CORRELATION

OF

PHYSICAL FORCES

GROVE

GROVE, Sir William Robert

CORRELATION

OF

PHYSICAL FORCES.

WILSON AND OGILVY, 57, SKINNER STREET, SNOWHILL, LONDON

THE

CORRELATION

OF

PHYSICAL FORCES.

BY

W. R. GROVE, M.A. F.R.S.

BARRISTER-AT-LAW.

SECOND EDITION.

LONDON:

SAMUEL HIGHLEY, 32, FLEET STREET.

1850.

PREFACE.

The views contained in the following Essay were first advanced in a lecture delivered at the London Institution in January 1842, and subsequently more fully developed in a course of lectures on the subject in 1843, reported at the time in the Literary Gazette. At the request of the proprietors of the Institution, I prepared an abstract of those lectures, which was printed by the managers for distribution to the proprietors. Having been asked by many where copies of the work were to be procured, I published a separate edition, which is now out of print.

An objection was made to the former edition, which seemed to me in some respect founded; viz., that as the work purported to be a record of lectures, and was given in a form different to that which it would have assumed if communicated to a scientific society, I should have gone further, and made it more suitable to the general reader, by a more didactic account of the different subjects on which it treated—the greater number of them being of recent origin. To have fully carried this out would have converted it into an elementary treatise, which would have been foreign to its object, and distracted the reader's attention from the main argument. I have, however, introduced in this edition a short sketch of each subject, more particularly those of recent date,—such as definite electrolysis, photography, &c.—which will, I hope, be sufficient to enable those readers who have not directed their attention to such matters to follow the argument, though I have, from the order in which the subjects are treated, been obliged, in some particulars, to anticipate these descriptive statements.

I have also enlarged the notes, by giving references to the original memoirs in which the branches of science alluded to are to be found, as well as to those which bear on the main arguments; where these memoirs are numerous, or not easy of access, I have referred to treatises in which they are collated. To prevent the reader's attention being interrupted, I have in the notes referred to the pages of the text, instead of to interpolated letters.

Being only able to devote to science short and irregular intervals from the duties of a laborious profession, and being, therefore, unable to give to this essay the developments which are necessary to do full justice to the subject, I should probably not now venture to bring it forward for the first time; but, the views having been published, the question reduced itself to whether the work should be allowed to expire, or whether its existence should be prolonged.

As my views have been favourably received and confirmed rather than altered, by such subsequent consideration as I have been able to give to them, by notices and translations of this work, and by conversations with many English and foreign men of science, whose opinions are entitled to weight,—I have decided on republishing them.

I am much indebted to Mr. Brayley, Librarian of the London Institution, for assistance in revising the proofs and preparing the notes.

4, *Hare Court, Temple,*
Oct. 1, 1850.

CORRELATION

OF

PHYSICAL FORCES.

WHEN natural phenomena are for the first time observed, a tendency immediately developes itself to refer them to something previously known,—to bring them within the range of acknowledged sequences. The mode of regarding new facts, which is most favourably received by the public, is that which refers them to recognised views,—stamps them into the mould in which the mind has been already shaped. The new fact may be far removed from those to which it is referred, and may belong to a different order of analogies, but this cannot then be known, as its co-ordinates are wanting. It may be questionable whether the mind is not so moulded by past events that it is impossible to advance an entirely new view, but, admitting such possibility, the new view, necessarily founded on insufficient data, is likely to be more incorrect and prejudicial than even a strained attempt to reconcile the new discovery with known facts.

The theory consequent upon new facts, whether it be a co-ordination of them with known ones, or the more difficult and dangerous attempt at remodelling the public ideas, is generally enunciated by the discoverers themselves of the facts, or by those to whose authority the world at that period defers; others are not bold enough, or if they be so, are unheeded. The earliest theories thus enunciated obtain the firmest hold upon the public mind, for at such a time there is no power of testing, by a sufficient range of experience, the truth of the theory; it is accepted solely or mainly upon authority: there being no means of contradiction, its reception is, in the first instance, attended with some degree of doubt, but as the time in which it can fairly be investigated far exceeds that of any lives then in being, and as neither the individual nor the public mind will long tolerate a state of abeyance, a theory shortly becomes, for want of a better, admitted as an established truth: it is handed from father to son, and gradually takes its place in education. Succeeding generations, whose minds are thus formed to an established view, are much less likely to abandon it. They have adopted it, in the first instance, upon authority, to them unquestionable, and subsequently to yield up their faith would involve a laborious remodelling of ideas, a task which the public as a body will and can rarely undertake, the frequent occurrence of which is indeed inconsistent with the very existence of man in a social state, as it would induce an anarchy of thought—a perpetuity of mental revolutions.

This necessity has its good ; but the evil with regard to the point we are considering is, that by this means, theories the most immature frequently become the most permanent ; for no theory can be more immature, none is likely to be so incorrect, as that which is formed at the first flush of a new discovery, and though time exalts the authority of those from whom it emanated, time can never give to the illustrious dead such means of analysing and correcting erroneous views as subsequent discoveries confer.

Take for instance the Ptolemaic System, which we may almost literally explain by the expression of Shakspeare : "He that is giddy thinks the world turns round." We now see the error of this system, because we have all an immediate opportunity of refuting it, but this identical error was received as a truth for centuries, because, when first promulgated, the means of refuting it were not at hand, and when the means of its refutation became attainable, mankind had been so educated to the supposed truth, that they rejected the proof of its fallacy.

I have premised the above for two reasons : first, to obtain a fair hearing by requesting as far as possible that dismissal from the minds of my readers of preconceived views by and in favour of which all are liable to be prejudiced ; and secondly, to defend myself from the charge of undervaluing authority, or treating lightly the opinions of those to whom and to whose memory mankind looks with reverence. Properly to value authority, we should estimate it together with its means of information: if a dwarf on the shoulders of a giant can see further than the giant, he is no less a dwarf in comparison with the giant.

The subject on which I am about to treat,—viz. the relation of the affections of matter to each other and to matter, peculiarly demands an unprejudiced regard. The different aspects under which these agencies have been regarded; the different views which have been taken of matter itself; the metaphysical subtleties to which these views unavoidably lead, if pursued beyond fair inductions from existing experience, present difficulties almost insurmountable.

The extent of claim which my views on this subject may have to originality, must be left to the judgment of the reader; they became strongly impressed upon my mind at a period when I was much engaged in experimental research, and were, as I then believed, and still believe regarding them as a whole, new: expressions in the works of different authors bearing more or less on the subject have subsequently been pointed out to me, some of which go back to a distant period. An attempt to analyse these, and to trace how far I have been anticipated by others, would probably but little interest the reader, and in the course of it I should constantly have to make distinctions showing wherein I differed, and wherein I agreed with others. I might quote authorities which appear to me to oppose, and others which appear to coincide with certain of the views I have put forth, but this would interrupt the consecutive development of my own ideas, and might render me liable to the charge of misconstruing those of others; I therefore think it better to give at the conclusion such references to different authors as bear upon the subject treated of, which I have discovered, or which have been pointed out to me since the delivery of the lectures of which this essay is a record.

The more extended our research becomes, the more we find that knowledge is a thing of slow progression, that the very notions which appear to ourselves new have arisen, though perhaps in a very indirect manner, from successive modifications of traditional opinions. Each word we utter, each thought we think, has in it the vestiges, is in itself the impress, of antecedent words and thoughts. As each material form, could we rightly read it, is a book containing in itself the past history of the world: so, different though our philosophy may now appear to be from that of our progenitors, it is but theirs added to or subtracted from, transmitted drop by drop through the filter of antecedent, as ours will be through that of subsequent, ages.—The relic is to the past as is the germ to the future.

Though many valuable facts, and correct deductions from them, are to be found scattered amongst the voluminous works of the ancient philosophers; yet, giving them the credit which they pre-eminently deserve for having devoted their lives to purely intellectual pursuits, and for having thought, seldom frivolously, often profoundly, nothing can be more difficult than to seize and apprehend the ideas of those who reasoned from abstraction to abstraction,—who, although, as we now believe, they must have depended upon observation for their first inductions, afterwards raised upon them such a complex superstructure of syllogistic deductions, that, without following the same paths, and tracing the same sinuosities which led them to their conclusions, such conclusions are to us unintelligible. To think as another thought, we must be placed in the same situation as he was placed: the errors of commentators generally arise from their reasoning upon the reasonings of their text, either in blind obedience to its dicta, without considering the circumstances under which they were uttered, or in viewing the images presented to the original writer from a different point to that from which he viewed them. Experimental philosophy, or even the theories fairly deducible from experiment, present such difficulty in a less degree; though the theories or explanations of a fact be different, the fact remains the same. It is itself the exponent of its discoverer's thought: known phenomena have led him to elicit from nature the new phenomenon; and, though he may be wrong in his reasoning upon this after its discovery, the reasonings which conducted him to it are themselves valuable, and, having led from known to unknown truths, can seldom be wholly erroneous.

Very different views existed amongst the ancients as to the objects to be pursued by physical investigation, and as to the objects likely to be attained by it. I do not here mean the moral objects,—such as the attainment of the *summum bonum*, &c.—but the acquisitions in knowledge which such investigations were likely to confer. Utility was one object in view, and this was to some extent attained by the progress made in astronomy and mechanics; Archimedes, for instance, seems to have constantly had this end in view; but, while pursuing natural knowledge for the sake of knowledge, and the power which it brings with it, the greater number seemed to entertain an expectation of arriving at some ultimate goal, some point of knowledge, which would give them a mastery over the mysteries of nature, and would enable them to know what was the most intimate structure of matter, and the causes of the changes it exhibits. Where they could not discover, they speculated. Leucippus, Democritus, and others, have given us their notions of the ultimate atoms of which matter was formed, and of the *modus agendi* of nature in the various transformations which matter undergoes.

The expectation of arriving at ultimate causes or essences continued long after the speculations of the ancients had been abandoned, and continues even to the present day to be a very general notion of the objects to be ultimately attained by physical science. Francis Bacon, the great remodeller of science, entertained this notion, and thought that, by experimentally testing natural phenomena, we should be enabled to trace them to certain primary essences or causes whence the various phenomena flow. These he speaks of under the scholastic name of "forms,"—a term derived from the ancient philosophy, but differently applied. He appears to have understood by "form" the essence of quality,—that in which, abstracting everything extraneous, a given quality consists, or that which, superinduced on any body, would give it its peculiar quality: thus the form of transparency is that which constitutes transparency, or that by which, when discovered, transparency could be produced or superinduced. To take a specific example of what I may term the synthetic application of his philosophy:—"In gold there meet together yellowness, gravity, malleability, fixedness in the fire, a determinate way of solution, which are the simple natures in gold; for he who understands form and the manner of superinducing this yellowness, gravity, ductility, fixedness, faculty of fusion, solution, &c., with their particular degrees and proportions, will consider how to join them together in some body, so that a transmutation into gold shall follow."

On the other hand, the analytic method, or "the inquiry from what origin gold or any other metal or stone is generated from its first fluid matter or rudiments, up to a perfect mineral," is to be perceived by what Bacon calls the latent process, or a search for "what in every generation or transformation of bodies, flies off, what remains behind, what is added, what separated, &c.; also, in other alterations and motions, what gives motion, what governs it, and the like." Bacon appears to have thought that qualities separate from the substances themselves were attainable, and if not capable of physical isolation, were at all events capable of physical transference and superinduction.

Subsequently to Bacon a belief has generally existed, and now to a great extent exists, in what are called secondary causes, or consequential steps, wherein one phenomenon is supposed necessarily to hang on another, and that on another, until at last we arrive at an essential cause, subject immediately to the First Cause. This notion is generally prevalent both on the Continent and in this country: nothing is more familiar than the expression "study the effects in order to arrive at the causes."

Instead of regarding the proper object of physical science as a search after essential causes, I believe it ought to be, and must be, a search after facts and relations,—that although the word Cause may be used in a secondary and concrete sense, as meaning antecedent forces, yet in an abstract sense it is totally inapplicable: we cannot predicate of any physical agency that it is abstractedly the cause of another; and if, for the sake of convenience, the language of secondary causation be permissible, it should be only with reference to the special phenomena referred to, as it can never be generalised. The misuse, or rather varied use, of the term Cause, has been a source of great confusion in physical theories, and philosophers are now by no means agreed as to their conception of causation. The most generally received view of causation refers it to invariable antecedence—*i. e.* we call that a cause which invariably precedes, that an effect which invariably succeeds. Speaking with reference to physical phenomena, it is difficult to separate the idea of causation from that of force, and these have been regarded as identical by some philosophers. To take an example which will contrast these two views: if a floodgate be raised, the water flows out; in ordinary parlance, the water is said to flow *because* the floodgate is raised: the sequence is invariable; no floodgate, properly so called, can be raised without the water flowing out, and yet in another, and perhaps more strict sense, it is the gravitation of the water which *causes* it to flow. But though we may truly say that, in this instance, gravitation causes the water to flow, we cannot in truth abstract the proposition, and say, generally, that gravitation is the cause of water flowing, as water may flow from other causes,—atmospheric pressure, for instance, which causes water to flow into an exhausted receiver,—and gravitation may, under certain circumstances, arrest, instead of causing, the flow of water.

Upon neither view, then, can we get at anything like abstract causation. If we regard causation as invariable sequence, we can find no case in which a given antecedent is the only antecedent to a given sequent: thus, if water could flow from no other cause than the withdrawal of a floodgate, we might say abstractedly that this was the cause of water flowing. If, again, adopting the view which looks to causation as a force, we could say that water could be caused to flow only by gravitation, we might say abstractedly that gravitation was the cause of water flowing,—but this we cannot say; and if we seek and examine any other example, we shall find that causation is only predicable of it in the particular case, and cannot be supported as an abstract proposition; yet this is constantly attempted. Nevertheless, in all the particular cases where we speak of Cause, we certainly refer to some antecedent power or force: we never see motion or any change in matter take effect without regarding it as produced by some previous change; and, when we cannot trace it to its antecedent, we mentally refer it to one. The common error, if I am right in supposing it to be such, consists in the abstraction of cause, and in supposing in each case a general secondary cause,—a something which is not the first cause, but which, if we examine it carefully, must have all the attributes of a first cause, and an existence independent of, and dominant over, matter.

A doubt has recently been thrown upon the prior existence of cause to effect, and their simultaneity argued with much ability. On this view time would cease to be a necessary element in causation, and the idea of cause, except perhaps as referred to a primeval creation, would cease to exist. We could not, however, even if we adopted this view, dispense with the element of time in the sequence of phenomena; the effect being thus regarded as ever accompanied simultaneously by its appropriate cause, we should still refer it to some antecedent effect; and our reasoning, as applied to the successive production of all natural changes, would be the same. If we struck out of our vocabulary the word "cause," our language, in speaking of successive changes, would be unintelligible to the present generation; and the same arguments which apply to the simultaneity of cause with effect would apply to the simultaneity of force with motion. The word "force," and the idea it aims at expressing, might, indeed, be objected to by the purely physical philosopher as representing a subtle mental conception, and not a sensuous perception or phenomenon. To avoid its use, however, if open to no other objection, would be so far a departure from recognised views as to render language scarcely intelligible.

Electricity and magnetism afford us a very instructive example of the belief in secondary causation. Subsequent to the discovery by Oersted of electro-magnetism, and prior to that by Faraday of magneto-electricity, electricity and magnetism were believed by the highest authorities to stand in the relation of cause and effect—*i. e.* electricity was regarded as the cause, and magnetism as the effect; and where magnets existed without any apparent electrical currents to cause their magnetism, hypothetical currents were supposed, for the purpose of carrying out the causative view; but magnetism may now be said with equal truth to be the cause of electricity, and electrical currents may be referred to hypothetical magnetic lines: again, if electricity cause magnetism, and magnetism cause electricity, why then electricity causes electricity, which becomes, so to speak, a *reductio ad absurdum* of the doctrine.

To take another instance, which may render these positions more intelligible. By heating bars of bismuth and antimony in contact, a current of electricity is produced, and if their extremities be united by a fine wire, the wire is heated. Now here the electricity in the metals is said to be caused by heat, and the heat in the wire to be caused by electricity, and in a concrete sense this is true; but can we thence say abstractedly that heat is the cause of electricity, or that electricity is the cause of heat? Certainly not,—for if either be true, both must be so, and the effect then becomes the cause of the cause, or, in other words, a thing causes itself. Any other proposition on this subject will be found to involve similar difficulties, until, at length, the mind will become convinced that abstract secondary causation does not exist, and that a search after essential causes is vain.

The position which I seek to establish in this Essay is, that the various affections of matter which constitute the main objects of experimental physics, viz., heat, light, electricity, magnetism, chemical affinity, and motion, are all correlative, or have a reciprocal dependence. That neither, taken abstractedly, can be said to be the essential or the proximate cause of the others, but that either may, as a force, produce the others: thus heat may mediately or immediately produce electricity, electricity may produce heat; and so of the rest, each merging itself as the force it produces becomes developed: and that the same must hold good of other forces, it being an irresistible inference that a force cannot originate otherwise than by generation from some antecedent force or forces.

The term force, although used in very different senses by different authors, in its limited sense may be defined as that which produces or resists motion. Although strongly inclined to believe that the other affections of matter, which I have above named, are, and will ultimately be resolved into, modes of motion, it would be going too far, at present, to assume their identity with it; I therefore use the term force, in reference to them, as meaning that active principle inseparable from matter which induces its various changes.

Let us begin with Motion,—the most obvious, the most distinctly conceived of all the affections of matter. Visible motion, or relative change of position in space, is a phenomenon so obvious to simple apprehension, that to attempt to define it would be to render it more obscure; but with motion, as with all physical appearances, there are certain vanishing shadows or undefined limits at which the obvious mode of action gradually disappears; to detect the continuing existence of the phenomena we are obliged to have recourse to other methods of investigation, and we frequently apply other names to the effects so recognized.

Thus sound is motion; and although in the earlier periods of philosophy the identity of sound and motion was not traced out, and they were considered distinct affections of matter,—indeed, at the commencement of the present century a theory was advanced that sound was transmitted by the vibrations of an ether,—we now so readily resolve sound into motion, that to those who are familiar with acoustics the phenomena of sound immediately present to the mind the idea of motion, *i. e.*, motion of ordinary matter.

Again, with regard to light: no doubt now exists that light moves, or is accompanied by motion. Here the phenomena of motion are not made evident by the ordinary sensuous perception, as for instance the motion of a visibly moving projectile would be, but by an inverse deduction from known relations of motion to time and space: as all observation teaches us that bodies in moving from one point in space to another occupy time, we conclude that, wherever a continuing phenomenon is rendered evident in two different points of space at different times, there is motion, though we cannot see the progression. A similar deduction convinces us of the motion of electricity.

As we in common parlance speak of sound moving, although sound is motion, it requires no great stretch of imagination to conceive light and electricity as motions, and not as things moving. If one end of a long bar of metal be struck, a sound is soon perceptible at the other end. This we now know to be a vibration of the bar; sound is but a word expressive of the mode of motion impressed on the bar; so one end of a column of air or glass subjected to a luminous impulse gives a perceptible effect of light at the other end: this can equally be conceived to be a vibration, or transmitted motion of the transparent column: this question will, however, be further discussed hereafter;

for the present we will confine ourselves to motion within the limits to which the term is usually restricted.

With the perceptible phenomena of motion a mental conception has been invariably associated to which I have before alluded and to which the term force is given,—the which conception, when we analyse it, refers us to some antecedent motion. If we except the production of motion by heat, light, &c., which we shall consider in the sequel, when we see a body moving we look to motion having been communicated to it by matter which previously moved.

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: supposing, however, that motion is not an invariable function of matter, but that matter can be at rest, matter at rest would never of itself cease to be at rest; it would not move unless impelled to such motion by some other moving body, or body which has moved. This proposition applies not merely to impulsive motion, as when a ball at rest is struck by a moving spring, or pressed by a spring which has previously been moved, but to motion caused by attractions such as magnetism or gravitation. Suppose a piece of iron at rest in contact with a magnet at rest; if it be desired to move the iron by the attraction of the magnet, the magnet or the iron must first be moved; so before a body falls it must first be raised. A body at rest would therefore continue so for ever, and a body once in motion would continue so for ever, in the same direction and with the same velocity, unless impeded by some other body, or affected by some other force than that which originally impelled it; but it is very generally believed that if the visible or palpable motion be arrested by impact on another body, the motion ceases, and the force which produced it is annihilated.

Now the view which I venture to submit is, that force cannot be annihilated, but is merely subdivided or altered in direction or character. First, as to direction. Wave your hand: the motion, which has apparently ceased, is taken up by the air, from the air by the walls of the room, &c., and so by direct and reacting waves, continually comminuted, but never destroyed. It is true that, at a certain point, we lose all means of detecting the motion, from its minute subdivision, which defies our most delicate means of appreciation, but we can indefinitely extend our power of detecting it accordingly as we confine its direction, or increase the delicacy of our examination. Thus, if the hand be moved in unconfined air, the motion of the air would not be sensible to a person at a few feet distance; but if a piston of the same extent of surface as the hand be moved with the same rapidity in a tube, the blast of air may be distinctly felt at several yards distance. There is no greater absolute amount of motion in the air in the second than in the first case, but its direction is restrained, so as to make its means of detection more facile. By carrying on this restraint, as in the air-gun, we get a power of detecting the motion, and of moving other bodies at far greater distances. The puff of air which would in the air-gun project a bullet a quarter of a mile, if allowed to escape without its direction being restrained, as by the bursting of a bladder, would not be perceptible at a yard distance, though the same absolute amount of motion be impressed on the surrounding air.

It may, however, be asked, what becomes of force when motion is arrested or impeded by the counter-motion of another body? This is generally believed to produce rest, or entire destruction of motion, and consequent annihilation of force: so indeed it may, as regards the motion of the masses, but a new force, or new character of force, now ensues, the exponent of which, instead of visible motion, is heat. I venture to regard the heat which results from friction or percussion as a continuation of the force which was previously associated with the moving body, and which, when this impinges on another body, ceasing to exist as gross, palpable motion, continues to exist as heat.

Thus, let two bodies, A and B, be supposed moving in opposite directions, (putting for the moment out of the question all resistance, such as that of the air, &c.), if they pass each other without contact each will move on for ever in its respective direction with the same velocity, but if they touch each other the velocity of the movement of each is reduced, and each becomes heated: if this contact be slight, or such as to occasion but a slight diminution of their velocity, as when the surfaces of the bodies are oiled, then the heat is slight; but if the contact be such as to occasion a great diminution of motion, as in percusison, or as when the surfaces are roughened, then the heat is great, so that in all cases the resulting heat is proportionate to the diminished velocity. Where, instead of resisting and consequently impeding the motion of the body A, the body B gives way, or itself takes up the motion originally communicated to A, then we have less heat in proportion to the motion of the body B, for here the operation of the force continues in the form of palpable motion: thus the heat resulting from friction in the axle of a wheel is lessened by surrounding it by rollers; these take up the primary motion of the axle, and the less, by this means, the initial motion is impeded, the less is the resulting heat. Again, if a body move in a fluid, the heat produced is very trifling, because the particles of the fluid themselves move, and continue the motion originally communicated to the moving body: for every portion of motion communicated to them this loses an equivalent, and where both lose, then an equivalent of heat results.

As the converse of this proposition, it should follow that the more rigid the bodies impinging on each other the greater should be the amount of heat developed by friction, and so we find it. Flint, steel, hard stones, glass, and metals, are those bodies which give the greatest amount of heat from friction or percussion, while water, oil, &c., give little or no heat, and from the ready mobility of their particles lessen its development when interposed between rigid moving bodies. Thus, if we oil the axles of wheels, we have more rapid motion of the bodies themselves, but less heat; if we increase the resistance to motion, as by roughening the points of contact, so that each particle strikes against and impedes the motion of others, then we have diminished motion, but increased heat. I cannot present to my mind any case of heat resulting from friction which is not explicable by this view; friction, according to it, is simply impeded motion. The greater the impediment, the more force is required to overcome it, and the greater is the resulting heat; this resulting heat being a continuation of indestructible force, capable, as we shall presently see, of reproducing palpable motion, or motion of definite masses.

Hitherto I have taken no distinction as to the physical character of the bodies impinging on each other; but Nature gives us a remarkable difference in the character or mode of the force eliminated by friction, accordingly as the bodies which impinge are homogeneous or heterogeneous: if the former, heat alone is produced; if the latter, electricity.

We find, indeed, instances given by authors of electricity resulting from the friction of homogeneous bodies; but, as I stated in my original lectures, I have not found such facts confirmed by my own experiments; and this conclusion has been corroborated by some experiments of Professor Erman, communicated to the meeting of the British Association in the year 1845, in which he found that no electricity resulted from the friction of perfectly homogeneous substances; as, for instance, the ends of a broken bar. Such experiments as these will, indeed, be seldom free from slight electrical currents, on account of the practical difficulty of fulfilling the condition of perfect homogeneity in the substances themselves, their size, their temperature, &c., but the effects produced are very trifling and vary in direction, and the resultant effect is nought. Indeed, it would be difficult to conceive the contrary. How could we possibly image to the mind or describe the direction of a current from the same body to the same body, or give instructions for a repetition of the experiment? It would be unintelligible to say, that in rubbing two pieces of bismuth together, a current of electricity circulated from bismuth to bismuth, or from iron to iron, or from glass to glass, for the question immediately occurs,—from which bismuth to which does it circulate? And should this question be answered, by calling one piece A, the other B, this would only apply to the particular specimens employed, and without heterogeneity, or a distinction in qualities, the phenomenon is absolutely indescribable: we may say that it circulates from rough glass to smooth, from cast iron to wrought, for here there is not homogeneity. It is, indeed, conceivable that when the motion is continuous in a definite direction, electricity may result from the friction of homogeneous bodies. If A and B rub against each other, revolving in opposite directions, concentric currents of positive and negative electricity may be conceived circulating within the metals, and be described by reference to the direction of their motion, but this would be a distinct phenomenon from those we are considering, and is, experimentally, unknown.

We may say, then, that in our present state of knowledge, where the mutually impinging bodies are homogeneous, heat and not electricity is the result of friction and percussion; where the bodies impinging are heterogeneous, we may safely state that electricity is always produced by friction or percussion, although heat in a greater or less degree accompanies it; but when we come to the question of the ratio in which electricity is produced, as determined by the different characters of the substances employed, we find very complex results. Bodies may differ in so many particulars which influence more or less the development of electricity, such as their chemical constitution, the state of their surfaces, their state of aggregation, their transparency or opacity, their power of conducting electricity, &c., that the *normæ* of their action are very difficult of attainment. As a general rule, it may be said that the development of electricity is greater when the substances employed are broadly distinct in their physical and chemical qualities, and more particularly in their conducting powers, but up to the present time the laws governing such development have not been even approximatively determined.

I have said, in reference to the various forces or affections of matter, that either of them may, *mediately* or *immediately*, produce the others, and this is all I can venture to predicate of them in the present state of science: but I will venture as an opinion, formed after much consideration, that science is rapidly progressing towards the establishment of immediate or direct relations between all these forces. Where at present no immediate relation is established between any of them, electricity generally forms the intervening link or middle term.

Motion, then, will directly produce *heat* and *electricity*, and electricity, being produced by it, will produce *magnetism*,—a force which is always developed by electrical currents at right angles to the direction of those currents, as I shall subsequently more fully explain. *Light* also is readily produced by motion, either directly, as when accompanying the heat of friction, or mediately, by electricity resulting from motion; as in the electrical spark, which has all the attributes of common light, its sole difference being, as far as I am aware, the position of the fixed lines in its spectrum,—a difference which obtains with light emanating from different sources, or seen through different media. In the decompositions and compositions which the terminal points proceeding from the conductors of an electrical machine develope when immersed in different chemical media, we get the production of *chemical affinity* by electricity, of which motion is the initial source. Lastly, *motion* may be again reproduced by the forces which have emanated from motion; thus, the divergence of the electrometer, the revolution of the electrical wheel, the deflection of the magnetic needle, are, when resulting from frictional electricity, palpable movements reproduced by the intermediate modes of force, which have themselves been originated by motion.

If we now take HEAT as our starting point, we shall find that the other modes of force may be readily produced by it. To take motion first: this is so generally, I think I may say invariably, the immediate effect of heat, that we may almost resolve heat into motion, and view it as a mechanically repulsive force, a force antagonist to attraction of cohesion or aggregation, and tending to *move* the particles of all bodies, or to separate them from each other.

If we put aside the sensation which heat produces in our own bodies, and regard heat simply as to its effects upon inorganic matter, we find that, with a very few exceptions which I shall presently notice, the effects of what we call heat are simply an expansion of the matter acted upon, and that the matter so expanded has the power also of communicating expansion to all bodies in contiguity with it. Thus, if the body be a solid,—for instance, iron, a liquid, say water, or a gas, say atmospheric air,—each of these, when heated, is expanded in every direction: in the two former cases, by increasing the heat to a certain point, we change the physical character of the substance, the solid becomes a liquid, and the liquid becomes a gas; these, however, are still expansions, particularly the latter, when, at a certain period, the expansion becomes rapidly and indefinitely greater. But what is, in fact, done in order to heat a substance, or to increase the heat of a substance? it is merely approximated to some other heated, that is, to some other expanded substance. Let us now divest the mind of the impression that heat is in itself anything substantive, and suppose that these phenomena are regarded for the first time, and, without any preconceived notions on the subject; let us introduce no hypothesis, but merely express as simply as we can the facts of which we have become cognizant; to what do they amount? to this, that matter has associated with it a molecular repulsive power, a power of dilatation which is communicable by contiguity or proximity.

Heat, thus viewed, is motion, and this molecular motion we may readily change into the motion of masses, or motion in its most ordinary and palpable form: for example, in the steam engine the piston and all its concomitant masses of matter are moved by the molecular dilatation of the vapour of water. In this case the motion of the mass becomes the exponent of the amount of heat of the molecules; nor do we, by any of our ordinary methods, test heat in any other way than by its purely dynamical action. The various modifications of the thermometer and pyrometer are all measurers of heat by motion: in these instruments liquid or solid bodies are expanded and elongated, *i. e.*, moved in a definite direction, and, either by their own visible motion, or by the motion of an attached index, communicate to our senses the amount of the force by which they are moved. There are, indeed, some delicate experiments which tend to prove that a repulsive action between separate masses is produced by heat. Fresnel found that mobile bodies heated in an exhausted receiver repelled each other to sensible distances; and Baden Powell found that the coloured rings usually called Newton's rings change their breadth and position, when the glasses between which they appear are heated, in a manner which showed that the glasses repelled each other. There is, however, some difficulty in presenting these phenomena to the mind in the same aspect as the molecular repulsive action of heat.

The phenomena of what is termed latent heat have been generally considered as strongly in favour of that view which regards heat either as actual matter, or, at all events, as a substantive entity, and not a motion or affection of ordinary matter.

The hypothesis of latent matter is, I venture with diffidence to think, a dangerous one—it is something like the old principle of Phlogiston, it is not tangible, visible, audible; it is, in fact, a mere subtle mental conception, and ought, I submit, only to be received on the ground of absolute necessity, the more so as these subtleties are apt to be carried on to other natural phenomena, and so they add to the hypothetical scaffolding which is seldom requisite, and should be sparingly used even in the early stages of discovery. As an instance, I think a striking one, of the injurious effects of this, I will mention the analogous doctrine of "invisible light;" and I do this, meaning no disrespect to its distinguished author, any more than, in discussing the doctrine of latent heat, I can be supposed, in the slightest degree, to aim at detracting from the merits of the illustrious investigators of the facts which that doctrine seeks to explain. Is not "invisible light," in fact, a contradiction in terms? has not light ever been regarded as that agent which affects our visual organs? Invisible light, then, is darkness, and if it exist, then is darkness light. I know it may be said, that one eye can detect light where another cannot; that a cat may see where a man cannot; that an insect may see where a cat cannot; but then it is not invisible light to those who see it: the light, or rather the object seen by the cat, may be invisible to the man, but it is visible to the cat, and, therefore, cannot abstractedly be said to be invisible. If we go further, and find an agent which affects certain substances similarly to light, but does not, as far as we are aware, affect the visual organs of any animal, then is it an erroneous nomenclature which calls such an agent light,—a deviation from the plain accepted meaning of words which, when it takes place, is always injurious to that precision of language which is the safest guard to knowledge, and from the absence of which physical science has materially suffered.

Let us now shortly examine the question of latent heat, and see whether the phenomena cannot be as well, if not more satisfactorily, explained without the hypothesis of latent matter. Latent heat is supposed to be the matter of heat, associated, in a masked or dormant state, with ordinary matter, not capable of being detected by any test, so long as the matter with which it is associated remains in the same physical state, but communicated to or absorbed from other bodies, when the matter with which it is associated changes its state. To take a common example: a pound or given weight of water at 172°, mixed with an equal weight of water at 32°, will acquire a mean temperature, or 102°; while water at 172°, mixed with an equal weight of ice at 32°, will be reduced to 32°. By the theory of latent heat this phenomenon is thus explained:—In the first case, that of the mixture of water with water, both the bodies being in the same physical state no latent heat is rendered sensible, or sensible heat latent; but in the second, the ice changing its condition from the solid to the liquid state, abstracts from the liquid as much heat as it requires to retain it in the liquid state, which it renders latent, or retains associated with itself, as long as it remains liquid, but of which heat no evidence can be afforded by any thermoscopic test.

I believe this and similar phenomena, where heat is connected with a change of state, may be explained and distinctly comprehended without recourse to the conception of latent heat, though it requires some effort of the mind to divest itself of this idea, and to view the phenomena simply in their dynamical relations. To assist us in so viewing them, let us first parallel with purely mechanical actions certain simple effects of heat, where change of state (I mean such change as from the liquid to the solid, or gaseous to the liquid state) is not concerned. Thus, place within a receiver a bladder, and heat the air within to a higher temperature than that without it, the bladder expands; so force the air mechanically into it by the air-pump, the bladder expands; cool the air on the outside, or remove its pressure mechanically by an exhausting pump, the bladder also expands; conversely, increase the external repellent force, either by heat or mechanical pressure, and the bladder contracts. In the mechanical effects, the force which produced the distension is derived from, and at the expense of, the mechanical power employed, as from muscular force, from gravitation, from the reacting elasticity of springs, or any similar force by which the air-pump may be worked. In the heating effects the force is derived from the chemical action in the lamp or source of heat employed.

Let us next consider the experiment so arranged that the force which produces expansion in the one case, produces a correlative contraction in the other: thus, if two bladders with a connecting neck between them be half filled with air, as the one is made to contract by pressure the other will dilate and *vice versâ;* so a bladder partly filled with cold air, and contained within another filled with hot air, expands, while the external space contracts, exhibiting a mere transfer of the same amount of repulsive force, the mobility of the particles, or their mutual attraction of cohesion, being the same in each body; in other words, the repulsive force acts in the direction of least resistance until equilibrium is produced; it then becomes a static or balanced, instead of a dynamic or motive force.

Let us now consider the case where a solid is to be changed to a liquid, or, a liquid to a gas; here a much greater amount of heat or repulsive force is required, on account of the cohesion of the particles to be separated. In order to separate the particles of the solid, precisely as much force must be parted with by the warmer liquid body as keeps an equal quantity of it in its liquid state; it is, indeed, only with a more striking line of demarcation, the case of the hot and cold bladder,—a part of the repellent power of the hot particles is transferred to the cold particles, and separates them in their turn, but the antagonist force of cohesion or aggregation necessary to be overcome, being in this case much stronger, requires and exhausts an exactly proportionate amount of repellent force mechanically to overcome it; hence the different effect on a body such as the common thermometer, the expanding liquid of which does not undergo a similar change of state. Thus, in the example above given, of the mixture of cold with hot water, the hot and cold water and the mercury of the thermometer being all in a liquid state before, and remaining so after contact, the resulting temperature is an exact mean; the hot water contracts to a certain extent, the cold water expands to the same extent, and the thermometer either sinks or rises the same number of degrees, accordingly as it had been previously immersed in the cold or in the hot solution, its mercury gaining or losing an equivalent of repellent force. In the second instance, viz. the mixture of ice with hot water, the substance we use as an indicator, *i. e.*, mercury, does not undergo the same physical change as those whose dynamical relations we are examining. The force,—viewing heat simply as mechanical force,—which is employed in loosening or tearing asunder the particles of the solid ice, is abstracted from the liquid water, and from the liquid mercury of the thermometer, and in proportion as this force meets with a greater resistance in separating the particles of a solid than of a liquid, so the bodies which yield the force suffer proportionately a greater contraction.

If we compare the action of heat on the two substances—water and mercury—alone, and throw out of our consideration the ice, we shall be able to apply the same view: thus, if a given source of heat be applied to water containing a mercurial thermometer, both the water and mercury gradually expand, but in different degrees; at a certain point the attractive force of the molecules of the water is so far overcome that the water becomes vapour. At this point, the heat or force, meeting with much less resistance from the attraction of the particles of steam than from those of the mercury, expends itself upon the former; the mercury does not further expand, or expands in an infinitesimally small degree, and the steam expands greatly. As soon as this arrives at a point where circumambient pressure causes its resistance to further expansion to be equal to the resistance to expansion in the mercury of the thermometer, the latter again rises, and so both go on expanding in an inverse ratio to their molecular attractive force. If the circumambient pressure be increased, as by confining the water at the commencement of the experiment within a less expansible body than itself, such as a metallic chamber, then the mercury of the thermometer continues to rise; and if the experiment were continued, the water being confined and not the mercury, until we have arrived at a degree of repulsive force which is able to overcome the cohesive power of the mercury, so that this expands into vapour, then we get the converse effect; the force expends itself upon the mercury, which expands indefinitely, as the water did in the first case, and the water does not expand at all.

Another very usual mode of regarding the subject may embarrass at first sight, but a little consideration will show that it is explicable by the same doctrine. Water which has ice floating in it will give, when measured by the thermometer, the same temperature as the ice; *i. e.*, both the water and ice contract the mercury of the themometer to the point conventionally marked as 32°. It may be said, how is this reconcileable with the dynamical doctrine, for, according to that, the solid should take from the mercury of the thermometer more repulsive power than the liquid; consequently, the ice should contract the mercury more than the water?

My answer is, that in the proposition as thus stated, the quantities of the water, ice, and mercury, are not taken into consideration, and hence a necessary dynamical element is neglected: if the element of quantity be included, this objection will not apply.

Let the thermometer, for instance, contain 13½ oz. of mercury, and stand at 100°; if placed in contact with an unlimited quantity of ice at 32°, the mercury will sink to 32°. If the same thermometer be immersed in an unlimited quantity of water at 32°, the mercury sinks also to 32°; not absolutely, perhaps, because, however great the quantity of water or ice, it will be somewhat raised in temperature by the warmer mercury. This elevation of temperature above 32° will be smaller in proportion as the quantity of water or ice is larger than the quantity of mercury; and, as we know of no intermediate state between ice and water, the contact of a thermometer at a temperature above the freezing point with any quantity of ice exactly at the freezing point would, theoretically speaking, liquefy the whole, provided it had sufficient time; for as every portion of that ice would in time have its temperature raised by the contact of the warmer body, and as any elevation of temperature above the freezing point liquefies ice, every portion should be liquefied. Practically speaking, however, in both cases, that of the water and of the ice, when the quantity is indefinitely great, the thermometer falls to 32°.

Now place the same thermometer at 100°, successively in one oz. of water at 32°, and in one of ice at 32°; we shall find in the former case it will be lowered only to 54°, in the latter to 32°: apply to this the doctrine of repulsive force, and we get a satisfactory explanation.

In the first case, the quantities both of ice and water being indefinitely great in respect to the mercury, each reduces it to its own temperature, viz., 32°, and the ice cannot reduce the mercury below 32°, because it would receive back repulsive power from the newly formed water, and this would become ice; in the second case, where the quantities are limited, the mercury does lose more repulsive power by the ice than by the water, and the observations made in reference to the first illustration apply.

The above doctrine is beautifully instanced in the experiment of Thilorier, by which carbonic acid is solidified. Carbonic acid gas, retained in a strong vessel under great pressure, is allowed to escape from a small orifice; the sudden expansion requires so great a supply of force that in furnishing the demands of the expanding gas certain other portions of the gas contract to such an extent as to solidify: thus, we have reciprocal expansion and contraction going on in one and the same substance, the time being too limited for the whole to assume an uniform temperature, or in other words, an uniform extent of expansion.

In commencing the subject of heat, I asked my reader to put out of consideration the sensations which heat produces in our own bodies; I did this because these sensations are likely to deceive and have deceived many as to the nature of heat. These sensations are themselves occasioned by similar expansions to those which we have been considering; the liquids of the body are expanded, *i. e.*, rendered less viscid by heat, and, from their more ready flow, we obtain the sensation of agreeable warmth. By a greater degree of heat, their expansion becomes too great, and gives rise to a sense of pain, and, if pushed to extremity, as with the heat which produces a burn, the liquids of the body are dissipated in vapour, and an injury or destruction of the organic structure takes place. A similar though converse effect may be produced by intense cold; the application of frozen mercury to the animal body produces a burn similar to that produced by great heat, and accompanied with a similar sensation. These effects, therefore, in no respect alter the phenomenal effects; heat is still expansion, cold is contraction.

In entering upon this subject, I also said that there were a few exceptions as to heat being always manifested by an expansion of matter. One class of these exceptions is only apparent: moist clay, animal or vegetable fibre, and other substances of a mixed nature, which contain matter of different characters, some of which is more and some less volatile, *i. e.*, expansible, are contracted on the application of heat; this arises from the more volatile matter being dissipated in the form of vapour or gas; and the interstices of the less volatile being thus emptied, the latter contracts by its own cohesive attraction, giving thus a *primâ facie* appearance of contraction by heat. The pyrometer of Wedgwood is explicable on this principle.

The second class of exceptions, though much more limited in extent, is less easily explained. Water, fused bismuth, and probably some other substances (though the fact as to them is not very clearly established), expand as they approach very near to the freezing or solidifying point. The most probable explanation of these exceptions is, that at the point of maximum density the molecules of these bodies assume a polar or crystalline condition, that by the particles being thus arranged in linear directions like *chevaux de frise*, interstitial spaces are left, containing matter of less density, so that the specific density of the whole mass is diminished.

We cannot examine piecemeal the ultimate structure of matter, but, in addition to the fact that the bodies which evince this peculiarity are bodies which, when solidified, exhibit a very marked crystalline character, there are experiments which show that water between the point of maximum density and its point of solidification polarizes light circularly; showing, if these experiments be correct, a structural alteration in water, and one analogous to that possessed by certain crystalline solids, and to that possessed by water itself, where it is forcibly made to assume a polarized condition by the influence of magnetism. Whether this be, or be not, regarded as an admissible explanation of the exception to the otherwise invariable effect of expansion by heat, must be left to the judgment of each individual who thinks upon the subject; at all events, no theory of heat yet proposed removes the difficulty, and therefore it equally opposes every other view of the phenomena of heat, as it does that which I have here considered, and which regards heat as communicable expansive force.

The general phenomena of heat may, I believe, be all explained upon a purely dynamical view, and without having recourse to the hypothesis of latent matter. Many, however, of the phenomena of heat are involved in much mystery, particularly those connected with specific heat, or that relative proportion of heat which equal weights of different bodies require to raise them from a given temperature to another given temperature, which appear to depend in some way hitherto inexplicable upon the molecular constitution of different bodies.

The view of heat which I have taken, viz., to regard it simply as a communicable molecular repulsive force, is supported by many of the phenomena to which the term specific or relative heat is applied; for example, bodies as they increase in temperature increase in specific heat. Those metals whose rate of expansion increases most rapidly when they are heated, increase most in specific heat; and their specific heat is reduced by percussion, which, by approximating their particles, makes them specifically more dense. When, however, we examine substances of very different physical characters, we find that their specific heats have no relation to their density or rate of expansion by heat; their differences of specific heat must depend upon their intimate molecular constitution in a manner accounted for (as far as I am aware) by no theory of heat hitherto proposed.

In all these cases, however, though the comparative effects of specific heat may not be explicable by any known theory, the absolute effect upon each separate substance is simply expansion, though when bodies differing in their physical characters are used, the expansion varies if measured by the correlative contractions exhibited by the substances producing it.

Thus much for the *correlation* of heat with motion: and if heat be a force capable of producing *motion*, and motion be capable of producing the other modes of force, it necessarily follows that heat is capable, mediately, of producing them: we will, therefore, content ourselves with inquiring how far heat is capable of immediately producing the other modes of force. It will immediately produce *electricity*, as shown in the beautiful experiments of Seebeck, one of which I have already cited, which experiments proved, that when dissimilar metals are made to touch, or are soldered together, and heated at the point of contact, a current of electricity flows through the metals having a definite direction according to the metals employed, which current continues as long as an increasing temperature is gradually pervading the metals, ceases when the temperature is stationary, and flows in the contrary direction with the decrement of temperature.

Another class of phenomena which have been generally attributed to the effects of radiant heat, and to which, from this belief, the term thermography has been applied, may also, in their turn, be made to exhibit electrical effects—effects here of Franklinic or static electricity, as Seebeck's experiments showed effects of voltaic or dynamic electricity.

If polished discs of dissimilar metals—say, zinc and copper,—be brought into close proximity, and kept there for some time, and either of them has irregularities upon its surface, a superficial outline of these irregularities is traceable upon the other disc, and *vice versâ*. Many theories have been framed to account for this phenomenon, but whether it be due or not to thermic radiations, the relative temperature of the discs, their relative capacities and conducting and radiating powers for heat, undoubtedly influence the phenomena.

Now, if two such discs in close proximity be connected with a delicate electroscope, and then suddenly separated, the electroscope is affected, shewing that the reciprocal radiation, from surface to surface, has produced electrical force. I cite this experiment in treating of heat as an initial force, because at present the probabilities are in favour of thermic radiation producing the phenomenon. The origin of these so-called thermographic effects is, however, a question open to much doubt, and needs much further experiment. When I first published the experiment which shewed that the mere approximation of metallic discs would give rise to electrical effects, I mentioned that I considered the fact of the superficial change upon the surface of metals in proximity, and, *à fortiori*, in contact, would explain the development of electricity in Volta's original contact experiment, without having recourse to the contact theory, *i. e.* a theory which supposes a force to be produced by mere contact of dissimilar metals without any molecular or chemical change. I have seen nothing to alter this view. Mr. Gassiot has repeated and verified my experiment with more delicate apparatus and under more unexceptionable circumstances, and without saying that radiant heat is the initial force in this case, we have evidence by the superficial change which takes place in bodies closely approximated that some molecular change is taking place, some force is called into action, by their proximity, which produces changes in matter as it expends, or rather transmits itself; and, therefore, is not a force without molecular change, as the supposed contact force would be. The force in this, as in all other cases, is not created, but developed by the action of matter on matter, and not annihilated, as it is shewn by this experiment to be convertible into another mode of force.

To say that heat will produce *light*, is to assert a fact apparently familiar to every one, but there may be some reason to doubt whether the expression to produce light is correct in this particular application; the relation between heat and light is not analogous to the correlation between these and the other four affections of matter. Heat and light appear to be rather modifications of the same force than distinct forces mutually dependent. The modes of action of radiant heat and of light are so similar, both being subject to the same laws of reflection, refraction, double refraction, and polarization, that their difference appears to exist more in the manner in which they affect our senses, than in our mental conception of them.

The experiments of Melloni, which have mainly contributed to demonstrate this close analogy of heat and light, afford a beautiful instance of the assistance which the progress of one branch of physical science renders to that of another. The discoveries of Œrsted and Seebeck led to the construction of an instrument for measuring temperature, incomparably more delicate than any previously known. To distinguish it from the ordinary thermometer, this instrument is called the *thermo-multiplier*. It consists of a series of small bars of bismuth and antimony, forming one zig-zag chain of alternations arranged parallel to each other, in the shape of a cylinder or prism; so that the points of junction, which are soldered, shall be all exposed at the bases of the cylinder: the two extremities of this series are united to a Galvanometer,—that is, a flat coil of wire surrounding a freely-suspended magnetic needle, the direction of which is parallel to the convolutions of the wire. When radiant heat impinges upon the soldered ends of the multiplier, a thermo-electric current is induced in each pair; and, as all these currents tend to circulate in the same direction, the energy of the whole is increased by the co-operating forces: this current, traversing the helix of the galvanometer, deflects the needle from parallelism by virtue of the electro-magnetic tangential force, and the degree of this deflexion serves as the index of the temperature.

Bodies examined by these means shew a remarkable difference between their transcalescence, or power of transmitting heat, and their transparency: thus, perfectly transparent alum arrests more heat than quartz so dark coloured as to be opaque; and alum coupled with green glass Melloni found was capable of transmitting a beam of brilliant light, while, with the most delicate thermoscope, he could detect no indications of transmitted heat: on the other hand, rock-salt, the most transcalescent body known, may be covered with soot until perfectly opaque, and yet be found capable of transmitting a considerable quantity of heat. Radiant heat, when transmitted through a prism of rock-salt, is found to be unequally refracted, as is the case with light; and the rays of heat thus elongated into what is, for the sake of analogy, called a spectrum, are found to possess similar properties to the primary or coloured rays of light: thus rock-salt is to heat what colourless glass is to light; it transmits heat of all degrees of refrangibility. Alum is to heat as red glass to light; it transmits the least, and stops the most refrangible rays; and rock-salt covered with soot represents blue glass, transmitting the most, and stopping the least refrangible rays.

Certain bodies, again, reflect heat of different refrangibility: thus paper, snow, and lime, although perfectly white,—that is, reflecting light of all degrees of refrangibility, reflect heat only of certain degrees; while metals, which are coloured bodies,—that is, bodies which reflect light only of certain degrees of refrangibility, reflect heat of all degrees. Radiant heat incident upon substances which doubly refract light is doubly refracted; and the emergent rays are polarised in planes at right angles to each other, as is the case with light. Thus, the phenomena of light are imitated closely by those of radiant heat; and the same theory which is considered most plausibly to account for the phenomena of the one will necessarily be applied to the other agent.

In certain cases heat appears to become partially converted into light, by changing the matter affected by heat: thus gas may be heated to a very high point without producing light, or producing it to a very slight degree; but the introduction of solid matter,—for instance, the metal platinum into the highly heated gas,—instantly exhibits light. Whether the heat is converted into light, or whether it is concentrated and increased in intensity by the solid matter, so as to become visible, may be open to some doubt: the fact of solid matter, when ignited by the oxyhydrogen jet decomposing water, as will be presently explained, would seem to indicate that the heat was rendered more intense by condensations in the solid matter, as water in this case decomposed by a heated body, which body has been itself heated by the combining elements of water. The apparent effect, however, of the introduction of solid incombustible matter into heated gas is a conversion of heat into light.

There is another method by which heat would probably be made to produce luminous effects, though I am not aware that the experiment has ever been made.

If we concentrate into a focus by a large lens a dim light, we increase the intensity of the light. Now if a heated body be taken, which, to the unassisted eye, has just ceased to be visible, it seems probable that by collecting and condensing by a lens the different rays which have so ceased to be visible, light would reappear at the focus. The experiment would, however, prove little more than the fact already known, viz., that by increasing the intensity of heat, light is produced, though it would exhibit this effect in a more striking form.

With regard to *chemical affinity* and *magnetism*, perhaps the only method by which in strictness the force of heat may be said to produce them is through the medium of electricity, the thermo-electrical current, produced, as before described, by heating dissimilar metals, being capable of deflecting the magnet, of magnetising iron, and exhibiting the other magnetic effects, and also of forming and decomposing chemical compounds, and this in proportion to the progression of heat: this has not, indeed, as yet been proved to bear a measurable qualitative relation to the other forces produced by it, because so little of the heat is utilized or converted into electricity, much being dissipated, without change, in the form of heat.

Heat, however, directly affects and modifies both the magnet and chemical compounds; the union of certain chemical substances is induced by heat, as, for instance, the formation of water by the union of oxygen and hydrogen gases: in other cases this union is facilitated by heat, and in many instances, as in ammonia and its salts, it is weakened or antagonized: in many of these cases, however, the force of heat seems more a determining than a producing influence, yet to be this, it must have an immediate relation with the force whose reaction it determines: thus, although gunpowder, touched with an ignited wire, subsequently carries on its own combustion or chemical combination, independently of the original source of heat, yet the chemical affinities of the first portion touched must be exalted by, and at the cost of, the heat of the wire; for to disturb even an unstable equilibrium requires a force in direct relation with those which maintain equilibrium.

Since the first edition of this essay was published, I have communicated to the Royal Society some experiments by which an important exception to the general effect of heat on chemical afflnity is removed, and the results of which induce a hope that a generalized relation will ultimately be established between heat, chemical affinity, and physical attraction. I find that if a substance capable of supporting an intense heat, and incapable of being acted upon by water or either of its elements,—such, for instance, as platinum, or iridium,—be raised to a high point of ignition and then immersed in water, bubbles of permanent gas ascend from it, which on examination are found to consist of mixed oxygen and hydrogen in the proportion in which they form water. The temperature at which this is effected is, according to Dr. Robinson, who has since written a valuable paper on the subject, =2386°. Now when mixed oxygen and hydrogen are exposed to a temperature of about 800°, they combine and form water; heat therefore appears to act differently upon these elements according to its intensity, in one case producing composition, in the other decomposition. It seems not a far-strained theory to account for these diverse effects by the fact that the constituent molecules of the oxyhydrogen gas are at ordinary temperatures in a state of stable equilibrium, the attractive force being in excess; while at higher temperatures the equilibrium is unstable, the attractive and repulsive forces being balanced. In the former case, to destroy the equilibrium an antagonising force greater than that which it maintains must be employed; in the latter, any force either antagonising or co-operating will destroy the equilibrium, provided it is a disturbing force, and not absolutely uniform in its action.

If, for instance, we suppose four molecules, A, B, C, D, to be in a balanced state of equilibrium between attracting and repelling forces, the application of a repulsive force between B and C, though it may still further separate B and C, will approximate A to B and C to D, and may bring them respectively within the range of attractive force; or, supposing the repulsive force to be in the centre of an indefinite sphere of particles, all these, excepting those immediately acted on by the force, will be approximated, and having from attraction assumed a state of stable equilibrium, they will retain this, because the repulsive force divided by the mass is not capable of overcoming it. But if the repulsive force be increased in quantity and of sufficient intensity, then the attractive force of all the molecules may be overcome, and decomposition ensue. Thus, water below a certain temperature, and mixed gas above a certain higher temperature, is the state of stable equilibrium, whilst between these limiting temperatures the equilibrium is unstable.

Upon this view, heat has the same relation to chemical affinity as it has to physical attraction; its immediate tendency is antagonistic to both, and it is only by a secondary action that chemical affinity is apparently promoted by heat. Heat may also, upon this view, promote changes of the equilibrium of chemical affinity among mixed compound substances, by decomposing certain compounds and separating elementary constituents whose affinity is greater, when they are brought within the sphere of attraction for the substance with which they are mixed, than for those with which they were originally chemically united: thus an intense heat being applied to a mixture of chlorine and the vapour of water occasions the production of muriatic acid, liberating oxygen.

Carrying out this view, it would appear that a sufficient intensity of heat might yield indefinite powers of decomposition, and there seems some probability of bodies now supposed to be elementary being decomposed or resolved into further elements by the application of heat of sufficient intensity; or, reasoning conversely, it may fairly be anticipated that bodies which will not enter into combination at a certain temperature will enter into combination if their temperature be lowered, and that thus new compounds may be formed by a proper disposition of their constituents when exposed to an extremely low temperature.

ELECTRICITY is that affection of matter or mode of force which most distinctly and beautifully relates other modes of force, and exhibits to a great extent, in a quantitative form, its own relation with them, and their reciprocal relations with it and with each other. From the manner in which the peculiar force called electricity is apparently transmitted through certain bodies, such as metallic wires, the term *current* is commonly used to denote its apparent progress. It is very difficult to present to the mind any theory which will give a definite conception of its *modus agendi:* the early theories regard its phenomena as produced either by a single fluid idio-repulsive, but attractive of all matter, or else as produced by two fluids, each idio-repulsive but attractive of the other. No substantive theory has been proposed other than these two; but although this is the case, I think I shall not be unsupported by many who have attentively studied electrical phenomena, in viewing them as resulting not from the action of a fluid or fluids, but as a molecular polarization of ordinary matter, or as matter acting by attraction and repulsion in a definite direction. Thus, the transmission of the voltaic current in liquids is viewed by Grotthus as a series of chemical affinities acting in a definite direction: for instance, in the electrolysis of water, *i. e.* its decomposition when placed between the poles or electrodes of a voltaic battery, a molecule of oxygen is supposed to be displaced by the exalted attraction of the neighbouring electrode; the hydrogen liberated by this displacement unites with the oxygen of the contiguous molecule of water; this in turn liberates its hydrogen, and so on, the current being nothing else than this molecular transmission of chemical affinity. Again, the electric spark, the brush, and similar phenomena, the old theories regarded as actual emanations of the matter or fluid, Electricity; I venture to regard them as produced by an emission of the material itself from whence they issue, and a molecular action of the gas or intermedium through or across which they are transmitted.

The colour of the electric spark, or of the voltaic arc, (*i. e.* the flame which plays between the terminal points of a powerful voltaic battery) is dependent upon the substance of the metal, subject to such modifications of the intermedium: thus, the electric spark or arc from zinc is blue, from silver, green, from iron, red and scintillating, precisely the colours afforded by these metals in their ordinary combustion; a portion of the metal is also found to be actually transmitted with every electric or voltaic discharge: in the latter case, indeed, where the quantity of matter acted upon is greater than in the former, the metallic particles emitted by the electrodes or terminals can be readily collected, tested, or weighed. It would thus appear that the electrical discharge arises from an actual repulsion and severance of the electrified matter itself, which flies off at the points of least resistance.

A careful examination of the phenomena attending the electric discharge, or the voltaic arc, which is the electric discharge acting on greater portions of matter, tends to modify considerably our previous idea of the nature of the electric force of ignition, and also of combustion. The voltaic arc is, perhaps, strictly speaking, neither ignition nor combustion: it is not simply ignition, because the matter of the terminals is not merely brought to a state of incandescence, but is physically separated and partially transferred from one electrode to another, much of it being dissipated in a vaporous state: it is not combustion, for the phenomena will take place independently of atmospheric air, oxygen gas, or any of the bodies usually called supporters of combustion, combustion being, in fact, chemical union, attended with heat and light. In the voltaic arc we may have no chemical union; for if the experiment be performed in an exhausted receiver, or in nitrogen, the substance forming the electrodes is condensed, and precipitated upon the interior of the vessel, chemically speaking, in an unaltered state: thus, to take a very striking example, if the voltaic discharge be taken between zinc terminals in an exhausted receiver, a fine black powder of zinc is deposited on the sides of the receiver; this can be collected, and takes fire readily in the air by being touched with a match, or ignited wire, instantly burning into white oxide of zinc: to an ordinary observer, the zinc would appear to be burned twice;

first in the receiver, where the phenomenon presents all the appearance of combustion; and secondly, in the real combustion in air. With iron the experiment is equally instructive. Iron is volatilised by the voltaic arc in nitrogen or in an exhausted receiver, and when a scarcely perceptible film has lined the receiver, this is washed with an acid, which then gives, with ferrocyanide of potassium, the prussian blue precipitate: in this case we readily distil iron, a metal by ordinary means *fusible* only at a very high temperature.

Another strong evidence that the voltaic discharge consists of the material itself of which the terminals are composed, is the peculiar rotation which is observed in the light when iron is employed, the magnetic character of this metal causing its molecules to rotate by the influence of the voltaic current.

Excepting those cases where infinitesimally small quantities of matter are acted on, and our means of detection fail, electrical effects are known to us only as changes of ordinary matter. It seems to me as easy to imagine these changes to be effected by a force acting in definite directions, as by a fluid which has no independent or sensible existence, and which, it must be assumed, is associated with, or exerts a force acting upon ordinary matter, or matter of a different order from the supposed fluid. As the idea of the hypothetic fluid is pursued, it gradually vanishes, and resolves itself into the idea of force. The hypothesis of matter without weight, presents in itself, as I believe, fatal objections to the theories of electrica *fluids*, which are entirely removed by viewing electricity as force, and not as matter.

To commence, then, with electricity as an initiating force, we get *motion* directly produced by it in various forms; for instance, in the attraction and repulsion of bodies, evidenced by mobile electrometers, such as that of Cuthbertson, where large masses are acted on: the rotation of the fly-wheel, another form of electrical repulsion, and the deflection of the galvanometer needle, are also modes of palpable, visible motion. Electricity directly produces *heat*, as shewn in the ignited wire, the electric spark, and the voltaic arc; in the latter the most intense heat with which we are acquainted,—so intense, indeed, that it cannot be measured, as every sort of matter is dissipated by it.

In the phenomenon of electrical ignition, as shewn by a heated conjunctive wire, the relation of force and resistance, and the correlate character of the two forces, electricity and heat, are strikingly demonstrated. Let a thin wire of platinum join the terminals of a voltaic battery of suitable power, the wire will be ignited, and a certain amount of chemical action will take place in the cells of the battery,—a definite quantity of zinc being dissolved and of hydrogen eliminated in a given time. If now the platinum wire be immersed in water, the heat will, from the circulating currents of the liquid, be more rapidly dissipated, and we shall instantly find that the chemical action in the battery will be increased, more zinc will be dissolved, and more hydrogen eliminated for the same time.

Reverse the experiment, and instead of placing the wire in water, place it in the flame of a spirit lamp, so that the force of heat meets with greater resistance to its dissipation. We now find that the chemical action is less than in the first or normal experiment. If we place the wire in other different gaseous or liquid media we shall find that the chemical action of the battery will be proportioned to the facility with which the heat is circulated or radiated by these media, and we thus establish an alternating reciprocity of action between these two forces: a similar reciprocity may be established between electricity and motion, magnetism and motion, and so of many other forces. If we cannot realize it with all, it is because we have not yet eliminated interfering actions. If we carefully think over the matter, we shall, unless I am much mistaken, arrive at the conclusion that it cannot be otherwise, unless we suppose that a force can arise from nothing, —can exist without antecedent force.

In the phenomenon of the voltaic arc, the electric spark, &c., to which I have already adverted, electricity directly produces *light* of the greatest known intensity. It directly produces *magnetism* as shown by Oersted, who first distinctly proved the connexion between electricity and magnetism. These two forces act upon each other, not in straight lines, as all other known forces do, but in a rectangular direction; that is, bodies affected by dynamic electricity, or the conduits of an electric current, tend to place magnets at right angles to them; and, conversely, magnets tend to place bodies conducting electricity at right angles to them. Thus an electric current appears to have a magnetic action, in a direction cutting its own at right angles; or, supposing its section to be a circle, tangential to it: if, then, we reverse the position, and make the electric current form a series of tangents to an imaginary cylinder, this cylinder should be a magnet. This is effected in practice by coiling a wire as a helix or spiral, and this, when conducting an electrical current, is to all intents and purposes a magnet. A soft iron core placed within such a helix has the property of concentrating its power, and then we can, by connexion or disconnexion with the source of electricity, instantly make or unmake a most powerful magnet.

The representation of transverse direction by magnetism and electricity appears to have led Coleridge to parallel it by the transverse expansion of matter, or length and breadth, though he injured the parallel by adding galvanism as depth: whether a third force exists which may bear this relation to electricity and magnetism is a question upon which we have no evidence.

Lastly, electricity produces *chemical affinity*, and by its agency we are enabled to obtain effects of analysis or synthesis with which ordinary chemistry does not furnish us. Of these effects we have examples in the brilliant discoveries, by Davy, of the alkaline metals, and in the peculiar crystalline compounds made known by Crosse and Becquerel.

Light is, perhaps, that mode of force the reciprocal relation of which with the others has been the least traced out. Until the discoveries of Niepce, Daguerre, and Talbot, very little could be definitely predicated of the action of light in producing other modes of force. Certain chemical compounds, among which stand preeminent the salts of silver, have the property of suffering decomposition when exposed to light. If, for instance, recently formed chloride of silver be submitted to luminous rays, a partial decomposition ensues; the chlorine is separated and expelled by the action of light, and the silver is precipitated: by this decomposition the colour of the substance changes from white to blue. If now, paper be impregnated with chloride of silver, which can be done by a simple chemical process, then partially covered with an opaque substance, a leaf for example, and exposed to a strong light, the chloride will be decomposed in all those parts of the paper where the light is *not* intercepted, and we shall have, by the action of light, a white image of the leaf on a purple ground: if similar paper be placed in the focus of a lens in a camera-obscura, the objects there depicted will decompose the chloride, just in the proportion in which they are luminous; and thus, as the most luminous parts of the image will most darken the chloride, we shall have a picture of the objects with reversed lights and shadows. The picture thus produced would not be permanent, as subsequent exposure would darken the light portion of the picture: to fix it, the paper must be immersed in a solution which has the property of dissolving chloride of silver, but not metallic silver: iodide of potassium will effect this, and the paper being washed and dried will then preserve a permanent image of the depicted objects. This was the first and simple process of Mr. Talbot: but it is defective as to the purposes aimed at, in many points. First, it is not sufficiently sensitive, requiring a strong light and a long time to produce an image: secondly, the lights and shadows are reversed: and thirdly, the coarse structure of the finest paper does not admit of the delicate traces of objects being distinctly impressed. These defects have been to a great extent remedied by a process subsequently discovered by Mr. Talbot, and which bears his name. The photographs of M. Daguerre, with which all are now familiar, are produced by holding a plate of highly-polished silver over iodine; a thin film of iodide of silver is thus formed on the surface of the metal, and when these iodized plates are exposed in the camera, a chemical alteration takes place; the portions of the plate on which the light has impinged part with some of the iodine, or are otherwise changed—for the theory is somewhat doubtful—so as to be capable of ready amalgamation. When, therefore, the plate is placed over the vapour of heated mercury, the mercury attaches itself to the portions affected by light, and gives them a white frosted appearance; the intermediate tints are less affected, and those parts where no light has fallen, by retaining their original polish, appear dark: the iodide is then washed off by hyposulphite of soda, which has the property of dissolving it, and there remains a picture in which the lights and shadows are as in nature, and the molecular uniformity of the metallic surface enables the most microscopic details to be depicted with perfect accuracy. By using chloride of iodine, or bromide of iodine, instead of iodine, the equilibrium of chemical forces is rendered still more unstable, so that images may be taken in an indefinitely short period,—a period practically instantaneous.

It would be foreign to the object of this essay to enter upon the many beautiful details into which the science of photography has branched out, and the many valuable discoveries and practical applications to which it has led. Amongst other things, it has afforded a means of automatic or self-registration of many periodical phenomena; such as the heights of the barometer and thermometer, the variations of the magnet, the electrical state of the atmosphere, &c.

A vast number of substances are notably affected by light, even those apparently the most inalterable in character, such as metals; so numerous, indeed, are the substances affected, that it has been supposed, not without reason, that matter of every description is altered by exposure to light.

I have used the term light, and affected by light, in speaking of photographic effects; but though the phenomena derived their name from light, it has been doubted by many competent investigators whether the phenomena of photography are not mainly dependent upon a separate agent accompanying light, rather than upon light itself. It is, indeed, difficult not to believe that a picture, taken in the focus of a camera obscura, and which represents to the eye all the gradations of light and shade shewn by the original luminous image, is not an effect of light; certain it is, however, that the different coloured rays exercise different actions upon various chemical compounds, and that the effects on many, perhaps on most of them, are not proportionate in intensity to the effects upon the visual organs; those effects, however, appear to be more of degree than of specific difference, and, without pronouncing myself positively upon the question, hitherto so little examined, I think it will be safer to regard the action on photographic compounds as resulting from a function of light: so viewing it, we get light as an initiating force, capable of producing, mediately or immediately, the other modes of force. Thus, it immediately produces chemical action, and, having this, we at once acquire a means of producing the others. In my Lectures of 1843, I shewed an experiment by which the production of all the other modes of force by light is exhibited:—I may here shortly describe it. A prepared Daguerreotype plate is enclosed in a box filled with water, having a glass front, with a shutter over it; between this glass and the plate is a gridiron of silver wire; the plate is connected with one extremity of a galvanometer coil, and the gridiron of wire with one extremity of a Breguet's helix—an elegant instrument, formed by a coil of two metals, the unequal expansion of which indicates slight changes in temperature; the other extremities of the galvanometer and helix are connected by a wire, and the needles brought to zero. As soon as a beam of either day-light or the oxyhydrogen light is, by raising the shutter, permitted to impinge upon the plate, the needles are deflected: thus, light being the initiating force, we get *chemical action* on the plate, *electricity* circulating through the wires, *magnetism* in the coil, *heat* in the helix, and *motion* in the needles.

There are other apparently more direct agencies of light in producing electricity and magnetism, such as those observed by Morichini and others, as well as its effects upon crystallization, but these results have hitherto been of so indefinite a character that they can only be regarded as presenting fields for experiment, and not as proving the relations of light to the other forces.

Light would seem directly to produce heat in the phenomena of what is termed absorption of light: in these we find that heat is developed in some proportion to the disappearance of light. To take the old experiment of placing a series of different-coloured pieces of cloth upon snow exposed to sunshine: the black cloth absorbing the most light, and developing the most heat, sinks more deeply in the snow than any others; the other colours or shades of colour sink the more deeply in proportion as they absorb or cause to disappear the more light, until we come to the white cloth which remains upon the surface. The heating powers of different colours are, however, not by any means in exact proportion to the intensity of their light as affecting the visual organs. Thus red light, when produced by refraction from a prism of glass, produces greater heating effect than yellow light, in the phenomena of absorption, as has been observed by Sir J. Herschel. The red rays appear, however, to produce a dynamic effect greater than any of the others; thus they penetrate water to a greater depth than the other colours; but then we get the further anomaly that, according to Dr. Seebeck, when light is refracted by a prism of water the yellow rays produce the greater heating effect. The subject, therefore, requires much more experiment before we can ascertain the rationale of the action of the forces of light and heat in this class of phenomena. An experiment has occurred to me on this subject, which I have endeavoured to try, but have not yet succeeded in procuring the apparatus requisite for it. It is to pass a beam of light through two plates of tourmaline or similar substance, and, by a delicate thermoscope, to examine the temperature of the second plate, or that on which the light last impinges; first, when it is in a position to transmit the polarized beam coming from the first plate: and, secondly, to examine it when it has been turned round through an arc of 90°, and the polarized beam is absorbed. It is dangerous to prophesy in a matter of experimental science; but I should rather expect that, if the experiment be carefully performed, the temperature of the second plate would be more raised in the second case than in the first, and it might afford interesting results when tried with light of different colours.

It is generally—as far as I am aware, universally—true that, while light continues as light, even though reflected or transmitted by different media, little or no heat is developed, provided the media are transparent;

and, as far as we can judge, it would appear that, if a medium were perfectly transparent, not the slightest heating effect would take place; but wherever light is absorbed, then heat takes its place, affording us apparently an instance of the conversion of light into heat, and of the fact that the force of light is not, in fact, absorbed or annihilated, but merely changed in character, becoming in this instance converted into heat by impinging on solid matter; as in the instance mentioned in treating of heat, this force was shown to be converted into light by impinging on solid matter. As, however, I have before observed, this correlation of light and heat is not so distinct as with the other affections of matter. The experiments, indeed, of Melloni would seem to show that light may exist in a condition in which it does not produce heat, which our instruments are able to detect.

Light was regarded, by what was termed the corpuscular theory, as being in itself matter or a specific fluid emanating from luminous bodies, and producing the effects of sensation by impinging on the retina. This theory gave way to the undulatory one, which is generally adopted in the present day, and which regards light as resulting from the undulation of a specific fluid to which the name of ether has been given, which hypothetic fluid is supposed to pervade the universe, and to permeate the pores of all bodies.

In a Lecture which I delivered in January, 1842, I stated that it appeared to me more consistent with known facts to regard light as resulting from a vibration of the molecules of matter itself, and not from a specific ether pervading it; just as sound is propagated by the vibrations of wood, or as waves are by water. I am not here speaking of the character of the vibrations of light, sound, or water, which are very different from each other, but am only comparing them so far as they illustrate the propagation of force by motion in the matter itself.

The transparency or opacity of a body appears to depend entirely upon its molecular arrangement. Thus, if striæ occur in a lens or glass through which objects are viewed, the objects are distorted: increase the number of these striæ, the distortion is so increased that the objects become invisible, and the glass ceases to be transparent, though remaining translucent; but alter completely the molecular structure, as by slow solidification, and it becomes opaque. Take, again, an example of a liquid and a gas: a solution of soap is transparent, air is transparent, but agitate them together so as to form a froth or lather, and this, though consisting of two transparent bodies, is opaque; and the reflection of light from the surface of these bodies, when so intermixed, is strikingly different from its reflection before mixture, in the one ease giving to the eye a mere general effect of whiteness, in the other the images of objects in their proper shapes and colours.

Crystalline bodies, again, affect light definitely according to their structure. I might weary you with examples, showing that, in every case which we can trace, the effects of light are changed by any and every change of structure, and that light has a definite connexion with the structure of the bodies affected by it. I cannot but think that it is a strong assumption to regard ether, a purely hypothetical creation, as changing its elasticity for each change of structure, and to regard it as penetrating the pores of bodies of whose porosity we have no proof, the which pores must, moreover, have a definite and peculiar communication also assumed for the purpose of the theory. The advocates of the etherial hypothesis certainly have this advantage, that the ether, being hypothetical, can have its characters modified or changed without any possibility of disproof either of its existence or modifications.

I was not aware, at the time that I first adopted the above view and brought it forward in my Lectures, that the celebrated Leonard Euler had published a somewhat similar theory; and, though it has been considered defective by a philosopher of high repute, I cannot see the force of the arguments by which it has been assailed; and therefore, for the present, though with diffidence, I still adhere to it. The fact itself of the correlation of the different modes of force is to my mind a very cogent argument in favour of their being affections of the same matter; and though electricity, magnetism, and heat, might be viewed as produced by undulations of the same ether as that by means of which light is supposed to be produced, yet this hypothesis offers greater difficulties with regard to the other affections than with regard to light: thus conduction and non-conduction are not explained by it; the transmission of electricity through long wires in preference to the air which surrounds them, and which must be at least equally pervaded by the ether, is irreconcileable with such an hypothesis. The phenomena exhibited by these forces afford, as I think, equally strong evidence with those of light, of ordinary matter acting from particle to particle, and having no action at a distance. The experiments of Faraday on electrical induction, showing it to be an action of contiguous particles, are strongly in favour of this view, and many experiments which I have made on the voltaic arc, some of which I have already mentioned in this essay, are, to my mind, confirmatory of it.

One of the objections to which this view is open, is the necessity involved in it of an universal plenum; for if light, heat, electricity, &c., be affections of ordinary matter, then matter must be supposed to be everywhere where these phenomena are apparent, and consequently there can be no vacuum. The answer appears to me to be, first, that we have no proof of a vacuum ever having been formed; the Torricellian, the most perfect with which we are acquainted, is filled with the vapour of mercury: Davy's experiments on this point prove, at all events, the formation of a vacuum to have been up to his time impracticable. Secondly, the other two theories equally suppose the non-existence of a vacuum; according to the emissive or corpuscular theory, the vacuum is filled by the matter itself, of light, heat, &c.; according to the etherial, it is filled by the all-penetrating ether. Of the existence of matter in the interplanetary spaces we have some evidence in the diminishing periods of comets, and where, from its highly attenuated state, we cannot test the character of the medium by which the forces are conveyed, we may, if we please, call such medium ether.

At the utmost, our assumption, on the one hand, is, that wherever light, heat, &c., exist, ordinary matter exists, though it may be so attenuated that we cannot recognise it by the tests of other forces, such as gravitation. On the other hand, a specific matter without weight must be assumed, of the existence of which there is no evidence, but in the phenomena for the explanation of which its existence is supposed. To account for the phenomena, the ether is assumed; and to prove the existence of the ether, the phenomena are cited. For these reasons, and others above given, I think that the assumption of the universality of ordinary matter is the least gratuitous.

"Ουδεν τι τυ παντος κενον πελει υδε περισσον."

Magnetism, as was proved by the important discovery of Faraday, will produce *electricity*, but with this peculiarity,—that in itself it is static; and, therefore, to produce a dynamic force, motion must be superadded to it: it is, in fact, directive, not motive, altering the direction of other forces, but not, in strictness, initiating them. It is difficult to convey a definite notion of the force of magnetism, and of the mode in which it affects other forces. The following illustration may give a rude idea of magnetic polarity. Suppose a number of wind-vanes, say of the shape of arrows, with the spindles on which they revolve arranged in a row, but the vanes pointing in various directions, a wind blowing from the same point with an uniform velocity, will instantly arrange these vanes in a definite direction, the arrow-heads or narrow parts pointing one way, the swallow-tails or broad parts another. If they be delicately suspended on their spindles a very gentle breeze will so arrange them, and a very gentle breeze will again deflect them, or, if the wind cease, and they have been originally subject to other forces, such as gravity from unequal suspension, they will return to irregular positions, themselves creating a slight breeze by their return. Such a state of things will represent the state of the molecules of soft iron; electricity acting on them,—not, indeed, in straight lines, but in a definite direction,—produces a polar arrangement, which they will lose as soon as the dynamic inducing force is removed.

Let us now suppose the vanes, instead of turning easily, to be more stiffly fixed to the axles, so as to be turned with difficulty; it will require a stronger wind to move them and arrange them definitely; but, when so arranged, they will retain their position; and, should a gentle breeze spring up in another direction, it will not alter their position, but will itself be definitely deflected. Should the conditions of force and stability be intermediate, both the breeze and the vanes will be slightly deflected; or, if there be no breeze, and the spindles be all moved in any direction, preserving their linear relation, they will themselves create a breeze.

Thus it is with the molecules of hard iron or steel in permanent magnets; they are polarised with greater difficulty, but, when so polarised, they cannot be affected by a feeble current of electricity. Again, if the magnets be moved, they themselves originate a current of electricity; and, lastly, the magnetic polarity and the electric current may be both mutually affected, if the degrees of motion and stability be intermediate.

The above instance will, of course, be taken only as an approximation, and not as binding me to any closer analogy than is generally expected of a mechanical illustration. It is difficult to convey by words a definite idea of the dual or antithetic character of force involved in the term polarity. The illustration I have employed, may, I hope, somewhat aid in elucidating the manner in which magnetism acts on the other dynamic forces; *i. e.* definitely directing them, but not initiating them, except while in motion.

Magnets being moved in the direction of lines joining their poles, produce electrical currents in such neighbouring bodies as are conductors of electricity, in directions transverse to the line of motion; and if the direction of motion or the position of the magnetic poles be reversed, the current of electricity flows in a reverse direction. Magnetism can, then, through the medium of electricity, produce *heat*, *light*, and *chemical affinity*. *Motion* it can directly produce under the above conditions; *i. e.* a magnet being itself moved will move other ferreous bodies: these will acquire a static condition of equilibrium, and be again moved when the magnet is also moved. By motion or arrested motion only, could the phenomena of magnetism ever have become known to us. A magnet, however powerful, might rest for ever unnoticed and unknown, unless it were moved near to iron, or iron moved near to it, so as to come within the sphere of its attraction.

But even with other than ferreous, or what have been termed magnetic substances, all will be moved when placed near the poles of very powerful magnets,—some taking a position axially, or in the line from pole to pole of the magnet; others equatorially, or in a direction transverse to that line,—the former being attracted, the latter apparently repelled by the poles of the magnet. These effects, according to the views of Faraday, show a generic difference between the two classes of bodies, magnetics and diamagnetics; according to others, a difference of degree or a resultant of magnetic action; the less magnetic substance being forced into a transverse position by the magnetization of the more magnetic medium which surrounds it.

According to the view I have taken, magnetism may be produced by the other forces, just as the vanes in the instance given are definitely deflected, but cannot produce them except when in motion: motion, therefore, is to be regarded in this case as the initiative force. Magnetism will, however, directly affect the other forces—light, heat, and chemical affinity, and change their direction or mode of action; or, at all events, will so affect matter subjected to these forces, that their direction is changed. Since these lectures were delivered, Faraday has discovered a remarkable effect of the magnetic force in occasioning the deflexion of a ray of polarized light.

When light is reflected from the surface of water, glass, or many other media, it undergoes a change which disables it from being again similarly reflected in a direction at right angles to that at which it has been originally reflected. Light so affected is said to be polarized; it will always be capable of being reflected in planes parallel to the plane in which it has been reflected, but incapable of being reflected in planes at right angles to that plane. At planes having a direction intermediate between the original plane of reflexion, and a plane at right angles to it, the light will be capable of being partially reflected, and more or less so according as the direction of the second plane of reflexion is more or less coincident with the original plane. Light, again, when passed through a crystal of Iceland spar, is what is termed doubly refracted, *i. e.*, split into two divisions or beams, each having half the luminosity of the original incident light; each of these beams is polarized in planes at right angles to each other, and if they be intercepted by the mineral Tourmaline, one of them is absorbed, so that only one polarized beam emerges. Similar effects may be produced by certain other reflections or refractions. A ray of light once polarized in a certain plane continues so affected throughout its whole subsequent course, and at any indefinite distance from the point where it originally underwent the change, the direction of the plane will be the same, provided the media through which it is transmitted be air, water, or certain other transparent substances which need not be enumerated. If, however, the polarized ray, instead of passing through water, be made to pass through oil of turpentine, the definite direction in which it is polarized will be found to be changed, and the change of direction will be greater according to the length of the column of interposed liquid. Instead of being an uniform plane, it will have a curvilinear direction, similar to that which a strip of card would have if forced along two opposite grooves of a rifle-barrel. This curious effect, which was discovered by M. Arago, is produced in different degrees by different media; the direction also varies, the rotation, as it is termed, being sometimes to the right hand and sometimes to the left, according to the peculiar molecular character of the medium through which the polarized ray is transmitted.

If now, the ray of polarized light pass through water, or through any transparent liquid or solid which does not alter or turn aside the plane of polarization, and the column, say of water, through which it passes be subjected to the action of a powerful magnet, the line of magnetic force, or that which would unite the poles of the magnet, being in the same direction as the ray of polarized light, the water acquires the same properties as the oil of turpentine,—the plane of polarization is rotated, and the direction of this rotation is changed by changing the direction of the magnetic force: thus, if we suppose a polarized ray to pass first in its course the north pole of the magnet, then between that and the south pole, it will be deflected, or curved to the right; while if it meet the south pole first in its course, it will, in its journey between that and the north pole, be turned to the left. If the substance through which the ray is transmitted be of itself capable of deflecting the plane of polarization, as, for instance, oil of turpentine, then the magnetic influence will increase or diminish this rotation, according to its direction. A similar effect to this is observed with polarized heat when the medium through which it is transmitted is subjected to magnetic influence.

Whether this effect of magnetism is rightly termed an effect upon light and heat, or is a molecular change of the matter transmitting the light and heat, is a question the resolution of which must be left to the future; at present, the answer to it would depend upon the theory we adopt. If the view of light and heat which I have stated be adopted, then we may fairly say that magnetism, in these experiments, directly affects the other forces; for light and heat being, according to that view, but motions of ordinary matter, magnetism, in affecting these movements, affects the forces which occasion them. If, however, the other theories be adhered to, it would be more consistent with the facts to view these results as exhibiting an action upon the matter itself, and the heat and light as secondarily affected.

When substances are *undergoing* chemical changes, and a magnet is brought near them, the direction or lines of action of the chemical force will be changed. There are many old experiments which probably depended upon this effect, but which were erroneously considered to prove that permanent magnetism could produce or increase chemical action: these have recently been extended by Mr. Hunt and Mr. Wartmann, and are now better understood.

The above cases are applicable to the subject of the present essay, inasmuch as they show a relation to exist between magnetic and the other forces, which relation is, in all probability, reciprocal; but in these cases there is not a production of light, heat, or chemical affinity, by magnetism, but a change in their direction or mode of action.

There is, however, that which may be viewed as a dynamic condition of magnetism; *i. e.* its condition at the commencement and the termination, or during the increment or decrement of its development. While iron or steel is being rendered magnetic, and as it progresses from its non-magnetic to its maximum magnetic state, or recedes from its maximum to zero, it exhibits a dynamic force; the molecules are, as far as we can infer, in motion. Similar effects can then be produced to those which are produced by a magnet whilst in motion.

An experiment which I published in 1845 tends, I think, to illustrate this, and in some degree to show the character of the motion impressed upon the molecules of a magnetic metal at the period of magnetization. A tube filled with the liquid in which magnetic oxide of iron had been prepared, and terminated at each end by plates of glass, was surrounded by a coil of coated wire. To a spectator looking through this tube a flash of light was perceptible whenever the coil was electrized, and less light was transmitted when the electrical current ceased, showing a symmetrical arrangement of the minute particles of magnetic oxide while under the magnetic influence.

While magnetism is in the state of change above described, it will produce the other forces; but it may be said, while magnetism is thus progressive, some other force is acting on it, and therefore it does not initiate: this is true, but the same may be said of all the other forces; they have no commencement that we can trace. We must ever refer them back to some antecedent force equal in amount to that produced, and therefore the word initiation cannot in strictness apply, but must only be taken as signifying the force selected as the first: this is another reason why the idea of abstract causation is inapplicable to physical production. To this point I shall again advert.

Electricity may thus be produced directly by magnetism either when the magnet as a mass is in motion, or when its magnetism is commencing, increasing, decreasing, or ceasing; and heat may similarly be directly produced by magnetism. I have, since the first edition of this essay was published, communicated to the Royal Society a paper by which I think I have satisfactorily proved that whenever any metal susceptible of magnetism is magnetized or de-magnetized, its temperature is raised. This was shown, first, by subjecting a bar of iron, nickel, or cobalt, to the influence of a powerful electro-magnet, which was rapidly magnetized and de-magnetized in reverse directions, the electro-magnet itself being kept cool by cisterns of water, so that the magnetic metal subjected to the influence of magnetism was raised to a higher temperature than the electro-magnet itself, and could not, therefore, have acquired its increased temperature by conduction or radiation of heat from the electro-magnet; and secondly, by rotating a permanent steel magnet with its poles opposite to a bar of iron, a thermo-electric pile being placed opposite the latter.

There is every probability that magnetism, in the dynamic state, either when the magnet is in motion, or when the magnetic intensity is varying, will also directly produce chemical affinity and light, though, up to the present time, such has not been proved to be the case; the reciprocal effect, also, of the direct production of magnetism by light and heat has not yet been experimentally established.

I have used, in contradistinction, the terms dynamic and static to represent the different states of magnetism. The applications I have made of these terms may be open to some exception, but I know of no other words which will so nearly express my meaning.

The static condition of magnetism resembles the static conditiom of other forces: such as the state of tension existing in the beam and cord of a balance, or in a charged Leyden phial. The old definition of force was, that which caused change in motion, and yet even this definition presents a difficulty: in a case of static equilibrium, such, for instance, as that which obtains in the two arms of a balance, we get the idea of force without any palpable apparent motion: whether there be really an absence of motion may be a doubtful question, as such absence would involve in this case perfect elasticity, and in all other cases, a stability which, in a long course of time, nature generally negatives, showing, as I believe, an inseparable connection of motion with matter, and an impossibility of a perfectly immobile or durable state. So with magnetism; I believe no magnet can exist in an absolutely stable state, though the duration of its stability will be proportionate to its original resistance to assuming a polarized condition. This, however, must be taken merely as a matter of opinion: we have, in support of it, the general fact that magnets do deteriorate in the course of years; and we have the further general fact of the instability, or fluxional state, of all nature, when we have an opportunity of fairly investigating it at different and remote periods: in many cases, however, the action is so slow that the changes escape human observation, and until this can be brought to bear over a proportionate period of time, the proposition cannot be said to be experimentally or inductively proved, but must be left to the mental conviction of those who examine it by the light of already acknowledged facts.

Chemical Affinity, or the force by which dissimilar bodies tend to unite and form compounds differing generally in character from their constituents, is that mode of force of which the human mind has hitherto formed the least definite idea. The word itself—*affinity*—is ill chosen, its meaning, in this instance, bearing no analogy to its ordinary sense; and the mode of its action is conveyed by certain conventional expressions, no dynamic theory of it worthy of attention having been adopted. Its action so modifies and alters the character of matter, that the changes it induces have acquired, not perhaps very logically, a generic contra-distinction from other material changes, and we thus use, as contra-distinguished, the terms physical and chemical. The nearest approach, however, that we can form to a comprehension of chemical action, is by regarding it (vaguely perhaps) as a molecular motion. It will directly produce *motion* of definite masses, by the resultant of the molecular changes it induces: thus, the projectile effects of gunpowder may be cited as familiar instances of motion produced by chemical action. It may be a question whether, in this case, the force which occasions the motion of the mass is a conversion of the force of chemical affinity, or whether it is not, rather, a liberation of other forces existing in a state of static equilibrium; but, at all events, through the medium of electricity chemical affinity may be directly and quantitatively converted into the other modes of force. By chemical affinity we can directly produce *electricity;* this latter force was, indeed, said by Davy to be chemical affinity acting on masses: it appears, rather, to be chemical affinity acting in a definite direction through a chain of particles: but by no definition can the exact relation of chemical affinity and electricity be expressed, as the latter, however closely related to the former, yet exists where the former does not, as in a metallic wire, which, when electrified, or conducting electricity, is, nevertheless, not chemically altered, or, at least, not known to be chemically altered.

Volta, the antitype of Prometheus, first enabled us definitely to relate the forces of chemistry and electricity. When two dissimilar metals in contact are immersed in a liquid belonging to a certain class, and capable of acting chemically on one of them, what is termed a voltaic circuit is formed, and, by the chemical action, that peculiar mode of force called an electric current is generated, which circulates from metal to metal, across the liquid, and through the points of contact.

Let us take as an instance of the conversion of chemical force into electrical, the following, which I made known some years ago. If gold be immersed in hydrochloric acid, no chemical action takes place. If gold be immersed in nitric acid, no chemical action takes place; but mix the two acids, and the immersed gold is chemically attacked and dissolved: this is an ordinary chemical action, the result of a double chemical affinity. In hydrochloric acid, which is composed of chlorine and hydrogen, the affinity of chlorine for gold being less than its affinity for hydrogen, no change takes place; but when the nitric acid is added, this latter containing a great quantity of oxygen in a state of feeble combination, the affinity of oxygen for hydrogen opposes that of hydrogen for chlorine, and then the affinity of the latter for gold is enabled to act, the gold combines with the chlorine, and chloride of gold remains in solution in the liquid. Now in order to exhibit this chemical force in the form of electrical force, instead of mixing the liquids, place them in separate vessels or compartments, but so that they may be in contact, which may be effected by having a porous material, such as unglazed porcelain, asbestos, &c., between them. Immerse in each of these liquids a strip or wire of gold: as long as these pieces of gold remain separated no chemical or electrical effect takes place; but the instant they are brought into metallic contact, either immediately or by connecting each with the same metallic wire, chemical action takes place—the gold in the hydrochloric acid is dissolved, electrical action also takes place, the nitric acid is deoxidized by the transferred hydrogen, and a current of electricity may be detected in the metals or connecting metal, by the application of a galvanometer or any instrument appropriate for detecting such effect.

There are few, if any, chemical actions which cannot be experimentally made to produce electricity: the oxidation of metals, the burning of combustibles, the combination of oxygen and hydrogen, &c., may all be made sources of electricity. The common mode in which the electricity of the voltaic battery is generated is by the chemical action of water upon zinc: this action is increased by adding certain acids to the water, which enable it to act more powerfully upon the zinc, or in some cases act themselves upon it; and one of the most powerful chemical actions known—that of nitric acid upon oxidable metals—is that which produces the most powerful voltaic battery, a combination which I made known in the year 1839: indeed, we may safely say, that when the chemical force is utilized, or not wasted, but all converted into electrical force, the more powerful the chemical action, the more powerful is the electrical action which results.

The quantity of the electric current, as measured by the quantity of matter it acts upon in its different phenomenal effects, is proportionate to the quantity of chemical action which generated it; and its intensity, or power of overcoming resistance, is also proportionate to the intensity of chemical affinity when a single voltaic pair is employed, or to the number of reduplications, when the well-known instrument called the voltaic battery is used.

The mode in which the voltaic current is increased in intensity by these reduplications, is in itself a striking instance of the mutual relations and dynamic analogies of different forces. Let a plate of zinc or other metal possessing a strong affinity for oxygen, and another of platinum or other metal possessing little or no affinity for oxygen, be partially immersed in a vessel, A, containing dilute nitric acid, but not in contact with each other; let platinum wires touching each of these plates have their extremites immersed in another vessel, B, containing also dilute nitric acid: as the acid in vessel A is decomposed by the chemical affinity of the zinc for the oxygen of the acid, the acid in vessel B is also decomposed, oxygen appearing at the extremity of the wire which is connected with the platinum: the chemical power is conveyed or transferred through the wires, and for every unit of oxygen which combines with the zinc in the one vessel an unit of oxygen is evolved from the platinum wire in the other. The platinum wire is thus thrown into a condition analogous to zinc, or has a power given to it of determining the oxygen of the liquid to its surface, though it cannot, as is the case with zinc, combine with it under similar circumstances. If we now substitute for the platinum wire which was connected with the platinum plate a zinc wire, we have, in addition to the determining tendency by which the platinum was affected, the chemical affinity of the oxygen in vessel B for the zinc wire: thus we have, added to the force which was originally produced by the zinc of the combination in vessel A, a second force, produced by the zinc in vessel B, co-operating with the first; two pairs of zinc and platinum thus connected produce, therefore, a more intense effect than one pair; and if we go on adding to these alternations of zinc, platinum, and liquid, we obtain an indefinite exaltation of chemical power, just as in mechanics we obtain accelerated motion by adding fresh impulses to motion already generated.

The same law of combination which holds good in chemical combinations also obtains in electrical effects, when these are produced by chemical actions. Dalton proved that the constituents of a vast number of compound substances always bore a definite quantitative relation to each other: thus, water, which consists of one part by weight of hydrogen united to eight parts of oxygen, cannot be formed by the same elements in any other than these proportions; you can neither add to nor subtract from the normal ratio of the elements, without entirely altering the nature of the compound. Further, if any element be selected as unity, the combining ratios of other elements will bear an invariable quantitative relation to that and to each other: thus, if hydrogen be chosen as 1, oxygen will be 8, chlorine will be 36; that is, oxygen will unite with hydrogen in the proportion of 8 parts by weight to 1, while chlorine will unite with hydrogen in the proportion of 36 to 1, or with oxygen in the proportion of 36 to 8. Numbers expressing their combining weights, which are thus relative, not absolute, may, by a conventional assent as to the point of unity, be fixed for all chemical reagents; and, when so fixed, it will be found that bodies generally unite in those proportions, or in simple multiples of them: these proportions are termed *Equivalents.*

Now a voltaic battery, which consists usually of alternations of two metals, and a liquid capable of acting chemically upon one of them, has, as we have seen, the power of producing chemical action in a liquid connected with it by metals upon which this liquid is incapable of acting: in such case the constituents of the liquid will be eliminated at the surfaces of the immersed metals, and at a distance one from the other. For example, if the two platinum terminals of a voltaic battery be immersed in water, oxygen will be evolved at one and hydrogen at the other terminal, exactly in the proportions in which they form water; while, to the most minute examination, no action is perceptible in the intervening stratum of liquid.

It was known before Faraday's time that, while this chemical action was going on in the subjected liquid, a chemical action was going on in the cells of the voltaic battery, but it was scarcely if at all known that the amount of chemical action in the one bore a constant relation to the amount of action in the other. Faraday proved that it bore a direct equivalent relation: that is, supposing the battery to be formed of zinc, platinum, and water, the amount of oxygen which united with the zinc in each cell of the battery was exactly equal to the amount evolved at the one platinum terminal, while the hydrogen evolved from each platinum plate of the battery was equal to the hydrogen evolved from the other platinum terminal. Supposing the battery to be charged with hydrochloric acid, instead of water, while the terminals are separated by water, then for every 36 parts by weight of chlorine which united with each plate of zinc, eight parts of oxygen would be evolved from one of the platinum terminals: that is, the weights would be precisely in the same relation which Dalton proved to exist in their chemical combining weights. This may be extended to all liquids capable of being decomposed by the electric current, thence called *Electrolytes:* and as no voltaic effect is produced by liquids incapable of being thus decomposed, it follows that voltaic action is chemical action taking place at a distance, or transferred through a chain of media, and that the chemical equivalent numbers are the exponents of the amount of voltaic action for corresponding chemical substances.

As heat, light, magnetism, or motion, can be produced by the requisite application of the electric current, and as this is definitely produced by chemical action, we get these forces very definitely, though not immediately, produced by chemical action. Let us, however, here inquire, as we have already done with respect to other forces, how far other forces may directly emanate from chemical affinity.

Heat is an immediate product of chemical affinity. I know of no exception to the general proposition that all bodies in chemically combining produce heat; *i. e.* if solution be not considered as chemical action; and even there, when cold results, it is from a change of consistence, as from the solid to the liquid state, and not from chemical action.

We shall find that the same view of the expenditure of force which we have considered in treating of latent heat holds good as to the expenditure of chemical force when regarded with reference to the amount of heat or repulsive force which it engenders, the chemical force being here exhausted by mechanical expansion—that is, by heat. Thus, in the chemical action of the ordinary combustion of coal and oxygen, the expenditure of fuel will be in proportion to the expansibility of the substances heated; water passing freely into the state of steam will consume more fuel than if it be confined and kept at a temperature above its boiling point.

The definite thermic effects produced by chemical changes have been lately much studied by Mr. Graham, M. Hess, Dr. Andrews, and Mr. Joule. The results obtained by them are complicated, and do not admit of enunciation in such simple propositions as would be intelligible without much detail: it would be premature to enter on these, the more so as M. Hess and Dr. Andrews have arrived at contrary results, the former concluding that in chemical combinations the quantity of heat is determined by the acid, the latter that it is determined by the basic ingredient.

Light is also directly produced by chemical action, as in the flash of gunpowder, the burning of phosphorus in oxygen gas, and all rapid combustions: indeed, wherever intense heat is developed, light accompanies it. In many cases of slow combustion, such as the phenomena of phosphorescence, the light is apparently much more intense than the heat; the former being obvious, the latter so difficult of detection that for a long time it was a question whether any heat was eliminated, and I am not aware that, at the present day, any thermic effects from certain modes of phosphorescence, such as those of phosphorescent wood, putrescent fish, &c., have been detected.

Chemical action produces *magnetism* whenever it is thrown into a definite direction, as in the phenomenon of electrolysis. I may adduce the gas voltaic battery, as presenting a simple instance of the direct production of magnetism by chemical synthesis. Oxygen and hydrogen in that combination chemically unite; but instead of combining by intimate molecular admixture, as in the ordinary cases, they act upon water, *i. e.* combined oxygen and hydrogen, placed between them so as to produce a line of chemical action, and a magnet adjacent to this line of action is deflected, and places itself at right angles to the line. What a chain of molecules does here, there can be no doubt all the molecules entering into combination would produce in ordinary chemical actions; but in such cases, the direction of the lines of combination being irregular and confused, there is no general resultant by which the magnet can be affected.

What the exact nature of the transference of chemical power across an electrolyte is, we at present know not, nor can we form any more definite idea of it than that given by the theory of Grotthus. We have no knowledge as to the exact nature of any mode of chemical action, and, for the present, must leave it as an obscure action of force, of which future researches may simplify our apprehension.

Catalysis, or the chemical action induced by the mere presence of a foreign body, embraces a class of facts which must considerably modify many of our notions of chemical action: thus oxygen and hydrogen, when mixed in a gaseous state, will remain unaltered for an indefinite period; but the introduction to them of a slip of clean platinum will cause more or less rapid combination, without being in itself in any respect altered. On the other hand, oxygenated water, which is a compound of one equivalent of hydrogen plus two of oxygen, will, when below a certain temperature, remain perfectly stable; but touch it with platinum in a state of minute division, and it is instantly decomposed, one equivalent of oxygen being set free. Here, again, the platinum is unaltered, and thus we have synthesis and analysis effected apparently by the mere contact of a foreign body. It is not improbable that the increased electrolytic power of water by the addition of some acids, such as the sulphuric and phosphoric, where the acids themselves are not decomposed, depends upon a catalytic effect of these acids; but we know too little of the nature and rationale of catalysis to express any confident opinion on its modes of action, and possibly we may comprehend very different molecular actions under one and the same name. In no case does catalysis yield us new power or force; it only determines or facilitates the action of chemical force, and, therefore, is no creation of force by contact.

The force so developed by catalysis may be converted into a voltaic form thus: in a single pair of the gas battery above alluded to, one portion of a strip of platinum is immersed in a tube of oxygen, the other in one of hydrogen, both the gases and the extremities of the platinum being connected by water or other electrolyte; a voltaic combination is thus formed, and electricity, heat, light, magnetism, and motion, produced at the will of the experimenter.

In this combination we have a striking instance of correlative expansions and contractions, analogous, though in a much more refined form, to the expansions and contractions by heat and cold detailed in the early part of this essay, and illustrated by the alternations of two bladders partially filled with air: thus, as by the effect of chemical combination in each pair of tubes of the gas battery the gases oxygen and hydrogen lose their gaseous character and shrink into water, so at the platinum terminals of the battery, when immersed in water, water is decomposed, and expands into oxygen and hydrogen gases. The same force which changes gas into liquid at one point of space, changes liquid into gas at another, and the exact volume which disappears in the one place reappears in the other; so that it would appear to an inexperienced eye as though the gases passed through solid wires.

Gravitation, inertia, and aggregation, were but cursorily alluded to in my original lectures; their relation to the other modes of force seemed to be less definitely traceable, but the phenomenal effects of gravitation and inertia, if there be such a force as inertia, being motion and resistance to motion, in considering motion I have in some degree included their relations to the other forces.

Mosotti has mathematically treated of the identity of gravitation with cohesive attraction, and Plücker has recently succeeded in shewing that crystalline bodies are definitely affected by magnetism, and take a position in relation to the lines of magnetic force dependent upon their optical axis or axis of symmetry.

What is termed the optic axis is a fixed direction through crystals, in which they do not doubly refract light, and which direction, in those crystals which have one axis of figure, or a line around which the figure is symmetrical, is parallel to the axis of symmetry. When submitted to magnetic influence such crystals take up a position, so that their optic axis points diamagnetically or transversely to the lines of magnetic force; and when, as is the case in some crystals, there is more than one optic axis, the resultant of these axes points diamagnetically. The mineral Cyanite is influenced by magnetism in so marked a manner that when suspended it will arrange itself definitely with reference to the direction of terrestrial magnetism, and may, according to Plücker, be used as a compass-needle.

There is scarcely any doubt that the force which is concerned in aggregation is the same which gives to matter its crystalline form; indeed, many inorganic bodies, if not all, which appear amorphous, are, when closely examined, found to be crystalline in their structure: we thus get a reciprocity of action between the force which unites the molecules of matter and the magnetic force, and through the medium of the latter the correlation of the attraction of aggregation with the other modes of force may be established.

I believe that the same principles and mode of reasoning as have been adopted in this essay might be applied to the organic as well as the inorganic world; and that muscular force, animal and vegetable heat, &c., might, and at some time will, be shewn to have similar definite correlations; but I have purposely avoided this subject, as pertaining to a department of science to which I have not devoted my attention. I ought, however, while alluding to this subject, shortly to mention some experiments of Professor Matteucci, recently communicated to the Royal Society, by which it appears that whatever mode of force it be which is propagated along the nervous filaments, this mode of force is definitely affected by currents of electricity. His experiments shew that when a current of positive electricity traverses a portion of the muscle of a living animal in the same direction as that in which the nerves ramify — *i. e.* a direction from the brain to the extremities—a muscular contraction is produced in the limb experimented on, showing that the nerve of motion is affected; while, if the current, as it is termed, be made to traverse the muscle in the reverse direction, or towards the nervous centres, the animal utters cries, and exhibits all the indications of suffering pain, scarcely any muscular movements being produced; shewing that in this case the nerves of sensation are affected by the electric current, and therefore that some definite polar condition exists, or is induced, in the nerves, to which electricity is correlated, and that probably this polar condition constitutes nervous agency. There are other analogies given in the papers of M. Matteucci, and derived from the action of the electrical organs of fishes, which tend to corroborate and develope the same view.

I have now gone through the affections of matter for which distinct names have been given in our received nomenclature: that other forces may be discovered, differing as much from these as these differ from each other, is highly probable, and that when discovered, and their modes of action fully traced out, they will be found to be related *inter se*, and to these, as these are to each other, I believe to be as far certain as certainty can be predicted of any future event.

It may in many cases be a difficult question to determine what constitutes a distinct affection of matter or mode of force. It is highly probable that different lines of demarcation would have been drawn between the forces already known, had they been discovered in a different manner, or first observed at different points of the chain which connects them. Thus, radiant heat and light are mainly distinguished by the manner in which they affect our senses; were they viewed according to the way in which they affect inorganic matter, very different notions would possibly be entertained of their character and relation.

Electricity, again, was named from the substance in which, and magnetism from the district where, it first happened to be observed, and a chain of intermediate phenomena have so connected electricity with galvanism that they are now regarded as the same force, differing only in the degree of its intensity and quantity, though for a long time they were regarded as distinct.

The phenomenon of attraction and repulsion by amber, which originated the term *electricity*, is as unlike that of the decomposition of water by the voltaic pile, as any two natural phenomena can well be. It is only because the historical sequence of scientific discoveries has associated them by a number of intermediate links, that they are classed under the same category. What is called voltaic electricity might equally, perhaps more appropriately, be called voltaic chemistry. I mention these facts to show that the distinction in the names may frequently be much greater than the distinction in the subjects which they represent, and *vice versâ*, not as at all objecting to the received nomenclature on these points; nor do I say it would be advisable to depart from it: were we to do so, inevitable confusion would result, and objections equally forcible might be found to apply to our new terminology.

Words, when established to a certain point, become a part of the social mind; its powers and very existence depend upon the adoption of conventional symbols;

and were these suddenly departed from, or varied, according to individual apprehensions, the acquisition and transmission of knowledge would cease. Undoubtedly, neology is more permissible in physical science than in any other branch of knowledge, because it is more progressive; new facts or new relations require new names, but even here it should be used with great caution.

> "Si forte necesse est
> "Indiciis monstrare recentibus abdita rerum,
> "Fingere cinctutis non exaudita Cethegis,
> "Continget; dabiturque licentia, sumpta pudenter."

Even should the mind ever be led to dismiss the idea of various forces, and regard them as the exertion of one force, or resolve them definitely into motion; still we could never avoid the use of different conventional terms for the different modes of action of this one pervading force.

Reviewing the series of relations between the various forces which we have been considering, it would appear that in many cases where one of these is excited or exists, all the others are also set in action: thus, when a substance, such as sulphuret of antimony, is electrified, at the instant of electrization it becomes *magnetic* in directions at right angles to the lines of electric force; at the same time it becomes *heated* to an extent greater or less according to the intensity of the electric force. If this intensity be exalted to a certain point the sulphuret becomes luminous, or *light* is produced: it expands, consequently *motion* is produced; and it is decomposed, therefore *chemical action* is produced. If we take another substance, say a metal, all these forces except the last are developed; and although we can scarcely apply the term chemical action to a substance hitherto undecomposed, and which, under the circumstances we are considering, enters into no new combination, yet it undergoes that species of polarization which, as far as we can judge, is the first step towards chemical action, and which, if the substance were decomposable, would resolve it into its elements. Perhaps, indeed, some hitherto undiscovered chemical action is produced in substances which we regard as undecomposable: there are experiments to show that metals which have been electrized are permanently changed in their molecular constitution. Thus, with some substances, when one mode of force is produced all the others are simultaneously developed. With other substances, probably with all matter, some of the other forces are developed, whenever one is excited, and all may be so were the matter in a suitable condition for their development, or our means of detecting them sufficiently delicate.

The term Correlation, which I selected as the title of my Lectures in 1843, strictly interpreted, means a necessary mutual or reciprocal dependence of two ideas, inseparable even in mental conception: thus, the idea of height cannot exist without involving the idea of its correlate, depth; the idea of parent cannot exist without involving the idea of offspring. It has been scarcely, if at all, used by writers on physics, but there are a vast variety of physical relations to which, if it does not in its strictest original sense apply, cannot certainly be so well expressed by any other term. There are, for example, many facts, one of which cannot take place without involving the other; one arm of a lever cannot be depressed without the other being elevated—the finger cannot press the table without the table pressing the finger.

The probability is, that, if not all, the greater number of physical phenomena are correlative, and that, without a duality of conception, the mind cannot form an idea of them: thus, motion cannot be perceived or probably imagined without parallax or relative change of position. The world was believed fixed until, by comparison with the celestial bodies, it was found to change its place with regard to them: had there been no perceptible matter external to the world, we should never have discovered its motion. In sailing along a river, the stationary vessels and objects on the banks seem to move past the observer: if at last he arrives at the conviction that he is moving, and not these objects, it is by correcting his senses by reflection derived from a more extensive previous use of them: even then he can only form a notion of the motion of the vessel he is in, by its change of position with regard to the objects it passes,—that is, provided his body partakes of the motion of the vessel, which it only does when its course is perfectly smooth, otherwise the relative change of position of the different parts of the body and the vessel inform him of its alternating, though not of its progressive movement. So, in all physical phenomena, the effects produced by motion are all in proportion to the relative motion: thus, whether the rubber of an electrical machine be stationary, and the cylinder mobile, or the rubber mobile and the cylinder stationary, or both mobile in different directions, or in the same direction with different degrees of velocity, the electrical effects are, *cæteris paribus*, precisely the same, provided the relative motion is the same, and so, without exception, of all other phenomena. The question of whether there can be absolute motion, or, indeed, any absolute isolated force, is purely the metaphysical question of idealism or realism—a question for our purpose of little import; sufficient for the purely physical inquirer, the maxim "*de non apparentibus et non existentibus eadem est ratio.*"

The sense I have attached to the word correlation, in treating of physical phenomena, will, I think, be evident, from the previous parts of this essay, to be that of a reciprocal production; in other words, that any force capable of producing another, may, in its turn, be produced by it,—nay, more, can be itself resisted by the force it produces, in proportion to the energy of such production, as action is ever accompanied and resisted by reaction: thus, the action of an electro-magnetic machine is reacted upon by the magneto-electricity developed by its action.

To many, however, of the cases we have been considering, the term correlation may be applied in a more strict accordance with its original sense: thus, with regard to the forces of electricity and magnetism in a dynamic state, we cannot electrize a substance without magnetizing it,—we cannot magnetize it without electrizing it:—each molecule, the instant it is affected by one of these forces is affected by the other; but, in transverse directións, the forces are inseparable and mutually dependent,—correlative, but not identical.

The evolution of one force or mode of force into another has induced many to regard all the different natural agencies as reducible to unity, and as resulting from one force which is the efficient cause of all the others: thus, one author writes to prove that electricity is the cause of every change in matter; another, that chemical action is the cause of everything; another, that heat is the universal cause, and so on. If, as I have stated it, the true expression of the fact is, that each mode of force is capable of producing the others, then any view which regards either of them as abstractedly the efficient cause of all the rest, is erroneous: the view has, I believe, arisen from a confusion between the abstract or generalized meaning of the term cause, and its concrete or special sense; the word itself being indiscriminately used in both these senses. Another confusion of terms has arisen, and has, indeed, much embarrassed me in enunciating the propositions put forth in these pages, on account of the imperfection of scientific language;

an imperfection in great measure unavoidable, it is true, but not the less embarrassing.

Thus, the words light, heat, electricity, and magnetism, are constantly used in two senses,—viz. that of the force producing, or the subjective idea of force or power, and of the effect produced, or the objective phenomenon. The word motion, indeed, is only applied to the effect, and not to the force, and chemical affinity is generally applied to the force, and not to the effect; but the other four terms are, for want of a distinct terminology, applied indiscriminately to both.

I may have occasionally used the same word at one time in a subjective, at another in an objective sense; all I can say is, that this cannot be avoided without a neology, which I have not the presumption to introduce, or the authority to enforce. Again, the use of the term forces in the plural might be objected to by those who do not attach to the term force the notion of a specific agency, but of one universal power associated with matter, of which its various phenomena are but diversely modified effects.

Whether the imponderable agents, viewed as force and not as matter, ought to be regarded as distinct forces or as distinct modes of force, is probably not very material, for, as far as I am aware, the same result would follow either view; I have therefore used the terms indiscriminately, as either happened to be the more expressive for the occasion.

Throughout this essay I have placed motion in the same category as the other affections of matter. The course of reasoning adopted in it, however, appears to me to lead inevitably to the conclusion that these affections of matter are themselves modes of motion; that, as in the case of friction, the gross or palpable motion, which is arrested by the contact of another body, is subdivided into molecular vibrations or undulations, which vibrations are heat or electricity as the case may be; so the other affections are only matter moved or molecularly agitated in certain definite directions. A notion has rapidly gained ground of late years that the passage of electricity and magnetism causes vibrations in an ether permeating the bodies through which the current is transmitted, being an application of the same ethereal hypothesis to these imponderables which had previously been applied to light; others, in speaking of some of their effects, say that electricity and magnetism cause or produce by their passage vibrations in the particles of matter, and regard the vibrations produced as an occasional, though not always a necessary, effect of the passage of electricity, or of the increment or decrement of magnetism. The view which I would venture to suggest is, that such vibrations are themselves electricity or magnetism; or, to express it in the converse, that dynamic electricity and magnetism are themselves motion, and that permanent magnetism, and Franklinic electricity, are static conditions of force bearing a similar relation to motion which gravitation does.

To discuss this theory in detail, and to anticipate objections to it, would lead me into specialties too long, and too foreign to my present object, to be entered upon here; in the course of this essay my principal aim has rather been to show the relation of forces, as evinced by acknowledged facts, than to enter upon any detailed explanation of their specific modes of action. Whether the regarding electricity, light, magnetism, &c. as simply motions of ordinary matter be or be not admissible, certain it is, that all past theories have resolved, and all existing theories do resolve, the actions of these forces into motion. Whether it be that, on account of our familiarity with motion, we refer other affections to it, as to a language which is most easily construed and most capable of explaining them; whether it be that it is in reality the only mode in which all material force is rendered evident; or whether it be that it is the only mode in which our minds, as contradistinguished from our senses, are able to conceive material agencies; certain it is, that all hypotheses hitherto framed to account for the varied phenomena of nature have resolved them into motion. In vain has the mind hitherto sought to comprehend, or the tongue to explain, natural agencies by other means than by motion. Take, for example, the theories of light to which I have before alluded: one of these supposes light to be a highly rare matter, emitted from—*i. e.* put in *motion* by—luminous bodies; a second supposes that the matter is not emitted from luminous bodies, but that it is put into a state of vibration or undulation, *i. e. motion* by them; and thirdly, light may be regarded as an undulation or *motion* of ordinary matter, and propagated by undulations of air, glass, &c., as I have before stated. In all these hypotheses, matter and motion are the only conceptions. We in vain struggle to escape from these ideas; if we ever do so, our mental powers must undergo a change of which at present we see no prospect.

The great problem which remains to be solved, in regard to the correlation of physical forces, is the establishment of their equivalents of power, or their measurable relation to a given standard. Viewed in their static relations, or in the conditions requisite for producing equilibrium or quantitative equality of force, a remarkable relation between chemical affinity and heat is that discovered in many simple bodies by Dulong and Petit, and extended to compounds by Neumann and Avogadro. Their researches have shewn that the specific heats of certain substances, when multiplied by their chemical equivalents, give a constant quantity as product,—or, in other words, that the combining weights of such substances are those weights which require equal accessions or abstractions of heat, equally to raise or lower their temperature. To put the proposition more in accordance with the view we have taken of the nature of heat: each body has a power of communicating or receiving molecular repulsive power, exactly equal, weight for weight, to its chemical or combining power. For instance, the equivalent of lead is 104, of zinc 32, or, in round numbers, as 3 to 1: these numbers are therefore inversely the exponents of their chemical power, three times as much lead as zinc being required to saturate the same quantity of an acid or substance combining with it; but their power of communicating or abstracting heat or repulsive power is precisely the same, for three times as much lead as zinc is required to produce the same amount of expansion or contraction in a given quantity of a third substance, such as water.

Again, a great number of bodies chemically combine in equal volumes, *i. e.*, in the ratios of their specific gravities; but the specific gravities represent the attractive powers of the substance, or are the numerical exponents of the forces tending to produce motion in masses of matter towards each other; while the chemical equivalents are the exponents of the affinities or tendencies of the molecules of dissimilar substances to combine, and saturate each other: consequently, here we have to some extent an equivalent relation between these two modes of force—gravitation and chemical attraction.

Were the above relations extended into an universal law, we should have the same numerical expression for the three forces of heat, gravity, and affinity; and as electricity and magnetism are quantitatively related to them, we should have a similar expression for these forces; but at present the bodies in which this parity of force has been discovered, though in themselves numerous, are small compared with the exceptions, and, therefore, this point can only be indicated as promising a generalization, should subsequent researches alter our knowledge as to the elements and combining equivalents of matter.

With regard to what may be called dynamic equivalents, *i. e.*, the definite relation to time of the action of these varied forces upon equivalents of matter, the difficulty of establishing them is still greater. If the proposition which I stated at the commencement of this paper be correct, that motion may be subdivided or changed in character, so as to become heat, electricity, &c., it ought to follow that when we collect the dissipated and changed forces, and reconvert them, the initial motion, affecting the same amount of matter with the same velocity, should be reproduced, and so of the changes in matter produced by the other forces; but the difficulties of proving the truth of this by experiment will, in many cases, be all but insuperable; we cannot imprison motion as we can matter, though we may to some extent restrain its direction. Electricity promises us the best means of effecting this, but little has hitherto been done in carrying out the inquiry.

In investigating the relations of the different forces, I have in turn taken each one as the initial force or starting-point, and endeavoured to show how the force thus arbitrarily selected could mediately or immediately produce or be merged into the others: but it will be obvious to those who have attentively considered the subject, and brought their minds into a general accordance with the views I have submitted to them, that no force can, strictly speaking, be initial, as there must be some anterior force which produced it: we cannot create force or motion any more than we can create matter. Thus, to take an example previously noticed, and recede backwards; the spark of light is produced by electricity, electricity by motion, and motion is produced by something else, say a steam-engine—that is, by heat. This heat is produced by chemical affinity, *i. e.*, the affinity of the carbon of the coal for the oxygen of the air: this carbon and this oxygen have been previously eliminated by actions difficult to trace, but of the pre-existence of which we cannot doubt, and in which actions we should find the conjoint and alternating effects of heat, light, chemical affinity, &c. Thus, tracing any force backwards to its antecedents, we are merged in an infinity of changing forms of force; at some point we lose it, not because it has been in fact created at any definite point, but because it resolves itself into so many contributing forces, that the evidence of it is lost to our senses or powers of detection; just as, in following it forward into the effect it produces, it becomes, as I have before stated, so subdivided and dissipated as to be equally lost to our means of detection.

Can we, indeed, suggest a proposition, definitely conceivable by the mind, of force without antecedent force? *I* cannot, without calling for the interposition of creative power, any more than I can conceive the sudden appearance of a mass of matter come from nowhere, and formed from nothing. The impossibility, humanly speaking, of creating or annihilating matter, has long been admitted, though, perhaps, its distinct reception in philosophy may be set down to the overthrow of the doctrine of Phlogiston, and the reformation of chemistry, at the time of Lavoisier. The reasons for the admission of a similar doctrine as to force appear to be equally strong. With regard to matter, there are many cases in which we never practically prove its cessation of existence, yet we do not the less believe in it: who, for instance, can trace, so as to re-weigh, the particles of iron worn off the tire of a carriage wheel? who can re-combine the particles of wax dissipated and chemically changed in the burning of a candle? By placing matter undergoing physical or chemical changes under special limiting circumstances, we may, indeed, acquire evidence of its continued existence, weight for weight,—and so we may, in some instances of force, as in definite electrolysis: indeed, the evidence we acquire of the continued existence of matter is by the continued exertion of the force it exercises, as, when we weigh it, our evidence is the force of attraction; so, again, our evidence of force is the matter it acts upon. Thus, matter and force are correlates, in the strictest sense of the word; the conception of the existence of the one involves the conception of the existence of the other: the quantity of matter again, and the degree of force, involve conceptions of space and time. But to follow out these abstract relations would lead me too far into the alluring paths of metaphysical speculation.

That the theoretical portions of this essay are open to objection I am fully conscious. I cannot, however, but think that the fair way to test a theory is to compare it with other theories, and to see whether upon the whole the balance of probability is in its favour. Were a theory open to no objection it would cease to be a theory, and would become a law; and were we not to theorize, or to take generalized views of natural phenomena until those generalizations were sure and unobjectionable,—in other words, were laws,—science would be lost in a complex mass of unconnected observations, which would probably never disentangle themselves. Excess on either side is to be avoided; although we may often err on the side of hasty generalization, we may equally err on the side of mere elaborate collection of observations, which, though sometimes leading to a valuable result, yet, when cumulated without a connecting link, frequently occasion a costly waste of time, and leave the subject to which they refer in greater obscurity than that in which it was involved at their commencement.

Collections of facts differ in importance, as do theories: the former, in many instances, derive their value from their capability of generalization; while, conversely, theories are valuable as methods of co-ordinating given series of facts, and more valuable in proportion as they require fewer exceptions and fewer postulates. Facts may sometimes be as well explained by one view as by another, but without a theory they are unintelligible and incommunicable. Let us use our utmost effort to communicate a fact without using the language of theory, and we fail; theory is involved in all our expressions; the knowledge of bygone times is imported into succeeding times by terms involving theoretic conceptions. As the knowledge of any particular science developes itself, our views of it become more simple; hypotheses, or the introduction of supposititious views, are more and more dispensed with; words become applicable more directly to the phenomena, and, losing the hypothetic meaning which they necessarily possessed at their reception, acquire a secondary sense, which brings more immediately to our minds the facts of which they are indices. The scaffolding has served its purpose. The hypothesis fades away, and a theory or generalized view of phenomena more independent of supposition, but still full of gaps and difficulties, takes its place. This in its turn, should the science continue to progress, either gives place to a more simple and wider generalization, or becomes, by the removal of objections, established as a law. Even in this more advanced stage words importing theory must be used, but phenomena are now intelligible and connected, though expressed by varied forms of speech.

To think on nature is to theorize; and difficult it is not to be led on by the continuities of natural phenomena to theories which appear forced and unintelligible to those who have not pursued the same path of thought; which, moreover, if allowed to gain an undue influence, seduce us from that truth which is the sole object of our pursuit.

The equivalent ratios in which the greater number of substances chemically combine, held good in so many instances, that the atomic doctrine was believed to be universally applicable, and called by some a law; and yet, when we follow it in the combinations of substances whose mutual chemical attractions are very feeble, we find the relation fade away, and we seek to recover it by applying a separate and arbitrary multiplier to the different constituents.

By doing this, chemists may make every combination assume in expression an equivalent form; but they have passed from the original law, which contemplated only definite multiples, and the very hypothetic expressions of atoms, which the apparently simple relations of combining weights first led them to adopt; they are obliged to vary and to contradict in terms, by dividing that which their hypothesis and the expression of it assumed to be indivisible.

A similar result seems gradually obtaining in regard to the doctrine of compound radicals. The discovery of cyanogen by Gay-Lussac was probably the first inducement to the doctrine of compound radicals; a doctrine which is now generally, perhaps too generally, received in organic chemistry. As, in the case of cyanogen, a body obviously compound discharged in almost all its reactions the functions of an element, so in many other cases it was found that compound bodies in which a great number of elements existed might be regarded as binary combinations, by considering certain groups of these elements as a compound radical; that is, as a simple body when treated of in relation to the more complex substances of which it forms part, and only as non-elementary when referred to its internal constitution.

Undoubtedly, by approximating in theory the reactions of inorganic and of organic chemistry, by keeping the mind within the limits of a beaten path instead of allowing it to wander through a maze of isolated facts, the doctrine of compound radicals has been of service; but, on the other hand, the indefinite variety of changes which may be rung upon the composition of an organic substance, by different associations of its primary elements, makes the binary constituents vary as the minds of the authors who treat of them, and makes their grouping depend entirely upon the strength of the analogies presented to each individual mind. From this cause, and from the extreme license which has been taken in theoretic groupings deduced from this doctrine, a serious question arises whether it may not ultimately, unless carefully restricted, produce confusion rather than simplicity, and be to the student an embarrassment rather than an assistance.

Where to draw the line,—where to say thus far we may go, and no farther, in any particular class of analogies or relations which Nature presents to us; how far to follow the progressive indications of thought, and where to resist its allurements,—is a question of degree which must depend upon the judgment of each individual or of each class of thinkers; yet it is consolatory that thought is seldom expended in vain.

I have throughout endeavoured rather to discard hypotheses than to add to them. If some of the views have been adopted upon insufficient data, I still hope that this essay will not prove valueless. It was not until I had long reflected on the subject, that I ventured to publish my views: their publication may induce others to think on their subject-matter. They are not put forward with the same objects, nor do they aim at the same elaboration of detail, as memoirs on newly discovered physical facts: they purport to be a method of mentally regarding known facts, some few of which I have myself made known on other occasions, but the great mass of which have been accumulated by the labours of others, and are admitted as established truths. Every one has a right to view these facts through any medium he thinks fit to employ, but some theory must exist in the mind of every one who reflects upon the many new phenomena which have recently, and more particularly during the present century, been discovered. It is by a generalized or connected view of past acquisitions in natural knowledge that deductions can best be drawn as to the probable character of the results to be anticipated. It is a great assistance in such investigations to be intimately convinced that no physical phenomenon can stand alone: each is inevitably connected with anterior changes, and as inevitably productive of consequential changes, each with the other, and all with time and space; and, either in tracing back these antecedents or following up their consequents, many new phenomena will be discovered, and many existing phenomena hitherto believed distinct will be connected and explained: explanation is, indeed, only relation to something more familiar, not more known—*i. e.* known as to causative or creative agencies. In all phenomena, the more closely they are investigated the more are we convinced that, humanly speaking, neither matter nor force can be created or annihilated, and that an essential cause is unattainable. —Causation is the will, Creation the act, of God.

NOTES AND REFERENCES.

PAGE

7\. The reader who is curious as to the views of the ancients, regarding the objects of science, will find clues to them in the second book of Aristotle's Physics, and in the first three books of the Metaphysics. See also the Timæus of Plato and Ritter's History of Ancient Philosophy, where a sketch of the Philosophy of Leucippus and Democritus will be found.

8\. Bacon's Novum Organum, book 2nd, aph. 5 and 6.

9\. Hume's Enquiry concerning Human Understanding, S. 7, London, 1768.

Brown's Inquiry into the Relations of Cause and Effect, London, 1835.

The illustration I have used of a floodgate has been objected to, as being one to which the term cause would scarcely be applied, but after some consideration I have retained it: if cause be viewed only as sequence, it must be limited to sequence under given conditions or circumstances, and here, given the conditions, the sequence is invariable. I see no difference *quoad* the argument, between this illustration and that of Brown of a lighted match and gunpowder (4th edit. p. 27), to which my reasoning would equally well apply.

10\. Herschel's Discourse on the Study of Natural Philosophy, pp 88 and 149.

11\. Quarterly Review, vol. lxviii. p. 212.

Whewell, On the Question "Are Cause and Effect Successive or Simultaneous" (Cambridge Philosophical Transactions, vol. vii. p. 319).

12\. Herschel's Discourse, p. 93.

Ampere, Théorie des Phénomènes Electro-dynamiques, Memoirs in the Ann. de Chimie et de Physique, and works from 1820 to 1826, Paris.

14\. Lamarck, "Sur la Matière du Son" (Journal de Physique, vol. xlix. p. 397).

17\. D'Alembert, Traité de Dynamique, pp. 3 and 4, Paris, 1796.

PAGE

17. Babbage, On the Permanent Impression of our Words and Actions on the Globe we inhabit, 9th Bridgewater Treatise, ch. ix.
20. In speaking of heat being increased by roughening the points of contact, I have expressed myself in a manner which may be misunderstood. I do not mean that rough surfaces always yield more heat by friction than smooth ones; such is not the case. Frequently, there being more particles brought into contact, there is more impediment to motion by the friction of smooth bodies than by that of rough ones: to such cases the reasoning in the text equally applies. I had in my mind the cases of smooth bodies sliding past each other.

    Erman, Influence of Friction upon Thermo-Electricity (Reports of the British Association, 1845).
22. Becquerel, Dégagement de l'Electricité par Frottement, Traité de l'Electricité, tom. ii. p. 113, et seq.
23. Wheatstone, On the Prismatic Decomposition of Electrical Light (Notices of Communications to the British Association, p. 11, 1835).
25. Bacon, De formâ calidi, nov. org. book 2, aph. 20.

    Rumford, An Enquiry concerning the Source of Heat which is excited by Friction (Phil. Trans. p. 80, 1798).

    Davy on the Conversion of Ice into Water by Friction (West of England Contributions, p. 16).

    Of Heat or Calorific Repulsion (Elements of Chemical Philosophy, p. 69).

    Carnot, Réflexions sur la Puissance Motrice du Feu, Paris, 1824.

    Joule, On the Mechanical Equivalent of Heat (Phil. Trans. 1850, p. 61).
26. Baden Powell, On the Repulsive Power of Heat (Phil. Trans. 1834, p. 485).

    Fresnel, Annales de Chimie, tom. xxix. pp. 57 and 107.

    Moser, On Invisible Light (Taylor's Scientific Memoirs, vol. iii. pp. 461 and 465).
27. Black, On Latent Heat (Elements of Chemistry, p. 144, et passim, 1803).
30. The experiments of Henry and Donny have shewn that the cohesion of liquids, as far as their antagonism to rupture goes, is much greater than has been generally believed. These experiments, however, make no difference in the view I have put forth, as, whatever be the character of the attraction, there is a molecular attraction to be overcome in changing bodies from the solid to the liquid state, which must require and exhaust force.

PAGE

30. Donny, Sur la Cohésion des Liquides (Mémoires de l'Académie Royale de Bruxelles, 1843).

Henry, Proceedings of the American Philosophical Society, April 1844 (Silliman's Journal, vol. xlviii. p. 215).

34. Thilorier, Solidification de l'Acide carbonique (Ann. de Ch. et de Phys. tom. lx. p. 432).

Doubtless other actions than those mentioned in the text interfere in producing the sensations of heat and cold; but I think it will be seen that these will not affect the arguments as to the nature of heat.

35. I. Wedgwood, Thermometer for Measuring the Higher Degrees of Heat (Phil. Trans. 1782, p. 305; and 1786, p. 390).

36. Despretz, Recherches sur le Maximum de Densité de l'Eau pure et des Dissolutions aqueuses (Ann. de Ch. et de Ph. tom. lxx. p. 45, and tom. lxxiii. p. 296).

The result mentioned in the text of circular polarization by water, at temperatures below its maximum density, has recently been negatived by M. Biot (Comptes Rendus de l'Académie des Sciences, Paris, 1850, p. 281).

37. Dulong and Petit, and Regnault. See their memoirs abstracted and referred to in Gmelin's Handbook of Chemistry, translated by Watts for the Cavendish Society, vol. i. p. 242, et seq.

39. Grove, Electricity produced by approximating Metals: Report of a Lecture at the London Institution (Literary Gazette, 1843, p. 39).

40. Gassiot, Phil. Mag. Oct. 1844.

Roget, On the Improbability of the Contact exciting Force: Treatise on Galvanism (Library of Useful Knowledge, S. 113).

Faraday, Phil. Trans. 1840, p. 126.

41, 42. Melloni, Sur la Polarization de la Chaleur: Recherches sur plusieurs Phénomènes calorifiques (Annales de Chimie et de Ph. tom. xlv. pp. 5-68; tom. xli. pp. 375-410; tom. xlviii. pp. 198, 218).

Forbes, On the Refraction and Polarization of Heat (Transactions of the Royal Society of Edinburgh, vol. xiii. pp. 131 and 168).

43. T. Wedgwood, On the Production of Light and Heat by different Bodies (Phil. Trans. vol. lxxxii. p. 272).

In speaking of heat and light, I have used the word conversion as applied to the evolution of one mode of force when produced by another. I find this word, or similar expressions (such as transformation, &c.), have been used before as applied to certain imponderables, and objected to. The objection seems to me to rest upon the theory that the imponderables are substantive

PAGE

entities. If they be viewed as forces, or as modes of motion, it does not seem to me to apply. Except in this portion of the text, I have used the word sparingly; but in treating of these subjects, it is impossible to use a phraseology which will be entirely unexceptionable.

45. GROVE, On the Decomposition of Water into its Constituent Gases by Heat (Phil. Trans. 1847, p. 1).

46. ROBINSON, On the Effect of Heat in lessening the Affinities of the Elements of Water (Transactions of the Royal Irish Academy, vol. xxi. p. 2).

47. GROVE, Water decomposed by Chlorine and Heat (Phil. Trans. 1847, p. 20).

48. DUFAYE, SYMMER, WATSON, and FRANKLIN, Theories of Electric Fluid and Electric Fluids (Priestley's History of Electricity, pp. 429—441).

GROTTHUS, Sur la Décomposition de l'Eau et des Corps qu'elle tient en dissolution à l'aide de l'Electricité galvanique (Ann. de Chimie, tom. lviii. p. 54).

49. FUSINIERI, Du Transport des Matières pondérables qui s'opère dans les Décharges électriques (Archives de l'Electricité; Supplément à la Bibliothèque universelle de Genève, tom. iii. p. 597).

50. GROVE, On the Voltaic Arc (Report of Lecture at the Royal Institution, Lit. Gaz. and Athenæum, Feb. 7, 1845; Phil. Trans. 1847, p. 16).

52. DAVY, On the Properties of Electrified Bodies in their relations to Conducting Powers and Temperature (Phil. Trans. 1821, p. 428)

53. GROVE, On the Effect of surrounding Media on Voltaic Ignition (Phil. Trans. 1849, p. 49).

54. OERSTED, Expériences sur l'Effet du Conflict électrique sur l'Aiguille aimantée (Ann. de Ch. et de Ph. tom. xiv. p. 417).

COLERIDGE, Table Talk, vol. i. p. 65.

DAVY, Decomposition of the fixed Alkalies (Phil. Trans. 1808, p. 1).

BECQUEREL, Des Composés électro-chimiques (Traité de l'Electricité, vol. iii. c. 13).

CROSSE, Transactions of the British Association, vol. v. p. 47; Proceedings of the Electrical Society, p. 320.

55. NIEPCE and DAGUERRE, Historique et Description des Procédés du Daguerréotype, Paris, 1839.

TALBOT, Photogenic Drawing and Calotype (Phil. Mag. March 1839 and August 1841).

PAGE

57. Brooks and Ronalds, Automatic Registration by Photography (Phil. Trans. 1847, pp. 59 and 111).

Herschel, Chemical Action of the Solar Spectrum on various Substances (Phil. Trans. 1840, p. 1, and 1842, p. 181).

Hunt, Researches on Light, Lond. 1844.

59. Somerville (Mrs.), On the Magnetising Power of the more refrangible Solar Rays (Phil. Trans. 1826, p. 132).

Morichini's experiments are given in Mrs. Somerville's paper.

Herschel, On the Absorption of Light in Coloured Media viewed in connexion with the Undulatory Theory (Phil. Mag. Dec. 1833).

Seebeck, Heat of Coloured Rays (Brewster's Optics, p. 90).

61. For the first enunciations of the Corpuscular and Undulatory Theories, see Newton's Optics, Hooke's Micographia, and Huyghens' Tractatus de Lumine.

63. Though I suggested this view orally without being aware that it had been previously advanced, I should have hesitated in bringing it forward here, on account of my inability to work out the mathematical details, had it not been sanctioned by so great a name as that of Euler, who cannot be supposed to have overlooked any irresistible arguments against it, the more so in a matter so much controverted and discussed as the undulatory theory of light was in his time. Dr. Young, who opposed it, was afterwards obliged to call to his assistance the vibrations of the ponderable matter of the refracting media, to explain why rays of all colours were not equally refracted, and other difficulties. One of his arguments in support of the existence of a permeating ether was, "that a medium resembling in many properties that which has been denominated Ether does exist, is undeniably proved by the phenomena of Electricity." This seems to me, if I may venture to say so of any thing proceeding from so eminent a man, scarcely logical: it is supporting one hypothesis by another, and considering that to be proved which its most strenuous advocates admit to be surrounded by very many difficulties.

The experiments of M. Foucault, published while these sheets were going through the press, by proving that light travels with less velocity through water than through air, are in favour of the undulatory theory, and, as far as I have been able at present to judge, in favour of the view of it proposed by Euler.

I find a general readiness to admit the doctrine of propagation by undulations of ordinary matter as applied to heat,

PAGE

but not so much so when it is sought to be applied to light. It seems to me impossible, after the close analogies between heat and light made known by MELLONI and FORBES, that a theory applicable to the one should not be applicable to the other of these forces (YOUNG, Phil. Trans. 1800, p. 126; HERSCHEL, Encyc. Metrop. art. Light, pp. 450 and 738; NEWTON'S Optics, p. 322; WHEWELL'S Hist. Induc. Sc. vol. ii. p. 449; FOUCAULT, Comptes Rendus, Paris, 1850, p. 65).

64. The question of the divisibility of matter and of the limitation of its expansibility may collaterally enter into consideration here. The well-known paper of WOLLASTON, in which, from the absence of notable refraction near the margin of the Sun and the planet Jupiter, he considered himself entitled to conclude that the expansion of the earth's atmosphere had a definite limit, and was balanced at a certain point by gravitation, has been shewn to be inconclusive by Dr. WHEWELL, and has also been impugned upon other grounds by Dr. WILSON. There is a point not adverted to in these papers, and which WOLLASTON does not seem to have considered, viz. that there is no evidence that the apparent discs of the Sun and of Jupiter shew us their real discs or bodies. Sir W. HERSCHEL regards the margin of the visible discs as that of clouds or a peculiar state of atmosphere, and the rapidly changing character of the apparent surfaces renders some such conclusion necessary. If this be so, refraction of an occulted star could not be detected,—at all events, in the denser portion of the atmosphere.

Sir W. HERSCHEL'S observations go to prove that the Sun and Jupiter have dense atmospheres, while WOLLASTON'S were believed to prove that they have no appreciable atmospheres (WOLLASTON, Phil. Trans. 1822, p. 89; WHEWELL, Phil. of the Induct. Sc. vol. i. p. 419; WILSON, Trans. of the Roy. Soc. of Edin. vol. xvi. p. 79; Sir W. HERSCHEL, Phil. Trans. 1793, p. 201; and 1801, p. 300).

DAVY admits that he could not succeed in procuring a vacuum, but found electricity much less readily conducted or transmitted by the best vacuum he could procure, than by the ordinary Boylean vacuum.

MORGAN found no conduction by a good Torricellian vacuum; and, although DAVY does not seem to place much reliance on MORGAN'S experiments, there was one point in which they were less liable to error than those of DAVY. MORGAN, whose experiments seem to have been very carefully conducted, operated with hermetically sealed glass tubes and by induced electri-

PAGE

city, while DAVY sealed a platinum wire into the extremity of the tube in which he sought to produce a vacuum. I have found in very numerous experiments which I made to exclude air from water, that platinum wires most carefully sealed into glass allow liquids to pass between them and the glass, and this gives every reason to believe, that gases may equally pass through, but in such extremely minute quantities, that a long time would be requisite for their detection by ordinary tests. DAVY supposed that the particles of bodies may be detached, and so produce electrical effects in a vacuum; and such effects were more likely to obtain in his experiments, where a wire projected into the exhausted space, than in MORGAN'S, where the induced electricity was diffused over the surface of the glass (MORGAN, Phil. Trans. vol. lxxv. p. 272; DAVY, Phil. Trans. 1822, p. 64; Elements of Chemical Philosophy. p. 97).

65. Diminishing Periods of Comets (HERSCHEL'S Outlines of Astronomy, p. 357).

67. FARADAY, Evolution of Electricity from Magnetism (Phil. Trans. 1832, p. 125).

68. FARADAY, Magnetic Condition of all Matter (Phil. Trans. 1846, p. 21; Phil. Mag. 1846, p. 249).

BECQUEREL, Ann. de Ch. et de Ph. tom. xxxvi. p. 337; Comptes Rendus, Paris, 1846, p. 147; and 1850, p. 201.

69. MALUS, Polarization of Light by Reflexion (Mémoires d'Arcueil, tom. ii. p. 143).

By double refraction (BREWSTER'S Optics, p. 159).

70. ARAGO, Circular Polarization by Solids (Mémoires de l'Institut, 1811).

BIOT, Circular Polarization by Liquids (Mémoires de l'Institut, 1817).

FARADAY, On the Magnetization of Light (Phil. Trans. 1846, p. 1).

WARTMANN, Rotation of Plane Polarization of Heat by Magnetism (Journal de l'Institut, No. 644).

PROVOSTAYE et DESSAINES, Ann. de Ch. et de Phys. Oct. 1849.

72. HUNT, Influence of Magnetism on Molecular Arrangement (Phil. Mag. 1846, vol. xxviii. p. 1; Memoirs of Geological Society, vol. i. p. 433).

WARTMANN, Phil. Mag. 1847, vol. xxx. p. 263.

73. GROVE, Experiment on Molecular Motion of a Magnetic Substance (Electrical Mag. 1845, vol. i. p. 601).

On the direct Production of Heat by Magnetism (Proceedings of the Royal Society, 1849, p. 826).

PAGE

After this paper was communicated and ordered to be printed in the Philosophical Transactions, I found that I had been anticipated by Mr. Van Breda, who communicated, in 1845, a paper to the Institut on the subject: his paper appears in the Comptes Rendus under an erroneous title, which accounts for its having been overlooked: he does not give thermometric measures of the heat he obtained, nor did he produce heating effects by a permanent steel magnet, or with other metals than iron; but there can be no doubt he was correct as to the result he obtained (Comptes Rendus, Oct. 27, 1845).

See also an experiment by Mr. Joule, Phil. Mag. 1843, to which he has called my attention since my paper was read.

77. Davy, Electricity defined as Chemical Affinity acting on Masses (Phil. Trans. 1826, p. 389).

Volta, Electricity excited by the mere Contact of conducting Substances (Phil. Trans. 1800, p. 403).

Grove, Gold-leaf Experiment (Comptes Rendus, Paris, 1839, p. 567).

78. Grove, Voltaic Action of Sulphur, Phosphorus, and Hydrocarbons (Phil. Trans. 1845, p. 351).

79. Grove, New Voltaic Combination (Phil. Mag. vol. xiv. p. 388; vol. xv. p. 287).

81. Dalton, New System of Chemistry, London, 1810.

82. Faraday, Definite Electrolysis (Phil. Trans. 1834, p. 77).

84. Graham, Heat disengaged in Chemical Combinations (Proceedings of the Chemical Society, vol. i. p. 106; and vol. ii. p. 51).

Andrews, Phil. Trans. 1844, p. 21.

Hess, Poggendorff's Annalen, vol. lii. p. 107.

Joule, Phil. Mag. vol. xxii. p. 204.

86. Catalysis by Platinum (Dobereimer, Ann. de Ch. et de Phys. tom. xxiv. p. 93; Dulong and Thenard, Ann. de Ch. et de Phys. tom. xxiii. p. 440).

87. Grove, Gas Voltaic Battery (Phil. Mag. vol. xxi. p. 417; Phil Trans. 1843, p. 91).

88. Mosotti, Forces which regulate the Internal Constitution of Bodies (Taylor's Scientific Memoirs, vol. i. p. 448).

Plücker, Repulsion of the Optic Axes of Crystals by the Poles of a Magnet (Taylor's Scientific Memoirs, vol. v. p. 353).

Magnetic Action of Cyanite (Literary Gazette, 1849, p. 431).

89. Matteucci, Correlation of Electric Current and Nervous Force (Phil. Trans. 1850, p. 287).

A paper, by Dr. Carpenter, on certain Relations of the Vital and Physical Forces, has lately been read to the Royal Society, but

PAGE

is not yet printed; and others, by Dr. Fowler and Mr. Newport, were read to the meetings of the British Association in 1849 and 1850: a work, entitled "Proteus," has also been published on the subject by Dr. Radcliffe.

93. Molecular changes in electrized metals (Nairne, Phil. Trans. 1780, p. 334, and 1783, p. 223; Grove, Electrical Mag. vol. i. p. 120; Peltier, Archives de l'Electricité, vol. v. p. 182; Fusinieri, id. p. 516).

100. Dulong and Petit, Relation between Specific Heat and Chemical Equivalents (Ann. de Ch. et de Phys. tom. x. p. 395).

Neumann, Poggendorff's Annalen, vol. xxiii. p. 1.

Avogadro, Ann. de Ch. et de Phys. tom. lv. p. 80.

The paper of Mr. Joule, on the Mechanical Equivalent of Heat, referred to in the note to p. 25, reached me too late to be discussed in the text. The conclusion he deduces from his experiments is, that a fall of 772 lbs through a space of one foot is able to raise the temperature of a pound of water through one degree of Fahrenheit's scale.

106. The distinction which I have made between hypothesis and theory may, perhaps, be considered finely drawn, but it is consistent with the etymon of the words, and is not unsupported by authority.

107. It will not, I trust, be supposed that I do not recognise a great natural truth in the definite combining ratios presented by a vast number of compounds: the doubt I entertain is, whether the doctrine of simple multiples, or, still more, the atomic theory, can fairly be applied to cases where the multiplier of the atoms of each constituent is obliged to be varied empirically for each compound; as, if it be permitted to select at will a multiplier or division, there is no conceivable combination which may not be presented in an atomic form.

THE END.

WILSON AND OGILVY, 57, SKINNER STREET, SNOWHILL, LONDON.

OCTOBER.

MEDICAL

AND

SCIENTIFIC

WORKS,

PUBLISHED BY

HIGHLEY AND SON, 32, FLEET STREET, LONDON,

Booksellers, by Appointment,
to
The Royal College of Surgeons of England.

1852.

STANDARD WORKS
ON THE
DISEASES OF TROPICAL CLIMATES.

I.

BRYSON ON THE DISEASES, &c. OF AFRICA.

(*By Order of the Lords Commissioners of the Admiralty.*)

Report on the Climate and Diseases of the African Station. Compiled from Documents in the Office of the Director-General of the Medical Department, under the immediate direction of Sir W. BURNETT, M.D. &c. By ALEXANDER BRYSON, M.D. Royal 8vo. half-bound, 8s.

II.

DANIELL ON THE DISEASES, &c. OF GUINEA.

Sketches of the Medical Topography and Native Diseases of the Gulf of Guinea, Western Africa. By WILLIAM F. DANIELL, M.D., Assistant-Surgeon to the Forces. 8vo. 10s. 6d.

III.

DR. JAMES JOHNSON AND MR. J. RANALD MARTIN.

The Influence of Tropical Climates on European Constitutions. By Dr. JAMES JOHNSON. *Sixth Edition*, with important Additions, by Mr. J. R. MARTIN, late Presidency Surgeon, &c., Calcutta. 8vo. 18s.

IV.

BOYLE'S DISEASES OF AFRICA.

A Practical Medico-Historical Account of the Western Coast of Africa: together with the Symptoms, Causes, and Treatment of the Fevers and other Diseases of Western Africa. By JAMES BOYLE, late Colonial Surgeon to Sierra Leone. 8vo. 12s.

V.

ROGERS ON CHOLERA IN THE MADRAS ARMY.

Reports on Asiatic Cholera, in Regiments of the Madras Army, from 1828 to 1844; with Introductory Remarks on its Modes of Diffusion and Prevention, and Summary of the General Method of Treatment in India. By SAMUEL ROGERS, F.R.C.S., and Surgeon of the Madras Army. 8vo. 10s. 6d.

VI.

SIR J. ANNESLEY'S DISEASES OF INDIA.—*Second Edit.*

Sketches of the most Prevalent Diseases of India, comprising a Treatise on Epidemic Cholera in the East, &c. &c. By Sir JAMES ANNESLEY, late of the Madras Medical Establishment. *Second Edition*, 8vo. 18s.

Pamphlets.

ON TREATMENT of ASIATIC CHOLERA. By ARCHIBALD BILLING, M.D. 6d.

HOMŒOPATHY UNVEILED; or, Observations on Hahnemann, his Doctrines, and Treatment of Disease. By W. P. BRODRIBB, M.R.C.S.E. Second Edition. 1s.

CHEMICAL GAZETTE; or, Journal of Practical Chemistry in all its Applications to PHARMACY, ARTS, and MANUFACTURES. On 1st and 15th of each Month. Price 6d.; Stamped, 7d.

THE ADVANTAGES of ETHER and CHLOROFORM in OPERATIVE SURGERY: An Address delivered to the Hunterian Society on 9th February, 1848, by T. B. CURLING, F.R.C.S.E. 1s.

ON the USE of CHLOROFORM in DENTAL SURGERY. By J. C. CLENDON, M.R.C.S.E. 6d.

THE COMPANION to the MICROSCOPE. By C. GOULD, Esq. 2s. 6d.

ON the SUCCESSFUL TREATMENT of STRICTURES of the URETHRA supposed to be INTRACTABLE: A Lecture delivered before the Medical Society in London, April, 1851, by G. J. GUTHRIE, Esq. F.R.S. 1s. 6d.

ON EPILEPSY: In Answer to the Question—What is the Nature of the Internal Commotion which takes place during the Epileptic Paroxysm? By JOHN JACKSON, M.R.C.S.E. 6d.

A SKETCH of the LIFE and SOME ACCOUNT of the WRITINGS of the late DR. JAMES JOHNSON. By his Son, J. H. JOHNSON. 2s.

OBSERVATIONS on the TREATMENT of CLEFT PALATE by MECHANICAL MEANS. By JOHN MAPLESON, Dentist and Cupper to the Queen. 1s.

REPORT on QUARANTINE and the PLAGUE. By GAVIN MILROY, M.D. 2s.

ESSAY on ANÆSTHETIC AGENTS. By W. H. MORTIMER, Surgeon-Dentist. 1s.

STATISTICAL REPORT of the EPIDEMIC CHOLERA in JAMAICA. By JOHN PARKIN, M.D. 1s. 6d.

SCRIPTURE AUTHORITY for the MITIGATION of the PAINS of LABOUR by CHLOROFORM and other ANÆSTHETIC AGENTS. By PROTHEROE SMITH, M.D. 1s.

ESSAYS on MENTAL DERANGEMENT. By JAMES VEITCH, M.D. 2s.

PAST and PRESENT STATE of INTRAMURAL BURYING-PLACES, with Practical Suggestions, &c. By GEO. A. WALKER, Surgeon. 1s.

Wilson and Ogilvy, 57, Skinner Street, Snowhill, London.