sliding vertical door containing a hole about 2½ inches in diameter, which was accurately closed with a disk of coloured glass. In all cases the deposits were furthest from the light—and naturally so, seeing that a coloured object absorbs the heat more readily than a white one, and keeps the side of the bottle nearest to it of a higher temperature than the other parts.

One more point remains to be noticed. When mercury was exposed to the light in a tall narrow glass, no reliable results were obtained, that is, no deposit was formed that appeared to arise from the condensation of vapour. On two or three occasions metallic tears were seen in the vessel, but it was never clear to me that they did not arise from some shaking or disturbanee of the vessel. I could not reproduce even this unsatisfactory result in a narrow vessel, though I carefully tried for it by furnishing the vessel with a cap and stopcock and exhausting it with a syringe. I was also further surprised to find that a barometer-tube of thick glass charged with camphor and exhausted, produced little or no deposit even on the warmest days, and by exposure to direct sunshine. No sooner, however, had I dismissed the action of light from this subject, than the whole matter became clear. A thick glass tube by exposure to the light does not cool unequally, but slowly varies in temperature throughout its mass, so that no deposit either of mercury or of camphor is possible. If, however, the tube be thin, of large diameter and mounted, so that while one part is exposed to radiation the other part is protected, partial cooling is possible, and a deposit is produced. This, too, furnishes an explanation of a fact that had often surprised me. In barometers of large bore there is a deposit of mercury in the Torricellian vacuum on the side nearest the light. I had never seen this in a tube of small bore, though I had frequently looked for it in my own instrument. Some of the barometers of large bore in the International Exhibition have very fine deposits of mercury vapour in the Torricellian vacuum, but in such cases they are mounted so that the tube is more or less exposed. Where the tube is boxed in and protected from radiation there is little or no deposit.

King's College, London,
Long Vacation, 1862.

XLVIII. *Remarks on the Forces of Inorganic Nature.* *By* J. R. Mayer*.

THE following pages are designed as an attempt to answer the questions, What are we to understand by "Forces"?

* Translated from the *Annalen der Chemie und Pharmacie*, vol. xlii. p. 233 (May 1842), by G. C. Foster, B.A., Lecturer on Natural Philosophy

and how are different forces related to each other? Whereas the term *matter* implies the possession, by the object to which it is applied, of very definite properties, such as weight and extension; the term *force* conveys for the most part the idea of something unknown, unsearchable, and hypothetical. An attempt to render the notion of force equally exact with that of matter, and so to denote by it only objects of actual investigation, is one which, with the consequences that flow from it, ought not to be unwelcome to those who desire that their views of nature may be clear and unencumbered by hypotheses.

Forces are causes: accordingly, we may in relation to them make full application of the principle—*causa æquat effectum.* If the cause c has the effect e, then $c=e$; if, in its turn, e is the cause of a second effect f, we have $e=f$, and so on: $c=e=f\ldots=c$. In a chain of causes and effects, a term or a part of a term can never, as plainly appears from the nature of an equation, become equal to nothing. This first property of all causes we call their *indestructibility.*

If the given cause c has produced an effect e equal to itself, it has in that very act ceased to be: c has become e; if, after the production of e, c still remained in whole or in part, there must be still further effects corresponding to this remaining cause: the total effect of c would thus be $> e$, which would be contrary to the supposition $c=e$. Accordingly, since c becomes e, and e becomes f, &c., we must regard these various magnitudes as different forms under which one and the same object makes its appearance. This capability of assuming various forms is the second essential property of all causes. Taking both properties together, we may say, causes are (quantitatively) *indestructible* and (qualitatively) *convertible* objects.

Two classes of causes occur in nature, which, so far as experience goes, never pass one into another. The first class consists of such causes as possess the properties of weight and impenetrability; these are kinds of Matter: the other class is made up of causes which are wanting in the properties just mentioned, namely Forces, called also Imponderables, from the negative property that has been indicated. Forces are therefore *indestructible, convertible, imponderable objects.*

in Anderson's University, Glasgow.—Considerable attention having of late been called to the author of this paper, as one of the earliest propounders of the doctrine of the Indestructibility of Force, and especially of the idea of the equivalence of Heat and Work, it will probably interest many readers of the Philosophical Magazine to have placed in their hands his earliest publication on the subject. For some account of Mayer and of his further labours, see Prof. Tyndall's lecture "On Force," Phil. Mag. S. 4. vol. xxiv. pp. 64–66.

We will in the first instance take matter, to afford us an example of causes and effects. Explosive gas, $H+O$, and water, HO, are related to each other as cause and effect, therefore $H+O=HO$. But if $H+O$ becomes HO, heat, *cal.*, makes its appearance as well as water; this heat must likewise have a cause, x, and we have therefore $H+O+x=HO+cal.$ It might, however, be asked whether $H+O$ is really $=HO$, and $x=cal.$, and not perhaps $H+O=cal.$, and $x=HO$, whence the above equation could equally be deduced; and so in many other cases. The phlogistic chemists recognized the equation between *cal.* and x, or Phlogiston as they called it, and in so doing made a great step in advance; but they involved themselves again in a system of mistakes by putting $-x$ in place of O; thus, for instance, they obtained $H=HO+x$.

Chemistry, whose problem it is to set forth in equations the causal connexion existing between the different kinds of matter, teaches us that matter, as a cause, has matter for its effect; but we are equally justified in saying that to force as cause, corresponds force as effect. Since $c=e$, and $e=c$, it is unnatural to call one term of an equation a force, and the other an effect of force or phenomenon, and to attach different notions to the expressions Force and Phenomenon. In brief, then, if the cause is matter, the effect is matter; if the cause is a force, the effect is also a force.

A cause which brings about the raising of a weight is a force; its effect (*the raised weight*) is, accordingly, equally *a force*; or, expressing this relation in a more general form, *separation in space of ponderable objects is a force*; since this force causes the fall of bodies, we call it *falling force*. Falling force and fall, or, more generally still, falling force and motion, are forces which are related to each other as cause and effect—forces which are convertible one into the other—two different forms of one and the same object. For example, a weight resting on the ground is not a force: it is neither the cause of motion, nor of the lifting of another weight; it becomes so, however, in proportion as it is raised above the ground: the cause—the distance between a weight and the earth—and the effect—the quantity of motion produced—bear to each other, as we learn from mechanics, a constant relation.

Gravity being regarded as the cause of the falling of bodies, a gravitating force is spoken of, and so the notions of *property* and of *force* are confounded with each other: precisely that which is the essential attribute of every force—the *union* of indestructibility with convertibility—is wanting in every property: between a property and a force, between gravity and motion, it is therefore impossible to establish the equation required for a rightly conceived causal relation. If gravity be called a force, a cause is supposed which produces effects without itself diminishing, and incorrect conceptions of the causal connexion of things are thereby fostered. In order that a body may fall, it is no less necessary that it should be lifted up, than that it should be heavy or possess gravity; the fall of bodies ought not therefore to be ascribed to their gravity alone.

It is the problem of Mechanics to develope the equations which subsist between falling force and motion, motion and falling force, and between different motions: here we will call to mind only one point. The magnitude of the falling force v is directly proportional (the earth's radius being assumed $=\infty$) to the magnitude of the mass m, and the height d to which it is raised; that is, $v=md$. If the height $d=1$, to which the mass m is raised, is transformed into the final velocity $c=1$ of this mass, we have also $v=mc$; but from the known relations existing between d and c, it results that, for other values of d or of c, the measure of the force v is mc^2; accordingly $v=md=mc^2$: the law of the conservation of *vis viva* is thus found to be based on the general law of the indestructibility of causes.

In numberless cases we see motion cease without having caused another motion or the lifting of a weight; but a force once in existence cannot be annihilated, it can only change its form; and the question therefore arises, What other forms is force, which we have become acquainted with as falling force and motion, capable of assuming? Experience alone can lead us to a conclusion on this point. In order to experiment with advantage, we must select implements which, besides causing a real cessation of motion, are as little as possible altered by the objects to be examined. If, for example, we rub together two metal plates, we see motion disappear, and heat, on the other hand, make its appearance, and we have now only to ask whether *motion* is the cause of heat. In order to come to a decision on this point, we must discuss the question whether, in the numberless cases in which the expenditure of motion is accompanied by the appearance of heat, the motion has not some other effect than the production of heat, and the heat some other cause than the motion.

An attempt to ascertain the effects of ceasing motion has never yet been seriously made; without, therefore, wishing to exclude *à priori* the hypotheses which it may be possible to set up, we observe only that, as a rule, this effect cannot be supposed to be an alteration in the state of aggregation of the moved (that is, rubbing, &c.) bodies. If we assume that a certain quantity of motion v is expended in the conversion of a rubbing substance m into n, we must then have $m+v=n$, and $n=m+v$; and when n is reconverted into m, v must appear again in some form or other. By the friction of two metallic plates continued for a very long time, we can gradually cause the cessation of an immense quantity of movement; but would it ever occur to us to look for even the smallest trace of the force which has disappeared in the metallic dust that we could collect, and to try to regain it thence? We repeat, the motion cannot have been annihilated; and contrary, or positive and negative, motions cannot be regarded as $=0$, any more than contrary motions can come out of nothing, or a weight can raise itself.

Without the recognition of a causal connexion between motion and heat, it is just as difficult to explain the production of heat as it is to give any account of the motion that disappears. The heat cannot be derived from the diminution of the volume of the rubbing substances. It is well known that two pieces of ice may be melted by rubbing them together *in vacuo*; but let any one try to convert ice into water by pressure*, however enormous. Water undergoes, as was found by the author, a rise of temperature when violently shaken. The water so heated (from 12° to 13° C.) has a greater bulk after being shaken than it had before; whence now comes this quantity of heat, which by repeated shaking may be called into existence in the same apparatus as often as we please? The vibratory hypothesis of heat is an approach towards the doctrine of heat being the effect of motion, but it does not favour the admission of this causal relation in its full generality; it rather lays the chief stress on uneasy oscillations (*unbehagliche Schwingungen*).

If it be now considered as established that in many cases (*exceptio confirmat regulam*) no other effect of motion can be traced except heat, and that no other cause than motion can be found for the heat that is produced, we prefer the assumption that heat proceeds from motion, to the assumption of a cause without effect and of an effect without a cause,—just as the chemist, instead of allowing oxygen and hydrogen to disappear without further investigation, and water to be produced in some inexplicable manner, establishes a connexion between oxygen and hydrogen on the one hand and water on the other.

The natural connexion existing between falling force, motion, and heat may be conceived of as follows. We know that heat makes its appearance when the separate particles of a body approach nearer to each other: condensation produces heat.

* Since the original publication of this paper, Prof. W. Thomson has shown that pressure has a sensible effect in liquefying ice (*Conf.* Phil. Mag. S. 3. vol. xxxvii. p. 123); but the experiments of Bunsen and of Hopkins have shown that the melting-points of bodies which expand on becoming liquid are raised by pressure, which is all that Mayer's argument requires.—G. C. F.

And what applies to the smallest particles of matter, and the smallest intervals between them, must also apply to large masses and to measureable distances. The falling of a weight is a real diminution of the bulk of the earth, and must therefore without doubt be related to the quantity of heat thereby developed; this quantity of heat must be proportional to the greatness of the weight and its distance from the ground. From this point of view we are very easily led to the equations between falling force, motion, and heat, that have already been discussed.

But just as little as the connexion between falling force and motion authorizes the conclusion that the essence of falling force is motion, can such a conclusion be adopted in the case of heat. We are, on the contrary, rather inclined to infer that, before it can become heat, motion—whether simple, or vibratory as in the case of light and radiant heat, &c.—must cease to exist as motion.

If falling force and motion are equivalent to heat, heat must also naturally be equivalent to motion and falling force. Just as heat appears as an *effect* of the diminution of bulk and of the cessation of motion, so also does heat disappear as a *cause* when its effects are produced in the shape of motion, expansion, or raising of weight.

In water-mills, the continual diminution in bulk which the earth undergoes, owing to the fall of the water, gives rise to motion, which afterwards disappears again, calling forth unceasingly a great quantity of heat; and inversely, the steam-engine serves to decompose heat again into motion or the raising of weights. A locomotive engine with its train may be compared to a distilling apparatus; the heat applied under the boiler passes off as motion, and this is deposited again as heat at the axles of the wheels.

We will close our disquisition, the propositions of which have resulted as necessary consequences from the principle "causa æquat effectum," and which are in accordance with all the phenomena of Nature, with a practical deduction. The solution of the equations subsisting between falling force and motion requires that the space fallen through in a given time, *e. g.* the first second, should be experimentally determined; in like manner, the solution of the equations subsisting between falling force and motion on the one hand and heat on the other, requires an answer to the question, How great is the quantity of heat which corresponds to a given quantity of motion or falling force? For instance, we must ascertain how high a given weight requires to be raised above the ground in order that its falling force may be equivalent to the raising of the temperature of an equal weight of water from 0° to 1° C. The attempt to show that such an equation is the expression of a physical truth may be regarded as the substance of the foregoing remarks.

By applying the principles that have been set forth to the relations subsisting between the temperature and the volume of gases, we find that the sinking of a mercury column by which a gas is compressed is equivalent to the quantity of heat set free by the compression; and hence it follows, the ratio between the capacity for heat of air under constant pressure and its capacity under constant volume being taken as =1·421, that the warming of a given weight of water from 0° to 1° C. corresponds to the fall of an equal weight from the height of about 365 metres*. If we compare with this result the working of our best steam-engines, we see how small a part only of the heat applied under the boiler is really transformed into motion or the raising of weights; and this may serve as justification for the attempts at the profitable production of motion by some other method than the expenditure of the chemical difference between carbon and oxygen—more particularly by the transformation into motion of electricity obtained by chemical means.

XLIX. *The Excavation of the Valleys of the Alps.* *By* A. C. Ramsay, *F.R.S.*†

IN the month of March last I read a memoir to the Geological Society on the Glacial Origin of the Swiss and other Lakes, which has since been published in that Society's Quarterly Journal for August. In that memoir I incidentally alluded (p. 200) to the existence of the chief Alpine valleys *before* the glaciers attained their greatest extension, which valleys were afterwards "*modified* in form by the weight and grinding power of ice in motion."

In a previous memoir, published in 1859, I stated that "it is certain all glaciers must deepen their beds by erosion, and it may be that, when a glacier filled a valley" almost to the brim, "the thickness of the ice was not equal to the present mass added to the superincumbent weight indicated by the signs (striation, &c.) on the slopes above the present surface of the glacier." But though glaciers certainly have a powerful effect in deepening their beds, it has always appeared to me a difficult and perhaps an impossible point to determine to what extent the great Alpine valleys have been eroded by ice—whether, in fact, they have been chiefly scooped out by it, or whether, as I always believed,

* When the corrected specific heat of air is introduced into the calculation this number is increased, and agrees then with the experimental determinations of Mr. Joule.

† Communicated by the Author.