The Discovery of the Law of Conservation of Energy.

With Facsimile Reproductions (nos. V and VI) of MAYER's and JOULE's earliest printed contributions, and of a manuscript note by SADI CARNOT (no. VII).

Contents: 1. Introductory; 2. ROBERT MAYER; 3. JAMES PRESCOTT JOULE; 4. MAYER and JOULE; 5. CARNOT, SÉGUIN, and COLDING; 6. Additional bibliography; 7. English translation of MAYERS's first printed paper (1842); 8. Transcription of the page of SADI CARNOT's manuscripts reproduced in facsimile no. VII.

Facsimile no. V. J. R. MAYER: Bemerkungen über die Kräfte der unbelebten Natur (*Annalen der Chemie und Pharmacie*, vol. 42, 233-40, Heidelberg, 1842).

Facsimile no. VI. J. P. JOULE: On the calorific effects of magneto-electricity and the mechanical value of heat (*Report of the thirteenth meeting of the British Association*, Cork, August 1843; *Transactions* of the sections, p. 33, London 1844.

Facsimile no. VII. SADI CARNOT: Manuscript note in the archives of the Academy of Sciences, Paris. Date c. 1824-32.

1. — *Introductory.*

The greatest bane of our studies is their secondhandedness. Among the scientists, historians, and philosophers, who take an interest in them, only a few make genuine attempts to return to the sources; and of these few, only a very few are able to reach those sources. Indeed many of the earlier scientific writings are available only in the largest libraries. We hope to change these conditions by printing in *Isis* not only the text of some of the scientific classics, but the ipsissima verba of the originals. That is, we shall publish exact photographic equivalents of them. To be sure, this can be done only for a small number of the shorter works, but from the methodological point of view this does not matter so much because our chief purpose is to train our readers to want the originals. Having acquired the habit of referring to the very sources, and of basing their judgment upon their own autopsies, they will surely find ways of obtaining any other original which they may need in the course of their investigations.

These facsimile publications will be especially useful to scholars teaching the history of science, as well for their own use as for the use of their classes.

The following (unnumbered) facsimiles have already appeared in *Isis*.

I. ABRAHAM DE MOIVRE : Approximatio ad summam terminorum binomii $(a + b)^n$ in seriem expansi (Nov. 12, 1733; published in London, 1738). Edited by R. C. ARCHIBALD. *Isis*, 8, 671-83, 1926.

II. BLAISE PASCAL : Essay pour les coniques. (Paris 1640). With an English translation by FRANCES MARGUERITE CLARKE and a preface by DAVID EUGENE SMITH. *Isis*, 10, 16-20, 2 pl., 1928.

III. H. C. OERSTED : Experimenta circa effectum conflictus electrici in acum magneticam. (Copenhagen, 1820). With preface by G. SARTON, and portrait. *Isis*, 10, 435-40, 1928.

IV. H. C. OERSTED : Experiments on the effect of a current of electricity on the magnetic needle. (London, 1820). Original English translation of III. *Isis*, 10, 441-44, 1928.

2. — *Robert Mayer* (1814-78).

The discovery of the existence of an invariable relation between heat and work was made independently at almost the same time by two men who were utterly unlike and whose processes of thought were as different as their own personalities. JULIUS ROBERT MAYER was a German physician with but little physical and mathematical knowledge. In the course of his service as a doctor aboard a Dutch ship he discovered the law of conservation of energy, by a sudden intuition. This great discovery, comparable in its suddenness to a religious conversion, occurred while he was in the harbor of Surabaya (N. E. Java) in July 1840. His whole life was henceforth devoted to the development and defense of that idea; after a few years of happy work, the effort which he had to make to continue his activity in the face of untoward circumstances was so great that his health, physical and mental, was undermined and wrecked. The largest part of his life was spent in his native city, Heilbronn, where he died in 1878.

He wrote his first paper on the subject, entitled « *Ueber quantitative und qualitative Bestimmung der Kräfte* », on June 16, 1841, and sent it to POGGENDORFF, who rejected it. This was very fortunate, for that paper contained gross errors. The first paper to be published was written half a year later in the beginning of 1842 and submitted to LIEBIG who accepted it for the *Annalen der Chemie und Pharmacie* of which he was the editor together with FRIEDRICH WÖHLER. That paper, entitled *Bemerkungen über die Kräfte der unbelebten Natur*, was published in vol. 42, 233-40, Heidelberg, 1842. This was the first paper containing a clear statement of the law of conservation of energy, and an attempt to determine the mechanical equivalent of heat. Our Facsimile Reprint No. V is a photograph of it. We also reprint the English translation by G. C. FOSTER, published in the *Philosophical Magazine* (vol. 24, 371-77, 1862) at the request of TYNDALL and JOULE.

MAYER published elaborations of his views in many other papers of which the most important are : (1) *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel* (Heilbronn, 1845); (2) *Beiträge zur Dynamik des Himmels* (Heilbronn, 1848); (3) *Bemerkungen über das mechanische Aequivalent der Wärme* (Heilbronn, 1851). It should be noted that these three important essays were printed, not in scientific journals, but independently at the author's own risks. However, the latter had obtained sufficient recognition before his death to warrant the publication of two editions of his collected writings (Stuttgart, 1867; Stuttgart, 1874). A third edition was admirably prepared by a fervent disciple, JACOB J. WEYRAUCH (2 vols., Stuttgart, 1893). The second volume includes minor writings, letters, and biographical documents; it also includes the text of the unpublished memoir of 1841.

A partial summary by JOHN TYNDALL of the memoir of 1845 will be found in the *Philosophical Magazine* (vol. 28, 25-42, 1864). The same journal contains English versions of the *Beiträge* of 1848, by H. DEBUS (vol. 25, 241-48, 387-409, 417-28, 1863), and of the *Bemerkungen* of 1851, by G. C. FOSTER (vol. 25, 493-522, 1863).

3. — *James Prescott Joule* (1818-89).

MAYER was neither a mathematician, nor an experimental physicist. His great discovery had been accomplished by purely abstract means; he had made no experiments himself, but had used with considerable ingenuity the experimental results of others. It is clear that the grand generalization which he had attained in a flash of genius, however suggestive, would have remained unconvincing if it had not been supported by new experimental data. We have witnessed recently a similar sequence of events with regard to the theory of relativity. As long as it remained a mathematical abstraction, it was considered with general indifference or scepticism, but as soon as the experimental confirmations were provided, the whole scientific world, with but few exceptions, hastened to accept it.

In the present case, MAYER's philosophical discovery was confirmed and completed by the experimental investigations made independently at about the same time by the English physicist JAMES PRESCOTT JOULE. As opposed to MAYER, who was primarily a philosopher, JOULE was a metrologist. His main interest lay in exact measurements, and his special genius showed itself at its best in the invention of methods enabling one to obtain more and more accuracy in quantitative experiments. He thus devised many different methods by means of which the mechanical equivalent of heat (or the thermal equivalent of work) could be determined with increasing precision. His first paper on the subject was read on August 21, 1843, at the thirteenth annual meeting of the British Association for the Advancement of Science, at Cork, in south Ireland. It is interesting to note that it was read in the chemical section; even so, MAYER's first paper was published in a chemical journal. We may thus say that the greatest generalization of nineteenth century physics was introduced to the scientific world through the chemical door!

JOULE's paper « *On the calorific effects of magneto-electricity, and on the mechanical value of heat* » was printed in the *Philosophical Magazine* (vol. 23, 263-76, 347-55, 435-43, 1843), but a summary of it appeared in the *Reports of the Cork meeting* (Transactions of the sections, p. 33). The full paper is too long to be reproduced, but we give a facsimile (Reprint no. VI) of the summary, prepared presumably by the author. I must warn the reader that reference to the paper itself is necessary to have a correct idea of the enormous experimental labor which it represents. These memorable experiments were made at Broom Hill, near Manchester. The original memoir was reprinted in « *The Scientific Papers of* JAMES PRESCOTT JOULE » edited by the Physical Society of London, within the author's life time (vol. 1, 123-59, 1884; a second volume appeared in 1887).

4. — *Mayer and Joule.*

A few years elapsed before the importance of MAYER's and JOULE's ideas began to be realized. When the scientific world was finally aware of it, it accorded some recognition to the discoverers, yet a longer perspective was needed for a complete appreciation of their achievements. This is natural enough; the bigger a thing is, the further we must be to see the whole of it. JOULE was the happier; as early as 1850 he was admitted into the Royal Society and two years later one of the royal medals was awarded to him. (1) MAYER does not seem to have obtained any recognition before 1858, when the *chemist* CHRISTIAN FRIEDRICH SCHÖNBEIN caused him to be elected an honorary member of the Scientific Society of Basle. But by that time, MAYER had disappeared so completely from the world, that LIEBIG who was praising him in a lecture delivered in Munich in that very year, spoke of him as if he had died in the insane asylum where he had been confined. MAYER died only twenty years later, but his troubles were not over, for in proportion as the scientists became more convinced of the pregnancy of that great discovery, they were more inclined to discuss the relative merits of the two protagonists.

The main champions of MAYER were chemists like SCHÖNBEIN and LIEBIG, and the philosopher EUGEN DÜHRING, who called him the Galileo of the nineteenth century (2), but also the English physicist, JOHN TYNDALL. His adversaries were P. G. TAIT and WM. THOMSON (KELVIN), and also HELMHOLTZ. A long

(1) The other royal medal of that year (1852) was given to THOMAS HENRY HUXLEY.

(2) EUGEN DÜHRING : Kritische Geschichte der allgemeinen Prinzipien der Mechanik (Berlin, 1873); ROBERT MAYER, der Galilei des neunzehnten Jahrhunderts (Chemnitz, 1880).

controversy took place in the *Philosophical Magazine* (vols. 24 to 26, and 28, 1862-64). Having read a large part of it, with that impartiality which a greater distance makes relatively easy, I have come to the conclusion that TYNDALL had the better part. He showed more insight and more generosity. Read for example his open letter to JOULE (vol. 24, 173-75, 1862).

MAYER's adversaries were generally bent on discrediting him entirely, while his friends, and notably TYNDALL, were anxious to give JOULE his due. In the letter to JOULE, he concluded « I do not think the public estimate of your labours can be in the least affected by any recognition which may be accorded to MAYER. There is room for both on this grand platform. Certainly, had MAYER never written a syllable on the mechanical theory of heat, I should not deem your work a whit nobler than I now hold it to be. »

This must also be our conclusion. It cannot be denied that ROBERT MAYER was the first to explain the first principle of thermodynamics, which has remained until yesterday one of the most fundamental conceptions of science. Recent discoveries are now calling it in question because our ideas of energy and of mass have been deeply modified, but this simply means that the principle of conservation of energy will take (once more) a different shape; it is not likely that it will be entirely discarded. If MAYER was the first to formulate it with some clearness, and to calculate the constant implied, nevertheless his discovery was essentially incomplete for it lacked experimental support. That support was given, in an admirable manner, with precision and abundance, by JAMES PRESCOTT JOULE.

These two names, MAYER and JOULE, must remain united in our grateful hearts. It is not possible to compare quantitatively two men who where so essentially different. MAYER's views were vague but broad, and he was the first. JOULE reached the same general conclusion a little later, but after having followed an entirely different path. We might compare them to two travelers who, coming from opposite directions, reached at about the same time the summit of the same mountain. The one thing that might be said to JOULE's advantage is this : MAYER's work was in itself insufficient, it could not stand without the experimental confirmation which JOULE provided; on the contrary, JOULE's achievement was independent of MAYER's and needed no external support. JOULE built his theory upon a firm foundation, while MAYER built his upon sand; yet the fact remains that MAYER had seen the main truth and its implications before JOULE and more completely than he.

5. — *Carnot, Séguin, and Colding.*

The priority discussion between the defenders of MAYER and those of JOULE introduced other claimants. We may leave out of the question RUMFORD and DAVY whose works prepared the discovery but cannot be considered as an anticipation of it. Of course in the field of science, every step is preceded and conditioned by an infinity of others, yet when we speak of a definite discovery, we cannot give the credit of it to those who prepared it but only to those who made it.

We may leave out also the work of CARNOT and the discovery of the second law of thermodynamics (1824), which was much anterior to that of the first law but did not influence it. MAYER refused until the end to take any interest in it; JOULE devoted his energy to the full establishment of the first law. The synthesis of both laws and the systematization of thermodynamics was accomplished by other men, chiefly HERMANN HELMHOLTZ, WILLIAM THOMSON (KELVIN) and RUDOLF CLAUSIUS.

However, SADI CARNOT (1796-1832) was not simply the discoverer of the second law; he had also anticipated with astounding directness and lucidity the discovery of the first law. And not only that but one can deduct from his notebooks a definite value for the mechanical equivalent of heat, 370 kilogrammeters corresponding to one calorie. As compared with the modern value 426.5 this was a little better than MAYER's original value, 365. It is probable that but for his untimely death at the age of thirty-six CARNOT would have published this second immense discovery of his; at any rate it remained buried in his manuscripts and unknown to the world until 1878. If the fates had been kinder to him, he would have fully deserved to be called the discoverer of both principles, the father of thermodynamics. In a sense these titles may still be given to him, but with a restriction, — for it is a wise historical rule to give full credit only for published discoveries. One might claim that his discovery of the second law was in itself sufficient to oblige us to hail him as the father of thermodynamics, and it is sure that, all considered, none of the early protagonists deserved that honor more fully than he did.

To return to published documents, something must be said of two other men, the Frenchman MARC SÉGUIN and the Dane LUDVIG AUGUST COLDING. SÉGUIN, who called himself Séguin ainé, was a nephew of the brothers MONTGOLFIER (3); he was born at Annonay (Vivarais, Ardêche) in 1786, and died there in 1875. He was a typical engineer, and his main work *De l'influence des chemins de fer et de l'art de les tracer et de les construire* (XIX + 501 p., 6 pl., Paris, 1839), bore the significant epigraph « L'industrie est devenue la vie des peuples. » It is in that work that one may find (p. 382, 383, 389, 403) an anticipation of MAYER's intuition, the proportionality of heat and work, but it contains no attempt to determine the coefficient of proportionality. (This was misunderstood by JOULE and TAIT; see TYNDALL in *Philos. Mag.*, vol. 28, 29-31, 1864, with English translation of pertinent passages).

LUDVIG AUGUST COLDING (1815-88) presented in 1843 to the Danish Academy of sciences a memoir entitled « *Nogle Saetninger om Kraefterne* ». This memoir contained not only views very similar to those of MAYER's, but also the account of original experiments. Unfortunately it was not published until 1856, and even then only in Danish. Moreover it fell short of MAYER and JOULE's work for the same reason as SÉGUIN's : it proved the proportionality of heat and work, but failed to calculate the factor of proportionality.

Indeed the astounding feature of MAYER's philosophical activity was the determination from the very beginning of the mechanical equivalent of heat. That determination was very inaccurate, yet the very fact that MAYER had undertaken it lifted his work from the field of irresponsible theorizing into that of pure physics, and placed him far above SÉGUIN and COLDING, at the very side of JOULE.

6. — *Additional bibliography.*

Robert Mayer. — The main source is the excellent critical

(3) Inventors of balloons; they were born near Annonay.

edition of MAYER's works and letters by JACOB J. WEYRAUCH (2 vols., Stuttgart, 1893). See also WEYRAUCH : R. M. zur Jahrhundertfeier seiner Geburt (105 p., 3 portraits, Stuttgart, 1915).

S. FRIEDLAENDER : R. M. (210 p., Klassiker der Naturwissenschaften, Leipzig, 1905). A. E. HAAS : *Die Entwicklungsgeschichte des Satzes von der Erhaltung der Kraft* (Wien, 1909). WILHELM OSTWALD : *Grosse Männer* (Leipzig, 1910, 61-100). BERNHARD HELL : *M. und das Gesetz von der Erhaltung der Energie* (165 p., Frommanns Klassiker der Philosophie, Stuttgart, 1925).

James Prescott Joule. — As the two volumes of « Scientific papers » and « Joint scientific papers » were edited by the Physical Society of London during JOULE's life-time (1884, 1887) they contain no biographical information, except the portrait, engraved by C. H. JEENS, originally published in *Nature*.

OSBORNE REYNOLDS : *Memoir of James Prescott Joule* (*Manchester Literary and Philosophical Society*; VIII + 196 p., portrait; Manchester, 1892). ALEX. WOOD : *Joule and the study of energy* (VIII + 88 p., London, 1925; *Isis*, 8, 566).

Sadi Carnot. — *Réflexions sur la puissance motrice du feu* (103 p., portrait, Paris, 1878). This reprint of CARNOT's work of 1824 is followed by a biographical sketch, and by extracts from CARNOT's manuscripts proving that he had actually anticipated MAYER and JOULE, including a determination of the mechanical equivalent of heat (see our facsimile no. VII, and its transcription in § 8).

E. ARIÈS : *L'œuvre scientifique de Sadi Carnot* (160 p., Paris, 1921; *Isis*, 4, 598). *Carnot Centenary commemoration.* (32 p., Engineering foundation, New York, 1925).

Marc Séguin. — *Marc Séguin*, 1786-1875 (20 p., portrait; Comité du monument Marc Séguin, Annonay, 1913).

Ludvig August Colding. — *Nogle Saetninger om Kraefterne* (Academy of sciences of Copenhagen, 1856, 20 p., 1 pl.). *Naturvidenskabelige Betragtninger over Slaegtskabet mellem det aandelige Livs Virksomheder og de almindelige Naturkraefter* (ibidem, 1856, 136-68).

Traduction of COLDING's memoir in Ido, with a preface by WILHELM OSTWALD (20 p., Paris, 1913; *Isis*, 1, 522-24).

7. — *English translation of Mayers' first printed paper* (1842). *Remarks on the forces of inorganic nature*(4).

The following pages are designed as an attempt to answer the questions, What are we to understand by « Forces »? 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 aequat 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 : $c = e = f ... = c$. — 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, etc., we must regard these various magnitudes as different forms under which one and the same object makes its

(4) Translated from the *Annalen der Chemie und Pharmacie*, vol. 42. p. 233 (May 1842), by G. C. Foster, B. A., Lecturer on Natural Philosophy 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. 24. pp. 64-66. (G. C. F.)

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.*

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, not 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 the 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, etc.) 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 (5), 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. And what applies to the smallest particles of matter, and the smallest intervals between them, must also apply to large masses and to measurable 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.

(5) Since the original publication of this paper, Prof. W. THOMSON has shown that pressure has a sensible effect in liquefying ice (*Phil. Mag.*, S. 3. vol. 37, 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.

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, etc. — 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 aequat 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 (1). 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.

8. — *Transcription of the page of Sadi Carnot's manuscripts reproduced in facsimile no. VII.*

These manuscripts were given by CARNOT's family to the French Academy of Sciences in 1878 and extracts were published in the same year (see § 6). Our facsimile and its transcription are borrowed from that publication (p. 89, 94). It is impossible to date that extract exactly; it is posterior to 1824 and anterior to August 24, 1832, when CARNOT died.

« La chaleur n'est autre chose que la puissance motrice, ou plutôt que le mouvement qui a changé de forme. C'est un mouvement dans les particules des corps. Partout où il y a destruction de puissance motrice, il y a, en même temps, production de chaleur en quantité précisément proportionnelle à la quantité de puissance motrice détruite. Réciproquement, partout où il y a destruction de chaleur, il y a production de puissance motrice.

« On peut donc poser en thèse générale que la puissance motrice est en quantité invariable dans la nature, qu'elle n'est jamais, à proprement parler, ni produite, ni détruite. A la vérité, elle

(1) 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. (G. C. F.)

change de forme, c'est-à-dire qu'elle produit tantôt un genre de mouvement, tantôt un autre; mais elle n'est jamais anéantie.

« D'après quelques idées que je me suis formées sur la théorie de la chaleur, la production d'une unité de puissance motrice nécessite la destruction de 2,70 unités de chaleur.

« Une machine qui produirait 20 unités de puissance motrice par kilogramme de charbon devrait anéantir $\frac{20 \times 2{,}70}{7000}$ de la chaleur développée par la combustion; $\frac{20 \times 2{,}7}{7000} = \frac{8}{1000}$ environ, c'est-à-dire moins de $\frac{1}{100}$. »

Facsimile No. V : Robert Mayer. Photographed from the Annalen der Chemie und Pharmacie (vol. 42, 233-40, Heidelberg, 1842).

Bemerkungen über die Kräfte der unbelebten Natur;

von *J. R. Mayer.*

Der Zweck folgender Zeilen ist, die Beantwortung der Frage zu versuchen, was wir unter „Kräften" zu verstehen haben, und wie sich solche untereinander verhalten. Während mit der Benennung Materie einem Objecte sehr bestimmte Eigenschaften, als die der Schwere, der Raumerfüllung, zugetheilt werden, knüpf sich an die Benennung Kraft vorzugsweise der Begriff des unbekannten, unerforschlichen, hypothetischen. Ein Versuch, den Begriff von Kraft ebenso präcis als den von Materie aufzufassen, und damit nur Objecte wirklicher Forschung zu bezeichnen, dürfte mit den daraus fliefsenden Consequenzen, Freunden klarer hypothesenfreier Naturanschauung nicht unwillkommen seyn.

Kräfte sind Ursachen, mithin findet auf dieselbe volle Anwendung der Grundsatz: *causa aequat effectum.* Hat die Ursache c die Wirkung e, so ist $c = e$; ist e wieder die Ursache einer andern Wirkung f, so ist $e = f$, u. s. f. $c = e = f \ldots = c$. In einer Kette von Ursachen und Wirkungen kann, wie aus der Natur einer Gleichung erhellt, nie ein Glied oder ein Theil eines Gliedes zu Null werden. Diese erste Eigenschaft aller Ursachen nennen wir ihre *Unzerstörlichkeit.*

Hat die gegebene Ursache *c* eine ihr gleiche Wirkung *e* hervorgebracht, so hat eben damit *c* zu seyn aufgehört; *c* ist zu *e* geworden; wäre nach der Hervorbringung von *e*, *c* ganz oder einem Theile nach noch übrig, so müfste dieser rückbleibenden Ursache noch weitere Wirkung entsprechen, die Wirkung von *c* überhaupt also e ausfallen, was gegen die Voraussetzung $c = e$. Da mithin *c* in *e*, *e* in *f* u. s. w. übergeht, so müssen wir diese Gröfsen als verschiedene Erscheinungsformen eines und desselben Objectes betrachten. Die Fähigkeit, verschiedene Formen annehmen zu können, ist die zweite wesentliche Eigenschaft aller Ursachen. Beide Eigenschaften zusammengefafst sagen wir: Ursachen sind (quantitativ) *unzerstörliche* und (qualitativ) *wandelbare* Objecte.

Zwei Abtheilungen von Ursachen finden sich in der Natur vor, zwischen denen erfahrungsmäfsig keine Uebergänge stattfinden. Die eine Abtheilung bilden die Ursachen, denen die Eigenschaft der Ponderabilität und Impenetrabilität zukommt, — Materien; die andere die Ursachen, denen letztere Eigenschaften fehlen, — Kräfte, von der bezeichnenden negativen Eigenschaft auch Imponderabilien genannt. Kräfte sind also: *unzerstörliche, wandelbare, imponderable Objecte.*

Wir wollen zuerst die Materien zur Aufstellung eines Beispiels von Ursachen und Wirkungen benützen. Knallgas, H + O, und Wasser HO verhalten sich wie Ursache und Wirkung, also H + O = HO. Wird aus H + O, HO, so kommt aufser Wasser noch Wärme, cal. zum Vorschein; diese Wärme mufs ebenfalls eine Ursache, x, haben; es ist also: H + O + x = H + cal.; es könnte sich nun fragen, ist wirklich H + O = HO, und x = cal., und nicht etwa H + O = cal., und x = HO, worauf sich aus obiger Gleichung ebenfalls schliefsen liefse u. dgl. m. Die Phlogistiker erkannten die Gleichung von cal. u. x das sie Phlogiston nannten, und thaten damit einen grofsen Schritt vorwärts, verwickelten sich aber wieder dadurch in ein System von Irrthümern, dafs sie statt O, — x setzten, also beispielsweise H = HO + x erhielten.

Die Chemie, deren Gegenstand es ist, den zwischen den Materien stattfindenden ursächlichen Zusammenhang in Gleichungen zu entwickeln, lehrt uns, dafs einer Materie als Ursache eine Materie als Wirkung zukomme; aber mit gleichem Rechte kann man auch sagen, dafs einer Kraft als Ursache, eine Kraft als Wirkung entspreche. Da $c = e$, und $e = c$, so ist es naturwidrig, das eine Glied, wo Gleichung eine Kraft, das andere eine Wirkung von Kraft oder Erscheinung zu nennen, und an die Ausdrücke Kraft und Erscheinung verschiedene Begriffe zu knüpfen; kurz also: ist die Ursache eine Materie, so ist auch die Wirkung eine solche; ist die Ursache eine Kraft, so ist auch die Wirkung eine Kraft.

Eine Ursache, welche die Hebung einer Last bewirkt, ist eine Kraft; ihre Wirkung, *die gehobene Last*, ist also ebenfalls *eine Kraft;* allgemeiner ausgedrückt heifst dies: *räumliche Differenz ponderabler Objecte ist eine Kraft;* da diese Kraft den Fall der Körper bewirkt, so nennen wir sie *Fallkraft*. Fallkraft und Fall, und allgemeiner noch Fallkraft und Bewegung sind Kräfte, die sich verhalten wie Ursache uad Wirkung, Kräfte, die in einander übergehen, zwei verschiedene Erscheinungsformen eines und desselben Objectes. Beispiel: eine auf dem Boden ruhende Last ist keine Kraft; sie ist weder Ursache einer Bewegung, noch der Hebung einer andern Last, wird diefs aber in dem Mafse, in welchem sie über den Boden gehoben wird; die Ursache, der Abstand einer Last von der Erde und die Wirkung, das erzeugte Bewegungsquantum stehen, wie die Mechanik weifs, in einer beständigen Gleichung.

Indem man die Schwere als Ursache des Falls betrachtet, spricht man von einer Schwerkraft und verwirrt so die Begriffe von Kraft und Eigenschaft; gerade das, was jeder Kraft wesentlich zukommen mufs, die *Vereinigung* von Unzerstörlichkeit und

Wandelbarkeit, geht jedweder Eigenschaft ab; zwischen einer Eigenschaft und einer Kraft, zwischen Schwere uud Bewegung läfst sich defshalb auch nicht die für ein richtig gedachtes Causalverhältnifs nothwendige Gleichung aufstellen. Heifst man die Schwere eine Kraft, so denkt man sich damit eine Ursache, welche, ohne selbst abzunehmen, Wirkung hervorbringt, hegt damit also unrichtige Vorstellungen über den ursächlichen Zusammenhang der Dinge. Um dafs ein Körper fallen könne, dazu ist seine Erhebung nicht minder nothwendig, als seine Schwere, man darf daher auch letzterer allein den Fall der Körper nicht zuschreiben.

Es ist der Gegenstand der Mechanik, die zwischen Fallkraft und Bewegung, Bewegung und Fallkraft, und die zwischen den Bewegungen unter sich bestehenden Gleichungen zu entwickeln; wir erinnern hier nur an einen Punkt. Die Gröfse der Fallkraft v steht — den Erdhalbmesser $= \infty$ gesetzt — mit der Gröfse der Masse m und mit der ihrer Erhebung d, in geradem Verhältnisse; $v = md$. Geht die Erhebung $d = 1$ der Masse m in Bewegung dieser Masse von der Endgeschwindigkeit $c = 1$ über, so wird auch $v = mc$; aus den bekannten zwischen d und c stattfindenden Relationen ergiebt sich aber für andere Werthe von d oder c, mc^2 als das Mafs der Kraft v; also $v = md = mc^2$; das Gesetz der Erhaltung lebendiger Kräfte finden wir in dem allgemeinen Gesetze der Unzerstörbarkeit der Ursachen begründet.

Wir sehen in unzähligen Fällen eine Bewegung aufhören, ohne dafs letztere eine andere Bewegung, oder eine Gewichtserhebung hervorgebracht hätte; eine einmal vorhandene Kraft kann aber nicht zu Null werden, sondern nur in eine andere Form übergehen und es fragt sich somit, welche weitere Form die Kraft, welche wir als Fallkraft und Bewegung kennen gelernt, anzunehmen fähig sey? Nur die Erfahrung kann uns hierüber Aufschlufs ertheilen. Um zweckmäfsig zu experimentiren, müssen wir Werkzeuge wählen, welche neben dem, dafs sie eine Bewegung wirklich zum Aufhören bringen, von den zu untersuchenden Objecten möglichst wenig verändert werden. Reiben wir z. B. zwei Metallplatten an einander, so werden wir Bewegung verschwinden, Wärme dagegen auftreten sehen und es fragt sich jetzt nur, ist die *Bewegung* die Ursache von Wärme. Um uns über dieses Verhältnifs zu vergewissern, müssen wir die Frage erörtern, hat nicht in den zahllosen Fällen, in denen unter Aufwand von Bewegung Wärme zum Vorschein kommt, die Bewegung eine andere Wirkung als die Wärmeproduktion und die Wärme eine andere Ursache als die Bewegung?

Ein Versuch, die Wirkungen der aufhörenden Bewegung nachzuweisen, wurde noch nie ernstlich angestellt; ohne die möglicherweise aufzustellenden Hypothesen zum Voraus wiederlegen zu wollen, machen wir nur darauf aufmerksam, dafs diese Wirkung in eine Veränderung des Aggregationszustandes der bewegten, sich reibenden x x Körper in der Regel nicht gesetzt werden könne. Nehmen wir an, es werde ein gewisses Quantum von Bewegung v, dazu verwendet, eine reibende Materie m, in n zu verwandeln, so müfste $m + v = n$, und $n = m + v$ evn, und bei der Rückführung von n in m müfste v in irgend eine Form wieder zu Tage kommen. Durch sehr lange fortgesetztes Reiben zweier Metallplatten können wir nach und nach ein ungeheures Quantum von Bewegung zum Aufhören bringen: kann uns aber beifallen, in dem gesammelten Metallstaub auch nur eine Spur der entschwundenen Kraft wieder finden, und daraus reduciren zu wollen? Zu Nichts, wir wiederholen, kann die Bewegung nicht geworden seyn und entgegengesetzte, oder positive und negative Bewegungen können nicht $= 0$ gesetzt werden, so wenig aus 0 entgegengesetzte Bewegungen entstehen können, oder ein Last sich von selbsten hebt.

So wenig sich, ohne Anerkennung eines ursächlichen Zusammenhanges zwischen Bewegung und Wärme, von der entschwundenen Bewegung irgend Rechenschaft geben läfst, so wenig läfst sich auch ohne jene die Entstehung der Wärme erklären. Aus der Volumensverminderung der sich reibenden Körper kann dieselbe nicht hergeleitet werden. Man kann bekanntlich durch Zusammenreiben zwei Eisstücke im luftleeren Raume schmelzen; man versuche nun, ob man durch den unerhörtesten Druck Eis in Wasser verwandeln könne? Wasser erfährt, wie der Verfasser fand, durch starkes Schütteln eine Temperaturerhöhung. Das erwärmte Wasser (von 12° und 13° C.) nimmt nach dem Schütteln ein gröfseres Volumen ein, als vor demselben; woher kommt nun die Wärmemenge, welche sich durch wiederholtes Schütteln in demselben Apparate beliebig oft hervorbringen läfst? Die thermische Vibrationshypothese inclinirt zu dem Satze, dafs Warme die Wirkung von Bewegung sey, wurdigt aber dieses Causalverhältnifs im vollen Umfange nicht, sondern legt das Hauptgewicht auf unbehagliche Schwingungen.

Ist es nun ausgemacht, dafs für die verschwindende Bewegung in vielen Fällen (exceptio confirmat regulam) keine andere Wirkung gefunden werden kann, als die Wärme, für die entstandene Wärme keine andere Ursache als die Bewegung, so ziehen wir die Annahme, Wärme entsteht aus Bewegung, der Annahme einer Ursache ohne Wirkung und einer Wirkung ohne Ursache vor, wie der Chemiker statt H und O ohne Nachfrage verschwinden, und Wasser auf unerklärte Weise entstehen zu lassen, einen Zusammenhang zwischen H und O einer- und Wasser anderseits statuirt.

Den natürlichen, zwischen Fallkraft, Bewegung und Wärme bestehenden Zusammenhang können wir uns auf folgende Weise anschaulich machen. Wir wissen, dafs Wärme zum Vorschein kommt, wenn die einzelnen Massentheile eines Körpers sich näher rücken; Verdichtung erzeugt Wärme; was aun für die kleinsten Massentheile und ihre kleinsten Zwischenräume gilt, mufs wohl auch seine Anwendung auf grofse Massen und mefsbare Räume finden. Das Herabsinken einer Last ist eine wirkliche Volumensverminderung des Erdkörpers, mufs also gewifs mit der dabei sich zeigenden Wärme im Zusammenhange stehen: diese Wärme wird der Gröfse der Last und ihrem Abstande genau proportional seyn müssen. Von dieser Betrachtung wird man ganz einfach zu der besprochenen Gleichung von Fallkraft. Bewegung und Wärme geführt.

So wenig indessen aus dem zwischen Fallkraft und Bewegung bestehenden Zusammenhange geschlossen werden kann: das Wesen der Fallkraft sey Bewegung, so wenig gilt dieser Schlufs für die Wärme. Wir möchten vielmehr das Gegentheil folgern. dafs um zu Wärme werden zu können, die Bewegung, — sey sie eine einfache, oder eine vibrirende, wie das Licht, die strahlende Wärme etc., — aufhören müsse, Bewegung zu seyn.

Wenn Fallkraft und Bewegung gleich Wärme, so mufs natürlich auch Wärme gleich Bewegung und Fallkraft seyn. Wie die Wärme als Wirkung entsteht, bei Volumsverminderung und aufhörender Bewegung, so verschwindet die Wärme als Ursache unter dem Auftreten ihrer Wirkungen, der Bewegung, Volumsvermehrung, Lasterhebung.

In den Wasserwerken liefert die, auf Kosten der Volumensverminderung, welche der Erdkörper durch den Fall des Wassers beständig erleidet, entstehende und wieder verschwindende Bewegung, fortwährend eine bedeutende Menge von Wärme: umgekehrt dienen wieder die Dampfmaschinen zur Zerlegung der Wärme in Bewegung oder Lasterhebung. Die Locomotive mit ihrem Convoi ist einem Destillirapparate zu vergleichen; die unter dem Kessel angebrachte Wärme geht in Bewegung über, und diese setzt sich wieder an den Axen der Räder als Wärme in Menge ab.

Wir schliefsen unsere Thesen, welche sich mit Nothwendigkeit aus dem Grundsatze „causa aequat effectum“ ergeben und mit allen Naturerscheinungen im vollkommenen Einklange stehen, mit einer praktischen Folgerung. — Zur Auflösung der zwischen

Fallkraft und Bewegung stattfindenden Gleichungen mufste der Fallraum für eine bestimmte Zeit, z. B. für die erste Secunde durch das Experiment bestimmt werden; gleichermafsen ist zur Auflösung der zwischen Fallkraft und Bewegung einer- und der Wärme anderseits bestehenden Gleichungen die Frage zu beantworten, wie grofs das einer bestimmten Menge von Fallkraft oder Bewegung entsprechende Wärmequantum sey. Z. B. wir müssen ausfindig machen, wie hoch ein bestimmtes Gewicht über den Erdboden erhoben werden müsse, dafs seine Fallkraft aequivalent sey der Erwärmung eines gleichen Gewichtes Wasser von 0° auf 1° C.? Dafs eine solche Gleichung wirklich in der Natur begründet sey, kann als das Resume des bisherigen betrachtet werden.

Unter Anwendung der aufgestellten Sätze auf die Wärme- und Volumensverhältnisse der Gasarten findet man die Senkung einer ein Gas comprimirenden Quecksilbersäule gleich der durch die Compression entbundenen Wärmemenge und es ergiebt sich hieraus, — den Verhältnifsexponenten der Capacitäten der atmosphärischen Luft unter gleichem Drucke und unter gleichem Volumen = 1,421 gesetzt, dafs dem Herabsinken eines Gewichtstheiles von einer Höhe circa 365m, die Erwärmung eines gleichen Gewichttheiles Wasser von 0° auf 1° entspreche. Vergleicht man mit diesem Resultate die Leistungen unserer besten Dampfmaschinen, so sieht man, wie nur ein geringer Theil der unter dem Kessel angebrachten Wärme in Bewegung oder Lasterhebung wirklich zersetzt werde und dies könnte zur Rechtfertigung dienen, für die Versuche, Bewegung auf anderem Wege als durch Aufopferung der chemischen Differenz von C und O, namentlich also durch Verwandlung der auf chemischem Wege gewonnenen Elektricität in Bewegung, auf erspriefsliche Weise darstellen zu wollen.

Ausgegeben am 31ten Mai 1842.

Facsimile No. vi : J. P. Joule. Photographed from the Report of the meeting of the British Association, Cork, 1843. Transactions of the Sections, p. 33.

On the Calorific Effects of Magneto-Electricity, and the Mechanical Value of Heat. By J. P. JOULE.

Although it had been long known that fine platinum wire can be ignited by magneto-electricity. it still remained a matter of doubt whether heat was evolved by the *coils* in which the magneto-electricity was generated: and it seemed indeed not unreasonable to suppose that *cold* was produced there, in order to make up for the heat evolved by the other parts of the circuit. The author had endeavoured therefore to clear up this uncertainty by experiment. His apparatus consisted of a small compound electro-magnet, immersed in water, revolving between the poles of a powerful stationary magnet. The magneto-electricity developed in the coils of the revolving electro-magnet was measured by an accurate galvanometer; and the temperature of the water was taken before and after each experiment by a very delicate thermometer. The influence of the temperature of the surrounding atmospheric air was guarded against by covering the revolving tube with flannel, &c., and by the adoption of a system of interpolation. By an extensive series of experiments with the above apparatus the author succeeded in proving that heat is evolved by the coils of the magneto-electrical machine, as well as by any other part of the circuit, in proportion to the resistance to conduction of the wire and the square of the current; the magneto-, having, under comparable circumstances, the same calorific power as the voltaic electricity. Prof. Jacobi, of St. Petersburgh, had shown that the motion of an electro-magnetic engine generates magneto-electricity in opposition to the voltaic current of the battery. The author had observed the same phænomenon on arranging his apparatus as an electro-magnetic engine; but had found that no additional heat was evolved on account of the conflict of forces in the coil of the revolving electro-magnet, and that the heat evolved by the coil remained, as before, proportional to the square of the current. Again, by turning the machine contrary to the direction of the attractive forces, so as to *increase* the intensity of the voltaic current by the assistance of the magneto-electricity, he found that the evolution of heat was still proportional to the square of the current. The author discovered, therefore, that the heat evolved by the voltaic current is invariably proportional to the square of the current, however the intensity of the current may be varied by magnetic induction. But Dr. Faraday had shown that the chemical effects of the current are simply as its quantity. Therefore he concluded that in the electro-magnetic engine, a part of the heat due to the chemical actions of the battery is lost by the circuit, and converted into mechanical power; and that when the electro-magnetic engine is turned *contrary* to the direction of the attractive forces, a *greater* quantity of heat is evolved by the circuit than is due to the chemical reactions of the battery, the overplus quantity being produced by the conversion of the mechanical force exerted in turning the machine. By a dynamometrical apparatus attached to his machine, the author has ascertained that, in all the above cases, a quantity of heat, capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale, is equal to a mechanical force capable of raising a weight of about 838 pounds to the height of one foot.

Facsimile No. vii : Sadi Carnot. Manuscript note in the Archives of the Institut, Paris. Date c. 1824-32.

La chaleur n'est autre chose que la puissance motrice ou plutôt que le mouvement, qui a changé de forme. C'est un mouvem^t dans les particules des corps partout où il y a destruction de p. m. il y a en même temps production de chaleur en quantité précisément proportionnelle à la q^té de p. m. détruite. Réciproquement partout où il y a destruction de chaleur il y a production de p. m.

On peut donc poser en thèse générale que la p. m. est en quantité invariable dans la nature qu'elle n'est jamais à proprement parler ni produite, ni détruite, ~~qu'elle~~ qu'à la vérité elle change de forme c. a. d. qu'elle produit tantôt un genre de mouvement tantôt un autre ~~mais qu'elle existe toujours~~ mais elle n'est jamais anéantie.

D'après quelques idées que je me suis formées sur la théorie de la chaleur, la production d'une unité de puissance motrice nécessite la destruction de 2,70 unités de chaleur.

Une machine qui produirait 20 unités de p. m. par kilog. de charbon devrait anéantir $\frac{20 \cdot 2,70}{7000}$ de la chaleur développée par la combustion; $\frac{20 \cdot 2,7}{7000} = \frac{8}{1000}$ environ c. à d. moins de $\frac{1}{100}$