ON THE PRINCIPLE OF THE CONSERVATION OF ENERGY.[1]

IN a popular lecture, distinguished for its charming simplicity and clearness, which Joule delivered in the year 1847,[2] that famous physicist declares that the living force which a heavy body has acquired by its descent through a certain height and which it carries with it in the form of the velocity with which it is impressed, is the *equivalent* of the attraction of gravity through the space fallen through, and that it would be "absurd" to assume that this living force could be destroyed without some restitution of that equivalent. He then adds: "You will therefore be surprised to hear that until very *recently* the universal opinion has been that living force could be absolutely and irrevocably destroyed at any one's option." Let us add that to-day, after forty-seven years, the *law of the conservation of energy*, wherever civilisation reaches, is accepted as a fully established truth and receives the widest applications in all domains of natural science.

The fate of all momentous discoveries is similar. On their first appearance they are regarded by the majority of men as errors. J. R. Mayer's work on the principle of energy (1842) was rejected by the first physical journal of Germany; Helmholtz's treatise (1847) met with no better success; and even Joule, to judge from an intimation of Playfair, seems to have encountered difficulties with his

[1] Translated from Professor Mach's manuscript by Thomas J. McCormack.

[2] *On Matter, Living Force, and Heat*, Joule: *Scientific Papers*, London, 1884 I, p. 265.

first publication (1843). Gradually, however, people are led to see that the new view was long prepared for and ready for enunciation, only that a few favored minds had perceived it much earlier than the rest, and in this way the opposition of the majority is overcome. With proofs of the fruitfulness of the new view, with its success, confidence in it increases. The majority of the men who employ it cannot enter into a deep-going analysis of it; for them, its success is its proof. It can thus happen that a view which has led to the greatest discoveries, like Black's theory of caloric, in a subsequent period in a province where it does not apply may actually become an obstacle to progress by its blinding our eyes to facts which do not fit in with our favorite conceptions. If a theory is to be protected from this dubious rôle, the grounds and motives of its evolution and existence must be examined from time to time with the greatest care.

The most multifarious physical changes, thermal, electrical, chemical, and so forth, may be brought about by mechanical work. When such alterations are reversed they yield anew the mechanical work in exactly the quantity which was required for the production of the part reversed. This is the *principle of the conservation of energy*; "energy" being the term which has gradually come into use for that "indestructible something" of which the measure is mechanical *work*.

How did we acquire this idea? What are the sources from which we have drawn it? This question is not only of interest in itself, but also for the important reason above touched upon. The opinions which are held concerning the foundations of the law of energy still diverge very widely from one another. Many trace the principle to the impossibility of a perpetual motion, which they regard either as sufficiently proved by experience, or as self-evident. In the province of pure mechanics the impossibility of a perpetual motion, or the continuous production of *work* without some *permanent* alteration, is easily demonstrated. Accordingly, if we start from the theory that all physical processes are purely *mechanical* processes, motions of molecules and atoms, we embrace also, by this *mechanical* conception of physics, the impossibility of a perpetual motion in the *whole* physical domain. At present this view probably counts the most adherents. Other inquirers, however, are for accepting only a purely *experimental* establishment of the law of energy.

It will appear, from the discussion to follow, that *all* the factors mentioned have co-operated in the development of the view in question; but that in addition to them a logical and purely formal factor, hitherto little considered, has also played a very important part.

I. THE PRINCIPLE OF THE EXCLUDED PERPETUAL MOTION.

The law of energy in its modern form is not identical with the principle of the excluded perpetual motion, but it is very closely related to it. The latter principle, however, is by no means new, for in the province of mechanics it has controlled for centuries the thoughts and investigations of the greatest thinkers. Let us convince ourselves of this by the study of a few historical examples.

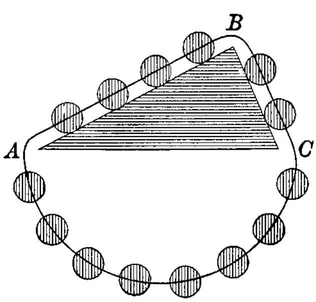

Fig. 1.

S. Stevinus, in his famous work *Hypomnemata mathematica*, Tom. IV, *De statica*, (Leyden, 1605, p. 34), treats of the equilibrium of bodies on inclined planes.

Over a triangular prism $A B C$, one side of which, $A B$, is horizontal, an endless cord or chain is slung, to which at equal distances apart fourteen balls of equal weight are attached, as represented in cross-section in Figure 1. Since we can imagine the lower symmetrical part of the cord $A B C$ taken away, Stevinus concludes that the four balls on $A B$ hold in equilibrium the two balls on $B C$. For if the equilibrium were for a moment disturbed, it could never subsist: the cord would keep moving round forever in the same direction,—we should have a perpetual motion. He says:

"But if this took place, our row or ring of balls would come once more into their original position, and from the same cause the eight globes to the left would again be heavier than the six to the right, and therefore those eight would sink a second time and these six rise, and all the globes would keep up, of themselves, *a continuous and unending motion, which is false.*"[1]

Stevinus, now, easily derives from this principle the laws governing equilibrium on the inclined plane and numerous other fruitful consequences.

In the chapter "Hydrostatics" of the same work, page 114, Stevinus sets up the following principle: "Aquam datam, datum sibi intra aquam locum servare,"—a given mass of water preserves within water its given place. This principle is demonstrated as follows (see Fig. 2):

"For, assuming it to be possible by natural means, let us suppose that *A* does not preserve the place assigned to it, but sinks down to *D*. This being posited, the water which succeeds *A* will, for the same reason, also flow down to *D*; *A* will be forced out of its place in *D*; and thus this body of water, for the conditions in it are everywhere the same, *will set up a perpetual motion, which is absurd.*"[2]

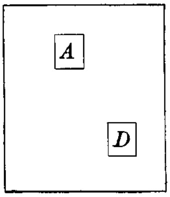

Fig. 2.

From this all the principles of hydrostatics are deduced. On this occasion Stevinus also first develops the thought so fruitful for modern analytical mechanics that the equilibrium of a system is not destroyed by the addition of rigid connexions. As we know, the principle of the conservation of the centre of gravity is now sometimes deduced from D'Alembert's principle with the help of that remark. If we were to reproduce Stevinus's demonstration to-day, we should have to change it slightly. We find no difficulty in imagining the cord on the prism possessed of unending uniform motion if all hindrances are thought away, but we should protest against the assumption of an accelerated motion or even against that of a uniform motion, if the resistances were not removed. Moreover, for greater precision of proof, the string of balls might be replaced

[1] "Atqui hoc si sit, globorum series sive corona eundem situm cum priore habebit, eademque de causa octo globi sinistri ponderosiores erunt sex dextris, ideoque rursus octo illi descendent, sex illi ascendent, istique globi ex sese *continuum et aeternum motum efficient, quod est falsum.*"

[2] "*A* igitur, (si ullo modo per naturam fieri possit) locum sibi tributum non servato, ac delabatur in *D*; quibus positis aqua quae ipsi *A* succedi teandem ob causam deffluet in *D*, eademque ab alia istinc expelletur, atque adeo aqua haec (cum ubique eadem ratio sit) *motum instituet perpetuum, quod absurdum fuerit.*"

by a heavy homogeneous cord of infinite flexibility. But all this does not affect in the least the historical value of Stevinus's thoughts. It is a fact, Stevinus deduces apparently much simpler truths from the principle of an impossible perpetual motion.

In the process of thought which conducted Galileo to his discoveries at the end of the sixteenth century, the following principle plays an important part, that a body in virtue of the velocity acquired in its descent can rise exactly as high as it fell. This principle, which appears frequently and with much clearness in Galileo's thought, is simply another form of the principle of excluded perpetual motion, as we shall see it is also in Huygens.

Galileo, as we know, arrived at the law of uniformly accelerated motion by *a priori* considerations, as that law which was the "simplest and most natural," after having first assumed a different law which he was compelled to reject. To verify his law he executed experiments with falling bodies on inclined planes, measuring the times of descent by the weights of the water which flowed out of a small orifice in a large vessel. In this experiment he assumes as a fundamental principle, that the velocity acquired in descent down an inclined plane always corresponds to the vertical height descended through, a conclusion which for him is the immediate outcome of the fact that a body which has fallen down one inclined plane can, with the velocity it has acquired, rise on another plane of any inclination only to the same vertical height. This principle of the height of ascent also led him, as it seems, to the law of inertia. Let us hear his own masterful words in the *Dialogo terzo* (*Opere* Padova, 1744, Tom. III). On page 96 we read:

"I take it for granted that the velocities acquired by a body in descent down planes of different inclinations are equal if the heights of those planes are equal."[1]

Then he makes Salviati say in the dialogue:[2]

"What you say seems very probable, but I wish to go further and by an experiment so to increase the probability of it that it shall amount almost to absolute demon-

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[1] "Accipio, gradus velocitatis ejusdem mobilis super diversas planorum inclinationes acquisitos tunc esse aequales, cum eorundem planorum elevationes aequales sint."

[2] "Voi molto probabilmente discorrete, ma oltre al veri simile voglio con una

stration. Suppose this sheet of paper to be a vertical wall, and from a nail driven in it a ball of lead weighing two or three ounces to hang by a very fine thread *AB* four or five feet long. (Fig. 3.) On the wall mark a horizontal line *DC* perpendicular to the vertical *AB*, which latter ought to hang about two inches from the wall. If now the thread *AB* with the ball attached take the position *AC* and the ball be let go, you will see the ball first descend through the arc *CBD* and passing beyond *B* rise through the arc *BD* almost to the level of the line *CD*, being prevented from reaching it exactly by the resistance of the air and of the thread. From this we may truly conclude that its impetus at the point *B*, acquired by

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esperienza crescer tanto la probabilità, che poco gli manchi all'agguagliarsi ad una ben necessaria dimostrazione. Figuratevi questo foglio essere una parete eretta al orizzonte, e da un chiodo fitto in essa pendere una palla di piombo d'un'oncia, o due, sospesa dal sottil filo *A B* lungo due, o tre braccia perpendicolare all' orrizonte, e nella parete segnate una linea orrizontale *D C* segante a squadra il perpendicolo *A B*, il quale sia lontano dalla parete due dita in circa, trasferendo poi il filo *A B* colla palla in *A C*, lasciata essa palla in libertà, la quale primier amente vedrete scendere descrivendo l'arco *C B D*, e di tanto trapassare il termine *B*, che scorrendo per l'arco *B D* sormonterà fino quasi alla segnata parallela *C D*, restando di per vernirvi per piccolissimo intervallo, toltogli il precisamente arrivarvi dall' impedimento dell'aria, e del filo. Dal che possiamo veracemente concludere, che l'impeto acquistato nel punto *B* dalla palla nello scendere per l'arco *C B*, fu tanto, che bastò a risospingersi per un simile arco *B D* alla medesima altezza ; fatta, e più volte reiterata cotale esperienza, voglio, che fiechiamo nella parete rasente al perpendicolo *A B* un chiodo come in *E*, ovvero in *F*, che sporga in fuori cinque, o sei dita, e questo acciocchè il filo *A C* tornando come prima a riportar la palla *C* per l'arco *C B*, giunta che ella sia in *B*, inoppando il filo nel chiodo *E*, sia costretta a camminare per la circonferenza *B G* descritta in torno al centro *E*, dal che vedremo quello, che potrà far quel medesimo impeto, che dianzi concepizo nel medesimo termine *B*, sospinse l'istesso mobile per l'arco *E D* all'altezza dell'orizzonale *C D*. Ora, Signori, voi vedrete con gusto condursi la palla all'orizzontale nel punto *G*, e l'istesso accadere, l'intoppo si metesse più basso, come in *F*, dove la palla descriverebbe l'arco *B J*, terminando sempre la sua salita precisamente nella linea *C D*, e quando l'intoppe del chiodo fusse tanto basso, che l'avanzo del filo sotto di lui non arivasse all' altezza di *C D* (il che accaderebbe, quando fusse più vicino al punto *B*, che al segamento dell' *A B* coll'orizzontale *C D*), allora il filo cavalcherebbe il chiodo, e segli avolgerebbe intorno. Questa esperienza non lascia luogo di dubitare della verità del supposto : imperocchè essendo li due archi *CB*, *D B* equali e simil-mento posti, l'acquisto di momento fatto per la scesa nell'arco *CB*, è il medesimo, che il fatto per la scesa dell'arco *D B* ; ma il momento acquistato in *B* per l'arco *C B* è potente a risospingere in su il medesimo mobile per l'arco *B D* ; adunque anco il momento acquistato nella scesa *DB* è eguale a quello, che sospigne l'istesso mobile pel medesimo arco da *B* in *D*, sicche universalmente ogni momento acquistato per la scesa dun arco è eguale a quello, che può far risalire l'istesso mobile pel medesimo arco : ma i momenti tutti che fanno risalire per tutti gli archi *B D*, *B G*, *B J* sono eguali, poichè son fatti dal istesso medesimo momento acquistato per la scesa *CB*, come mostra l'esperienza : adunque tutti i momenti, che si acquistano per le scese negli archi *D B*, *G B*. *J B* sono eguali."

its descent through the arc CB, is sufficient to urge it through a similar arc BD to the same height. Having performed this experiment and repeated it several times, let us drive in the wall, in the projection of the vertical AB, as at E or at F, a nail five or six inches long, so that the thread AC, carrying as before the ball through the arc CB, at the moment it reaches the position AB, shall strike the nail E, and the ball be thus compelled to move up the arc BG described about E as centre. Then we shall see what the same impetus will here accomplish, acquired now as before at the same point B, which then drove the same moving body through the arc BD to the height of the horizontal CD. Now gentlemen, you will be pleased to see the ball rise to the horizontal line at the point G, and the same thing also happen if the nail be placed lower as at F, in which case the ball would describe the arc BJ, always terminating its ascent precisely at the line CD. If the nail be placed so low that the length of thread below it does not reach the height of CD (which would happen if F were nearer B than to the intersection of AB with the horizontal CD), then the thread will wind itself about the nail. This experiment leaves no room for doubt as to the truth of the supposition. For as the two arcs CB, DB are equal and similarly situated, the momentum acquired in the descent of the arc CB is the same as that acquired in the descent of the arc DB; but the momentum acquired at B by the descent through the arc CB is capable of driving up the same moving body through the arc BD; hence also the momentum acquired in the descent DB is equal to that which drives the same moving body through the same arc from B to D, so that in general every momentum acquired in the descent of an arc is equal to that which causes the same moving body to ascend through the same arc; but all the momenta which cause the ascent of all the arcs BD, BG, BJ, are equal since they are made by the same momentum acquired in the descent CB, as the experiment shows: therefore all the momenta acquired in the descent of the arcs DB, GB, JB are equal."

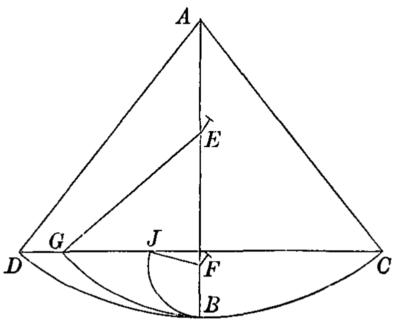

Fig. 3.

The remark relative to the pendulum may be applied to the inclined plane and leads to the law of inertia. We read on page 124:[1]

"It is plain now that a movable body, starting from rest at A and descending down the inclined plane AB, acquires a velocity proportional to the increment of

[1] "Constat jam, quod mobile ex quiete in A descendens per AB, gradus acquirit velocitatis juxta temporis ipsius incrementum: gradum vero in B esse maximum acquisitorum, et suapte natura immutabiliter impressum, sublatis scilicet causis

its time: the velocity possessed at B is the greatest of the velocities acquired, and by its nature immutably impressed, provided all causes of new acceleration or retardation are taken away: I say acceleration, having in view its possible further progress along the plane extended; retardation, in view of the possibility of its being reversed and made to mount the ascending plane

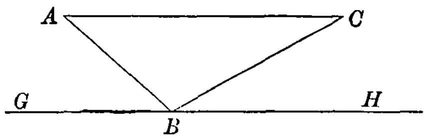

Fig. 4.

BC. But in the horizontal plane GH its equable motion, according to its velocity as acquired in the descent from A to B, will be continued *ad infinitum*."

Huygens, in every respect the lineal successor of Galileo, forms a sharper conception of the law of inertia and generalises the principle respecting the heights of ascent which was so fruitful in Galileo's hands. He employs the latter principle in the solution of the problem of the centre of oscillation and is perfectly clear in the statement that the principle respecting the heights of ascent is identical with the principle of the excluded perpetual motion.

The following important passages then occur, (Hugenii, *Horologium oscillatorium, pars secunda*). *Hypotheses*:

"If gravity did not exist, nor the atmosphere obstruct the motions of bodies, a body would keep up forever the motion once impressed upon it, with equable velocity, in a straight line."[1]

In part fourth of the *Horologium de centro oscillationis* we read:

"If any number of weights be set in motion by the force of their gravity, the common centre of gravity of the weights as a whole cannot possibly rise higher than the place which it occupied when the motion began.

"That this hypothesis of ours may arouse no scruples, we will state that it simply imports, what no one has ever denied, that heavy bodies do not move *upwards*.—And truly if the devisers of the new machines who make such futile attempts to construct a perpetual motion would acquaint themselves with this princi-

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accelerationis novae, aut retardationis: accelerationis inquam, si adhuc super extenso plano ulterius progrederetur; retardationis vero, dum super planum acclive BC fit reflexio: in horizontali autem GH aequabilis motus juxta gradum velocitatis ex A in B acquisitae in infinitum extenderetur.

[1] "Si gravitas non esset, neque aër motui corporum officeret, unumquodque eorum, acceptum semel motum continuaturum velocitate aequabili, secundum lineam rectam."

ple, they could easily be brought to see their errors and to understand that the thing is utterly impossible by mechanical means."[1]

There is possibly a Jesuitical mental reservation contained in the words "mechanical means." One might be led to believe from them that Huygens held a non-mechanical perpetual motion for possible.

The generalisation of Galileo's principle is still more clearly put in Proposition IV of the same chapter:

"If a pendulum, composed of several weights, set in motion from rest, complete any part of its full oscillation, and from that point onwards, the individual weights, with their common connexions dissolved, change their acquired velocities upwards and ascend as far as they can, the common centre of gravity of all will be carried up to the same altitude that it occupied before the beginning of the oscillation."[2]

On this last principle now, which is a generalisation, applied to a system of masses, of one of Galileo's ideas respecting a single mass and which from Huygens's explanation we recognise as the principle of excluded perpetual motion, Huygens grounds his theory of the centre of oscillation. Lagrange characterises this principle as precarious and is rejoiced at James Bernoulli's successful attempt, in 1681, to reduce the theory of the centre of oscillation to the laws of the lever, which appeared to him clearer. All the great inquirers of the seventeenth and eighteenth centuries broke a lance on this problem and it led ultimately, in conjunction with the principle of virtual velocities, to the principle enunciated by D'Alembert in 1743

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[1] "Si pondera quotlibet, vi gravitatis suae, moveri incipiant; non posse centrum gravitatis ex ipsis compositae altius, quam ubi incipiente motu reperiebatur, ascendere.

"Ipsa vero hypothesis nostra quominus scrupulum moveat, nihil aliud sibi velle ostendemus, quam, quod nemo unquam negavit, gravia nempe sursum non ferri.—Et sane, si hac eadem uti scirent novorum operum machinatores, qui motum perpetuum irrito conatu moliuntur, facile suos ipsi errores deprehenderent, intelligerentque rem eam mechanica ratione haud quaquam possibilem esse."

[2] "Si pendulum e pluribus ponderibus compositum, atque e quiete dimissum, partem quamcunque oscillationis integrae confecerit, atque inde porro intelligantur pondera ejus singula, relicto communi vinculo, celeritates acquisitas sursum convertere, ac quousque possunt ascendere; hoc facto centrum gravitatis ex omnibus compositae, ad eandem altitudinem reversum erit, quam ante inceptam oscillationem obtinebat."

in his *Traité de dynamique*, though previously employed in a somewhat different form by Euler and Hermann.

Besides this, the Huygenian principle respecting the heights of ascent became the foundation of the "law of the conservation of living force," as it was enunciated by John and Daniel Bernoulli and employed with such signal success by the latter in his *Hydrodynamics*. The theorems of the Bernoullis differ only in form from Lagrange's expression in the *Analytical Mechanics*.

The manner in which Torricelli reached his famous law of efflux for liquids leads again to our principle. Torricelli assumed that the liquid which flows out of the basal orifice of a vessel cannot by its velocity of efflux ascend to a greater height than its level in the vessel.

Let us next consider a point which belongs to pure mechanics, the history of the principle of *virtual motions* or *virtual velocities*. This principle was not first enunciated, as is usually stated, and as Lagrange also asserts, by Galileo, but earlier, by Stevinus. In his *Trochleostatica* of the above-cited work, page 72, he says:

"Observe that this axiom of statics holds good here:

"As the space of the body acting is to the space of the body acted upon, so is the power of the body acted upon to the power of the body acting."[1]

Galileo, as we know, recognised the truth of the principle in the consideration of the simple machines, and also deduced the laws of the equilibrium of liquids from it.

Torricelli carries the principle back to the properties of the centre of gravity. The condition controlling equilibrium in a simple machine, in which power and load are represented by weights, is that the common centre of gravity of the weights shall not sink. Conversely, if the centre of gravity cannot sink equilibrium obtains, because heavy bodies of themselves do not move upwards. In this form the principle of virtual velocities is identical with Huygens's principle of the impossibility of a perpetual motion.

John Bernoulli, in 1717, first perceived the general significance

[1] "Notato autem hic illud staticum axioma etiam locum habere:
"Ut spatium agentis ad spatium patientis
Sic potentia patientis ad potentiam agentis."

of the principle of virtual movements for all systems; a discovery stated in a letter to Varignon. Finally, Lagrange gives a general demonstration of the principle and founds upon it his whole *Analytical Mechanics.* But this general demonstration is based after all upon Huygens and Torricelli's remarks. Lagrange, as is known, conceives simple pulleys arranged in the directions of the forces of the system, passes a cord through these pulleys, and appends to its free extremity a weight which is a common measure of all the forces of the system. With no difficulty, now, the number of elements of each pulley may be so chosen that the forces in question shall be replaced by them. It is then clear that if the weight at the extremity cannot sink, equilibrium subsists, because heavy bodies cannot of themselves move upwards. If we do not go so far, but wish to abide by Torricelli's idea, we may conceive every individual force of the system replaced by a special weight suspended from a cord passing over a pulley in the direction of the force and attached at its point of application. Equilibrium subsists then when the common centre of gravity of all the weights together cannot sink. The fundamental supposition of this demonstration is plainly the impossibility of a perpetual motion.

Lagrange tried in every way to supply a proof free from extraneous elements and fully satisfactory, but without complete success. Nor were his successors more fortunate.

The whole of mechanics, thus, is based upon an idea which, though unequivocal, is yet unwonted and not coequal with the other principles and axioms of mechanics. Every student of mechanics, at some stage of his progress, feels the uncomfortableness of this state of affairs; every one wishes it removed; but seldom is the difficulty stated in words. Accordingly, the zealous pupil of the science is highly rejoiced when he reads in a master like Poinsot (*Théorie générale de l'équilibre et du mouvement des systèmes*) the following passage, in which that author is giving his opinion of the *Analytical Mechanics*:

"In the meantime, because our attention in that work was first wholly engrossed with the consideration of its beautiful development of mechanics, which seemed to spring complete from a single formula, we naturally believed that the science was completed or that it only remained to seek the demonstration of the principle of virtual velocities. But that quest brought back all the difficulties that we had overcome by the principle itself. That law so general, wherein are mingled the vague and unfamiliar ideas of infinitely small movements and of perturbations of equilibrium, only grew obscure upon examination; and the work of Lagrange supplying nothing clearer than the march of analysis, we saw plainly that the clouds had only appeared lifted from the course of mechanics because they had, so to speak, been gathered at the very origin of that science.

"At bottom, a general demonstration of the principle of virtual velocities would be equivalent to the establishment of the whole of mechanics upon a different basis: for the demonstration of a law which embraces a whole science is neither more nor less than the reduction of that science to another law just as general, but evident, or at least more simple than the first, and which, consequently, would render that useless."[1]

According to Poinsot, therefore, a proof of the principle of virtual movements is tantamount to a total rehabilitation of mechanics.

Another circumstance of discomfort to the mathematician is, that in the historical form in which mechanics at present exists, dynamics is founded on statics, whereas it is desirable that in a science which pretends to deductive completeness the more special statical theorems should be deducible from the more general dynamical principles.

In fact, a great master, Gauss, gave expression to this desire in his presentment of the principle of least constraint (Crelle's *Journal für reine und angewandte Mathematik*, Vol. IV, p. 233) in the follow-

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[1] "Cependant, comme dans cet ouvrage on ne fut d'abord attentif qu'à considérer ce beau développement de la mécanique qui semblait sortir tout entière d'une seule et même formule, on crut naturellement que la science etait faite, et qu'il ne restait plus qu'à chercher la démonstration du principe des vitesses virtuelles. Mais cette recherche ramena toutes les difficultés qu'on avait franchies par le principe même. Cette loi si générale, où se mêlent des idées vagues et étrangères de mouvements infinement petits et de perturbation d'équilibre, ne fit en quelque sorte que s'obsurcir à l'examen; et le livre de Lagrange n'offrant plus alors rien de clair que la marche des calculs, on vit bien que les nuages n'avaient paru levé sur le cours de la mécanique que parcequ'ils étaient, pour ainsi dire, rassemblés à l'origine même de cette science.

"Une démonstration générale du principe des vitesses virtuelles devait au fond revenir a établir le mécanique entière sur une autre base: car la demonstration d'une loi qui embrasse toute une science ne peut être autre chose que la reduction de cette science à une autre loi aussi générale, mais évidente, ou du moins plus simple que la première, et qui partant la rende inutile."

ing words: "Proper as it is that in the gradual development of a science, and in the instruction of individuals, the easy should precede the difficult, the simple the complex, the special the general, yet the mind, when once it has reached a higher point of view, demands the contrary course, in which all statics shall appear simply as a special case of mechanics." Gauss's own principle, now, possesses all the requisites of universality, but its difficulty is that it is not immediately intelligible and that Gauss deduced it with the help of D'Alembert's principle, a procedure which left matters where they were before.

Whence, now, is derived this strange part which the principle of virtual motion plays in mechanics? For the present I shall only make this reply. It would be difficult for me to tell the difference of impression which Lagrange's proof of the principle made on me when I first took it up as a student and when I subsequently resumed it after having made historical researches. It first appeared to me insipid, chiefly on account of the pulleys and the cords which did not fit in with the mathematical view, and whose action I would much rather have discovered from the principle itself than have taken for granted. But now that I have studied the history of the science I cannot imagine a more beautiful demonstration.

In fact, through all mechanics it is this self-same principle of excluded perpetual motion which accomplishes almost all, which displeased Lagrange, but which he had yet to employ, at least tacitly, in his own demonstration. If we give this principle its proper place and setting, the paradox is explained.

The principle of excluded perpetual motion is thus no new discovery; it has been the guiding idea, for three hundred years, of all the great inquirers. But the principle cannot properly be *based* upon mechanical perceptions. For long before the development of mechanics the conviction of its truth existed and even contributed to that development. Its power of conviction, therefore, must have more universal and deeper roots. We shall revert to this point.

II. MECHANICAL PHYSICS.

It cannot be denied, that an unmistakable tendency has prevailed, from Democritus to the present day, to explain *all* physical events *mechanically*. Not to mention earlier obscure expressions of that tendency we read in Huygens the following :[1]

"There can be no doubt that light consists of the *motion* of a certain substance. For if we examine its production, we find that here on earth it is principally fire and flame which engender it, both of which contain beyond doubt bodies which are in rapid movement, since they dissolve and destroy many other bodies more solid than they : while if we regard its effects, we see that when light is accumulated, say by concave mirrors, it has the property of combustion just as fire has, that is to say, it disunites the parts of bodies, which is assuredly a proof of *motion*, at least in the *true philosophy*, in which the causes of all natural effects are conceived as *mechanical* causes. Which in my judgment must be accomplished or all hope of ever understanding physics renounced."[2]

S. Carnot,[3] in introducing the principle of excluded perpetual motion into the theory of heat, makes the following apology :

"It will be objected here, perhaps, that a perpetual motion proved impossible for *purely mechanical actions*, is perhaps not so when the influence of *heat* or of electricity is employed. But can phenomena of heat or electricity be thought of as due to anything else than to *certain motions of bodies*, and as such must they not be subject to the general laws of mechanics?"[4]

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[1] *Traité de la lumière*, Leyden, 1690, p. 2.

[2] L'on ne sçaurait douter que la lumière ne consiste dans le *mouvement* de certaine matière. Car soit qu'on regarde sa production, on trouve qu'içy sur la terre c'est principalement le feu et la flamme qui l'engendrent, lesquels contient sans doute des corps qui sont dans un mouvement rapide, puis qu'ils dissolvent et fondent plusieurs autres corps des plus solides : soit qu'on regarde ses effets, on voit que quand la lumière est ramasseé, comme par des miroires concaves, elle a la vertu de brûler comme le feu, c-est-à-dire qu'elle desunit les parties des corps; ce qui marque assurément du *mouvement*, au moins dans la *vraye Philosophie*, dans laquelle on conçoit la cause de tous les effets naturels par des raisons de *mechanique*. Ce qu'il faut faire à mon avis, ou bien renoncer à toute espérance de jamais rien comprendre dans la Physique."

[3] *Sur la puissance motrice du feu.* (Paris, 1824.)

[4] "On objectra peut-être ici que le mouvement perpétuel, démontré impossible par les *seules actions mécaniques*, ne l'est peut-être pas lorsqu'on emploie l'influence soit de la *chaleur*, soit de l'électricité ; mais peut-on concevoir les phénomènes de

These examples, which might be multiplied by quotations from recent literature indefinitely, show that a tendency to explain all things mechanically actually exists. This tendency is also intelligible. Mechanical events as simple motions in space and time best admit of observation and pursuit by the help of our highly organised senses. We reproduce mechanical processes almost without effort in our imagination. Pressure as a circumstance that produces motion is very familiar to us from daily experience. All changes which the individual personally produces in his environment or humanity brings about by means of the arts in the world, are effected through the instrumentality of *motions*. Almost of necessity, therefore, motion appears to us as the most important physical factor. Moreover, mechanical properties may be discovered in all physical events. The sounding bell trembles, the heated body expands, the electrified body attracts other bodies. Why, therefore, should we not attempt to grasp all events under their mechanical aspect, since that is so easily apprehended and most accessible to observation and measurement? In fact, no objection *is* to be made to the attempt to elucidate the properties of physical events by mechanical *analogies*.

But modern physics has proceeded *very far* in this direction. The point of view which Wundt represents in his very interesting treatise *On the Physical Axioms* is probably shared by the majority of physicists. The axioms of physics which Wundt sets up are as follows:

1. All natural causes are motional causes.
2. Every motional cause lies outside the object moved.
3. All motional causes act in the direction of the straight line of junction, and so forth.
4. The effect of every cause persists.
5. Every effect involves an equal countereffect.
6. Every effect is equivalent to its cause.

These principles might be studied properly enough as fundamental principles of mechanics. But when they are set up as axioms

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la chaleur et de l'électricité comme dus à autre chose qu'à des *mouvements quelconques des corps* et comme tels ne doivent-ils pas être soumis aux lois générales de la mécanique?"

of physics, their enunciation is simply tantamount to a negation of all events except motion.

According to Wundt, all changes of nature are mere changes of place. All causes are motional causes (page 26). Any discussion of the philosophical grounds on which Wundt supports his theory would lead us deep into the speculations of the Eleatics and the Herbartians. Change of place, Wundt holds, is the *only* change of a thing in which a thing remains identical with itself. If a thing changed *qualitatively*, we should be obliged to imagine that something was annihilated and something else created in its place, which is not to be reconciled with our idea of the identity of the object observed and of the indestructibility of matter. But we have only to remember that the Eleatics encountered difficulties of exactly the same sort in motion. Can we not also imagine that a thing is destroyed in *one* place and in *another* an exactly similar thing created? After all, do we really know *more* why a body leaves one place and appears in another, than why a *cold* body grows *warm*? Granted that we had a perfect knowledge of the mechanical processes of nature, could we and should we, for that reason, *put out of the world* all other processes that we do not understand? On this principle it would be really the simplest course to deny the existence of the whole world. This is the point at which the Eleatics ultimately arrived, and the school of Herbart stopped little short of reaching the same goal.

Physics treated in this sense supplies us simply with a diagram of the world, in which we do not know reality again. It happens, in fact, to men who give themselves up to this view for many years, that the world of sense from which they start as a province of the great familiarity, suddenly becomes, in their eyes, the supreme "world-riddle."

Intelligible as it is, therefore, that the efforts of thinkers have always been bent upon the "reduction of all physical processes to the motions of atoms," it must yet be affirmed that this is a chimerical ideal. This ideal has often played an effective part in popular lectures, but in the workshop of the serious inquirer it has discharged scarcely the least function. What has really been achieved in mechanical physics is either the *elucidation* of physical processes by more familiar *mechanical analogies*, (for example, the theories of light and of electricity,) or the exact *quantitative* ascertainment of the connexion of mechanical processes with other physical processes, for example, the results of thermodynamics.

III. THE PRINCIPLE OF ENERGY IN PHYSICS.

We can know only from *experience* that mechanical processes produce other physical transformations, or *vice versa*. The attention was first directed to the connexion of mechanical processes, especially the performance of work, with changes of thermal conditions by the invention of the steam-engine, and by its great technical importance. Technical interests and the need of scientific lucidity meeting in the mind of S. Carnot led to the remarkable development from which thermodynamics flowed. It is simply *an accident of history* that the development in question was not connected with the practical applications of *electricity*.

In the determination of the maximum quantity of *work* that, in general, a heat-machine, or, to take a special case, a steam-engine, can perform with the expenditure of a *given* amount of heat of combustion, Carnot is guided by mechanical analogies. A body can do work on being heated, by expansion under pressure. But to do this the body must receive heat from a *hotter* body. Heat, therefore, to do work, must pass from a hotter body to a colder body, just as water must fall from a higher level to a lower level to put a mill-wheel in motion. Differences of temperature, accordingly, represent forces able to do work exactly as do differences of height in heavy bodies. Carnot pictures to himself an ideal process in which no heat flows away unused, that is, without doing work. With a given expenditure of heat, accordingly, this process furnishes the maximum of work. An analogue of the process would be a mill-wheel which scooping its water out of a higher level would slowly carry it to a lower level without the loss of a drop. A peculiar property of the process is, that with the expenditure of the same work the water can be raised again exactly to its original level. This property of *reversibility* is also shared by the process of Carnot. His process also can be reversed by the expenditure of the same amount of work, and the heat again brought back to its original temperature level.

Suppose, now, we had *two* different reversible processes A, B, such that in A a quantity of heat, Q, flowing off from the temperature t_1 to the lower temperature t_2 should perform the work W, but in B under the same circumstances it should perform a greater quantity of work, $W + W'$; then, we could join B in the sense assigned and A in the reverse sense into a *single* process. Here A would reverse the transformation of heat produced by B and would leave a surplus of work W', produced, so to speak, from nothing. The combination would present a perpetual motion.

With the feeling, now, that it makes little difference whether the mechanical laws are broken directly or indirectly (by processes of heat), and convinced of the existence of a *universal* law-ruled connexion of nature, Carnot here excludes for the first time from the province of *general* physics the possibility of a perpetual motion. *But it follows, then, that the quantity of work W, produced by the passage of a quantity of heat Q from a temperature t_1 to a temperature t_2, is independent of the nature of the substances as also of the character of the process, so far as that is unaccompanied by loss, but is wholly dependent upon the temperatures t_1, t_2.*

This important principle has been fully confirmed by the special researches of Carnot himself (1824), of Clapeyron (1834), and of Sir William Thomson (1849), now Lord Kelvin. The principle was reached *without any assumption whatever* concerning the nature of heat, simply by the exclusion of a perpetual motion. Carnot, it is true, was an adherent of the theory of Black, according to which the sum-total of the quantity of heat in the world is constant, but so far as his investigations have been hitherto considered the decision on this point is of no consequence. Carnot's principle led to the most remarkable results. W. Thomson (1848) founded upon it the ingenious idea of an "absolute" scale of temperature. James Thomson (1849) conceived a Carnot process to take place with water freezing under pressure and, therefore, performing work. He discovered, thus, that the freezing point is lowered 0·0075° Celsius by every additional atmosphere of pressure. This is mentioned merely as an example.

About twenty years after the publication of Carnot's book a further advance was made by J. R. Mayer and J. P. Joule. Mayer, while engaged as a physician in the service of the Dutch, observed, during a process of bleeding in Java, an unusual redness of the venous blood. In agreement with Liebig's theory of animal heat he connected this fact with the diminished loss of heat in warmer climates, and with the diminished expenditure of organic combustibles. The total expenditure of heat of a man at rest must be equal to the total heat of combustion. But since *all* organic actions, even the mechanical actions, must be placed to the credit of the heat of combustion, some connexion must exist between mechanical work and expenditure of heat.

Joule started from quite similar convictions concerning the galvanic battery. A heat of association equivalent to the consumption of the zinc can be made to appear in the galvanic cell. If a current is set up, a part of this heat appears in the conductor of the current. The interposition of an apparatus for the decomposition of water causes a part of this heat to disappear, which on the burning of the explosive gas formed, is reproduced. If the current runs an electromotor, a portion of the heat again disappears, which, on the consumption of the work by friction, again makes its appearance. Accordingly, both the heat produced and the work produced, appeared to Joule also as connected with the consumption of material. The thought was therefore present, both to Mayer and to Joule, of regarding heat and work as equivalent quantities, so connected with each other that what is lost in one form universally appears in another. The result of this was a *substantial* conception of heat and of work, and *ultimately a substantial conception of energy*. Here every physical change of condition is regarded as energy, the destruction of which generates work or equivalent heat. An electric charge, for example, is energy.

In 1842 Mayer had calculated from the physical constants then generally recognised that by the disappearance of one kilogramme-calorie 365 kilogrammetres of work could be performed, and *vice versa*. Joule, on the other hand, by a long series of delicate and varied experiments beginning in 1843 ultimately determined the mechanical equivalent of the kilogramme-calorie, more exactly, as 425 kilogrammetres.

If we estimate every change of physical condition by the *mechanical work* which can be performed upon the *disappearance* of that condition, and call this measure *energy*, then we can measure all physical changes of condition, no matter how different they may be, with the same common measure, and say: *the sum-total of all energy remains constant.* This is the form that the principle of excluded perpetual motion received at the hands of Mayer, Joule, Helmholtz, and W. Thomson in its extension to the whole domain of physics.

After it had been proved that heat must *disappear* if mechanical work was to be done at its expense, Carnot's principle could no longer be regarded as a complete expression of the facts. Its improved form was first given, in 1850, by Clausius, whom Thomson followed in 1851. It runs thus: "If a quantity of heat Q' is transformed into work in a reversible process, *another* quantity of heat Q of the absolute[1] temperature T_1 is lowered to the absolute temperature T_2." Here Q' is dependent only on Q, T_1, T_2, but is independent of the substances used and of the character of the process, so far as that is unaccompanied with loss. Owing to this last fact, it is sufficient to find the relation which obtains for some one well-known physical substance, say a gas, and some definite simple process. The relation found will be the one which holds generally. We get, thus,

$$\frac{Q'}{Q'+Q}=\frac{T_1-T_2}{T_1} \qquad (1)$$

that is, the quotient of the available heat Q' transformed into work divided by the sum of the transformed and transferred heats (the total sum used), the so-called *economical coefficient* of the process, is,

$$\frac{T_1-T_2}{T_1}.$$

IV. THE CONCEPTIONS OF HEAT.

When a cold body is put in contact with a warm body it is observed that the first body is warmed and that the second body is

[1] By this is meant the temperature of a Celsius scale, the zero of which is 273° below the melting point of ice.

cooled. We may say that the first body is warmed *at the expense of* the second body. This suggests the notion of a thing, or heat-substance, which passes from the one body to the other. If two masses of water m, m', of unequal temperatures, be put together, it will be found, upon the rapid equalisation of the temperatures, that the respective changes of temperatures u and u' are inversely proportional to the masses and of opposite signs, so that the algebraical sum of the products is,

$$m\,u + m'\,u' = 0.$$

Black called the products $m\,u$, $m'\,u'$, which are decisive for our knowledge of the process, *quantities of heat*. We may form a very clear *picture* of these products by conceiving them with Black as measures of the quantities of some substance. But the essential thing is not this picture but the *constancy* of the sum of these products in simple processes of conduction. If a quantity of heat disappears at one point, an equally large quantity will make its appearance at some other point. The retention of this idea leads to the discovery of specific heat. Black, finally, perceives that also something else may appear for a vanished quantity of heat, namely: the fusion or evaporation of a definite quantity of matter. He adheres here still to his favorite view, though with some freedom, and considers the vanished quantity of heat as still present, but as *latent*.

The generally accepted notion of a caloric, or heat-stuff, was strongly shaken by the work of Mayer and Joule. If the quantity of heat can be increased and diminished, people said, heat cannot be a substance, but must be a *motion*. The subordinate part of this statement has become much more popular than all the rest of the doctrine of energy. But we may convince ourselves that the motional conception of heat is now as unessential as was formerly its conception as a substance. Both ideas were favored or impeded solely by accidental historical circumstances. It does not follow that heat is not a substance from the fact that a mechanical equivalent exists for quantity of heat. We will make this clear by the following question which bright students have sometimes put to me. Is there a mechanical equivalent of electricity as there is a mechanical equivalent of heat? Yes, and no. There is no mechanical equivalent of *quantity* of electricity as there is an equivalent of *quantity* of heat, because the same quantity of electricity has a very different capacity for work, according to the circumstances in which it is placed; but there *is* a mechanical equivalent of electrical energy.

Let us ask another question. Is there a mechanical equivalent of water? No, there is no mechanical equivalent of quantity of water, but there is a mechanical equivalent of weight of water multiplied by its distance of descent.

When a Leyden jar is discharged and work thereby performed, we do not picture to ourselves that the quantity of electricity disappears as work is done, but we simply assume that the electricities come into different positions, equal quantities of positive and negative electricity being united with one another.

What, now, is the reason of this difference of view in our treatment of heat and of electricity? The reason is purely historical, wholly conventional, and, what is still more important, is wholly indifferent. I may be allowed to establish this assertion.

In 1785 Coulomb constructed his torsion balance, by which he was enabled to measure the repulsion of electrified bodies. Suppose we have two small balls, A, B, which over their whole extent are similarly electrified. These two balls will exert on one another, at a certain distance r of their centres, a certain repulsion p. We bring into contact with B now a ball C, suffer both to be equally electrified, and then measure the repulsion of B from A and of C from A at the same distance r. The sum of these repulsions is again p. Accordingly something has remained constant. If we ascribe this effect to a substance, then we infer naturally its constancy. But the essential point of the exposition is the divisibility of the electric force p and not the simile of substance.

In 1838 Riess constructed his electrical air-thermometer (thermoelectrometer). This gives a measure of the quantity of heat produced by the discharge of jars. This quantity of heat is not proportional to the quantity of electricity contained in the jar by Coulomb's measure, but if q be this quantity and c be the capacity, is proportional to $q^2/2c$, or, more simply still, to the energy of the charged jar. If, now, we discharge the jar completely through the thermometer, we obtain a certain quantity of heat, *W*. But if we make the discharge through the thermometer into a second jar, we obtain a quantity less than *W*. But we may obtain the remainder by completely discharging both jars through the air-thermometer, when it will be again proportional to the energy of the two jars. On the first, incomplete discharge, accordingly, a part of the electricity's capacity for work was lost.

When the charge of a jar produces heat its energy is changed and its value by Riess's thermometer is decreased. But by Coulomb's measure the quantity remains unaltered.

Now let us imagine that Riess's thermometer was invented before Coulomb's torsion balance, which is not a difficult feat, since both inventions are independent of each other; what would be more natural than that the "quantity" of electricity contained in a jar should be measured by the heat produced in the thermometer? But then, this so-called quantity of electricity would decrease on the production of heat or on the performance of work, whereas it now remains unchanged; in that case, therefore, electricity would not be a *substance* but a *motion*, whereas it is now still a substance. The reason, therefore, why we have other notions of electricity than we have of heat, is purely historical, accidental, and conventional.

This is also the case with other physical things. Water does not disappear when work is done. Why? Because we measure quantity of water with scales, just as we do electricity. But suppose the capacity of the water for work were called quantity, and had to be measured, therefore, by a mill instead of by scales; then this quantity also would disappear as it performed the work. It may, now, be easily conceived that many substances are not so easily got at as water. In that case we should be unable to carry out the one kind of measurement with the scales whilst many other modes of measurement would still be left us.

In the case of heat, now, the historically established measure of "quantity" is accidentally the work-value of the heat. Accordingly, its quantity disappears when work is done. But that heat is not a substance follows from this as little as does the opposite conclusion that it is a substance. In Black's case the quantity of heat remains constant because the heat passes into no *other* form of energy.

If any one to-day should still wish to think of heat as a substance, we might allow that person this liberty with little ado. He would only have to assume that that which we call quantity of heat was the energy of a substance whose quantity remained unaltered, but whose energy changed. In point of fact we might much better say, in analogy with the other terms of physics, energy of heat, instead of quantity of heat.

When we wonder, therefore, at the discovery that heat is motion, we wonder at something that was never discovered. It is perfectly indifferent and possesses not the slightest scientific value, whether we think of heat as a substance or not. The fact is, heat behaves in some connexions like a substance, in others not. Heat is latent in steam as oxygen is latent in water.

V. THE CONFORMITY IN THE DEPORTMENT OF THE ENERGIES.

The foregoing reflexions will gain in lucidity from a consideration of the conformity which obtains in the behavior of all energies, a point to which I called attention long ago.[1]

A weight P at a height H_1 represents an energy $W_1 = PH_1$. If we suffer the weight to sink to a lower height H_2, during which work is done, and the work done is employed in the production of living force, heat, or an electric charge, in short is transformed, then the energy $W_2 = PH_2$ is still *left*. The equation subsists,

$$\frac{W_1}{H_1} = \frac{W_2}{H_2}, \qquad (2)$$

or, denoting the *transformed* energy by $W' = W_1 - W_2$ and the *transferred* energy, that transported to the lower level, by $W = W_2$,

$$\frac{W'}{W' + W} = \frac{H_1 - H_2}{H_1}, \qquad (3)$$

[1] I first drew attention to this fact in my treatise *Ueber die Erhaltung der Arbeit*, Prague, 1872. Before this, Zeuner had pointed out the analogy between mechanical and thermal energy. I have given a more extensive development of this idea in a communication to the *Sitzungsberichte der Wiener Akademie*, December, 1892, entitled *Geschichte und Kritik des Carnot'schen Wärmegesetzes*. Compare also the works of Popper (1884), Helm (1887), Wronsky (1888), and Ostwald (1892).

an equation in all respects analogous to equation (1) at page 41. The property in question, therefore, is by no means peculiar to heat. Equation (2) gives the relation between the energy taken from the higher level and that deposited on the lower level (the energy left behind); it says that these *energies* are proportional to the *heights of the levels*. An equation analogous to equation (2) may be set up for *every* form of energy; hence the equation which corresponds to equation (3), and so to equation (1), may be regarded as valid for every form. For electricity, for example, H_1, H_2 signify the potentials.

When we observe for the first time the agreement here indicated in the transformative law of the energies, it appears surprising and unexpected, for we do not perceive at once its reason. But to him who pursues the comparative historical method that reason will not long remain a secret.

Since Galileo, mechanical work, though long under a different name, has been a *fundamental concept* of mechanics, as also a very important notion in the applied sciences. The transformation of work into living force, and of living force into work, suggests directly the notion of energy—the idea having been first fruitfully employed by Huygens, although Thomas Young first called it by the *name* of "energy." Let us add to this the constancy of weight (really the constancy of mass) and we shall see that with respect to mechanical energy it is involved in the very definition of the term that the capacity for work or the potential energy of a weight is proportional to the height of the level at which it is, in the geometrical sense, and that it decreases on the lowering of the weight, on transformation, proportionally to the height of the level. The zero level here is wholly arbitrary. With this, equation (2) is given, from which all the other forms follow.

When we reflect on the tremendous start which mechanics had over the other branches of physics, it is not to be wondered at that the attempt was always made to apply the notions of that science wherever this was possible. Thus the notion of mass, for example, was imitated by Coulomb in the notion of the quantity of electricity. In the further development of the theory of electricity, the notion of work was likewise immediately introduced in the theory of potential, and heights of electrical level were measured by the work of unit of quantity raised to that level. But with this the preceding equation with all its consequences is given for electrical energy. The case with the other energies was similar.

Thermal energy, however, appears as a special case. Only by the peculiar experiments mentioned could it be discovered that heat is an energy. But the measure of this energy by Black's quantity of heat is the outcome of fortuitous circumstances. In the first place, the accidental slight variability of the capacity for heat c with the temperature, and the accidental slight deviation of the usual thermometrical scales from the scale derived from *the tensions of gases*, brings it about that the notion "quantity of heat" can be set up and that the quantity of heat ct corresponding to a difference of temperature t is nearly proportional to the energy of the heat. It is a quite accidental historical circumstance that Amontons hit upon the idea of measuring temperature by the tension of a gas. It is certain in this that he did not think of the work of the heat.[1] But the numbers standing for temperature, thus, are made proportional to the tensions of gases, that is, to the work done by gases, with otherwise equal changes of volume. It thus happens that *temperature heights* and *level heights of work* are proportional to one another.

If properties of the thermal condition varying greatly from the tensions of gases had been chosen, this relation would have assumed very complicated forms, and the agreement between heat and the other energies above considered would not subsist. It is very instructive to reflect upon this point. A *natural law*, therefore, is not implied in the conformity of the behavior of the energies, but this conformity is rather conditioned by the uniformity of our modes of conception and is also partly a matter of good fortune.

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[1] Sir William Thomson first consciously and intentionally introduced (1848, 1851) a *mechanical* measure of temperature similar to the electric measure of potential.

VI. THE DIFFERENCES OF THE ENERGIES AND THE LIMITS OF THE PRINCIPLE OF ENERGY.

Of every quantity of heat Q which does work in a reversible process (one unaccompanied by loss) between the absolute temperatures T_1 T_2, only the portion

$$\frac{T_1 - T_2}{T_1}$$

is transformed into work, while the remainder is transferred to the lower temperature-level T_2. This transferred portion can, upon the reversal of the process, with the same expenditure of work, again be brought back to the level T_1. But if the process is not reversible, then more heat than in the foregoing case flows to the lower level, and the surplus can no longer be brought back to the higher level T_2 without some *special* expenditure. W. Thomson (1852), accordingly, drew attention to the fact, that in all non-reversible, that is, in all real thermal processes, quantities of heat are lost for mechanical work, and that accordingly a dissipation or waste of mechanical energy takes place. In all cases, heat is only partially transformed into work, but frequently work is wholly transformed into heat. Hence, a tendency exists towards a diminution of the *mechanical* energy and towards an increase of the *thermal* energy of the world.

For a simple, closed cyclical process, accompanied by no loss, in which the quantity of heat Q_1 is taken from the level T_1, and the quantity Q_2 is given to the level T_2, the following relation, agreeably to equation (2), exists,

$$-\frac{Q_1}{T_1} + \frac{Q_2}{T_2} = 0.$$

Similarly, for any number of compound reversible cycles Clausius finds the algebraical sum

$$\Sigma \frac{Q}{T} = 0,$$

and supposing the temperature to change continuously,

$$\int \frac{dQ}{T} = 0 \quad \ldots\ldots\ldots \quad (4)$$

Here the elements of the quantities of heat deducted from a given level are reckoned negative, and the elements imparted to it, positive. If the process is not reversible, then expression (4), which Clausius calls *entropy*, increases. In actual practice this is always the case, and Clausius finds himself led to the statement:

1. That the energy of the world remains constant.
2. That the entropy of the world tends toward a maximum.

Once we have noted the above-indicated conformity in the behavior of different energies, the *peculiarity* of thermal energy here mentioned must strike us. Whence is this peculiarity derived, for, generally every energy passes only partly into another form, as does thermal energy? The explanation will be found in the following.

Every transformation of a special kind of energy A is accompanied with a fall of potential of that particular kind of energy, including heat. But whilst for the other kinds of energy a transformation and therefore a loss of energy on the part of the kind sinking in potential is connected with the fall of the potential, with heat the case is different. Heat can suffer a fall of potential without sustaining a loss of energy, at least according to the customary mode of estimation. If a weight sinks, it must create perforce kinetic energy, or heat, or some other form of energy. Also, an electrical charge cannot suffer a fall of potential without loss of energy, i. e., without transformation. But heat can pass with a fall of temperature to a body of greater capacity and the same thermal energy still be preserved, so long as we regard *every quantity* of heat as energy. This it is that gives to heat, besides its property of energy, in many cases the character of a material *substance*, or quantity.

If we look at the matter in an unprejudiced light, we must ask if there is any scientific sense or purpose in still considering as energy a quantity of heat that can no longer be transformed into mechanical work, (for example, the heat of a closed equably warmed material system). The principle of energy certainly plays in this case a wholly superfluous rôle, which is assigned to it only from habit.[1] To maintain the principle of energy in the face of a knowl-

[1] Compare my *Analyse der Empfindungen*, 1886.

edge of the dissipation or waste of mechanical energy, in the face of the increase of entropy is equivalent almost to the liberty which Black took when he regarded the heat of liquefaction as still present but latent.[1] It is to be remarked further, that the expressions, "energy of the world" and "entropy of the world," are slightly permeated with scholasticism. Energy and entropy are *metrical* notions. What meaning can there be in applying these notions to a case in which they are not applicable, in which their values are not determinable?

If we could really determine the entropy of the world it would represent a true, absolute measure of time. In this way is best seen the utter tautology of a statement that the entropy of the world increases with the time. Time, and the fact that certain changes take place only in a definite sense, are one and the same thing.

VII. THE SOURCES OF THE PRINCIPLE OF ENERGY.

We are now prepared to answer the question, What are the sources of the principle of energy? All knowledge of nature is derived in the last instance from experience. In this sense they are right who look upon the principle of energy as a result of experience.

Experience teaches that the sense-elements $\alpha\,\beta\,\gamma\,\delta\,\ldots$ into which the world may be decomposed, are subject to change. It tells us further, that certain of these elements are *connected* with other elements, so that they appear and disappear together; or, that the appearance of the elements of one class is connected with the disappearance of the elements of the other class. We will avoid here the notions of cause and effect on account of their obscurity and equivocalness. The result of experience may be expressed as fol-

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[1]A better terminology appears highly desirable in the place of the usual perplexing one. Sir Wm. Thomson (1852) appears to have felt this need, and it has been clearly expressed by F. Wald (1889). We should call the work which corresponds to a vanished quantity of heat its mechanical substitution-value; while that work which can be *actually* performed in the passage of a thermal condition A to a condition B, alone deserves the name of the *energy-value* of this change of condition. In this way the *arbitrary* substantial conception of the processes would be preserved and misapprehensions forestalled.

lows: *The sensuous elements of the world* ($\alpha\,\beta\,\gamma\,\delta$ . . . .) *show themselves to be interdependent.* This interdependence is best represented by some such conception as is in geometry that of the mutual dependence of the sides and angles of a triangle, only much more varied and complex.

As an example, we may take a mass of gas enclosed in a cylinder and possessed of a definite volume (α), which we change by a pressure (β) on the piston, at the same time feeling the cylinder with our hand and receiving a sensation of heat (γ). Increase of pressure diminishes the volume and increases the sensation of heat.

The various facts of experience are not in all respects alike. Their common sensuous elements are placed in relief by a process of abstraction and thus impressed upon the memory. In this way the expression is obtained of the features of *agreement* of entire groups of facts. The simplest sentence which we can utter is, from the very nature of language, an abstraction of this kind. But account must also be taken of the *differences* of related facts. Facts may be so nearly related as to contain the same kind of $\alpha\,\beta\,\gamma$ . . . but the relation be such that the $\alpha\,\beta\,\gamma$ . . . of the one differ from the $\alpha\,\beta\,\gamma$ . . . of the other only by the number of equal parts into which they can be divided. Such being the case, if rules can be given for deducing *from one another* the numbers which are the measures of these $\alpha\,\beta\,\gamma$ . . ., then we possess in such rules the *most general* expression of a group of facts, as also that expression which corresponds to all its differences. This is the goal of quantitative investigation.

If this goal is reached what we have found is that between the $\alpha\,\beta\,\gamma$ . . . of a group of facts, or better, between the numbers which are their measures, a number of equations exists. The simple fact of change brings it about that the number of these equations must be smaller than the number of the $\alpha\,\beta\,\gamma$ . . . If the former be smaller by one than the latter, then one portion of the $\alpha\,\beta\,\gamma$ . . . is *uniquely* determined by the other portion.

The quest of relations of this last kind is the most important function of special experimental research, because we are enabled by it to complete in thought facts that are only partly given. It is self-evident that only experience can ascertain that between the

$\alpha \beta \gamma \ldots$ relations exist and of what kind they are. Further, only experience can tell that the relations that exist between the $\alpha \beta \gamma \ldots$ are such that changes of them can be reversed. If this were not the fact all occasion for the enunciation of the principle of energy, as is easily seen, would be wanting. In experience, therefore, is buried the ultimate well-spring of all knowledge of nature, and consequently, in this sense, also the ultimate source of the principle of energy.

But this does not exclude the fact that the principle of energy has also a logical root, as will now be seen. Let us assume on the basis of experience that one group of sensuous elements $\alpha \beta \gamma \ldots$ determines *uniquely* another group $\lambda \mu \nu \ldots$ Experience further teaches that changes of $\alpha \beta \gamma \ldots$ can be *reversed.* It is then a logical consequence of this observation, that every time that $\alpha \beta \gamma \ldots$ assume the same values this is also the case with $\lambda \mu \nu \ldots$ Or, that purely *periodical* changes of $\alpha \beta \gamma \ldots$ can produce no *permanent* changes of $\lambda \mu \nu \ldots$ If the group $\lambda \mu \nu \ldots$ is a mechanical group, then a perpetual motion is excluded.

It will be said that this is a vicious circle, which we will grant. But psychologically, the situation is essentially different, whether I think simply of the unique determination and reversibility of events, or whether I exclude a perpètual motion. The attention takes in the two cases different directions and diffuses light over different sides of the question, which logically of course are necessarily connected.

Surely that firm, logical setting of the thoughts noticeable in the great inquirers, Stevinus, Galileo, and the rest, which, consciously or instinctively, was supported by a fine feeling for the slightest contradictions, has no other purpose than to limit the bounds of thought and so exempt it from the possibility of error. In this, therefore, the logical root of the principle of excluded perpetual motion is given, namely, in that universal conviction which existed even before the development of mechanics and co-operated in that development.

It is perfectly natural that the principle of excluded perpetual motion should have been first developed in the simple domain of pure mechanics. Towards the transference of that principle into the domain of general physics the idea contributed much that all physical phenomena are mechanical phenomena. But the foregoing discussion shows how little essential this notion is. The issue really involved is the recognition of a general interconnexion of nature. This once established, we see with Carnot that it is indifferent whether the mechanical laws are broken directly or circuitously.

The principle of the excluded perpetual motion is very closely related to the modern principle of energy, but it is not identical with it, for the latter is to be deduced from the former only by means of a definite *formal conception*. As may be seen from the preceding exposition, the perpetual motion can be excluded without our employing or possessing the notion of *work*. The modern principle of energy results primarily from a *substantial* conception of work and of every change of physical condition which by being reversed produces work. The strong need of such a conception, which is by no means necessary, but in a formal sense is very convenient and lucid, is exhibited in the case of J. R. Mayer and Joule. It was before remarked that this conception was suggested to both inquirers by the observation that both the production of heat and the production of mechanical work was connected with an expenditure of substance. Mayer says: "Ex nihilo nil fit," and in another place, "The creation or destruction of a force (work) lies beyond the domain of human activity." In Joule we find this passage: "It is manifestly *absurd* to suppose that the powers with which God has endowed matter can be destroyed."

Some writers have observed in such statements the attempt at a *metaphysical* establishment of the doctrine of energy. But we see in them simply the formal need of a simple, clear, and living grasp of the facts, which receives its development in practical and technical life, and which we carry over, as best we can, into the province of science. As a fact, Mayer writes to Griesinger: "If, finally, you ask me how I got involved in the whole affair, my answer is simply this: Engaged during a sea voyage almost exclusively with the study of physiology, I discovered the new theory for the sufficient reason that I *vividly felt the need of it.*"

The substantial conception of work (energy) is by no means a necessary one. And it is far from true that the problem is solved with the recognition of the need of such a conception. Rather let us see how Mayer gradually endeavored to satisfy that need. He first regards quantity of motion, or momentum, mv, as the equivalent of work, and did not light, until later, on the notion of living force ($mv^2/2$). In the province of electricity he was unable to assign the expression which is the equivalent of work. This was done later by Helmholtz. The formal need, therefore, is *first* present, and our conception of nature is subsequently gradually *adapted* to it.

The laying bare of the experimental, logical, and formal root of the present principle of energy will perhaps contribute much to the removal of the mysticism which still clings to this principle. With respect to our formal need of a very simple, palpable, substantial conception of the processes in our environment, it remains an open question how far nature corresponds to that need, or how far we can satisfy it. In one phase of the preceding discussions it would seem as if the substantial notion of the principle of energy, like Black's material conception of heat, has its natural limits in facts, beyond which it can only be artificially adhered to.

Ernst Mach.

University of Prague.