the reaction is thereby only greatly diminished, the ratio rapidly approaching to zero, but still going on at this greatly and increasingly reduced rate.

2. It will be shown that 'electrification by induction' in no wise differs from 'electrification by conduction,' but that the supposed difference is again merely subjective, due to a want of understanding of what is actually taking place. Nor is contact necessary in order to convey 'electricity' from one body to another at a distance, since the supposed 'conductors' are merely *ducts* of lesser resistance substituted for bodies (generally the air) of greater resistance. And, inasmuch as each 'conductor,' so called, possesses some resistance, it follows that the removal of all resistance, hence the absence of any intervening body whatever, would be the most efficient 'duct' that could be provided.

3. Arguments will be adduced to show that the resistance of thin wires is less than the resistance of thicker wires of the same material, which is the opposite of what is currently believed to be the case.

It should be pointed out, perhaps, that in these statements we do not intend to dispute or cast doubt on any of the observed facts of electrical phenomena, and that our opposition is directed solely against the current explanations of these facts—i. e. the theories concerning them. It is true enough that to obtain a maximum E.M.F. it is necessary to connect the source of electricity by wires with the machine to be driven, and to complete the circuit. It is equally true that thinner wires will, under like conditions, produce a current of greater intensity[1] than thicker wires. It is also true that, in order to obtain a higher 'potential' from the same source, thinner wires are necessary. But, notwithstanding all this, the statements of the various laws relating thereto are based on misconceptions from beginning to end.

First, let us consider why the intensity along thin wires is greater than along thicker wires. From the foregoing diagram the answer must be obvious. In Fig. 14 B^1 and A were supposed to be of equal mass. Hence, after equalization between A and B^1, the initial difference between these two having been equal to 100, both will stand at 50. But, if we supposed B^1, B^2, &c., to be each the one-hundredth part of A

[1] We use the term 'intensity' in analogy to pressure in fluids; a term used in this sense in most technical works on electricity.

in respect of mass, then, after equalization between A and B^1, the latter would have gained ninety-nine units and the former would have lost one unit only. In such case B^2 would be attacked by B^1, proportionally to a difference of 99 as against 50 in the former instance. And, as in that case the six intervening bodies, B^1 to B^6, would in all represent 0·06 part of the quantity of matter contained in A, after their complete equalization with A, their temperature would be

$$\frac{\text{Temperature}}{\text{Quantity}},$$

which in the former case (by giving the quantity of each cube the value of 100) would be

$$\frac{100}{700},$$

and in the second case

$$\frac{100}{106},$$

where the numerator is always the temperature, and the denominator the quantity of matter. In the first case, therefore, after complete equalization, B^6 would have a temperature of 14·3° C.; and in the second case its temperature would be 94·3° C. From which it is evident that C would be attacked in the second case with a so much greater intensity; not, however, because the resistance of the thinner wires is greater, but because it is lesser, and required less of the heat of A to be neutralized.

It follows from the foregoing that the total resistance of the intervening matter will be proportional to its total quantity. The total resistance of six pounds of copper, for instance, would be the same whether the six pounds of copper were only a yard in length, with a corresponding diameter, or a mile long and correspondingly thinner. The total E. M. F. would in each case be the same, even though intensity and 'quantity' (or voltage and ampères) may differ. This difference in intensity may again be well illustrated by heads of water. Leaving out of consideration the retarding effects of friction on water flowing in pipes, which perhaps may not apply in the case of 'electricity,' we may obtain from the same *initial* head of water large volumes with a quickly diminishing velocity, or small volumes of a higher and more slowly decreasing velocity. But the total dynamic effect at the end of the equalization (supposing the flow to be from one tank to another until equilibrium of level be established) would practically be the same.

The connecting wires are merely the ducts along which equalization takes place between two bodies in different states of excitation. Thin wires have merely a *lesser capacity* than thick wires, which is not the same as greater resistance. Resistance is a primal quality, and is the same for the same substance, increasing and diminishing in direct proportion as the quantity; whilst capacity is incidental, and will be directly proportional to sectional area.

While on this point we may allude to another 'electric' contrivance, technically known as the transformer, by which currents of higher potentials may be converted into currents of lower potentials, or vice versa; and we shall again see identity of principle in dissimilar phenomena. Such a transformer consists of a mass of iron which is excited from some 'electric' source, and the excitation is thence conveyed to the point where it is to be utilized. In the form which Faraday first gave it, it consisted of a thick iron ring round which are wound two helices of copper wire, as shown in Fig. 15.

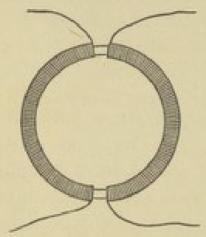

FIG. 15.

The two ends of the one helix are connected with the 'electric' source, and the two ends of the other with the lamp to be lighted or the machine to be driven, as the case may be. If both helices are of the same thickness, the intensity of both currents will remain the same; but, if the wire from the 'electric' generator be thinner than the wire which conveys the 'electricity' to the lamp, the *intensity* will be decreased and the *quantity* increased. But, if thicker, then the quantity will be decreased and the intensity increased. It is by this means that high-tension electricity is in practice converted into low, and low into high-tension electricity[1].

[1] Our authority for these statements, as well as for the use of the terms employed, is chiefly John W. Urquhart's *Electric Light, its Production and Use.*

Hydraulics and pneumatics afford exactly analogous phenomena; and had we uniform terms whereby to designate the *principles* of these manifestations, instead of having a distinctive nomenclature for each group of phenomena, the identical expression would hold good in each and every case, and we could then always explain the invisible or insensible by analogous phenomena where the process is accessible to the senses.

These transformers, so called, clearly show that 'electrification' is merely a process of equalization between two bodies in different states. For in such a case the transformer merely plays the part of the cube *B* interposed between *A* and *C* in our former illustration. We may represent it diagrammatically, as in Fig. 16, where *A* is connected with *B*

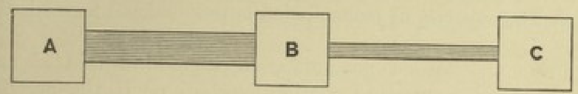

Fig. 16.

by a larger duct than *B* is with *C*. We may conceive *A* to be a tank filled with water, and *B* and *C* empty at the beginning; or we may conceive *A* to be hot, and *B* and *C* cold; or, again, we may conceive *A*, *B*, and *C* to be three bodies of equal weight standing one above the other, *A* being at the bottom or at the top; or we may conceive *A* to be in a higher 'electric' state: the principle of the conversion of high to low pressure, or low to high pressure, would always be the same.

But from the above we can derive proof in support of another contention of ours, viz. that 'electricity' is due to the equalization of a body in a higher state of excitation with one in a lower state. Hence it should be possible to charge our transformer, and utilize this excitation at some other time and some other place. That is, we should be able to excite a mass of matter, disconnect it from the source of 'electricity,' and make the power available at will at some future time, by connecting it with another body in a lower state of excitation, and interpose in the circuit either a lamp or a machine, as the case may be.

That this may actually be done, and is being done, is well known; for storage batteries, as well as Leyden jars, may be considered as converters as well as storers of electricity.

Thus at every turn do we find the real nature of phenomena veiled behind words and subjective conceptions. The sphinx has made her problems insolvable by couching them in misleading terms. She asks, 'Tell me, what is electricity?' instead of, To what conditions are due those phenomena which you call electrical?

The argument concerning the supposed greater resistance of thin wires may be disposed of by summarizing the results of our discussion and experiments in the following laws:—

1. The *intensity* of a reaction is proportional to difference of states.

2. The *resistance* of any connecting body is for the same material proportional to its quantity.

3. The *capacity* of transmission is proportional to sectional area.

From the second generalization it follows that if the quantity of connecting matter be less, or if the specific resistance of the material used be less, a larger quantity of excitation would reach the body to be excited, and conversely; which has been experimentally demonstrated in the preceding chapter.

There is one point above enumerated that still requires explaining. It is commonly believed that, when the circuit of a battery is broken, the 'electric current' ceases altogether. This is not true, however; batteries do not retain their power indefinitely even when the circuit is broken. The same is true of Leyden jars, which, though they may be kept in a charged condition for a considerable period, lose their charge in time, though very slowly.

We may deduce an explanation for this from the table on page 220. By a reference to the last line of this table it will be seen that the difference between each adjacent pair in the direction A to C is less and less, and the reaction will be proportionately feebler. Now, according to the law of reciprocity, *every reaction is mutual and directed to contrary parts.* Considering the reaction in the above illustration to be one between A and C, the intervening bodies being regarded merely as the resistance to be overcome, it is evident that A can be modified by C in proportion only as C is modified by A. A double reaction must take place; there must be a counter-current of 'negative electricity'—as it is commonly phrased—from C to A. But on looking again at the table it will be seen that the return flow would be infinitely less. For the difference between C and B^6 is a mere fraction; and after equalization this fraction would be still less when transmitted from B^6 to B^5, and the 'negative' or return current would be almost infinitesimal by the time it had reached A. Returning from A towards C, the fractions would again diminish in geometric progression, and thus rapidly approach towards zero, yet without entirely reaching it, until equalization was complete. In other words, the process itself is not arrested, but merely the ratio of the process is greatly diminished; and hence the time of equalization is spread over a correspondingly longer period.

Let us suppose now a second wire to connect C and A; then, if it were possible to conceive both wires to be of exactly the same length, thickness, and specific resistance, and at the beginning in exactly the same state—i.e. in equalization with C at the point at which contact is made with A —the same process just described would take place in both wires. The reaction would proceed at a double pace, but still approaching towards zero, and being very slow and gradually diminishing. But, since such perfect equality is hardly ever attainable, a double current would be set up. C would gain preponderance over A along one set of wires, and A would gain preponderance over C along the other. So that 'negative' electricity would 'flow' along one wire from C towards A, and 'positive' from A towards C along the other wire, simultaneously; whereby the reaction would take place infinitely more rapidly, because thereby the difference of state between any two adjacent particles would be constantly at its maximum.

An instructive illustration of this principle is afforded when a narrow-necked bottle filled with water is held neck downwards. The weight of the water being much greater than that of the air, the former has a tendency to fall down. But here, too, the reaction is mutual. The falling water in displacing air must itself be displaced by air in proportion, i.e. volume for volume. This takes place freely when we pour water, or when it falls freely in air, as the rain drops. But in the case of a bottle turned upside down the walls of the vessel act as effectual insulators against the entrance of the air; and hence water can trickle down one side of the narrow neck only in proportion as the air can force its way upwards on the other side. The result is that the emptying of the bottle will proceed very slowly, and more slowly in proportion as the neck is narrower. But if we establish a second communication between air and bottle the reaction will at once be hastened.

CHAPTER IX

INDUCTION

At present I believe ordinary induction in all cases to be an action of contiguous particles consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances; and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces.—FARADAY.

FROM what we have learned of 'conduction' it will be apparent that, before an intermediary body ('conductor') can 'conduct' electricity from one body to another, it must itself be 'electrified.' Good 'conductors' are easily 'electrified,' and as easily 'de-electrified'; but bad 'conductors'—i. e. bodies of high resistance—will be more difficult to 'electrify,' and correspondingly difficult to 'de-electrify.' 'Conductors' and 'electrics,' then, differ from each other in respect of degree of resistance, and in nothing else. This, of course, is well known, but had to be mentioned because of the importance of this fact in connexion with what is called 'electricity by induction,' and the still more important conclusions to which the recognition of this fact will lead in the next chapter.

'Conduction,' then, may be conceived of as the successive 'electrification' of a series of bodies. That these bodies form a continuous solid—a bar or a length of wire—need not interfere with this conception. Our connecting wire might consist of a number of small metal disks, equal in diameter to the cross section of a wire, and put together in close contact, face to face, so as to form a continuous round bar.

In Fig. 17 let *A* be the source of 'electricity'; *B* a body to be 'electrified'; and *c* the metallic wire, or series of disks, connecting *A* and *B*. Before an 'electric current' from *A* can reach *B* along this column of disks, the resistance of each succeeding disk would have to be overcome. This is demonstrated by the fact that a sensible time elapses before the 'current' from *A* can reach *B*. True that this time is ordinarily inappreciable; but, nevertheless, if the distance—or rather the intervening resistance—be increased, the time can actually be measured, and has been so measured. Or, again,

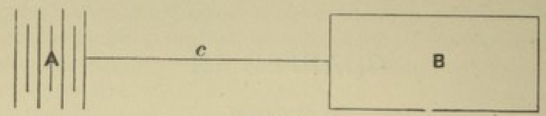

Fig. 17.

the fact that this 'electricity' passes from molecule to molecule may be demonstrated by interposing in some part a disk of very great resistance (or of very great length, resistance coils for instance). Then it could be shown that 'electricity' has proceeded as far as the last of the disks on the *A* side of *x* (Fig. 18), but not to the next disk on the other side.

Suppose now we had a piece of glass interposed as above, and then made metallic contact between the two halves of the conductor, as shown in Fig. 18. The reader will see at once what would take place; *B* would be 'elec-

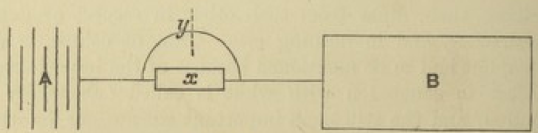

Fig. 18.

trified' along this metallic duct as if the glass had not been present at all.

Or, say that a long glass bar were interposed at *x*, and that a very thin glass disk separated the wire *y* at the place indicated by the dotted line, then a 'current' which might not be strong enough to pass along the long glass bar might be strong enough to overcome the resistance of the thin glass disk, and again the 'current' *would pass along the line of least resistance.*

Say, then, that our whole arrangement of *A* and *B*, together with the metallic connexion, were entirely embedded in glass, as shown in Fig. 19, and that *A* were in a high state of excitation. Of course *A* will tend to equalize—or, in common phraseology, will 'give off electricity'—on all sides. But, the glass offering a so much greater resistance than the metal, the latter would receive more excitation, and that because as soon as it receives excitation it passes the same on to *B*; hence the disk adjacent to *A* will be in a lower state of excitation than the adjacent glass particles, until equalization in the whole system is complete. The whole mass of metal, *A*, *B*, and *c*, when just equalized, will be in a much higher state of excitation than the exterior parts of the glass. Of this we may convince ourselves by now being able to obtain sparks from any part of *A*, *B*, or *c*, by approaching it with another 'conductor,' while we cannot obtain any 'electricity' from the glass.

From the above it is evident that both *A* and *B* may be

FIG. 19.

in perfect contact with each other by means of glass, and yet not be able to modify each other if the resistance of the glass be too great. Equally obvious is it that for glass may be substituted any other substance of equally great resistance, such as resin, india-rubber, or—*air*.

Now the conditions we have been describing, of bodies being embedded in glass, are the conditions under which every 'electrical' experiment has yet been made; only instead of glass the substance in which the bodies experimented on are embedded is *air*—a body or substance with which we are so very familiar that it is little wonder that it should be entirely overlooked: just as we are often apt to ignore a very intimate friend.

We live at the bottom of an ocean, and everything that is and moves on the surface of our earth is as truly embedded in the air as is the fly in the amber. But this air, as is well known, offers great resistance both to 'heat' and 'electricity.'

It is necessary, therefore, to study the 'electrical' properties of this air before we can rightly interpret the experiments made in it. Put in place of glass, wherever mentioned in the above experiments, air, and the reader will not fail to see in what particular 'induction' differs from 'conduction.'

The point in respect of which the two forms of 'electrification' differ from each other is unquestionably in their distinctive names. Before one piece of copper can 'electrify' the next piece of copper, it must itself be 'electrified'; and the same is, of course, true of air, only that the air requires much more 'electrification' than does copper. But there is yet another circumstance to be noted, and that is that when copper is 'electrified' it is embedded in a highly resisting medium—the air; and hence with moderately strong 'currents' all the excitation, or nearly all, would pass along the copper wire. The explanation of this is as follows. Supposing that one of the excited copper disks should momentarily 'electrify' the particles of air next to it; in the next moment the copper disk will equalize with the next one, and through it with the others, whereby it will again be reduced, almost instantly, to a lower state of excitation, so that the adjacent particles of air, if excited at all, would give off this excitation to the copper, which, owing to its quick 'conductivity,' is constantly kept in a low state of excitation. In other words, the copper would always be in a lower state of excitation than the film of air surrounding it.

That the air is an 'electric,' and would behave as an 'electric,' may be inferred from the fact of its being a bad 'conductor.' But this can also be demonstrated experimentally. In charging Leyden jars, for instance, we have noticed currents of air passing *from the jars*, and an electroscope placed at a distance of about one yard was excited. It occurred to us that it was the 'electrified' particles of air striking the electroscope which caused the excitation of the latter. On protecting the instrument against these currents by interposing a sheet of paper, the electroscope was not affected. In further experiments we found that it was necessary to shield the brass only in order to prevent the leaves from separating. As regards the air currents themselves, we have arrived at the conclusion that the particles of air are first attracted by the excited Leyden jar and then fly in an opposite direction, being attracted, or displaced, by other particles of air not 'electrified.' To see this double action of attraction and repulsion it is only necessary to bring a lighted taper near to the brass knob of an electric machine or of a Leyden jar while being charged, and it will be seen that that part of the flame nearest to the knob is attracted by the latter (as shown at *a* in Fig. 20), while the remaining part is blown away from the knob. Of this seeming repulsion we shall speak again. The point of present interest is that air is 'electrified,' and, though a bad 'conductor,' *it may yet act as a carrier of electricity by convection*; and this will explain the principle on which depend the phenomena of induction. The 'electrification' is as truly by contact—or 'conduction'—as when the two bodies are metallically connected. The only difference is that the copper wire is visible while the particles of air are not. The distance at which such 'electrification' can take place through the medium of the air will, of course, depend on the intensity of the current.

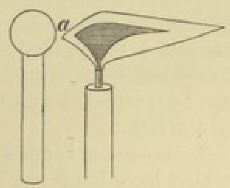

Fig. 20.

What is called 'induction,' therefore, is merely 'electrification' through the medium of the 'electrified' atmosphere. In the 'electric transformer,' for instance, we have a mass of iron which is 'electrified,' and from it the 'electricity' is again conducted away. Instead of iron, we could make a 'transformer' of dry air, provided we could find suitable means whereby to insulate it from the rest of the atmosphere. Every case of 'induction' is really an experimental verification of what is here said. A bath of mercury, in place of a bath of air, is only substituting one substance for another; through either of these 'electricity' can pass, though more readily through the one than the other. But, the difference being merely one of degree, there is no valid reason for calling one and the same thing in the one case 'induction' and in the other 'conduction.'

That the air is an 'electric,' and that every 'electric' is also a 'conductor,' but of greater resistance, is well known. The 'electrification' of the air is proved by innumerable familiar experiments. A classical experiment, which will prove our point more clearly perhaps than any other, is that made by Professor Tesla before the Royal Institution on Feb. 3,

1892. He employed a dynamo capable of producing 20,000 alternations per second, which, by suitable condensers, were multiplied until the alternations reached one million to one and a half million per second. Metal plates were placed on the roof and walls of the room, and connected with the terminals, by which arrangement the whole atmosphere of the room was 'electrified,' as was made perceptible by bringing into the space tubes or globes from which the air had been partially exhausted. 'Such tubes, *though without any metallic connexions*, yet glow and throb as if powerful currents of electricity were being sent through them from an ordinary induction coil.'

This remarkable phenomenon can be explained only on the supposition that the air acted as a 'conductor,' in the sense we have explained; i.e. that the whole atmosphere of the room had been thrown into a high state of excitation.

This experiment proves, only on a larger scale, what a very general experience has already demonstrated on a much more modest scale. For it is well known that 'electrification' across air space is possible, and that the thickness of air that can be traversed by an 'electric current' varies as the strength of the latter.

India-rubber is another 'non-conductor'; but the accidents which have happened in this country and in America, where high-tension 'electricity' is used, have shown that this substance is capable of conducting sufficient 'electricity' to cause instantaneous death both to men and horses [1].

Air, then, is a 'conductor' in the same sense as any other body, though very sluggish in this capacity; and what is called 'electrification by induction' or 'electrification at a distance' is as truly 'electrification by contact,' or 'by conduction,' as when the circuit is made with metal; only that in the former case air takes the place of the metallic connexions.

We have thus disposed of the distinctions between 'positive' and 'negative' 'electricities'; 'conductors' and 'electrics'; and between 'electricity by induction' and 'by conduction.' Furthermore, we believe that we have shown the identity of causation of all 'electrical' phenomena, whether produced by

[1] In New York an overhead wire broke and fell to the ground. A horse touching the india-rubber coating of this wire fell down dead.

friction, by heat, or by chemical action. (Of magnetism we shall speak further on.) And by so doing we have pulled down many of those artificial barriers by which identical phenomena have been divided into distinct compartments, so that we are now enabled to take a better survey of the whole, and to see the unity of principle in all these different manifestations.

We cannot refrain from mentioning here one circumstance which has obtruded itself on our notice during all these investigations; viz. the general tendency of philosophers to find distinctions and to classify as much as possible, whereas the true aim of science ought to be to generalize, and not to classify. In doing the former we naturally look for points of agreement; but, if the mind is bent on classification, it unconsciously seeks for points of difference, which it tends to supply most readily even where they do not exist.

In this, as in the preceding chapter, our object has been to show that the supposed two 'electricities' are identical in kind and causation, differing from each other in degree only, and not to explain the different special manifestations of 'electricity.'

CHAPTER X

ON ACTION AT A DISTANCE

It is, probably, of great importance that our thoughts should be stirred up at this time to a reconsideration of the general nature of physical force, and especially to those forms of it which are concerned in actions at a distance. These are, by the dual powers, connected very intimately with those which occur at insensible distances; and it is to be expected that the progress which physical science has made in latter times will enable us to approach this deep and difficult subject with far more advantage than any possessed by philosophers at former periods. At present we are accustomed to admit action at sensible distances, as of one magnet upon another, or of the sun upon the earth, as if such admission were itself a perfect answer to any inquiry into the nature of the physical means which cause distant bodies to affect each other; and the man who hesitates to admit the sufficiency of the answer, or of the assumption on which it rests, and asks for a more satisfactory account, runs some risk of appearing ridiculous or ignorant before the world of science.—FARADAY.

THE results of the last five chapters may be summarized in the following propositions:—

1. When of two or more bodies one happens to be in a higher state of excitation than the other, equalization takes place: that is, a body in a higher state of excitation reacts with a body in a lower state of excitation, increasing the excitation of the latter, and at the same time reducing its own.

2. This transmission and reception of excitation from body to body is commonly known as 'electricity,' and only manifests itself when some resistance, not sufficient to completely arrest this equalization, is interposed.

3. The 'electrification,' then, of one body by another is simply a process of equalization; and this equalization of two bodies will take place more readily *in proportion as the intervening resistance is less.*

This last proposition, and especially that part which we have italicized, and which has been proved repeatedly in the preceding chapters, leads to an obvious and almost inevitable inference; viz. that the absence of all resistance would be the most favourable condition for 'the transmission of electricity'—or of any other form of excitation—from one body to another. And, as all matter, however great its 'conductivity' may be, offers some resistance, the conclusion seems to us obvious that the absence of all intervening matter would be more favourable to 'conduction' than the best known 'conductor.' In short, this opens up the old dispute as to whether action at a distance is possible or not.

We had no intention of touching upon this highly controversial subject; indeed, we did not know that we should be brought face to face with it. But, as it happened, it lay across our path, and, fortunately, the road from which we are approaching it presents this old problem in an entirely new

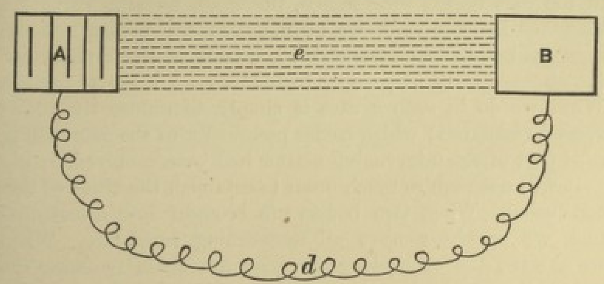

FIG. 21.

light, which promises a final settlement of a perplexing question. The problem is a purely metaphysical one; it is based on no facts, and is supported by no facts, as will presently be shown. Nevertheless the thesis that bodies cannot act at a distance is a stumbling-block to the natural philosopher; a stumbling-block which requires to be cleared away before we can proceed on our journey, and, therefore, the question shall receive here the attention it deserves.

In the preceding chapter it has been shown that the facility with which 'electricity' can pass from one body to another depends entirely and solely on intervening resistance, and that distance has really nothing whatever to do with it. Let *A* (Fig. 21) be a source of 'electricity'; *B* the body to be 'electrified'; *e* a body of high resistance intervening between

A and B; and d a metallic wire joined to A and B respectively. Along the line e, A and B might be only a few inches apart, while d might be many miles long; yet, if the total resistance of the latter were less than that of the air or glass intervening between A and B along the line e, the 'current' from A to B would pass along the much longer wire d.

The accepted explanation of this is that 'contact' is necessary; but this we have already shown to be based on an error suggested by the term itself. The two bodies are in indirect 'contact' with each other already, by being immersed in the same atmosphere[1]; and, to bring this more clearly before the mind, we might, for the air, substitute glass, india-rubber, or resin, in which case the 'contact' or 'connexion' could not be doubted; yet we should have practically the same result as in the case of the air. When, therefore, metallic wires are stretched from A to B, we do not 'connect' bodies hitherto unconnected, but merely *substitute* a body of lesser resistance for another body of greater resistance—i.e. a metal for air. What we do in such a case is simply to reduce the intervening resistance; which comes practically to the same thing as if part of the intervening matter had been removed.

It will not require much proof to establish the truth of this conclusion. When two bodies are brought into direct contact, we simply remove all intervening resistance. When we shorten the connecting wires, we are again reducing the resistance. And when we make metallic connexion between two bodies at a distance we *merely substitute one body for another*, the lesser resisting metal for the more strongly resisting air. For it must be remembered, what we all know

[1] It is necessary to bear in mind distinctly that, when two bodies are joined to each other by a third body, they are as truly *separated* by that body as connected; this depending entirely on the point of view of the observer. Thus two pieces of brass joined by a metal wire for the purpose of facilitating the passage of 'electricity' are said to be placed in 'contact' with each other. But, when a piece of india-rubber is placed between two such metals in order to prevent the passage of 'electricity,' then two such bodies are said to be separated or 'insulated' by such body. Thus the brushes on a dynamo 'connect' or establish 'contact' between the wires of the field-magnet and the armature; while the ebonite between the two halves of the commutator 'separate' the two halves. It will be seen that the distinction is merely a verbal one, and that the resultant effects do not depend on 'contact' or 'separation,' but on difference of resistance. There is 'contact' or 'separation'—according to the point of view taken—in both instances; but the resistance of the two 'separating' or 'connecting' bodies is not the same.

and yet are all apt to forget, that the air itself is a body; and, therefore, that any two bodies in the atmosphere are as truly 'connected' with each other by means of the atmosphere as two bodies in the same water are connected with each other by means of the water, or two pieces of iron set in copper are connected with each other by means of the copper. When it is said, therefore, of two bodies which have been joined by means of a copper wire, that 'connexion has been established between them,' this is but a very loose statement of what has actually taken place. There was 'contact' between the two bodies before, the connecting medium being the air. The copper wire, therefore, has simply displaced portions of the atmosphere—a lesser resisting body having taken the place of one of much greater resistance. The metallic wires may, therefore, be regarded as ducts—a passage through which the excitation may pass—but not as 'conductors,' save in a metaphorical sense. The wires do not *conduct* the 'electricity,' but actually resist it; the resistance, however, is not sufficient to prevent the passage.

In the Leyden jar the glass acts the part of the air between the two metallic surfaces. In this case nobody would question the 'contact,' the glass being a solid, yet it but acts the same part between the inner and outer tinfoil as would a layer of air of a resistance equivalent to that of the glass. A thin wire passing through the glass from metal to metal, or in any other way connecting one surface with the other, would facilitate equalization between the two metals in like manner as when two bodies separated by air are connected by a body of lesser resistance.

So far, then, it does not require any special proof that equalization—or conduction—is facilitated in proportion as resistance is reduced, since the fact is too well known. It only remains to prove that the absence of any intervening body would be still better than the best known 'conductor,' the bodies being at a distance. To this end we should have to remove the intervening atmosphere. There is, however, no need for us to devise special experiments to prove this, as the necessary experiments have already been made by the late Professor Tyndall, and since then by others. Interposing between a source of heat and a thermo-pile a tube so arranged as to be capable of being exhausted or filled with different gases[1], he demonstrated, (1) that the more perfect the vacuum the more readily is the thermo-pile affected by the source of heat at the other end of the tube; (2) that when air or gas is admitted in the tube the transmission of heat is less in proportion as the quantity of air or gas admitted is greater; (3) that with different gases at the same density the transmission of heat varies (due, of course, to the difference of resistance of the various gases), and that *with the same gas the 'conductivity' is inversely proportional to the quantity of gas present.*

The same is true of 'electricity,' as may be proved by the following experiment. Take two platinum wires and pass them through a glass vessel in the manner shown in Fig. 22. Connect the two ends of one with a battery, and the two ends of the other with an electroscope. Let the distance of the two wires from each other within the glass vessel be such that at ordinary pressure no 'current' passes from one to the other. Now exhaust the air (by which matter is removed, and 'contact' made less perfect), and a 'current' will pass from one wire to the other. In a second experiment let the wires be near enough for a 'current' to pass from one to the other; and then compress air into the vessel (by which 'contact' would be made more firmly), and the 'current' will cease to flow. Or fuse two platinum wires into the two ends of a glass tube a certain distance apart, so that no spark shall pass from one to the other when connected with a moderate source of 'electricity.' By exhausting this tube, sparks will be obtained, the brilliancy increasing with continued exhaustion. These phenomena of vacuum tubes are too well known to need here any further description.

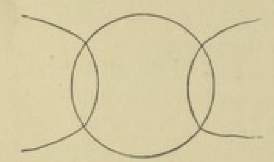

FIG. 22.

From all this it will be clear that the fact that metallic connexions facilitate the passage of 'electricity' from one body to another is due, not to the supposed 'contact,' but to *lesser resistance.* If we substitute a thicker wire for a thinner one, we simply replace a greater proportion of the strongly

[1] An account of these experiments, together with a full description of the apparatus employed, will be found in Tyndall's *Heat a Mode of Motion*, ch. x.

resisting air by the lesser resisting body. And if we bring two bodies into direct contact with each other we only achieve the same thing as would be achieved if we could remove all intervening matter; that is, if we could establish a perfect vacuum between two bodies.

It is not true, therefore, that thin wires offer greater resistance than thick wires; they merely have a lesser capacity. The resistance for the same material must in each case be as the quantity of matter: the resistance of two pounds of copper, for instance, must necessarily be double that of one pound. As already pointed out, thicker wires conduct more freely than thin ones because their capacity is greater.

The points which are proved for certain are:—

1. That distance affects the reaction only in so far as the resistance of the intervening matter is greater.

2. That when the resistance is reduced by substituting a lesser resisting medium for a more strongly resisting medium, by which the two bodies are 'connected' or 'separated' —whichever term the reader may choose—the 'passage of electricity' is facilitated.

3. Where the intervening medium is a gas, the more of it is removed, the more readily does action take place; and, conversely, the more of it is compressed between two bodies, the less readily will 'electricity,' 'light,' or 'heat,' pass from one body to another.

4. With the same 'conductor' the passage of 'electricity' is facilitated as the 'conductor' is shortened—and vice versa—and that without altering the distance of the two bodies.

Each of these indisputable facts shows that distance has absolutely nothing to do with the reaction, but merely resistance; and that, the less there is of the latter, the more readily do the two bodies react with each other. The facts point all one way; but such ill-chosen terms as 'contact,' 'conduction,' and 'connexion,' suggest altogether different ideas. Put in place of 'contact' 'substitution of one body for another'; for 'connexion' 'separation' (e.g. the glass which separates the two metallic surfaces of the Leyden jar; or the air and copper wires which separate the two elements of the voltaic cell); and, for 'conduction,' ducts or channels: and an entirely different theory will at once suggest itself to the mind. But in this chapter we are not concerned merely with 'electricity,' but intend to settle finally a much vexed problem, which has long been the object of discussion; viz. the problem whether action at a distance, generally, is possible or not.

The great stumbling-block in this discussion—as in all others—is again the manner in which the problem has been presented, together with the mental concepts of the philosopher. We need not stop here to analyze the views which have been put forth by different philosophers who have paid attention to this subject, nor to point out the errors which befogged their mental vision. The reader will find an excellent summary of these discussions in a little work, *Das Räthsel von der Schwerkraft*, by Dr. C. Isenkrahe (Brunswick, 1879). We shall here merely quote a passage from a letter of Sir Isaac Newton to Dr. Bentley, in order to show why such luminaries of the human intellect were unable to see what to us seems as plain as the noon-day sun; and we trust to be able to show the reader that it was again the *ignis fatuus* of suggestive words and false concepts by which these great intellects were decoyed from the path of philosophy into the quagmire of metaphysical word-quibblings.

'It is inconceivable,' wrote Newton[1], 'that *inanimate brute* matter (*sic!*) should, without the mediation of something else, which is not material, operate upon and affect other matter *without mutual contact*. . . That gravitation 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 in philosophical matters has a competent faculty of thinking, can ever fall into it.' (The italics are ours.)

This passage was never dictated by Newton the observer, the philosopher, who was 'searching on the strand of the ocean of truth,' but by Newton the man, or Newton the metaphysician, in whom his individual observations were for the time overshadowed by the ideas he had inherited from his predecessors. In the above passage we have the idea of brute and inanimate matter, which requires 'something which is not material' before it can act. But it was Newton himself who told us that 'to every action there is always opposed an equal reaction'; that, in fact, it is a question not of one body *acting on*, but of one body *reacting with*, another body. Hence

[1] Quoted from Mill's *System of Logic*, Book v, ch. iii, § 3.

the question should not be, whether a body can *act on* another body at a distance, but whether it can *react with* another body without being in actual contact with the latter.

When a body falls to the ground, we say—and say so after Newton, be it remembered—that it is attracted towards some point within this globe. The falling body, therefore, does not *act on* that point or object towards which it is drawn, but is merely drawn towards, and in its turn is drawing also towards itself, that part of the globe with which it is in mutual reaction. The question now is, Can these two bodies attract each other without something between them? To this question observation alone can give an answer. When a body is allowed to fall in air, it does so with a certain velocity. If, for the air, we substitute another medium, say that of water, the velocity is lessened; indeed, if the bodies be lighter than water, their fall may be entirely prevented by this medium. On the other hand, if bodies be allowed to fall in vacuo, they fall more readily. These facts do not seem to point to 'contact,' but would rather point to an opposite conclusion.

Again, when a body falls and in its fall strikes against another body which arrests its progress, the latter, undoubtedly, *is* acted on by the former; and that kind of action certainly can only take place by contact. But then let us remember that the falling body was not in reaction with the body it struck against—was not attracted by it; and the falling of the former was not due to any attraction between it and the latter. The body struck against was merely in the path of the body falling; and but for this fact the former would not have had any share in the reaction at all.

Let us remember, therefore, the distinction we have drawn in a former chapter between primary and secondary, or kinematic and dynamic, reactions. At present we are concerned with the former, with reactions which take place between two bodies drawn towards each other, and not with reactions between a moving and an obstructing body.

It is not, therefore, on account of any observed facts that Sir Isaac Newton found it inconceivable that bodies should be able to affect each other at a distance; but, as he himself unconsciously admitted, because for the moment his own discoveries were overshadowed by the inherited idea that matter was 'brute' and 'inanimate.'

But the idea of bodies being capable of acting at a distance was reduced to a perfect absurdity by a most ingenuous question in which the whole problem has been put. 'Can a body act where it is not?' asked the sphinx; and this question also suggests the answer. It is obviously a question suggested by the mind, which had previously worked out the answer. Indeed, it may safely be averred that the question had never been formulated until the answer had been worked out, and was so formulated as to admit of none other than the previously accepted conclusion. And whence this conclusion? Not from the facts, but from ideas. As Mill so pertinently observes,

'Our general ideas contain nothing but what has been put into them, either by our passive experience, or by our active habits of thought; and the metaphysicians in all ages, who have attempted to construct the laws of the universe by reasoning from our supposed necessities of thought, have always proceeded, and only could proceed, by laboriously finding in their own minds what they themselves had formerly put there, and evolving from their ideas of things what they had first involved in those ideas. In this way all deeply-rooted opinions and feelings are enabled to create apparent demonstrations of their truth and reasonableness, as it were out of their own substance.'

And such must have been the origin of the question, Can a body act where it is not?

It is a question suggested by words and ideas, and not a problem propounded by observed facts. All problems were solved by metaphysicians by mere naming. A name was given to every phenomenon, and straightway this *name* was endowed with body or spirit—was made into an entity. The tendency of a body to fall was called its 'gravity,' and thus 'gravity' became an entity which was supposed to account for the phenomenon. Henceforth it was no longer a question as to why bodies fell to the ground: that was already explained; they simply obeyed 'the law of gravitation.' All that remained requiring explanation was to determine the 'nature of gravity,' whether it was 'material' or 'immaterial,' whether 'inherent' or 'external,' and whether it could do what hitherto no mortal could accomplish, i. e. *act where it is not.*

Of course the facts are loud in their answer that action at a distance actually does take place. But then, it would seem, that only made it necessary so to explain the facts as to harmonize them with accepted conceptions. They had to be mutilated, amended, so that they might be squeezed into the Procrustean bed of anthropomorphic conceptions. Hence the abundant crop of 'ethers' and 'mundane particles,' which, having neither body, parts, nor spirits, are endowed with properties which are denied to visible matter. And thus, by following the *ignis fatuus* of mental concepts, the absurdest of all absurdities, the 'mundane particles' of Le Sage have actually received serious attention, and have been discussed by philosophers who have long since learned to sneer at the crude anthropomorphic notions of the ancients that the earth is supported by an elephant, or rests on the shoulders of the mighty Atlas.

And why? Because it is inconceivable that the earth, this mass of 'brute' and 'inanimate' matter, should know when, where, and how to move. But it was not considered inconceivable, or even improbable, that the particles created by M. Le Sage should possess sufficient 'intelligence' to know exactly when, where, and how to strike against the earth, in order to keep the latter in her proper place.

In all such discussions, if properly analyzed, the philosopher cannot fail to discover that he has drifted into the hopeless quagmire of metaphysical word-quibblings. The supposed necessities of thought lie in the ideas suggested by the words in which the problems have been expressed. Words will at first be suggested by the phenomena observed; and the words which are called up by any newly observed phenomena will be in accordance with those ideas of the observer to which they bear the closest resemblance. A tuning-fork would never have been called a tuning-*fork* by a blind man who may have heard the sound, but who knew nothing of the shape of the instrument. The sound might possibly recall to his mind the sound of a bell or a flute, as the case may be; whilst a savage, knowing nothing of either music or a fork, might in all probability characterize it as 'a two-legged speaking stone.' But, once things have been named in accordance with the psychological law that 'new sensations are compared with prior similar sensations, and comprehended in accordance therewith'—a law to which the mind of a philosopher and the mind of an ignorant savage are equally subject—it is henceforth the words which suggest the ideas, and the facts are, for the most part, entirely ignored.

'Inherent'! 'act on'! 'being acted on'! 'emission'! 'conduction'! How fertile in suggestion are not all these words! But, then, things do not 'act on,' but are *reacting with*, each other. The reciprocity and relativity of things are entirely obliterated by the suggestions of those terms in which the observed facts have been committed to memory. And yet the evidence in support of this relativity and reciprocity is as plain, as universal, and as conclusive as anything can be, if we can but divest ourselves of the distorting spectacles of a misleading terminology, which suggest to the mind, by connotation and association of ideas, theories for which there is absolutely no foundation in fact.

Why still *inherent*, after it has been discovered that 'every reaction is mutual and directed to contrary parts'? But this law of reciprocity and relativity was one of those 'polished pebbles' which the great seer picked up 'on the strand of the ocean of truth,' and which his followers have cherished ever since as a pretty find to be delighted with, but as without any further utility. It is mentioned in every text-book, but only to be disregarded again. Though recognized as a fact, it was not powerful enough to prevail against the spell of inherited ideas that have so deeply impressed themselves on the human mind in the infancy of the race, and have perpetuated their rule from generation to generation. When the first railway engine ran along the metals in Germany, and one of the more intelligent onlookers turned triumphantly to a sceptical peasant who doubted the possibility of cars running without horses, with the words: 'There now, you see it is possible'; the latter merely shook his head and replied: 'Aber es müssen doch Pferde drinnen sein[1].' And so, notwithstanding the multitudinous observations that bodies do react at a distance, and that intervening bodies, instead of facilitating this reaction, are always a hindrance (*vide* the falling of bodies *in vacua* and *in media*; the transmission of light, heat, and electricity through *vacua* or *media*), and notwithstanding the above-quoted and universally accepted law of reciprocity, we still hear philosophers exclaiming with the scepticism of the German peasant: 'But something must be in it; or, if not in it, then outside it, but very close to it.'

[1] 'But there must be horses inside.'

The necessity is not in the facts, but in the mind. The conclusion of the possibility of such interaction at a distance is inconceivable only because it conflicts with the mental concepts, or because *the words* in which familiar phenomena have been committed to memory suggest different conclusions.

What is it that makes it inconceivable that bodies should react (not *act*, but *re*-act) at a distance? The fact that a body falls to the ground more readily *the less of matter* there is in its way? Or the fact that such a body penetrates and passes through such matter, which—by assumption—might serve as a communicating agency? (This fact alone is sufficient evidence that the intervening matter hinders rather than facilitates the mutual attraction of two bodies.) The phenomena of magnets, which turn their ends towards the distant poles of the earth? Or that light, heat, and electricity cannot pass so well through denser as through thinner atmospheres, or through a vacuum?

None of these, but the conception of *dead* matter and a necessary *agent*—the dualistic idea, begotten of the anthropomorphic view of nature—and abstract axioms suggested by this view, such as 'a body cannot act where it is not'—'dead matter cannot move itself'—'each effect must have had a *cause*' (in the sense of an external active agency); and so forth.

That such views were held and defended by the greatest intellects cannot lend to them the slightest support. Great minds are as liable to be misled as small ones; and in science opinions count as nothing. And it is mere opinion when subjective conceptions, or dialectic arguments, are marshalled against principles deduced from the phenomena themselves.

This will appear all the more forcibly if it be remembered that even the greatest philosopher can reason only by comparing his ideas and concepts with each other; and that many ideas are ours only because they were our fathers', or have been acquired, in the words of Mill, 'by mere habit and inculcation.' To these errors every human mind is liable; and it is no disrespect to the great men of our race if such errors committed by them are subsequently pointed out. The greatest reverence we can pay to the memory of such men is to show ourselves worthy of the inheritance they left us by continuing the labours in which and through which they achieved their greatness. But, whilst revering their memory, and gratefully acknowledging the immense services of their labours, science can recognize neither pope nor dogma.

Because, therefore, Newton found it inconceivable that 'inanimate,' 'brute' matter should be able to act at a distance, and because this view has been held and defended by other luminaries down to our own times, we must not shrink from exposing what facts compel us to regard as a mischievous fallacy. Was it not just as inconceivable that such a *mighty* body (as our earth was once conceived to be, before it had dwindled into comparative insignificance) as the terrestrial globe should float in space without a prop to prevent it 'from falling to the ground'? That, contrary to the testimony of 'our own eyes,' we should be required to believe that this body is journeying round the sun without any one pulling it by a string? But, conceivable or not—yet it is so; and the '*isms*' or '*ities*' which are supposed to be in or behind every particle of matter will have to give way to the logic of facts, and join the elephant, the phlogiston, and the departed 'spirits' of Lord Bacon and his predecessors.

But to the question 'Can a body act where it is not?' we can now give a conclusive answer by showing the error involved in the question itself. We reply, Inasmuch as all action is reciprocal, the question should be 'Can two bodies react with each other when separated by space or by other bodies?' The reader cannot fail to see the essential difference between the two queries. To the former he can reply at once, without rising from his easy chair. In that form the question is an appeal to his 'mind,' to his 'common sense'—or, more correctly, to his prejudices—and the 'mind' responds promptly with an emphatic 'No! The substance of the question is inconceivable and absurd.'

But the other question is an interrogation concerning matters of fact; and before the philosopher could give an answer he would have to go into his laboratory, or into the larger laboratory of nature, whence he is sure to return with an answer something like the following:—

Since no body can change itself save by interaction with another body, and since that other body is always necessarily outside itself, every reaction between two bodies practically takes place at a distance. If the two bodies are not in close proximity to each other, then I have found that intervening matter tends to prevent this reaction in proportion to the quantity present, when experimenting with the same kind of matter; or, in case of different kinds of matter, in proportion to its resistance. I have also found that with less matter, or with matter of lesser resistance, intervening between the two bodies, the reciprocal modification takes place more readily at a much greater distance than when *nearer* to each other but separated by bodies of *greater resistance;* and that this reciprocal modification takes place more readily through a vacuum than through media. Furthermore I have found that this tendency between two bodies to react with each other—when in such a reacting state—is so great that often intervening bodies are pushed aside or are modified in some way in consequence; i.e. the two reacting bodies conquer the resistance of the separating medium. The facts, then, are, not only that bodies can react at a distance, but that they can do so *even if separated from each other by intervening matter*, provided the resistance of the latter be not too great.

This is the answer which observation and a study of phenomena give.

CHAPTER XI

ON ACTION AT A DISTANCE (*continued*)

In the preceding chapter we have again followed the method we have adopted throughout this essay, viz., to combat such errors as are based on false conceptions by tracing them to their sources, as this seems to us the only efficient means to uproot them. Facts of themselves cannot prevail against preconceived ideas, as the theory against which they may happen to be marshalled has its own account of them—an account which to the prejudiced mind must, of necessity, seem the more reasonable because of its being the more agreeable. Hence it is that in a work dealing with physical science so much space is devoted to philosophic controversies. As we have said before, mental concepts play as important a part in scientific reasoning as do facts—we might even say a far more important part, since in reasoning it is ideas, and not facts, which are compared, and from which inferences are drawn. This is true in every case, whether the ideas themselves be true or false. Reasoning is a mental process, essentially a function of the mind; and the 'mind' consists simply of the mental representations of nature. It was as necessary, therefore, to examine these mental representations as to examine the facts themselves for which they are supposed to stand.

The facts themselves which bear on this discussion have been dealt with in the previous chapters, notably in the last but one. We have seen that in the transmission of 'electricity' from body to body it is not 'contact' and distance which are determining factors, but resistance; that, the less of resistance there is, the more easily does such transmission take place, quite independently of distance.

We shall now give a few other instances which will prove the same point. Put, in place of 'electricity,' 'heat,' and the same principle will hold good. The passage of 'heat' from one body to another will take place all the more readily the less the intervening resistance. This has been proved conclusively by Professor Tyndall in the experiments above referred to. By allowing the rays of a lamp to pass through a tube filled with gases, he has shown that the quantity of 'heat' passing is in an inverse ratio to the quantity of air or gas contained in the tube. If the transmission of heat depended on 'conduction,' the reverse should have been the case; whereas a vacuum proved the most efficient condition.

It is clear, therefore, that bodies can and do act at a distance. For, even if it be assumed that it was the remaining traces of gas which passed the heat along the tube from particle to particle, the action at a distance must still be admitted; and that because, after the tube had been exhausted, the particles of gas could not be so close to each other as before such exhaustion. But, if we admit that one particle can heat another at a distance, it is no longer a question as to whether action at a distance is conceivable or possible, but merely as to the *extent* of distance at which such action can take place. And, if we conceive absolutely nothing to separate two particles from each other, it is difficult to imagine what should prevent the passage of heat from one particle to another. It seems to us that Faraday's conception, of regarding two particles which are next to each other—that is, having nothing between them—as *contiguous*, is a perfectly philosophic one.

There is one circumstance in which distance influences the action between two bodies, which, however, has nothing to do with the problem itself as to whether action at a distance can take place or not. It is this: Suppose a hot body surrounded by bodies colder than itself. It would react, or tend to react, with all such bodies; or, in common phraseology, it would 'radiate heat' in all directions. The further away a body is, therefore, the less of such 'heat' would reach it; and the law which holds good of the radiation of 'light' would also hold good of every other kind of excitation. In this sense, then, and in this sense only, distance plays a part in the interaction of bodies. A very distant body, though colder, may not be warmed by a source of 'heat'; not, however, from want of 'conduction,' or 'want of knowledge[1]' of each other on the part of the two bodies, but because the intervening resistance may be too great, and the 'heat,' by radiation, so distributed all round as to be near zero before it has reached the body in question.

But even here the 'mind' (from this new point of view) cannot conceive distance to have anything to do with it if there were not matter intervening. Say, for instance, that we conceived two bodies only to exist in the universe, at any distance we may choose to think of, the two being at different temperatures. Then, with the law of reciprocity and the tendency to equalization fully impressed on our mind, it would be impossible to conceive how the 'heat' could possibly radiate in any other direction than towards the only 'colder' body in the universe; for, since every action is mutual, it could not 'give off heat' where no body existed to be heated. Which reasoning we do *not* here offer as proof of the truth of our conclusion, but merely as showing that the inconceivability of things depends entirely on mental states; or, as Mill expresses it, 'is, in truth, very much an affair of accident, and depends on the past history and habit of our own mind.'

Such abstract reasonings, which necessitate the supposition of impossible conditions, are, indeed, worthless and mischievous in the extreme. But they are useful when, as in the present case, they are employed merely to disprove another hypothesis similarly based on assumptions.

But it is not impossible to prove that bodies at a distance can react with each other in full force where such radiation as above described is out of the question. The most suitable agency for our experiment is, perhaps, air.

Let us take a tube, for instance, and let that tube be of any length. If we exhaust the air at one end, causing there locally

[1] 'Want of knowledge on the part of the two bodies.' This phrase has been suggested by the following passage in Dr. C. Isenkrahe's above-quoted work (*Das Räthsel von der Schwerkraft*):—'Nimmt nun Zöllner an, ein Atom *A* zöge ein zweites, von ihm entferntes Atom *B* in Folge einer Lustempfindung an, stiesse in Folge des Gegentheils ein drittes *C* ab, so liegt doch zunächst die Frage vor: *Wer bringt denn dem Atom* A *die Nachricht, dass die Atome* B *und* C *überhaupt existiren*, wer klärt es über die Qualitäten derselben auf, die in ihm das Lust- oder Unlustgefühl zur Folge haben?' Anything more crudely anthropomorphic it would be difficult to think of.

a rarefaction, the air in the remainder of the tube would expand proportionally, and the pressure throughout would be the same. If the tube be now divided by a movable partition, a closely fitting piston, this would oppose the expansion of the air in the denser part in proportion to the resistance it can offer by friction and gravity. But, if this resistance is not sufficient to prevent equalization from taking place, the piston will be moved forward by the expanding air until the pressure is equal on both sides.

Let us now imagine such a tube several miles long, and the piston in it also to be several miles in length, so that at each end of the piston there is left a small air-chamber only. Then, if we can imagine a piston of such immense length to have but little weight and friction, it will be seen that, when the air is compressed or exhausted in one of these chambers, this piston would be pushed in one or the other direction. In short, the two chambers when of unequal pressure would tend to equalization, no matter how far apart; and, further, the facility of this equalization would depend not on distance, but on the amount of intervening resistance.

The piston here supposed may be heavy, or very tight, and successfully resist the equalization. But nevertheless this mass of matter would be pressed against more strongly at one end than at the other. And, if these two chambers were connected by a long tube, *a channel of lesser resistance* would have been established, corresponding to 'conductors' or connecting wires. And if this connecting tube be of small diameter the reaction would proceed with a greater force, though at a lesser rate, than if a larger tube had been provided.

Now this is an experiment which is made on a fairly extensive scale with the pneumatic post. The transmission of the carriages in these tubes depends entirely on the inequality of air pressure on either side of the carriage. In place of the carriage we may fill nearly the whole of the tube with earth, leaving a short air-space at either end. Let one of these chambers be exhausted, and this column of earth would be pressed against in the direction of the rarefied chamber. Suppose now—and the supposition is by no means a difficult one—that the pressure on all sides be equal save in the direction where the rarefied chamber is, and that the intervening earth has neither weight nor friction, then it is clear that the air in the denser chamber would expand, pushing the intervening column forward, *in consequence of another volume of air a mile distant having been rarefied.*

If the reader should find the least difficulty in conceiving this, let him think of a tube of like length filled with water, having two air-chambers, as shown in the annexed figure (Fig. 23). *A* and *B* are two air-chambers separated by a long horizontal column of water. Let the air in one of the chambers be compressed or exhausted, and the column of water will move backwards or forwards, as the case may be. But in such case the water would move because it is pushed by the denser air in one of these chambers; and this air would expand because a rarefaction has taken place in the other, *distant* chamber. In this case it is clear that the water is a passive and retarding, and not a 'conducting' or mediating, agent.

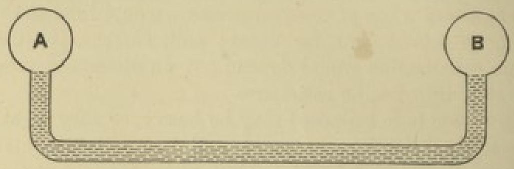

FIG. 23.

We will mention two more experiments, made and described by Professor Tyndall, which on first thought might seem to make for the contrary of our conclusion. One relates to the phenomena of 'light' and the other to those of 'sound.'

Professor Tyndall has shown that if a pencil of light be allowed to pass through a vacuum, or through an air-chamber in which any floating particles have previously been allowed to subside, such space is not illuminated, but the light nevertheless passes through it and reappears on the other side. The apparatus he employed for the purpose is the now familiar box with windows on opposite sides for the light to pass through, and a glass front through which the interior can be viewed by the observer. Either the box is exhausted or the floating particles are merely allowed to subside, and a pencil of light is then allowed to fall on one of the side windows. Looked at from the front the box appears darkened;

nevertheless the light reappears on the other side of the opposite window.

This experiment is very useful to demonstrate—not the presence of a 'luminiferous ether'—but that action at a distance is possible, and that it actually does take place everywhere around us, though, from ancient habit of thought, people may not always be able to perceive it. In the case of bodies falling to the ground there is obviously action at a distance, since it is universally recognized that any intervening matter (including the atmosphere) retards their progress. If the possibility of action at a distance be denied in principle, it would be necessary to invent another ether which would 'conduct' gravity. Of course several such ethereal theories have actually been propounded. Our present object, however, is, not to create ethers, but to dispel the illusory conception concerning this luminiferous ether.

It is the erroneous conception that contact between bodies is necessary before they can affect each other which gives rise to the necessity for the assumption of such occult agencies. The necessity for the hypothesis of a luminiferous ether is again a mental one, and not a requirement of actually observed facts. The argument, if stated in plain terms, would amount to this: 'Contact, as is known (by supposition!), is a necessary condition for the transmission of agencies; but here we have no visible or material body to connect the two ends of our enclosed box; hence there *must* be something that is invisible and immaterial.' To which, however, the philosopher should add—'There *must* be, because otherwise I am unable to explain the phenomenon.' In other words, the impossibility of understanding or explaining a phenomenon is offered as demonstration of the existence of a something of which there is absolutely no evidence whatever—save what is prompted by ignorance.

We may draw some useful conclusions from the above experiment. First of all, let us note some of the recorded observations. An observer standing in front of the box, at right angles to the direction of the passage of the ray of light, does not see any light in the box, which appears to be perfectly darkened. If, therefore, it were the 'luminiferous ether' which 'conducts' the light, or excitation producing the sensation of light, from window to window, how is it that this 'ether' does not throw out the light on all sides, or at any rate illumine the box? Other experiments, as well as common experience, tell us that, the more of matter there is between a source of light and our eyes, the less 'light' will reach the latter. If, therefore, the 'luminiferous ether' was in sole possession of the interior of that box, unhampered by 'brute matter,' it should have been resplendent with light; instead of which the box, looked at from the front, was dark, while the issuing light on the side opposite to that of the lamp was more brilliant than when the box was filled with air and floating matter.

Let us repeat it. When such a box is filled with matter and a ray of light is passed through it, the issuing light is less brilliant than when the box is exhausted, and an observer looking at it from the front sees the box illuminated. But when the box is exhausted the inside appears dark, whilst the issuing light at the opposite end is more brilliant. Clearly crude matter has something to do with these phenomena, which can be explained, as will now be shown, without the aid of a supposed 'luminiferous ether,' and made to prove the possibility of action at a distance.

The interpretation of the phenomena of light requires a study of physiology, into which we cannot enter at present. Nevertheless, the reader will have no difficulty in following our explanation of the above experiments. Light is but one kind of excitation, proceeding from highly excited (incandescent) matter, and imparting excitation to other matter; this in its turn excites, mediately or immediately, our optic nerve, producing in us a sensation called 'light.' This subjective sensation may be produced locally by simply exciting the optic nerve, either by pressing the eyeball, by electric excitation, or even by cutting the nerve. When the light passes through a vacuum and we are looking into the space without being exposed to the direct action of the rays, we see no 'light'; and that for the simple reason that there is nothing there that could be excited by the lamp, and therefore nothing that could excite our optic nerve. But, on looking at the other side of the box where the ray emerges, we again see 'light,' because the excitation from the lamp, having passed through the exhausted box (*by transmission of excitation at a distance without contact*), imparts excitation to the floating solid particles on the other side, and these in their turn excite our optic nerve.

If such a box be successively filled with different gases or liquids, the emerging light will be found to vary in intensity: just as when similar experiments are made with heat or electricity; i. e. depending entirely on intervening resistance. Thus the light of the sun is often enfeebled with different states of atmospheric conditions, and in the case of dense fogs is entirely, or almost entirely, cut off.

But if it were a question of an ether passing the light through the box, an ether which we are told is omnipresent, then why should the box appear darkened after 'brute' matter is removed?

We will now sum up the results of these experiments. In the first place, we are bound to conclude that the supposed luminiferous ether is not a *source* of light. This is easily proved. For on drawing the shutters, with a resplendent sun outside and the room full of this 'luminiferous ether,' we shall be in utter darkness nevertheless. But on striking a match we shall at once have light in the room. For a *source* of light, therefore, we must look to crude matter.

Let us see now how far it can be claimed for this ether that it is a *transmitter* of light. The ray of light passing through a box, as above described, neither illuminates the interior of the latter in the absence of any solid particles, nor does it send any light through a window at right angles to the direction of the passage of the ray of light. The *dispersion* of light, it is thus proved, is performed entirely and solely by 'brute' matter; but why this should be so when this ether is permeating space in all directions is not clear. Let it be remembered that all that which is called diffused daylight is dispersed or refracted light, with which the luminiferous ether, therefore, can have no concern. Indeed, according to the foregoing experiment, the defenders of this unique immaterial substance would have to claim for it another and peculiar property in addition to its many other qualities; viz. that it 'conducts' light from a source to an object only when the object is directly opposite to the luminant, i. e. in a straight line; whereas all other forms of transmission are performed by crude matter. But in that case, we believe, this luminiferous ether might very conveniently be dispensed with altogether, since under this view it does not appear to be so indispensable as seems to have been supposed.

Another point which the above experiment makes quite clear is that 'light' can find its own way without any guide or 'conductor,' provided there be nothing to arrest it. That is to say, a luminous body will always be visible if nothing—meaning absolutely nothing—be interposed between such luminous body and the observer. The question as to what is to 'conduct' the light to the observer is too crudely anthropomorphic to be seriously considered. Far more pertinent is the question, What is to stop it reaching the observer when there is nothing between?

Whilst, therefore, light can experimentally be shown to require neither matter nor ether to 'conduct' it, it is equally easy to demonstrate that both for its production and dispersion, or refraction, crude matter is absolutely necessary; whereas the necessity for an ether is purely a mental one.

We now come to the phenomena of 'sound.' We have not made the experiment ourselves, but accept the results obtained by Professor Tyndall and others, that the sound of a bell under an exhausted jar is muffled. We also accept the conclusion that, if it were possible to establish a perfect vacuum, the sound would not be heard at all. But, then, what is 'sound'? and what are the conditions which produce these sensations? Here the wave-theory cannot be doubted, since it is proved that a piano-string if set in vibration sets the air in motion; and, conversely, if the air is set in motion, so as to cause a vibration in a string, the latter will convey to our ear the sensation of sound. It is further proved that a wind can carry a sound *towards* or *from* us; while in case of a sounding bell or a tuning-fork the vibrations can actually be felt and arrested, and the sound may be cut off. When, therefore, a bell is struck, it vibrates, causing a vibration of the surrounding air, which vibration spreads in the aerial ocean in a manner analogous to the waves produced on a sheet of water by throwing into it a pebble. These waves eventually reach the drum of our ear, causing there locally a sensation which is designated by the name 'sound.'

If it were not for the vibrations of the air, this sensation would not be evoked. There is no such thing as an objective 'sound,' nor do bodies 'emit sound,' but it is purely a subjective sensation, due to a local disturbance caused by the undulations of the air.

It is in this respect that the sensation of 'sound' differs essentially from the sensation of 'light'; the latter is due to direct excitation, whereas 'sound' is due to a mechanical disturbance. That is the reason why we can see the flash of lightning so much sooner than we can hear the roll of the thunder. The brilliant spark, due no doubt to incandescent matter (gaseous or solid), can excite our optic nerve because the intervening air offers no great resistance—and that the more readily the less there is of resisting media between it and our eyes. But the sensation of sound is due to the undulations or mechanical motions of the air; and these undulations reach our ear after a comparatively much longer time. In the absence of an intervening atmosphere, therefore, we should not hear any thunder at all, since the thunder is due to the *mechanical* motions or undulations of the atmosphere; whereas the sensation of 'light' is produced by the incandescence, or high state of excitation, of distant particles of matter[1]. And this explains why 'sound' is not 'transmitted' so well through a vacuum, since mechanical or dynamical action is not possible without contact[2]. Light may, therefore, be regarded as a process of equalization between the eye and other bodies in a higher or lower state of excitation, while 'sound' is due to a mechanical disturbance.

And now, to conclude this discussion, we would once more remind the reader of the essential distinction we have drawn in a previous chapter between primary and secondary or kinematic and dynamic reactions. The former takes place in consequence of some inequality or disturbed equilibrium; the latter when a third body obstructs this kinematic reaction. When, for instance, the two air-chambers in our last preceding illustration (see Fig. 23) are at different pressures, they tend to equalize; and this is what we call a kinematic reaction, which not only can, but always does, take place at a distance.

[1] Cf., *re* the causes of lightning, Book IV, chapter vi, p. 398.

[2] The bell under an exhausted jar is merely set in vibration, and will, no doubt, vibrate more freely in vacuum because being unresisted; but, there being nothing else to be set in vibration by it, the drum of our ear remains undisturbed. This will also explain why water 'conducts' sound better than air; and why doctors use pieces of wood when wishing to listen to the beatings of the heart.

But when a third body, as the water in the supposed case, obstructs this kinematic equalization, and is pressed against by the expanding air, then this is a dynamic reaction, in which case 'contact' is always a necessary condition, since if such body did not happen to be in the way it would not be affected by the former reaction. All kinematic reactions, therefore, can take place at a distance, provided there be no insurmountable intervening resistance; while 'contact' is only essential where we wish to utilize such kinematic 'forces' for human purposes, or where objects obstruct the free equalization of two bodies in nature.

We may heat a cold body from a hotter one at a distance; we may also electrify, attract, or repel one body by another at a distance; but we cannot make one body strike, press, or push another body at a distance unless there is some mediate body which may be pushed or pressed against the other body. The difference between mechanical or dynamic actions, on the one hand, and kinematic actions, on the other, is as clear as essential. And it is only because greater attention has been paid to dynamic actions, with which men are naturally more familiar, that philosophers have hitherto failed to recognize the possibility—we should say the actual fact—that bodies can and do act at a distance, and do so in consequence of the universal tendency to equalization: a tendency so strong as to be able to overcome obstacles.

'Sound' is a sensation caused by dynamic effects, as has just been shown, and hence the seeming exception to the rule; while 'light' is due to kinematic causes—an excitation of the optic nerve by a body in a higher or lower state of excitation. 'Light' would 'come' to us all the more readily the less there is of matter intervening between our eyes and the source of excitation; whereas 'sound' depends entirely on the undulating qualities of the intervening matter.

CHAPTER XII

MAGNETISM

I cannot conclude this series of researches [on magnecrystallic force] without remarking how rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develope more and more their importance, and their extreme attraction as an object of study. A few years ago magnetism was to us an occult power, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and, through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself.—FARADAY.

WHAT is magnetism? The answer to this question is as easy as the explanation of the phenomena covered by the word is difficult. Magnetism is a word consisting of nine letters, is said to be derived from Magnesia, a town in Lydia, where a peculiar iron ore is to be found possessing the power of attracting iron, and is used to denote this peculiar property. This is a complete explanation of 'magnetism.'

Lest the reader may be inclined to regard this explanation as banter, or as an outburst of ill-timed pleasantry, we hasten to disabuse his mind by assuring him that it is a serious conclusion arrived at after long, laborious thinking, and is meant in the most solemn and literal sense. And as it is necessary for the reader to realize the full significance, or rather insignificance and unimportance, of the word, which is supposed to explain so much where it merely obscures, we shall enter somewhat more fully into a consideration of this magic word. We shall attempt to give an outline of its origin, which, if not literally true, is at least substantially so; and then we shall be able to see how little is really explained by it.

An iron ore found near a town named Magnesia possesses the peculiar property of attracting iron. Other iron ores found elsewhere do not possess this property, so that some designation was necessary to distinguish the one from the other. Such distinctive designations are generally derived from some peculiar property (as lodestone, from the use of the magnet to 'lode' or guide), or from the locality where it has been found or first observed, or from the name of its discoverer. The ore in question was distinguished as Magnesian ore, until the name itself was dropped and the qualifying adjective used by itself as the name of the substance. Such instances are very common. Thus a cooling apparatus invented by Liebig was at first called a Liebig's condenser, but is now familiarly called a 'Liebig,' the qualifying noun having been adopted as a distinctive name of this particular apparatus. The 'farad,' the 'ohm,' the 'ampère,' and 'volt' are other instances; these terms standing respectively for the units of Ohm, Ampère, &c. So also a 'Soxhlet,' a 'Westphal,' &c., are names by which chemists know certain apparatus named after their inventors. So likewise a 'magnet' is simply the name of that peculiar iron ore found near Magnesia.

If a like body is subsequently found elsewhere, or a different body is discovered possessing similar properties, then this fact is announced by saying that such and such a body exhibits properties similar to that of the Magnesian ore or *magnet*—the latter word being merely a contraction or abbreviation of the former phrase. But language always tends to contraction, and the phrase 'like a magnet' is soon contracted into the shorter 'magnetic' or 'magnetism.' In saying, therefore, that such and such a body is 'magnetic,' we simply say that it is 'like a magnet,' or like the Magnesian ore. In course of time the words 'magnet,' 'magnetic,' 'magnetism,' are mentally associated with the peculiar property exhibited by the ore from which these names have been derived; and 'magnetism' comes to mean 'the property of attracting iron.'

Let the reader now try to explain the phenomenon itself by saying that 'the peculiarity of a body to attract iron is due to its property of attracting iron,' and he will see at once how little he has really explained. Yet when for the last phrase the shorter word 'magnetism' is employed, as, for instance, 'the peculiarity of a body to attract iron is due to *magnetism*' (i. e. to the property of attracting iron), the sentence at once assumes a philosophic air, and *sounds* like an explanation. In truth, however, the mind has simply been lulled to rest by a meaningless word, and has been further betrayed into regarding this abracadabric word, *magnetism*, as a *something*, an entity. The ancients would have conceived it as a man or a woman, according to the accidental grammatical gender. To Bacon it was a 'spirit'; more recently it became a 'force'; and to the modern philosopher, who scorns anthropomorphism, spirits, and forces alike, it is a 'form of energy.' 'Magnetism' can enter a body, or may be conferred on a body, and may be 'imprisoned.' Thus we read in *Lessons in Elementary Practical Physics* (by Balfour Stewart and W. W. Haldane Gee), 'The magnetization is thus allowed to enter, and when entered is kept there. . . . A kind of trap is thus laid for the magnetization, *this* being invited *to enter* through an open door, which is immediately shut, so that *the guest* is converted into *a prisoner*.' The italics are ours, and are here used to point out the anthropomorphic conception which seems to have obtruded itself on the authors in spite of themselves.

We have dwelt at this length on the derivation of the word 'magnetism,' in order to show that it is nothing more or less than an obscured statement of fact (to wit, that bodies called 'magnetic' possess the peculiarity of attracting iron), and hence cannot serve the purpose of an explanation. The phenomena have merely been named, and subsequently this name is offered as an explanation. But, instead of explaining, such makeshifts only render explanation more difficult, inasmuch as such arbitrary names separate certain groups of phenomena from analogous manifestations, leading to classification instead of generalization. To-day we have positive and negative magnetism; para- and diamagnetism; magnetics and non-magnetics; magnetic and electric attraction; magnetism and electricity; and so forth: all of which terms separate natural phenomena into distinctive groups. But, as has previously been pointed out, the tendency of science should be to generalize, and not to classify; for only by this means can we hope to discover the principle underlying all manifestations.

'Magnetism' explains nothing; like all other 'isms' and 'ities,' it only names, classifies, separates, and mystifies, and thereby achieves the very opposite to what should be the aim of true science. We shall disregard it, therefore, entirely, disregard also the limitations of phenomena imposed by the word 'magnetism,' and shall devote ourselves to an examination of the conditions under which such phenomena as are commonly comprehended under the term 'magnetic' take place.

Divested of all limitations, 'magnetism' means the attraction which one body has for another body. This definition would include the attraction which excited glass or resin manifests for other bodies. We are well aware that the latter are commonly excluded from the term, as their attraction is attributed to 'electricity'—i. e. a property manifested by amber. But this distinction is based on an error, inasmuch as a magnet is supposed to attract the iron group of metals only, whereas excited glass or resin is not supposed to attract these metals. We shall now show by experiment that this distinction does not hold good. And here we may mention that the experiments now to be described have been suggested by our theory, and not that the theory here set forth has been deduced from the experiments; so that the latter are again to be regarded as true *a posteriori* verifications.

We reasoned thus: If the property of attracting bodies (commonly called 'magnetism') be a primal quality, it must be possessed by all bodies without exception. If, on the other hand, it be incidental to aggregation, shape, or combination, then the causes of this peculiarity should be discoverable in chemical or physical conditions. Now we shall be able to prove conclusively—

1. That all bodies without a single exception can be brought into a state in which they are capable of attracting other bodies.

2. That, if all those bodies are to be called 'magnetic' which can attract iron, then all bodies can be 'magnetized,' since on all bodies without exception can be conferred this property of attracting iron, whether the latter be itself 'magnetized' or not.

3. That in all bodies thus 'magnetized' this peculiarity will be retained for a longer or lesser period; that the same holds good of iron, which in different states of hardness will retain this state for longer or shorter periods; so that it is again merely a question of difference of degree, and not of kind.

4. That all 'paramagnetics' are 'diamagnetics,' and all 'diamagnetics' are 'paramagnetics'; the supposed different manifestations again being due to difference of degree, and not of kind.

5. That the 'polarity' of magnets is a property incidental to shape and aggregation, and not a primal quality.

6. That there is attraction only, and not repulsion; and that what is called repulsion is merely masked attraction in opposite directions.

We shall take these points in the above order, and then discuss the inferences to be drawn from the experiments now to be described.

Iron may be 'magnetized' by friction, torsion, pressure, or induction. 'Magnetization' by induction takes place when one piece of iron is 'magnetized' by a permanent magnet or by 'electricity.' This 'magnetization,' however evoked, is retained by iron for a longer or a shorter period, depending, as it would seem, on different degrees of hardness, and possibly also on the chemical admixture of other bodies, such as oxygen in the natural lode-stone, and carbon in steel. But whether either of or both these causes affect the permanency of the 'magnetization' need not concern us, as we shall simply attribute it to greater or lesser resistance or persistence, to whatever causes these differences in resistance may be due. This general law will always hold good, as we shall presently see; and that because the permanency of the evoked 'magnetism' is always inversely as the facility with which bodies can be brought into the 'magnetic' state. The more easily a body can be excited, the sooner will it lose the property of attracting other bodies.

Iron may be 'magnetized' by friction. This may be seen in every metal workshop where iron is drilled or planed. The drill becomes strongly 'magnetic,' and the filings adhere to it in long fringes. The drill being steel, this 'magnetization' is retained for longer periods than in soft iron, which loses its 'magnetization' almost instantly. We have already seen that by friction are also generated both 'heat' and 'electricity'; that 'heat' may be converted into 'electricity' and 'magnetism,' 'magnetism' into 'electricity' or 'heat,' and 'electricity' into 'heat' or 'magnetism.' Furthermore, that any one of these can be converted into mechanical motion, or be derived from it, as when bodies are rubbed against each other. From this alone it must be clear that we have not to do with four distinct 'ities' or 'isms,' but simply with four distinct manifestations of identical causes.

But iron is not the only substance on which can thus be conferred the property of attracting other bodies. Glass, india-rubber, resins, wood, paper, and in fact *any* solid (as will presently be shown), can by friction be made to attract other bodies—in short, can be made 'magnetic.' But here it will probably be objected that glass, resin, &c., are only 'electrified,' and not 'magnetized.' This distinction shall now be considered by the aid of experiments. 'Magnetized' iron attracts iron, cobalt, and nickel, but none of the other substances; whereas 'electrified' glass and resin are supposed not to attract or be attracted by 'magnets.' This supposition, however, seems to be based on a foregone conclusion, since the facts prove the contrary in a most striking manner.

The apparatus we have employed consisted of a needle made of soft iron, 8 inches long, ½ inch wide in the centre, tapering to points, and about twice the thickness of ordinary writing paper, and mounted on a needle point like a compass needle. The remaining parts of the apparatus consisted of a vulcanite rod 12 inches long by ⅜ inch thick, and a shellac rod 9½ inches long by 1 inch thick. By rubbing one end of either of these rods with a piece of undyed silk cloth, the needle was deflected when the rod was brought within a distance of 3 inches from the point of the needle. The excitation of the rod soon diminished, but even after the lapse of ten to fifteen minutes it still possessed sufficient power to attract the needle, and to pull it through a complete circle forwards or backwards if brought to within about a quarter of an inch of the point.

If the vulcanite or shellac rod is rubbed with a silk handkerchief and brought to within half an inch of iron filings, the latter fly against the rod as they would against a powerful magnet. With glass the experiment is more difficult; but if this is held in the hand with silk wrapped round it, and rubbed with amalgam, the same effects can be produced. But with indiarubber the experiment is quite easy and striking. If, instead of iron filings, some unspun silk is cut up into small bits of say 1 to 2 mm. long, these will behave with an excited glass rod in a manner analogous to iron filings with a magnet. They will not only fly towards the glass, but show a distinct polarity; for if the glass rod be passed over them at a distance not sufficiently near to make them fly against the rod they will stand up on end and follow the motions of the rod in the characteristic manner exhibited under like conditions by iron filings towards a magnet. For the iron needle in the above experiment we have substituted permanent magnets, which could be swung round a circle several times, backwards and forwards, by excited vulcanite, shellac, or glass. The vulcanite retains this power for several minutes, whereas glass retains it for a few seconds at most. These experiments were repeated with an excited electrophorus. The vulcanite disk of this instrument, which was excited by beating it with a silk handkerchief, behaved like the vulcanite rod. But we have also succeeded in deflecting both iron and permanent magnets with the brass disk (or condenser) of the electrophorus. However, to be successful with the latter the instrument had to be charged to a high degree, until on putting on it the condenser a very bright spark was obtained; and even then the experiment has to be made dexterously, as the brass soon loses its charge.

If for the iron or magnet in the above experiment strips of copper, zinc, or aluminium be substituted, the action is the same. Other substances, such as paper, glass, resin, pith, ivory, silk, &c. (all of which have been tried), need not be particularly mentioned, inasmuch as the phenomena with these substances are familiar.

The point of interest is that metals, and more particularly iron and permanent magnets, are thus attracted by excited vulcanite, resin, and glass. Now it will be clear that if excited vulcanite will attract iron or a magnet the reverse is also necessarily true—that magnets will attract excited glass, resin, or vulcanite.

Thus one great distinction between attraction due to 'frictional electricity' and 'magnetic' attraction falls to the ground. But, inasmuch as this mutual attraction between magnets or iron and the substances enumerated does not take place unless the latter substances are excited, it follows that, for the purpose of this mutual attraction, both bodies must be excited. Now when a magnet is brought near a piece of soft iron the latter is temporarily converted into a magnet; so that the iron may be regarded as being first 'magnetized' by 'induction,' and then, both being 'magnets,' they attract each other.

To test whether this explanation would also hold good in case of vulcanite we made the following experiment. A piece of clock spring, of the same dimensions as the needle above described, was mounted, and by bringing near it an excited vulcanite rod several times (about three or four times is sufficient) was converted into a feeble but distinct 'magnet.' And even the soft iron needle above referred to was after several experiments found to be 'magnetic,' though it soon lost this 'magnetism.' From which it appears that the same explanation would hold good as regards the attraction of iron, whether the attracting body be a permanent magnet or excited vulcanite.

It is not true in all cases that both bodies have to be specially excited before they can attract each other; for, as is well known, an excited glass rod can attract pith or any other light body without the latter having to be rubbed. The probable explanation, however, is that these bodies are first excited by the rod 'inductively.' This preliminary excitation, before attraction can take place, probably means no more than overcoming the resistance or persistence of the body. This explanation seems all the more probable because even permanent magnets do not turn instantly when cautiously approached by another magnet, as has been mentioned in a previous chapter. This would at once explain why in some cases both bodies have to be excited and not in others. A permanent magnet may not be powerful enough to overcome the persistence (or inertia) of glass or vulcanite, whilst it can easily do so in case of iron and a few other metals. But, if we excite the vulcanite or glass independently, then these bodies can attract or be attracted by magnets.

That there should be differences in the various substances is hardly a matter for surprise, as if all substances behaved exactly alike in point of degree as well as of kind we should not consider them to be different. But there is a great distinction between difference in degree and difference in kind. Our contention is that as regards *primal* qualities of matter there is no distinction of *kind* at all, but only of degree; and that whatever primal quality is possessed by any one body is possessed by all. Thus hydrogen is a gas, nickel is a solid. But we now know that hydrogen behaves chemically like a metal, can be liquefied and solidified; and that nickel is capable of existing in the gaseous form. And, though we have not yet succeeded in gasifying nickel by itself, its carbide is gaseous, and nickel is at present actually separated from its ore on a commercial scale by such a process[1]. This shows that nickel can exist in a gaseous state, and that it and hydrogen differ in this respect only in having their boiling points far apart. So likewise bodies may possess different degrees of that quality called their 'magnetism'; and because some bodies do not exhibit this quality in such a marked degree as iron, or that the conditions and manifestations under which this quality can be made manifest differ, it would be rash to conclude that there is any difference of kind. If we strike iron, glass, wood, or water, how different are the manifestations; and yet these differences are only due to different degrees of hardness of the substances struck.

But we have further experiments of great interest bearing on this point. A magnet will not attract unexcited vulcanite, but will attract unexcited iron. The explanation we have just given is that previously to being attracted the iron is excited by the magnet. This point is proved by the fact that two pieces of unexcited iron do not attract each other any more than will unexcited vulcanite and iron. Now if a piece of soft iron be rubbed against silk, a file, leather, wood, or paper, it does not attract iron unless it retains some 'magnetism.' That steel can be 'magnetized' by such means has already been referred to; so can some pieces of softer iron, if of such a nature that they can retain the excitation for some time. But soft iron, as is well known, is as easily 'demagnetized' as 'magnetized,' as is proved by temporary electro-magnets, which act as powerful magnets whilst under the influence of the 'electric current,' but lose this 'magnetism' as soon as the 'electric current' is interrupted. A piece of soft iron, therefore, loses the excitation imparted to it by friction as soon as the latter is discontinued. To this fact, then, must we look for an explanation why unexcited resin is not acted on by magnets; and why such metals as copper, brass, &c., are ordinarily not 'magnetic.'

[1] Brunner-Mond process. See *Journal of the Society of Chemical Industry*, vol. xiv.

That brass may exhibit this property of 'magnetism' has already been proved experimentally, since we have succeeded in deflecting a magnetic needle with the highly charged brass condenser of an electrophorus [1]. The points here to be noted are that the experiment only succeeds when the electrophorus is very highly charged, and that the brass very quickly loses this power of attracting a magnet. This explains why certain metals are ordinarily not magnetic, and why they cannot be magnetized even temporarily by friction. The explanation is that as their resistance is very feeble they lose this magnetism almost instantaneously with the cessation of the excitation. But this property is also shared by iron (generally considered as *the* magnetic), which, when very soft and pure, behaves like any of the 'non-magnetic' metals; i. e. is 'demagnetized' as quickly as 'magnetized.'

On the other hand, copper can act as a magnet under the influence of active excitation. If a strip of copper be mounted like a magnet and placed between two Leyden jars each connected to one end of the electric machine previously described, it will turn towards the two jars as soon as the machine is set in motion. We have also tried zinc, brass, iron, and magnets —all of which behaved in like manner. And that excited copper can attract iron and even magnetize it is well known. Of course the current explanation is that the 'electricity' of the copper has been converted into 'magnetism' in iron; but so is the 'solidity' of the ice converted into 'liquidity' in the water, or the vibration ('mechanical energy') of the tuning-fork converted into 'sound energy' in our ears. If the iron exposed to the electric current be soft, there will be no 'magnetism' left in it, any more than in the copper immediately after the passage of the current ceases, whilst during the current both copper and iron are 'magnetic.' If, therefore, harder iron retains this property for a shorter or longer period after the cessation of the electric current, then this is clearly due to its greater hardness or greater molecular resistance. But there is no need on this account to regard it as something *sui generis*.

In support of our view that both bodies must be excited

[1] Not unless the electrophorus yielded a very large spark and loud report on bringing the condenser to the vulcanite plate could we succeed with the experiment.

in order to produce the phenomena of mutual attraction, and that the soft iron is 'magnetized' by the magnet previously to being attracted, we would mention that the electrophorus when but weakly charged does not cause a deflection in the iron; but when sufficiently charged to cause such deflection the iron is subsequently found to be 'magnetic.' Indeed steel may be 'magnetized' in this way by repeated charges from an electrophorus. So that brass and iron not only attract each other when there is sufficient excitation in the former to excite the latter 'inductively,' but the iron may actually be converted into a permanent magnet by such charges from *brass*, while the latter does not retain this property. Which points in the direction that the peculiarity of iron as against brass and other metals may be due to its greater resistance as compared to that of other substances.

From these considerations two conclusions follow: (1) That brass can deflect a magnet, if in a sufficient state of excitation; (2) that feeble excitations will not enable it to do so. As an explanation of the latter phenomenon we can only surmise that, the 'conductivity' of brass being greater than that of soft iron, its charge is soon lost. The same explanation would hold good in respect of other metals. This conclusion seems to be strengthened by the fact that a copper disk, when placed between the poles of a powerful horseshoe magnet, shows no signs of currents; but, when the disk is revolved with a certain velocity, 'electric currents' are induced in the latter, which may be augmented by increased speed; and this again confirms the conclusion that the rate of generation of excitation must be greater than the rate of diffusion.

If, therefore, iron is instantly converted into a magnet when brought near a permanent magnet, we must ascribe this peculiarity to the degree of resistance which iron possesses: being comparatively easily excited, and yet possessing sufficient resistance to retain this excitation in order to be able to react with the magnet. In other words, its resistance is not too great to be instantly excited, *if the magnet be sufficiently powerful* (and this italicized condition is important) and not so small but that it can retain temporarily this excitation. But if the magnet be feeble in comparison with the quantity or quality of the iron with which it is brought into contact, so that the diffusion equals the excitation induced in the latter, then this attraction will not be manifested, though with smaller particles of iron—or with iron of lesser resistance—the 'magnetism' of the inducing body can be demonstrated.

Yet another fact. Very soft iron will lose its 'magnetism' instantly; harder iron will retain it somewhat longer; whilst steel will retain it for a still longer period. We can thus construct a scale of magnetic attraction between the softest and hardest iron, in which *resin or vulcanite would occupy an intermediary position.* Thus we may excite soft iron, steel, and vulcanite, and we shall find the vulcanite still to be 'magnetic' after the soft iron has lost this property. In view of this fact it would be difficult to define 'magnetism' so as to exclude vulcanite from the category.

Again, nickel and cobalt are also 'magnetic,' but in a lesser degree than iron. But we have just shown by an experiment with the electrophorus, which everybody can easily make for himself, that brass, too, is 'magnetic,' though in a still lesser degree than either nickel or cobalt. It is, therefore, merely a question of difference in degrees; and if a scale were prepared in which substances were ranged according to the length of time they retained this power of attracting iron when excited, and another according to the facility with which this excitation could be evoked, we should have two different scales, and in each the softest iron would be separated from the hardest iron by other substances, at present not classed as 'magnetics,' that would occupy intermediary positions.

We have promised above to show that all bodies without a single exception can be brought into a state in which they are capable of attracting other bodies, or, briefly, that all bodies are 'magnetic'; but have hitherto confined ourselves to some few substances only. But, inasmuch as all bodies are known to be either para- or diamagnetic, it will be sufficient, in order to establish this point, to prove the identity of what are called para- and diamagnetism, and we will, therefore, postpone the fulfilment of our promise until we come to the discussion of diamagnetism.

This will dispose of points (1) and (2), enumerated at the beginning of this chapter. Under point (3) we have said that in all bodies thus 'magnetized,' or excited, the power of attracting other bodies will be retained for a longer or lesser period, and that this also holds good of iron in different states of hardness. This, too, has already been disposed of; so that we now come to point (4), that all 'paramagnetics are diamagnetics,' and all 'diamagnetics are paramagnetics'; the difference being merely one of degree.

But, before approaching the phenomena of 'diamagnetism,' it will be necessary to deal with another property of magnets, i.e. that known as 'polarity.'

CHAPTER XIII

POLARITY OF MAGNETS

The two characteristic properties of magnets are their power (1) of attracting iron and (2) of assuming a definite direction relatively to the poles of the earth, or relatively to other magnets. With the former we have dealt already; the latter peculiarity is known as 'polarity.' This 'polarity,' again, exhibits itself in two distinct manifestations. One is the tendency of a magnet to assume definite directions along its longitudinal axis; and the other the selective difference which the two poles of a magnet exhibit relatively to the two poles of the earth, or to the poles of other permanent magnets.

We shall deal first with the former property, i. e. the tendency of a magnet to place itself meridionally with its longer axis. 'Magnetism' will henceforth mean to us simply attraction, a property which we have shown to be evokable in all bodies under suitable conditions, and which is retained by different substances for longer or shorter periods: just as 'heat' or 'electricity' is retained by different bodies for longer or shorter periods. This attraction, then, is a primal quality of all matter.

It is not so with 'polarity,' not even in the case of magnets, as we shall now show. If a clockspring, say about one inch wide and six inches long, be 'magnetized,' then one of its ends will point north and the other south; while the sides will be east and west respectively. But if now a small piece, *a*, of a lesser size than the width of the 'magnetized' spring, be cut off, as shown by the dotted lines in Fig. 24, and this small piece be suspended or mounted so as to be free to rotate, it will be found to assume a position north and south with its longer axis, as against its former position of east and west when forming part of the whole magnet. This points to the conclusion that the 'polarity' of magnets

Fig. 24.

relatively to the earth, or relatively to other magnets, is due to a distribution of mass, rather than to a property of the molecules; hence is an *incidental* quality.

It will not be difficult to verify this conclusion, and to find a simple explanation of the phenomenon. Let two bar magnets be placed with their opposite poles towards each other at a certain distance apart, and let a small piece of soft iron, mounted so as to be free to rotate, be placed between these magnets, as shown in Fig. 25, then this piece of iron will place itself axially in a line with the two magnets, and will always return to this position if moved out of it.

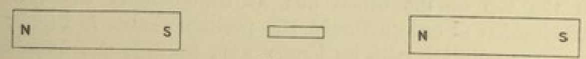

Fig. 25.

The explanation of this is very simple, and follows directly from the principles laid down in these pages. Every reaction is determined by difference of states, mass, and distance[1]. As regards the difference of states between the iron and magnets (or iron and other bodies) to which we attribute their attraction for each other[2], the whole mass of the soft iron may be assumed to be in an equal state throughout;

[1] By 'mass' we simply mean greater or lesser quantities of the *same* substance, and not in a Newtonian sense, which compares different kinds of matter. By it we simply mean that two pounds of iron are double the mass of one pound of iron, without presuming to say what quantities of other substances would be a corresponding mass.

[2] We attribute the attraction of two bodies for each other to difference of states, in accordance with the general principle we have proved to hold good in so many instances. That it is also true of magnets is proved by the fact that when two magnets are placed against each other with their opposite poles touching (i. e. in states of equalization) they no longer attract iron while in this combination. Further proofs will be afforded in the succeeding pages.

in other words, each molecule of the piece of iron may be assumed to be equally attracted by the magnets on either side. But in that case the attraction along its longer axis is bound to be greater, and that for the simple reason that the aggregation in this direction is greater. Other things being equal, attraction will be proportional to mass; which means

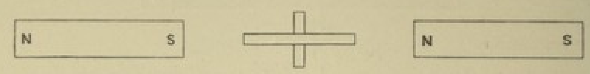

FIG. 26.

that ten particles would react with a ten-fold greater strength than one particle. And if the length of the piece of iron be ten times its width, or thickness, then the attraction along the longer axis must be correspondingly greater. Having been so mounted that, while free to rotate, it cannot fly towards either magnet, the piece of iron yields to the *greater attraction*, along the line of its longer axis, and places itself axially between the two magnets as shown in Fig. 25.

On placing a similar piece of iron, only much longer, on the top of the other, and at right angles to it, the combination at once assumed the position shown in Fig. 26; the cross pointing with its longer axis towards the poles of the two magnets.

We then took two strips of iron of equal length and width, mounted at right angles. The position of the cross was precisely that which we expected, and which is shown in Fig. 27. For now, both pieces of iron being equally

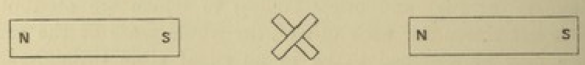

FIG. 27.

attracted by the two magnets, neither could assume the axial position, and, therefore, adopted the mean, each part of the cross cutting diagonally the line connecting the two magnets.

In another experiment we took a square piece of iron and mounted it on the revolving stand. It at once assumed a position pointing with two of its angles towards the magnets, as shown below (Fig. 28), and returned to this position when moved out of it.

All these experiments prove that the poles always lie at the two ends of the longest axis, and that whether the experiment be made with iron or with permanent magnets. We have already shown above that, when a narrow strip, shorter than the width, is cut off a permanent magnet, such a piece changes its poles, which are now along the longer axis.

To prove this further we had a dozen magnets made exactly one inch square and nearly a quarter of an inch thick, with holes and brass caps in the centres, so that they could be mounted on needle points. These pieces were laid edge to edge, so as to form one continuous bar, and were thus magnetized by an electric current. Their poles, therefore, were on two opposite sides, and acted thus when several of them were in combination. But, when separated and

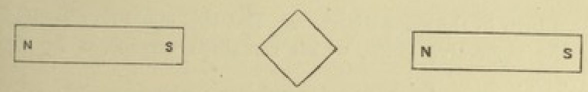

Fig. 28.

mounted singly, they still had north and south poles, but this time not along their straight ends, but diagonally from corner to corner—i. e. along their longest axes. And, whether relatively to the earth's meridian, or when placed between the opposite poles of two magnets, they always assumed a position like that of the square piece of soft iron shown in Fig. 28.

But if, instead of two bars crossed, or square pieces, a disk or round ring be taken it will retain any position that may be given it, provided, of course, that the distribution of the metal is the same all round the centre. We have made this experiment with a steel ring made in two halves, about six inches in diameter and half an inch thick. While the two halves were together, the ring manifested no 'polarity,' and but very feeble 'magnetism'; but when separated and suspended by threads each half assumed polar positions.

For the soft iron in all the above experiments we may, of course, substitute permanent magnets, and the result will always be the same, whether in respect of the terrestrial poles or in respect of permanent magnets.

It is thus proved that 'polarity' is incidental to shape or distribution, and due to *attraction along the line of greatest aggregation*[1].

We have thus accounted for the 'polarity' of magnets in general, but not for their distinctive poles. From the foregoing considerations we were led to believe that the difference between north and south poles may also depend on some slight difference of mass; but our experiments in this direction were of a decidedly negative character. We have magnetized a piece of clockspring, 1 inch broad and about 5 inches long, and have cut it across diagonally, as shown in Fig. 29, expecting to find that the broader end of each would there-

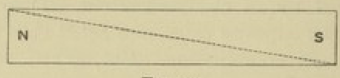

FIG. 29.

upon turn in the same direction. But this made no alteration in the respective poles, which remained unaffected by this division.

Another experiment undertaken with the same object, to prove that the north and south polarity of magnets is only an incidental property, was more successful; and, though perhaps not quite conclusive as to what causes one pole to be of an opposite (or seemingly opposite) kind to the other, the experiments we are about to describe leave no doubt whatever that this quality is incidental, depending entirely on certain conditions, and is not a primal quality of matter. And, as in this essay we are concerned with primal qualities only, this subject of 'polarity,' after we have proved it to be an incidental quality, will be of secondary interest only for our present purpose.

[1] This may still further be illustrated by rolling a magnet in iron filings, or by sprinkling filings on a sheet of paper above a magnet. The attraction is greatest at the poles, diminishing towards the centre, and for a certain space near the centre there is no magnetism at all. The length of this neutral zone bears a fixed relation to the width and thickness of the magnets. If the latter be cylindrical, then the thicker the magnet the longer will be this neutral zone. And if the magnet be a perfect cube its magnetism will be strongest at the corners and edges; i. e. always along the longest axes. Even with ordinary bar magnets of a square section it can be shown that they possess stronger magnetism at their edges than at any part of their flat surfaces.

We thought that if the distinctive 'polarity' was not due to distribution of mass it might be due to different degrees of excitation. We were first led to this belief from experiments made with the electrophorus and a soft iron needle mounted like a magnet. The excited vulcanite disk of the electrophorus, when brought near the needle, attracted the latter, as has already been mentioned. It was with the intention of determining the point of strongest attraction in the disk that the latter was presented to the needle in various positions, sometimes edgeways against the point of the needle, sometimes full-faced, and so on. We had noticed, namely, that in some parts of the disk the attraction was stronger than in others, being, as has subsequently been proved, strongest at or about the centre of the black side of the disk; much feebler at the edges; and weakest on the side covered with tinfoil. During these experiments it often happened that the needle was repelled by the vulcanite, but the exact causes of this repulsion we could not determine; for if, when the edge was brought near the needle, repulsion took place, then in a second experiment just the opposite might happen. Again, one end of the needle might be repelled by the disk in a certain position, and the other side attracted. At other times both ends would be repelled or attracted, as the case might be; and although we had repeatedly such attractions and repulsions we could not produce them at will.

It occurred to us that these repulsions and attractions might be due to different states of excitation, or rather to an unequal distribution of the excitation over the needle, whereby one end of the needle was more strongly attracted than the other, and hence the seeming repulsion of the point nearest the disk, because the opposite side was more strongly attracted.

To test this theory we made the following experiment. A piece of clockspring, about six inches long, was magnetized by two powerful bar magnets, by what is known as the double-stroke method, using the north-seeking poles of *both* magnets, starting in the centre of the clockspring, and drawing the magnets towards the ends. We thus obtained a magnet having two south poles. This magnet was mounted, and each end was found to be equally repelled by the south pole of another magnet. When mounted and left to itself, the magnet did not manifest any decided tendency of 'polarity' relatively to the earth (though it seemed as if an east and west tendency were stronger than that of north and south), but behaved more like an astatic needle. Subsequently we gave one half of this magnet about another half-dozen strokes like those it had received before, so that now one half of it was more strongly magnetized than the other. On being remounted, the side that had been more strongly magnetized pointed to the magnetic south, the spring lying in the magnetic meridian. Towards permanent magnets both poles were still south, as before, but the one which had received the stronger magnetization was more strongly so than the other.

Again we took two strips of clockspring, magnetized them as above, giving each three strokes, and then tested them against each other. Both had two south poles, and seemingly were perfectly indifferent to the magnetic meridian of the earth. One of these springs received an additional three strokes on one half of its length only; this end we shall henceforth call the marked end. Replaced on the stand, the magnet at once pointed with its marked end to the magnetic south. To the other clockspring both poles were still south, but the marked end much more strongly so than the other. We now gave the other clockspring an additional three strokes on one half of its length. By bringing the marked end of the one towards the marked end of the mounted spring, repulsion took place at a much greater distance (at least double) than before, showing that the two marked ends were more strongly antagonistic than previously. But, if the marked end of one of these magnets (which for the sake of distinction we shall call the field-magnet) was brought near the unmarked end of the mounted magnet, repulsion still took place, but at a much lesser distance, the two magnets coming within an eighth of an inch of each other before repulsion occurred, while the two marked ends repelled strongly at a distance of more than two inches. With the unmarked end of the field-magnet repulsion took place at both ends of the mounted magnet, but at much lesser distances, and again at a lesser distance near the unmarked end than near the marked end of the mounted magnet.

Another nine strokes were given to the marked end of the field-magnet, and also to the marked end of the mounted magnet, which now exhibited a very strong polarity towards the earth. The two marked ends now repelled each other at a distance of three inches, whereas the marked end of the field-magnet had to be brought within a quarter of an inch of the unmarked end of the mounted magnet before repulsion took place. But, if the marked end of the field-magnet was made to touch the unmarked end of the mounted one, they attracted each other, and the latter could be pulled round a complete circle; but if the two marked ends were brought together they repelled each other very strongly. Still more remarkable, the unmarked end of the field-magnet (i.e. the end of lesser magnetization) attracted the marked end (i.e. the end of stronger magnetization) of the mounted magnet, whilst very feeble repulsion took place when the two unmarked ends were brought together.

Both magnets were then tested with one of the bar magnets, and both ends still proved to be south to the latter, though the marked ends were very much more strongly so than the others.

To us these experiments seem conclusive that south and north poles of magnets depend on different degrees of excitation: just as the 'polarity' itself depends on distribution of mass.

Yet another experiment. We again used one of the magnetized clocksprings mounted on the stand, but this time substituting for the magnetized clockspring used in the former experiment as field-magnet one of the powerful bar magnets which had been used for the magnetization of the former[1]. The south pole of this magnet repelled both poles, and the north pole attracted both poles, of the mounted clockspring. But when the north pole was brought towards the centre of the mounted magnet, the two magnets being perpendicular to each other as shown in Fig. 30, the marked end at once turned towards the field-magnet: just as would an ordinary magnet under like circumstances with its south pole. When the experiment was repeated, now bringing the south pole of the field-magnet towards the centre of the other, the unmarked end turned towards it, but only to within a certain distance (about 45°), and then stopped there.

[1] The dimensions of these magnets were $6'' \times \frac{3}{4}'' \times \frac{1}{8}''$.

This shows clearly that the attraction of the two poles for each other depends on different degrees of excitation.

We repeated the last experiment, taking two such bar magnets so as to form a compound magnet. Again the unmarked end of the mounted magnet was turned towards the south pole of the compound magnet, but this time nearer than before. With three such magnets combined, the attraction was still greater, the two poles approaching to within half an inch.

All these phenomena were obtained when the magnets were brought at right angles towards the centre of the mounted clockspring. But, if brought towards the ends of the latter, then both ends were attracted by the north and repelled by the south pole of the field-magnet, though with markedly different intensities.

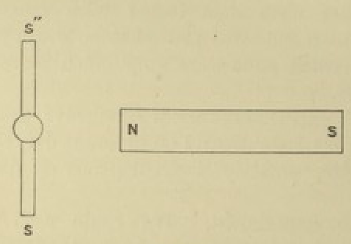

FIG. 30.

It should be mentioned that when two magnets are thus made, both having two south poles, on continuing the magnetization on one half of the magnet only, the distance at which repulsion takes place between the two marked ends increases with the increase of partial magnetization; whereas the distance of repulsion between the marked and unmarked ends as steadily decreases, until a point is reached at which the two marked ends powerfully repel each other, whilst the opposite ends attract. With powerful ordinary magnets both poles will still show south magnetism, but of markedly different degrees, as has been shown by the above experiment.

We will now describe another interesting experiment, one which we had made previously to those just now described, but have reserved to be mentioned in second place because the phenomena will be more intelligible after the experience of the former experiments. A number of bar magnets of the dimensions already described (6 inches × $\frac{3}{4}$ inch × $\frac{1}{8}$ inch) were placed side by side with their like poles together, and placed end to end with a number of similar magnets—arranged in like manner—touching with their opposite ends, so as to form a compound magnet, as shown in Fig. 31. For the most

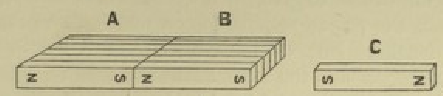

Fig. 31.

part eight magnets were used in each row, so that the compound magnet consisted of sixteen pieces. In bringing now the end of a single bar magnet of like dimensions near the end of this compound magnet, as shown in the illustration, the phenomena were just what might have been expected; that is, there was strong attraction between the unlike poles. But if the S end of the solitary magnet was brought near the S ends of the pile some very striking and unexpected phenomena were witnessed. Thus the S end of this magnet would be repelled by some of the S ends of the pile and attracted by others. This attraction was often so strong that individual magnets could be drawn out a certain distance, often nearly the whole length of the magnet (as shown in Fig. 32), when the attraction ceased, and the two poles again repelled each other.

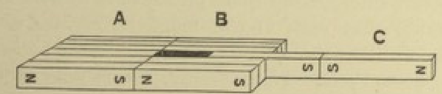

Fig. 32.

On taking out such attracted magnets and examining them, they were found to behave normally with the test magnet: that is, the like poles repelled and the unlike poles attracted each other. If one of these magnets thus pulled out happened to be the first, second, or third in the pile and had been replaced in a different order, its attractive power towards the like pole of the same test magnet was thereby either diminished or increased, or there was distinct repulsion. Or, again, when the magnet was replaced in its former position, and by testing it had been ascertained that its S pole manifested the same attraction towards the S pole of the test magnet as before, and then another magnet was added to the hinder series, it and all the other magnets in the row often lost this attraction.

A more detailed description of the laws evoking these phenomena it is impossible to give, for the reason that we were unable to ascertain the precise conditions on which these changes of polarity depend. But we have never yet failed to produce these phenomena (and the experiment has been repeated many times and on several occasions) by changing the order of the magnets in case of non-success at first, or by adding or removing one magnet in either one or the other of the two rows. As a general rule we found that, if the total 'magnetism' of the hinder row of magnets (marked *A* in the illustration) was less than the total 'magnetism' of the series *B*, then the S poles of *B* were attracted by the S pole of the test magnet, some more, some less, and some not at all. Thus if six of the magnets were selected which were somewhat thinner, and placed against six thicker magnets; or if one or two magnets more were placed in the one series than in the other: then almost invariably *some* of the magnets in the lesser part proved 'magnetic' to the like pole of the test magnet. Often when the experiment would not succeed, or the attraction was but very slight, not sufficient to pull the magnet out of position, we merely had to change their order to be able to pull one or more of these magnets out as before.

Now before interpreting these phenomena it will be well to bear in mind that each of the magnets, resting as it does against the unlike pole of another magnet in the series behind it, is attracted there. If, therefore, the S pole of the test magnet can draw out one of these magnets by its S pole away from the opposite pole of another magnet, it is clear that in such case the attraction between the two S poles must be stronger than the attraction between the unlike poles of the magnet thus pulled away and the magnet behind it. To this must be added the weight of the magnet itself and its friction on the table, all of which are overcome by the attraction which the S pole of one magnet exerts on the S pole of another.

These experiments again show (1) that the north and south polarity of magnets is merely an incidental property; (2) that it is not due to any difference in kind, since the same poles of two magnets can attract and repel each other without having their own polarity changed.

The former series of experiments will explain the phenomena of the latter. We have seen that magnets may be 'magnetized' more strongly in one half than in the other; and that two magnets thus magnetized, both having two south poles as compared with a third magnet, have different poles relatively to each other and relatively to the earth. Relatively to each other, because the more strongly magnetized end of the one attracts the less strongly magnetized end of the other, whilst their like poles repel; and relatively to the earth because the more strongly magnetized end points south. If two such magnets were tested relatively to each other, or relatively to the earth, no one could suspect that such magnets had both poles alike.

One circumstance, however, must be pointed out which does not well agree with our conclusions. When one half of the magnet is more strongly magnetized than the other, both poles being *south*-seeking, then the heavier-charged pole will point south; but, when similar magnets are made with two *north*-seeking poles, then the heavier-charged ends will point north. And, as we are unable to account for this apparent exception, we must leave it at present in this state. But we have seen sufficient in our experiments (points of detail which it would take too long to enumerate here) to make us confidently hope that this exception may be proved in future to be but a seeming exception; and that in time it will be established beyond doubt that the difference in the two poles depends on different degrees of magnetization.

It is not impossible, however, that molecular constitution or arrangement, which may give *direction* to the attraction, may have something to do with these phenomena. But, as we have already said, these matters are of secondary importance only to our present purpose, since we are concerned chiefly about primal qualities of matter. And that 'polarity' in general, as well as the distinct character (whether real or apparent) of the two poles, are but incidental, we believe we have established beyond doubt.

CHAPTER XIV

PARA- AND DIAMAGNETISM

There can be no doubt that the magnetic force, the diamagnetic force, and the magne-optic or magnecrystallic force, will, when thoroughly understood, be found to unite or exist under one form of power, and be essentially the same.—FARADAY.

WE have arrived at the conclusion that the power of bodies to attract each other under certain conditions (i. e. when in different states of excitation) is a primal quality, and as such is common to all bodies. This conclusion at once excludes the possibility of two kinds of magnetism. The distinction established between para- and diamagnetism is based on the observed fact that some bodies when placed between the poles of electro-magnets assume axial positions, whilst others under like conditions place themselves equatorially to the poles of the electro-magnet. From these phenomena it is perfectly legitimate to infer that bodies which are 'paramagnetic' manifest a tendency to move *towards* and those which are 'diamagnetic' manifest a tendency to move *away from* the electro-magnet. This is merely a statement of actually observed phenomena. But, when such terms as 'attraction' and 'repulsion' are applied to these phenomena, then the statement contains, not merely an expression of observed facts, but also a quasi-explanation of the causes of such phenomena. 'Attraction' not only conveys the idea of two bodies moving towards each other, but at the same time offers the explanation that this mutual approach is due to itself—implying that the bodies pull each other. So likewise the term 'repulsion' implies that bodies receding from each other push each other away.

But when two bodies thus recede from each other it is by no means so obvious as is generally supposed that this happens in consequence of mutual repulsion. Let us give an illustration or two. We will place two magnets, *A* and *B*, at an angle to each other, as shown in Fig. 33, and place another magnet, *C*, mounted so as to be free to rotate, in such a position that it shall be nearer to *A* than to *B*. Now the magnet *C* would be attracted by both *A* and *B*; but, *A* being so much nearer, its attraction on *C* would be stronger than that of *B*, hence *C* would assume a position as shown in the illustration. By a previous experiment we have seen that, if two magnets are placed over each other so that their opposite poles shall meet, such combination loses its magnetism. Let

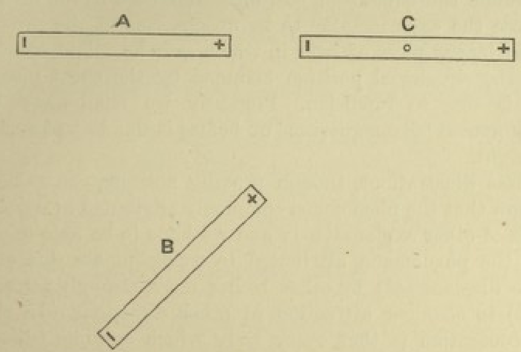

FIG. 33.

us place now a second magnet on the top of *A* in this manner. Then, the magnetism of *A* being neutralized by the second magnet, the attraction between *B* and *C* will now be greater than between *A* and *C*, and *C* will move towards *B*. Suppose an observer to be ignorant of the presence of *B*, he would in all probability attribute this to repulsion between *C* and the magnet placed on *A*, while in reality *A* and *C* would still attract each other (*A* now being an astatic needle which would attract either pole of *C*), and *C* has moved away from *A*, not because of any repulsion between *A* and *C*, but *because of the greater attraction in the direction of B*. Remove *B*, and *C* will again turn towards the astatic needle *A*.

Again, the receding of bodies from each other may be due to displacement, as when oil is displaced by water, hydrogen by air, and so on. These bodies *recede* from our globe under these conditions, not, however, because they are repelled, but because other bodies which are attracted more strongly displace them. Now such displacement is possible also in the case of magnets. In a former chapter we have shown that, when two pieces of iron at right angles to each other are placed between the poles of a magnet, both will tend to place themselves axially, but that, if one piece is much longer than the other, this would be attracted more strongly, and hence the shorter piece would of necessity have to assume the equatorial position. But here again we may suppose the experimenter to be unconscious of the presence of the longer piece of iron, in which case he might conclude that the equatorial position assumed by the short piece of iron is due to repulsion. Presently we shall show that the supposed 'diamagnetism' of bodies is due to just such an oversight.

These illustrations, though proving nothing, are sufficient to show that the phenomena commonly attributed to repulsion admit of other explanations; and we hope to be able to show that the phenomena attributed to repulsion are due either (*a*) to displacement by other bodies more strongly attracted, or (*b*) to stronger attraction in other directions. In short, our contention is that every body which exhibits 'diamagnetic' properties is really attracted by electro-magnets, but assumes the equatorial position in consequence of displacement by another body—in many instances by the air. And, though we shall adduce in support of this view several experiments of our own, we shall rely chiefly on those very experiments of Faraday from which he deduced his theory of 'diamagnetism,' to show that all so-called 'diamagnetics' are 'paramagnetic' if tested with a body less strongly attracted than themselves, and 'diamagnetic' if tested in connexion with or in presence of a body more strongly attracted: just as a body becomes positively electrified if rubbed against a body of lesser resistance, and negatively if rubbed against a body of greater resistance.

We have already seen that glass, india-rubber, or shellac, will attract magnets, iron, zinc, copper, brass, and aluminium. The same substances are attracted by Leyden jars while in the act of being charged. For this purpose two Leyden jars were connected one to each of the conductors of the electric machine previously described. The two Leyden jars may thus be regarded as two poles of an electro-magnet, between which we have placed, in addition to the above-named substances, paper, glass, and ebony, all of which assumed the axial position between the two jars. When two substances, say iron and zinc, or zinc and copper, were combined in the form of a cross, then the body of stronger attraction placed itself axially, or nearly axially, as the case might be, so that the other body of necessity was forced into the equatorial position; but tested singly every substance we have tried was 'paramagnetic' to the excited jars.

As previously mentioned, a burning taper, if brought near one of the brass knobs of the jars or machine, was strongly blown away; but a little protuberance of that part of the flame nearest the ball showed distinctly that the flame was really attracted—very markedly so—and that the blowing in an opposite direction of the remaining portion of the flame was due to air currents coming from all parts of the machine and excited jars, and which were strong enough to be felt by the hand.

These air currents themselves, however, do not necessarily prove repulsion, as might at first be supposed; but may be regarded as being due to attraction. Thus: the particles of air, being at first attracted by the jars, become excited, and then, being in equalization with the jars, are attracted more strongly in other directions by bodies in a lesser state of excitation. That the air particles flying away from the Leyden jars are electrified may be inferred from the previously mentioned fact that they can excite a gold-leaf electroscope placed at a distance, and that the electroscope gives no such indications when protected against there currents by a sheet of paper.

But a direct experimental proof that the flying away of the excited particles of air is due, not to repulsion, but to attraction in other directions, is afforded by the pith-ball pendulum, which behaves similarly to air. If an excited glass rod be brought near a piece of pith, suspended as shown in Fig. 34 (in which *B* is a brass stand and *p* a piece of pith suspended by a thread of silk) the pith will fly towards the rod, adhere to it for a few seconds, and then fly towards the brass stand, adhere to this for a rather shorter period, and again return to the glass rod. If, instead of the glass rod, an electric battery is employed, and the pith ball suspended between the two brass terminals of such a battery, the pith ball will fly alternately from positive to negative and from negative to positive pole[1]. Seemingly it is alternately attracted and repelled by both poles; in truth, however, it is merely attracted by alternate poles, and that because the pith equalizes its state of excitation with the pole it happens to be in contact with, whereby the attraction

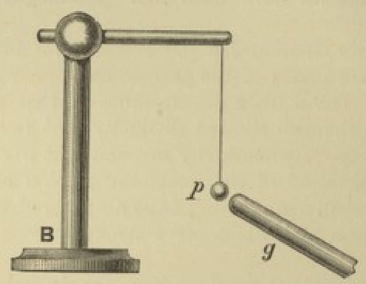

FIG. 34.

between it and the other pole becomes stronger. Thus in the above illustration the pith ball hangs vertically from the brass stand: but, if the latter be connected with a source of electricity, the pith ball at once flies towards it. Or, if an excited glass rod is brought near it, it flies towards the same, quickly equalizes with the rod, and is thereby raised to a higher state of excitation than the brass stand, and consequently flies towards this. The observer who only takes account of glass rod and pith ball, but disregards the brass stand, is liable to regard the flying away of the pith ball from the glass as being due to mutual repulsion. But that there was really attraction between brass and pith is proved by the fact that the pith ball adheres for some seconds to the brass, just as

[1] See Urbanitzky, *Electricity in the Service of Man.*

it did to the glass rod, and that even though the glass rod be withdrawn at the moment the pith flies from it.

The fact that the pith ball does not adhere quite so long to the brass as it does to the glass at once suggests the explanation that the brass, being a much better conductor, would more quickly equalize with the pith ball than would glass. This conclusion we have subsequently confirmed by substituting vulcanite or paper for the brass, in which case the pith ball coming from the excited glass rod adhered for a longer time to those bodies than to the latter.

For the pith ball we have substituted cork, paper, silk, foils of aluminium, tin, copper, zinc, magnesium, &c., all with the same results; which proves conclusively the general law that bodies in different states of excitation attract each other.

It is obvious that any body attracted simultaneously in two or more different directions, but with unequal intensities, will gravitate [1] in the direction of greatest attraction. Equally obvious is it that if a body be attracted simultaneously in all directions with equal intensity, and then through some circumstance the attraction in one particular direction be increased, the body would then move in that particular direction. But the same would happen if the state of excitation of all surrounding objects *save one* were lowered, in which case the body would again be attracted more strongly towards the one now in a higher state of excitation than the rest.

But there is yet another circumstance to bear in mind—the

[1] The reader may probably object to the use of the term 'gravitate' in connexion with bodies attracted by other bodies in a horizontal or upward direction. We would point out, therefore, that the term is here used designedly in preference to the word 'attracted,' in order to show that the *peculiarity of gravitation* as against other forms of attraction lies chiefly in the limited application of the term. If we speak of sun and planets 'gravitating' towards each other, no matter what the direction, why should we not use the same terms for identical phenomena when observed with bodies on the earth? The phenomena are identical, i. e. two bodies approaching, or tending to approach, each other. Whether the *causes* are also identical or not, is a different matter, which can only be decided by investigation. But, if we employ distinctive terms, this question is decided (unconsciously) without any such investigation, by the words themselves. We would point out, therefore, that both here and elsewhere we purposely use the terms which are employed in *other* groups of phenomena for the *corresponding* manifestation, in order to bring home to the mind that the supposed difference is mainly due to the limited use of certain words, and that though the *meaning* of such words does not warrant such narrowness. We would again instance such words as 'conductors' and 'insulators,' or 'connecting' and 'separating' two bodies by putting a third one between them, all of which are used unphilosophically and derive conventional meanings from association with particular conditions only.

ever-present disturbing element of the earth. Under the ever-varying conditions so repeatedly dwelt on in these pages, one would expect that each body would constantly be impelled in different directions. And such is truly the case; the ever-present attraction of the earth, however, is much stronger than are the slight changes in surrounding objects under normal conditions. A suspended pith ball, even when protected against air currents, may constantly be impelled in different directions with varying intensities, and yet remain quiescent because the force or power of attraction in a horizontal direction, due to some inequalities in state of excitation of surrounding bodies, may not be sufficient to overcome the superior attraction of the globe: and through this disturbing element many such reactions are hidden from view.

In the foregoing experiments and explanations we have made use chiefly of solids, because it is more easy for the mind to comprehend the phenomena. But the same is true of the invisible air. Our pith ball pendulum may be suspended from the ceiling: and, when an excited glass rod is brought near it, it will simply fly away and return again. In that case, however, the equalization will take place with the atmosphere, of which in such experiments as a rule no notice is taken.

So far then we have seen attraction only, and what seemed repulsion has been shown to be attraction in an opposite direction, or displacement by a body more strongly attracted. Approaching now the phenomena of 'diamagnetism' as observed by Faraday, we shall endeavour to show from Faraday's own experiments that these were due mainly to displacement by bodies more strongly attracted than those under observation. In some of his experiments, where the body investigated was immersed in a liquid or combined with another body, this will be obvious. In other instances the body of greater 'magnetism' was the air, which the reader will do well to remember is a very bad conductor, and hence an *excellent electric*.

And now as to the experiments of Faraday. The experiments relate to the supposed dia- and paramagnetism of bodies. As is now well known, all bodies placed between the poles of an electro-magnet assume definite positions relatively thereto; some substances assuming axial, others equatorial, whilst still others assume intermediary positions. The first class of substances Faraday had termed paramagnetic, the second diamagnetic, whilst the third class were considered as feebly para- or diamagnetic, according as they approached more nearly to one or the other of the two cardinal positions. Now Faraday himself supplies us with the *experimentum crucis* whereby to prove that these different positions are all due to attraction; and that all bodies possess the same kind of 'magnetism,' differing in degree only. To wit, bodies seemingly repelled by the poles of the electro-magnet are repelled because their own state of excitation more nearly approaches the state of excitation of the magnets than of other bodies; hence these latter are more strongly attracted than the former. In other words, *the seeming repulsion of bodies under observation is due to displacement by other bodies more strongly attracted, but of which no account has been taken by the experimenter.*

We shall now quote from Faraday's *Researches in Electricity*, italicizing such parts to which we wish to draw special attention.

'§ 2362. A clear solution of the proto-sulphate of iron was prepared, in which one ounce of the liquid contained seventy-four grains of the hydrated crystals; a second solution was prepared containing one volume of the former and three volumes of water; a third solution was made of one volume of the stronger solution and fifteen volumes of water. These solutions I will distinguish as Nos. 1, 2, and 3; the proportion of crystals of sulphate of iron in them were respectively as 16, 4, and 1 per cent. nearly. These numbers may, therefore, be taken as representing (generally only '2423') the strength of the magnetic part of the liquids.

'§ 2363. Tubes like that before described (2279) were prepared and filled respectively with these solutions and then hermetically sealed, as little air as possible being left in them. Glasses of the solutions were also prepared, large enough to allow the tubes to move freely in them, and yet of such size and shape as would permit of their being placed between the magnetic poles. In this manner the action of the magnetic forces upon the matter in the tubes could be examined and observed, both when the tubes were in diamagnetic media, as air, water, alcohol, &c., and also in magnetic media, either stronger or weaker in magnetic force, than the substances in the tubes.

'§ 2364. *When these tubes were suspended in air between the poles, they all pointed axially or magnetically, as was to be expected; and with forces apparently proportionate to the strengths of the solutions.* When they were immersed in alcohol or water they also pointed in the same direction; the strongest solution very well, and also the second, *but the weakest solution was feeble in its action*, though very distinct in its character (2422).

'§ 2365. When the tubes, immersed in the different ferruginous solutions, were acted upon, the results were very interesting. The tube No. 1 (the strongest magnetically), when in solution No. 1, had no tendency, under the influence of the magnetic power, to any particular position, but remained wherever it was placed. Being placed in solution No. 2, it pointed well axially, and in solution No. 3 it took the same direction, but with still more power.

'§ 2366. *The tube No. 2, when in the solution No. 1, pointed equatorially,* i. e. as heavy glass, bismuth, or a diamagnetic body generally, in air. *In solution No. 2 it was indifferent,* not pointing either way; *and in solution No. 3 it pointed axially,* or as a magnetic body. The tube No. 3, containing the weakest solution, pointed equatorially in solutions Nos. 1 and 2, and not at all in solution No. 3.

'§ 2367. Several other ferruginous solutions, varying in strength, were prepared, and, *as a general and constant result,* it was found that *any tube pointed axially if the solution in it was stronger than the surrounding solution, and equatorially if the tube solution was the weaker of the two.*'

In connexion herewith we would request the reader to reperuse the account (given at the opening of the preceding chapter) of experiments made by ourselves by placing bars, crosses, squares, and disks of iron between the poles of permanent magnets. The two series of experiments are

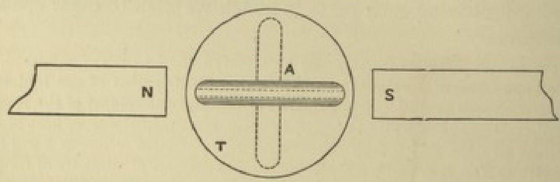

FIG. 35.

analogous, and the explanation of the one affords the explanation of the other.

When a piece of iron was placed between the magnets, as in Fig. 25, it placed itself axially, showing that it was 'paramagnetic'; but, if joined crosswise to another and larger piece of iron, it placed itself equatorially: that is, in the 'diamagnetic' position. Here the explanation is obvious, and will supply the solution of the phenomena above described by Faraday.

Let us suppose the vessel of the outer fluid in which the tubes were floated to be a tray of such size that the tube could freely turn round in any direction of the compass. In Fig. 35 such an arrangement is represented. *T* is the supposed tray, and *A* the tube, both of which contain iron solutions. The dotted lines represent the tube *A* in another position.

Now, when the solutions in both *T* and *A* are of equal strength, the poles of the magnet would attract all the particles of the liquid with an equal force. If, instead of a solution, we had iron filings, these would all fly in the direction of the two poles and range themselves in the well-known manner described by Faraday. But the liquid is acted on by a third body, i.e. the earth, and by its constitution does not assume a position as would solids, but will fill the tray to an even or nearly even level. Now, if the tube were turned in the direction of the dotted lines, the space it formerly occupied would straightway be filled by the solution displaced by the tube. But, the solution, now occupying the space formerly occupied by the tube, being equal in strength and character to the solution contained in the latter, there would practically be no difference in the whole arrangement. The effect would be the same as if a portion had been pipetted out, and an equal quantity of the same strength had been put in its place. This will explain why the tube No. 1, when in solution No. 1 (§ 2365), 'had no tendency to any particular position,' though strongly magnetic when in air, alcohol, or water. The simple explanation is that *equal* volumes of *equal* solution are *equally* attracted; and, before the tube immersed in an equal solution to that it contained could swing round, it would have to displace a volume of *equal* strength but nearer to the point of attraction, which, of course, it could not do.

But it is very different when the two solutions are of different strengths. Say that the solution in the tube be stronger, then it will be attracted more forcibly than an equal part of the weaker solution. Hence the tube containing the stronger solution will place itself axially, displacing the weaker solution which had filled that space: just as a heavier fluid (say water) when poured into a lighter fluid (say oil) will displace the latter, forcing the oil upwards—not because the oil is repelled by the earth, but because the water is more strongly attracted.

Let us consider now the positions reversed, the solution in the tube being weaker than the solution in which it floats. If this tube be turned axially, it would displace an equal quantity of fluid which is more strongly attracted, because more concentrated, than itself; hence it will place itself at right angles (or equatorially), not because it is repelled by the magnets, but because an equal volume of liquid, but much stronger, is attracted more powerfully; hence the tube has to retire to a position further from the two poles, in order to make room for the stronger solution it attempted to displace. That the solution in the tube is not repelled by the magnets, but attracted, is proved by the fact that *when the same tube is placed in a solution weaker than itself* it again becomes strongly 'paramagnetic.'

Obviously the phenomena would have been the same if two such tubes, containing solutions of different strengths, had been joined at right angles and suspended between the poles. The stronger solution, i. e. the more strongly attracted body, would in every case be 'paramagnetic,' and the weaker solution 'diamagnetic.'

We trust the reader will agree with us in regarding this as an *experimentum crucis* in favour of our contention.

Now how does this apply to the supposed 'diamagnetism' of other bodies? All the bodies experimented on have been suspended in a fluid between the poles of a magnet; i.e. in the air, a fluid which is so often entirely ignored. Now air is a most powerful electric, and, when between the poles of a powerful electro-magnet, partakes of the character of a viscous fluid. It was Faraday himself, who, according to Tyndall, 'was the first to arrest by a magnet the motion of a spinning cube of copper.' Professor Tyndall repeated the experiment with a silver medal, of which he gives the following account:—'Suspending the medal between the poles P P of the magnet, I send the current through the coil. The medal hangs between the poles; it is neither attracted nor repelled, but if we seek to move it we encounter resistance[1]. To turn the medal round, this resistance must be overcome, the silver moving as if it were surrounded by a viscous fluid. This extraordinary effect may also be made manifest in another way. Causing a rectangular plate of copper to pass quickly to and fro like a saw between the poles P P, with their points turned towards it; you seem, though you see nothing, to be sawing through a mass of cheese or butter. No effect of this kind is noticed when the magnet is not active: the copper plate

[1] Which shows that *air is attracted by powerful magnets*, and held there 'with a firm grasp.'

then encounters nothing but the infinitesimal resistance of the air [1].'

It is in this fluid that the different bodies have been experimented with; and the deviations of the latter from the axial position only demonstrate how much more strongly the air is magnetic than such bodies.

Now we have shown that glass, one of the 'diamagnetics,' will attract a magnet, which is the same as being attracted by a magnet. But then our experiment was made with feebly excited glass in an ordinary atmosphere; whilst the experiments of Faraday were made with powerful electro-magnets, whereby both air and glass were strongly electrified, and the air more strongly than the glass, because of its being a better electric [2].

The law may be thus stated:—

Of two bodies of equivalent mass, a magnet will be attracted more strongly by the one whose state of excitation is further removed from its own;

Or, if the states of excitation be equal, by the one whose relative mass is greater.

And, inasmuch as all bodies are magnetic, and can attract each other under like conditions, this law applies to all matter without exception.

A body visibly tending in one direction only is no evidence that it does not tend in ever so many other directions at the same time; for it necessarily follows in the direction of greatest influence, or the line of least resistance [3].

As regards terrestrial bodies, this pull in all directions is

[1] *Heat a Mode of Motion*, p. 35.

[2] The experiments made in vacuo by Faraday seem to be opposed to this view. But, then, these admit of explanations on the same principle; viz. that portions of air (perhaps *argon*!) may have remained in the tube, which displaced the body under observation. The experiments with the ferruginous solutions seem to us conclusive.

[3] This explanation may be applied to magnets as to all other bodies. Magnets we would have to assume to be attracted in all directions, though with unequal force. When suspended by itself, and the bodies attracting it are at a comparative distance, such laterally acting forces might not be strong enough to pull a magnet out of its vertical, in which direction it is pulled by the superior attraction of the earth. That thus suspended it has a selective direction in the horizontal plane, or, when mounted as a dipping needle, in the vertical plane, is, of course, well known; it is equally well known that those points which cause the magnet to assume these positions are at a considerable distance, which clearly proves our contention that matter is acted on at a distance, and that magnets are simultaneously acted on in all directions. Say now that we place two magnets with their like poles against

not manifest under ordinary circumstances, and that because there is a common source of attraction which preponderates, and causes them to appear as impelled in one direction only, viz. the attractive force which the earth as a whole exerts on all circumterrestrial objects. Inasmuch as attraction is not only proportional to difference of states, but also proportional to mass, it is evident that the attraction exerted by the whole mass of our globe on minute parts on its surface must under ordinary circumstances, necessarily far exceed the attractive force of such parts on each other. Moreover, equalization takes place more or less readily between bodies near each other on the surface of the earth; and hence this attractive force between adjacent bodies is constantly being reduced to a vanishing quantity. Nevertheless, if the excitation of some bodies be artificially increased or decreased, and they be brought near each other while in different states, this attraction can be, and has been shown to be, possessed by all matter. Thus, whilst the term 'magnetism' is associated with steel and iron, the same phenomena of attraction and repulsion are exhibited under favourable circumstances by all other substances.

Thus, a magnet free to move will assume a position coinciding with the magnetic meridian, the attraction of the earth being greatest on it in that direction. But, if a wire connected with an electric battery be brought near it and parallel to it, the magnet would at once prove 'diamagnetic' to the excited wire. And, if two such wires, capable of being excited simultaneously and independently of each other, be placed at right angles to such a magnet, then, if the electrification of one wire be much stronger than that of the other, the magnet will be found to assume a position parallel to the weaker wire, and at right angles to the one more strongly excited. In other words, the magnet will always move in the direction of greatest attraction, which is in the direction of greatest difference. And from this point of view a magnet might be said to be both para- and diamagnetic relatively to the earth under varying conditions.

each other, then at that point there will be no attraction, or but a very feeble attraction, in proportion as their states more nearly approach each other; and in that case the magnets will be more strongly attracted in opposite directions.

To sum up: bodies will attract each other with a force—

(*a*) *proportional to difference in states of excitation;*

(*b*) *proportional to mass;*

(*c*) *inversely proportional to intervening resistance.*

We shall, however, retain the term 'repulsion' as a convenient term, the reader clearly understanding that by it we do not mean to designate a distinct 'force,' but merely a phenomenon due to either diminished or total absence of attraction between two or more bodies, and a consequent greater attraction in an opposite direction. In this sense, and in this sense only, would we have the following summary statement to be understood:—

Bodies in like states of excitation repel, and in unlike states of excitation attract, each other.

Another law—a direct corollary of the primal law of persistence—should also be borne in mind; i.e. that *each body will retain every acquired state or position with an intensity proportional to that which was necessary to give it that position.*

This law has been explained in one of the opening chapters, and its application will be seen in the chapters that are to follow.

CHAPTER XV

SUMMARY AND CONCLUSIONS

THE fact that the same body will be positively or negatively electrified according as it is rubbed against one or other substance is positive proof that the difference is one of degree and not of kind. And so likewise the fact that one and the same substance can be shown to be 'dia-' or 'paramagnetic' relatively to other substances proves that the supposed two kinds of magnetism differ from each other in degree only, and not in kind: it being merely a question of greater attraction between *A* and *B* than between *A* and *C*.

And, again, the facts that such different bodies as glass, india-rubber, shellac, iron, steel, &c., can be 'magnetized' (or made to attract other bodies) either by friction or induction[1]; that this power of attraction is retained for longer or shorter periods by different substances in proportion as they are better or worse conductors; that in this respect other substances than the metals, or the iron group of metals, hold intermediary positions between the softest and hardest iron: all tend to prove that the distinction between 'electric' and 'magnetic' attraction is arbitrary, and no more philosophic than is the distinction between 'light' and 'heavy' bodies, hot and cold bodies, voltaic electricity and galvanism, and so forth.

We might go further than this, and show the identity of heat, electricity, and magnetism—not their interconvertibility but their identity—by showing that the distinctions are purely subjective, and that the classification has reference to our

[1] The electrification by an electrophorus, or as when an excited glass or vulcanite rod is brought near some suspended matter, and in fact every electrification by conduction, are instances of 'electrification by induction.' By the latter term we merely mean the *transference* of excitation from a body already excited, in contradistinction to generation.

sensations and not to the *causes* of those sensations. 'Heat'—that is, increase of temperature—we have seen to result from coerced states, or states of excitation. But 'magnetism' we have seen to be due to the same causes; whilst 'electricity' is produced under exactly the same conditions. After the preceding explanations and experiments we should now be able to give clear and definite answers to the questions, What is 'heat'? What is 'electricity' or 'magnetism'? and so forth, and accompany our answers by demonstration.

This may be done as follows: Rub a body against another body, then touch it, and the body will feel warm. Next, bring the object rubbed near an electroscope, or insert an electroscope in the circuit connecting the two bodies rubbed against each other, and there will be a deflection of the needle or a separation of the gold leaves, according as one or other kind of instrument is employed. Next, bring the body rubbed near a mounted magnet or light particles of other substances, and there will be attraction. If the bodies rubbed be such substances as lime, sugar, sand, sulphur, &c., and the rubbing be performed in the dark, there will also be 'light.' If in the circuit of such excited bodies be inserted some chemical substance, then (provided the excitation be powerful enough) there will be 'chemical action.' So that by merely rubbing two bodies we get—not in succession, nor by conversion of forces into each other, but *simultaneously*—'heat,' 'light,' 'electricity,' 'magnetism' (or, if we like, we might call the attraction also 'gravitation,' since two bodies flying against each other laterally may as truly be said to gravitate against each other as when this attraction takes place in a vertical direction), 'chemical action,' and under circumstances also 'sound.'

Whether we get all, or any, or none of these manifestations will depend entirely and solely on the tests that are employed to ascertain whether a change has actually taken place in the state of the bodies rubbed against each other in consequence of such friction. By mere inspection in broad daylight the verdict of an ordinary onlooker would be that no effect had been produced at all. By touching the body this view would be modified by saying that it resulted 'in the generation of heat.' By other tests, such as have just been described, 'the generation of other forces' above enumerated might be demonstrated in like manner. From which it will be seen that the classification of phenomena is really a classification of sensations; and that the so-called conversion of forces one into the other means neither more nor less than the substitution of one test for another, and consequently the evoking of one sensation instead of another.

We may now attempt to give a more philosophic view of the causes of these different manifestations. We have shown that there is a relativity between all matter filling the universe, and that there is a tendency to equilibrium whenever this relativity is disturbed. This holds good of all matter, whether as between distinct bodies or the hypothetical molecules or atoms of the same body. Indeed, the distinction between 'bodies' and 'molecules' is entirely conventional, as the principle of equalization between two particles of matter is the same whether we regard such particles as one body or as two distinct bodies. When, therefore, two bodies are rubbed against each other faster than the particles can adjust themselves to the consequent change, the latter will be thrown into a temporary state of excitation or coercion. And as a coerced state means an increase of temperature these bodies will feel warmer. Also, if the resistance of the two bodies is unequal, they will be heated unequally. For, though the two bodies are rubbed one against the other, they are not both exposed to the same conditions: *the one being rubbed against a harder and the other against a softer body than itself.*

It is in consequence of this *inequality of excitation* in the two bodies that equalization between them immediately takes place; and *it is this process of equalization to which are due the phenomena of electricity;* but we can only become aware of this equalization if some resistance is placed between the two bodies equalizing. Thus we have heat and electricity accounted for.

Now, as for magnetism, we have seen that it merely means attraction; and that when free to move, and their mutual attraction is strong enough to overcome the downward attraction of the earth (i. e. of 'gravitation'), two bodies in unequal states of excitation will fly against each other. Let, for instance, a light body be suspended, then this body will be attracted towards the earth. To this kind of attraction, being so common, has been given a special name, 'gravitation'; and the phrase that a body gravitates towards the earth is the same as saying that it is attracted towards the earth. But if an excited body be held above such particle of matter, and the attraction between this particle and the excited body be greater than between it and the earth, then it will as truly 'gravitate' towards (or be attracted by) that other body as towards the earth: it simply follows the direction of greatest attraction.

As regards 'repulsion,' we have already said that where it is not due to displacement it merely means attraction in opposite directions. In the case of repulsion of the like poles of magnets, this might not at first seem so obvious; but a little reflection will show that there is nothing forced in this explanation, which may even be demonstrated in various ways. That magnets are attracted in all directions of a sphere, and not merely in polar directions, is easily demonstrable. A magnet mounted like a compass needle assumes a position in direction of the poles; but, be it remembered, it does not point exactly to the poles, nor in every place in the same direction. Let a magnet pointing north and south in one place be brought near a place where a mass of iron, say a mountain of ironstone, is to the east or west of the magnet, then the latter will be deflected towards this mountain. The same argument will hold good if the magnet is mounted as a dipping-needle. The fact, therefore, that in most localities bar magnets will point approximately north and south is no proof that they are not attracted at the same time in all directions of the compass or of a sphere. The position a body would assume under such circumstances would always be *in the direction of greatest attraction*.

This is demonstrable by artificial experiment. Let a mounted magnet be placed in the midst of a number of other magnets, or be surrounded by several masses of iron. If the attractions of the latter (due either to different quantities, variable distance, &c.) vary, the magnet will always turn with its longest axis towards the point of greatest attraction. Let us now place between the magnet and the body towards which it is attracted something that would act either like an insulator or have the effect of neutralizing the attractive force, then the magnet will turn towards another body whose attraction will now be greatest; and this fact would have all the appearance of mutual repulsion. This is precisely what is being done when the like poles of two magnets are brought near each other. In a former experiment we have shown that magnets having south poles or north poles at both ends, but being magnetized in different degrees, will mutually repel each other with the greater force the more nearly the intensity of their magnetization is alike, and vice versa. Such a magnet placed in the midst of a ring of other magnets will turn towards the one between which and itself the *difference* of magnetization is greatest. In the annexed figure (Fig. 36) we show such an arrangement of magnets all having south

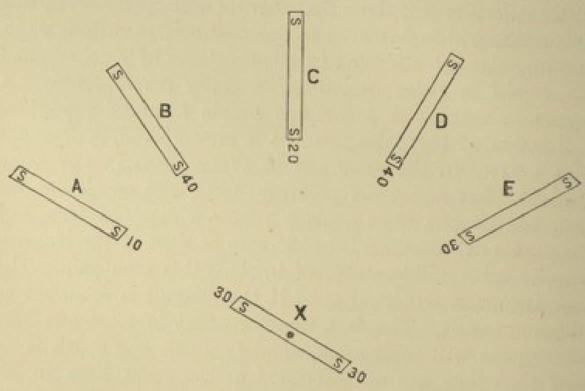

FIG. 36.

poles at both ends, the numbers being supposed to express the relative degrees of magnetization. As will be seen, the central magnet, *X*, is shown to point towards *A*, the difference there being greatest. Give now *A* a few additional strokes, so as to increase its magnetization, and *X* will be seen to be 'repelled' by it; in reality, however, it will now be more strongly attracted by other magnets.

We shall now be able to modify Newton's law of gravitation in a very important point. As is well known, Newton's law is that 'All bodies attract each other proportionally to mass and inversely proportionally to the square of the distance.' It is very certain that nobody has yet succeeded in demonstrating this law experimentally—at least as far as the first part of it is concerned. The usual assertion that the heavenly bodies attract each other 'in proportion to their mass' is no proof at all, since the 'mass' of the heavenly bodies is inferred from and calculated according to a hypothesis. If a larger body is found to exert for the same distance a lesser disturbing force than a smaller body, the former is said to be 'less dense' than the latter. If attraction depended merely on mass and distance, it is incredible that this should not be more manifest than is the case. No doubt the disturbing influence of the earth is great; but not so great but that we can in every respect exceed its power by artificial conditions. Thus the magnetism of the earth, which pulls a magnet in a north and south direction, is easily surpassed by a small piece of iron brought near to the magnet, and that because *a smaller power may act more strongly at a lesser distance than a much superior power at a correspondingly greater distance.* But this does not hold good of magnetism only, but also of that other attraction called gravitation. We know how easily gravitation is overcome, what little force is required to make bodies fly in an opposite direction, either by throwing them upwards, by centrifugal force, or by holding an excited glass or vulcanite rod above particles of matter. Let it be granted, therefore, that the attraction of the earth for a certain particle of matter is so much greater than that of another body that might be brought near it, then it is equally true that the attraction of such a body towards a particle of matter should be much greater than the attraction of the earth *on account of the greater nearness of the former.* Yet in no instance can this 'attraction proportional to mass' be made manifest.

But, if we modify the law as follows:—

Bodies attract each other in proportion to their different states of excitation, in proportion to relative mass[1], *and inversely proportionally to the square of the distance and intervening resistance;*

then we shall at once be able to demonstrate the law in a thousand different ways. Indeed, so soon as we regard gravitation merely as attraction, the application thereto of the

[1] Mass in the sense as previously defined (see p. 220, note).

same principle which is demonstrably true of every other kind of manifestation at once becomes manifest. Sir Isaac Newton himself was rather embarrassed concerning his law of universal attraction, because it did not harmonize with the phenomena of electricity and magnetism, which latter exhibit both attraction and repulsion; and the recognition of the fact made it necessary for him to make (reluctantly, as it is evident) a distinction between 'gravitation' and 'other forces,' as will be seen from his following third rule of reasoning:—'The qualities of bodies, *which admit neither intension nor remission of degrees*, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.'

The implication here is that 'gravity belongs to all bodies,' and *admits neither intension nor remission*, whereas magnetism does not belong to all bodies and does admit of both intension and remission. We have shown, however, that 'magnetism' is as universal as 'gravitation,' and in a future chapter we shall show, from the same phenomena from which Sir Isaac Newton deduced his law of universal attraction, that gravitation does admit of intension and remission, and that the heavenly bodies 'attract' and 'repel' each other in the same sense as do magnets. For the present we need only point to the fact that heavenly bodies alternately *recede* from as well as approach towards each other. But more of this in its proper place. At present we will conclude this part of our investigation by expressing the results in a series of generalizations:—

1. All bodies *in different states of excitation* attract each other, and, when free to move, fly bodily against each other.

2. Where translation in space is impossible, but the bodies are free to rotate, then such bodies will turn towards each other those parts in respect of which the attraction is greatest.

3. When neither free to move nor to rotate, this difference of excitation is transmitted from particle to particle until equalization is established, when the reaction ceases.

4. When two bodies are drawn towards each other in consequence of their unequal states of excitation, but have become equalized, then they will no longer attract each other,

but may be attracted by other bodies, causing each to move in an opposite direction from the other, as if receding from or mutually repelling each other.

5. When bodies are so conditioned that none of the tendencies mentioned in points 1, 2, and 3 can be satisfied, then states of coercion result (such instances are imprisoned air, permanent magnets, compressed springs, &c.).

To these laws we must add another very important one mentioned at the end of the preceding chapter and more fully discussed in an earlier chapter (on Relative Resistance), viz.:

6. Every body will retain any acquired state proportionally to its resistance, or the intensity to which this acquired state is due. (A law the application of which we shall see in the next chapter.)

The foregoing derivative laws will be found to afford ample and satisfactory explanations of each and every phenomenon,

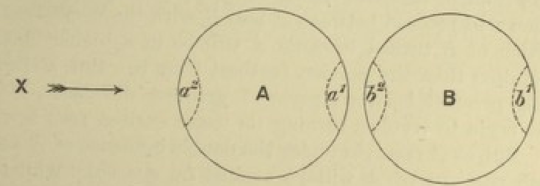

FIG. 37.

including those of magnetism, polarity, electrolysis, chemical action, &c., a few of which we will now attempt.

Let us consider the behaviour of two 'molecules,' or small masses of matter, towards each other, under changing conditions. Our hypothetical molecules may be conceived to consist of two spheres, *A* and *B* (see Fig. 37). Again, the spherical shape is not essential to our argument, but is only assumed for the sake of convenience. These two little spheres we will suppose to be 'contiguous' to each other, in the sense in which Faraday used this term[1]. Let now one of these spheres be exposed to the influence of a source of excitation, say the sun, whilst the other is shielded behind the first. In the above diagram *X* is the source of excitation. (It is

[1] 'I mean by contiguous particles those that are next to each other, not that there is no space between them.'

supposed also that, whilst A and B are free to revolve, they cannot move out of position.)

At first let us suppose A and B to be in relative equilibrium with each other. Then, exposing A to the excitation coming from X in the direction of the arrow, that part of A marked a^2 would be thrown into a higher state of excitation than the rest of the mass. In that case the attraction between X and the portion a^2 of A would be less than between X and the remaining part of A; a^2 would, therefore, be 'repelled' from X; more correctly, however, a^1 would now be attracted by X with a greater force than a^2. For similar reasons a^2 would be attracted by B with a greater force than a^1; inasmuch as the part a^1 is in equilibrium with B, whilst the part a^2 is now in a higher state of excitation. The obvious result of this will be that A will turn under this double influence, approaching the part a^1 nearer to X and a^2 nearer to B.

As A turns its more highly excited part towards B, equalization will proceed between A and B, with the result that the portion of B turned towards A will be in a higher state of excitation than the portion furthest from it. But, attraction being greatest between parts of greatest difference, B will now begin to revolve, turning its lesser excited part towards A. But, as A revolves under the double influence of X and B, fresh portions of A will be excited on one side, whilst excitation will be given off to B on the other; thus maintaining the conditions which have caused A to revolve; and in consequence all three bodies will continue to revolve until perfect equilibrium is established throughout their united mass.

We say 'all three bodies will continue to revolve'; for, while A is being heated by X, X will be cooled on the side nearest to A in proportion, and would, therefore, turn its more highly excited part (i. e. the part furthest from A) towards the latter.

Multiply the number of spheres indefinitely, and the same state of things would take place. The parts of the little spheres nearest the source of excitation would all be constantly 'positive,' and those furthest therefrom 'negative,' as shown in Fig. 38.

In this arrangement there is a 'positive' pole at X and a 'negative' pole at Z. Let this row of little spheres represent a bar magnet, and let it be divided into two at any part, as at *V* or *W*, and there would still be a 'positive' and a 'negative' pole in each part.

Let the source of excitation be changed from *X* to *Z*; or substitute at *X* a body in a lower state of excitation than the mass of spheres. In either case equalization would still proceed, but now in a reverse order; '*positive* electricity' proceeding from *Z* to *X*, and '*negative* electricity' from *X* to *Z*; hence the 'positive' pole of the little spheres would be in the part nearest to *Z* and the 'negative' pole nearest to *X*: and this would be equal to a 'reversal of current[1].'

Let equalization be established throughout the mass and also in respect of surrounding objects, then this interaction would cease. Or, let these little spheres be prevented from reciprocal adjustment—be in a state of coercion—as is the case with steel, and we should have a permanent magnet.

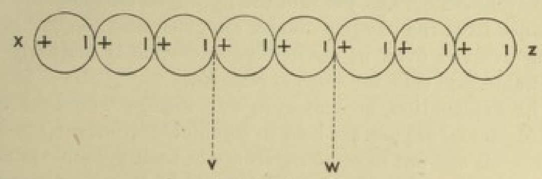

FIG. 38.

Again, the readiness with which these adjustments can take place will depend—

1. On the internal resistance;
2. On the degree of excitation.

And on these two elements depends the conductivity or electric property of any body.

Under this view the peculiar phenomena of 'polarity,' of 'inversion of currents,' and the fact that when a permanent magnet is divided into any number of parts each part still possesses a north and a south pole, are all simply explained; as are also the many peculiar phenomena of electro-chemistry, the explanation of which would be identical in principle with

[1] We would again remind the reader that we do not use the terms 'positive' and 'negative' electricity as being synonymous with what is called vitreous and resinous electricity, or as being different in kind. The sense in which we use these terms is purely one of relativity. The same electricity, or state of excitation, may be at the same time positive to one body and negative to another, as previously explained.

that just given. All that has to be done is to reduce in the mind the little spheres to an infinitesimal degree, so that they stand for the hypothetical atoms. As an illustration we will mention electrolysis, and shall now endeavour to show how the peculiar phenomena connected therewith can be explained on the above theory.

When water, for instance, is decomposed by means of an electric current, the oxygen is evolved at one pole of the battery and the hydrogen at the other. The two electrodes may, in fact, be in different vessels, provided there be communication between the two vessels. The peculiarity of the phenomenon is that the two gases, into which the molecule of water is broken up, escape separately and at a distance from each other. For, if we consider that, at the same electrode where oxygen is evolved, hydrogen must have been simultaneously liberated, and yet find the hydrogen escaping at some distance from the oxygen—under circumstances the two gases being evolved even in distinct vessels—this is certainly remarkable, and calls for some explanation.

The explanation, according to our theory, would be that hydrogen and oxygen are held in combination—or attract each other—on account of some *difference* of their specific states of excitation. And the explanation would be that the electrodes (or other substances) effecting such a separation must have exerted a greater attraction on the hydrogen and oxygen respectively than that which held these two elements in combination. This may be illustrated by two small bar magnets united to each other by their opposite poles. Should two stronger magnets be presented to this combination, the two smaller magnets would be separated, following the superior attraction of the more powerful magnets on either side.

Let us, then, assume oxygen and hydrogen to hold each other in combination on account of the difference in their specific states of excitation: regarding hydrogen as being 'positive' to the oxygen. To simplify our discourse, let us further arbitrarily assume the specific excitation of hydrogen to be represented by 100 and that of oxygen by 80. If, then, we wanted to separate hydrogen from oxygen, a 'force' acting in an opposite direction and greater than that holding the two elements in combination would be necessary. In other words, each element would have to be subjected to the influence of a body exerting a greater attraction on it than the attractive force of the element to which it is joined[1].

For instance, if two bodies of a specific excitation of 50 and 110, respectively, were presented to such a molecule, both the oxygen and the hydrogen would be more strongly attracted by these new elements than by each other: hence a separation of the molecule would be effected. This may be graphically represented as follows: In Fig. 39 let *B C*

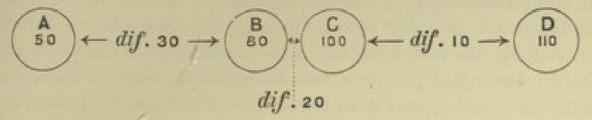

Fig. 39.

represent our compound molecule; *A* and *D* the two electrodes of a battery; the numbers expressing the supposed values of their respective specific excitations.

An inspection of this diagram will reveal the fact that, according to the foregoing theory, the molecule *B C* would at once perform a revolution of 180°, in order that *B* should be nearest to *D*, and *C* to *A*. And that because, their differences being greater, the attraction between *A* and *C* on the one hand is greater than that between *A* and *D*; whilst, on the other hand, the attraction between *D* and *B* is greater than that between *D* and *C*. Hence the relative state between the electrodes and the electrolyte would, after adjustment, be as follows:—

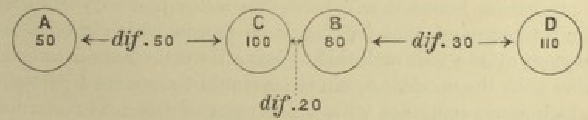

Fig. 40.

From an inspection of this diagram it will be seen that, whilst the attraction between *B* and *C* is equal to a difference of 20, that between *C* and *A* is equal to a difference of 50,

[1] By bringing a third body near a compound, it does not follow that the latter would necessarily be broken up: the third element might enter into combination, making a triple compound; in which case the former compound would have to be regarded as 'one molecule' reacting with the third atom.

and that between *B* and *D* equal to a difference of 30. So that *B* and *C* are each drawn in opposite directions with a greater force than that by which they are held together: hence their separation.

Thus may we conceive the decomposition of water to take place. The two electrodes of the electric battery, having a greater attraction, the one for the oxygen and the other for the hydrogen, than the force holding the two elements in combination, effect the decomposition of the water. The particles are literally torn asunder. That this is so is well proved by the fact that, when the electric disturbance is too feeble, decomposition does not take place. So far, then, our statement well agrees with known facts and accepted theories. It now remains to be shown in what manner our present theory can explain the fact that the two gases are evolved separately and at a distance from each other.

Let us first confine our attention to the pole at which oxygen is evolved, or the anode. Nearest it is a molecule of water, upon the oxygen of which the anode exerts a greater attractive force than does the hydrogen. Hence the oxygen leaves the hydrogen and combines with or is attached to the anode, whilst the hydrogen is set free. But this liberated (or nascent) hydrogen, having been disturbed out of its equilibrium, is in consequence in a higher state of excitation than the hydrogen of the adjacent molecule that has not been thus disturbed. The oxygen of the latter molecule would, therefore, have a greater affinity for this nascent hydrogen than for the hydrogen with which it is already in combination, hence it will leave the latter and combine with the former—and that for the same reason which caused the oxygen of the first molecule to leave its combination to combine with the anode. Again there would be nascent hydrogen, which again combines with the oxygen of the next molecule; and so on in turns.

A similar process, but in inverse order, takes place at the opposite pole—the cathode—which attracts the hydrogen. Thus, unless the anode and cathode chemically combine with them, the two gases are evolved at the respective electrodes.

Now, as soon as a gas bubble is evolved, it rises, on account of its greater buoyancy, to the surface of the water and escapes. This, however, is a secondary action, which, though taking place in consequence of electrolysis, is not dependent on it. The gas thus escaping sets at liberty the electrode, which then attracts another molecule in place of the one thus escaped. But if the evolved gases do not escape, or are not forcibly removed, the excitation of the electrodes is soon neutralized and the latter become inactive—a state known as polarization. The gas itself might under such circumstances act as an electrode; but in that case no further decomposition could take place, for then the excited oxygen, for instance, would combine with the just liberated hydrogen; so that for each molecule of water decomposed under such conditions a corresponding molecule would be formed. That is, the effect of the exciting gas surrounding the anode or cathode would be the same as that of the nascent gas when between the two poles, as above explained.

If attraction were merely proportional to mass and independent of states of excitation, then the liberated hydrogen atom near the anode (which attracts towards itself the oxygen) could not displace the hydrogen of the adjacent molecule of water, since on that theory they would be equivalent. The liberated hydrogen would have to rise at the anode where the oxygen rises, or else would have to travel through the liquid towards the cathode, in which case it would be seen to travel across the liquid. That, however, is not the case; the hydrogen which escapes at the cathode is not the identical hydrogen which was in combination with the oxygen escaping at the anode, but belonged to another and distant molecule. But, when once *states of excitation* are admitted as a factor, then it will immediately be seen that an atom of hydrogen in a higher state of excitation would possess greater attraction for oxygen than an atom of hydrogen already joined to and in equalization with it.

That such substitution by identical substances is possible is proved by the following fact. When iron wire is dissolved in sulphuric acid, the iron being in excess, at the end of complete neutralization of the acid a quantity of pulverulent metallic iron will be found in the flask as if it had been precipitated. The only possible explanation is that part of the iron already dissolved has been displaced by iron atoms from the yet undissolved wire, the latter being in a higher state of excitation, and acting on the solution like the anode of a battery. This clearly proves that one atom of iron can displace another atom of iron from its combinations, which it could not do if they were equivalent, since the inertia or resistance of the atom already combined with the acid would have to be overcome. But the fact that one atom of the same substance can thus displace another atom shows that they cannot be equivalent in every respect, and the only discoverable difference between two atoms of iron under such circumstances is that of *different degrees of excitation*. Nor is it possible to doubt that the condition we have italicized is a significant factor in the properties of all bodies, since every chemist is familiar with the important difference that exists between the nascent and ordinary conditions of the elements.

Now, the reactions here described are identical with our previous illustrations of molecular reactions. It is the same whether taking place between infinitesimal quantities or large masses; moreover the distinction between chemical and physical reactions is again a purely subjective one. For when two or more little spheres revolve in consequence of a disturbance in their states, as has just been explained, or when the radiometer revolves under the influence of light or heat, the phenomenon is considered as physical. But when it results in a severance or a rearrangement of the groupings of the atoms it is classed as chemical. The principle in both cases is, however, identical; the reaction in both being due to a difference of states and consequent processes of equalization.

To show that the distinction between chemical and physical phenomena is more subjective than real, owing to our inability to see what is taking place in the one case, it is only necessary to magnify in imagination the particles or atoms thus separated. Or, better still, take large masses of a substance, by which the process of electrolysis can be made visible. Two magnets attracting each other may be taken as representing a combination analogous to that of hydrogen and oxygen[1]. If

[1] Further on we shall show that gravitation, or the attraction of bodies towards the earth, is due to identical causes, i. e. to the difference between the states of excitation of such bodies and the aggregate state of excitation of the earth. Under this view the weight, or gravity, of a body would necessarily stand in some relation to its state of excitation; the greater the one, the lesser would be the other; and hence the product of the two ought to yield constants. As that property of bodies which is called their 'specific heat' would correspond to what we call 'specific states of

now two other and much stronger magnets be brought near this combination from opposite sides, these two are easily separated, each of the smaller magnets being pulled in opposite directions by the stronger ones. But the separation of larger magnets by smaller ones cannot be effected in like manner.

Now, such separation of magnets would clearly be classed under 'physical' phenomena; yet were the magnets infinitely small, so that we could form no idea of their state in combination, or of the precise nature of the process of their separation, they would necessarily be classed as 'chemical,' or under some other category represented by distinct subjective conceptions.

The fact that the nature of the resulting compound of separate elements, such as sulphur and iron, for instance, is different from the properties of the combining elements can form no objection to this view; since, as we have already pointed out, the properties of united magnets also differ materially from their properties taken singly. And the fact that both sulphur and iron can again be recovered in their original states seems to us sufficient evidence that their inherent qualities have not been changed, but that their different behaviour as compounds can be explained by the aforementioned axiom, that *the behaviour of any mass will be the aggregate behaviour of its component parts.*

It is here, as everywhere in the sciences, a confusion arising out of the confusion of language. The identity of 'forces' has long been conjectured by philosophers, and, indeed, may be said to be a firm belief with every student of science. But the demonstration has hitherto been impossible solely on account of the distinctive terminologies in the

excitation,' the reason why atomic weights when multiplied with their specific heat give mostly numbers approximating to constants will thus become apparent. In respect of gases, for instance, it is known that they combine with each other in equal volumes or multiples thereof. The higher the specific state of excitation of such a gas, the less will be its weight (for reasons to be shown hereafter) and vice versa. In the case of solids we may assume the same law to hold good; i.e. that the combination is according to volumes when in the gaseous state, but that such bodies are, under the normal conditions of our earth, in coerced states. At an extremely low temperature, for instance, many of our gases would be in a liquid state, whilst others would remain gaseous; but the combination would still be according to 'volumes,' though the *apparent* volume of such a liquefied gas would be much less than belongs to it, owing to its coerced state.

different departments of knowledge and the consequent multiplicity of distinctive conceptions. It is for this reason that we have deemed it necessary to abandon old terms as much as possible and to substitute more general terms which would make a comparison of the several facts with each other possible. Faraday, who perhaps more than any other man has striven to demonstrate the identity of 'forces,' felt the necessity for the adoption of such a general terminology in respect of related sciences. 'Whatever idea we employ to represent the power [of magnetism] ought ultimately to include electric forces,' he writes, 'for the two are so related that one expression ought to serve for both.' Unfortunately, he himself has added to this confusion by distinguishing between para- and diamagnetism, as has been shown.

CHAPTER XVI

HARDNESS AND SOLIDITY

Some bodies resist a separation of their parts more strongly than others; and the all-too-ready explanation of the cause thereof is another nymph or naiad bearing the name of Cohesion. We shall now inquire briefly into the mystery of this 'force' as we have done in the case of the others.

It has been shown in a previous chapter—

(1) That bodies pass from the liquid to the solid state when the particles of the mass cannot readily adjust themselves with each other, and hence press against each other.

(2) That when in this state they are in a state of coercion, hence in a state of excitation, which accounts for the heat manifested at the time of congelation or crystallization.

(3) That when the particles are melting they are again free to move, and to place themselves in adjustment with each other.

Free mobility, then, is an essential condition of adjustment in all matter. Where this adjustment is impossible, coercion, and, in consequence thereof, excitation, will result; which latter may manifest itself in the various familiar manners. But when equalization is thus prevented from taking place this tendency of the particles is not destroyed. Let, for example, two bodies press in opposite directions, and let the cause of this pressure continue; then the two will form a 'solid,' and the force by which they could be separated would have to be equal to the force with which they are urged against each other. As instances of such 'solids' we may mention the Magdeburg hemispheres, which when the air is exhausted form such a 'solid'; or we may take two magnets attracting each other, when the force necessary to effect a separation will be in exact proportion to their reciprocal attraction; or, again, the pith-ball pendulum when attracted by a glass rod. All these are but particular illustrations of a universal principle.

We have thus arrived at a probable theory of solids, which is simply another form of resistance—the particles resisting separation. In one of our opening chapters we have illustrated the law that every body retains every newly-acquired state by showing that, whatever amount of energy is required to change the state of a body, say from rest to motion, the same amount of energy would subsequently be required to reverse the process. Thus, when a railway truck is being moved, it would require the same amount of energy to bring it to rest again as was originally required to set it in motion (including the amount of energy which was necessary to overcome its resistance, and which we have called 'latent energy'). If, therefore, two particles attract each other and are pressed against each other with a certain amount of energy, it would require a corresponding force to separate them again, even after the impelling force is no longer acting.

Of course we are speaking here of the power with which bodies mutually attract each other, as above explained, and do not include dynamic effects, such as the acts of man or those coercive forces which act contrary to the natural tendencies of bodies. By merely pressing two bodies against each other we could not unite them against their molecular tendencies. And where this may be done it is not merely in virtue of the pressure exerted, but in consequence of the collateral conditions with which such pressure may be accompanied. To make bodies adhere or cohere they must be made to attract each other in accordance with the law above explained [1].

Suppose that we melt a mass of iron, and then allow it to cool. Manifestly the outside would cool more quickly than the inside; hence an interaction between the outer and inner particles would take place. While yet free to move, the

[1] Just as by rubbing two bodies against each other the degrees of heating or electrification of the two bodies will be unequal, whereby they can mutually attract each other, so is it conceivable that by pressing two bodies against each other a like difference in state of excitation might result in consequence of the pressure, which in case of some bodies may result in mutual attraction.

particles may move towards each other bodily [1]; but when this is no longer possible, the mass having become too sluggish, equalization may still proceed by conduction. But, the cooling process proceeding much faster than the power of conduction, the outer particles would be attracted inwards, and the inner particles would be attracted from the centre towards the outer surfaces, pressing against each other. On this theory the resulting hardness of the melt would be proportional to the force with which these particles have been drawn towards each other. The hardness of iron would thus depend on the difference of temperature between the outer layers and the centre, modified by the degree of conductivity which iron possesses. For the present we may leave this latter condition out of consideration, and simply attribute the hardness of the mass to the force with which the particles pressed against each other during the cooling process, and which, according to theory, would be proportional to the difference of internal and external temperatures.

Now, if this reasoning be correct, then the greater this difference of temperature the harder ought to be the resulting body. And this, as is well known, is actually the fact. If a casting be made in cold weather, or be artificially chilled, a very hard mass is obtained. Castings can in this way be made, and are being made for certain purposes, which equal steel in hardness; whereas if the cooling is allowed to proceed very slowly the castings are soft. Annealing and the hardening of steel are other instances which lend support to this view.

Another remarkable phenomenon, corroborative of this theory, is that the centre of a casting is never so hard as the other portions; and that because the particles at the centre all press outwards, and those of the exterior inwards.

We have obtained interesting experimental verification of this theory both with metals and with other substances. A fatty body, having a melting point of 52 °C., and whose normal congealing point was 38° C., was melted and heated to about 200° C., and then divided into two portions in similar test-tubes. One of these was immersed in a freezing mixture, and

[1] In accordance with our principles we are led to conclude that the hot and cold particles would attract each other, as being in different states of excitation. But here the influence of the earth would have to be taken into account, which would attract the colder and specifically heavier particles downwards, thus forcing the hotter and lighter particles upwards.

the other in hot water, and they were then left till the temperatures of both were equal. The processes to which these two portions were submitted were respectively analogous to those which are called 'hardening' and 'annealing' in respect of metals and glass; hence we shall designate the fat cooled in a freezing mixture as 'hardened,' and that cooled in hot water as 'annealed.' When subsequently examined these two substances were in appearance and to touch as unlike each other as if they had been distinct substances—the one being white, hard, and brittle; the other yellowish, dull, and soft. Their melting points, too, were considerably apart, as became manifest by immersing two lumps of equal size in a basin of hot water, the annealed portion melting much more quickly than the hardened one.

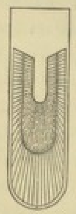

Fig. 41.

It has long been an embarrassing observation with chemists that the melting points of fats and other organic substances vary, hardly any two successive experiments with the same substance agreeing with each other; and the discrepancies between the results of different experimenters on the same substance, as published in the different tables, are still greater; due, no doubt, to the difference in conditions at different times and places being greater than when two or more experiments are carried out by the same person under conditions more nearly alike. These embarrassing variations will find a natural explanation by what has just been said.

But there was another notable circumstance in connexion with these experiments. There was a hole in the centre of both these congealed masses, and in the hardened portion the mass was at the same time drawn away from the sides of the test-tube. In Fig. 41 a slightly exaggerated view of the congealed fat and test-tube is given in section[1].

From this illustration it will be seen that the particles nearest the glass were drawn inwards, those at the centre being at the same time drawn outwards. The point of attraction must, therefore, have been somewhere between the centre and exterior, as represented by the dotted line.

We have repeated the experiment with lead, and have cast

[1] When water is frozen in a test-tube, by immersing the latter in a freezing mixture, the *radiations* towards the centre are made beautifully visible.

some molten lead into a test-tube about three-quarters of an inch in diameter, and then immersed the tube in cold water. Of course the glass was shattered into fragments by the immersion, leaving a cylindrical bar of lead, about five inches long, and with a hole in the centre about three-sixteenths of an inch in diameter and about one inch in depth. The lead was sensibly harder than a similar casting which was allowed to cool slowly. The same experiment was also tried with sulphur and a variety of other easily fusible substances. But the phenomena of hardening and annealing are so familiar that all we need say here is that the principle must be regarded as applying to all substances without exception.

However, in attempting to account for hardness on this principle the conductivity of substances must be borne in mind. Attraction will be due to difference in states of excitation; but substances of great conductivity would quickly equalize in consequence thereof, and thereby lessen the intensity of these molecular attractions, resulting in a correspondingly softer body. In the case of copper, for instance, the hardness, under like conditions, could not be correspondingly greater by cooling as in the case of iron, on account of the greater conductivity of the copper, which greatly facilitates equalization throughout the mass by conduction; hence the particles would not be urged against each other with such intensity. But particles thus drawn against each other would afterwards retain this state with a proportional intensity: hence, the more strongly they were drawn against each other, the greater would be the amount of energy required to separate them again.

We would offer this theory to account for hardness very guardedly and as one requiring further confirmation. But the theory seems to be in accordance with the general principles we have been discussing, and which we have succeeded in verifying in so many different ways. Certain it is that bodies will attract each other with a greater intensity in proportion as their difference in states is greater. This, combined with the other equally certain law that bodies retain any newly acquired state by virtue of their persistence (or *vis inertiae*), would in itself lead to the conclusion here set forth. Experiments, as far as these have been possible, all tend to confirm this view, as do also many natural phenomena hereafter to be mentioned. What is against it is that, according to it, the melting points should be a guide to the hardness of different substances. But then the important factor of conductivity would have to be taken into account. Thus the conductivity of ice, as has been proved by Faraday, is almost *nil*. This would account for ice being so hard notwithstanding its low melting point as compared with other substances. It is, in fact, not merely a question of melting points, but of *difference of states*, or differences of temperature between the interior and exterior mass. But this difference of states would not depend merely on the difference of the temperature of the melt and that of the cooling medium, but also on the conductivity, or specific resistance, of the substance itself at that particular temperature.

On the other hand, there are many considerations, besides those already mentioned, which lend great support to the theory. Carbon, for instance, could assume its crystalline form only at an extremely high temperature; at the same time it is a very bad conductor; both of which qualities would tend to produce great hardness; and, as is well known, the diamond belongs to the hardest of known bodies. The same may be said of spinel and other precious stones. Quartz, too, has a high melting point and is of a very slow conductivity. And if hardness be due to the conditions of formation; that is, if the tenacity with which the particles cohere be proportional to the intensity with which they have originally been impelled towards each other: then the great hardness of these bodies, notwithstanding their lightness as compared with other bodies of much lesser hardness, would receive a simple explanation in conformity with universal principles.

That the hardness of substances can be increased or diminished in the manner here pointed out is certain. *And, if any quality can thus be increased or decreased, the means by which this can be done must be regarded as the cause of the quality itself.* We have no doubt that when this view is adopted by other workers more and more confirmatory evidence will be brought forward. As it is, a number of recorded phenomena which have hitherto remained unexplained find their natural explanation in the law of persistence; i.e. that bodies retain every acquired state with an energy proportional to that with which this state has been acquired. We have already cited the variable hardness and melting points of fats, which have hitherto been a standing source of embarrassment to chemists, as confirmatory evidence. As other confirmatory evidences we may refer to the remarkable phenomena exhibited by solid carbonic anhydride and liquefied air. These substances can be liquefied, or solidified, with great difficulty only. In the light of current conceptions one would expect liquefied air to be extremely volatile; in fact, one would have expected that as soon as the conditions under which air has been liquefied are removed it would expand, after the manner of compressed air. Instead of this it seems, from the accounts of Professor Dewar, to be a liquid of considerable stability, which can be kept in open vessels, and to volatilize which, for the purpose of obtaining intense cold, a vacuum pump is applied! In like manner solid CO_2 can be handled and freely exposed to the air; and it is stated that a piece of this solidified gas, about seven inches long and two inches in diameter, required five hours for complete volatilization.

These phenomena seem to us explicable only on the principles here elucidated. The air resists a change of state up to a certain point. But when its resistance is neutralized it retains this new state with a proportional intensity, as has been explained in the chapter on Relative Resistance in connexion with a moving railway truck.

We might, perhaps, mention yet another circumstance, which to our thinking points in the same direction. If a sheet of paper be rubbed with a piece of india-rubber, and then placed on a smooth surface of wood or glass, it is pulled towards the latter. By rubbing the paper it has, of course, been excited, and the more it has been excited the more will it cohere to the wood—thus again confirming the theory that bodies attract each other proportionally to difference in states.

Hardness may, then, be regarded as resistance due to the energy with which the particles have originally been impelled towards each other.

Thus, magnetism, attraction of bodies excited by rubbing, hardness, solidity, &c., are all explained by one and the same principle, viz. *as being due to mutual attraction*, and in this sense are all identical with gravitation.

We have put forward this theory in order to show that many other phenomena besides those directly bearing on the subject of our inquiry can be explained without any forcing by the few principles we have adduced, and that experiment and observation, wherever such are possible, readily fall in with the theory. The subject might be enlarged on, and more evidence adduced, were it not that space forbids.

BOOK IV

GRAVITATION

CHAPTER I

INTRODUCTORY

To account for a law of nature means, and can mean, nothing more than to assign other laws more general, together with collocations, which laws and collocations being supposed, the partial law follows without any additional supposition.—Mill.

In the preceding book our main object has been to show:—

Firstly, that the different groups of phenomena commonly attributed to distinct agencies or 'forces'[1] are all explicable by one and the same set of principles, and that the so-called 'conversion of forces' resolves itself into a study of phenomenology, and not into a study of different 'forces.'

Secondly, that the principles we have adduced are capable of affording simple, intelligent, and harmonious explanations of most dissimilar manifestations.

One condition generally (but erroneously, as we believe) required of a theory as proof of its truth, viz. 'its being able to explain,' is thus complied with. But we do not rely on this proof, since we hold this view to be fallacious. Any

[1] We trust the critical reader will not think us guilty of deliberate misrepresentation because of our insistence on using a term which has been abandoned by modern philosophers. In strictness of phrase we should speak of 'forms of energy'; but the phrase is clumsy and unwieldy, whilst 'force' is more euphonious. And any difference beyond that of sound we are unable to discover. True, 'force' may be understood in different ways; but so may 'form of energy.' We here merely substitute 'force' for 'form of energy,' leaving it to the reader to give to this term the same meaning as he would to its Greek synonym.

theory deduced from the same body of facts which the theory is to explain is sure to agree with the facts, since in such a case the theory mostly resolves itself into a restatement of the observed phenomena in other words. To give but one illustration: a magnetic needle inserted in the circuit of a thermopile, or other battery, will swing in opposite directions under varying conditions; from which fact is deduced the theory (hypothesis would be more appropriate) of two fluids. By subsequently explaining the phenomenon and showing that the needle does move in opposite directions 'as required by theory' is no proof at all, but merely a reversal of the first process.

Nor is the fertility of a theory, i. e. its leading to new discoveries, admissible evidence of its truth; since in most cases these new discoveries follow from empirical knowledge, and not from the theory—the theory being in such case merely a kind of *memoria technica*, to commit to the memory empirically-acquired knowledge, or to facilitate discourse on such subjects. Who would now question the error of the phlogiston theory? Yet this was as capable of fertility, within the particular group of phenomena to which it related, as the oxygen theory. Sir Humphry Davy might have discovered the metal potassium by 'phlogistonizing' potash, as well as by deoxidation.

It is different, however, when a theory is deduced from general principles, our knowledge of which is not based on partial observation, and which have not been deduced from that particular group of phenomena which they are to explain. In such a case the 'ability to explain' may be regarded as evidence of some moment, since every agreement of a fact with a theory thus deduced may be regarded in the light of an *a posteriori* verification.

The theories, or explanations rather, we have offered of such diverse phenomena as heat, electricity, magnetism, &c., have all been deduced from the same set of principles, viz. persistence, reciprocity, and equalization. These principles themselves are not mere figments: they are demonstrable in every phenomenon that is accessible to observation—as was indeed to be expected of any universal principles. Persistence and reciprocity have been enunciated by Newton (first and third laws); while equalization follows of necessity from the law of reciprocity (or Newton's third law). But even as regards the law of equalization, though deducible from the law of reciprocity, we need not rely merely on argument. The law is demonstrable in every observation or experiment within human reach; and the knowledge of this *unrecognized* law is coextensive with human experience. For, though the law is unrecognized, nobody would expect to warm one body by another, or to accelerate the motion of one body by another, unless the temperature or velocity, as the case may be, of the one is greater than that of the other; and even then the extent of possible change will beforehand be calculated on the assumption that when the temperatures or velocities of two such bodies are equal the end of the reaction will be reached.

Many were the illustrations and experiments we have cited; and we venture to affirm that there is no principle in natural philosophy supported by stronger or more universal evidence than are the principles just named. It is from these we have deduced our theories whereby to account for most dissimilar phenomena. Not only have we shown that such manifold and dissimilar manifestations can be explained by the aid of the above principles, but we have found simple and adequate explanations of many as yet unexplained phenomena; and that, be it remembered, *without the aid of any occult agencies or imponderabilities*, but always on strictly physical principles which fall within the possibilities of experimental verification.

In not a few instances our deductions seemed astounding, inasmuch as they ran counter to what are regarded as well-established theories, and with some writers even as 'basic facts' of physical science. In all such cases we have not contented ourselves with proving—either from observation or experiment—the truth of our deductions, but have devoted considerable time and space to the discussion of the rival theory and an examination of the evidence on which such theory is based. In every such case of conflicting theories we believe we have succeeded in demonstrating the error of the current theory. We will only mention the doctrine of Joule concerning the mechanical equivalent of heat, and Sir William Thomson's doctrine concerning the dissipation of energy, discussed in a previous part.

We have shown heat to be due to resistance or coercion (i.e. to be due to arrested motion rather than a mode of motion);

that electricity, magnetism, sound, and light result practically from like conditions; and that the conversion of the one into the other resolves itself into a conversion of sensations, or modes of observation, rather than a conversion of supposed occult agencies. Under such circumstances we believe we are entitled to regard every agreement of fact with deductions made from such universally-established principles as a true *a posteriori* verification, which helps to establish the more firmly the truth and universality of the principles from which such deductions have been made.

There are two more points which we may urge as evidence of some moment. One of these is the uniformity of explanation of most dissimilar phenomena, by which all phenomena are brought within one all-embracing general induction, which well accords with the modern scientific idea of the unity of nature and identity of principles in all natural manifestations. Were such a principle, that can explain all phenomena by one and the same supposition, a mere hypothesis, it would in itself be a great step in advance, inasmuch as it would enable the most discordant observations to be ranged under one universal and all-embracing conception. But when, as in the present case, the principle (or principles, though the three laws are really but different aspects of one and the same principle) is manifest everywhere around us, known to everybody, though unrecognized—and that merely because it does not always fit in with accepted theories—its recognition and acceptance must be of all the greater moment.

The other point which is greatly in favour of the principles advanced is that deductions made therefrom lead to the discovery of errors. For whenever a deduction from these principles conflicted with accepted theories we were careful to examine both theories, and for the most part have succeeded in discovering an error in the old theory.

We have shown, then, (1) the truth of the principles both by observation and experiment; (2) that theories deduced therefrom are verifiable by facts; (3) that most dissimilar phenomena yield simple, rational, and for the most part easily verifiable explanations; (4) moreover, that there is no assumption of any imponderability or occult agency in any of the explanations; and (5) that the different 'forces' or agencies are but different manifestations of identical principles (unity of nature).

Having shown, therefore, that the various supposed 'forces' are all but manifestations of these principles, we might well include the phenomena of gravitation among the rest, and forthwith proceed to apply these principles to the explanation of the phenomena of gravitation, which, as will presently be seen, are easily explicable by the same set of principles. There certainly are no *a priori* reasons why the phenomena of gravitation should alone make an exception to these universal laws. Indeed, the laws of persistence and reciprocity have been deduced by Newton from the phenomena of gravitation, and at present are applied to no other group of phenomena to the same extent as they are to those phenomena. We have shown these principles to have a more universal application than was supposed by Newton, which circumstance should only strengthen their importance in connexion with the phenomena of gravitation. Presently we shall show that the law of equalization is also applicable; and that, though not demonstrable in respect of terrestrial gravitation, the principle may be seen at work in celestial bodies.

History, however, warns us that in advancing a new theory no amount of positive proof in support of such theory is sufficient unless the older theory already in possession of the mind, and obstinately urging its rights of possession, is first dislodged by showing its errors. Before attempting, therefore, to apply the principles we have hitherto adduced to the explanation of the phenomena of gravitation, we shall briefly review current theories so far as they bear on *the explanation of causes*, and examine the evidence on which such accepted theories are based. The plan we shall adopt will be as follows: (1) We shall briefly consider the teachings of Newton and then review the Newtonian theory, pointing out how much of the latter is based on fact and how much of it is mere assumption. (2) We shall consider the value of mathematical proof as bearing on *causes* of natural phenomena. (3) We shall show that astronomical results are based on empiricism, hence are no evidence of the truth of the theory to which such results are commonly attributed, and which results are generally advanced as proof of the theory itself.

Having done so, we shall proceed to explain the causes of terrestrial gravitation, and offer such evidence of the truth of our theory as we have been able to collect either from general observation or by carefully devised experiments. Subsequently to this we shall show that the planetary motions, with all their regularities and irregularities, are deducible from the same principles, and that many discrepancies, or hitherto unexplained phenomena of the heavens, find a simple and harmonious explanation by the same laws by which all terrestrial phenomena can be explained.

No doubt our explanation will in parts look very unlikely in the light of accepted theories; but if the reader will have patience to follow us through the whole of the argument he will find in the later chapters every seeming discrepancy dealt with and objections fully considered.

CHAPTER II

THE TEACHINGS OF NEWTON

I wish we could derive the rest of the phenomena of nature by the same kind of reasoning from mechanical principles; for I am induced by many reasons to suspect that they may all depend on certain forces by which the particles of bodies, by some causes hitherto unknown, are either mutually impelled towards each other, and cohere in regular figures, or are repelled and recede from each other; which forces being unknown, philosophers have hitherto attempted the search of nature in vain; but I hope the principles here laid down will afford some light either to that or some truer method of philosophy.—NEWTON.

BE it understood, once and for all, that by the Newtonian theory we do not mean the teachings of Sir Isaac Newton as laid down in his immortal work, but rather the modern theories currently attributed to Newton. The two are far from being identical. Sir Isaac Newton's position as put forth by himself is practically unassailable; for, though in his work there are many assumptions without adequate foundation in fact, and some even which can be proved to be untrue, he has never put them forth as more than bare conventional assumptions, and has never allowed himself to draw any inferences from such assumptions. And, as we are about to submit the theories associated with the name of the great philosopher to a rather searching criticism, it is but justice to draw attention to the fact that the errors we shall point out are not those of the master himself, though he is commonly credited with the doctrines in which they are embodied.

Sir Isaac Newton was too careful a thinker to affirm anything which he could not prove positively, and hence he carefully avoided touching on anything which related to *causes*. His object was to determine quantitative relations only; and, though for convenience of discourse it was necessary to *assume* causes, he was ever careful to point out that such assumptions were made merely for convenience of expression, and repeatedly warned his readers that such assumptions were not to be regarded as facts or even as probable theories. 'For I here design only to give a mathematical notion of those forces, without considering their physical causes and seats [1].'

The paragraph of which these words form the concluding part, as well as the words following, are worth quoting in full, as they not merely define the scope of the task which Sir Isaac Newton set himself, but are full of warnings to the reader lest he might mistake phrases necessary for purposes of discourse for actually proven facts. We have italicized portions to which we wish to draw particular attention:—

'These *quantities* of forces, we may *for brevity's sake call by the names* of motive, accelerative, and absolute forces; and *for distinction's sake* consider them with respect to the bodies that tend to the centre; to the places of those bodies; and to the centre of force towards which they tend: that is to say, I refer the motive force to the body, as an endeavour and propensity of the whole towards a centre, arising from the propensities of the several parts taken together—the accelerative force to the place of the body, as a certain power or energy diffused from the centre to all places around to move the bodies that are in them; and the absolute force to the centre, as endued *with some cause*, without which those motive forces would not be propagated through the spaces round about; *whether that cause is some central body* (such as is the loadstone, in the centre of the force of magnetism, or the earth in the centre of the gravitating force) or anything else *that does not yet appear. For I here design only to give a mathematical notion of those forces, without considering their physical causes and seats.* . . . I likewise call attractions and impulses, in the same sense, accelerative and motive; and use the words attraction, impulse or propensity of any sort towards a centre promiscuously, and indifferently, one for another; *considering those forces not physically, but mathematically*: wherefore the reader is not to imagine that by those words I anywhere take upon me *to define the kind, or the manner of any action, the causes or the physical reasons thereof, or that I attribute forces, in a true and physical sense, to certain centres* (which are only mathematical points); when at any time I happen to speak of centres as attracting, or as endued with attractive powers.'

Again, in his *System of the World*, and therefore *after completion* of his investigations, and after having reviewed all the evidence he was able to collect, he says:—

'But our purpose is only to trace out the *quantity and properties of this force from the phaenomena* [2], and to apply what we discover in some simple cases as

[1] *Principia* (Davis' edition), vol. i. p. 5.

[2] Here in our edition an asterisk refers us back to book i, proposition lxix, theorem xxix, scholium, where we read as follows: 'I here use the word attraction in general for any endeavour, of what kind soever, made by bodies to approach to each other—whether that endeavour arise from the action of the bodies themselves, as tending mutually to or agitating each other by

principles by which, *in a mathematical way*, we may estimate the effects thereof in more involved cases; for it would be endless and impossible to bring every particular to direct and immediate observation.

'We said *in a mathematical way* [these italics are Newton's own, the rest are ours] to avoid all question about the nature or quality of this force, *which we would not be understood to determine by any hypothesis*, and therefore call it by the general name of' . . . . &c.

Newton's position in regard to qualities or causes of gravitation is here clearly defined. Not being able to arrive at any satisfactory theory, he contented himself with 'deducing' from the phenomena 'the quantitative relations of the forces in a mathematical way'; and he went only so far in his speculations as strict empiricism enabled him to do; and, as a true philosopher, affirmed positively only that about which he had positive evidence. But this did not include 'gravitation,' as is so commonly supposed. The term 'gravitation' was to Newton a mere word descriptive or expressive of certain phenomena, and nothing else. He did not discover 'the force of gravity,' as is vulgarly supposed by those who delight in retelling the fable about the falling apple: apples were known to fall to the ground before Newton's time. Nor did he acquaint us with the nature or quality of gravity, but merely determined the quantitative relations of those manifestations which are commonly attributed to the mystical force called gravity. At most it may be said that he affirmed the identity of those forces which make bodies on or near the earth gravitate towards some point thereof with those which hold the planets at certain distances from the sun. In this respect he may be regarded as the pioneer of that philosophy which ever since his time has tended to diminish the number of distinct, independent, and often antagonistic forces, and which has led to the recognition of the unity of nature and identity of causes. But, so far from determining the nature of gravity, he did not even affirm its existence. 'Not that I affirm,' he says, 'gravity to be essential to bodies: by their *vis insita* I mean nothing but their *vis inertiae*.' And then he adds, '*This is immutable. Their gravity is diminished as they recede from the earth*[1].'

spirits emitted, or whether it arises from the action of the aether or of the air, or of any medium whatsoever, whether corporeal or incorporeal, anyhow impelling bodies placed therein towards each other. In the same general sense I use the word impulse, *not defining in this treatise the species or physical qualities of forces*, but investigating *the quantities and mathematical proportions of them*, as I observed before in the definitions.'

[1] Vol. ii. p. 162.

And this, in truth, is all that he affirmed positively concerning the quality of gravitation, as in point of fact it was all he was able to demonstrate.

Unlike most writers who invent theories and who are anxious to have their creations adopted as literally true, Newton seemed constantly to have been afraid that his readers might accept more as true than he himself was prepared to accept: hence, we presume, the repeated and emphatic warnings throughout his works. And even on the last page of his *Principia* we find:—

'But hitherto I have not been able to discover the cause of those properties of gravity from phaenomena, and I frame no hypotheses; for whatever is not deduced from the phaenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy.'

In other words, at the end of his investigations he came to the conclusion that he had not sufficient evidence to frame even a hypothesis as to the causes of the mutual attraction of bodies. All he proved was that bodies tend towards each other with an intensity which increases inversely as the square of the distance, and which corresponds to a certain force. But he did not say what that force was, nor even that it existed; but merely that, supposing that the phenomena were caused by a certain definite force, then that force would have to be of such and such a quantity. Yet it is to this careful writer that is attributed the discovery of two forces, a centripetal and a rectilinear one, to which, as is so positively asserted in modern text-books, are due the circumsolar motions of planets!

Newton's position may be summarized as follows: Given two forces (he said in effect), one pulling towards a centre and the other urging forward in a straight line, then a body thus acted on will perform a curvilinear motion. And, assuming further the circumsolar motions of the planets to be due to two such forces, then from the phenomena we can deduce the relative quantities of two such forces *capable of producing* the observed phenomenon. The phenomena of other planets may be treated in like manner, and then all their motions may be referred to a uniform standard. And what is that unit to be? Simply a unit of the assumed force, a certain quantity of mechanical power expressed in any convenient terms. As such a convenient term of a unit Newton employed the term 'mass.' But 'mass,' in the sense in which Newton used it, is synonymous with weight, or pressure; expressive of quantitative relations as measured by relative effects. Thus, if two bodies were found by experiment to perform under like conditions the same amount of work, their 'mass,' 'force,' 'power,' or 'energy' were considered as being equal, there being no other standard of measurement than the work performed. Bulk was to be disregarded, since otherwise the unit could not have been maintained. *A* and *B* were of equal 'mass' when they performed equal work; and, if *A* happened to be twice the volume, it still contained the same mass only, or say the same weight—just as a cubic foot of iron is about seven times the weight of a cubic foot of water, and will, therefore, under like conditions, perform seven times the amount of work by falling. All these assumptions are perfectly legitimate in the sense in which Newton made use of them, 'to determine quantitative relations in a mathematical way,' by referring all observed phenomena to some uniform standard of comparison. This is the Gospel of Gravitation according to Sir Isaac Newton.

But according to his followers it is assumed to be incontestably proven that gravity is inherent in matter, is proportional to mass, and manifests itself in mutual attraction only. The planetary motions themselves are not held to be phenomena that still require to be accounted for, 'since there is mathematical proof,' as it is confidently asserted, that these motions are actually due to two such forces as Newton assumed as a mere matter of convenience, for the purpose of referring observed effects to a uniform standard. Into the value of this supposed mathematical proof we shall presently inquire somewhat more fully, and show, not that it is fallacious, but, looked at in the light of explaining a physical phenomenon, utterly worthless.

We have shown that Newton carefully abstained from assigning any cause or giving any explanation concerning the phenomena of gravitation. 'I do not even form an hypothesis,' are his words; and, indeed, throughout his works he did not even suggest a probability, or so much as hint at a probable theory, of these mystical phenomena. Is it possible that a man should spend his life in brooding over such problems, should play for years on the strand of the ocean of truth—to use a metaphor of his own—without ever giving a thought as to the probable origin of those precious pebbles he was fortunate enough to pick up there? We doubt it, and have good reasons to do so. Newton did speculate on it a good deal; but because he could not see in gravitation a force or agency different from that which causes all other phenomena; because he could not persuade himself that gravity was a force *sui generis*, and yet could not find a connecting link between the phenomena of gravitation and all other phenomena around him; he abstained from offering any suggestion, and warned his readers against hypotheses, 'whether metaphysical or physical, whether of occult qualities or mechanical.' He could not deduce an explanation from the phenomena in the same way as he could deduce quantitative relations, so he abandoned the task as one impossible of solution. Certain it is that no explanation would have satisfied him that could not be applied universally to all phenomena in the same way as his quantitative determinations. Our ground for this remark is the following and concluding paragraph of his *Principia*, which, concise and eloquent, gives us a clear insight into Newton's mind concerning the subject here alluded to:—

'And now we might add something concerning a certain most subtle spirit which pervades and lies hid in all gross bodies; by the force and action of which spirit the particles of bodies mutually attract one another at near distances, and cohere, if contiguous; and electric bodies operate to greater distances, as well repelling as attracting the neighbouring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; *and all sensation is excited, and the members of animal bodies move at the command of the will*, namely, BY THE VIBRATIONS OF THIS SPIRIT *mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles*. But these are things that cannot be explained in few words, *nor are we furnished with that sufficiency of experiments* which is required to an accurate determination and demonstration of the laws by which this electric and elastic spirit operates.'

This paragraph explains why Newton was so careful to guard against formulating a theory, and also shows us what kind of theory could alone have satisfied his mind. It would have to account, not merely for bodies falling to the ground and planets revolving in their orbits, but also for the cohesion of contiguous bodies; for the emission, reflection, refraction, and inflection of light; for electrical phenomena; for repulsion as well as attraction; for the heating of bodies; and for sensations, i. e. for the coursing of blood in animals 'from the outward organs of sense to the brain and from the brain into the muscles.'

Who after reading this passage can say that Newton did not try to formulate a theory concerning the causes of the phenomena the study of which was the main object of his life's work? And yet not a theory, not a hypothesis, even so much as hinted at. Truly a great philosopher, not so much for what he has bequeathed to us as because of his command of self in discriminating between facts and fancies, and leaving to the world such parts of his work only which could stand the test of time.

> 'Was glänzt, ist für den Augenblick geboren;
> Das Echte bleibt der Nachwelt unverloren.'

And that which Newton affirmed as true is true to-day; and what there is false or erroneous in a 'Newtonian theory'—that is, theories unjustly attributed to Newton or associated with his name—is but the ivy which since his time has grown round his beautiful and solid edifice.

CHAPTER III

REVIEW OF THE NEWTONIAN THEORY

In order to seek truth it is necessary once in the course of our life to doubt, as far as possible, of all things. As we were at one time children, and as we formed various judgements regarding the objects presented to our senses, when as yet we had not the entire use of our reason, numerous prejudices stand in the way of our arriving at the knowledge of truth; and of these it seems impossible for us to rid ourselves, unless we undertake, once in our lifetime, to doubt of all those things in which we may discover even the smallest suspicion of uncertainty.—DESCARTES.

THE current theory of gravitation postulates that gravity is inherent in bodies, since it affirms that gravity is proportional to mass. According to the law of reciprocity there can be nothing inherent in a body, but merely relative. Hence to find the cause of gravity it is necessary to discover in respect of what property or propensity there exists any relativity. Now from the laws of reciprocity and equalization it is not difficult to adduce, *in general terms*, that *the gravity of bodies for each other* (since by itself, and without reference to some other body, nothing could have gravity) must be due to some difference, some disturbed equilibrium; so that to solve the problem would mean to discover in respect of what quality there exists any difference. That this quality cannot be that of relative weight is obvious, since *weight* is only the measure of the intensity with which bodies are drawn towards each other. The weight or the gravity of a body is one and the same thing, and is but the manifest effect of some unknown cause. By making the name of a certain manifestation into an abstraction, a phenomenon is not explained, but more thoroughly mystified. *Gravity* (noun substantive) no more accounts for *gravity* (noun adjective) than does magnetism (the subtle quality of magnets) account for magnetism (the visible manifestation of magnets), or heat for temperature.

Yet the axiom that 'gravity is proportional to mass' amounts, as will presently be seen, to no more than saying that 'gravity is proportional to weight,' or 'gravity is proportional to gravity.' It is only necessary to inquire into the manner in which the *mass* of a body is ascertained, and to examine into the meaning of the term as used by Newton and by current writers respectively, in order to see that the latter have given this term a significance never intended by the former, and for which there is not the faintest shadow of justification. As has been pointed out in the preceding chapter, Newton employed the term 'mass' as a unit of comparison, his standard being *weight*. 'The quantity of matter is the measure of the same arising from its density and bulk conjointly. . . . It is this quantity that I mean hereafter everywhere under the name of body or mass. *And the same is known by the weight of each body.*' It is quite true that Newton regarded *weight* as the measure of *mass*, not only when comparing bodies of like substances, but also in respect of heterogeneous bodies—a fallacy based on a false analogy which we have discussed in the first book of this work. But in the uses which he made of the term the error was of no moment, since he merely 'deduced principles from the phenomena, and then the phenomena from the principles, *in a mathematical way*'; and his purposes would have been equally well served had he employed some algebraic sign to stand for a unit quantity of force. With Newton 'mass was proportional to weight'; but in current treatises 'weight is proportional to mass'; which is a distinction with a difference, since this *mass* is no longer regarded merely as a convenient term, or as a conventional unit of comparison, but as *a cause of attraction*, and the 'wonderful agreement between the weight and mass of a body' is now confidently put forth as 'most conclusive evidence' (!) of the truth of the theory of universal gravitation, and that gravity depends solely on 'mass.'

We have already shown (book i. ch. iv) that we have no knowledge as to what constitutes 'equality of mass' in respect of different substances; and that, whatever comparison may be made between any two dissimilar bodies, the relations will be true only under precisely the same conditions. This applies even to their weight. Equal weights of gold and iron in air will not be of equal weight in other media or in vacuo. (Further on we shall show that they are not of equal weights in the same medium at different temperatures.) Nor is there the slightest reason to suppose that, if removed to some other planet (say Venus or Jupiter), they would still equipoise each other. No doubt it is assumed that they would do so; but this assumption is based on the other assumption, that 'weight is proportional to mass,' the sole evidence for which is that 'mass is proportional to weight.'

The arguments by which this theory—that attraction is proportional to mass—is supported are based partly on observation and partly on mathematics. The former either prove nothing or make for the contrary; and the latter are absolutely irrelevant as far as physical causes are concerned. We shall deal with the former first, and then discuss the value of mathematical proof.

With terrestrial gravitation we shall deal more fully further on. Briefly, what we know of this is that bodies fall to the ground, and *generally* seem to tend towards some common centre. We say generally, because of some observed deviations accounted for, but not proved, as being due to distribution of 'mass.' It is also known from observation that the weight of bodies (bulk for bulk) varies, and that there is no discoverable relation between weight and hardness, or weight and chemical equivalents, *but that there is a strongly apparent relation between weight and the numerical product of chemical equivalents and specific heat.* Further, it has been ascertained that bodies weigh less at the equator than in parts nearer the poles, which agrees (seemingly) with one part of the accepted theory, and disagrees with the other.

The current explanation is that, the diameter of the earth being greater at the equator, the centrifugal force is correspondingly greater; and, secondly, that, the distance of the body being further removed from the centre (*sic!*), the attraction is less, in accordance with the law of inverse square of distance. The first explanation seems plausible enough, and would also be satisfactory if by itself it could account for the phenomenon. But against the second a serious objection may be raised, as will appear from the following considerations. According to the theory now under consideration, attraction depends on 'mass,' i.e. the quantity of matter. Let us see what follows from this. In Fig. 42 let *A* and *B* be two spheres composed of the same matter, but one much larger than the other. Then, if the quantity of matter in *B* be twice that of *A*, a body on its surface should be attracted with double the force. But attraction is assumed to be, not only as mass,

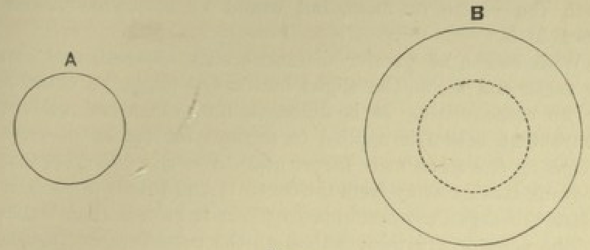

Fig. 42.

but also inversely as the square of the distance. Now this law does not relate to the centre of the mass, but to the distance of each particle composing the body. In respect of two bodies at a considerable distance apart the force may, *for convenience of computation*, be referred to their centres, though according to the doctrine of 'attraction proportional to mass' even this would not be correct on the supposition of an equal distribution of matter. But, if we consider the attraction exerted by the earth as a whole on bodies on its surface, this supposition is not admissible at all, as will appear from the following argument.

Let *A* and *B* (Fig. 43) be two bodies, which, for clearness'

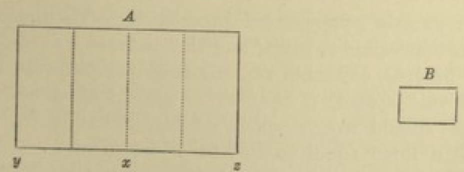

Fig. 43.

sake, are here represented by rectangles. Let *A* be divided into equal parts, as shown by the dotted lines. Now if of equal mass each of these parts would exert the same pull *at equal distances*; but the parts *further* from B would attract the latter with a lesser force in proportion to their greater distance. Dividing A into two equal parts, it is clear that the half $x z$ would exert a greater pull on B than the half $x y$; hence, in strictness of language, the centre of gravity of A in respect of B could not be expected to coincide with the centre of mass, but would be somewhat nearer towards B.

With bodies at greater distances, such as earth and sun or earth and moon, the slight inaccuracy might be of little or no consequence. It is different, however, when such a convenient fiction is applied to account for *bodies on earth weighing less when near larger quantities of matter*, because they are further away from the centre! And this is practically what the argument amounts to when it is said that bodies at the equator are lighter than at the poles because further away from the centre.

In the annexed diagram (Fig. 44) we give the shape of the

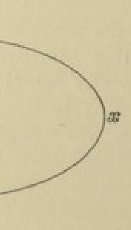

FIG. 44.

earth in grossly exaggerated proportions. Then, according to 'attraction being as mass,' and the distance $a c$ being twice that of $b c$ (the distances representing quantities of matter, and not void spaces), a body at a, instead of weighing less than at b, should weigh nearly double. That is, $a d$ should act with a force equal to $b c$; to which would have to be added the force of the matter between d and c, which would be less than that of $a d$ by its greater distance only. But even now we are not quite accurate, as we should not merely consider the matter between surface and centre, but also that between the two surfaces; i.e. from a to x, and from b to y.

This follows of necessity from the doctrine that 'attraction is proportional to mass'; i.e. depends on 'quantity of matter.' But by referring the attraction exerted by the earth *on bodies on its surface* to its centre this theory of 'mass' is set at nought, and the centre of the earth—a mere mathematical point—is endowed with the virtue of pulling bodies towards itself.

The fallacy of this explanation will at once become apparent when we consider the explanation offered for another phenomenon, viz. that bodies taken down mines weigh less than on the surface; and the explanation is that the matter above attracts the body in an opposite direction, hence the lesser weight. But if this matter exerts a sensible attraction upwards, when a body is weighed in the depths of a mine, it is reasonable to suppose that it would also attract a body downwards when weighed on the surface. Now it will be observed that there is a great discrepancy between the two explanations of the respective phenomena. When a body on a high eminence is weighed and found to be lighter, this is attributed to distance from the centre; and when we descend into a mine and find the weight to be less than at the surface the explanation is that there is less of matter to attract it. In the one case the explanation assumes that gravity resides in a mathematical point in the centre of the earth; in the other that it depends on quantity of matter. Distance from the centre, however, could only account for lesser weight where there is space intervening between the two bodies; but the explanation cannot hold good when this greater distance from the centre is due to larger quantities of matter, as when bodies are weighed on the surface of the earth at the equator. Besides, the centre of the earth can have nothing to do with the weight of bodies on the surface of the earth[1]; since all the particles composing the earth, right through to the opposite surface, should exert an attractive force if the theory of 'attraction proportional to mass' were true. The theory of regarding the force of gravity as collected at the centres of bodies can have mathematical application only, as when sun and earth are compared with each other; and even

[1] Of course we do not assert that anyone contends that the attraction of a body is located, so to speak, in a mathematical point at the centre; but all the reasoning is such as if based on such an assumption.

then it is, as has been shown, a mere matter of convention, or a matter of necessity, the necessity arising from the fact that the problem would become too complicated if attempts were made to determine the attraction of the several parts separately. Assuming, therefore, the average density of the earth in equatorial direction to be the same as in polar direction, and the law 'attraction proportional to mass' to be true, then bodies at the equator, instead of being lighter, should be heavier than nearer the poles. The explanation of being further from the centre of the earth is then worthless, since the greater distance in that case might mean a greater quantity of matter[1].

Of course, it might be assumed that at the equator the matter is of lesser density—an explanation which is freely applied to planets—and thus account for what, in the light of the above theory, must be regarded as a curious phenomenon. But with suppositions of any sort we have no concern. Whether the theory that gravity depends on mass be true or false, an explanation different to the one now under consideration would be required to account for the fact that the attraction of the earth along its greater axis is less than along its shorter.

We have dwelt at such length on the above point because the theory of gravitation is generally represented as the most perfect, and as one which is verified by every known fact. This, however, is far from true. The points of disagreement between the theory and the facts by far outnumber those which make for it—that is, as far as concerns the theory that gravitation is proportional to mass. And as this doctrine forms the basis of many other theories (extending even to chemistry, as will be shown further on) we deem it of the utmost importance to show that the doctrine is based on the merest assumption and is without a scintilla of evidence—

[1] Parenthetically we might here remark that, if this theory were true, then the point of attraction of the earth for bodies on its surface could not be in the centre, for reasons above explained, but would always be somewhat nearer towards the body attracted. For to assume that the point of attraction is in the centre would involve the assumption that the opposite half of the globe, and therefore the matter further removed from the body in question, exerts a force equal to that of the nearer half. And from the fact of the uneven surface of the earth it would follow, according to the theory under consideration, that the point of attraction should vary at different parts of the earth. But with this subject we shall deal more fully in a subsequent chapter.

unless we regard as such the argument from the fallacious analogy that two pounds of iron are twice as heavy as one pound.

Here on earth the theory of 'mass' is not borne out by facts: attraction is actually less where the diameter of the earth is greatest. If we turn to the heavens, matters are even less satisfactory. Sun and earth do not always attract each other with the same force, as they do not remain at constant distances. Sometimes they approach, at other times they recede from, each other. And as this alternate approaching and receding takes place with the utmost regularity it cannot be attributed to external influences, which are not the same at any two successive periods of the earth's revolution round the sun. True that, when further away from the sun, the earth travels more slowly than when nearer to it. But this, so far from accounting for the phenomenon, is itself a phenomenon that requires explaining. We shall consider the arguments based on this correspondence between velocity and distance further on. Here we merely note the fact that, though there is no reason to suppose that 'the quantity of matter' in sun and earth varies periodically, yet their distances from each other do so vary: which circumstance goes far more to prove that their mutual attraction varies than that it remains constant.

But there is one argument put forth—often with great exultation—as a kind of *experimentum crucis*, which is supposed to prove the contention that gravitation depends on 'mass'—whatever this vague word may mean. The argument runs somewhat as follows:—'The force of attraction exerted by the different planets and their satellites has been ascertained from their observed perturbations, or other influences, which they exert on each other; and when these forces are reduced to equal distances there is a most wonderful agreement in the correspondence between the 'masses' of these several bodies and their 'force of gravity.' The case is variously stated by different writers, but when stripped of all extraneous matter the argument is always the same: viz. the agreement between (supposed) facts and theory is advanced as conclusive evidence of the truth of the latter. 'We have thus a mass of irresistible evidence,' says Sir G. B. Airy [1], 'to prove that the attractions

[1] 'Popular Astronomy' (Ipswich Lectures), 1866 edition.

of the sun upon the planets and upon our moon, of the planets upon their satellites, and of the planets upon one another, do follow the law of gravitation.' This he proves by showing that their attractions are proportional to their weights. But when proceeding to estimate the weight of these several orbs he makes the following admission:—'And here is involved an important principle, namely, "*that the weight of a body is proportional to the attraction which it exerts.*"'

Now this admission completely destroys the value of the evidence. For, in first deducing the weight from the observed attraction, and then showing that the attraction exerted by a planet is proportional to its weight, the wonder is, not that there should be such 'a remarkable agreement between theory and fact,' but that this kind of reasoning should ever have been put forth seriously to prove anything save that the theory agrees with itself—and, we might add, with nothing else.

Of the planets and their satellites we can know from observation their volumes and the perturbations which they occasion. These observations we may assume to be correct. Of the *force* to which these attractions or perturbations are due we only know one thing for certain, namely, that it acts, like every other 'force,' inversely as the square of the distance. All the rest is mere assumption. Jupiter is many times larger than the earth, but its 'gravity' is not proportionally greater. From this it is inferred that Jupiter is of lesser 'density' than the earth. This again follows from the cardinal assumption that attraction is proportional to mass. But, this last assumption not being proved, the inference is not necessarily an inevitable one. Density, then, is obtained by dividing 'weight' (i. e. power exerted) by volume, after the following manner:—

$$\frac{w}{v} = d.$$

Then it follows, of course, that

$$d \times v = w,$$

which simply proves a well-known rule of arithmetic, that if a sum is divided by one of its factors the quotient will be the other factor. And inversely, if divisor and quotient are multiplied the product will be equal to the dividend.

This is all that the agreement between theory and observation amounts to. It is not an agreement between fact and theory, but an agreement between arithmetical computations made from hypothetical data. Jupiter produces certain effects on the planetary system; so do the other planets and their satellites. In order to be able to compare the relative forces of these planets, some unit standard of comparison is required. As this unit standard the earth may be adopted; and we can then say that the force of Jupiter is so many times that of the earth. So far the computations are based on empiricism, and will be correct in proportion to the accuracy of the observation. The forces exerted by the different planets are thus compared with each other, and by computation it is easy to calculate, from the perturbations, the force of any body or bodies to which such perturbations are due. All these observations and calculations, it will be noted, have reference to quantity (quantities of force, but not of matter). They have nothing whatever to do with causes, and can suggest none. True that from a perturbation may be inferred the presence of a body, and the existence of Neptune has been thus inferred. Nevertheless the discovery of Neptune does not prove the truth of that part of the current theory of gravitation which affirms that gravity is proportional to mass. Quite the contrary: the circumstances connected with the discovery of Neptune prove most conclusively that we know absolutely nothing of 'mass' or of the causes of gravitation. At most it may be considered as strong, though not as conclusive, evidence that gravitation is universal. And, as the discovery of Neptune is put forth as a crowning victory (a kind of *experimentum crucis*) in proof of the current theory of gravitation, we shall examine somewhat more closely into the circumstances which have led to its discovery.

Perturbations had been observed in the different planets; and long observation had attributed these perturbations to the influence of the different planets on each other. It is these perturbations which have been measured by their effects, and which Newton has reduced to a common unit. He made three generalizations:—

1. That all reactions are mutual;

2. That the influence of two bodies on each other is inversely as the square of the distance; and

3. That the force is proportional to 'mass.'

The first and second he inferred from actual observation. Each disturbance in the planetary system could be traced to the influence of some other planet; and, as the power thus exerted was the greater the nearer were the bodies reacting with each other, he was safe in inferring the second generalization. But, apart from these observations made on the planets, he had in terrestrial phenomena ample proof in support of these two laws.

The third generalization was a mere hypothesis based on the fallacious analogy that under like conditions double the quantity of the *same* matter produces a double effect. From this he inferred that a double effect always necessarily implied a double quantity of matter. But he based no argument on this latter assumption; and, careful reasoner that he was, stated plainly that by mass he merely meant weight. This is not necessarily true, however. If a mass of copper and a mass of iron are brought near a magnet, the iron will exert a greater force on the magnet than will the copper; whilst near a non-magnetic body the reverse might be the case. Seeing, then, that on earth the relative pull which two bodies exert on a third body need not necessarily remain the same, whatever be the causes, there is certainly no reason to suppose that the same might not be true of celestial bodies. There is no reason to suppose that two dissimilar substances, as copper and iron for instance, which equipoise each other on earth, would equally do so on any and every other planet; the sole warrant for such an assumption is again the fundamental doctrine we are now endeavouring to disprove. Indeed, we shall prove by experiment that, even without what are called magnetic influences, two dissimilar bodies do not remain constant in their relative weights under different conditions on earth, or in different parts of the globe. But before adducing any evidence on behalf of our own theory of gravitation—which we shall deduce from the principles we have already established—we shall proceed with our examination of the evidence by which the current theory is supported.

Turning now to the circumstances that have led to the discovery of Neptune, we shall see that the presence of this planet was inferred from the first two generalizations; whereas the supposed 'mass' of Neptune, in the sense of quantity of matter contained in it, is as yet an unsolved problem, and is likely to remain so for some time. The discovery was made as follows:

Perturbations were observed which could not be attributed to any of the known planets. From the law that every reaction is mutual, it was inferred that there must be a body to which the observed perturbations were due. From the extent of the perturbations, and by the aid of the empirical law of the relative forces required to cause a perturbation in any planet, the force of the unknown body could approximately be calculated. From the nature of the disturbance could be inferred the direction whence the disturbance proceeded, and hence the direction where to look for the unknown body. But the distance, the size, or the 'density' of that body could obviously not be inferred. Neptune having been discovered, its distance was ascertained and its size. From the size, its volume was calculated; and, the force having already been ascertained, the density was found by dividing the power, which that body was found to exert on other planets, by its volume.

Say now that subsequent measurements gave to Neptune a volume different from that of the first determination. Then it is clear that a different 'density' would have to be assigned to it. And, the density being always the quotient of the force, or power, of the body divided by its volume, it follows of necessity that by multiplying density and volume we shall obtain the product of the two factors into which the empirically ascertained force had originally been resolved.

The argument, therefore, stands thus: Force is attributed to 'mass'; and 'mass' is calculated from force ascertained by direct observation. 'Force' and 'mass' therefore (or 'weight' and 'mass') are synonymous terms; two different names for one and the same thing. And yet the agreement between weight and mass is put forth as evidence in support of the hypothesis.

As already mentioned, we are dwelling at such length on this subject because the theory that attraction is proportional to mass plays as important a part, not merely on the subject of gravitation, but on physical science generally, as did the conception that the earth was flat, and formed the centre of the universe, on astronomy. And, as this theory is now accepted as beyond any possible doubt, it is all the more necessary to show that it is without any foundation in fact, and unsupported by any evidence, direct or indirect, whatsoever.

We have used the term 'force' or 'power' in connexion with planets in place of weight, and we shall now explain our reasons for doing so, which at the same time will show how seriously reasoning may be influenced by wrong terms and false conceptions. By weight is meant the force or power by which one body is drawn towards another, or the force or power that would be required to counteract such tendency of two bodies to fly towards each other. If double the strength were required to prevent a body from falling to the ground as compared with another body, it is said to be twice as heavy or double the weight. This is true of bodies on earth; but the same terms could only be applied to the sun and planets if it were found that the conditions there are the same. In Newton's own words, 'To the same natural effects we must as far as possible assign the same causes.' Now the effects between the sun and the planets are not the same as between the earth and bodies on the earth. The latter are always known to tend downwards, and, as far as appearances go, with constant forces. But sun and earth are not always tending towards each other, but as often manifest the opposite tendency. To pull sun and earth asunder would not always require the same force; since when the earth moves from perihelion towards aphelion the weight of the earth as referred to the sun must of necessity be different than when moving in the opposite direction. Indeed, it is safe to infer that in the one case it would require as much force to draw the earth towards the sun as in the other case to pull it away. These periodical approachings and recedings of sun and earth cannot be attributed to any influence of planets in an opposite direction, because of the regularity of these motions and their independence of the positions of the planets. Whether it be true, therefore, or not, of *terrestrial* gravitation that 'it admits neither of intension nor remission of degrees'—as was supposed by Newton, and we shall show further on that it is not true—it certainly does not apply to sun and planets. There obviously are both intension and remission of degrees, since they do not remain at the same distance apart. And if we are to adopt Newton's above-cited rule, viz. 'to the same natural effects we must assign the same causes [1],' then we shall be forced to attribute the periodical receding of planets from the sun to their mutual repulsion, in the same manner as we do

[1] Newton says 'as far as possible,' which shows that he was aware of the difficulty involved.

in the case of analogous phenomena observed with bodies on earth.

Now this fact, that the planets do not fall into the sun notwithstanding the assertion that they mutually attract each other, but, quite contrary to this theory, actually recede from the sun at regular intervals, is as yet an unexplained problem. In works on astronomy, however, the fact is generally glossed over, and it is made to appear as if the recessions were most satisfactorily accounted for. But on closer examination the supposed explanation turns out to be either mere rhetoric or a series of equations of abstract mathematical problems. Why, if sun and planets constantly attract each other proportionally to their quantities of matter, and inversely proportionally to the square of the distance, do they not fall into each other and all into the sun? The question is a perfectly legitimate one, seeing that every writer on astronomy has deemed it necessary to offer explanations. But these explanations are far from satisfactory, as we shall now endeavour to show.

The reason assigned is that the centripetal and the rectilinear forces always exactly balance each other. Thus, when a planet is nearer the sun it moves faster, and hence offers a correspondingly greater resistance to being diverted by the centripetal force from its rectilinear path. And, when further from the sun, the planet moves slower, and hence is more easily counterbalanced by the now weaker centripetal force which draws it towards the sun. But why is the motion of planets accelerated when nearer the sun, or why are planets nearer the sun when moving faster? So little is known about this that we do not even know which of the two questions would be the right one. Nobody seems able to tell whether the planets move faster because nearer the sun; or whether they are nearer the sun because they are moving faster; or whether both these phenomena are the joint result of some as yet unknown cause. (In a subsequent chapter we shall show the latter to be the case; and shall deduce these remarkable phenomena from the principles we have been discussing.) Of course an explanation is offered that when the planet is furthest from the sun it is drawn by the latter, and hence the acceleration of speed in moving towards perihelion. But why, then, is this drawing towards the sun arrested just at the point where the power of the sun is greatest? The current answer is that now the velocity in orbit is so great as to be stronger than the centripetal force; and hence, the former now preponderating, the planet again moves away from the central orb.

Many serious objections might be raised against this explanation, were it not that one general objection is sufficient, if not to disprove, at all events to discredit, the explanation. This objection is that this theory is deduced from the phenomena themselves, and not from any known physical principles; and hence agreement between phenomena and explanation cannot have the weight of valid evidence.

But there are two more objections to these explanations. One is the assumption that the circumsolar motions of planets are actually due to the joint action of two such forces into which Newton has conventionally resolved these motions. Briefly, the contention is that planets have a tendency to move *uniformly* forward in a *straight line*, from which course they are diverted by a *centripetal force*; and that to the action of these two forces conjointly are due their curvilinear orbits round the sun. Now there is nothing in the facts to justify the assumption that planets *tend to move in a straight line*, or that their motion is *uniform*. The facts are the very reverse of this. No celestial body is known to move either uniformly or in a straight line; nor is there any evidence of planetary motions being due to '*impressed* forces.' On the other hand, there can be little doubt that the assumption of two such forces is due to observations made, not on celestial, but on terrestrial bodies, which, when impelled, do tend to move in a straight line, from which course they are diverted by the attraction of the earth. In other words, the assumption concerning planetary motions is based on analogy. There is, however, no evidence of the analogy holding good; for, when we impel a body, that body simply follows the direction of the impelling force. Terrestrial gravitation may be considered as a second impelling force. But, whilst there is no doubt that the curvilinear path of terrestrial bodies thus propelled is due to the joint action of two forces, viz. the impelling force and the attraction of the earth, there is no evidence, either direct or indirect, that planetary motions are due to two such independent forces. The assumption rests entirely on analogy, and is one which we shall show further on to have no justification whatever. Relying, therefore, on the dangerous process of reasoning from analogy, and the fact that two such forces conjointly would be *capable* of producing the observed planetary motions, the theory, originally merely assumed by Newton for the sake of convenience of explanation, has come to be accepted as a well-established fact. Many even assume that Newton himself has actually proved planetary motions to be due to two such forces. Newton, however, has done nothing of the kind; nor has he even attempted to do so. What Newton did prove was the *proposition* that 'A body by two forces conjoined will describe the diagonal of a parallelogram in the same time as it would describe the sides, by those two forces apart[1].' It is the truth of this mathematical proposition that Newton proved, and not that planetary motions are actually due to two such forces.

The mathematical proof of this proposition can, however, hardly be accepted as conclusive proof of, nor even as lending force to, the argument. This supposed proof consists in showing by mathematical formulae and deductions that, *given two such forces, such and such would result.* But, then, the truth of the result of such mathematical calculations must always depend entirely on the truth of the original assumptions. From the velocity and curve described by planets Newton deduced the 'forces'; this he did for several planets, deducing from the phenomena the 'forces,' and then, by reversing the process, he obtained (mathematically, of course) the actual path and velocities described. Without desiring to be irreverent towards mathematicians, nor in any way desiring to minimize the undoubtedly great value of mathematics as an *aid* to science, we would submit that abstract mathematical formulae can never prove a fact in nature. We trust the illustration we are about to give will not be considered as emanating from a frivolous mind; it is made use of solely because it well expresses what we wish to say in regard to the value of mathematics in proving a fact in nature. Say, then, that a man be sent to market with eighteen pence in his pocket, and returns with only

[1] In Corollary II of the same law Newton says—'And hence is explained *the composition of any direct force* A D, *out of any two oblique forces* A B *and* B D.' All, therefore, that Newton did was to mentally resolve the observed motions of the planets into two separate 'forces' which *could* produce such results. From that part of Corollary II which we have italicized, it will be readily seen that the same might be done for any body moving in a straight line, which might be assumed as being the oblique result of two forces acting at right angles.

six pence; and that we wish to account for the missing twelve pence without knowing anything of the actual facts of the case, our knowledge being limited to the fact that so much of the original amount is missing. By way of hypothesis, we might assume that he bought six apples at two pence each. On this assumption we could then propose to ourselves some mathematical propositions as follows: First, Given the price of apples and the number of apples bought, to find the amount spent. Second, Given the amount spent and the number of apples bought, to find the value of each apple. Third, Given the amount spent and the price of each apple, to find the number of apples. Needless to say that all these equations will come out remarkably confirmatory of each other, without on that account establishing the truth of the original assumption. On investigation the facts might turn out to be that the price of apples was not two pence; that their number was not six; and, thirdly, that apples had nothing to do with the transaction. Nay more, he might have spent more than twelve pence, having received some additional moneys on his way. The simile is by no means an exaggerated one, as will presently appear.

Now this is precisely the case with the mathematical proofs as far as they affect the *causes* of gravitation. As already pointed out, Newton did not give his mathematical demonstrations as proof of the assumptions, but merely tried to deduce quantitative relations. He *assumed* that the planets were originally started in their course at a certain distance from the sun and with a certain velocity. This distance and velocity he deduced from the phenomena, and then proceeded somewhat as follows: Given the orbit, to find the focus. Given velocity and distance, to find the orbit. Given orbit and distance, to find the velocity; and so forth. Now these mathematical relations, being based on data deduced from actual observation, must of necessity tally with each other; but this would be in no way confirmatory of the original assumptions.

The other objection is the assumption that the planets have all been started with certain initial velocities. By what? it may be asked. Of course, the answer, 'We do not know,' is as legitimate as it is certainly candid. Nor would we be justified in asking this question, if it were not for the universally accepted law that *every reaction is mutual and directed to contrary parts.* If a planet was originally set in motion, it must, if this law of reciprocity be true, have been started by another body which at the same time must have undergone a corresponding change. Instead of which, we have the bare assumption that the bodies have been started with different initial velocities, and now keep on moving for ever and ever,—and that although they periodically change their velocities with great regularity.

The appeal often made that the astronomical discoveries made since the adoption of the Newtonian theory are further proof of the truth of the theory is based on the fallacy that the fertility of a theory in new discoveries is necessarily a proof of the truth of the theory itself. To this it might be replied that the history of the sciences is full of discoveries due to deductions made from false theories. Thales had foretold an eclipse of the sun long before the Newtonian theory was formulated. The time of the equinoxes could as well be inferred from the geocentric as from the heliocentric theory; and so might have been, and actually have been, inferred eclipses of the moon and sun. The fact is that such discoveries are due to *the empiricism* underlying such theories, and not to the theories themselves. Present-day astronomy is entirely empirical. All astronomical predictions are based on empirical tables, mostly averages of many years' observations. The discovery of Neptune is often cited as being due to the Newtonian theory. But to what part of the Newtonian theory? *Clearly to the law of reciprocity.* Perturbations were observed, and from the law that to every action there is a corresponding reaction it was reasoned that there must be somewhere a body to the presence of which these perturbations were due. It was not inferred from the doctrine we are here combating, nor from the proposition that gravitation is proportional to mass, but from the principle of reciprocity, which Newton had discovered in physical phenomena; which obtruded itself on our own attention in considering biological phenomena; which in the present essay we have shown to be a prime factor in every physical phenomenon; and which at present, although universally admitted as an abstract truth, is almost entirely ignored in most of the physical sciences.

But we must bring this review to a close. Our object was to show that, as far as it relates to an explanation of causes, the current theory of gravitation is unsatisfactory, and that some of the fundamental assumptions are supported by no evidence of value. This was necessary, because the doctrine that gravitation is proportional to mass, and depends solely on mass, is at the very basis of many current theories concerning phenomena connected both with terrestrial and interplanetary gravitation; and moreover dominates the mind, unconsciously, just as the conception that the earth was a flat expanse must have influenced the mind of a philosopher who held that view, as illustrated in one of the opening chapters. With this doctrine in the reader's mind it would be difficult to prove anything that ran contrary to it, no matter how strong the evidence by which it was supported. And to discredit, not to disprove, this doctrine was the main object of the present chapter.

A theory as to the causes of gravitation should be able to account for all the irregularities and exceptions, as well as for apparent regularities; and should be able to account for all the various phenomena by well-known physical principles. Now we shall undertake to do this. But if the agreement of our explanations with the phenomena were all we could offer in the shape of evidence we would regard our theory as a worthless hypothesis, agreeing with Newton that in physical science there is no room for hypotheses whether physical or metaphysical.

Now, inasmuch as we shall deduce all the phenomena from the principles we claim to have established, and each of which is capable of experimental verification by any means accessible to experiment, we believe that we are justified in regarding the agreement of deductions made from such principles with the phenomena as true *a posteriori* verifications; and that all the more so because the principles have *not* been deduced from the phenomena which we undertake to explain by them.

CHAPTER IV

DEFINITIONS AND PRINCIPLES OF MOTION

The identity of principles underlying all natural phenomena has long been suspected by natural philosophers. With Newton this seems to have been a deep conviction, as would appear from his preface and the concluding paragraph of his *Principia*. And this belief has been shared by every scientific thinker of note since his time, whilst each new discovery has but served to strengthen and confirm it.

Principles so universal and general, one would surmise, ought to be everywhere apparent and easy of discovery; and so indeed they are. The principles of persistence, resistance, reciprocity, and equalization are everywhere apparent. They are the common knowledge of philosophers as well as of the most illiterate. Nobody expects to be able to effect a change of any kind without some expenditure of effort, some resistance to be overcome; which shows that *resistance* is common empirical knowledge, though not formulated into an abstract principle, or recognized as such. Persistence, or 'inertia,' too, is well known, and is indeed identical with resistance, only looked at from a different standpoint. Not less familiar to everybody is the tendency to equalization, though the principle as such has not yet been recognized. It is the unconscious knowledge of this tendency on which so many every-day operations depend. To heat a body, it is brought near one hotter than itself; and the empirical knowledge of the ignorant as well as the higher knowledge of the educated alike expect that the hotter body will be cooled while heating the other, and that this reciprocal heating and cooling will have reached its limit when both are at equal (more correctly at *equivalent*) temperatures: which expectations are based (consciously or unconsciously) on inferences from the law of reciprocity and the tendency to equalization.

These principles are neither new nor hidden, still less are they mere inventions of the mind. Every calculation of power required to achieve a certain result, or of power available from a certain disturbed equilibrium (technically known as 'difference of potentials'), whether in respect of steam, water, or electric motors, is alike based on the reality of these principles, though they are not clearly recognized as such. There can be no question as to the reality of these principles. They are as universal as they are common knowledge. Any physical fact which can with certainty be predicted is inferred (often unconsciously) from any one or all of these principles, and that whether the prediction be made by the illiterate or by the scientifically educated. Take away the *empirical*, though as yet unformulated, belief in persistence, resistance, reciprocity, and equalization, and there is not a single physical fact that could be predicted from any given conditions.

The truth of the principles themselves being established, two questions obtrude themselves. Firstly, are these principles sufficient to explain all phenomena? And, secondly, if so, how is it that principles so obvious should for so long have escaped detection, notwithstanding the diligent search of the ablest workers of the race? To the first question a partial answer has already been given by showing how many dissimilar phenomena can be traced to the interaction of these few and simple principles. A further answer will be given in the pages that are to follow, where the phenomena of terrestrial and celestial gravitation will be explained in like manner.

To the second question our reply is that the principles have remained undiscovered because they have never been looked for. All the philosophers engaged in the study of nature were either searching after 'forces,' or were endeavouring to discover the 'nature' of some supposed 'force.' No one, as far as we are aware, has yet attempted to look for the causes of phenomena to their antecedent or simultaneous conditions, but men have invariably assumed—tacitly and confidently—the existence of some subtle 'spirit,' 'force,' or 'form of energy,' *behind* the phenomenon. The problems they were engaged on were always to find out what these 'spirits' or 'forces' were like; what was their *modus operandi*; where they dwelt; and what became of them.

We appeal to the history of the sciences in confirmation of this view. On every page of it, down to our own days, we meet with questions like these:—What is electricity? what is magnetism? what is heat? and so forth. And, no matter what has been the progress of our *knowledge of facts*, the problems have remained the same. Thermal phenomena were always, and still are, attributed to 'heat'—an abstract entity, and not a quality or manifestation—and the question to be solved remained for ever, What is heat? The ancients held it to be a substance, different from 'crude matter' in being more subtle. Later 'it' was a 'spirit'; Tyndall concluded that 'it' was 'a mode of motion'; others that 'it' was a 'form of energy.' And all this search after the mythical 'it' can be traced, as has previously been shown, to the dualistic conception of matter *and* force; the former of which was and is supposed as passive, whilst the latter is the hypothetical entity whereby to account for the exceptions to the fundamental theory that 'dead' matter does not move nor change.

Only two routes were open to investigators by which the solution of the mystery could be found. These are the dualistic theory of matter *and* force (dead matter urged by some active principle), and the theory of reciprocity (i. e. that all reactions are due to the reciprocal modifications of bodies). Of these two routes the former alone has been followed, and hence the universal failure to discover the undiscoverable. The 'forces' could nowhere be found, whilst objections to the theory were rapidly increasing. And, just as among primitive people (inappropriately called savages) an individual is renamed so as to save him from some threatened danger, so these 'spirits' or 'forces' have been rechristened from time to time as ill-boding difficulties multiplied, and the search after the same thing was continued under a new name.

The alternative road is opened by the law of reciprocity. Under this view not only is there no longer any need for occult agencies, but there is no room for such assumptions. From this point of view, any observed change in one body must at once be referred to a corresponding but opposite change in some other body. *The modifications are mutual*, caused by one body in another in consequence of some disturbed equilibrium, and in accordance with the universally apparent tendency of bodies to relative equilibrium. *It was owing to a belief in this law of reciprocity that Neptune was discovered*, and not because of the belief in the abstract entity 'gravity.' Nor is it either correct or philosophic to say that two bodies attract each other *because of their gravity, or in proportion to their gravity*, since the gravity is inferred from the bodies attracting—or gravitating towards—each other. Gravity is not a *cause* but a *result* of the phenomenon: a verbal statement of an observed fact.

Thus, if the fact of the relativity of all matter is once recognized, every phenomenon may be traced to an interaction —a mutual modification—of two (or more) bodies. Such mutual modification could only take place where the relative equilibrium of bodies is disturbed; and since, in consequence of the persistence and the *variable* resistance of the several substances, *no two bodies could adjust themselves relatively to each other without creating literally endless series of other disturbances in consequence thereof*, the ever-recurring changes in nature, as well as the great diversity of phenomena, are accounted for by strictly physical causes.

The hypothesis of occult 'forces' apart from matter is as incompatible with the law of reciprocity as it is irreconcilable with facts. If a 'force' be *inherent* in a body, why should a change in one body be accompanied by a corresponding change in another body? And how are we to explain this? Is it a *transfer* of force from one body to another? This seems at one time to have been the general belief[1]; but at present the 'forces' are held to be 'inseparable, though distinct from matter.'

The law of reciprocity we have seen to be true of all physical phenomena. It is not necessary for us to prove that it is also true of those phenomena comprised under the term 'gravity,' since in connexion therewith it is already accepted, though its full importance is not yet recognized even here. If the law be universal, as was held by Newton in respect of the mutual attraction of bodies, and as is believed

[1] Derived, no doubt, as most hypotheses, from analogy; as when Newton speaks of 'impressed forces,' in analogy to the throwing of a projectile.

in an even much wider sense by ourselves, then it must play an important part in every phenomenon of both terrestrial and celestial gravitation, and should be discoverable. The 'starting of planets *at the beginning* (*sic!*) with certain initial velocities' is then as unwarranted an assumption as it is unsatisfactory. No less unsatisfactory is the other assumption that, having been started, they now continue for ever *because moving in an unresisting medium*. These are not explanations, but assumptions, and assumptions in the teeth of evidence. If planets are drawn towards the sun, then they can no longer be said to be unresisted. But, not to dwell too much on such abstract discussions, the unsatisfactoriness of such *quasi* explanations even to astronomers is sufficiently testified to by the many endeavours to fill the unresisting void with a most rigid and powerful immaterial ether!

On the other hand, if we assume these planetary motions, as every other motion, to take place in accordance with the law of reciprocity, then the causes of these motions should still be operative and should be discoverable—as indeed they are so soon as all assumptions are brushed away, and the phenomena are approached with nothing but well-proven physical principles in the mind.

We shall now attempt to deduce the phenomena of both terrestrial and celestial gravitation from the principles we have established, and then compare our results with the phenomena. To this end it will be convenient to enumerate here the primary principles as well as their corollaries—or 'derivative laws'—for future reference; remarking only that each one of these 'laws,' whether primary or derivative, is demonstrable by experiment, and is, therefore, a real physical law, and not a mere hypothesis.

We shall preface our table of 'laws,' more correctly inductions or generalizations, by definitions of a few leading terms which henceforth we shall have to use in a sense somewhat different from that generally associated with them. The sole object to be achieved by these definitions is clearness of language, and nothing else. We shall base no argument on, nor draw any inferences from, the ideas which the terms are to convey. But from our investigations it follows as a matter of necessity that our conceptions concerning phenomena in general, as well as their causes, should be modified in accordance with the results achieved. The dualistic conception of matter *and* force, for instance, is no longer a tenable one. We shall, however, not propose new terms in place of these, because, firstly, we are unable to coin a brand-new word that shall convey an exact meaning of an idea which is not quite clear in our own mind (the reason for which will appear presently); secondly, because a change of terms does not necessarily effect a change of ideas. Words are but the spoken or written symbols of ideas, and their meanings vary just as the ideas for which they stand change. But the converse is not true: a new term for an old one seldom begets new ideas. Few words in modern language, unless they are names of common objects, have the same meaning as they had in earlier times (e.g. calx, spirits, ether, confiscate, affinity, duke, lord, baron, burgher, villain or villein, curiosity, &c.). On the other hand, there are many new terms which still represent the same ideas as those which they supplanted. Nor is it our object to reconstruct our language so much as to change old conceptions in conformity with recent results. In the use of language we are bound by usage; but, if our conceptions are modified, the retention of old terms (save where it is possible to coin new ones without creating confusion) is less objectionable than the forcible reconstruction of human speech.

We have said that the conceptions of matter and force have to be abandoned, and that we have no clear ideas ourselves as to the precise terms or ideas that are to take their place. The difficulty arises from the circumstance that these terms convey at present *definite ideas of* THINGS concerning which nothing is known. The statement, no doubt, is paradoxical; but, if it were not, it would not truly express the actual state of the case. Everybody seems to have a notion of *matter* and *force*, and both are spoken of as freely as if there could be no doubt about their meaning. *Matter* fills the universe, and *force* rules it. The *nature* of matter, when 'deprived of its qualities,' none—save daring metaphysicians—have ever pretended to understand, any more than the 'nature of force.' But about their actual existence there seems to be little doubt[1].

[1] Of course we are aware of the extremely few exceptions, which only serve to accentuate the rule.

Now the present authors must confess that they have as little knowledge of 'matter' as of 'force'; and, to be still more precise, they have, like Sir William Thomson (now Lord Kelvin) and Faraday, far less knowledge of the former than of the latter. We cannot even accept the common definition which consists of an enumeration of qualities. Extension, hardness, taste, smell, colour, &c., are all incidental qualities, and may serve to define the common idea of 'body,' but not that of matter. We do not know what constitutes these qualities, and hence cannot know what those bodies that possess these qualities are composed of. And this is the reason why we refrain from coining a new term for something about which we are unable to form a definite idea. All we can assert positively, on the ground of the result of our inquiry, is that the *dualism* must be abandoned; and this can be done without entering on dangerous metaphysical ground, or undertaking the creation of new and perhaps confusing terms.

We have drawn a distinction between primal and incidental qualities; designating by the former those qualities of bodies which are independent of shape or aggregation, and by the latter those qualities which only attach to combinations, but which are destroyed by the destruction of such a combination. Now, hardness, shape, taste, smell, colour, &c., all belong to the latter category. Whether extension should be included in this list only, or whether it should also be regarded as a primal quality belonging to the *hypothetical* 'atoms,' we are as unwilling as we are unable to decide, as this would involve many speculative questions.

Subtracting thus from 'matter' all those qualities which belong to aggregates (i.e. 'bodies') only, and those also which are doubtful, we have nothing left as 'primal qualities' save *persistence* or *resistance*, the power to resist modification and to modify[1]. This is all we know of either *matter* or *force*; and, as previously pointed out, either term could be made to serve for both. But, having regard to the usage of language, and to avoid confusion, both might be retained in the sense in which we are employing the terms 'persistence' and 'resistance.' By the former we designate *a quality*, the quality to resist;

[1] The power to modify follows from the power to resist. For, since both bodies resist, but *resist unequally*, both are bound to undergo modification.

and by the latter the *quantity* or *degree* of this quality. In this sense, then, 'matter' would stand for the unknown something which offers resistance; and 'force' for the degree (or quantity) of this resistance. 'The matter composing our earth,' for instance, would mean the *resistance* of this planet (not only of the solid globe, but of the planet as a whole as compared with another planet, however far this resistance may extend); and 'the force exercised by the earth' on another body would merely mean the *measure* of this resistance. We shall thus avoid all questions as to the 'nature' or 'quantity' of 'matter' to which these powers are due[1].

There are two or three more terms which require defining. The term 'body' is used by us in the sense in which 'matter' is commonly employed; viz. to designate some sensible or tangible object. In addition thereto we may state that by 'a body' we also mean any aggregate, whether small or great, simple or complex, which, in respect of another aggregate, behaves like one body. Thus, in respect of the moon, the earth is considered as *one body*, notwithstanding its complex nature, and would include all matter which is affected by any lunar influences on the earth. But the earth itself may be considered as many 'bodies' in respect of their mutual relations to each other. A magnet, for instance, as a whole reacts with other magnets, or with the earth as a whole; but we may consider the effects of the molecules within a magnet on each other. In each case that part, or aggregate, the action of which relatively to some other part, or aggregate, is considered, would be included in the term 'body.'

[1] The primal qualities belong to all matter, but are possessed in variable degrees by different bodies. An enumeration of the primal qualities would, in fact, be a definition of matter; and an enumeration of these qualities *plus* the relative degrees of resistance for each would be an enumeration of the several forms or kinds of matter. The former would relate to quality (standing for 'matter'), and the latter to relative quantity (standing for 'force'). The qualities cannot be destroyed, but the relative degrees may be varied indefinitely. This does not admit, even in imagination, annihilation of quality. We cannot conceive, for instance, of a body void of temperature, though we may think of a body as the coldest of all, and its temperature could be expressed relatively to others in the same manner as that of any other body. We abstain from stating as to what constitutes the primal qualities save in the general terms of 'relative states of excitation,' or the resistance a body is capable of offering to another body. Beyond this power of resisting or modifying we do not know anything of matter.

Two other terms are 'states' and 'reaction.' Both have already been defined, so we shall merely restate the uses to which they are put, which will best explain the meanings we attach to them. All bodies may be considered *relatively to other bodies* as being either in a state of relative equilibrium; in a state of reaction (or process of equalization); or in a state of coercion. The meaning of these three states has been abundantly explained. By *reaction* we mean any change or modification whatsoever (including motion or translation in space) which two bodies effect in each other. Both terms have, of course, a relative meaning only. A body may be in equilibrium relatively to one body, whilst it is in reaction with another, and in a coerced state relatively to a third.

By 'states of excitation,' which term again has a relative meaning only, we mean a disturbed equilibrium requiring readjustment; and this would include bodies in a state of reaction, as well as those in states of coercion. But, as no body can adjust itself to the altered state of any second body without its equilibrium being disturbed relatively to still other bodies, it follows that all matter is in a constant state of excitation and reaction[1]. By relative equilibrium we merely designate the point at which the relative resistance of two bodies is exactly balanced, and at which point neither is able to effect any further modification in the other.

In these pages we have often instanced thermal changes to illustrate our points. We do not regard heat, however, as synonymous with excitation, but merely as one of its manifestations. Heat itself being due, as has been shown, to states (or acts) of coercion (an effect, therefore, and not a cause of excitation), it follows that there are states of excitation without the manifestation of heat. Electrical phenomena offer many illustrations. We employ the term 'excitation' to designate a fact which is clearly discernible, but the nature of which we do not understand. And this is true of all the above terms which we have thought necessary to define. We do not lay any stress, however, on the terms or on the definitions, but merely on the facts for which they stand. These have been proved, and it is from these that we shall reason, and not from the names which necessity of speech requires us to employ.

[1] A view which is the very opposite of 'dead' matter.

First Principles.

As first principles we must accept, on the evidence adduced in the preceding chapters—

1. That all bodies tend to persist in any given state, and to resist change. (*Law of Persistence.*)

2. That the resistance of bodies in respect of any change is unequal. (*Law of Unequal Resistance.*)

3. That, the relative resistance of bodies being variable under varying conditions, they tend to modify each other until they are in relative equilibrium. (*Law of Equalization.*)

(Thus relative conductivities of different bodies vary at different temperatures. So likewise do the chemical affinities. Aniline, for instance, expels ammonia from its salts in hot solutions, while ammonia liberates aniline from its salts in the cold. Numerous other illustrations might be mentioned. Further on we shall give experimental evidence that even the combining proportions of the elements vary at different temperatures.)

4. That all reactions are mutual, and take place in consequence of some disturbed equilibrium (primary or kinematic); or in consequence of one body obstructing another (secondary or dynamic). (*Law of Reciprocity.*)

These are the four leading principles, of which the following are derivative laws. Having already dealt with general phenomena, we shall here enumerate only those laws which relate more particularly to gravitation; i.e. the laws of motion. It will be necessary to modify Newton's laws in some important particulars. We shall have to omit all that is hypothetical (as, for instance, that planets tend to move in a straight line), and to modify other parts in conformity with the principles here established.

PROPOSITION I.

Two bodies being in different states of excitation and free to move will move towards each other.

This proposition we have demonstrated already. By suspending small fragments of matter by a fibre of unspun silk, after the manner of a pith-ball pendulum, and bringing near them excited glass, shellac, india-rubber, paper, or any other substance of high resistance, such particles, *including* *all the metals*, are attracted by the excited body. Bodies may be excited either by friction, by chemical means (voltaic electricity), by induction, by light (radiometer), or by heat. A series of experiments, in addition to those already given, will be described in the next chapter.

Proposition II.

A body attracted simultaneously by two or more bodies in opposite directions will assume a position between such bodies, and proportionally nearer to the one by which it is more strongly attracted.

Let A in Fig. 45 be a body simultaneously attracted by B and C in the directions of the arrows.

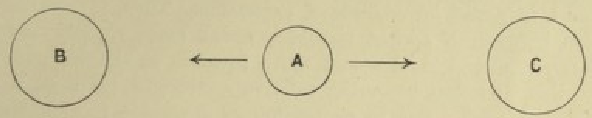

Fig. 45.

Then, if equally attracted on either side, its position will be central; if unequally, it will be nearer to the body towards which it is drawn more strongly.

Proposition III.

A body whose aggregate excitation as compared with another body remains constant will remain at the same distance from that body, provided that at the same time the conditions in all other directions remain constant also.

Such constancy of conditions never obtains; and the proposition is merely inserted as an abstract theorem. From which follows—

Proposition IV.

If the aggregate state of excitation of any body, A, remains constant in respect of a second body, B, but varies in respect of a third body, C, the relative distances between A and B will vary also.

This proposition, though stated in respect of A only, must be assumed to apply at the same time to B and C also. Let the degree of excitation between *A* and *B*, for instance (Fig. 45), remain constant, but between *A* and *C* be increased, either by *C* becoming more strongly excited (if *C* is positive to *A*) or by falling to a lower state of excitation (if it is negative to *A*), then, in either case, *A* will be more strongly drawn towards *C*[1]. Then, if *B* is not at the same time attracted in opposite directions, both *A* and *B* will be drawn nearer towards *C*. But, supposing *B* to be held in position by opposite forces; or, better still, supposing *B* and *C* to be fixed and only *A* free to move: then *A* will be drawn towards *C* and hence be further removed from *B*.

This variation in attraction may be due to reciprocal equalization. To this end we may suppose *B* to be a very hot body, say a sun; *A* a body in a much lower state of excitation, and *C* in a still lower state of excitation. Then, if equalization between *B* and *A* should proceed more rapidly than between *C* and *A*, *A* would move towards *C*, the attraction in this direction becoming relatively stronger than in the direction of *B*. But, in proportion as *A* recedes from *B* and approaches towards *C*, equalization between *A* and *C* would proceed more rapidly than between *A* and *B*. Intermediate between *B* and *C* there would be a point at which the equalization of *A* with both these bodies proceeds at an even rate. But, on reaching this point, *A* would not come to rest, but proceed towards *C*, in virtue of the law of persistence, continuing thenceforward with a velocity equal to that it had acquired when just reaching this point. Up to that point the velocity of *A* would have been an increasing one, since the force by which it was impelled (the unequal equalization) would be continually acting[2], though in a diminishing degree. Thenceforward, however, its velocity would be a decreasing one; since now it would be drawn in an opposite direction. This backward pull in the direction of *B* would also be augmented by the fact of *A* being now so much nearer to *C* proportionally to *B*. But, being so much nearer, equalization between *C* and *A* would now proceed much more rapidly than between *A* and *B*, and

[1] As previously explained, we are using the terms 'positive' and 'negative' for the sake of convenience, and merely denote thereby whether a certain body is in a higher or in a lower state of excitation than another body. Thus, if, in Fig. 45, *B* is in a very high state of excitation, *C* in a much lower state, and *A* intermediary between the two, then *A* would be positive to *C* and negative to *B*.

[2] See Prop. VIII, end of this chapter, on Acceleration and Retardation.

a return motion would result in the direction of B in the same manner and with a similar result. Thus A would oscillate to and fro between B and C; and this oscillation would continue while there was any difference of states of excitation between these three bodies.

PROPOSITION V.

A body fixed in position, but free to revolve, will turn towards an adjacent[1] *body that side in respect of which there is greatest difference of states, and in the direction of least resistance.*

Thus, in the foregoing illustration (Fig. 45), the distance of A relatively to B and C is determined by the aggregate states of relative excitation of these three bodies; hence the relative position of A, though moving backwards and forwards, may at all times be considered as determined by these conditions. But A would not be in an equal state of excitation throughout its mass; some parts would be in a higher, some in a lesser, state of excitation, so that some parts would be drawn more

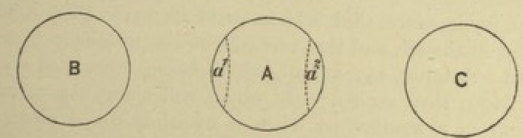

FIG. 46.

by B and others by C; hence the exact position in space of A would be determined by the mean or aggregate state of excitation of its parts. But if one part be more strongly attracted by B, and another part more strongly by C, it will turn those parts towards these bodies.

In Fig. 46 let B be in a very high, C in a very low state of excitation, and A of an excitation intermediary between the two. This would determine the distances of A relatively to B and C. But the side a^1 of A would be exposed to the exciting influence of B, and the side a^2 of A to the opposite influence of C. The result would be that A would be more strongly excited at a^1 than at a^2, the side turned towards C.

[1] We use the term 'adjacent' simply to denote the body next to it, irrespectively of distance; i. e. in the sense Faraday employed the term 'contiguous.'

In consequence a^1 would be more strongly attracted by C, and a^2 by B; hence A would perform a revolution, turning its less excited side in direction of B, and its more excited side towards C. But, in so revolving, the excitation of the side turned towards C would be continually lessened by equalization, and that of the other side as continually be increased. And hence this revolution would continue so long as A was exposed to these alternate influences.

What is true of A is true of both B and C. The side of B turned towards A would have its state lowered by so much as that of A is increased; hence the opposite and more excited side of B would be the more strongly attracted by A. The same is true of C, though in reversed order. So that, while A is revolving, B and C will revolve also, and from the same causes.

Proposition VI.

If a body while revolving and free to move be unequally resisted on opposite sides, its revolution will result in translation of place.

Say, for instance, that a body could be made to spin while floating in the air, and that one side of its periphery touched a hard substance, say a wall, whilst being unresisted on the other side; then such a body would roll along the resisting medium in a manner as when a wheel is rolled along the floor. If the resistance be equal on all sides, no such translation would take place in consequence of mere revolution. Thus, if we spin a top perfectly vertically, it will revolve round its axis while remaining in the same place. But if some hard substance be brought near it from one side, say a board, and the friction of its point touching the table be not too great, it will roll along the sides of this board; and that because the point touching this board is thereby retarded, while the other side, not being resisted to the same extent, would still continue to fly round.

This translation of a revolving body consequent on unequal resistance may be well illustrated by a ball rolling along the ground. If thrown into the air, such a ball will fly without revolving, except, of course, when such a revolving motion is imparted to it in the act of throwing. But, if the ball touch the ground, that side would be more retarded than the other, resulting in a revolving motion, and the revolving motion in translation of position. In the case of the ball the revolution itself is due to this unequal resistance. But if a body be already revolving from other causes, and press or be pressed against on one side more than on the other, this would result in translation in place. The experiment with a top spinning on a piece of glass succeeds very easily, it being only necessary to bring a piece of stiff paper near the periphery of the top.

Proposition VII.

A body set in motion, either by dynamic or kinematic agencies, will tend to continue its motion at any given point with the already acquired velocity.

This follows directly from—is, in fact, but another expression of—the Law of Persistence. From which follows:—

Proposition VIII.

While the agency actuating any body is still effective the motion will be accelerative, and that even though the power of the impelling agency be at the same time diminishing.

This has been illustrated in a previous chapter (cf. p. 147 ff.) Magnets and the pendulum afford other ready illustrations. If a magnet, *A* (Fig. 47), free to rotate, be placed in position

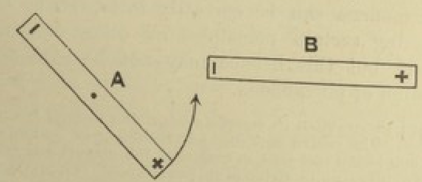

Fig. 47.

relatively to a magnet, *B*, as shown in the illustration, then it will move its + end in direction of the arrow, at first slowly, but with an accelerating speed, and will attain its maximum velocity when just opposite *B*, i.e. the point of attraction. Instead of coming to rest there, however, it will still move forward in virtue of its already acquired velocity. But now, moving against resistance (by which we mean the resistance of the backward-pulling magnet, *B*, and not merely that of the air), its velocity will gradually decrease, and the magnet, *A*, will come to rest at a point *beyond* that towards which it was originally drawn[1].

The accelerative velocity of falling bodies is due to the same principle. Gravity being a constantly acting force, it adds velocity to already acquired velocity at every successive point in the descent of a falling body.

This law of acceleration has an important bearing on another principle we established in a previous chapter; viz. that the intensity of a reaction diminishes as the reacting bodies approach nearer to equilibrium. When seeking experimental verification of this latter principle, the former should never be lost sight of; and that because in most—we might say all—instances the resultant phenomena will be affected by the concurrent operation of both these principles. Although, therefore, actual experiment will in most instances show, in the beginning at least, a gradual increased manifestation of power, the law, that the intensity of a reaction, and hence the power developed, decreases as equalization is approached, is nevertheless true in every instance. The seeming exception manifesting itself in actual experiment is due to the concurrent operation of the two principles just explained.

And so it is with all natural phenomena. In each instance several laws, or principles, have to be taken into account before phenomena can be correctly interpreted. Of course in considering certain principles all other conditions may be ignored; but this licence may not be extended to the interpretation of phenomena.

[1] We regard the reaction as complete when the magnet has made a full single journey. The return motion, and its subsequent oscillations, are but repetitions, due to the fact that in each case the magnet has moved beyond its point of equilibrium, in virtue of its persistence. (Cf. pp. 152, 153.)

CHAPTER V

GRAVITY

We shall begin by applying the principles enumerated in the last chapter to the explanation of the phenomena of terrestrial gravitation. From the Law of Reciprocity we infer that if a body on earth is drawn downwards there must be another body which at the same time is drawn upwards by the falling body. And from the Law of Equalization we infer that two such bodies attract each other *in consequence of some difference in states*, and that this mutual attraction is *proportional to that difference*. We thus arrive at a theory concerning the different weights of bodies.

Weight is merely the force, power, or intensity with which bodies are pulled downwards; the explanation of this difference of weight must be sought for in the causes to which the downward attraction of bodies is due. Neither 'weight' nor 'mass' can explain these causes; moreover the two terms are synonymous in the sense in which they are commonly used in connexion with gravitation. The *weight* of a body, as currently taught, is its *mass*, and its *mass* is its *weight*. It would be quite as true, therefore, to say that the mass of a body is due to its weight as that its weight depends on its mass.

With the present authors weight merely means attraction; and of two bodies the one will be *heavier* relatively to a third body which is more strongly attracted by that body. Relatively to a magnet, a piece of iron would be heavier than a piece of copper. Or we might attach any two dissimilar bodies to a lever, after the manner employed by

Faraday in his investigations concerning the diamagnetism of bodies, and then the body more strongly attracted by the electro-magnet would be the 'heavier.' This difference of attraction we always ascribe to difference of excitation; hence we conclude that bodies in a state of relative equilibrium do not attract each other.

We assume that these principles apply to gravitation as to every other kind of manifestation. There are no *a priori* reasons why we should regard gravitation as being an exception to the general law. The phenomena of gravitation differ from other phenomena in nothing save our conceptions and association of ideas. The falling of a body towards our earth is the result of a mutual attraction between that body and the earth. So likewise might we speak of particles of matter flying upwards or laterally towards an excited rod of vulcanite as 'falling' towards it, were it not that with the word 'falling' is commonly associated a downward tendency in a vertical direction. A body moving vertically downwards is said to be 'falling'; if moving vertically upwards, it is said to be 'jumping' or 'flying upwards'; if moving laterally, it is 'flying' either to or from a certain body: in all of which expressions the anthropocentric standpoint is clearly discernible. Let us discard these distinctive terms, and simply call the phenomena of two bodies tending towards each other *attraction*, and all these confusing distinctions at once vanish. There is no 'up' or 'down,' or 'right' or 'left' side, in nature; and, though this is recognized and universally admitted, it should also be specially committed to memory: a necessity which is all the stronger because the mind is so apt to be misled by ideas associated with certain terms.

Gravity, then, is merely the mutual attraction of two bodies; and we conclude that this reciprocal attraction is governed by the same laws as those physical manifestations which are more amenable to observation and investigation. The truth of this conclusion will become more manifest when we come to deal with bodies in space, as there the phenomena are more accessible to observation; whilst as regards bodies on earth we can only observe the *beginning*, but never the *end*, of the reaction, since the point towards which falling bodies are tending is inaccessible. In respect of terrestrial gravitation we are, therefore, almost entirely dependent on speculation. Our chief object in this and the next chapter will be, therefore, to deduce both causes and phenomena from the general principles, compare these deductions with known facts, and marshal what evidence we have been able to collect in support of our conclusions.

As already stated, we shall have to amend Newton's conclusion, that 'all bodies attract each other proportionally to their mass,' by adding the proviso, *if in different states of excitation, and proportionally to this difference.* Of course, the quantity or *mass* (in its etymological sense) of matter is an equally important factor. But then we should not employ the term 'quantity' or 'mass' in the metaphysical sense used by Newton, but in a physical sense. Double the quantity of a substance will, under like conditions, exert a double power. This is a matter of course; we shall, therefore, disregard *mass* or quantity and simply consider difference in states of excitation. That *quantity* of matter in itself does not cause attraction, we have already shown by many facts; on the other hand, we have seen that all bodies can attract each other if free to move and *in different states of excitation.*

To prove the applicability of this law to terrestrial gravitation we should have to show, first, that there is such a permanent difference of states between the interior of the earth and bodies on its surface; and, secondly, that the weight of bodies can be modified by increasing or decreasing their states of excitation. As regards increasing the 'weight,' i. e. the mutual power of attraction which two bodies on the surface of the earth can exert on each other, we have given ample demonstration. As regards increasing or decreasing the weight of bodies relatively to the earth, we shall deduce some experiments further on.

First, let us consider whether there are any indications that the interior of our globe is in a different state of excitation to the exterior. On this point we believe there can be no doubt. The hot springs, volcanoes, &c., which bring us messages from the interior, as well as the fact that in descending deep shafts the temperature increases with increasing depth [1], all prove that the conditions of the interior must be

[1] And that notwithstanding that we can penetrate to an almost infinitesimal depth only, not more than $\frac{1}{4000}$ the distance from surface to centre. It may be well to bring this important fact home to the mind by a comparison. If

immensely different from those which prevail on the surface of the globe. These circumstances would at once explain why bodies on the surface do not, under ordinary conditions, attract each other, yet have this in common that they are all of them attracted towards the centre of the earth.

We have shown that when castings are made of iron, lead, or tallow, there is always a contraction of the exterior mass inwards in direction of the centre, whereas the interior mass is attracted outwards, so as to leave a hollow in the centre. We have also shown that this double attraction, inwards and outwards, of the particles is the greater in proportion to the difference between internal and external temperatures, resulting in a corresponding greater hardness of the resultant casting.

Assuming the contraction to be due to simple cooling, there would seem to be no reason why the particles in the centre of a hot mass should at the same time press outwards. But, in the light of the principle that bodies in different states of excitation attract each other, the phenomenon is at once explained and falls into line with many other phenomena, which, apart from the above principle, would seem to have no relation or connexion with the phenomenon under consideration. Thus the central particles of a molten mass of metal would have no attraction for each other, whilst they would be attracted outwards by the exterior colder particles.

Now, it is well known that two like bodies in different electrical states can attract each other; whilst, when in like states, they 'repel' (seemingly) each other; which seeming repulsion we have attributed to stronger attraction in other directions. This difference of states may be produced by friction, percussion, heat, chemical action, light, &c., provided that, in the case of similar bodies, the *degree* of excitation varies. Indeed, as has been pointed out previously, the electrical phenomena evoked by rubbing dissimilar substances against each other, or submitting them together to the action of heat, light, or chemical agencies, are due to the different resistance of such bodies. Electrical phenomena may, therefore, be produced either by subjecting dissimilar bodies to like conditions (in which case different degrees of

we reduced the earth to a globe of about twenty-four inches in diameter, a scratch on the varnish would about represent the depth to which we can penetrate into the interior.

excitation will result in consequence of the varying resistance of such bodies), or by subjecting like bodies to dissimilar conditions. In each case the resulting phenomenon is due to identical causes; that is, to a different state of excitation of the two bodies; and the resultant reaction is merely due to the process of equalization.

Thus, in the case of a molten mass, the central and exterior particles are in different states, and hence the fact that, whilst the exterior particles press towards the centre, the central parts press outwards. This is not only a confirmation of the principle, and the theory we have deduced from that principle, but is also a splendid confirmation of the—abstractly—accepted Law of Reciprocity, that to every action there is opposed an equal reaction.

Under the view that attraction is proportional to mass, and attributing the contraction of a molten mass, while cooling, merely to the cooling, no explanation is afforded why the interior particles should press outwards, leaving the centre hollow or in a less dense state than the exterior parts. Let us imagine a mass of molten metal revolving in space, so as not to be influenced by one-sided attraction, as when such castings are made on earth; then, being exposed to identical influences all round, such a mass would be equally cooled in all parts of its surface, the particles of which would, therefore, be drawn inwards at the same time as the hotter particles of the centre were drawn outwards, and that for the following reasons:—

1. Bodies attract each other if in different states of excitation, and proportionally to this difference.

2. Bodies in equal states of excitation do not attract each other.

Fig. 48 is intended to represent diagrammatically such a mass of molten metal. The particles in the centre of the mass would be in about equal states of excitation, and, therefore, would have but little attraction for each other; whilst the exterior would be in a much lower state of excitation, and, therefore—since 'every reaction is mutual and directed to contrary parts'—the former would be attracted outwards with a force equal to that by which the exterior particles are drawn inwards. The result would be a sphere partly hollow in the centre (that is, after complete cooling), the particles tending inwards and outwards, with a zone, *z*, intermediary between the two surfaces, as shown in Fig. 48.

This is an inference from our principles, and is the actual case with castings of any kind. If, therefore, we find the law verified where experiment is possible, we must, in accordance with Newton's own rules of reasoning in philosophy, assume it to be true under all like circumstances.

Three obvious objections will here occur to the reader. Firstly, the fact that falling bodies tend towards the centre.

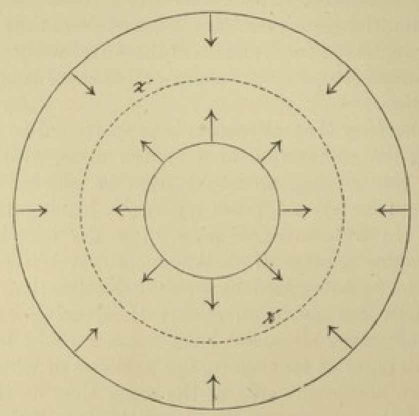

FIG. 48.

Secondly, that a hollow in the centre of our earth is contrary to legitimate deductions, which assign to it a much greater density than to the exterior portions. Thirdly, that if the weight of bodies on the earth's surface is due to the causes here assigned it should be possible artificially to increase or diminish their relative weights.

Of the first two objections we can at once dispose; since, whether it be true or not that a greater density is required for the interior of this globe, or that all bodies tend exactly towards the centre in all parts of the earth, this would not in the least affect the possible truth of our inferences that the central parts of our earth are drawn outwards as much as bodies on its surface tend in direction of the centre, and that in consequence of this the centre of our earth must be in a lesser state of density, even to the possibility of forming a hollow filled with fluid matter in a high state of incandescence.

Taking the above objections in the order enumerated, let us suppose two concentric rings joined by elastic bands, as shown in Fig. 49, the interior ring being thus drawn outwards and the exterior inwards. Continuing the radial lines representing these elastic rings, it is clear that they would all meet in the centre. In other words, the phenomena as observed on the exterior would be precisely the same whether the exterior ring were actually drawn towards the centre or merely towards a concentric ring, as here supposed, with an intermediary neutral zone.

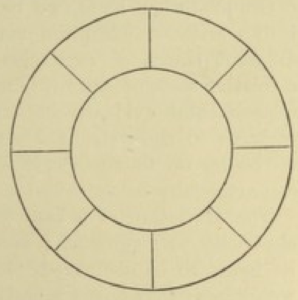

FIG. 49.

The same explanation holds good concerning the greater density; for obviously the phenomena would be the same whether the supposed mass of matter were compressed in a concentric zone somewhere intermediary between centre and surface, or located in the centre.

Whilst these explanations prove nothing, they effectually disarm such possible objections; and this is all they are intended for.

It is different with the third objection; for here a possible experimental verification of the theory is suggested. Is it possible to increase or decrease the weight of bodies by cooling or heating, or by otherwise decreasing or increasing their state of excitation?

This question we shall unhesitatingly answer in the affirmative, and adduce experiments, as well as arguments from well-established phenomena, to prove our point. But as the difference in weight thus created must necessarily be infinitesimal, and as it is beyond our means to supply a decided *experimentum crucis* (though such is by no means beyond the reach of some of our larger scientific institutions), we shall have to discuss rather fully the weight and value of the evidences presently to be adduced.

Before doing so, however, we do not consider it as an illegitimate proceeding to remind the reader of the fact that in support of the current theory, that all bodies attract each other proportionally to mass, there is absolutely no experimental evidence whatever. Bodies on earth do not manifest any attraction for each other, *save when in different states of excitation.* The answer to the absence of any evidence of this mutual attraction of bodies is still that given by Newton himself; viz. that the attraction of the globe, as a whole, is so much greater in comparison with the attraction which two bodies on earth have for each other that the latter is masked by the former. Which explanation is simply a plain admission of the absence of experimental evidence.

On the other hand, we have been able to show that all bodies without exception can attract each other when in different states of excitation, but do not so act when in a relative state of equilibrium. This is strong and extensive experimental evidence in support of our theory. It now remains to show that the attraction which the earth exerts on bodies on its surface, in short that the *weight* of bodies, can artificially be decreased or increased.

The excitation of a body may be increased by heat, light, electricity, or magnetism; and in each case this inference will be found to be confirmed. By heating a body it is made to weigh less: that is, not merely is its specific gravity lowered, but its absolute weight is less, and it regains its former weight on cooling.

This is not a new observation: it has been known to chemists and has puzzled philosophers for a long time. A platinum crucible, when weighed hot, weighs less by several mgrs. than when cold. We have tried similar experiments with a great many substances—brass, copper, iron, glass, carbon, water (the two latter in sealed glass tubes), &c.—and have found this to be true of all these substances. And what is of more importance, the loss in weight for the same weight of matter, and the same difference of temperature, varied for different substances, as might have been anticipated.

[Two brass weights of 100 grammes each were placed in the two pans of a balance which turned with a tenth of a mgr., and were carefully equipoised. Then one of them was placed

Archimedes Principle

$\Delta W = \rho_{air}\, g\, \beta\, \Delta T\, Volume = \frac{1.2\times10^{-3} g}{cc} \times \frac{980 cm}{sec^2} \times \frac{2\times10^{-5}}{°C} \times 100°C$

for half an hour in a water oven. When replaced on the balance, it weighed—as nearly as we could determine[1]—ten mgrs. less. The weights were quickly changed into the opposite pans, when the result was the same. As the heated weight cooled, so the pan descended, at first somewhat rapidly, then more slowly, and finally came to rest again, the pointer being in the same position as at the beginning of the experiment, viz. at zero. This experiment we have repeated several times, and with different substances, always with the same general result.

The fact that bodies weigh less when heated was known to Faraday, who gave as his explanation that the ascending air currents sent the beam up. This explanation, however, is not a satisfactory one; since air currents ascending from the pan and striking the beam would press the pan as much downwards as they press the beam upwards. In fact, the explanation is not much better than if we proposed to propel a ship by blowing air on to the sails from bellows placed on the same vessel. At the same time, with the idea fixed in the mind that gravity is an invariable quantity, such an explanation would naturally seem plausible, as being under the circumstances the only one the mind could fix on.

The interpretation of this phenomenon is by no means easy; and experiments to decide the point in question are extremely difficult, for the following reasons. Ascending air currents, no doubt, would tend to raise the beam of a delicate balance. At the same time, if those air currents rose from the pan attached to the beam, we may suppose that there would be a corresponding downward pressure. To make this clear, we need only conceive air currents issuing from a body free to move, and striking against a fixed object: in which case the effect of the air currents would be to push the movable body, whence they are issuing, in a contrary direction. But there is yet another important element to be taken into consideration. Ascending hot air currents would cause the beam on that side to lengthen, and thus increase the downward pull of that half of the beam. That this is not a mere hypothetical assumption

[1] We say 'as nearly as we could determine,' because as soon as the weight is removed from the hot oven it begins to cool, and it is very difficult to ascertain its exact weight at the moment when placed upon the balance. The method we adopted was to place at first five or six mgrs. into the pan in which the heated weight was to be placed; and, on finding that this was not sufficient to equipoise the cold weight, we moved the rider until the pointer was again about zero.

may be proved by bringing the hand, or a warm object, near one end of the beam, when that side will presently descend. On the other hand, if a hot object be held under the beam and over the pan, without touching the latter, the beam will in most cases (but not always) move upwards. In some few instances, however, the beam descended if but a moderately warm body was employed. The effects of a warm body in one pan of a balance are manifold and of opposing character.

1. Being hotter, it is lighter.

2. In warming pan and beam, these are made correspondingly lighter.

3. Ascending air currents tend to send the beam up.

The effect of all these three would be in one direction, viz. to make the pan ascend.

4. The expansion of the beam on one side, under the heating influence of the ascending hot air, would cause a downward motion of the pan.

To these might be assigned yet other probable causes, which we need not enter into. Whether the pan ascends or descends, therefore, the effect would in each case be but the balance of a complexity of causes; and it would be difficult to apportion to each cause its share in the final result.

Considerations like these make it difficult to obtain definite experimental verifications. Not that confirmatory experiments are wanting, for many could be and will be cited in addition to those already mentioned. But their acceptance as evidence would depend entirely on what theory were accepted. In the light that gravity is a fixed and unalterable quantity—admitting of neither intension nor remission—all such experiments would perforce have to be voted out of court; and, though explanations of the phenomena might be difficult, or even impossible, still the possibility of a variable weight would have to be rejected because incompatible with the accepted doctrine that gravity is invariable. On the other hand, if the truth of our fundamental principles be admitted, then all these experiments, slight as are their indications, will at once become confirmatory evidence.

What we wish to impress on the reader is that it is not direct evidence which always establishes a great truth of physical science. On no account could these experiments establish the theory we are here advancing; nor, indeed, has our theory been deduced from these phenomena, which are merely cited as *confirmatory evidence.* If the principles of persistence, resistance, reciprocity, and equalization are admitted, then these experiments are strongly confirmatory; and that all the more so because the phenomena are at present without any other explanation and contrary to the current theory.

It is in this guarded sense that we advance here the several experiments and observations we have been able to make, as showing that, wherever a deduction made from our theory admits of experimental verification, the results, however feeble, are always confirmatory; and in no case, as far as we are able to see, make for the contrary.

There are, however, other considerations which lead to more definite conclusions. As already mentioned, a platinum crucible after having been heated and just allowed to cool, so as to be no longer sensibly warm to touch, still weighs less, if immediately placed on the balance, than its original weight; and it will often take many minutes until the balance will again assume its normal position. Our explanation of this is that, though cool to touch, the molecules are still in a state of excitation, and hence their lesser weight.

Still more definite are the results of the following experiment. A supersaturated solution of sodium hyposulphite was obtained by dissolving three parts by weight of the salt in one part of hot water, and allowing it to cool. The glass-stoppered flask was then placed on the balance and weighed along with a small crystal of the same salt. By now dropping the crystal into the solution, crystallization set in; and although the weight of solution, flask, and all, did not exceed 60 grammes, and the difference of temperature before and after crystallization was not more than 20° C. (at the utmost), the difference in weight amounted to 25 mgr.; whereas with a brass weight of 100 grammes and a difference of temperature of over 80° C. the difference in weight was only 10 mgr. Whether this difference in weight be attributed to ascending air currents or to expulsion of moisture from the pores of the glass (?), these facts would remain unexplained. Moreover, in the case of heated brass weights equilibrium is established in a comparatively short time, whereas in the case of the hyposulphite it requires several hours.

Another experiment was made as follows. A glass tube sealed at one end was contracted in the middle. In the lower portion were placed about 10 c.c. of water, and in the upper portion a stick of dry potassium hydrate of about an equal weight, and the tube was sealed. After cooling, the tube was weighed and then turned upside down, so that the water could flow on to the potassium hydrate. The stick of potassium hydrate partially dissolved, and the solution crystallized. On weighing it was found to be lighter by about 20 mgr. On shaking the glass the crystals partially redissolved, and the tube became heavier; but after some time the crystals reformed, and the tube weighed again less.

In general, when equal weights of different substances are taken, and both are heated to the same temperature and then replaced on the balance, they will no longer be of equal weight, though after cooling they will regain their former equilibrium.

Another interesting fact bearing on this point is recorded by Faraday. In his *Note on New Electro-Magnetical Motions*[1] he describes some experiments he made in order to verify a conclusion 'that in every part of the terrestrial globe an electro-magnetic wire, if left to the free action of terrestrial magnetism, will move in a plane . . . perpendicular to the dip of the needle and in a direction perpendicular to the current of electricity passing through it.' To prove this he used a copper wire bent twice at right angles, the ends dipping into cups of mercury, and suspended by a thread of silk from a delicate lever, which would indicate any change in the weight of the copper wire. 'The connexions were then made with a voltaic instrument, but I was surprised to find that the wire seemed to become lighter in both directions, though not so much when its motion was towards the south as towards the north. On further trial it was found to ascend on the contacts being made, whatever its position to the magnetic meridian, and *I soon ascertained that it did not depend on the earth's magnetism, nor on any local magnetic action of the conductors, or surrounding bodies on the wire.*' (The italics are ours.) In quoting this observation of Faraday in support of our theory, it is only right to say that he did not so interpret this phenomenon. Being imbued with the belief,

[1] Loc. cit. vol. ii. pp. 151 ff.

founded by Newton, that gravity is an unalterable force, depending simply on mass and distance, it never occurred to Faraday to seek for an explanation of any phenomenon in the variability of the force of gravitation. Thus, when he observed that platinum crucibles weighed less when hot than when cold, he looked for an explanation in the ascending air currents; and when he found an electrified wire to ascend he looked first to magnetism for an explanation. And, having satisfied himself that the phenomenon was not due to what is generally called 'magnetism,' he looked in other directions, and thought to have found the explanation, as will appear from the following statement:—'After some examination I discovered the cause of this unexpected phenomenon. An amalgamated piece of the thin copper wire was dipped into clean mercury . . . In this position the cohesive attraction of the mercury raised a little elevation of the metal round the wire of a certain magnitude, which tended to depress the wire by adding to its weight. When the mercury and the wire were connected with the poles of the voltaic apparatus, this elevation visibly diminished in magnitude by an apparent alteration in the cohesive attraction of the mercury, and a part of the force which before tended to depress the wire was thus removed. This alteration took place equally, whatever the direction in which the current was passing through the wire and the mercury, and *the effects ceased the moment the connexions were broken.*' (The italics are our own.) . . . 'Thus it appears that when a fine amalgamated copper wire dips into mercury, and a current of voltaic electricity passes through the combination, a peculiar effect is produced at the place where the wire first touches the mercury, equivalent to a diminution of the cohesive attraction of the mercury.'

Faraday himself does not seem to have been quite satisfied with this explanation, for he adds:—'Whether the effect is an actual diminution of the attraction of the particles of the mercury, or depends on some other cause, remains as yet to be determined.'

Though confirmatory of our theory, we would not be understood as regarding even this experiment as conclusive. The interpretation of the phenomenon presents difficulties analogous with those of the balance. There is a multiplicity of opposing causes at work, and it is impossible to apportion to each its due share. Copper and mercury, both being excited, would each be drawn in opposite directions by bodies in lower states of excitation. On the other hand, copper and mercury would still attract each other; for, their specific resistance not being the same, one would be more strongly excited than the other (i. e. one would be 'positive' to the other, as when bodies of unequal hardness are rubbed against each other). With all these demonstrable effects it is not incompatible to suppose also that through electrification both mercury and copper are made lighter relatively to the earth; but positive demonstration is difficult, for reasons already stated.

In this chapter we are describing several experiments bearing on this point, but all are open to one and the same objection, viz. that the phenomena admit of other interpretations. The following points, however, are distinctly in favour of our contention:—

1. The fact that the diminution in weight for the same increase of temperature is not the same in different bodies.

2. That in case of bodies weighed during or immediately after crystallization (as with a cold saturated solution of sodium hyposulphite) the loss in weight is considerably greater than with, say, metals heated to a much higher temperature.

The heat evolved by crystallization is slight, the flask being just warm to the touch; and, for purposes of comparison, the metals may be enclosed in similar flasks, with the same general results stated above; that is, the hyposulphite will sustain a much greater temporary loss in weight than the metals. Yet under such conditions, whether the lesser weight be ascribed to air currents or to loss of moisture, the metals (being about 60° C. hotter than the solution) ought to be the lighter.

Much more conclusive than any of these experiments (which, however, are all *confirmatory*) are the phenomena of the heavens, with which we shall deal in subsequent chapters. But an *experimentum crucis*, though not within our own restricted means and opportunities, is by no means impossible; though it would require the resources of some public institution to carry it out. This is to be based on the first of the above-mentioned points, that for the same change in conditions the loss of weight is not the same in different substances.

Pendulums might be constructed of different materials which should swing together, say at Greenwich, and then the same pendulums should again be tried together in different latitudes. Our firm belief is that they would not swing together in different parts; and, moreover, a pendulum, *A*, which swings faster than another pendulum, *B*, near the equator, would swing slower than *B* nearer the poles. If these experiments were made and found to conform to prediction, the point could be decided beyond question. All known facts are, reasoning *a priori*, in favour of this expectation. Pendulums do vary in different parts of our globe; so do the weights of bodies in different latitudes and altitudes. But as to whether these variations are the same or not for *different* substances has never yet been tried; and, as the actual difference must in any case be extremely small, it would escape detection unless expressly looked for.

Once more: the weight of bodies does vary in different parts. And, if the *degree of variation* should be found to be the same for different substances (which is contradicted by balance experiments), it would be *the one solitary property of matter* in respect of which there is sameness—in respect of which the degrees of intension and remission are the same. The exception is too anomalous to be accepted on *a priori* grounds.

Not to be guilty of being carried away by our own theories, and of seizing on any and every point which may be in their favour whilst neglecting points which make for the contrary, we have considerably underestimated the value of the foregoing experiments. We may be pardoned, therefore, if we are now anxious to point out that, inconclusive though the *direct* evidence may be in so great an issue, it is far stronger than any evidence, direct or indirect, on which the current theory is based. In fact, the evidence is insufficient only because of the opposition of an already accepted rival theory. Were it not for this fact, the evidence might be regarded as overwhelming—since facts and conclusions agree so well with each other in every particular.

But the influence, on the mind, of a deep-rooted theory is not to be underrated. Let the reader think for a moment what kind of demonstration would have been likely to be considered conclusive of the fact that plumb-lines converged towards a common centre at the time when this earth was regarded as a flat expanse. The differences by measurement would necessarily have been very small, and the philosopher's preference for a theory with which he was familiar too great to admit such trifling results as convincing. And so it is with our own case. The differences are too slight, and on that account we cannot urge them as conclusive.

But the belief that plumb-lines are parallel has not been abandoned in deference to results obtained by measurements, but in consequence of this being incompatible with the belief in the spherical shape of our earth. Now a person might measure plumb-lines with ever so cunning instruments and try to prove their absolute parallelism, and yet would fail to gain credence. Such is the influence of an accepted theory on 'actual observation' and 'experimental demonstration.'

Every one of the experiments mentioned in this chapter would be 'clear demonstration' if the theory in support of which they have been adduced were already accepted A Cavendish experiment in support of the theory that attraction depends on mass would then scarcely be considered as of any value, *as the swinging round of the lead balls would merely show that there was greater difference of excitation in one direction than in the other:* an explanation which we regard as the true one. The Schiehallion experiment would be regarded as a result that might have been predicted, as the mountain with its mass of *coerced* matter is sure to be in a higher state of excitation than a corresponding volume of air on the other side[1]; whilst the evidence in favour of *the identity of causes* with other phenomena (with which those of gravity agree in every other respect) would then be regarded as so overwhelming as to exclude the possibility of any other explanation.

These remarks are not intended as a rhetorical substitute for facts, but merely as a plea for an impartial consideration of our case, and a warning to the reader lest he may, *unconsciously*, throw his convictions in the balance against our evidence, in which case the latter is bound to be found wanting, whatever its weight.

[1] Yet these are about the only two actual experiments in support of the current theory (both of which can be explained much more satisfactorily on the theory of excitation); whilst the indirect evidence is *nil*.

We have stated before, and repeat it once more, that we do not rely for proof of our theory on *direct* evidence, but on the truth of our inductions. These experiments and observations are dealt with only in order to show that the facts conform to the legitimate deductions from the general principles. If the principles here relied on were a theory deduced from the same body of facts as are to be explained, and framed purposely so as to be capable of supplying a possible explanation, they could not be regarded as trustworthy. But, having established the general laws from phenomena unconnected with gravitation, their truth does not depend on these experiments; whilst their conforming to expected results is confirmatory. And this is all we aim at in this chapter and those that are to follow: viz., to show that the phenomena of gravitation agree with, or are even confirmatory of, the theory; and that in no case is there a known fact that makes positively for the contrary.

And now we shall mention some experiments we have made to test a startling conclusion from these principles. It occurred to us that if the weight of bodies should vary with different temperatures, and vary unequally, then the combining proportions of the chemical elements should vary accordingly; and should do so because in that case what are equivalents at a certain temperature could no longer be equivalents as soon as that temperature is varied. Surely this is a most startling inference; enough to cause any chemist to reject the theory itself on account of this very conclusion to which it leads. And yet, when one comes to consider why such an inference should appear so preposterously incredible, it will be found that it is merely because of the basic assumption that gravity is an unalterable quantity, and not because the contrary is incompatible with our general knowledge of nature. All other equivalents vary. Different bodies exposed to the same change of conditions are not modified to the same extent; they expand and contract unequally; transmit light, heat, electricity, magnetism, and sound unequally; melt, volatilize, or solidify unequally: in short, in respect of any quality we can think of, excepting that of weight, all bodies have their own equivalents, but are never equal. So that on *a priori* grounds of actual experience we should be far more justified in assuming that the intensity of attraction between different bodies, or their *weight*, would also vary. Then why should this inference from principles, that gravity does vary, seem so incredible as to incline one at first to reject the principles rather than admit the inference? On analysis we can find but one answer to this question, which is to be found in the fundamental belief that gravity admits of neither intension nor remission, depending solely on mass, or quantity of matter. But if it be borne in mind that mass and weight are synonymous; that 'matter' is merely a hypothetical assumption; and that 'the quantity of matter,' under this view, is but another way of saying the weight of matter, or the manifestation of certain forces or powers: then it will at once be seen that there is nothing preposterous or incompatible with actual observation in saying that either weight, or quantity, or mass, should vary. For the 'forces'—that is, the intensity with which bodies act on each other—do vary; and, as Faraday has pointed out, it is these forces which constitute the matter.

A priori, therefore, there is no reason why this inference should be regarded as fatal to the theory itself. Yet we must confess that we did so regard it, and were almost ashamed to make the experiments to see whether by any chance the inference might be true nevertheless. Such is the power of accepted ideas and unconscious prejudices. We expected decidedly negative results only, but were rewarded for our infidelity to our principles by more decided confirmatory results than those of any of the other experiments.

Our experiments were conducted as follows. A solution of pure disodium hydrogen phosphate (Na_2HPO_4) was prepared, a few drops of phenol phthaleïne were added thereto, and, after boiling the solution to expel air and CO_2, dilute phosphoric acid was added until but the faintest pink coloration was perceptible when there was a temperature of about 35° C. This solution was introduced into a test-tube, previously drawn out at one end, and again boiled. While still boiling, the blowpipe flame was applied to the drawn-out end, and the tube sealed. During boiling the solution became intensely pink; when cooled down to normal temperature, all colour disappeared, and reappeared again on heating: thus giving an alkaline reaction when hot, and an acid reaction when cold.

This experiment we regard as far more conclusive than the weighing of bodies at different temperatures, because in this instance the objections of ascending air currents, or the expulsion of air and moisture from the pores of bodies by heat, cannot be urged. There are, it is true, other objections of a chemical nature to be made, but fortunately these can be met by actual experiments, as will presently be seen.

The first objection that arose was the polybasic character of the acid used. But we were guided in our first choice by the consideration that we should use non-volatile substances, as far as possible, and fixed on an alkaline phosphate because of the great stability of phosphoric acid; whilst the alkali was dictated by necessity, as it was necessary that the salt should be soluble. It is known, however, that sodium hydrate escapes in appreciable quantities with the aqueous vapour on boiling. Assuming, therefore, that a slight decomposition of the salt takes place on being heated, and that a portion of the base is in a state of vapour in the empty part of the sealed tube, the solution should be acid when hot: but the experiment has shown in the most decided manner that *the hot solution was alkaline.*

Another possible source of error might be the possible presence of CO_2, to which phenol phthaleïne is sensitive[1]. It might be argued that, on heating, these traces of CO_2 are expelled from the liquid, whilst, on cooling, the CO_2 is reabsorbed. To test this, we cooled the lower portion of the tube only. If the discharge of the pink coloration should be due to the reabsorption of CO_2, then the latter would have to pass through the upper portions of the liquid, and the discoloration would have to proceed from the surface downwards, or not at all. The result was most satisfactory; for the lower (cooled) portion, as far as immersed in the cold water, became perfectly colourless, whilst the upper portion of the liquid retained its pinkiness while hot.

But on this point we have even more conclusive evidence. For, on testing different salts in like manner, we found that the alkaline earths behaved in an opposite manner: being strongly alkaline when cold and acid when hot. The experiments

[1] The only indicators we have found to be of any value are phenol phthaleïne and azoresorufine. Even lacmoid undergoes changes when boiled, which invalidates its indications.

were conducted with monobasic acid and base, dibasic, &c., and the results were decisive, in so far as a change of temperature has almost always shown a change in the relative combining proportions. But, as was to be expected, the degree of difference varied in each case. We subjoin a table of a few out of the many substances which have been tried, and the results obtained; specifying those chiefly in which the indications were distinct and reliable. (The merest trace of Ca in an alkaline salt is sufficient to give an opposite result.)

	Cold.	*Hot.*
Sodium chloride	Acid	Alkaline
Potassium ,,	,,	,,
Magnesium ,,	Alkaline	Acid
Calcium ,,	,,	,,
Barium ,,	,,	,,
Sodium sulphate	Acid	Alkaline
Potassium ,,	,,	,,
Magnesium ,,	Alkaline	Acid
Sodium nitrate	Acid	Alkaline (very feebly)
Potassium ,,	No marked effect. If anything, alkaline when cold, and acid on heating	
Sodium tartrate	Acid	Alkaline
Potassium ,,	,,	,,
Sodium oxalate	,,	,,
Potassium ,,	,,	,,
Sodium acetate	No perceptible change	
Barium nitrate	Alkaline	Acid

Ammonium chloride and oxalate, tested with litmus, or lacmoid, seem to be alkaline in the cold, and acid when hot; the latter especially shows a marked difference in this direction. But, owing to the unreliability of the indicator, and the possible presence of traces of calcium (having used the ordinary commercial salt in this instance), we are uncertain of the true behaviour of ammonium salts.

These experiments show that the combining proportions of any two elements which are *equivalent* at any given temperature are equivalent at that temperature only; and that with a change of temperature their relative equivalents vary. Of course, the variation is no greater than what might be attributed to experimental error; which explains why this fact should have escaped detection. It belongs to that class of phenomena which cannot be discovered by direct observation, more especially where an already accepted theory bars even the contemplation of such a possibility. Thus, as before remarked, no amount of experimental evidence could have demonstrated that plumb-lines were not exactly parallel, until the spherical shape of our globe had been recognized. If any-one should doubt this, we would remind him of the fact that the inference made from the Copernican system, that Venus must show phases analogous to the moon, was not admitted by philosophers (!) even after Galileo had pointed his telescope to Venus, and had shown that the inference was actually confirmed by observation. We might point to like circumstances in more recent times, for instance the objections that have been urged against Lyell's theory of the action of waves on geological formations, and Darwin's theory of the origin of species.

Touching these experiments, the critical reader will find that not only is there nothing in the facts to oppose the interpretation here given them, but that these results could be deduced from what are already established facts—were it not that the theory of mass barred the inference. Thus it is known that the combining proportions of the elements, as far as ascertainable, are in proportion to volume. For instance, gases will combine with each other in equal volumes or in multiples thereof; whilst the weights of their combining proportions are most arbitrary. But it is also known that the coefficients of expansion and contraction of the various gases are not precisely the same. From these two facts alone the conclusion is inevitable that the combining proportions by weight cannot be the same at different temperatures. This may explain why the results, as regards the atomic weights, obtained by different experimenters, vary often in a greater degree than can be explained by experimental error. This variation is greatest when the determinations are made by the cryoscopic method, as in that case the solvent employed and the temperature at which congelation takes place must necessarily modify the results.

There is yet another circumstance connected with physico-chemical science from which we hope that new evidence may be obtainable. It is the fact, broadly recognized, that there is a relation between atomic weights and specific heat of the elements. Under this new view this subject might receive attention, and possibly an answer may be found to the Why of the fact that some bodies are lighter than others. It is the belief of the present authors that, if experiments were made in the direction indicated by the theory here advanced, chemistry might lend great assistance to what are called the purely physical sciences; thus again confirming the great interdependence, and even the identity, of what are now considered, owing to our imperfect state of knowledge, as so many distinct sciences.

CHAPTER VI

THE EARTH

THIS chapter will be mainly speculative, in order to show how many familiar phenomena which hitherto could not be accounted for by any known principles of physics could be satisfactorily explained by the principles here advanced. To this we may add that each element of the explanation offered can be verified experimentally.

The attraction of circumterrestrial bodies by our earth has already been attributed to a difference of states of excitation between these exterior bodies and the internal parts of our globe. To prove this, certain experiments have been adduced to show that the gravity of bodies may be increased or decreased in accordance with this law. And, as whatever agencies can increase or diminish any quality must be considered the cause of that quality, we are bound to assume that the attraction by which bodies on the surface of the earth are drawn towards its centre is due to the causes here assigned. Our general knowledge of the interior of the earth well agrees with this view, inasmuch as there is every reason to believe that the interior is in a much higher state of excitation than the exterior. This is already universally accepted as a fact, and is based on what may be regarded as very good evidence. Hot springs, the molten lava of volcanoes, and such other indications coming to us at times from the interior, prove incontestably a source of heat in the interior of our earth far exceeding any temperature which the sun can produce on its surface. This well accords with our own theory.

In a former part it has been mentioned that a mass of molten iron would be cooled on the surface, while the interior would still be in a molten state. Suppose now that we could cast a large mass of iron in the shape of a sphere, and, while cooling, cause it to revolve round an axis. Let us suppose also the gravity of our earth to have no influence on this casting, so that the particles could range themselves undisturbed by any such one-sided attraction as is necessarily acting during the course of our artificial experiments, and consider what would take place.

Let us assume our molten mass to be perfectly homogeneous throughout. Then its surface—that is, at the line of junction with ambient matter, say the surrounding atmosphere—would at once be subjected to an influence essentially different from the conditions of any of the interior particles. The statement of our supposed experiment may be still further simplified by limiting the influence of the external atmosphere to a cooling effect. In that case a pellicle (skin, bark, epidermis, crust, or whatever else we may like to call it [1]) will be formed, which would naturally be in a lower state of excitation than the interior. The innermost and outermost particles would, in consequence, mutually attract each other, the latter being drawn inwards and the former outwards. The result of this would be that the matter of the supposed sphere would divide itself, the matter at the centre flying towards the periphery, thus forming a hollow or partly rarefied core in the centre.

While the exterior is yet soft and plastic, it will contract; but in time the crust is bound to thicken and to harden, until it has reached a degree of rigidity when contraction can no longer keep pace with cooling. Hence the necessary rarefaction of the interior part as this cooling process from outside proceeds inwardly.

Another result would be that the exterior would be colder and harder than the interior, and be densest at a point intermediary between centre and surface. Conceiving such a globe of iron, with a rigid outer crust and a fluid or semi-fluid mass in the interior, to be revolving round an axis, we would have to conclude, in accordance with well-known mechanical principles, that the heaviest particles would fly towards the exterior. But which would be the heaviest particles in such a globe unaffected by our earth? There is no

[1] Although these terms have, by convention and usage, different applications, the *causes* of the different objects for which they severally stand are, in our opinion, the same; i. e. all these are exterior envelopes which develop differently from the remaining mass because subjected to different conditions (external influences).

such thing as absolute weight. Gravity is but another name for attraction. And the 'heaviest' particles in the interior would, therefore, be those particles most strongly attracted towards the exterior; and these would be the particles in highest state of excitation [1].

This, then, is a rough mental representation of the interior state of our earth: a hard, hollow shell filled with matter in a much higher state of excitation than the exterior.

It must further be concluded that what is taking place on the surface of the earth is taking place in reversed order in the interior. Fluid matter on the surface, when hot, rises, and when cold, descends. Thus hot air and hot water ascend, being displaced by colder layers of air or water. In the interior of

[1] 'Heaviest,' that is, relatively to the interior surface of such a hypothetical hollow, and not relatively to ourselves. From the point of view of a man

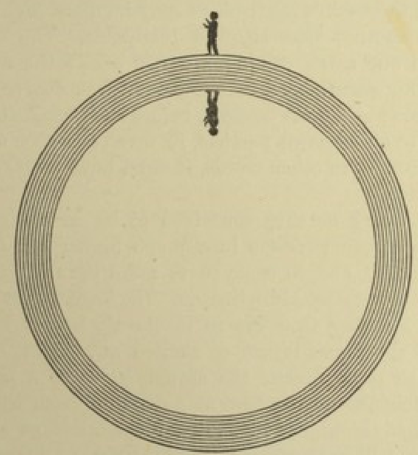

Fig. 50.

standing on the outer surface of the earth, they would be 'lightest,' inasmuch as they press 'upwards.' But, if we suppose a man inside such a hollow sphere with his head in direction of the centre, then bodies which pressed outwards would be regarded by such a man as 'heavy.' It is important to bear in mind the relativity of meaning of this term, so as to avoid confusion. In the annexed figure, for instance, we represent a hollow with a man on either side. From this it will be seen at once that bodies pressing outwards would be 'heavy' to the man in the interior and 'light' to the man on the exterior surface. When we speak, therefore, of bodies being 'light' or 'heavy,' we mean this of the two bodies attracting each other relatively to each other, and not relatively to man's position on the globe.

this crust we must assume corresponding but opposite processes to take place: colder matter falling towards the centre, hotter matter rising towards the surface, pressing against it, partially cooling, and then flying off towards the more highly excited matter in the centre of the earth.

On the surface of the earth these cooling and heating processes in different parts are the cause of air currents, hurricanes, storms, and so on. The same, then, must take place in the interior of our earth. Let us try to account for lightning storms on our earth. The air, as is known, is a very powerful electric, being a bad conductor. It has been shown that air can be electrified; and Professor Tesla has succeeded in electrifying to a very high degree the air of a large room. Now, the electrification of portions of our atmosphere, as has been done by Professor Tesla, shows that the assumption that one portion of the earth's atmosphere may be more highly electrified than another part is quite justifiable. The sun's rays coming to our earth would naturally excite the upper portions of our atmosphere to a much higher degree than the lower layers: and that because, before such excitation can be imparted to an adjacent particle, its own resistance must have been overcome; in other words, it must have been 'electrified to saturation.'

Our air may be thus conceived to be in a high electric state; the upper portions in a much higher state than the lower portions, and in some parts more than in others [1], for reasons to be mentioned further on. The lower or intermediary layers of air may thus act as insulators between the solid earth and the upper layers of excited air. Another circumstance that must favour the accumulation of a much more highly electrified atmosphere at a distance from the earth is the fact that the higher the state of excitation of a molecule the further will it recede. But, whilst air is a bad conductor, moisture conducts very well. And as the air gets saturated with moisture, which here and there is partially condensed, forming streaks in the atmosphere, channels of such veins of

[1] That portions of the air can be electrified to a much higher degree than adjacent portions is proved by the fact that air between the two poles of an electro-magnet can be excited sufficiently to be felt as a viscous fluid when a piece of metal is passed to and fro between the poles. Yet at but a few inches away no such resistance is discoverable. It is analogous when a piece of glass or asbestos is heated to whiteness without the heat being felt in the same piece a few inches further off.

moisture may be established, reaching within such a distance of the highly electrified stratum that the latter can overcome the resistance of the intervening dry air, and discharge itself along these veins, as if a conductor had been provided between earth and the upper electrified stratum. Such discharges can be made artificially, simply by sending up into the air, by means of a kite or a balloon, a good 'conductor,' as has been done by Benjamin Franklin and the unfortunate Richmann.

This will also explain why high mountains intercept clouds so easily; and why in mountainous regions thunderstorms are more frequent than elsewhere [1]. That the air is not a homogeneous mass, but has within itself different densities and distributions, can easily be made visible in any place by gently puffing some smoke into the air and then watching its unequal diffusion. The particles of air cannot be all in the same state of excitation; they dance about in accordance with the law of equalization, those in a higher state of excitation flying towards others in a lower state of excitation. And as the attractive power, or the field of influence, of each particle is necessarily a limited one, there must naturally be many distinct centres of attraction within the atmosphere itself. In wandering over the Yorkshire moors in perfectly calm weather, we were surprised at the marked climatic differences which could be felt within distances not more than two or three yards apart, as we walked along their rugged and undulating slopes. A representation of what the atmospheric condition must be like may be obtained by looking at a mass of clay baked by the sun and irregularly torn in different ways. Or, still better, if a soap solution be made, by dissolving one part of soap in fifteen or twenty parts of water, and then allowing it to cool in a flat basin, it shows veins analogous to the cracks in baked clay.

As corresponding reactions must take place in the interior of the earth, there, too, must we conceive of some parts being excited to a much higher state, other parts at the same time sinking to a much lower state of excitation, either in consequence of abundant local equalization, or from exterior causes. There, too, the different substances possess various

[1] Professor Tyndall, in his *Forms of Water*, gives a good description of such interception of clouds by mountains, with illustrations, to which we would refer the reader.

degrees of resistance, and a high state of tension between two or more disparate parts of the interior may be cumulative until the intervening resistance is overcome, or some conducting veins have connected these two parts, resulting in a reciprocal equalization—or a storm.

That the electricity of our atmosphere is travelling along such veins is sufficiently proved by the zig-zag course which lightning takes. If the air were homogeneous throughout, these discharges could never be seen at all; for in that case they would proceed gradually, regularly, and in straight lines. There would be no sparks seen, because where there is continuity there never are any sparks; these can only be obtained by 'induction': that is, if the free passage of the 'current' is obstructed by a higher resisting medium, but one not strong enough to prevent its passage entirely from body to body. So it is with lightning. The flash is not seen as a continuous streak, but appears here and there, following an irregular course. Now, to account for this, we need only suppose that the veins along which the discharge takes place are here and there broken, and that in these places the electricity, breaking through the higher resisting medium, produces these brilliant sparks.

Regarding, then, the interior mass of our earth as pressing as much outwards as the exterior presses inwards; and that similar changes take place in the interior to those in the exterior atmosphere, giving rise to storms, &c.; we shall at once be able to account for certain phenomena, such as earthquakes, &c., which hitherto have resisted explanation on any of the accepted theories.

We shall mention but one remarkable phenomenon, which some years ago astonished the whole world; i. e. the eruption of Krakatoa. It has been computed that the mass of matter which was then thrown up into the air ascended to a distance of above twenty miles. But still more remarkable is it that this matter remained up in the atmosphere for several weeks. Now, whilst it might be quite possible to conceive local conditions and a local explosion to be such as to be capable of sending matter up twenty or more miles, it is not so easy to account, on current physical theories, for the matter thus shot up not coming down again as quickly. For, when a projectile has reached its maximum height, it would almost immediately commence to descend, and fall with an accelerating speed; and the descent for a distance of twenty miles should not occupy more than a few minutes.

To find a possible, and perhaps, we might add, a probable, explanation of this phenomenon, it is only necessary to inspect the following diagram (Fig. 51), supposed to represent a section of the earth (as we would have to conceive it in the light of our theory, and in accordance with well-known mechanical principles), and to reason therefrom. The earth is here represented as a hard and hollow shell filled with matter of a lesser density than the crust. The hard crust is divided by the line *g* into two concentric zones, *i* and *o*, of which the outer zone is pressing inwards—being attracted by the hotter matter of the interior—and the inner pressing outwards—being attracted by the colder exterior matter, as indicated by the arrows.

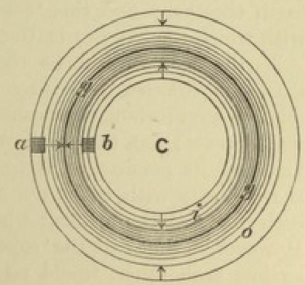

FIG. 51.

Every reaction being mutual, it follows of necessity that when a body is attracted inwards there must be a corresponding mass of matter in the interior, which is at the same time and with an equal intensity being drawn outwards towards that body. The point of attraction, or, more correctly, the *zone of gravitation*, would, according to this view, be along the line *g*; i.e. somewhere midway in the hard crust. Any matter on the inside of this line would press outwards, and matter on the exterior of this line would press inwards. Moreover, the inward pressure of the exterior zone, *o*, would be all the greater the colder it became as compared with the interior state of the earth. From this point of view we might conceive bodies in the interior of this hollow shell[1] to press constantly against the latter with a greater force than a corresponding

[1] It is not necessary to assume that the earth is actually hollow, but merely that the diffusion of the matter composing it, in respect of its state of excitation, is such as to form a zone against which the interior and exterior are pressing. We speak of a hollow earth merely as a matter of convenience, and for the sake of greater clearness of explanation.

mass of matter under similar conditions would press on the outside; inasmuch as the centrifugal force would be favourable to the outward pressure of bodies inside the zone of gravity, and opposed to the pressure of bodies on the outside.

Let, for instance, two bodies, *a* and *b*, in Fig. 51, attract each other in the direction marked by the little arrows; then the attraction of both, as between each other, would be equal; but the centrifugal force would press them both outwards. So that the tendency of *b* to fly to the exterior surface of the earth would be greater than the tendency of *a* to fall inwards.

The outer mass is held together and presses inwards by the attraction exerted on this cooled mass by the internal highly excited matter. It is 'cohesion' which keeps the mass of the earth together; and this 'cohesion' is due to the intensity with which the exterior matter is drawn inwards.

If the earth could be cooled throughout its mass, so that the particles composing it were in a perfect state of relative equilibrium, this 'cohesive force' as an active agency would cease; but the earth would still remain together in virtue of its persistence, and cohere with a tenacity equal to the intensity with which the particles were impelled towards each other at the moment when equalization was completed. However, in that case the 'cohesive force,' pulling the particles against each other, would no longer be a *constantly acting force*[1]; and external influences acting on it might gradually neutralize this resistance, when the least additional disturbance might cause such an orb to 'explode,' the particles being drawn in all directions of the compass, and the whole orb might suddenly be converted into a fragmentary mass.

A wild speculation, forsooth! Yet we can actually illustrate this by a laboratory experiment. When molten glass is dropped into cold water, hard glass tears—the well-known Rupert's drops—are obtained. A slight scratch on these drops causes them to explode. But this scratch is by no means necessary to cause the explosion; at least, not in larger masses of glass. It is a common experience with

[1] According to Proposition VIII (Law of Acceleration and Retardation), the force with which bodies on the surface of the earth press inwards would be cumulative while there was a difference of states between the interior and exterior; so that at the point of perfect equalization of the globe throughout its mass this cohesive power would be at its maximum.

chemists using glass mortars that the latter sometimes explode, apparently without cause, and are shattered into innumerable fragments. The explanation of this phenomenon is supplied by the two generalizations—

1. That bodies are attracted proportionally to their difference of states.

2. That bodies retain every new state with a force equivalent to that under which the state has been acquired.

When the glass is allowed to cool, the particles are drawn against each other by virtue of the difference in states of exterior and interior particles. The idea that all the particles are drawn against each other by a 'centripetal force' must be discarded. In the castings made with lead, &c., above described, we have seen that the central portions are drawn as much outwards as the outer portions are drawn inwards. A Rupert's drop, for instance, must be conceived to have a *zone* of attraction, the particles pressing from the centre outwards, and from the exterior inwards, against this zone.

From this view it follows that the outward pressure is equal to the inward pressure. But during the process of cooling the inward pressure of the exterior would be continually increasing on account of the cumulative effect of 'continually acting forces'; that is, while there is yet a difference of states between interior and exterior, the intensity with which the latter will be drawn in direction of the centre is cumulative; since the pull of every unit of time would have to be added to the intensity already acquired, just as in the case of falling bodies. At the moment of complete equalization throughout the mass this drawing-in would cease to act, and the particles would now cohere with an intensity proportional to that acquired at the moment of the cessation of the force. But, when the force which has drawn the mass together has ceased to act, other forces may still act on it. The particles are attracted in all directions with a greater or lesser intensity, tending to lessen the cohesion of the particles, until a point is reached when this passive cohesive force, or the persistence of the mass, is neutralized; after which the least additional pull in contrary directions would cause the particles to fly asunder, as if *repelling* each other with great force.

We have assumed that, as soon as the mass is cooled, the force by which the particles are drawn against each other ceases to act, and that henceforward this cohesive force is counteracted by the effects produced on the mass by other bodies. This, however, is not strictly true. The action of external influences would still tend to draw the particles more closely together whenever the external conditions were different from the internal conditions; and this must of necessity always more or less obtain. But this difference between exterior and interior is not now so great as in the first instance. So that, taking conductivity and internal resistance of masses into account, the pull in contrary directions may on the whole exceed the reciprocal attractions of the particles; and if the latter be very nearly equal to each other as regards their states of excitation, whilst any new external changes are quickly diffused throughout the mass, their cohesion will gradually be lessened as compared with the pulls the particles receive in opposite directions, until at last a complete rupture takes place.

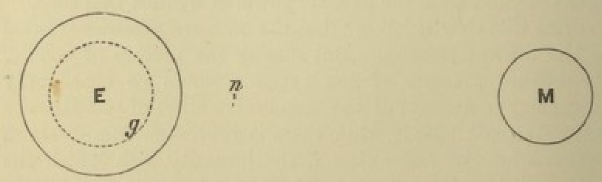

FIG. 52.

The same reasoning applies to our earth. At first the line of gravity or attraction must have been on the exterior of the molten mass, where a thin pellicle would have formed. It is always where two heterogeneous substances, or different conditions, meet that the strongest reaction obtains, and there is the point of greatest attraction. But this colder exterior would soon equalize with the adjacent particles, whereby the crust would be thickened, and the line of greatest reaction, or zone of attraction, would gradually be shifted inwards.

In the above diagram, Fig. 52, let this zone of attraction be represented by *g*. Then from there inwards the heat would be increasing and the attraction decreasing towards the centre, *E*, at which point we may conceive a stable equilibrium, a particle there being equally drawn in all directions. Receding from the line g outwards, the mass would be cooler and cooler, and the attraction become less and less, until a point, n, was reached between earth and sun, or earth and moon, at which a particle would be equally acted on by either sun and earth, or by moon and earth, and hence would be in temporary stable equilibrium.

But while this zone of attraction is moving inwards the matter or bodies exterior to it become equalized relatively to each other; hence, though still drawn *inwardly* towards the zone of gravitation, the particles will have lost their attraction or cohesion for each other, and begin to crumble, or to be rent along their lines of least resistance, in the same manner as a glass mortar is shattered into fragments as soon as the cohesive force has been neutralized.

Assuming, then, a planet, like our earth, to be cooled throughout its mass, the force with which its exterior is drawn inwards would be spent, and henceforth the mass would be held together by its persistence only. This persistence would at first be immense, since the force would have been cumulative while active, as before explained. Nevertheless, in course of time, which may embrace ages according to human thinking, this persistence would be neutralized by external influences, and eventually a point be reached at which the whole mass might suddenly be shattered into atoms, these apparently repelling each other, but in truth being attracted in diverse directions, they having no longer any attraction for each other.

'Gravitation' is by no means an invariable quantity or an invariable 'force.' While acting, it is cumulative, and, when no longer acting—that is, when bodies gravitating towards each other are equalized,—then every degree of counter-influence reduces this power: just as in the case of the steam-engine, as previously explained, while the steam is still expanding, however feebly, the velocity or the 'force' of the engine is increasing, since this 'force' is always adding to the already acquired velocity which the piston strives to retain; whilst, as soon as the expansiveness of the steam (which is the cause of the 'force') is spent, every resistance overcome by the moving piston lessens by so much its power of motion.

Since, then, every 'force,' including that of gravity, is due to a difference of states of the reacting bodies; and since the intensity of the reaction diminishes in proportion as equilibrium is established; it follows that every 'force' has its maximum and its minimum. This applies, of course, to falling bodies, as to every other form of manifestation.

To return to our subject. Assuming the earth to be such a hollow, the interior may be conceived of as the atmosphere of the interior of the earth. Here, too, some bodies will be 'heavier' than others; but their tendency will be in exactly reversed order to matter on the exterior. That is, if we imagine ourselves to be inside such a hollow, we should walk along the interior side of this sphere, our heads being in direction of the centre. And those particles of matter which press most strongly against the 'ground'—i. e. in an outward direction—would there be the 'heaviest'; and these would be the particles in highest state of excitation.

Let us restate this. Matter which on the exterior of the crust would be 'heaviest' would be 'lightest' in the interior; and that which is 'lightest' outside would be 'heaviest' inside; always, however, relatively to the position of man. On the surface of the globe, matter in a higher state of excitation tends to recede from the earth; i. e. is 'lighter,' because of its being displaced by the more strongly attracted 'colder' matter[1]. In the interior a particle which is near the zone and thereby becomes more nearly equalized with the exterior (by being cooled) recedes therefrom, tending towards the centre; here, becoming re-excited, it is again attracted towards the zone, falling against it, and, according to the quantity of the matter and the intensity of the pull, falls with a greater or lesser force towards the interior of the hard crust, causing earthquakes on the exterior of the part where this percussion takes place.

The spectacle we behold on a grand scale in the starry heavens, and which we can see mirrored on a small scale in the particles of dust dancing in a beam of light, is going on in the interior of our earth; is going on within every piece of

[1] For the sake of simplicity, we use 'colder' and 'hotter' instead of the less familiar 'lower and higher states of excitation.' The terms are to be understood in a conventional sense only, and not as implying sensible temperature merely. In respect of the same substance the sensible temperature would, of course, be a safe guide to the determination of relative states of excitation. But when considering different substances the specific heat would have to be considered in addition to sensible temperature in order to arrive at sound conclusions

matter we behold, though insensibly to us; is going on within every drop of water and within every particle of dust. The macrocosm is but a repetition and multiplication of the microcosm [1].

Let it be supposed that a body is somewhere near the centre, *C* (Fig. 51), and is attracted in the direction of *b* by *a*. Then, the force being a continually acting one, its velocity would be accelerative. Say, now, that in falling against the solid crust that part of the earth should be comparatively weak, so that the impact of the falling body could break through it. Then, having arrived at the point of attraction, the force which impelled it would cease to act—just as when the hand is withdrawn after having imparted motion to a stone. But the body would not come to rest there; during its fall it would have acquired a tremendous velocity, and it would persist in its path with a force equal to this acquired velocity. If, therefore, there should be a free passage for it, it would pass through the exterior of the crust and continue in its path until this force was spent. Such a body would not have been *thrown* upwards, but would literally have *fallen* upwards, in accordance with the laws of falling bodies.

But so soon as this highly excited matter had reached the exterior of the crust it would not only be impelled upwards by its already acquired velocity, but, in addition thereto, would be *pressed* upwards by the colder exterior matter pressing downwards. Hence it would tend to recede from the earth, and to remain above it until cooled down to a much lower degree than the aggregate state of excitation of the earth as a whole. And, until this lowering of state of excitation had reached a degree at which attraction between the earth and these particles would be greater than the attraction of the earth on the intermediary matter, they would tend to remain at a corresponding distance. This will supply the explanation why the matter thrown up from Krakatoa did not fall immediately after it had reached its maximum height, but came down very gradually [2].

[1] Still more beautifully can this alternate falling and rising be illustrated by the pith-ball pendulum, or by bringing an excited glass rod near some cork dust, when the light particles will be seen dancing towards and from the glass alternately.

[2] It may be objected that the resistance of the air is sufficient to account for this. But this dust, when it has continued to fall for weeks, once on earth,

We will now consider an imaginary experiment. It is known that bodies dropped down a well or shaft will fall with an accelerated velocity until they strike the bottom of the well or shaft. If two such shafts were made on diametrically opposite sides of the globe, then two bodies thrown into these two shafts would fall in opposite directions. Let us now imagine such a shaft to be continued until it passes right though the earth, and consider what we should expect to happen according to the laws of falling bodies. It is immaterial what point we fix on as the centre of attraction; whether this be at the centre of the earth or at a line somewhere near the line *g* in Fig. 51. Whilst on this side of the point of attraction the velocity of the falling body would be accelerative, the force impelling it, though decreasing, would be cumulative in its effect. Moreover the force would be an increasing one as the distance became less, but a decreasing one in proportion as the falling body, in approaching the point of attraction, approached more nearly to equalization. But, arrived at that point and not being intercepted by an insurmountable obstacle, it would continue in its path with a velocity equal to that it had acquired at this point—modified by the concurrently acting causes just mentioned. It would, therefore, pass beyond it. But, having passed the point of attraction, the 'force' which impelled it would no longer do so, but rather pull it backwards; so that the velocity would now be a gradually decreasing one, until the motion of the body, now due merely to its persistence, would be arrested as soon as it had overcome a corresponding amount of resistance. (In the present case we may assume the resistance to be solely due to the backward pull towards the point of attraction.)

Having thus come to a standstill, and the force by which it had been brought to this standstill—i. e. the backward pull towards the point of attraction—being still active, the body would begin to move backwards towards this point, its velocity being again accelerative, and it would, therefore, again pass beyond the point of attraction. This time, however, not having 'fallen' from an equal distance, it would not go beyond

could not be supported by the air for many seconds if again thrown up; and the resistance of the air at greater altitudes is much less than near the earth's surface.

the point of attraction to the same distance as in the first instance, but would come to rest at a point somewhat nearer. The same process would be repeated and repeated, each excursion beyond the point of attraction being less than the preceding one, until the body would finally come to rest—just as does a swinging pendulum or an oscillating magnet.

Though it is impossible to make such an experiment as is here considered, the correctness of the reasoning cannot be doubted. There is no hypothetical element introduced into it, the assumption being based on well-known physical principles. The only question that may arise is as to whether these principles apply equally to the phenomena of gravitation and to other manifestations. The answer to this is that we must compare the manifestations of gravitation, as far as they are amenable to observation, with other manifestations or 'forces.' And, if they should agree as far as comparable, there is no reason to suppose that they would differ in any other respect. Falling bodies move with an accelerating speed. We have seen the same to be true of every other continually acting force. A piston pushed by expanding steam has its speed accelerated while the steam is expanding, and reaches its maximum velocity at the point where this expansion ceases, after which its velocity gradually decreases. A magnet drawn towards another magnet also moves with an accelerative velocity until just opposite the point of attraction, and then passes beyond that point in the manner explained. We cannot throw a stone through the earth in order to make our demonstration complete; but it is known that two stones dropped on opposite sides of the globe would fall in opposite directions, and that their speed would be accelerative. Are we not bound to conclude that this acceleration would continue up to the point of attraction, and that on arriving at this point any such body would, if unopposed by obstacles, continue its path with the already acquired velocity, in virtue of its persistence?

The action of the pendulum actually demonstrates this to be so. For, though it does not pass through the earth, it has, by the nature of its construction (being suspended), a lowest point of attraction. A pendulum moved out of the vertical falls towards that point, but does not come to rest there. With long pendulums it is easy to see that the velocity in falling towards that lowest point is accelerative, and retardative in its upward journey.

There is yet another point of agreement between falling bodies and other physical phenomena. The 'force' developed by a falling body can be utilized in various ways to produce light, heat, electricity, magnetism, &c., by various well-known contrivances. Now, if we assumed gravity to remain constant, then it might be reasoned that there was a creation of 'energy.' Thus: The body in falling a certain distance can be made to perform work. If the 'gravity' of the body is said to be still the same after having performed this work as before, then there would be distinctly a creation of 'energy'—that is, by so much as the work it had performed in its fall. The usual answer to this is that a body in falling loses so much of its 'potential.' From which it follows that the further a body falls downwards the more nearly it approaches a point at which this power of doing work will be spent. In respect of every other manifestation we know that this point is reached at a state of equilibrium; and, as the phenomena of terrestrial gravity agree with all other phenomena (or 'forces') in every other particular, we can think of no reason why there should be an exception in this respect.

CHAPTER VII

SUN AND EARTH

In applying our theory to planetary motions, it will simplify matters if, for the present, we confine our attention to two bodies only, say the sun and earth, ignoring all the rest of the universe, and then try to deduce the behaviour of two such bodies relatively to each other from the principles we have been discussing. We shall pay no heed as to how far such deductions may agree with facts, or supposed facts, but simply discuss what should happen between two such bodies floating in space, and subject to the foregoing principles, and deal with objections, whether real or apparent, later on.

In the first place, sun and earth would attract each other, being in different states of excitation. And if sun and earth were not simultaneously attracted in opposite directions they would meet. But both sun and earth are pulled at the same time in other directions. Hence the distance of the earth relatively to the sun, and that of the sun relatively to the earth, would depend on the degree of intensity with which they were drawn (*a*) towards each other; and (*b*) from each other in opposite directions. What bodies draw them in other directions we shall not undertake to consider here.

First, then, let us consider the effect of the principles stated in Propositions I and II. As the earth is being heated by the sun, and the sun at the same time cooled by the earth, so their mutual attraction for each other will be lessened. And hence attraction in opposite directions (Proposition III) will be relatively stronger, and sun and earth will recede from each other. Of course, this nearer approach of their relative states of excitation may be due to other causes than their reciprocal equalization. The total excitation of either body might be either increased or decreased from purely internal causes, as has been explained in a previous chapter. But, whichever cause be operative, the result will be the same: *in proportion as the aggregate states of excitation of the two orbs are more nearly equal, the two orbs will recede from each other.*

Confining our remarks for the present to the earth alone, we may assume the internal changes nearly to balance each other, and attribute the increase and decrease of the earth's aggregate state of excitation to the influence of the sun. We do know for a fact that the parts nearest the sun receive a large amount of heat from this body, and that this heat is lost again when those parts are turned away from the sun. On hot days, for instance, more especially in tropical climates, the heat of the sun only is felt. But after sunset hot exhalations arise from the ground as from a heated stove. What parts of the universe are heated by these exhalations—whether this heat is imparted to the cold matter filling space, or whether it is merely heating the more distant and rarefied matter where equalization—away from the earth and in the absence of the sun's influence—can readily take place, and thus this heat be annihilated by equalization, in the manner as when ice is melting or crystals are being dissolved—we are unable to decide. But the fact remains that the earth is receiving heat from the sun and giving it off again.

It may be accepted as another fact which will scarcely be disputed, that when the earth is nearer to the sun the amount of heat received from this source must be greater than when further off. Hence the earth will periodically recede from the sun and approach again towards it, as explained in Propositions III and IV.

But, while this alternate heating and cooling of the earth as a whole is taking place, all parts of the earth will not be equally heated or cooled. That part of the earth turned away from the sun will always be colder than the part turned towards it. This colder part will, therefore, be more strongly attracted by the sun, whilst the hotter part will be more strongly attracted in an opposite direction; whence will result the revolution of the earth round its axis (Proposition V).[1]

[1] This axial revolution owing to the influence of the sun is well illustrated

But, this axial revolution given, and the earth being more strongly attracted by the sun than on the opposite side, this axial revolution must result in a circumsolar motion (Proposition VI). And, as the earth is alternately approaching to and receding from the sun, the path described round the sun must necessarily be elliptical.

These motions, then, would account for Kepler's first law, that the orbits of the planets are elliptical.

Kepler's second law might also be deduced from these principles. For it will be seen at once that, if these explanations can be proved to be true, there would necessarily be a causal connexion between the diurnal and circumsolar motions. The earth would be nearest the sun when comparatively coldest, when it would also move round its axis correspondingly faster, and in consequence also move faster in orbit, whilst, when further away from the sun, the influence of the heat of the latter would be weakened: firstly, because of the greater distance; secondly, because of the already high state of excitation of the earth. Hence the movements of the latter would be correspondingly slower. Further on, however, it will be shown that the second law of Kepler is only an approximation to the truth: just as his first law is now well known not to be strictly true, since the path of the earth is a zig-zag rather than a perfect ellipse.

Again, ignoring the disturbing elements of other celestial bodies, and conceiving earth and sun to be the only two bodies in the universe, that which has been said of the earth would apply equally to the sun, though in a modified degree. The sun would be cooled on one side, and hence its opposite side would be more strongly attracted by the earth, causing the former to revolve round its axis, though at a much slower rate in consequence of its much greater mass. But, moving round its axis and held in position by the earth, the sun would also move round the earth. And when the earth has completed her path round the sun; that is, when the earth in her circumsolar motions has arrived at the same place rela-

by the 'radiometer.' A further proof is afforded by the fact that plants are drawn towards the sun—or light. A hyacinth kept on the mantel-piece, and under a glass shade, bent towards the window. On turning the plant round, it first straightened itself, and then bent in an opposite direction. These instances clearly demonstrate the sun's attraction, the former (radiometer) on inorganic, and the latter on organic, bodies.

tively to the sun: the earth would not have described a complete circle, since it would have been met by the simultaneous and opposite movement of the sun. Whether this latter principle would actually hold good in respect of the sun, it is impossible to say, since the movements of the sun would at the same time be influenced by all other planets; hence the above would be true only on the supposition that sun and earth were the only two bodies in the universe.

But here our attention is arrested by other considerations. The sun as the central orb of a system of planets is in constant reaction with all these bodies scattered in different directions. To each motion of every planet there must, therefore, necessarily be a corresponding counter-impulse actuating the sun. But the sun cannot respond to all these opposing impulses, since it can revolve in one direction only. The effect of this on the sun would be a state of coercion and consequently of *heat*.

This reasoning is based on actual observation. A magnet—whether permanent or electro-magnet—if free to move, will revolve when another magnet is turned round near it, and then will not manifest either heat or electricity. But if not free thus to adjust itself, and the generated excitation is not diffused—as when conducted away by wires and utilized as 'electricity'—the magnet becomes heated. The same happens when the field-magnet is free to rotate, and several armatures are revolved near it, *but in different directions*. In that case the field-magnet cannot respond to all these opposing impulses, *and becomes heated*.

Thus not only can we deduce the complex planetary motions from our simple and well-established principles, but also find a mechanical explanation (verifiable by experiment) for the sun's heat. At present there is not even a tenable hypothesis to account for this constant and apparently undiminishing source of heat. There is a mere assumption that 'in the beginning' the whole universe was hot, but that parts of it have been cooled! But no notice is taken as to what has become of this heat; whilst the theory of 'dissipation of energy,' which affirms that all 'forms of energy' tend to be converted into 'heat,' is altogether opposed to the theory of cooling, even if such a conclusion were not put out of court by the law of reciprocity, according to which there can be no 'cooling' (by radiation) without a corresponding 'heating.' In a previous chapter we have shown that the elements of 'heating' and 'cooling' are contained within bodies themselves, and may take place without interchange of temperature with external bodies. Heating and cooling by radiation are, indeed, but secondary effects, consequent on the existence of some thermal inequalities of bodies. But the *primary source* of heat is due to states (or acts) of coercion. When this heat is merely diffused by radiation it is not lessened in the aggregate; and, if at one time—'in the beginning' of *eternity*?—all matter was in a state of incandescence, there could have been no possible chance of 'cooling' by radiation. But *equalization* offers a solution of this problem that can be confirmed by experiment. Heat is always generated when bodies are coerced into abnormal positions. Springs become heated, whether they are compressed or expanded; the same is true of rubber, and of air. On the other hand, heat disappears with the cessation of coercion.

Thus a perfectly philosophic theory concerning the Cosmos could be formulated which would satisfy the mind by conforming to the necessary conceptions of the eternity of the universe (a succession of changes without beginning or end), and which should be based on true physical principles deduced from actual facts, verifiable by experiment. A constant cooling of the sun should be accompanied by as constant a heating of the planets, including our earth; and it is impossible to believe that there should be no discoverable signs of this, if it were true. But under the view just set forth the explanation of the unabated enormous heat of the sun finds a simple explanation. It is these very planets, which themselves receive heat from the sun by radiation, that create this heat: and Mephisto's remonstrance to Faust, who was bewildered by the spectacle of his own conjurings—

> 'Machst du's doch selbst, das Fratzengeisterspiel!'

finds here a singularly appropriate application.

However, to return to our subject of planetary motions. The foregoing considerations, then, would account for:—

(*a*) The axial revolutions of planets;
(*b*) Their revolutions round a central orb;
(*c*) Their elliptical path round their central orb;

(*d*) Their variable speed;

(*e*) The correspondence between time and areas described.

But now let us turn to the objections, which are great, and which no doubt have already occurred to the reader:—

1. If the circumsolar motions be due to the axial revolutions, then the axis of the earth would have to be at right angles to her path in orbit. But in that case nights and days would be of equal length all over the earth, and the seasons in every part would then also remain constant. Either, then, we should have to account for variable seasons and days and nights by some other theory than the one now accepted; or else the present theory would fall to the ground. We freely admit that this objection, if not met most satisfactorily, would be absolutely fatal to our theory.

2. If the path in orbit be due to the axial revolution of the earth, then the circumference of the earth should be the length of the orbit divided by 365·25, or thereabouts. This would require a diameter for the earth of something like half a million miles.

(We may say at once that we do not question for one moment the measurements which fix the diameter of the *solid* mass of our earth at rather less than 8,000 miles.)

3. The theory requires variable diurnal motions, according to which the days would be unequal in length.

Now, as it is an actual fact that the days are unequal in length, this would seem but a minor objection; yet the fact that the revolutions of our earth relatively to the stars are constant adds another serious objection to the foregoing and much graver objections.

Great and grave as these objections *seem* to be, we shall presently show that they are more apparent than real, and that the discrepancy between the deductions and what are now believed to be the facts can be explained on the simplest mechanical principles; that, in short, the great discrepancy is to be looked for in our mental conceptions of these phenomena, rather than in the facts themselves. In the following chapters we shall deal with these objections seriatim.

CHAPTER VIII

THE CAUSES OF SEASONS AND VARIABLE DAYS AND NIGHTS

THE greatest of the foregoing objections, to our mind, is the first-mentioned one: i.e. the necessity to account for the seasons and the variable days and nights. We regard this, perhaps, as the gravest, because its solution has caused us the greatest embarrassment; and that notwithstanding the fact that the true causes (as we believe them to be) are of the simplest and—*once recognized*—most obvious character.

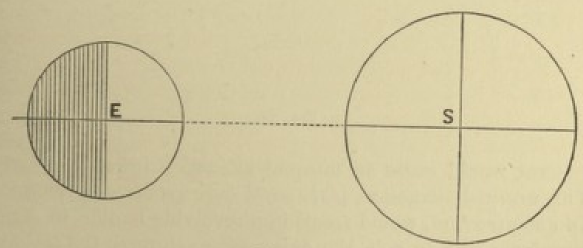

FIG. 53.

First, however, we will consider what other deductions follow from the laws of motion as laid down in the eight propositions in Chapter vi. For the present we will ignore the remaining objections, and temporarily assume them to be non-existent.

In the above illustration (Fig. 53) let *S* represent the sun and *E* the earth, both having their axes parallel to each other, and their equators in the same plane.

In that position the earth would, according to these laws, have three motions, each distinct from the other, and yet all due to the same cause. These motions are—

1. The reciprocating motion of earth and sun to and from each other.

2. The revolution of the earth round its axis.

3. The path in orbit.

It will presently be seen that to these three motions a fourth must be superadded; viz. a motion in direction of the poles.

Very little consideration will suffice to show that the earth could retain its equatorial position in the same plane only if the mass of the earth both north and south of the equator were of the same kind, the same quantity, and distributed in the same manner. The least inequality, either in the quantity of matter or in its aggregate resistance, in the two hemi-

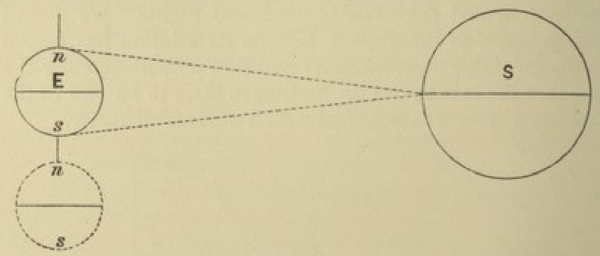

Fig. 54.

spheres, would cause an unequal attraction towards the sun. This unequal attraction, *if the earth were not already revolving in one direction*, would result in a revolving motion in direction from pole to pole, the axis passing through the plane of the equator.

But obviously a sphere cannot simultaneously revolve on two axes; and, if simultaneously impelled in two directions at right angles to each other, the body would respond to the stronger of the two impulses. But the other force, though too feeble to prevent this axial revolution at right angles to it, would still be active; and we shall now consider what its effects must be.

Firstly: The distance of the earth from the sun is at all times determined by its aggregate state of excitation relatively to the latter. The distance of the earth from the sun at any one moment may, therefore, be assumed as if it were fixed; that is, as if held at a particular distance by a cable.

Secondly: The earth is revolving, which gives to its axis a definite position.

Thirdly: There is the *unequal* attraction of the two poles towards the sun, which in its effect would be the same as if one pole were drawn towards the sun and the other pushed from it in an opposite direction. In Fig. 55 the direction of these various impulses is indicated by the arrows.

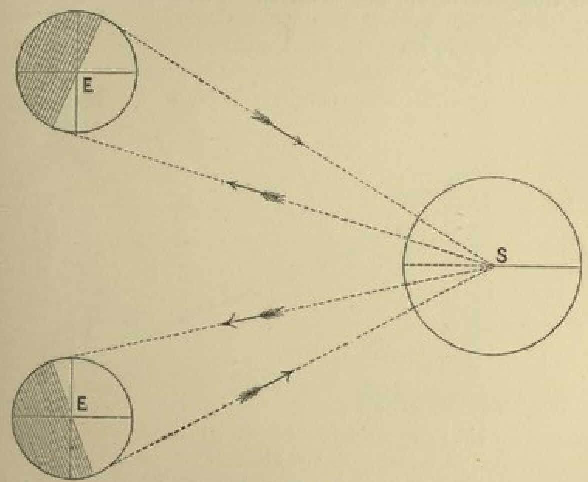

Fig. 55.

From this illustration it will be seen that the effect of these two forces would be to tilt the earth with its *n* pole towards and with its *s* pole from the sun, at the same time retaining the distance of the centre of the earth (or the aggregate distance of the mass) unaltered. But the revolution of the earth gives a fixity to the axis; hence the effect of these two forces is to depress the *n* pole in the direction of the equator, until the earth has assumed a position somewhere approaching to that indicated by the dotted lines (Fig. 54). In this position, however, the *n* pole will be more exposed to the influence of the sun's rays than the *s* pole; and a point will be reached where the sun's attraction for the *s* pole will be greater than for the *n* pole. The result of this would be a return motion in the direction of the axis until the *n* pole is far above the sun, and the *s* pole is more exposed to the sun's rays, as shown in Fig. 55; and so on in endless alternations.

To demonstrate this mechanically, the imaginary axis of the earth need only be conceived as a real spindle round which the earth is revolving, but on which it can slide up and down. In Fig. 56 let *a* be such a spindle, *E* a sphere which can freely move up and down on it, and let *c* be a cord

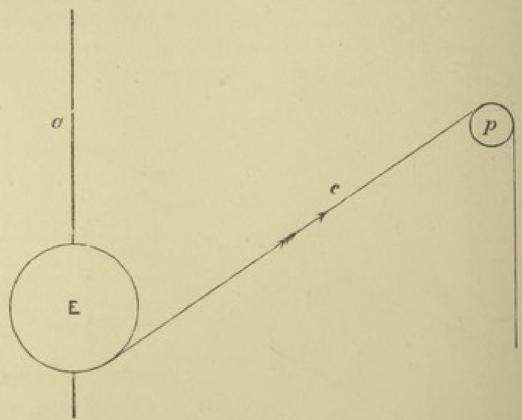

FIG. 56.

passing round a pulley, *p*, and pulling the lower end of this sphere. Then, not being able to move the spindle out of its plane, but the sphere being free to move up and down the same, it will rise on the latter to a higher level.

But the earth, being pulled 'upwards' by *a continually acting force* up to a certain point, will at that point have acquired its maximum velocity, and will, therefore, proceed, by virtue of this acquired velocity, beyond the point of equilibrium, until the backward pull, overcoming this tendency, arrests the motion, and causes the earth to return in an opposite direction.

Thus, then, the earth will have a fourth motion in the direction of its axis, which is due to identically the same causes as its other motions. The result of this fourth motion will be that the circumsolar path is in some parts depressed below and in other parts raised above the plane of the equator; thus taking a direction which cuts the plane of the equator at a certain angle.

The instrument depicted in Fig. 57 is well adapted to

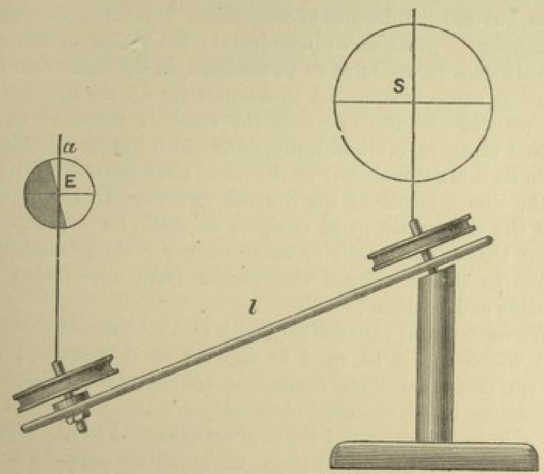

Fig. 57.

illustrate this fourfold motion of the earth, more particularly the fourth motion, as just explained.

The globe, *E*, representing the earth, is supposed to revolve round its axis, *a*. The lever, *l*, must be supposed to be shortened and lengthened while moving round *S*, so as to represent the reciprocating motion of the earth to and from the sun. As *l* is turned round, *E* would revolve round *S*; and the inclination of the lever, as shown in the engraving, would cause the earth to alternately rise and fall below the plane of the equator, as above explained. Thus the orbit would be inclined to the plane of the equator, yet *E* would always have its axis in parallel planes and at right angles to the plane of the equator.

If the instrument be now tilted, so that the lever, l, shall be horizontal, it will be seen at once that the axis of the earth relatively to the lever is in the same position as is generally represented, and which so well accounts for the change of seasons and variable days and nights.

Conceiving the lever to represent the plane of the ecliptic, the axis of the earth is inclined towards it, whichever position we give the instrument. There is, therefore, no difference of phenomena, but only a difference of conception. But this difference of conception is great and fundamental; for, whilst in the one case distinct forces have to be assumed, in the latter case the identical cause is sufficient to account for all the motions of the planets. In the one case the earth is supposed to be carried along in orbit in a direction which has no relation at all to its diurnal motions; in the other the translation in orbit is the direct result of the diurnal motions. In the former case the theory has to be accepted on faith, its only support being that it *is capable of accounting for the phenomena;* whilst in the other case verifications from observation are possible and plentiful.

We cannot deal exhaustively with all that mass of observation which might be cited in support of this theory; nor do we deem ourselves competent to enter into elaborate discussions of astronomical observations. But, from what we are able to gather from published astronomical tables, certain differences in the declination of stars at different times of the year could be well accounted for by this motion of the earth in an axial direction. For let a star be in line with the axis, or near the pole of the heavens, then this 'up and down' motion would not affect its position; whilst a star near the equator would be slightly displaced as the earth alternately rose above or below the equator; that is, its declination would be correspondingly greater or lesser at different times of the year. *And this variation would be greater in respect of some stars than of others, according to their relative positions in the heavens.* It is this last circumstance, the part we have italicized, which we hope will prove, if investigated by astronomers, confirmatory in the last degree of the theory here set forth. But this kind of verification we must leave to those more competent to deal with it.

The planetary motions are thus resolved into four distinct motions, all due to identical causes, as follows:—

1. The reciprocating motions to and from the central orb, *as the aggregate state of excitation of the whole mass varies*[1].

2. The reciprocating motions in polar directions, *as the relative states of excitation of the two hemispheres* (divided by the equator) *vary*.

3. The axial revolutions, *as the relative states of excitation of the two hemispheres* (divided meridionally) *vary*.

These are the three primary motions; the fourth is a secondary result of the third, and its effect is translation in space.

From these considerations it will be seen that all observed facts agree with our explanation at least as well as with the current theory; that no contradiction is involved in assuming the axis of the earth to be at right angles to its circumsolar path; and that the obliquity of the ecliptic can be explained by the 'up and down' motions of the earth on its axis. The only difference between the two explanations is that in the Newtonian theory two motions only are assigned to the earth—assigned, we may say, without any evident cause—whereas we are accounting for four motions directly deduced from general laws, and in accordance with mechanical principles.

But this latter circumstance is not the only point which favours the present theory. According to the laws here explained, the motions of a body will be *accelerative* so long as the force to which this motion is due is still active; and *retardative* beyond that point until the body comes to a temporary rest. According to this the return motion should at first be slow, again accelerative up to a certain point, then gradually diminish in velocity, and so on in succession. *And this is actually the case in respect of all the motions of the earth.* The movement of the earth when in the highest point of the ecliptic seems to be temporarily arrested; then

[1] Some parts of the earth being in a higher state of excitation than others, it follows that all parts of the earth are not drawn with equal force towards the sun. Some parts may be repelled whilst others are attracted. But, the cohesion of these bodies being greater than the opposite effect of the sun which tends to separate them, it follows that the distance of the earth from the sun will at all times be a mean—just as the depth to which a body composed of iron and cork would sink in water would be a mean of the tendencies of the two bodies.

the earth 'descends' with a gradually increasing velocity to the point where the ecliptic cuts the plane of the equator, whereupon the velocity of the 'downward' motion gradually diminishes until the lowest point is reached. Here again there seems to be a temporary rest, and then the earth again ascends the plane of the ecliptic, its velocity gradually increasing, then diminishing, and so forth in endless succession.

The same is true of the reciprocating motion which causes the earth to move towards and away from the sun; and is equally true of the diurnal revolutions and the consequent translation in orbit. The earth moves faster when nearer the sun, its daily motion being increased just in proportion as it approaches towards the sun, and being gradually diminished as it recedes therefrom.

All these four motions can thus be attributed to one and the same cause; the cause itself being capable of demonstration by artificial experiments, and agreeing well in every point with the well-known laws of mechanics. Out of these four distinct motions results the path which planets describe round their central orb.

The observed phenomena, the different positions of the earth at different times and the variable velocities, the seasons—in fact all observations—well agree with this explanation, if it be borne in mind that *motion is not arrested at the exact point of equilibrium, but that it continues beyond this point in virtue of the already acquired velocity.*

There are one or two other points arising out of these motions, more particularly out of the motion of the earth in polar directions, which we will here briefly mention, but which will be dealt with again further on.

A body spinning round its axis, as the disk of a gyroscope, gives to its axis a fixity of direction. The revolutions of such a disk do not in the least interfere with the gyroscope being moved from place to place in any direction, provided it be not attempted to alter the plane of revolution. It is only when it is attempted to bend the axis out of its plane that resistance is felt. But, notwithstanding this resistance, the axis may be bent out of its plane, provided the force be sufficient, and to an extent proportional to this force.

Now we have seen that the greater attraction of the sun for one pole than for the other is a force which tends to bend the axis out of its plane; and there is every reason to believe (from observed phenomena, as well as from purely theoretical considerations) that to a certain extent this does take place; the north pole of the axis being slightly bent in the direction of the sun when away from the latter, and the south pole being slightly drawn towards it when the earth is at the point of greatest 'depression.' Hence the 'up and down' motions of the earth, instead of being represented by a straight line, would more correctly be represented by a line very slightly curved towards its two extremities and nearly straight in regions nearer the plane of the equator.

If within our earth all the conditions and distribution of matter remained constant, the bend at one end would be exactly compensated for by the bend at the other; so that the *mean* position of the axis would remain the same. But, as such constancy of conditions does not obtain, it is possible that the axis may be bent out of its plane more at one end than is the case with the corresponding return bend at the other. On this supposition we shall further on be able to account for what is called 'the nutation of the axis.'

Let us consider now another point. According to the view here given of planetary motions, one is diurnal and three are annual. The motion round the axis is diurnal. The motions to and from the sun and the circumsolar motion are annual, inasmuch as these are the measure of our year. The fourth motion, i. e. the motion in polar directions on the axis, is also annual. Of these we shall here consider the third and fourth only.

The circumsolar motion is, by our supposition, directly due to the diurnal revolutions of our earth; and hence must be attributed *to the unequal excitation of different portions of our planet*. But the motion in polar directions is due to the same cause; and, as the earth revolves in the direction of the equator, it must be assumed that the inequality between the two points in the equator nearest to and furthest from the sun is greater than the difference between pole and pole.

But if this be the case the two periods cannot coincide; that is, a complete circumsolar revolution would require a longer time than the completion of a cycle from the highest to the lowest points of the axis (considering the two motions as distinct). The result of this would be that the 'descending' earth would cut the plane of the equator before the earth had made a complete revolution round the sun. But, as our solar year is reckoned from the time which elapses between two such junctions of the ecliptic and the equator, it follows that it will be somewhat shorter than the sidereal year. In other words, the inequality between these two periods would account for precession.

To make this more plain, let it be supposed that a body revolves round another body, and at the same time receives an 'up and down' reciprocating motion from an independent mechanism. Suppose also this reciprocating motion to take place in a lesser time than the completion of the revolution round the central body. In that case the 'equators' of the two bodies would always come to be in the same plane before a revolution was completed. To complete the analogy, let it be supposed that there is a third body fixed in position, so that the revolving body would be opposite it once in every complete revolution; then it will be clear that the periods when 'the ecliptic cuts the equator'—i. e. when the equators both of the revolving body and of the first stationary body coincide—will be shorter than the period which intervenes between two positions when the revolving body is opposite the fixed point which marks a complete revolution.

Of course, in all these computations it must be borne in mind that, while we are moving round the sun, the sun is also moving amongst the stars, and that the stars themselves are moving in space. And, there being no fixed point in the heavens to serve as a point of reference, it is really impossible to fix the exact time of such periodical revolutions. All we can know for certain is when our earth is in the same position relatively to one or other of the celestial bodies; but what part of such cycle has been described by our earth, and what part would have to be attributed to the translation in space of other celestial objects, we cannot venture to surmise. If the principles here laid down should be found to be true, then by their means it may, perhaps, be possible at some future time to determine the proper motions of two or more bodies in space and relatively to each other; and from such data it might be possible to deduce the required details. But actual observations (except in so far as they may help in making such deductions) are unable to solve this problem. We must conceive ourselves as being located on a particle of dust dancing in a sunbeam amongst innumerable other particles also dancing to and fro in obedience to immutable and eternal laws. We may, forsooth, determine when we are opposite another such particle of dust; but it would be rash to say whether this meeting is due to a periodic motion of that particle, or to a periodic motion of the particle on which we ourselves are located, or whether the motions of both have conspired to produce the effect.

CHAPTER IX

THE DIAMETER OF THE EARTH'S ORB

A mind just entering on the subject may consider it difficult to think of the powers of matter independent of a separate something to be called the matter, *but it is certainly far more difficult, and indeed impossible, to think of or imagine that* matter *independent of the powers. Now the powers we know and recognize in every phenomenon of the creation, the abstract matter in none; why then assume the existence of that of which we are ignorant, which we cannot conceive, and for which there is no philosophical necessity.*
—FARADAY.

WE now come to deal with the second objection; viz. that relating to the diameter of our earth. We have stated that the earth's motion in orbit is due to its diurnal revolutions; that the earth is literally *rolling* in space; and that her circumsolar path is likewise due to the diurnal revolutions.

Taking the mean distance of the earth from the sun to be 92,796,950 miles, the length of its orbit would be about 582,758,566 miles. This latter divided by 365·25 would give a mean distance travelled by the centre of our earth in one axial revolution of 1,591,145 miles. Now, on the assumption that the translation in space is due to the axial revolutions, this distance would be the circumference of the rolling orb; and the diameter of such an orb would be 506,734 miles. This is so much in excess of the 8,000 miles generally assigned to our earth that on the face of it one would be inclined to refuse to listen to any further arguments in support of what seems such a monstrous proposition. Yet not only can this proposition be supported by very strong arguments, but we shall find even some most remarkable verifications.

That this hard kernel of our earth, the diameter of which is about 8,000 miles, is not all that constitutes our planet, needs hardly any proof. It is universally admitted that the solid orb is surrounded by an atmosphere which rotates with it, and which must, therefore, be regarded as part and parcel of the earth considered as a planet in space. The real diameter of the planet would, therefore, be greater in proportion to the thickness of this envelope. To determine the real diameter of the whole planet—i.e. all the matter belonging to it—we should have to know how far this envelope extends. But, before this question can be answered, another question has to be dealt with. First of all, what is it that constitutes our planet; and, secondly, what determines its delimitations in space as against other celestial bodies?

Suppose that we could ascend in a balloon, or by some other means, to an indefinite distance away from the earth, and in the direction of the sun. Then, as we receded from the earth and approached the sun, so the attraction of the former would be lessened and that of the latter increased. A point would then be reached at which such a body would be attracted with an equal force by both sun and earth; and this point would mark the limits of these two bodies relatively to each other.

Such an experiment is, of course, out of the question. We can travel between sun and earth in imagination only. But we might take two magnets of unequal size and place them at a distance from each other, which would practically answer the same object. A particle of iron placed between two such magnets will be attracted by both, provided their distance from each other is well within each other's magnetic field. As this particle is moved to and fro between the poles of the magnets A and B (Fig. 58) it will be attracted more by the one than the other, and there will necessarily be a point of equilibrium; i.e. where the attraction in both directions is exactly equal. Supposing the two magnets to be spheres, then, by drawing an envelope round each sphere, the radius of which shall be represented by the lines cA and cB respectively, c being the point of equilibrium, we should have the relative spheres of influence of such two magnets in respect of each other.

We say *in respect of each other*, because this sphere of influence is relative, as is every other property, and would vary for the same body relatively to other bodies, according to difference in mass, distance, and relative states of excitation of these bodies. Thus, let the magnet A be of the same mass and at the same distance as in the illustration below, but rather less magnetic, then the point *c* would be shifted nearer towards *A*. Or let *A* be more strongly magnetic, then the point *c* will be shifted towards *B*. Or, again, let *A* be at a greater distance, or of a lesser or greater mass, and so forth; in each case the sphere of influence of *B* relatively to such a body would vary, according to the general law of equivalents.

Or, yet again, let two or more magnets be combined in a system, then they would act on another body outside their own system like one body. The companion planets of earth

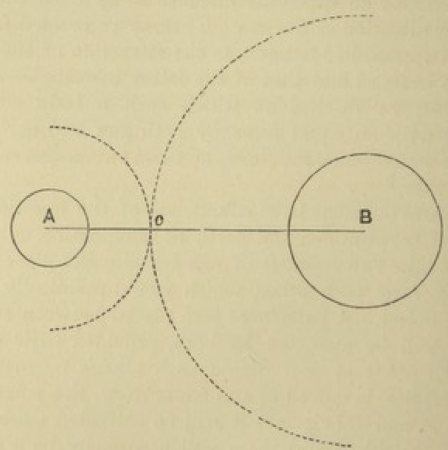

Fig. 58.

and moon may be regarded as such combined bodies. The sun in respect to the planet nearest to it may be regarded as a 'solitary' body, in a conventional sense, comprising all the matter within the sphere of influence of the *mass* of matter constituting the sun, as against the sphere of influence of the mass of matter of the planet opposed to it. But, if we speak of the sun in respect of earth and moon, then all the planets moving within the radius marking off the sun's influence as against the sphere of influence of the earth-moon must be considered as part and parcel of the sun. By including such planets, the total attraction of the sun may thereby be augmented or diminished; and that because attraction does not depend merely on mass, but on relative states of excitation.

If we take a number of magnets, for instance, and combine them into a compound magnet, it will depend on the manner in which they are combined as to whether the attractive force of the whole will be thereby augmented or diminished. A magnet, *A*, for instance, possesses a certain attractive power extending to a certain distance as compared with another magnet. If we now bring to it another magnet, it will depend on how the two are combined as to whether the magnetic field of *A* would be increased or diminished thereby. It would depend on whether this addition would result in an augmentation of the total state of excitation, or whether the combination would result in a greater approach towards equilibrium.

The same applies to celestial bodies. The attractive force of the sun is limited as compared to other bodies. If, for example, the bodies attracted to it were to neutralize its state of excitation down to that of another body, the sun would lose all attraction for such a body, and the latter would be more strongly attracted by some other body, even if of a much lesser mass, provided that its state of excitation differed ever so slightly from the intermediary body in question.

Of these general principles we have spoken already in a previous part, and therefore need not here dwell upon them any further.

Coming back to our main argument, from the length of the earth's orbit and the number of axial revolutions, we have deduced the diameter of the earth's sphere of influence as opposed to that of the sun. On the above computation, assuming the earth to revolve round the sun in a perfect circle, the diameter of this revolving sphere, as compared with that of the sun, would be inversely as the number of axial revolutions which the earth makes in one complete circumsolar revolution. The diameter of the earth's orb[1], as compared with that of the sun, would be as 1 : 365·25 (roughly). How this will tally with observation we shall

[1] We shall use the term 'orb' to denote the space included by the sphere of influence of any body, and in contradistinction to the hard or visible kernel of planets. Thus, when speaking of the 'earth's orb,' we shall mean the earth including its envelope, whatever may be its extent, relatively to some other body.

reserve for the next chapter, devoting the present one exclusively to theoretical considerations.

The chief question is, What is the extent of our planet? The discussion of this point is really more mental than physical. It all depends on what we include in our conception of the earth. If only that mass of matter bounded by the solid crust, i.e. that part of its matter which can support our bodies, then, roughly speaking, 8,000 miles. If that part of the atmosphere to which we can ascend without endangering our existence be included, then a few miles more would have to be added to its diameter. But it is needless to say that even this would not reach the limit of the matter belonging to our earth. The glaring 'dust' which was thrown up from Krakatoa, and which was seen moving round the earth for some weeks, has been computed to have been over twenty miles above the solid crust; and there is no reason whatever to assume that this was the limit of terrestrial matter. It is only proof that the matter beneath that floating dust was temporarily 'heavier,' and therefore could not be displaced by this dust. But can we assume from this that that floating and seemingly incandescent matter was the *lightest* of all—being the 'zero' of weight? Or shall we say that beyond and above it there was matter still 'lighter,' and above this another stratum 'lighter' still, and yet 'lighter,' until—what? Until there was no matter at all? This is just what we cannot conceive of. We cannot conceive of a vacuum in space. 'Lighter' and 'heavier' are relative terms and subjective conceptions. As a body recedes from the earth the attraction of the latter is diminished, and in proportion the attraction of the sun (if the motion of the receding body be in that direction) for that body is increased, until that point of equilibrium is reached where the body, could we experiment on the spot, would have no 'weight' at all until we pushed it a little way sun-wards or earth-wards (assuming, of course, the state of excitation of the said body to remain unaltered). From this point sun-wards the weight of such a body would again increase, but this time in respect of the sun.

Now, 'weight' and 'density' are but *resistance*. It is a mistaken conception to say that resistance is due to matter. It is far more correct, certainly far more philosophic, to say that matter is resistance, for we know of matter by resistance only. We know of relative 'hardness,' relative 'weight,' and relative 'density,' only by relative resistance. The conceptions of 'hardness,' 'weight,' 'density,' are subjective, engendered in the human mind in the infancy of the race, and in conformity with the direct experience of the senses.

Let us pursue this subject a little further, and we shall see how unphilosophic are these vulgar conceptions. The savage presses his thumbnail first into a lump of tallow and then into a piece of chalk, and says 'chalk is harder than tallow.' Our modern physicist does practically the same thing. He can scratch tallow with iron, and, therefore, quite correctly says that iron is harder than tallow. Nevertheless we can make a tallow candle pass through a sheet of iron if it be hurled against it with sufficient velocity. 'Ah! but that is due to impact,' our readers will say. Our answer is that we do not care by what pretty name it may be called; the fact is that the iron could not resist the onslaught of the tallow, whilst the cohesion of the particles of the tallow was sufficiently strong not to be disturbed to any great extent. Nay, far 'softer' bodies than tallow can perforate iron. With a comparatively feeble electric current paper can be perforated, if the two terminals be on opposite sides of the sheet. Glass has been perforated in this manner by overcharged Leyden jars; and there is no reason to suppose that the strongest armour plate might not be perforated as easily as a sheet of paper, provided the 'onslaught' be stronger than the resistance of the iron. What is it when the lightning 'strikes' an object, snapping a giant oak as if it were matchwood, or shattering massive buildings as if they were made of mere dust? We cannot attribute all these forces to 'hardness,' to 'substance,' or to 'mass.' But why the mind always associates such phenomena with 'matter,' 'solidity,' 'hardness,' &c., is because it depends for its conceptions on sense-impressions. It is for this reason that the primitive man spoke of 'thunderbolts' as being 'hurled with a strong arm by an invisible being' (after the manner in which the savage who first interpreted the phenomenon threw his spear or stone), or as of an arrow shot from a bow, as was the conception of the more civilized Greeks.

In philosophy these crude conceptions will not serve, however, and we must conceive such phenomena as being due to relative resistance. Matter itself may be thus conceived as resistance. And the resistance of that matter—either in respect of quantity or quality—will depend on its state relatively to other matter with which it is in reaction. Thus, the resistance of hydrogen and oxygen will be different when in the free state than when combined, and different in different states of excitation; that is, when in the form of gas, liquid, or solid. Again, their resistance will be different relatively to other bodies according to the state of excitation of such bodies. A copper wire laid on a piece of ice will sink into that ice and pass through it; if the copper is artificially heated, it will pass through the ice more readily. In a similar manner the passage of the copper through the solid ice can be accelerated if it be pressed down, or be electrically excited, or if it be attracted by some other body on the other side of the ice. And, in proportion as one body can more readily penetrate into another body, in that proportion are such bodies held to be 'soft' or 'hard' in respect of each other. Ordinarily 'hardness' or 'softness' of bodies is determined in respect of one and the same test, mostly our own muscular strength. But, as we have shown in respect of other manifestations, *all qualities of matter are relative*; and hence the quality of any one body differs according as the body with which it is compared, and the particular state of that body, differ. The term *relative resistance*, therefore, seems to our mind to cover the ground—as far, at least, as the present state of our knowledge is concerned.

From this point of view there will be no difficulty in defining the extent of our planet in respect of any other celestial body. *This will be co-extensive with the limits in space to which it can successfully resist the action of another body, say that of the sun or moon.*

It is a dispute of long standing as to whether a body can act where it is not. With the above view in mind, this question loses all its point and force. Matter does act at a distance; a magnet acts at a distance; the sun acts at a distance; a thousand daily experiences demonstrate to us this action at a distance; and the mind only fails to conceive of it because no distinction is made between *primary* (kinematic) and *secondary* (dynamic) effects. A brick cannot be moved if not touched, nor can it hurt a human being unless it is actually hurled against him; and from such experiences have been deduced the 'qualities of forces.' It is these crude and grossly erroneous conceptions of 'matter' and 'force' which are at the bottom of all this confusion.

Whether all space be filled with matter, need not be discussed. All space is of a surety 'filled' with resistance—more or less in different places for the same body, and more or less in the same place for different bodies. If anyone likes to regard resistance as being due to matter, then all space is filled with matter; and, if anyone should prefer to regard matter as resistance, then all space is filled with resistance. Matter is resistance, and resistance is matter: it is merely a question of terms and conceptions.

This agrees, we believe, with the conclusions of the most prominent thinkers of this century. It is a view substantially held by Lord Kelvin (Sir William Thomson), Professor Huxley, and others. Whilst Michael Faraday, in a letter to Richard Taylor[1], concludes as follows:—'The view now stated of the constitution of matter would seem to involve necessarily the conclusion that matter fills all space, or, at least, all space to which gravitation extends (including the sun and its system); for gravitation is a property of matter dependent on a certain force, *and it is this force which constitutes the matter.*'

From this point of view, the sphere of our planet would be co-extensive with its sphere of attraction; and, in revolving, all matter (or all 'space,' if that term be preferred) within that area would revolve with it, because attracted by and forming part of the mass which reacts with the sun. This hard solid crust would then have to be regarded merely as the centre of a mass of matter (or 'force') analogous to the hub of a wheel. In Fig. 59, let h represent the hub of a wheel, and r, r . . . wires spreading radially all round, the whole rolling against the line L.

Suppose a man sitting on the edge of the dark ring h, and suppose also that the radial wires, r, r . . ., whilst strong enough to maintain h at a certain distance from L, are yet invisible to the man; and you will have a mental representation of what takes place during the revolutions of the earth round the sun.

[1] *Loc. cit.* vol. ii. p. 293.

Nobody will question that at a certain distance between sun and earth a body will be more strongly attracted by the latter than by the former, or by the former than by the latter, as the body may happen to be nearer the one or the other. In other words, nobody will question that the earth has its own field of attraction as against the sun; hence all that is within that field of attraction would form part and parcel of the mass. Now, the distance between earth and sun is at all times determined by their relative states of excitation, as before explained. Hence the earth may be regarded at any one moment as being kept at a certain definite distance from the sun, as if held there by ropes or bars. But the earth revolves, and in revolving meets with a greater resistance on the side nearest the sun than on the opposite side; hence there

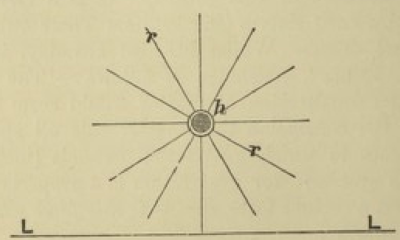

FIG. 59.

is greater retardation on the one side than on the other: from which follows the translation in orbit. The laws of rigid mechanics find, therefore, here application. The earth is drawn towards the sun, but cannot pass a certain line; *and this line is the rigid surface against which it presses.* To the eye nothing may be there impenetrable; but to the earth this invisible circle is like a hoop of adamant, against which it presses and along which it is rolling in space. This 'hardness' where there is nothing visible is demonstrable with magnets or electric batteries. Bring two magnets suspended by strings with their like poles towards each other, and there will be this '*pressure of invisible space*' preventing them from touching each other. Or, as previously mentioned, if a piece of metal be suspended between the two poles of an electro-magnet and made to rotate, and then the electric current be turned on, the metal will suddenly be arrested, and be held there rigid in 'invisible space'; and this though the experiment be made in vacuo, in 'empty space.'

Before coming to our verifications, we may make yet a few other reflections. That our atmosphere revolves with our earth is too obvious: since, if that were not so, we should be exposed to a constant wind sweeping from east to west with a velocity of about a thousand miles per hour. We also know that the air composing the earth's envelope may remain at comparative rest relatively to the earth, or move in various directions—east, north, south, or west—as if the earth and atmosphere did not revolve at all, and make together a journey round the sun at a tremendous speed. In fact, earth and envelope, and all that is contained within it, may be likened unto a boat which is scudding along the ocean, but whose speed leaves unaffected the dispositions and relative positions of the people and objects in that boat. They could move to and fro, and never notice that the vessel in which they are moving and have their being is changing its place relatively to other external objects.

Our earth is practically such a floating body, in and within which myriads of motions are taking place. Man and animals, rivers, air-currents, and so forth, are continually changing place, without adding to or taking from the quantity of matter which constitutes this planet. The waters of a river are moving, on them a vessel is scudding along, within that vessel men are moving to and fro, and within each man there are again thousands of motions and currents. And, if we wish to pursue our reflections further, we might take a single microscopic cell or blood corpuscle, and we should still see within it a miniature facsimile of the universe: bodies moving to and fro, changing relative positions, but without altering the shape or the quantity of matter of the body to which these particles belong.

From whichever point we may regard nature, we see the same spectacle, and find the same laws, the greater being always but a multiplication of the lesser.

The theory of vortices propounded by Descartes, and so strongly combated by Newton and his followers, is not opposed to, nor incompatible with, the Newtonian theory of gravitation, but rather complementary to it. Descartes saw a truth, and so did Newton; but each of them saw one side only, and, like the two pilgrims in the parable of the shield, each of them thought to see all there was to be seen or known. The motion of 'vortex within vortex' is not a theory but a fact; it does not, forsooth, explain anything, but rather is itself a phenomenon requiring explanation. There are motions within the small blood corpuscles, while the blood corpuscle itself is moving within man, man moving within a ship, the ship moving on the earth, the earth round the sun, and the whole solar system through infinite space. Newton saw another aspect of the same grand natural spectacle; he contemplated only the *force* with which bodies attract each other. 'This is immutable,' he said; 'their gravity is diminished as they recede from the earth.' This, too, is a fact, and not an explanation. And just as Descartes tried to deduce an explanation from what *he* saw—vortex within vortex—so Newton tried to deduce explanations from relative attraction. The two theories are neither incompatible with, nor do they exclude, each other, but are supplementary of each other; and both but parts of the more inclusive theory set forth in these pages in rough outlines.

That bodies are moving within bodies, without by their change of positions within those bodies affecting the motions of the latter, is a fact, though it does not explain the causes of these motions. But the same can be said of the Newtonian theory. That the attraction is lessened as bodies recede from each other, is a fact; but it does not give an explanation of the cause of this attraction, nor account for its variability. Our own belief is that, as regards bodies in space, it is not the greater distance which is the cause of the lesser attraction, so much as that the lesser attraction is the cause of bodies receding from each other; the cause of this lesser attraction being a greater approach to relative equilibrium.

As the difference in state of excitation between any two bodies, or systems of bodies, is increased or lessened, so do they approach to or recede from each other. But, whilst the intervening resistance between any two bodies may temporarily, or periodically, be increased or diminished, they may still hold to each other and behave as a whole body in respect of another body, because their attraction for each other is still greater than the attraction between either and a third body.

Thus we have large masses of matter which behave in respect of other masses of matter like one solitary body. Yet within this mass there are parts which are distinct from each other, changing their relative positions within that larger mass, without ceasing to be part of the same, and partaking of all its motions. Within these parts still smaller bodies are distinguishable; and, within these, yet smaller ones, until the dazzled mind takes refuge in the 'ultimate atoms.'

A 'body,' therefore, may be defined in the same terms as we have previously defined a 'molecule'; viz. as 'a mass of matter which in respect of another mass of matter behaves as one body.' If we speak, therefore, of the earth relatively to an object on its surface, then the solid globe only, with all that is in and on it, save the one body with which it is compared, is comprised in the term. But, when we speak of the earth relatively to the sun, then all that which is comprised within the earth's sphere of influence as against the sun must be comprised in the term. So, likewise, the term 'sun' when used in opposition to the earth would comprise all that is within the sun's sphere of influence relatively to the earth. Thus, dividing the distance between sun and earth into parts of 1 : 365·25, Venus and Mercury would form part and parcel of the sun, whilst the moon would clearly fall within the dominion of the earth. If we compare the sun to Jupiter, then the earth itself, and several of the other planets, would form part of the term 'sun.' And, if our whole solar system be compared with other solar systems, then the complex 'molecule' constituting the 'sun' would include all the planets, asteroids, &c., that fall within its sphere of influence as against that of the other sun to which it is opposed.

Under this view, and when speaking of the sun relatively to the exterior planets, or relatively to other systems, we may regard the earth as a speck of matter floating in the sun's atmosphere. Indeed, when we look at the dust particles dancing in a sun-beam; at the blood corpuscles moving to and fro in a drop of liquid; or at the planetary motions in space; we see but the same spectacle, due to the operations of the same eternal laws.

CHAPTER X

THE DIAMETER OF THE EARTH'S ORB (*continued*)

THAT the extent of the earth's orb relatively to the sun is equal to a radius of about 250,000 miles, has been deduced from purely theoretical considerations. We were led to make these speculations from the conclusion that the circumsolar motion of the earth is due to her axial revolutions. So far, then, we were still on purely theoretical grounds. We had absolutely no facts to rely on when making these speculations, to which we were prompted solely by the strong belief that a body free to move could not possibly revolve in space without such revolution resulting in translation in space.

When a pulley revolves, it remains in position only because it is generally kept there by a fixed spindle. But, if a board, free to move, be brought near such a revolving pulley, this board is forwarded by the pulley; which shows that, if the board were fixed and the revolving pulley free to move, the latter would move along the board. When a top is spun perfectly vertically, it does not manifest any such tendency to change its place, and that because the atmosphere—i. e. the resistance—is equal on all sides; besides, a certain amount of friction retains it in position. But if a top be spun on a smooth and level plane—a polished table or a glass slab—and a sheet of paper be gently pressed against it, the top at once rolls along that sheet of paper in a contrary direction to its axial revolutions; i. e. after the manner of a wheel rolling along the ground. This experiment is very easily made, and, if the paper be alternately held on opposite sides of the spinning top, the latter will move in opposite directions; i. e. will always roll along the paper held against it.

Thus the diameter of the earth's orb was calculated as above because the theory required it; and is based, therefore, entirely on theory. Having arrived at this stage, we looked for some data of verification. It is important to note that all the data we shall hereafter mention were not known to us at the time when the above theory and the foregoing conclusion, that the orb of the earth must be so immensely greater than what is generally assigned to it, were established in our minds. We attach importance to this circumstance, because, had all the data presently to be mentioned been previously known to us and formed the basis of the theory, the agreement between facts and theory would have been of no value in verifying the latter. But, inasmuch as all the data have been looked up since, and all of them so well agree with the theory, it would be a most remarkable coincidence if this agreement were purely accidental.

In the first place, we naturally were anxious to know whether *the direction* of the diurnal revolutions and motion in orbit would be in accordance with the above supposition. Our readers, more familiar with astronomy than we were at the time will not require to be informed that the motion in orbit of the earth, as of all the other planets, is in the same direction as if the planets were rolling against a solid circle, as above explained.

The assigned inclination of axis to orbit has been dealt with in a previous chapter, where the phenomenon (or the hypothesis rather, since the supposed inclination of the axis to the path in orbit is merely a hypothesis to account for the seasons) has been explained on mechanical laws and strictly in accordance with our general principles.

So far, then, everything agrees with the supposition save the enormous diameter that would have to be given to the earth. Calculating from the length of orbit and number of revolutions the diameter that would have to be assigned to the other planets, we were agreeably surprised to find that in the case of Jupiter and Saturn the figures agree most remarkably with the requirements of the theory. Thus, dividing the length of orbit of Jupiter by the number of axial rotations which the latter makes in one circumsolar revolution, and thence calculating the diameter of such an orb, we obtained a diameter for Jupiter of 90,000 miles; and for Saturn of

70,848 miles. The observed diameters are 84,346 miles and 70,136 miles respectively; which is certainly a most remarkable approximation[1].

It was this remarkable agreement of the above figures which led us to return to our theory, which we at first had hopelessly given up because of the great discrepancy between the actual diameter and that required by the theory.

Another point of interest to note is that the radius of the earth's orb agrees so remarkably with the distance between earth and moon; and, as the latter is known to belong to the earth as against the sun, there is nothing wild or hypothetical in the assumption that the revolving sphere within which this solid globe is situated extends from the centre to a distance of about 250,000 miles.

That the extent of the revolving orb must be immense seems evident from the fact of the enormous velocity with which our planet is travelling in its orbit. A body travelling at a rate of eighteen miles a second, without the least sensation of this being felt on the surface of the earth, certainly requires some explanation, if the enveloping atmosphere be supposed to extend no further than a few miles. A mass moving at such a tremendous rate would compress the atmosphere in front of it, rarefying the hinder atmosphere, which it would drag along like the tail of a comet, whilst the two sides would be almost atmosphereless and be swept by tremendous hurricanes. (Of course, it might be argued that where there is no atmosphere there can be no hurricane; but this, as already stated, is just what we cannot conceive of. Matter is drawn either by earth or sun; and hence will be *more rarefied* in some parts than in others. But an absolute vacuum in space is to us unthinkable.)

If we take a glass of water and gently rotate it, the water at first remains quiescent; but gradually motion is imparted to it by friction, at first to that part nearest to the sides of the glass, and this motion is gradually transmitted towards the centre, until the whole mass of water revolves with the

[1] See Appendix to this chapter. The times of rotation for Uranus and Neptune are not known from observation, but on these lines it would be easy to calculate them. Taking the diameter of Uranus to be 33,247 miles and its mean distance from the sun to be 1,753,869,000 miles, it should revolve in about 6h. 59′ 50″. Taking the diameter of Neptune to be 33,276 miles and its mean distance from the sun to be 2,745 998,000 miles, it should revolve in about 8h. 44′ 35″.

glass. If the vessel be now brought to rest, the water still continues to rotate for some time, until by the retarding influence of the sides of the vessel, the former process being reversed, the water is also brought again to a state of rest. Bearing this in mind, it is difficult to conceive how a body revolving on its axis, having a velocity on its surface of about a thousand miles per hour, and flying through space with a velocity of about 66,300 miles per hour, could, with a surrounding atmosphere of twenty or thirty miles only, retain this atmosphere so utterly unaffected by these motions as is actually the case. It is different, however, if we conceive this whole mass of matter—the solid earth *and* atmosphere—to revolve against another body, and through this revolution effect translation in space. For in the latter case, no matter how great the velocity may be, it would leave the atmosphere undisturbed.

Calculating in like manner the diameters of the other planets, as required by the above theory, we obtained the figures here given and placed side by side with the observed diameters[1].

Name of Planet.	Calculated Diameter.	Observed Diameter.
Mercury	808,036	3,058
Venus	572,836	7,510
Earth	499,344	7,926
Mars	415,978	4,363
Jupiter	90,800	84,846
Saturn	70,848	70,136
Uranus	45,243[2]	33,247
Neptune	. . . .	33,276

From this table it will be seen that the *observed* diameters vary irregularly; there being an increase from Mercury to Earth, then a decrease in the case of Mars, again an increase in the case of Jupiter, decreasing again in Saturn, Uranus, and Neptune. In the *calculated* diameters that o

[1] See Appendix to this chapter. The figures in the third column were taken from the *Encyclopaedia Britannica* (Ninth Edition).

[2] This figure is based on the supposition that the axial revolution of this planet is performed in 9h. 30m. But in the table from which we copy (see previous note) two marks of interrogation are put against this, thus characterizing it as very doubtful. Cf. note to p. 442.

Neptune is not included, the data (its time of rotation) not being available. But the others exhibit a remarkable phenomenon; viz. a gradual decrease from the interior to the exterior planets, which it is difficult to attribute to mere chance. Whatever may have been the true causes of the formation of our solar system, some regularity might be expected, which the observed diameters do not exhibit. But, so soon as we calculate the diameters of the planets as above, we not only have regularity, inasmuch as these gradually decrease, but the greatest volume is nearest the sun,—the volume of the sun itself being the greatest of all.

There is yet another point—a very striking one—to be noted. As the planets recede from the sun their 'density' is less; i.e. if we take the solid parts only. (We still use the term 'density' in accordance with accepted usage. It would be more correct, however, to speak of the lesser attraction, rather than of the lesser density, of these exterior planets, since 'density' is but an inference from the force of gravity which they display.) According to our general theory, a body would be the further from the sun *the higher its state of excitation*, and this agrees remarkably well with the phenomena; since the further planets are known (from their greater volumes, relatively to their effects) to be in a more rarefied state than those nearer the sun[1].

And now we might look to our general principles for an explanation why Jupiter and Saturn so closely agree with what is required by theory, as compared with the great discrepancy in respect of the planets nearer the sun. According to our general theory of excitation, those planets nearest the sun would be the coldest; and those furthest therefrom the hottest. We mean, of course, the internal heat, or the aggregate state of excitation, and not that the rays of the sun falling on Mercury or Venus are colder than the sun's rays falling on Jupiter. According to theory, Neptune and Jupiter more nearly approach to the state of the sun than any of the planets interior to them; a conclusion supported by the comparatively small attraction of these planets as compared with their volumes; and hence

[1] We might also point out here that the velocities of the planets in their orbits also decrease the further they are from the sun. This also well agrees with our general theory; for reasons which are obvious.

the supposed 'lightness' or inferior 'densities' of these bodies. They are most probably in a semi-incandescent state, and hence we see either the whole or nearly the whole atmosphere of these planets, or else the difference between atmosphere and central mass is but slight. On the other hand, concerning the nearer planets, we can penetrate the atmosphere and distinguish the internal hard 'kernel' of each planet from its surrounding atmosphere; and hence are led into the error of taking such dark and visible mass to be all there is of the planet.

According to this view, then, the bodies nearest the sun are the 'coldest,' and those furthest therefrom the 'hottest'[1]. But this *furthest* would but mark the extreme limit of the sun's influence relatively to some other sun. In Fig. 60, let S represent our sun, S' some other sun, and the shaded circles between the two represent planets. Then, the further we recede from S in the direction of S', the higher will be the state of excitation of these planets until we have approached say x. Thence, again, they will become 'colder' and 'colder' as they approach more nearly towards S'. Those between x and S would belong to one system, and those between x and S' to the other system.

FIG. 60.

Thus, the difference between suns and planets is again shown to be merely a difference of degree.

But now we must consider an objection. The companion planets, Earth and Moon, are included in the same orb, whilst, in the case of Jupiter and Saturn, their moons have not been included. The objection is a valid but not necessarily a serious one. Of this exception we cannot offer an explanation based on facts, but we can see the possibility of such an explanation being found in the direction we shall presently

[1] Not necessarily, however, as regards their surface temperature, which must be regarded as distinct from the *aggregate* heat. Thus the interior of our earth, which can throw up highly refractory silicates in a molten state, must be of a temperature beyond any artificial heat producible on the surface. In view of this it will be seen how small must be the heat which planets receive from the sun as compared with their own internal heat.

indicate. Our own strong point is that the theory, deduced from principles discovered in other fields, and not from the phenomena to the explanation of which it is here applied, agrees with so large a number of facts that we are bound to say, with Darwin, that we can hardly believe that a false theory would be capable of explaining them so readily and so satisfactorily.

The agreement of the calculated diameters with those observed in the case of Jupiter and Saturn, and also of that of the companion planets of Earth and Moon (and modern views of astronomers agree in regarding the moon as a companion planet rather than as a satellite of the earth); the agreement of the *direction* of rotation with that required by the theory; the accounting for planetary motions and for Kepler's laws by deductions from general principles, and in strict accordance with demonstrable laws of mechanics and general physics; the correspondence between the decreased power of attraction, volume for volume, and their higher state of excitation when further from the sun, as required by the theory: these and many other agreements, pointed out and yet to be mentioned, are too remarkable to permit of our abandoning the theory itself, because some exceptions—in themselves not necessarily fatal—yet require explanation.

We say the objection is not necessarily fatal, because, if we cannot explain it, we can at least show that an explanation is not impossible. According to theory, as well as according to observations, both Jupiter and Saturn more nearly approach the character of a sun (as regards volume and lesser 'density') than any of the planets interior to them. This circumstance, as well as the great distance of these planets from the sun, seem to admit the hypothesis that for small bodies in a much lower state of excitation than themselves, and at that distance, Jupiter, Saturn, and the exterior planets might act as central orbs, without necessarily affecting the volume-relations of these planets relatively to the sun. In other words, Jupiter and Saturn might act as true 'suns' to their satellites, whereas Earth and Moon might be regarded as *twin* planets. There are many points of difference between our moon and the satellites of these distant planets, and it is by no means improbable that the latter may have to be classed differently to the former. We do not know what the calculated 'densities' of these satellites are; but, if these should be found to be much greater than those of their central orbs, we should be inclined to regard this as confirmatory of the above view.

As before said, we do not offer this as a satisfactory explanation, but merely as showing that such is not impossible. And probably those better acquainted with astronomical facts than the present authors might be able to supply it, and perhaps, at the same time, also find an explanation of the many anomalies and irregularities of these distant satellites.

APPENDIX TO CHAPTER X, BOOK IV.

SYNOPTIC TABLE OF PLANETS.

	1	2	3	4	5	6	7	8	9
	Mean Distance from Sun[a]. (in miles)	Length of Orbit[b]. (in miles)	Periodic Time[a]. (in days)	Axial Revolution[a]. (in days)	Number of Rotations in Periodic Time[c].	Calculated Diameter of orb[d]. (in miles)	Observed Diameter of planet[a]. (in miles)	Mean Daily Motion[a].	Density[a]. (Earth as 1)
Mercury	35,392,000	222,261,760	87·9693	1·0038(?)	87·6	808,036	3,058	14,732·419	1·12
Venus	66,134,000	415,321,520	224·7008	0·9731 (?)	230·9	572,836	7,510	5,767·668	1·03
Earth	91,430,000	574,180,400	365·2564	1	365·2564	500,626	7,926	3,548·193	1·00
Mars	139,311,000	874,873,080	686·9797	1·0257	669·8	415,978	4,363	1,886·518	0·70
Jupiter	475,692,000	2,987,345,760	4,332·5848	0·4135	10,477·8	90,800	84,846	299·129	0·24
Saturn	872,137,000	5,477,020,360	10,759·2198	0·4370	24,620·6	70,848	70,136	120·455	0·13
Uranus	1,753,869,000	11,014,297,320	30,686·8208	0·3958 (?)	77,531·1	45,243	33,247	42·233	0·17
Neptune	2,745,998,000	17,244,867,440	60,126·7200	Unknown			33,276	21·406	0·16

[a] The figures in these columns are taken from the *Encyclopaedia Britannica* (Ninth Edition).

[b] Mean Distance × 6·28.

[c] Periodic Time divided by Time of Rotation $\left(\frac{\text{Column 3}}{\text{Column 4}}\right)$.

[d] Mean Distance from Sun multiplied by 2 and divided by number of rotations.

NOTE.—*In this table all the columns save the fourth and seventh show a regular increase or decrease.*

CHAPTER XI

SUGGESTIONS CONCERNING SUN SPOTS

We should not have touched on the subject of sun spots were it not for the fact that our theory has led us to the inference that, the nearer a planet is to the sun, the lower is its aggregate state of excitation; and, conversely, the further away it is, the higher is its state of excitation. This inference, it seemed to us, required substantiating. Not, indeed, because it is contrary to any known facts, nor because there is anything in the now current theories which conflicts with it. But, though there are no reasons, either as a matter of theory or from observation, for assuming that the nearer planets are, *in the aggregate*, in a higher state of excitation than those further away, we are under the impression that this is nevertheless supposed to be the case. Hence we thought it desirable to show that there is nothing pertaining to actual observation that is incompatible with our contrary conclusion, and that the tacit assumption that the planets nearer the sun are hotter is due to the habit of regarding the sun as the sole source of heat of the whole system.

But it is hardly possible to apply this theory without being forced to the conclusion that the sun spots may be planets in a very low state of excitation—what some writers inappropriately call 'dead' or 'died-out' planets: a term frequently applied to our moon—in more or less close proximity to the sun. The evidence, therefore, we shall here tender in support of our deduction, that, the further away a planet is from the central sun, the higher is its *aggregate* state of excitation, and vice versa, may at the same time offer a simple explanation of the phenomena of sun spots; whilst we ourselves are inclined to regard these phenomena as confirmatory evidence of our own theory. The many peculiarities of sun spots, which present so many difficulties from the point of view of accepted theories, are the very elements which give high probability to our own explanations.

That the coldest planet should be nearest the sun is a necessity if the cause of gravitation here assigned be a true one. We will not speak here of the mass of evidence we have adduced in support of the general principle that attraction is proportional to difference of states, but for the present will confine our remarks strictly to the phenomena presented by celestial objects. The accepted 'density' of the various planets decreases in proportion as they are further away from the sun. This is hardly in accordance with the idea that a planet is colder the further away it is from the sun, since density and lower temperatures are both manifestations of the same state. But it well agrees with the theory here advanced. Of course, the idea of the further planets being colder is entirely due to unconsciously attributing the heat of such bodies to the influence of the sun. Unquestionably Neptune receives less heat from the sun than does Mercury, Venus, or the Earth. But the fixed stars are still further away, and receive still less heat from this source. Yet no one will question that these enormously distant suns are, nevertheless, much hotter than any of the planets in our solar system. When we say, therefore, that Neptune is much hotter than any of the nearer planets, we are referring to its aggregate state of excitation, and not to its surface temperature. Of the earth we know for a certainty that it possesses a source of heat of its own, quite independent of that of the sun. Moreover, the rays of the sun affect the exterior surface only, and this heat does not seem to penetrate far into the interior. Compared with the heat our earth receives from the sun, the interior heat is far greater. At no part of the earth's surface could the sun's rays convert pumice-stone into a molten mass, or produce such intense heat as that in the interior, of which there is constant evidence.

There is nothing incompatible, then, in the statement that bodies nearer the sun are in a lesser state of *aggregate* excitation than those further away, whilst all known facts support this view. The rays of the sun, great as is their influence, cannot materially change the state of a planet throughout its mass. The sum-total of the reactions that are going on within a planet far outweighs the influence exerted on it by the sun's rays. And even if we assumed a planet like our earth to be perfectly cold throughout its mass, and that such a planet, in consequence thereof, should come very near to the sun, even to its surface, the sun could not so quickly heat such a mass of matter throughout, since the resistance of such a cold body would be immense.

If a lump of ice be thrown into boiling water, or on to a red-hot brick, it is not instantly melted and converted into vapour. A very sensible time elapses before it melts. We have kept in a room, in a tin can on a table at a distance of about two yards from the fire, a lump of ice about four inches wide and six inches deep, and it took more than twenty-four hours before all was melted.

But in respect of planets another important circumstance is to be borne in mind, viz. the immense resistance acquired through untold ages. We have seen that resistance is proportional to the force to which a body owes its state; and that, the greater the force that was required to coerce matter into a certain state, the greater will be its resistance to change. We have shown by experiment that the melting-point of tallow may be increased by rapid cooling; air when liquefied, and carbonic anhydride when solidified, retain their new states with a greater tenacity than bodies which are more easily converted into liquids or solids. Furthermore, we have seen that while a force is acting its effects are cumulative. Thus bodies on our earth, under the influence of the causes assigned, are drawn against each other with an ever-increasing intensity so long as these causes are active. And, even when these causes are no longer active, the cohesion of such bodies, or their attraction for each other, will be proportional to the *duration* and *intensity* of the active causes—owing to their acquired persistence.

The resistance, therefore, of such a planet to the influence of the heat of the sun would be immense. And, though such a mass of matter may be heated on the exterior surface to a very high degree, it would take ages before the heat would penetrate the whole mass and bring it to a state of incandescence. Such a body would probably revolve very quickly, and dance to and fro on the sun's surface, as does a lump of sodium or potassium on water, having one side turned towards the incandescent sun, and the other towards the extremely low atmosphere of Mercury; and thus be intensely heated on one side and very strongly cooled on the other.

This view may help in formulating a theory to account for the remarkable phenomena of sun spots and their 'irregular periodicity,' if we may use this phrase.

But without entering much further into this subject we would say a few words as regards the insufficiency of the current theory regarding sun spots. The view is generally held that the exterior only of the sun is in an incandescent state, there being a dark kernel in the interior. This theory is entirely opposed to what the phenomena of our own planet would lead us to believe to be the facts. In the case of the sun we should have to assume that the cooling process started in the interior, proceeding outwardly, which is contrary to what the constitution of our earth teaches; or that the sun has 'caught fire' on the exterior. The latter view is too ridiculous to bear discussion, whilst the former is contrary to observation. But, under the general theory here advanced, the explanation is both simple and natural, being in accordance with all phenomena accessible to investigation.

On February 20, 1894, a sun spot was seen with the naked eye, and had a circular shape. If it were true that planets are all the hotter the nearer they are to the sun, we should hardly be able to distinguish them from the sun itself. Still less is it conceivable to us that, within a body in such a high state of incandescence as the sun, the difference of temperature should be so great as to make these colder parts to appear as dark spots. There can be no question as to changes of temperature, storms, &c., on and within the sun. But a depression of temperature in one part, more especially in a highly excited mass, could never attain to any great difference, on account of the specific equalization which would constantly be taking place. The local cooling effect of planets on the sun (unless they be very near and *very cold*) must be infinitesimal compared with its mass and high temperature [1];

[1] The extreme temperatures of any two parts of our globe are not more than 100° C. apart, and this difference is due to the sun's influence. But the cooling effect of the planets on the sun must be less than the heating effect of the latter on the former.

and internal disturbances could not produce such local inequalities to an extent sufficient to be distinctly recognizable by the earth-dwellers. Still less could we account on this theory for the *periodicity* of these phenomena.

But under the above view a rational theory is possible, the elements of which could be demonstrated experimentally. For instance, let a planet with several satellites be very near the sun. One result would be that this whole system, i. e. principal planet and satellites, would appear at a distance as one solid mass, and hence of irregular shape; and, as the satellites would change their positions, the shape of this mass would constantly vary.

The explanation of this is a physiological one, which we can here but briefly touch upon. What we *see* is really not the external object, but an excitation in our own eyes. Say that one cell in our retina is strongly excited, then, by the

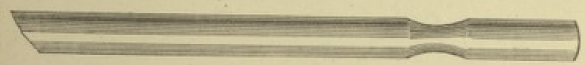

Fig. 61.

law of equalization, the adjacent cells will also be affected. An object will, therefore, seem larger (or the nearer, from experience and association) the stronger the excitation received[1]. If, then, two bodies make an impression on the eye at the same time, the one impression being much stronger than the other, the one which produces the stronger sensation on our eye will be 'seen' to a greater extent than the other object. By way of illustrating and proving this statement, we need only hold a pencil up to our eye, and then gaze at the sun or some other strong light, and the pencil will look thinner in those parts which have the light for background, as shown in Fig. 61. The narrowed part shows how that

[1] The image thrown on the retina by a magnifying glass is actually smaller than if looked at without a glass, but the *intensity* of the impression is much stronger. This may be proved by placing a sheet of paper behind an ordinary opera-glass and allowing the light passing through the instrument to fall on this paper. The illuminated disk on the latter, when placed where the eyes should be, is much smaller than when placed at the opposite end; but, the intensity in the former case being greater than in the latter, the object appears to be magnified. In truth, however, the sensation is stronger, and thereby conveys the idea of greater size, and, by association, of greater nearness.

part of the pencil, which is perfectly cylindrical, would appear to the eye when illuminated by the light on the other side.

Let the retina be represented by the following diagram (Fig. 62); the light portions, *l*, *l*, being those illuminated by the light, and the shaded portion, marked *p*, being that protected by the pencil. Then the cells on the margin of the shaded lines would affect the adjacent cells, endeavouring to lower their state of excitation; whilst the more highly excited cells in *l* would endeavour to excite the protected cells, in accordance with the law of equalization. If the relative intensity of the two impressions be equal, they will evenly balance each other, and the two objects will be seen normally. But, if one impression should preponderate over

FIG. 62.

the other, the one will be 'seen' to a proportionally greater extent. Thus let a thin black stroke be made on a white ground and looked at from a distance, then the black stroke will not be seen at all, the whole sheet appearing white. And, if a white stroke be made on a black ground, the whole will appear black from a certain distance, the white not being seen at all. On looking at the dial of a clock from a comparatively small distance, the white spaces between the numerals II, III, IV, &c., will not be seen, but appear as black patches, for the reasons assigned[1].

Still better is this illustrated by a revolving disk. If the greater part of the dial be white and a small portion only black, then, while revolving, the black will not be seen, its effects being nullified by the overpowering sensation of white. In like manner, when the black is in excess, on revolving, the

[1] In gazing at the sun, when a tree is interposed between the latter and the eyes, we have often observed that the branches of the tree shading the sun were entirely, or almost entirely, invisible, and the disk of the sun was seen as if through a tunnel in the foliage. On coming nearer to the tree, the thicker branches became visible; and the thinner branches could be seen only when coming very near to the tree.

whole disk will appear to be black. But, if black and white are in equivalent proportions, the impressions received will be a mean of the two, and the disk will appear as grey.

Suppose now a planet and its moons to be very near the sun, say just a few million miles away. Then the spaces between the moons and the central planet, being comparatively small as seen from the distance of the earth, would be extinguished; and a system of planets which, when looked at from a comparatively small distance, would be seen as distinct disks may at a much greater distance from us, and near to the great luminary, have the appearance of an irregular dark blotch. The spaces between the moons and the planet would appear quite dark[1]; the reason for which is that as the two impressions approach each other they at once unite, and that because the excitation of the intervening cells of the retina is overpowered. It is the common experience of astronomers that as the sun or some other celestial body comes within the telescopic field it jumps all of a sudden on to the wires, for reasons just given. This may be illustrated as follows. If thumb and forefinger be brought near each other, but without actually touching, while looking at a strong light, they will be seen as if connected by two protuberances.

The irregularity in the shape of sun spots presents, therefore, no difficulty to the theory that they are planets but a comparatively short distance from the sun, and revolving round it[2]. If very near the sun, earth and moon would not be seen, at this distance, as two distinct bodies, but as an oblong patch, narrowed towards the centre, with an umbra and a penumbra. Moreover, all the observed phenomena of sun spots—their large size; their equatorial position relatively to the sun; the uniformity of their movements, as regards the direction of their revolutions, with those of the other bodies of the solar system; their displacement

[1] Though not so dark as the planets themselves. There would be a shading from the body to the penumbra, unless the space between two planets were very small.

[2] In his *Popular Astronomy*, Flammarion mentions that 'one of the most eminent mathematicians that ever existed,' Le Verrier, after rigorously analyzing the motions of the planets, found perturbations in the motions of the planet Mercury 'which are not explained by the action of the other planets, and which could be explained if there were between Mercury and the Sun one or more planets revolving round the central body.'

relatively to each other—all seem to lend support to the above view.

At first it may seem difficult to conceive of (comparatively) cold bodies near to the sun. But the sun spots are a fact; and it is no more difficult to conceive of cold bodies a few million miles away from the sun than immediately[1] beneath the highly incandescent envelope,—as is implied by current explanations of these phenomena.

[1] Professor Helmholtz estimated the depth of the luminous envelope at 500 (!) miles. This estimate, considering the distance of the sun, is ludicrous, even if we put German leagues for miles.

CHAPTER XII

THE TESTIMONY OF THE STARS

In this chapter we propose to deal briefly, and in a general way only, with the objection arising from the testimony of the stars, which is supposed to prove incontestably that the diurnal revolutions of our earth are performed in equal times. Our theory, on the other hand, affirms that the speed of the diurnal revolutions varies, being greatest when the earth is nearest the sun, and least when furthest from this luminary. It also requires that the acceleration and retardation in speed, when approaching the sun or receding from it, shall be gradual.

Relatively to the sun, this theoretically required difference in speed, as well as this gradual increase and diminution, are well sustained by actual observation. But here we are met by the difficulty presented by the more regular appearance of the stars on the meridian. We say advisedly 'more regular,' because their regularity is by no means so perfect as might be inferred from the average text-book, as will presently be shown.

But first let us assume, by way of argument, that the stars follow each other in regular times, and appear on the meridian at perfectly regular intervals, whilst the sun appears on the meridian at varying periods. Then, as far as the phenomena go, we should have to assume either a proper motion for the sun, the stars remaining relatively fixed; or a proper motion for the stars, the sun remaining fixed relatively to the earth. Of these two the probability is unquestionably in favour of the former assumption—that is, if these two assumptions were the only possible alternatives. That, however, is not so. There are many causes which are operative, and which may

THE RIGHT ASCENSION OF α AND δ URSAE MINORIS.

(See page 459.)

DATE.	R.A. α *Ursae Minoris.*	R.A. δ *Ursae Minoris.*	INTERVAL.	± AS AGAINST PRECEDING INTERVAL.
	H M S	*H M S*	*H M S*	*S*
January 1	1 18 33·44	18 7 31·43	16 48 57·99	
,, 10	1 18 24·60	18 7 31·66	16 49 7·06	+ 9·07
,, 20	1 18 15·06	18 7 32·67	16 49 17·61	+ 10·55
February 1	1 18 3·43	18 7 34·84	16 49 31·41	+ 13·80
,, 10	1 17 54·74	18 7 37·13	16 49 42·39	+ 10·98
,, 20	1 17 46·62	18 7 39·32	16 49 53·40	+ 11·01
March 1	1 17 40·57	18 7 42· 9	16 50 2·22	+ 8·82
,, 10	1 17 34·91	18 7 46·05	16 50 11·14	+ 8·92
,, 20	1 17 30·62	18 7 49·59	16 50 18·97	+ 7·83
April 1	1 17 28·08	18 7 53·81	16 50 25 73	+ 6·76
,, 11	1 17 28·16	18 7 57·21	16 50 29·05	+ 3·32
,, 20	1 17 29·42	18 8 0·15	16 50 30·73	+ 1·68
May 1	1 17 32·85	18 8 3·08	16 50 30·23	− 0·50
,, 10	1 17 37·67	18 8 5·01	16 50 27·34	− 2·89
,, 20	1 17 44·10	18 8 6·80	16 50 22·70	− 4·64
June 1	1 17 52·96	18 8 8·13	16 50 15·17	− 7·53
,, 10	1 18 0·34	18 8 8·40	16 50 8·06	− 7·11
,, 20	1 18 9·85	18 8 8·07	16 49 58·22	− 9·82
July 1	1 18 20·49	18 8 7·17	16 49 46·68	− 11·54
,, 10	1 18 28·84	18 8 5·87	16 49 37·03	− 9·65
,, 20	1 18 38·36	18 8 3·67	16 49 25·31	− 11·72
August 1	1 18 49·54	18 8 0·47	16 49 10·93	− 14·38
,, 10	1 18 57·56	18 7 57·75	16 49 0·18	− 10·75
,, 20	1 19 5·04	18 7 54·27	16 48 49·23	− 10·95
September 1	1 19 13·30	18 7 49·62	16 48 36·32	− 12·91
,, 10	1 19 18·88	18 7 45·80	16 48 26·92	− 9·40
,, 20	1 19 23·62	18 7 41·68	16 48 18·06	− 8·86
October 1	1 19 27·03	18 7 36·97	16 48 9·94	− 8·12
,, 10	1 19 28·57	18 7 32·98	16 48 4·41	− 5·53
,, 20	1 19 29·41	18 7 28·48	16 47 59·07	− 5·34
November 1	1 19 27·95	18 7 24·21	16 47 56·26	− 2·81
,, 10	1 19 24·90	18 7 21·11	16 47 56·21	− 0·05
,, 20	1 19 20·65	18 7 17·88	16 47 57·23	+ 1·02
December 1	1 19 14·58	18 7 15·05	16 48 0·47	+ 3·24
,, 10	1 19 8·09	18 7 13·45	16 48 5·36	+ 4·89
,, 20	1 18 59·62	18 7 12·14	16 48 12·52	+ 7·16

account for the phenomena without our having to assume a perfectly equable diurnal motion for the earth.

1. The sun is moving in space, carrying the earth along with it.

2. The *apparent* path of the sun round the earth is inclined to the equator.

3. The path is elliptical.

4. There are the phenomena of aberration.

5. Difference in parallax of the stars, as seen from different positions of the earth in her orbit; which, however small, is sure to affect observations in some slight degree.

All these, and possibly many other causes of which we are as yet ignorant, may conspire to produce those differences in time to which are due the variable daily periods of the sun's appearance on the meridian.

We do not rely, therefore, on the evidence afforded by the sun's motion to prove the variable diurnal speed of our earth, as this may be due to a multiplicity of causes. But we do affirm that the variable speed of the earth in approaching and receding from the sun is *one* of these causes.

Now, the proof of this deduction should not be a difficult one; for, if true, any two stars should follow each other in periods which are longer or shorter, according as the revolutions of the earth are performed in a lesser or greater time.

Let us fix on two stars, say *α Ursae Minoris* and *δ Ursae Minoris*. The right ascension of these two stars, as given in the *Nautical Almanac* for 1890, for the days specified, will be found in Columns 2 and 3 of the opposite table. In Column 4 is given the interval of time which elapses between the transit of these two stars.

Assuming the distance of the two stars to be constant, and that the earth revolves with an equable motion, the interval of time between the transits of the two stars should always be the same. An inspection of the table, however, will show that this is not the case. The time which elapses between the two transits increases from January 1 to April 20 (that is, according to our imperfect table, in which thirty-six days only are specified); then the interval gradually decreases to November 10, after which there is again a gradual increase. Now when the earth moves fastest, which is in perihelion, the interval would be shorter than when the earth revolves more slowly, which would be in aphelion. And this agrees with the table.

Similar differences can be shown for other stars, though often there seem to be irregularities; but never of such a nature as to invalidate this general conclusion. In the evidence of the stars there is, therefore, nothing incompatible with the conclusion that the earth does not revolve with an equable motion; the only question can be as to the extent of this variation. It is certain that this variation in motion does not take place to an extent sufficient to account for the different lengths of the solar days. Equally certain is it that the causes put forth to account for the difference in the length of solar days account for part only of these differences, but not for the whole. There is left a hitherto unaccounted-for residuum.

Further than this we dare not venture to enter into this intricate subject. What we believe we may state as certain is that the reputed accuracy with which the stars make their appearance, as set forth in text-books, is not borne out by actual observations as set forth in more responsible publications. But then it is a common experience with text-books that they adapt the facts to an accepted theory, or carve and turn them so as to make certain theories appear the more exact.

CHAPTER XIII

PRECESSION AND NUTATION

In the production of natural phenomena two things always come into play, the intensity of the acting force, and the time during which it acts. Make the intensity great, and the time small, and you have sudden convulsion; but precisely the same apparent effect may be produced by making the intensity small, and the time great.—TYNDALL.

WHATEVER may be the *actual* motions of sun and planets relatively to each other, and however difficult it may be to deduce these in the presence of several such bodies, each having a disturbing influence on the others, it is not difficult to prove the laws themselves to which these motions are due.

These motions, all of which follow of necessity from one primal cause, we have resolved into four distinct kinds; viz.—

1. The reciprocating motions to and from the central orb, *as the aggregate state of excitation of the whole mass varies.*

2. The reciprocating motions in polar directions, *as the relative states of excitation of the two hemispheres* (divided by the equator) *vary.*

3. The axial revolutions, *as the relative states of excitation of the two hemispheres* (divided meridionally) *vary.*

4. The translation in space, *in consequence of the axial revolutions.*

The motions (1) and (4) result in the elliptical path. Motions (2) and (4) conspire to give this path an inclination to the plane of the equator. And the four motions taken together produce the phenomena of planetary movements.

Now the motions enumerated under the heads (2) and (3), i. e. the diurnal revolutions and the movement in direction of the axis, are both due to one and the same cause; i. e. *to the* *unequal distribution of excitation in opposite parts of the planet.*

The earth turns the side furthest from the sun towards the latter, because this part is more strongly attracted by the sun; and one of the poles moves alternately towards the sun, by an 'up and down' motion, for precisely the same reason. Were the planets not revolving already in the direction of the equator, a difference of excitation at the two poles would result in a revolution at right angles to that of the equator; and what are now the opposite poles of the earth would be turned alternately towards the sun.

Let a ball be thus simultaneously impelled in two directions, but with an unequal force, and such ball will revolve in obedience to the stronger impulse. From the very fact that the earth is revolving on its axis in the direction of the equator, we must conclude that the differences around the equator are greater than those round a circle that would pass through the two poles.

Thus there are two periodic motions of the earth, distinct from each other: the one round the sun, resulting from the diurnal revolutions of the earth; the other in the direction of the axis, or the 'up and down' motion of the planet, due to the difference in excitation near the two poles; and to this motion is due the inclination of the ecliptic.

Now, if these two periods were of exactly the same duration, the ecliptic would always cut the plane of the equator in the same place. But, if the period of the 'up and down' motion of the planet were shorter than that of the circumsolar motion, the path of the planet would cut the plane of the equator before the circle round the sun was completed; in other words, the nodes would have a retrograde movement.

This circumstance would account for the phenomenon of precession. Our solar year is always reckoned from a point where the two planes cut each other, at which point it is supposed that the earth has made a complete circumsolar revolution. On the assumption that the earth travels in the ecliptic impelled by a forward acting force, this would be true. But, if we resolve this motion into two distinct motions, it will at once be seen that such a point of intersection need not necessarily coincide with a complete revolution.

This view will explain another phenomenon of our system, and will again exhibit harmony and uniformity where at present there is seeming irregularity. It has been a cause of perplexity to Laplace, and others since his time, that, notwithstanding so many indications of uniformity in the solar system, the several planets seem all to be moving in different planes. But, under the above view, it will be seen at once that their several motions may actually be regarded as taking place in the same plane, from which each of them is making gradual, temporary, and regularly periodic excursions to the north and south.

Suppose, for instance, that all the planets belonging to our system were placed in line, one behind the other, the plane of the equator of each coinciding with that of the sun, and then let them start in their several movements according to the principles above explained. Having different velocities round their axes, different diameters, and being at different distances from the sun, the straight line would at once be broken. And, being not only at different distances from the sun, but also in different states of excitation, the extent to which each planet would move with its poles above and below the plane of the equator respectively would also vary. If, then, the actual motions be traced out, the planes enclosed by such paths would be, as a matter of necessity, at different inclinations; and that notwithstanding that the planets have all started from, and tend to revolve in, the same plane.

Nutation.—Hitherto we have assumed that this 'bobbing up and down' of the planets is taking place in a straight line, and in the direction of the axis. From a consideration of mechanical principles, however, it is hardly conceivable that the stronger attraction of the sun for one pole, and the simultaneous repulsion of the opposite pole, should have no effect on the position of the axis itself. It is quite true that revolution gives stability to the axis, as we may readily convince ourselves by means of the gyroscope. But the same instrument also enables us to demonstrate that the stability of the axis due to revolution has its limits, and that the axis may be deflected out of its plane just in proportion to the force applied in this direction.

The action of the sun on the two poles may be much less than would be required to deflect the axis to any considerable extent; nevertheless its effects cannot remain altogether unfelt. Thus, when the earth is in the extreme northern part of the ecliptic, the north pole of the axis may be very slightly inclined one way, righting itself again during the earth's southward course to a corresponding degree. If this were not so, the pole-star should always remain equidistant from the terrestrial pole; but, as is well known, this is not the case.

Here, for instance, are the north declinations of *a Ursae Minoris* at different times of the year 1890, as given in the *Nautical Almanac* for that year:—

January 1 . . .	88°	43′	32·00″
February 1. . .	88°	43′	32·3″
March 1 . . .	88°	43′	27·6″
April 1	88°	43′	18·9″
May 1	88°	43′	9·7″
June 1	88°	43′	3·4″
July 1	88°	43′	1·9″
August 1 . . .	88°	43′	5·3″
September 1 . .	88°	43′	13·3″
October 1 . . .	88°	43′	23·9″
November 1 . .	88°	43′	35·9″
December 1 . .	88°	43′	45·9″

It will be noticed that the variations are *gradual and periodic.*

We will now take a star nearer the equator, say γ *Geminorum*, whose north declination for the times stated was as follows:—

January 1 . . .	16°	29′	31·8″
February 10 . .	16°	29′	31·8″
March 2. . . .	16°	29′	31·4″
April 1	16°	29′	31·9″
May 1	16°	29′	32·4″
June 10	16°	29′	33·5″
July 10	16°	29′	34·8″
August 9 . . .	16°	29′	36·1″
September 8 . .	16°	29′	36·9″
October 8 . . .	16°	29′	36·5″
November 7 . .	16°	29′	35″
December 7 . .	16°	29′	33·2″

It will be noticed that the variation in latitude of this star is markedly less than that of the preceding one.

Assuming the stars to remain in the same relative positions with each other, this variation would have to be accounted for by the motions of our earth; and we will endeavour to show in what manner the theory of the earth's motion, as deduced in these pages from our principles, would satisfactorily account both for the periodic variations and the differences in variation.

There is good reason to believe that the positions of the stars relatively to each other do not change sufficiently to be noticed by a terrestrial observer. In any case, where we find the variations to be periodic, and these periods to coincide with the periodic motions of the earth, the inference is a safe one

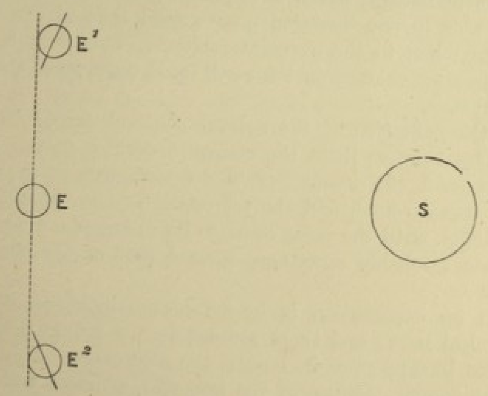

FIG 63.

that such appearances must be due to different positions of the earth, rather than to any proper motions of the stars. Now, a slight alternate and periodical inclination of the axis, as here described, would seem to account satisfactorily for these phenomena.

In the above diagram (Fig. 63) let S represent the sun; E, E^1, and E^2, the earth in three positions relatively to the sun; the dotted line being an elongation of the earth's axis when in the position E, and showing the deviations of the earth's axis from this line (extremely exaggerated) in the positions E^1, E^2, respectively. Then it will be clear that a star near the pole of the heavens would vary more than one in an equatorial direction; and that because in the latter direction the distance from E to E^2 would not make a sensible parallax, on account of the great distance of the stars; whilst in a polar direction a star would be thrown out of position all the more the greater its distance.

Let us now consider for a moment what might result in the course of long periods from such infinitesimally slight changes. If all the conditions affecting the poles of our earth remained constant, these alternate inclinations of the axis in travelling 'up and down' would exactly compensate each other. But these *exactly constant conditions* are just what we can discover nowhere in nature; hence it is possible that the inclination of the axis in one direction may exceed that in the other direction when in the opposite position. This may be due either to some changes in the earth itself, or to some changes external to it.

Let the deflection of the axis at one end exceed in each period by ever so little the return deflection in the other position, and the result would be that the poles would slowly travel round, and the terrestrial equator would shift accordingly, until the poles came to lie in the plane which at one time (probably ages ago) formed part of the plane of the equator.

Such an explanation is by no means inconsistent with mechanical laws; and there are some very strong points of evidence in support of it, besides the phenomena of nutation and the secular motions of the pole-star, some of which we will now briefly mention. According to the reports of explorers in polar regions, remains are to be found there indicating that at one time there flourished in those ice-covered tracts a tropical flora and fauna. Not long ago remains of lions and mammoths were found during the course of excavations in London; and such tropical remains can be traced from zone to zone. Such a slow and gradual shifting of the poles would not only explain such geological phenomena, but, what is more to the purpose, the explanation would not require the assumption of laws not now in operation, as if they had since been 'repealed.'

Take, as another case, the enormous rocks which there is apparently good evidence to show have been brought from the far north and deposited in Germany and elsewhere. Here again the current explanation is not a deduction from any general laws seen in operation to-day, but is a mere assumption of a hypothesis which *might* account for a most remarkable phenomenon. 'There was an ice period at one time when the Sun gave birth to Venus,' runs the explanation; 'the said rock being on the top of an iceberg, and the latter, floating southwards and melting, deposited the rock where it now reposes.' There is nothing impossible in this explanation, though there does seem to be a superfluous assumption of causes and forces for which we can find no evidence at present. But, apart from the above explanation, the forces which might have carried these gigantic boulders southwards can be seen in operation to-day. On the Alps, rocks are tumbling down from high mountain peaks, and are deposited on the ice covering the terraces and crevices below. These ice lakes and rivers are slowly creeping downwards, carrying these rocks with them, until the valleys are reached, where the ice melts and these rocks are deposited. In like manner we may conceive rocks to have been deposited on the icefields of the one time polar regions, perhaps through volcanic agencies, and these rocks to have taken many thousand years to travel down to the places where they are now found.

Another evidence lending support to this theory is the fact that the snow-line of glaciers is gradually receding. If this be due to the same cause, then there should be a gradual and corresponding advance of the snow-line in other parts of our globe; and, if this should be the case, it would be a strong confirmation of the theory, which we advance with great reserve, however, and merely by way of suggesting inquiry.

Another point deserving of mention in connexion herewith is a statement we have read that the observatories of Pulkowa and Berlin are gradually receding southwards: an observation confirmed by an expedition purposely sent to the Southern Hemisphere[1].

[1] Prince Krapotkine, in the *Nineteenth Century*, May, 1892, says: 'It has been remarked for some time since that Pulkowa and Berlin change from year to year their geographical positions. Their latitudes decrease; every year the two observatories seem to move away from the north pole a few inches.'

If the different places where remains have been discovered, indicating the presence of a tropical climate, were ascertained, it is possible that a clue might be found as to the direction in which the earth's axis is shifting. That it does not remain constant seems to our own minds pretty conclusive from the opposite consideration, even if for no other reasons—viz., that it would be the only thing constant we should know of in the universe. It is the slow changes which escape detection; and yet it is these slowly acting forces which produce the greatest changes.

CHAPTER XIV

CONCLUDING REMARKS

But because the parts of space cannot be seen, or distinguished from one another by our senses, therefore in their stead we use sensible measures of them. For from the positions and distances of things from any body considered as immovable, we define all places; and then with respect of such places, we estimate all motions, considering bodies as transferred from some of those places into others. And so instead of absolute places and motions we use relative ones; and that without any inconvenience in common affairs: but in philosophical disquisitions, we ought to abstract from our senses, and consider things themselves, distinct from what are only sensible measures of them. For it may be that there is no body really at rest, to which the places and motions of others may be referred.—Newton.

We have reached the limit of the task which we have set ourselves; but the work itself is not finished. We have gone as far as we were able, but have now reached a region into which we dare not venture to enter. Our object was simply to explain some physical laws, with special reference to the causes of gravitation. This we have done as best we could. But the application of these laws to the explanation of the phenomena of the heavens, though it should be undertaken before any theory is finally accepted as the true one, must be left to more competent hands. So far as we have dealt with astronomical phenomena, it was in a general sense only. Our sole object was to show, not only that the theory of gravitation here advanced is perfectly compatible with the more obvious of these phenomena, but that it is, as we venture to think, more in accordance with known laws and philosophic thought than are the hitherto current explanations of them.

But to deduce from physical laws the actual motions of celestial orbs is not so easy or simple a matter. The laws themselves can, indeed, be stated with the utmost precision, but not so the resultant phenomena of a number of bodies, each impelled by the same cause, but in ever so many different ways and directions. Had we to deal with two bodies only, uninterfered with by any external agencies, then the explanation would be simple. Even with three bodies, such as earth, moon, and sun, it might be possible to foretell what results would follow from the operations of certain determinate causes. But with a complex group, such as our solar system, it is not so easy a matter to deduce from a theory the precise motions of the different planets. We do not say that it is not possible, but it would not only involve far more elaborate mathematical calculations than are within our power, but would also require in many instances redeterminations of many of the data at present accepted as facts. The necessity for the redetermination of 'mass' and 'density,' for instance, if our theory be true, is obvious. The same applies also to many other data.

In the opening chapters of this volume we have given many illustrations showing the extent to which accepted theories may influence observation and interpretation. Now there can be no question that the accepted fundamental theories of gravitation must have had a great influence on the mind of every observer, and more especially on the interpretation which an observer would put on the phenomena connected therewith.

In opposition to this statement it may be urged that the calculations of astronomers, and the foretelling of coming events, are of so precise a nature as not to admit of any doubt as to the correctness of the methods employed. Our reply to such an objection is that these calculations are based on *empirical* data, and not on deductions from any theory. As already mentioned, astronomical events have been foretold by Thales, and others since his time (probably also before his time), when the theories concerning astronomical phenomena were altogether different from those now held to be true. The fact is that practical astronomy is one thing, and theory another.

Now, in order to verify our theory by the actual phenomena of the heavens, it would be necessary to know the *actual*—and not merely the relative—movements of the sun and planets. But this is precisely what we do not know. We only know for certain the phenomena we can observe from this roving planet on which we live, but do not with certainty know the exact concatenation of causes to which these phenomena are due. The accepted theory of our planetary system has nothing else to support it save that it explains the phenomena. Before it was formulated, there were several other theories in vogue which were based on the same kind of evidence, and which have only been rejected on the discovery of new facts, to explain which such theories were insufficient. The geocentric conception had to give way to the heliocentric conception of our system, *because the latter explains more harmoniously all the known phenomena.*

We have tried to deduce the motions of the sun and planets from our theory, and have succeeded in arriving, not at one, but at different alternative systems, each of which would not only agree with the theory here propounded, but would equally well explain all observed celestial phenomena. It is for this reason that we would not venture to theorize in matters which require knowledge from personal observation before any criticism is possible. We shall, therefore, confine our remarks on this head to some general considerations of the difficulties presented, and of the thoughts we have entertained on these points, trusting that the subject will be taken up by those to whom the necessary means of observation are accessible.

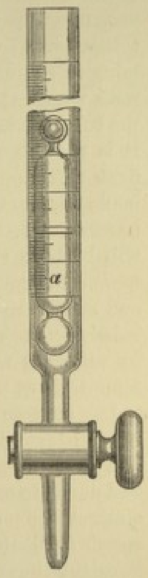

FIG. 64.

First, we will say a few words as to the nature of the phenomena and the difficulties of interpreting them correctly. Both these will be better appreciated, at least by those who are not familiar with celestial phenomena, by a simple illustration which is well within the reach of everybody. In Fig. 64 is shown an instrument in common use among chemists, called a burette. It is graduated, and the divisions are marked on it. Inside this tube is a small float, *a*, having a mark on it. If this burette be filled with water, the float rises to the surface; and, if the little tap at the lower end be opened and the water allowed to run out, the float descends as the level of the liquid in the burette is lowered. Now this simple instrument is eminently fitted to illustrate the difficulties in the way of correctly interpreting celestial phenomena; and under certain conditions, presently to be described, it would tax the skill of the greatest mathematician to determine the direction or rate of motion of this float within the tube.

For instance: If the burette be empty and water be poured into it very freely, the burette will be filled faster than the rate at which the float can ascend. The latter will then be seen rising slowly upwards in the column of water, until it has reached the level of the liquid. But if, while thus making its way upwards in a column of liquid, the tap be turned on, this column itself will sink, and with it, of course, the float swimming in it. If the rate of outflow be greater than the speed with which the float rises, the latter will seem to descend in the tube, yet relatively to the column of water it is rising. Now it is easy to regulate the outflow of the liquid so that the float will appear to remain stationary; that is, the mark made on it will always remain opposite a mark on the burette, and, therefore, will *seem* to be stationary. Yet it will be both rising and descending at the same time: rising *within* the column of water, and descending *with* the column and relatively to the burette, the two motions exactly counterbalancing each other. This state of things might be prolonged by allowing water to flow into the burette at the top at the same time as it is escaping at the lower end.

Let us assume now that the burette itself is also moving up and down, independently of the column of water within it, and that the observer himself is moving at the same time up and down, to and fro, and round in a circle, each of these three distinct movements taking place at varying and variable speeds: and the reader cannot fail to recognize the difficulties in calculating the actual proper motions of the float under such circumstances, when everything around, including the observer, is in constant motion, and there is absolutely no fixed point to take bearings from. The observer could only notice a change in the *relative* positions of float, burette, and himself; and he could attribute this change to an almost indefinite number of causes.

Now this is precisely the case with celestial observations, only that the latter are infinitely more complicated. All the heavenly bodies are moving to and fro, in many directions at the same time, with the most regular irregularities; hence, though we may be able to tell at what time we shall be opposite a certain point, we have as yet no means of telling to what concatenation of causes the periodic conjunctions are due. We look at the sun and at a certain star, note the change in relative positions, and then look for the causes of this change. At one time it was held that the earth remained fixed in space, and that the celestial phenomena were due to the movements of sun and stars. Copernicus reasoned that the sun was fixed and that the earth was moving. This view is held essentially at the present day, though it is now known that the sun itself is also moving. But, apart from the considerations and arguments advanced in these pages, it is fairly certain that the sun and all the stars are moving in space and on their axes, and nothing would be more unphilosophic than to assume that any one particular body should form an exception to a universal rule. If the sun is revolving on its axis—a proposition which we think nobody would now question—then it is equally certain that the sun is moving in space. But we are less certain as to what would constitute the 'sun' in this sense. Possibly this term may include the whole solar system; possibly the sun may have a multiplicity of such motions relatively to the several planets; or, again, the motions of the several planets may have a neutralizing effect on each other as regards their influence on the sun, in which case the motions of the latter would be due to the resultant or residual influences. The solar system can be explained on any of these suppositions.

Say, for instance, that the planetary motions are exactly what they are represented to be; this would not at all disagree with the theories here propounded. On the other hand, an alternative explanation—in fact there are several—is possible—perhaps we might even go so far as to say probable—which would equally well agree with all observed phenomena. But these are matters which we do not feel ourselves competent to deal with. Yet, by way of showing how the same phenomena are capable of alternate and equally satisfactory explanations, we will instance the periodic revolutions of our globe.

It is universally accepted that our earth makes 365[1] revolutions round its axis during a periodic revolution round

[1] We leave out fractions on purpose, since our object is to illustrate a principle rather than to explain actual phenomena. We shall commit the same inaccuracies throughout this argument, so as not unnecessarily to complicate the same by going into minute details.

the sun; that is, if the sun be taken as the point of reference. But, if we obtain our data from the stars, then the earth makes in the same period 366 axial revolutions. The explanation of this difference is, of course, a very simple one; and we shall take the trouble of reciting it merely because we shall presently make some deductions therefrom.

Let there be taken a straight line, and a disk of such relative dimensions that when the disk is rolled along this line from end to end it shall have made exactly 365 complete revolutions.

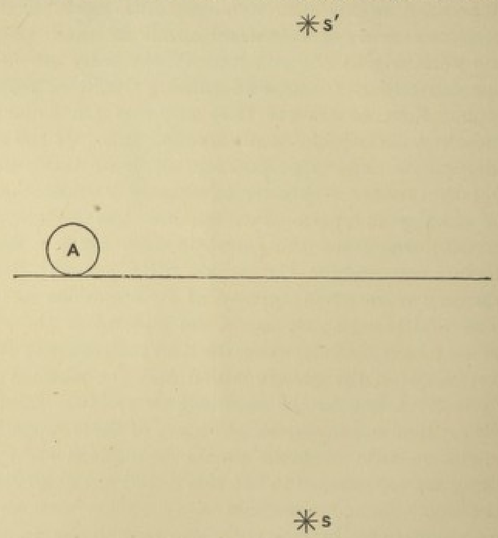

FIG. 65.

Suppose also two points, *S* and *S′*, at any distance on either side of the line. Then both points would be visible from a certain point of the disk an equal number of times, i. e. as many times as the disk made a complete revolution.

Let now this line be made into a circle round *S*, so that the latter shall be in the centre, and the ends of the line just meeting, as shown in Fig. 66.

The length of the now curved line would remain the same as before, the disk would still describe 365 complete revolutions in rolling along it, and the enclosed point *S* would consequently be seen 365 times. The only difference would be that at each complete revolution of *A* relatively to the curved line (that is, taking a revolution as complete when the same point in *A* touches the line on which it is rolling) the point *S* would be seen at the zenith from *A*; whereas on the former supposition, the disk being taken as rolling along a straight line, *S* would be seen in different positions at the end of each complete revolution. But, whilst *S* would be seen the same number of times as before, *S′* would now be seen 366 times, and that because, being *outside* the circle, the axis of *A*, in moving in the circular orbit, describes an additional complete revolution relatively to *S′*.

Suppose now that *A* (Fig. 66), in moving round *S*, were to describe twelve smaller circles [1], following a path like that indicated in Fig. 67. Then, in describing the twelve smaller circles, the axis of *A* would perform twelve complete revolutions relatively to *S*; and, if *S* were seen 365 times, we would have to allot twelve of the 365 axial revolutions of *A* relatively to *S* to the twelve smaller circles described, and allow for one additional revolution relatively to *S′*.

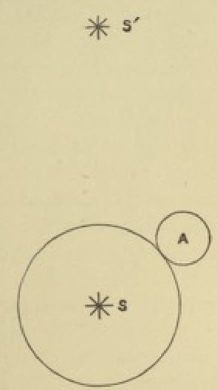

Fig. 66.

We will suppose now that *A*, in describing these twelve small circles, is travelling just as many times round a certain point. To make our supposition more comprehensible, it is only necessary to imagine that the earth is travelling round the moon, instead of the latter travelling round the former. The phenomena, of course, would be the same. Indeed, many writers seem to favour the view that earth and moon are revolving round each other, with the centre of rotation somewhere between the two companion planets. Whether this be actually the case or not will not affect our argument, which is simply to serve the purpose of showing how identical phenomena admit of different explanations.

[1] Suppose a disk, *X*, were to revolve round a central disk *S*, and *A* at the same time to revolve round *X*, and with *X* round *S*; then *A* would describe a path like that shown in Fig. 67.

Assuming, then, our earth to travel twelve times round the moon during each solar period, and during each circumlunar revolution to make twenty-eight additional (diurnal) revolutions round its axis, then, from the above explanation, it is obvious that the earth would make (apparently) twenty-eight revolutions relatively to the moon in each circumlunar period, and twenty-nine relatively to sun and stars and all other objects outside that circle. In that case twelve of the earth's diurnal revolutions would be periodic (additional)

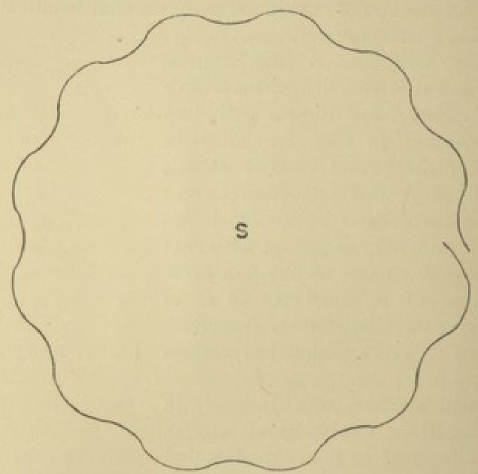

FIG. 67.

revolutions round the moon, and one a periodic (additional) revolution round the sun: which would give 336 lunar days, 365 solar days, and 366 sidereal days; that is, on the assumption that the earth is thus revolving round the moon. But, if we assume earth and moon to revolve together during each lunar period round a common centre, these relations would again have to be slightly modified.

The point of interest in this argument is, not as to which of these suppositions is true, but that either of these conditions would produce exactly the same phenomena as are observed;

which shows, firstly, that a theory deduced from phenomena is not necessarily true because such theory well agrees with observations; and, secondly, that such theory should not debar a rival theory from being carefully examined because the new explanation might lead to conclusions concerning the causes of such phenomena different from those already assigned to them.

We repeat it again, that the above illustration is not given as if we regarded it as being based on fact, or even as a probability. The truth is that we are very reluctant to utter any pronounced views concerning any astronomical observations; since we should have to rely for our data on second-hand information. But, on perceiving how physical phenomena, based on actual laboratory experiments, are capable of being wrongly interpreted (*vide* Joule's mechanical equivalent of heat; Sir William Thomson's theory of dissipation of energy; the double-fluid theory of electricity; and the doctrine of 'energy' itself: all of which are supposed to be based on actual facts[1]), we hesitate before we are willing to accept current explanations of observed astronomical phenomena (though never doubting the observations themselves), and still less do we feel competent to offer alternate explanations, not knowing how many of the published data are due to true observation, and how many are merely disguised interpretation. We have demonstrated certain principles; and from these we have deduced the chief motions which two bodies in space should perform relatively to each other. But, there being more than two such bodies, it is impossible to work out at the desk the actual motions of a complicated system of spheres, involving so many actions and counteractions.

[1] But which we have shown to be unconscious deductions from previously accepted theories.

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