Sadi Carnot

Reflections on the Motive Power of Heat

1825

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REFLECTIONS ON THE

MOTIVE POWER OF HEAT

Sadi Carnot at the age of 17 according to a portrait painted by Bailly in 1813 and published in the edition of 1878 by Gauthier-Villars, Paris.

REFLECTIONS ON THE

MOTIVE POWER

OF HEAT

AND ON MACHINES FITTED TO

DEVELOP THIS POWER

BY

S. CARNOT

TRANSLATED BY

R. H. THURSTON

Late *Director of Sibley College, Cornell University*

First President, A.S.M.E.

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1943

REFLEXIONS

SUR LA

PUISSANCE MOTRICE

DU FEU

SUR LES MACHINES

PROPRES A DEVELOPPER CETTE PUISSANCE.

Par S. CARNOT,

ANCIEN ELEVE DE L'ECOLE POLYTECHNIQUE.

A PARIS,

CHEZ BACHELIER, LIBRAIRE.

QUAI DES AUGUSTINS, N⁰. 55.

1824

Title Page of the Edition of 1824

DEDICATED TO

SADI CARNOT

President of the French Republic, 1887-1894

THAT distinguished member of the profession of engineering whose whole life has been an honor to his profession and to his country; and who, elevated to the highest office within the gift of the French Nation, has proven by the quiet dignity and the efficiency with which he has performed his august duties that he is a worthy member of a noble family, already rendered famous by an earlier Sadi Carnot, now immortal in the annals of science, and is himself deserving of enrollment in a list of great men which includes that other distinguished engineer, our own first president, George Washington.

FOREWORD

The Carnot Cycle! What memories this name must stir in every engineer! To some it will recall early struggles to comprehend the nature of heat engines; to others it represents the ideal of performance sought after in design and operation. Many will recall the keen delight that came from a thorough understanding of the Carnot cycle which forms one of the principal foundations of thermodynamics. Every student of this subject can now write the elementary principles of this cycle in our modern terms of heat and thermodynamics. How many have given a thought to the conception and development of the idea when many of the laws of physics as now stated were either imperfectly known or not yet discovered? What could be more interesting than to read the thoughts of the originator of the Carnot cycle and to follow his reasonings that led to his great discovery?

The pages that follow contain a reprint of a book written by Dr. Robert H. Thurston and published in 1890 by John Wiley and Sons, to whom acknowledgement is made for permission to reprint this manuscript. The text contains an introduction by Dr. Thurston followed by his translation from the French edition published by Gauthier-Villars of Sadi Carnot's brochure on "Reflexions sur la Puissance Motrice du Feu," together with a sketch of Sadi Carnot's brief life written by his brother Henri and appearing in the same edition.

The translator, Dr. Thurston, one of the founders of The American Society of Mechanical Engineers and its first President, was one of America's outstanding engineers. His writings influenced in a large measure the development of mechanical engineering in the English-speaking world. When Dr. Thurston writes of Sadi Carnot in the first sentence of his introduction

-"Carnot was perhaps the greatest genius in the department of physical science at least that this century has produced"-, one receives a new conception of the stature of Carnot which is further expanded in Dr. Thurston's later paragraphs. Our admiration of Carnot's genius increases when it is realized that this essay was published at an age of twenty-eight so that he must have conceived the idea at a still earlier age; Dr. Thurston suggests at twenty-four.

Henri Carnot's short account of his brother's life enhances further one's appreciation of the discoverer of the Carnot cycle. Sadi Carnot was born, reared and educated during the turbulent times of the French Revolution and Napoleonic era. In fact, his father had a part in these stirring events. He was trained for the army and studied military engineering. One can only conjecture as to the causes that turned his thoughts to the principles underlying the operation of heat engines. Perhaps then as now, war was directing people's thoughts from common-place affairs and stirring the imagination in fields of endeavor never before considered.

Carnot's "Reflexions sur La Puissance Motrice du Feu" appeared first as a small book published in Paris in 1824. In Carnot's time, the nature of heat as now understood, was not generally known. Carnot employs the older conception of heat as a substance that is present everywhere which he calls "caloric". If "quantity of heat" is used in place of caloric, his ideas become more plain. Fortunately, as Dr. Thurston points out, his reasoning and conclusions are independent of the nature of heat and thus his analysis holds true under our later ideas. Also, as Dr. Thurston states, Carnot did not conceive that a portion of the heat was converted into work. The discovery of the relation of heat and work by Joule came later. Some of Carnot's ideas of specific heats may also need revision in view of later physical discoveries.

An engineer will find much of secondary interest in Carnot's book. For instance, he seemed famliar with the development of the steam engine by English engineers and the efficiencies obtained. He speaks of the highest steam pressures as 6 atmospheres (75 lb gage) and condensation temperatures of 40^0 C (1040^0 F) corresponding to 27.8 inches vacuum. Carnot had a clear conception of the value of feed water heating which he describes in several paragraphs. One is surprised to find in the later part of his essay a clear conception of the cycle of the combustion gas turbine, which is only now undergoing commercial development. combustion gas turbine.

Carnot was a real scientist and a study of his text serves to increase one's appreciation of his clear conception of the heat cycle in spite of lack of physical and experimental data. We do him honor by the term "Carnot cycle" applied to the principles that he developed.

In closing, I express the deep appreciation of the Society to one of our fellow members, Mr. Robert H. Roy of Baltimore, Md., who personally undertook to set up and print this edition and thus to make again available an intensely interesting book of historical value to engineers.

A. G.

The Johns Hopkins University

November 28, 1942

THE WORK OF

SADI CARNOT

NICOLAS-LEONARD-SADI CARNOT was, perhaps, the greatest genius, in the department of physical science at least, that this century has produced. By this I mean that he possessed in highest degree that combination of the imaginative faculty with intellectual acuteness, great logical power and capacity for learning, classifying and organizing in their proper relations, all the facts, phenomena, and laws of natural science which distinguishes the real genius from other men and even from the simply talented man. Only now and then, in the centuries, does such a genius come into view. Euclid was such in mathematics; Newton was such in mechanics; Bacon and Compte were such in logic and philosophy; Lavoisier and Davy were such in chemistry; and Fourier, Thomson, Maxwell, and Clausius were such in mathematical physics. Among engineers, we have the examples of Watt as inventor and philosopher, Rankine as his mathematical complement, developing the theory of that art of which Watt illustrated the practical side; we have Hirn as engineer- experimentalist, and philosopher, as well; Corliss as inventor and constructor; and a dozen creators of the machinery of the textile manufactures, in which, in the adjustment of cam-work, the highest genius of the mechanic appears.

But Carnot exhibited that most marked characteristic of real genius, the power of applying such qualitites as I have just enumerated to great purposes and with great result while still a youth. Genius is not dependent, as is talent, upon the ripening and the growth of years for its prescience; it is ready at the earliest maturity, and sometimes earlier, to exhibit its marvelous works; as, for example, note Hamilton the mathematician and Mill the logician; the one becoming master of a dozen languages when hardly more than as many years of age, reading Newton's Principia at sixteen and conceiving that wonderful system, quaternions, at eighteen; the other competent to begin the study of Greek at three, learning Latin at seven and reading Plato before he was eight. Carnot had done his grandest work of the century in his province of thought, and had passed into the Unseen, at thirty-six; his one little volume, which has made him immortal, was written when he was but twenty-three or twenty-four. It is unnecessary, here, to enter into the particulars of his life; that has been given us in ample detail in the admirable sketch by his brother Henri, herewith published. It will be quite sufficient it to indicate, in a few words, what were the conditions amid which he lived and the relation of his work to that great science of which it was the first exposition.

At the time of Carnot, the opinion of the scientific world was divided, as it had been for centuries, on the question of the true nature of heat and light, and as it still is, to a certain extent, regarding electricity. On the one hand it was held by the best-known physicists that heat is a substance which pervades all bodies in greater or less amount, and that heating and cooling are simply the absorption and the rejection of this "imponderable substance" by the body affected; while, on the other hand, it was asserted by a small but increasing number that heat is a "mode of motion," a form of energy, not only imponderable, but actually immaterial; a quality of bodies, not a substance, and that it is identical, in its nature, with other forms of recognizable energy, as, for example, mechanical energy. A quarter of a century before Carnot wrote, the experiments of Rumford and of Davy had been crucial in the settlement of the question and in the proof of the correctness of the second of the two opposing parties; but their work had not become so generally known or so fully accepted as to be acknowledged as representative of the right views on the subject. The prevalent opinion, following Newton, was favorable to the first hypothesis; and it was in deference to this opinion that Carnot based his work on an in-accurate hypothesis;

though, fortunately, the fact did not seriously militate against its value or his credit and fame.

"With true philosophical caution, he avoids committing himself to this hypothesis; though he makes it the foundation of his attempt to discover how work is produced from heat."*

The results of Carnot's reasoning are, fortunately, mainly in-dependent of any hypothesis as to the nature of heat or the method or mechanism of development and transfer or transfor- mation of its energy. Carnot was in error in assuming no loss of heat in a completed cycle and in thus ignoring the permanent transformation of a definite proportion into mechanical energy; but his proposition that efficiency increases with increase of temperature-range is still correct; as is his assertion of its independence of the nature of the working substance.

Carnot's *"Reflexions sur la Puissance Motrice du Feu,"* published in 1824, escaped notice at the time, was only now and then
* Tait: Thermodynamics, p.13.

slightly referred to later, until Clapeyron seized upon its salient ideas and illustrated them by the use of the Watt diagram of energy, and might, perhaps, have still remained unknown to the world except for the fact that Sir William Thomson, that greatest of modern mathematical physicists, fortunately, when still a youth and at the commencement of his own great work, discovered it, revealed its extraordinary merit, and, readjusting Carnot's principles in accordance with the modern views of heat energy, gave it the place that it is so well entitled to in the list of the era-making books of the age. But it still remained inac-cessible to all who could not find the original paper until, only a few years since, it was reprinted by Gauthier-Villars, the great publishing house of Paris, accompanied by a biographical sketch by the younger brother, which it has been thought wise to reproduce with the translation of Carnot's book. In making the translation, also, this later text has been followed; and now, for the first time, so far as is known to the writer, the work of Carnot is made accessible to the reader in English.

The original manuscript of Carnot has been deposited by his brother in the archives of the French Academy of Sciences, and thus insured perpetual care. The work of Carnot includes not only the treatise which it is the principal object of this translation to give to our readers, but also a considerable amount of hitherto unpublished matter which has been printed by his brother, with the new edition of the book, as illustrative of the breadth and acuteness of the mind of the Founder of the Science of Thermodynamics.

These previously unpublished materials consist of memoranda relating to the specific heats of substances, their variations, and various other facts and data, and principles as well; some of which are now recognized as essential elements of the new science, even of its fundamental part. The book is particularly rich in what have been generally supposed to be the discoveries of later writers, and in enunciations of principles now recognized as those forming the base and the supporting framework of that latest of the sciences. As stated by Tait, in his history of Thermodynamics, the "two grand things" which Carnot originated and introduced were his idea of a "cycle" and the notion of its "reversibility," when perfect. "Without this work of Carnot, the modern theory of energy, and especially that branch of it which is at present by far the most important in practice, the dynamical theory of heat, could not have attained its now enormous development." These conceptions, original with our author, have been, in the hands of his successors, Clausius and other Continental writers, particularly, most fruitful of interesting and important results; and Clapeyron's) happy thought of so employing the Watt diagram of energy as to render them easy of comprehension has proved a valuable aid in this direction.

The exact experimental data needed for numerical computations in application of Carnot's principles were inaccessible at the date of his writing; they were supplied, later, by Mayer, by Colding, by Joule, and by later investigators. Even the idea of equivalence, according to Mon. Henri Carnot, was not originally familiar to the author of this remarkable work; but was gradually developed and defined as he progressed with his philosophy. It is sufficiently distinctly enunciated in his later writings. He then showed a familiarity with those notions which have been ascribed generally to Mayer and which made the latter famous, and with those ideas which are now usually attributed to Joule with similar result. He seems actually to have planned the very kind of research which Joule finally carried out. All these advanced views must, of course, have been developed by Carnot before 1832, the date of his illness and death, and ten or fifteen years earlier than they were made public by those who have since been commonly considered their discoverers. These until lately unpublished notes of Carnot contain equally well-constructed arguments in favor of the now accepted theory of heat as energy. While submitting to the authority of the greatest physicists of his time, and so far as to make their view the basis of his work, to a certain extent, he nevertheless adhered privately to the true idea. His idea of the equivalence of heat and other forms of energy was as distinct and exact as was his notion of the nature of that phenomenon. He states it with perfect accuracy.

In making his measures of heat-energy, he assumes as a unit a measure not now common, but one which may be' easily and conveniently reduced to the now general system of measurement. He takes the amount of power required to exert an energy equal to that needed to raise one cubic meter of water through a height of one meter, as his unit; this is 1000 kilogrammeters, taken as his unit of motive power; while he says that this is the equivalent of 2.7 of his units of heat; which latter quantity would be destroyed in its production of this amount of power, or rather work. His unit of heat is thus seen to be 1000 2.7, or 370 kilogrammeters. This is almost identical with the figure obtained by Mayer, more than ten years later, and from presumably the same approximate physical data, the best then available, in the absence of a Regnault to determine the exact values. Mayer obtained 365, a number which the later work of Regnault enabled us to prove to be 15 per cent too low, a conclusion verified experimentally by the labors of Joule and his successors. Carnot was thus a discoverer of the equivalence of the units of heat and work, as well as the revealer of the principles which have come to be known by his name. Had he lived a little longer, there can be little doubt that he would have established the facts, as well as the principles, by convincing proof. His early death frustrated his designs, and deprived the world of one of its noblest intellects, just when it was beginning its marvellous career.

The following sentence from Carnot illustrates in brief his wonderful prescience; one can hardly believe it possible that it should have been written in the first quarter of the nineteenth century: *"On peut donc poser en these generale que la puissance motrice est en quantite' invariable dans la Nature; qu'elle n'est jamais, a proprement parler, ni produite, ni detruite. A la verite, elle change de forme, c'est a dire qu'elle produit tantot un genre de mouvement, tantot un autre; mais elle n'est jamais aneantie."* It is this man who has probably inaugurated the development of the modern science of thermodynamics and the whole -range of sciences dependent upon it, and who has thus made it possible to construct a science of the energetics of the universe, and to read the mysteries of every physical phenomenon of nature; it is this man who has done more than any contemporary in his field, and who thus displayed a more brilliant genius than any man of science of the nineteenth century: yet not even his name appears in the biographical dictionaries; and in the Encyclopedia Britannica it is only to be found incidentally in the article on Thermodynamics.

Throughout his little book, we find numerous proofs of his clearness of view and of the wonderful powers of mind possessed by him. He opens his treatise by asserting that *"C'est a la chaleur que doivent etre attribues' les grands mouvements gui frappent nos regards sur la terre; c'est a elle que sont dues les agitations de*

l'atmosphere, l'ascension des nuages, la chut des pluies et des autres meteores, les courants d'eau qui sillonnent la surface du globe et dont l'homme est parvenue a employer pour son usage une faible partie; enfin les tremblements de terre, les eruptions volcanigue reconnaissent aussi pour cause le chaleur."

Carnot was the first to declare that the quantity of work done by heat, in any given case of application in the heat-energy, is determined solely by the range of temperature through which it fell in the operation, and is entirely independent of the nature of the working substance chosen as the medium of transfer of energy and the vehicle of the heat. His assumption of the materiality of heat led, logically, to the conclusion that the same quantity of heat was finally stored in the refrigerator as had, initially, left the furnace, and that the effect produced was a consequence of a fall of temperature analogous to a fall of water; but, aside from this error-which he himself was evidently inclined to regard as such, - his process and argument are perfectly correct.*

- **Account of Carnot's Theory of the Motive Power of Heat; Sir Wm. Thomson; Trans. Roy. Soc. of Edinburgh, xvi, 1849; and Math. and Phys. Papers, xli. vol. 1 (Cambridge, 1882), p.113. In this paper the corrections due to the introduction of the dynamic theory are first applied.**

Throughout his whole work are distributed condensed assertions of principles now well recognized and fully established, which indicate that he not only had anticipated later writers in their establishment, but that he fully understood their real importance in a theory of heat-energy and of heat engines. In fact, he often italicizes them, placing them as independent paragraphs to more thoroughly impress the reader with their fundamental importance. Thus he says:

"Partout ou' il existe une differnce de temperature, il peut y avoir production de puissance motrice;" and again, this extra-anticipation of modern science: *"le maximum de puissance resultant de l'emploi de la vapeur est aussi le maximum de puissance motrice realisab1e par quelque moyen que ce soit."*

"La puissance motrice de la chaleur est independante des agents mis en oeuvre pour la realiser; sa guantite est fixee uniguement par les temperatures des corps entre lesquels se fait, en dernier resultat, le transport du calorique."

"Lorsqu'un gaz passe, sans changer de temperature, d'un volume et d'une pression determine a un autre pression egalement determinees, le quantite de calorique absorbee ou abandonnee est toujours la meme, quelle que soit la nature du gaz choisi comme sujet d'experience."

Perhaps as remarkable a discovery as any one of the preceding (and one which, like those, has been rediscovered and confirmed by later physicists; one which was the subject of dispute between Clausius, who proved its truth by the later methods which are now the source of his fame, and the physicists of his earlier days, who had obtained inaccurate measures of the specific heats of the gases;-values which were finally corrected by Regnault, thus proving Carnot and Clausius to be right) is thus stated by Carnot, and is italicized in his manuscript and book:

"La difference entre le chaleur specifique sous pression constante et la chaleur specifique sous volume constant est la meme pour tous les gaz."

He bases his conclusion upon the simplest of thermodynamic considerations. He says that the increase of

volumes with the same differences of temperature are the same, according to Gay Lussac and Dalton; and that, therefore, according to the laws of thermodynamics as he has demonstrated them, the heat absorbed with equal augmentations of volume being the same, the two specific heats are constant, and their difference as well. As will be seen on referring to the text, he bases upon this principle a determination of the specific heats of constant volume, taking as his values of the determined specific heats of constant pressure those of Delaroche and Brard, making the constant difference 0.300, that of air at constant pressure being taken as the standard and as unity. The establishment of this point, in the face of the opposition, and apparently of the facts, of the best physicists of his time, was one of those circumstances which did so much to win for Clausius his great fame. How much greater credit, then, should be given Carnot, who not only anticipated the later physicists in this matter, but who must have enunciated his principle under far more serious discouragements and uncertainty!

It must be remembered, when reading Carnot, that all the "constants of nature" were, in his time, very inaccurately ascertained. It is only since the time of Regnault's grand work that it has been the rule that such determinations have been published only when very exactly determined. No change has been attempted in Carnot's figures, in any respect; as it would be far less satisfactory to read a paraphrased work, and the exact figures are now easily accessible to every one, and his computations may all be made, if desired, on the basis of modern data. Sir William Thomson has already performed this task in the paper appended.

Throughout the whole of this treatise, small as it is, we find distributed a singular number of these anticipations of modern thermodynamic principles. Studying the relation of heat-energy to work done, he concludes:

"La chute du calorique produit plus de puissance motrice dans les degres inferieurs que dans les degres superieurs."

We today admit that, since the one degree at a low temperature, and the corresponding quantity of heat, are larger fractions of the total temperature, and the total heat stored in the substance, than the one degree at a higher point on the scale of absolute temperature, this principle of Carnot has become obvious.

In the enunciation of the essential principles of efficiency of the heat-engine, we find the proofs of this same wonderful prescience. He asserts that, for best effect: "(1) The temperature of the working fluid must be raised to the highest degree possible, in order to secure a commensurate range of temperature; (2) The cooling must be carried to the lowest point on the scale that may be found practicable; (3) The passage of the fluid from the upper to the lower limit of temperature must be produced by expansion;" i.e., "it is necessary that the cooling of the gas shall occur spontaneously by its rarefaction;" which is simply his method of stating the now universally understood principle that, for highest efficiency, the expansion must be adiabatic, from a maximum to a minimum temperature. He goes on to explain these principles, and then says that the advantage of high-pressure engines lies *"essentiellement dans la faculte de rendre utile une plus grand chute de calorique."* This principle, as a practical system of operation, had already, as he tells us, been enunciated by M. Clement, and had been practised, as we well know, since the days of its originator; Watt; but Carnot saw clearly the thermodynamic principle which underlies it, and as clearly states it, for the first time. He sees clearly, too, the reasons for the attempts of Horn-blower and of Woolf, premature as they proved and as he also sees, in the introduction of the compound engine, and even suggests that this idea might be still further developed by the use of a triple-expansion engine, a type which is today just coming into use, more than a half-century after Carnot's date. He recognizes the advantages of the compound engine in better distribution of pressures and in distribution of the work of expansion, but does not, of course, perceive the then undiscovered limitation of the efficiency of the

simple engine, due to "cylinder condensation," which has finally led, perhaps more than any other circumstance, to its displacement so largely by the multi-cylinder machine. No one has more exactly and plainly stated the respective advantages to be claimed for air and the gases, used as working fluids in heat-engines, than does Carnot; nor does any one to-day better recognize the difficulties which lie in the path to success in that direction, in the necessity of finding a means of handling them at high temperatures and of securing high mean pressures. His closing paragraph shows his extraordinary foresight, and the precision with which that wonderful intellect detected the practical elements of the problem which the engineer, from the days of Savery, of Newcomen, and of Watt, has been called upon to study, and the importance of the work, which he began, in the development of a theory of the action, or of the operation, of the heat-engines, which should give effective assistance in the development of their improved forms:

"On ne doit pas flatter de mettre jamais a profit, dans la pratigue, toute la puissance des combustibles. Les tentatives que l'on ferait pour approcher ce resultat seraient meme plus nuisible qu'utile, si elles faisait negliger d'autres considerations importantes. L'economie du combustibles n'est qu'une des conditions a remplir par les machines a feu; dans beaucoup de circonstances, elle n'est que secondaire: elle doit souvent cedar le pas a la surete, a la solidite, a la duree de la machine, au peu de place qu'il faut lui faire occuper, au peu de frais de son etablissement, etc. Savoir apprecier, dans chaque cas, a leur juste valeur, les considerations de convenance et d'economie qui peuvent presenter; savoir discerner les plus importantes de celles qui sont seulement accessoires, les balancier toutes con venablement entre elles, afin de parvenir, par les moyens les plus faciles, au meilleur resultat: tel doit etre le principal talent de l'homme appele a diriger, a co-ordonner entre eux les travaux de ses semblables, a les faire concourir vers un but utile de quelque genre qu'il soit."

Such was the work and such the character of this wonderful man. Those who desire to follow more closely and to witness the process of development of the work of which this initial paper of Carnot was the introductory, should study the contribution of Sir William Thomson to this development, as published in 1849,-a paper which constitutes that physicist the virtual discoverer of Carnot and the godfather of the man and his thoughts.

From that time the additional progress so rapidly made in the new science was as inevitable as the development of a gold-field, once the precious metal has been found in paying quantities in the hitherto unvisited canons and gorges of a distant and unexplored mountain-range. But great as is the work since done, and great as have been the discoveries and the discoverers of later years, none claims our gratitude and compels our respect in greater degree than does the original discoverer-

<div align="center">SADI CARNOT</div>

R.H. Thurston

Cornell University

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THE LIFE OF

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SADI CARNOT

As THE life of Sadi Carnot was not marked by any notable event, his biography would have occupied only a few lines; but a scientific work by him, after remaining long in obscurity, brought again to light many years after his death, has caused his name to be placed among those of great inventors. In regard to his person, his mind, his character, nothing whatever has been known. Since there remains a witness of his private life the sole witness, has he not a duty to fulfill? Ought he not to satisfy the natural and legitimate interest which attaches to any man whose work has deserved a portion of glory?

Nicolas-Leonard-Sadi Carnot was born June 1, 1796, in the smaller Luxembourg. This was that part of the palace where our father then dwelt as a member of the Directory. Our father had a predilection for the name of Sadi, which recalled to his mind ideas of wisdom and poetry. His firstborn had borne this name, and despite the fate of this poor child, who lived but a few months, he called the second also Sadi, in memory of the celebrated Persian poet and moralist.

Scarcely a year had passed when the proscription, which included the Director, obliged him to give up his life, or at least his liberty, to the conspirators of fructidor. Our mother carried her son far from the palace in which violation of law had just triumphed. She fled to St. Omer, with her family, while her husband was exiled to Switzerland, then to Germany. Our mother often said to me, "Thy brother was born in the midst of the cares and agitations of grandeur, thou in the calm of an obscure retreat. Your constitutions show this difference of origin."

My brother in fact was of delicate constitution. He increased his strength later, by means of varied and judicious bodily exercises. He was of medium size, endowed with extreme sensibility and at the same time with extreme energy, more than reserved, almost rude, but singularly courageous on occasion. When he felt himself to be contending against injustice, nothing could restrain him. The following is an anecdote in illustration.

The Directory had given place to the Consulate. Carnot, after two years of exile, returned to his country and was appointed Minister of War. Bonaparte at the same time was still in favor with the republicans. He remembered that Carnot had assisted him in the beginning of his military career, and he resumed the intimate relation which had existed between them during the Directory. When the minister went to Malmaison to work with the First Consul, he often took with him his son, then about four years old, to stay with Madame Bonaparte, who was greatly attached to him.

She was one day with some other ladies in a small boat on a pond, the ladies rowing the boat themselves, when Bonaparte, unexpectedly appearing, amused himself by picking up stones and throwing them near the boat, spattering water on the fresh toilets of the rowers. The ladies dared not manifest their displeasure, but the little Sadi, after having looked on at the affair for some time, suddenly placed himself boldly before the conqueror of Marengo, and threatening him with his fist, he cried, "Beast of a First Consul, will you stop tormenting those ladies!"

Bonaparte, at this unexpected attack, stopped and looked in astonishment at the child. Then he was seized with a fit of laughter in which all the spectators of the scene joined.

At another time, when the minister, wishing to return to Paris, sought his son, who had been left with Madame Bonaparte, it was discovered that he had run away. They found him a long way off, in a mill, the mechanism of which he was trying to understand. This desire had been in the child's mind for days, and the honest miller, not knowing who he was, was kindly answering all his questions. Curiosity, especially in regard to mechanics and physics, was one of the essential traits of Sadi's mind.

On account of this disposition so early manifested, Carnot did not hesitate to give a scientific direction to the studies of his son. He was able to undertake this task himself when the monarchical tendencies of the new government had determined him to retire. For a few months only Sadi followed the course of M. Bourdon at the Charlemagne Lycee to prepare himself for the Polytechnic School.

The pupil made rapid progress. He was just sixteen years old when he was admitted to the school, the twenty-fourth on the list. This was in 1812. The following year he left it, first in artillery. But he was considered too young for the school of Metz, and he continued his studies at Paris for a year. To this circumstance is due the fact that he took part in March, 1814, in the military exploits of Vincennes, and not of the butte Chaumont, as almost all the historians of the siege of Paris declared. M. Chasles, one of Sadi's school-fellows, took pains to rectify this error at a seance of the Institute in 1869. If the pupils of the Polytechnic School did not earlier enter into the campaign, it was not because they had not asked to do so. I find in my brother's papers the copy of an address to the Emperor, signed by them December 29, 1813:

"**SIRE:** The country needs all its defenders. The pupils of the Polytechnic School, faithful to their motto, ask to be permitted to hasten to the frontiers to share the glory of the brave men who are consecrating themselves to the safety of France. The battalion, proud of having contributed to the defeat of the enemy, will return to the school to cultivate the sciences and prepare for new services."

General Carnot was at Anvers, which he had just been defending against the confederate English, Prussians, and Swedes, where the French flag yet floated, when he wrote to his son, April 12, 1814:

"**My DEAR SADI:** I have learned with extreme pleasure that the battalion of the Polytechnic School has distinguished itself, and that you have performed your first military exploits with honor. When I am recalled, I shall be very glad if the minister of War will give you permission to come to me. You will become acquainted with a fine country and a beautiful city, where I have had the satisfaction of remaining in peace while disaster has overwhelmed so many other places."

Peace being restored, Sadi rejoined his father at Anvers and returned with him into France. In the month of October he left the Polytechnic School, ranking sixth on the list of young men destined to service in the engineer corps, and went to Metz as a cadet sub-lieutenant at the school. Many scientific papers that he wrote there were a cited success. One is particularly referred to as very clever, a memoir on the instrument called the *theodolite* which is used in ~tronomy and geodesy.

I obtain these details from M. Ornvier, who was of the same rank as Sadi and who, later, was one of the founders of the *Ecole Centrale.* Among his other comrades besides M. Chasles, the learned geometrician just now referred to, was Gen. Duvivier, lamented victim of the insurrection of June 1848. I ought also to mention M. Robelin, Sadi's most intimate friend, who came to help me nurse him during his last lllness, and who published a notice

concerning him in the Revue encylopedigue, t. lv.

The events of 1815 brought General Carnot back into politics during the "Cent Jours" which ended in a fresh catastrophe. This gave Sadi a glimpse of human nature of which he could not speak without disgust. His little sub-lieutenant's room was visited by certain superior officers who did not disdain to mount to the third floor to pay their respects to the son of the new minister. Waterloo put an end to their attentions. The Bourbons reestablished on the throne, Carnot was proscribed and Sadi sent successively into many trying places to pursue his vocation of engineer, to count bricks, to repair walls, and to draw plans destined to be hidden in portfolios. He performed these duties conscientiously and without hope of recompense, for his name, which not long before had brought him so many flatteries, was henceforth the cause of his advancement being long delayed.

In 1818 there came an unlooked-for royal ordinance, authorizing the officers of all branches of the service to present themselves at the examinations for the new corps of the staff. Sadi was well aware that favor had much more to do with this matter than ability, but he was weary of garrison life. The stay in small fortresses to which the nature of his work confined him did not offer sufficient resources to his love of study. Then he hoped, and his hope was realized, that a request for a furlough would be obtained without difficulty, and would insure him the leisure that he sought. In spite of the friendly opposition of some chiefs of the engineer corps, testifying to a sincere regret at the removal from their register of a name which had gained honor among them, Sadi came to Paris to take the examination, and was appointed lieutenant on the staff, January 20, 1819.

He hastened to obtain his furlough, and availed himself of it to lead, in Paris and in the country round about Paris, a studious life interrupted but once, in 1821, by a journey to Germany to visit our father in his exile at Magdeburg. We had then the pleasure of passing some weeks all three together.

When, two years later, death took from us this revered father and I returned alone to France, I found Sadi devoting himself to his scientific studies, which he alternated with the culture of the arts. In this way also, his tastes had marked out for him an original direction, for no one was more opposed than he to the traditional and the conventional. On his music-desk were seen only the compositions of Lully that he had studied, and the concerti of Viotti which he executed. On his table were seen only Pascal, Moliere, or La Fontaine, and he knew his favorite books almost by heart. I call this direction original, because it was anterior to the artistic and literary movement which preceded the revolution of 1830. As to the sympathy of Sadi for the author of the *Provinciles*, it was due not only to the respect of the young mathematician for one of the masters of science, but his devoutly religious mind regarded with horror hypocrisy and hypocrites.

Appreciating the useful and the beautiful, Sadi frequented the museum of the Louvre and the Italian Theatre, as well as the Jardin des Plantes and the Conservatoire des Arts et Metiers. Music was almost a passion with him. He probably inherited this from our mother, who was an excellent pianist, to whom Dalayrac and especially Monsigny, her compatriot, had given instruction. Not content with being able to play well on the violin, Sadi carried to great length his theoretical studies.

His insatiable intellect, moreover, would not allow him to remain a stranger to any branch of knowledge. He diligently followed the course of the College of France and of the Sorbonne, He visited the workshops with eager interest, and made himself familiar with the processes of manufacture; mathematical sciences, natural history, industrial art, political economy,-all these he cultivated with equal ardor. I have seen him not only practise as an amusement, but search theoretically into, gymnastics, fencing, swimming, dancing, and even skating. In even

these things Sadi acquired a superiority which astonished specialists when by chance he forgot himself enough to speak of them for the satisfaction of his own mind was the only aim that he sought.

He had such a repugnance to bringing himself forward that, in his intimate conversations with a few friends, he kept them ignorant of the treasures of science which he had accumulated. They never knew of more than a small part of them. How was it that he determined to formulate his ideas about the motive power of heat, and especially to publish them? I still ask myself this question, -I, who lived with him in the little apartment where our father was confined in the Rue du Parc-Royal while the police of the first Restoration were threatening him. Anxious to be perfectly clear, Sadi made me read some passages of his manuscript in order to convince himself that it would be understood by persons occupied with other studies.

Perhaps a solitary life in small garrisons, in the work-room and in the chemical laboratory, had increased his natural reserve. In small companies, however, he was not at all taciturn. He took part voluntarily in the gayest plays, abandoning himself to lively chat. "The time passed in laughing is well spent," he once wrote. His language was at such times full of wit, keen without malice, original without eccentricity, sometimes paradoxical, but without other pretension than that of an innocent activity of intelligence. He had a very warm heart under a cold manner. He was obliging and devoted, sincere and true in his dealings.

Towards the end of 1826, a new royal ordinance having obliged the staff lieutenants to return to the ranks, Sadi asked and obtained a return to the engineer corps, in which he received the following year, as his rank of seniority, the grade of captain.

Military service, however, weighed upon him. Jealous of his liberty, in 1828, he laid aside his uniform that he might be free to come and go at will. He took advantage of his leisure to make journeys and to visit our principal centres of industry.

He frequently visited M. Clement Desormes, professor at the *Conservatoire des Arts et Metiers*, who had made great advances in applied chemistry. M. Desormes willingly took counsel with him. He was a native of Bourgogne, our family country, which circumstance, I believe, brought them together.

It was before this period (in 1824) that Sadi had published his *Reflexions sur la puissance motrice de feu*. He had seen how little progress had been made in the theory of machines in which this power was employed. He had ascertained that the improvements made in their arrangement were effected tentatively, and almost by chance. He comprehended that in order to raise this important art above empiricism, and to give it the rank of a science, it was necessary to study the phenomena of the production of motion by heat, from the most general point of view, independently of any mechanism, of any special agent; and such had been the thought of his life.

Did he foresee that this small brochure would become the foundation of a new science? He must have attached much importance to it to publish it, and bring himself out of his voluntary obscurity. In fact (as his working notes prove), he perceived the exist in relation between heat and mechanical work; and after having established the principle to which savants have given his name, he devoted himself to the researches which should enable him to establish with certainty the second principle, that of equivalence, which he already clearly divined. Thermodynamics was established from that time. But these researches were rudely interrupted by a great event- the- revolution of July, 1830.

Sadi welcomed it enthusiastically-not, however, it is evident, as a personal advantage.

Several old members of the Convention were still living, even of those who had become celebrated; no favor of the new government was accorded them. To the son of Philippe-Egalite was ascribed a saying which, if it was untrue, at least agreed well with the sentiment of his position: "I can do nothing for the members of the Convention themselves," he said, "but for their families whatever they will."

However it may be, some of those about him vaguely questioned my brother as to his desires in case one of us should be called to the Chamber of Peers, of which Carnot had been a member in 1815. We had on this occasion a brief conference. Unknown to us both, this distinction could be offered only to a title in some sort hereditary. We could not accept it without forsaking the principles of Carnot, who had combated the heredity of the peerage. The paternal opinion therefore came to second our distaste for the proposition, and dictated our reply.

Sadi frequented the popular reunions at this period without forsaking his role of a simple observer.

Nevertheless he was, when occasion demanded it, a man of prompt and energetic action. One incident will suffice to prove this, and to show the *sang-froid* which characterized him. On the day of the funeral of Gen. Lamarque, Sadi was walking thoughtfully in the vicinity of the insurrection. A horseman preceding a company, and who was evidently intoxicated, passed along the street on the gallop, brandishing his sabre and striking down the passers-by. Sadi darted forward, cleverly avoided the weapon of the soldier, seized him by the leg, threw him to the earth and laid him in the gutter, then continued on his way to escape from the cheers of the crowd, amazed at this daring deed.

Before 1830, Sadi had formed part of a *Renion polytechnique industrielle,* made up of old pupils of the school, with a plan of study in common. After 1830, he was a member of the *Association polytechnigue*, consisting also of graduates, the object being the popular propagation of useful knowledge. The president of this association was M. de Choiseul-Praslin; the vice-presidents, MM. de Tracy, August Comte, etc.

The hopes of the democracy meanwhile seeming to be in abeyance, Sadi devoted himself anew to study, and pursued his scientific labors with all the greater energy, as he brought to bear upon them the political ardor now so completely repressed. He undertook profound researches on the physical properties of gases and vapors, and especially on their elastic tensions. Unfortunately, the tables which he prepared from his comparative experiments were not completed; but happily the excellent works of Victor Regnault, so remarkable for their accuracy, have supplied to science, in this respect, the blanks of which Sadi Carnot was conscious.

His excessive application affected his health towards the end of June, 1832. Feeling temporarily better, he wrote gaily to one of his friends who had written several letters to him: "My delay this time is not without excuse. I have been sick for a long time, and in a very wearisome way. I have had an inflammation of the lungs, followed by scarlet-fever. (Perhaps you know what this horrible disease is.) I had to remain twelve days in bed, without sleep or food, without any occupation, amusing myself with leeches, with drinks, with baths, and other toys out of the same shop. This little diversion is not yet ended, for I am still very feeble."

This letter was written at the end of July.

There was a relapse, then brain fever; then finally, hardly recovered from so many violent illnesses which had weakened him morally and physically, Sadi was carried off in a few hours, August 24, 1832, by an attack of cholera. Towards the last, and as if from a dark presentiment, he had given much attention to the prevailing epidemic, following its course with the attention and penetration that he gave to everything.

Sadi Carnot died in the vigor of life, in the brightness of a career that he bade fair to run with glory, leaving memory of profound esteem and affection in the hearts of many friends.

His copy-books, filled with memoranda, attest the activity of his mind, the variety of his knowledge, his love of humanity, his clear sentiments of justice and of liberty. We can follow therein the traces of all his various studies. But the only work that he actually completed is this which is here published. It will suffice to preserve his name from oblivion.

His moral character has other claims on our recognition. Our only ambition here is to present a sketch of it. But, much better than through the perusal of these few pages, Sadi Carnot can be appreciated by reading the thoughts scattered through his memoranda, which are to be carefully collected. There are many practical rules of conduct which he records for himself; many observations that he desires to fix in his memory; sometimes an impression that has just come to him, grave or gay; sometimes too, though rarely, a trace of ill-humor directed against men or society. He never thought that these notes, the outpouring of his mind, would be read by other eyes than his own, or that they would some day be used to judge him. I find in them, for my part, touching analogies with the thoughts of my father, although the father and son had, unfortunately, lived almost always apart, by force of circumstances.

<div align="center">M. H. CARNOT</div>

REFLECTIONS ON THE

MOTIVE POWER OF HEAT

AND ON MACHINES FITTED TO

DEVELOP THAT POWER

EVERYONE knows that heat can produce motion. That it possesses vast motive-power no one can doubt, in these days when the steam-engines is everywhere so well known.

To heat also are due the vast movements which take place on the earth. It causes the agitations of the atmosphere, the ascension of clouds, the fall of rain and of meteors, the currents of water which channel the surface of the globe, and of which man has thus far employed but a small portion. Even earthquakes and volcanic eruptions are the result of heat.

From this immense reservoir we may draw the moving force necessary for our purposes. Nature, in providing us with combustibles on all sides, has given us the power to produce, at all times and in all places, heat and the impelling power which is the result of it. To develop this power, to appropriate it to our uses, is the object of heat-engines.

The study of these engines is of the greatest interest, their importance is enormous, their use is continually increasing, and they seem destined to produce a great revolution in the civilized world.

Already the steam-engine works our mines, impels our ships, excavates our ports and our rivers, forges iron, fashions wood, grinds grain, spins and weaves our cloths, transports the heaviest burdens, etc. It appears that it must some day serve as a universal motor, and be substituted for animal power, water-falls, and air currents.

Over the first of these motors it has the advantage of economy, over the two others the inestimable advantage that it can be used at all times and places without interruption.

If, some day, the steam-engine shall be so perfected that it can be set up and supplied with fuel at small cost, it will combine all desirable qualities, and will afford to the industrial arts a range the extent of which can scarcely be predicted. It is not merely that a powerful and convenient motor that can be pro- cured and carried anywhere is substituted for the motors already in use, but that it causes rapid extension in the arts m which it is applied, and can even create entirely new arts.

The most signal service that the steam-engine has rendered to England is undoubtedly the revival of the working of the coalmines, which had declined, and threatened to cease entirely, in consequence of the continually

increasing difficulty of drainage, and of raising the coal.* We should rank second the benefit to iron manufacture, both by the abundant supply of coal substituted for wood just when the latter had begun to grow scarce,

* It **may be said that coal-mining has increased tenfold in England since the invention of the steam-engine. It is almost equally true in regard to the mining of copper, tin, and iron. The results produced in a half-century by the steam-engine in the mines of England are to-day paralleled in the gold and silver mines of the New World--mines of which the working declined from day to day, principally on account of the insufficiency of the motors employed in the draining and the extraction of the minerals.**

and by the powerful machines of all kinds, the use of which the introduction of the steam-engine has permitted or facilitated. Iron and heat are, as we know, the supporters, the bases, of the mechanic arts. It is doubtful if there be in England a single industrial establishment of which the existence does not depend on the use of these agents, and which does not freely employ them. To take away to-day from England her steam-engines would be to take away at the same time her coal and iron. It would be to dry up all her sources of wealth, to ruin all on which her prosperity depends, in short, to annihilate that colossal power. The destruction of her navy, which she considers her strongest defence, would perhaps be less fatal.

The safe and rapid navigation by steamships may be regarded -as an entirely new art due to the steam-engine. Already this art has permitted the establishment of prompt and regular communications across the arms of the sea, and on the at rivers of the old and new continents. It has made it possible to traverse savage regions where before we could scarcely penetrate. It has enabled us to carry the fruits of civilization over portions of the globe where they would else have been wanting for years. Steam navigation brings nearer together the most distant nations. It tends to unite the nations of the earth as inhabitants of one country. In fact, to lessen the time, the fatigues, the uncertain ties, and the dangers of travel is not this the same as greatly to shorten distances?*

* **We say, to lessen the dangers of journeys. In fact, although the use of the steam-engine on ships is attended by some danger which has been greatly exaggerated, this is more than compensated by the power of following always an appointed and well-known route, of resisting the force of the winds which would drive the ship towards the shore, the shoals, or the rocks.**

The discovery of the steam-engine owed its birth, like most human inventions, to rude attempts which have been attributed to different persons, while the real author is not certainly known. It is, however, less in the first attempts that the principal discovery consists, than in the successive improvements which have brought steam-engines to the condition in which we find them to-day. There is almost as great a distance between the first apparatus in which the expansive force of steam was displayed and the existing machine, as between the first raft that man ever made and the modern vessel.

If the honor of a discovery belongs to the nation in which it has acquired its growth and all its developments, this honor cannot be here refused to England. Savery, Newcomen, Smeaton, the famous Watt, Woolf, Trevithick, and some other English engineers, are the veritable creators of the steam-engine. It has acquired at their hands all its successive degrees of improvement. Finally, it is natural that an invention should have its birth and especially be developed, be perfected, in that place where its want is most strongly felt.

Notwithstanding the work of all kinds done by steam-engines, notwithstanding the satisfactory condition to which they have been brought to-day, their theory is very little understood, and the attempts to improve them are still directed almost by chance.

The question has often been raised whether the motive power of heat* is unbounded, whether the possible improvements in

* We use here the expression motive power to express the useful effect that a motor is capable of producing. This effect can always be likened to the elevation of a weight to a certain height. It has, as we know, as a measure, the product of the weight multiplied by the height to which it is raised.

steam-engines have an assignable limit,-a limit which the nature of things will not allow to be passed by any means whatever; or whether, on the contrary, these improvements may be carried on indefinitely. We have long sought, and are seeking to-day, to ascertain whether there are in existence agents preferable to the vapor of water for developing the motive power of heat; whether atmospheric air, for example, would not present in this respect great advantages. We propose now to submit these questions to a deliberate examination.

The phenomenon of the production of motion by heat has not been considered from a sufficiently general point of view. We have considered it only in machines the nature and mode of action of which have not allowed us to take in the whole extent of application of which it is susceptible. In such machines the phenomenon is, in a way, incomplete. It becomes difficult to recognize its principles and study its laws. In order to consider in the most general way the principle of the production of motion by heat, it must be considered independently of any mechanism or any particular agent. It is necessary to establish principles applicable not only to steam-engines* but to all imaginable heat-engines, whatever the working substance and whatever the method by which it is operated. Machines which do not receive their motion from heat, those which have for a motor the force of men or of animals, a waterfall, an air current, etc., can be studied even to their smallest details by the mechanical theory. All cases are foreseen, all imaginable movements are referred to these general principles,

* We distinguish here the steam-engine from the heat-engine in general. The latter may make use of any agent whatever, of the vapor of water or of any other, to develop the motive power of heat.

firmly established, and applicable under all circumstances. This is the character of a complete theory. A similar theory is evidently needed for heat-engines. We shall have it only when the laws of Physics shall be extended enough, generalized enough, to make known beforehand all the effects of heat acting in a determined manner on any body.

We will suppose in what follows at least a superficial knowledge of the different parts which compose an ordinary steam-engine; and we consider it unecessary to explain what are the furnace, boiler, steam-cylinder, piston, condenser, etc.

The production of motion in steam-engines is always accompanied by a circumstance on which we should fix our attention. This circumstance is the re-establishing of equilibrium in the caloric; that is, its passage from a body in which the temperature is more or less elevated, to another in which it is lower. What happens in fact in a steam-engine actually in motion? The caloric developed in the furnace by the effect of the combustion traverses the walls of the boiler, produces steam, and in some way incorporates itself with it. The latter carrying it away, takes it first into the cylinder, where it performs some function, and from thence into the condenser, where it is liquefied by contact with the cold water which it encounters there. Then, as a final result, the cold water of the condenser takes possession of the caloric developed by the combustion. It is heated by the intervention of the steam as if it had been placed directly over the furnace. The steam is here only a means of transporting the

caloric. It fills the same office as in the heating of baths by steam, except that in this case its motion is rendered useful.

We easily recognize in the operations that we have just described the re-establishment of equilibrium in the caloric, its passage from a more or less heated body to a cooler one. The first of these bodies, in this case, is the heated air of the furnace; the second is the condensing water. The re-establishment of equilibrium of the caloric takes place between them, if not completely, at least partially, for on the one hand the heated air, after having performed its function, having passed round the boiler, goes out through the chimney with a temperature much below that which it had acquired as the effect of combustion; and on the other hand, the water of the condenser, after having liquefied the steam, leaves the machine with a temperature higher than that with which it entered.

The production of motive power is then due in steam-engines not to an actual consumption of caloric, but *to its transportation from a warm body to a cold body,* that is, to its re-establishment of equilibrium-an equilibrium considered as destroyed by any cause whatever, by chemical action, such as combustion, or by any other. We shall see shortly that this principle is applicable to any machine set in motion by heat.

According to this principle, the production of heat alone is not sufficient to give birth to the impelling power: it is necessary that there should also be cold; without it, the heat would be useless. And in fact, if we should find about us only bodies as hot as our furnaces, how can we condense steam? What should we do with it if once produced? We should not presume that we might discharge it into the atmosphere, as is done in some engines ;* the

*Certain engines at high pressure throw the steam out into the atmosphere instead of the condenser. They are used specially in places where it would he difficult to procure a stream of cold water sufficient to produce condensation.

atmosphere would not receive it. It does receive it under the actual condition of things, only because it fulfils the office of a vast condenser, because it is at a lower temperature; otherwise it would soon become fully charged, or rather would be already saturated.*

Wherever there exists a difference of temperature, wherever it has been possible for the equilibrium of the caloric to be re-established, it is possible to have also the production of impelling power. Steam is a means of realizing this power, but it is not the only one. All substances in nature can be employed for this purpose, all are susceptible of changes in volume, of successive contractions and dilatations, through the alternation of heat and cold. All are capable of overcoming in their changes of volume certain resistances, and of thus developing the impelling power. A solid body-a metallic bar for example- alternately heated and cooled increases and diminishes in length, and can move bodies fastened to its ends. A liquid alternately heated and cooled increases and diminishes in volume, and can overcome obstacles of greater or less size, opposed to its dilatation. An

* The existence of water in the liquid state here necessarily assumed, since without it the steam-engine could not be fed, supposes the existence of a pressure capable of preventing this water from vaporizing, consequently of a pressure equal or superior to the tension of vapor at that temperature. If such a pressure were not exerted by the atmospheric air, there would be instantly produced a quantity of steam sufficient to give rise to that tension, and it would be necessary always to overcome this pressure in order to throw out the steam from the engines into the new atmosphere. Now this is evidently equivalent to overcoming the tension which the steam retains after its condensation, as effected by ordinary means.

If a very high temperature existed at the surface of our globe, as it seems certain that it exists in its interior, all the waters of the

ocean would be in a state of vapor in the atmosphere, and no portion of it would be found in a liquid state.

aeriform fluid is susceptible of considerable change of volume by variations of temperature. If it is enclosed in an expansible space, such as a cylinder provided with a piston, it will produce movements of great extent. Vapors of all substances capable of passing into a gaseous condition, as of alcohol, of mercury, of sulphur, etc., may fulfill the same office as vapor of water. The latter, alternately heated and cooled, would produce motive power in the shape of permanent gases, that is, without ever returning to a liquid state. Most of these substances have been proposed, many even have been tried, although up to this time perhaps without remarkable success.

We have shown that in steam-engines the motive power is due to a re-establishment of equilibrium in the caloric; this takes place not only for steam-engines, but also for every heat-engine-that is, for every machine of which caloric is the motor. Heat can evidently be a cause of motion only by virtue of the changes of volume or of form which it produces in bodies.

These changes and not caused by uniform temperature, but rather by alternations of heat and cold. Now to heat any substance whatever requires a body warmer than the one to be heated; to cool it requires a cooler body. We supply caloric to the first of these bodies that we may transmit it to the second by means of the intermediary substance. This is to re-establish, or at least to endeavor to re-establish, the equilibrium of the caloric.

It is natural to ask here this curious and important question: Is the motive power of heat invariable in quantity, or does it vary with the agent employed to realize it as the intermediary substance, selected as the subject of action of the heat?

It is clear that this question can be asked only in regard to a given quantity of caloric,* the difference of the temperatures also being given. We take, for example, one body A kept at a temperature of 100^0 and another body B kept at a temperature of 0^0, and ask what quantity of motive power can be produced by the passage of a given portion of caloric (for example, as much as is necessary to melt a kilogram of ice) from the first of these bodies to the second. We inquire whether this quantity of motive power is necessarily limited, whether it varies with the substance employed to realize it, whether the vapor of water offers in this respect more or less advantage than the vapor of alcohol, of mercury, a permanent gas, or any other substance. We will try to answer these questions, availing ourselves of ideas already established.

We have already remarked upon this self-evident fact, or fact which at least appears evident as soon as we reflect on the changes of volume occasioned by heat: *wherever there exists difference of temperature, motive-power can be produced.* Reciprocally, wherever we can consume this power, it is possible to produce a difference of temperature, it is possible to occasion destruction of equilibrium in the caloric. Are not percussion and the friction of bodies actually means of raising their temperature, of making it reach spontaneously a higher degree than that of the surrounding bodies, and consequently of producing a destruction of equi-

* It is considered unnecessary to explain here what is quantity of caloric or quantity of heat (for we employ these two expressions indifferently), or to describe how we measure these quantities by the calorimeter. Nor will we explain what is meant by latent heat, degree of temperature, specific heat, etc. The reader should be familiarized with these terms through the study of the elementary treatises of physics or of chemistry.

librium in the caloric, where equilibrium previously existed? It is a fact proved by experience, that the

temperature of gaseous fluids is raised by compression and lowered by rarefaction. This is a sure method of changing the temperature of bodies, and destroying the equilibrium of the caloric as many times as may be desired with the same substance. The vapor of water employed-in an inverse manner to that in which it is used in steam-engines^s can also be regarded as a means of destroying the equilibrium of the caloric. To be convinced of this we need but to observe closely the manner in which motive power is developed by the action of heat on vapor of water. Imagine two bodies *A* and *B,* kept each at a constant temperature, that of *A* being higher than that of *B*. These two bodies, to which we can give or from which we can remove the heat without causing their temperatures to vary, exercise the functions of two unlimited reservoirs of caloric. We will call the first the furnace and the second the refrigerator.

If we wish to produce motive power by carrying a certain quantity of heat from the body *A* to the body *B* we shall proceed as follows:

(1)To borrow caloric from the body *A* to make steam with it-that is, to make this body fulfill the function of a furnace, or rather of the metal composing the boiler in ordinary engines-we here assume that the steam is produced at the same temperature as the body *A*.

(2)The steam having been received in a space capable of expansion, such as a cylinder furnished with a piston, to increase the volume of this space, and consequently also that of the steam. Thus rarefied, the temperature will fall spontaneously, as occurs with all elastic fluids; admit that the rarefaction may be continued to the point where the temperature becomes precisely that of the body *B*.

(3)To condense the steam by putting it in contact with the body *B,* and at the same time exerting on it a constant pressure until it is entirely liquefied. The body *B* fills here the place of the injection-water in ordinary engines, with this difference, that it condenses the vapor without mingling with it, and with-out changing its own temperature.*

The operations which we have just described might have been performed in an inverse direction and order. There is nothing no prevent forming vapor with the caloric of the body *B,* and at the temperature of that body, compressing it in such a way as to make it acquire the temperature of the body *A,* finally condensing it by contact with this latter body, and continuing the compression to complete liquefaction.

* We may perhaps wonder here that the body *B* being at the same temperature as the steam is able to condense it. Doubtless this is not strictly possible, but the slightest difference of temperature will determine the condensation, which suffices to establish the justice of our reasoning. It is thus that, in the differential calculus, it is sufficient that we can conceive the neglected quantities indefinitely reducible in proportion to the quantities retained in the equations, to make certain of the exact result.

The body *B* condenses the steam without changing its own temperatur this results from our supposition. We have admitted that this body may be maintained at a constant temperature. We take away the caloric as the steam furnishes it. This is the condition in which the metal of the condenser is found when the liquefaction of the steam is accomplished by applying cold water externally, as was formerly done in several engines. Similarly, the water of a reservoir can be maintained at a constant level if the liquid flows out at one side as it flows in at the other.

One could even conceive the bodies *A* and *B* maintaining the same temperature, although they might lose or gain certain quantities of heat. If, for example, the body *A* were a mass of steam ready to become liquid, and the body *B* a mass of ice ready to melt, these bodies might, as we know, furnish or receive caloric without thermometric change.

By our first operations there would have been at the same time production of motive power and transfer of caloric from the body *A* to the body *B*. By the inverse operations there is at the same time expenditure of motive power and return of caloric from the body *B* to the body *A*. But if we have acted in each case on the same quantity of vapor, if there is produced no loss either of motive power or caloric, the quantity of motive power produced in the first place will be equal to. that which would have been expended in the second, and the quantity of caloric passed in the first case from the body *A* to the body *B* would be equal to the quantity which passes back again in the second from the body *B* to the body A; so that an indefinite number of alternative operations of this sort could be carried on without in the end having either produced motive power or transferred caloric from one body to the other.

Now if there existed any means of using heat preferable to those which we have employed, that is, if it were possible by any method whatever to make the caloric produce a quantity of motive power greater than we have made it produce by our first series of operations, it would suffice to divert a portion of this power in order by the method just indicated to make the caloric of the body *B* return to the body *A* from the refrigerator to the furnace, to restore the initial conditions, and thus to be ready to commence again an operation precisely similar to the former, and so on: this would be not only perpetual motion, but an unlimited creation of motive power without consumption either of caloric or of any other agent whatever. Such a creation is entirely contrary to ideas now accepted, to the laws of mechanics and of sound physics. It is inadmissible. We should then conclude that the *maximum of motive power resulting* from the

employment of steam is also the maximum of motive power realizable by any means whatever. We will soon give a second more vigorous demonstration of this theory. This should be considered only as an approximation.

We have a right to ask, in regard to the proposition just enunciated, the following questions: What is the sense of the word *maximum* here? By what sign can it be known that this maximum is attained? By what sign can it be known whether the steam is employed to greatest possible advantage in the production of motive power?

Since every re-establishment of equilibrium in the caloric may be the cause of the production of motive power, every re-establishment of equilibrium which shall be accomplished without production of this power should be considered as an actual loss. Now, very little reflection would show that all change of temperature which is not due to a change of volume of the bodies can be only a useless re-establishment of equilibrium in the caloric.* The necessary condition of the maximum is, then, *that in the bodies employed to realize the motive power of heat there should not occur any change of temperature which may not be due to a change of volume.* Reciprocally, every time that this condition is fulfilled the maximum will be attained. This principle should never be lost sight of in the construction of heat-engines; it is its fundamental basis. If it cannot be strictly observed, it should at least be departed from as little as possible.

* We assume here no chemical action between the bodies employed to realize the motive power of heat. The chemical action which takes place in the furnace is, in some sort, a preliminary action,-an operation destined not to produce immediately motive power, but to destroy the equilibrium of the caloric, to produce a difference of temperature which may finally give rise to motion.

Every change of temperature which is not due to a change of volume or to chemical action (an action that we provisionally suppose not to occur here) is necessarily due to the direct passage of the caloric from a more or less heated body to a colder body. This passage occurs mainly by the contact of bodies of different temperatures;

hence such contact should be avoided as much as possible. It cannot probably be avoided entirely, but it should at least be so managed that the bodies brought in contact with each other differ as little as possible in temperature. When we just now supposed, in our demonstration, the caloric of the body *A* employed to form steam, this steam was considered as generated at the temperature of the body *A*; thus the contact took place only between bodies of equal temperatures; the change of temperature occurring afterwards in the steam was due to dilatation, consequently to a change of volume. Finally, condensation took place also without contact of bodies of different temperatures. It occurred while exerting a constant pressure on the steam brought in contact with the body *B* of the same temperature as itself. The conditions for a maximum are thus found to be fulfilled. In reality the operation cannot proceed exactly as we have assumed. To determine the passage of caloric from one body to another, it is necessary that there should be an excess of temperature in the first, but this excess may be supposed as slight as we please. We can regard it as insensible in theory, without thereby destroying the exactness of the arguments.

A more substantial objection may be made to our demonstration, thus: When we borrow caloric from the body *A* to produce steam, and when this steam is afterwards condensed by its contact with the body *B,* the water used to form it, and which we considered at first as being of the temperature of the body *A,* is found at the close of the operation at the temperature of the body *B.* It has become cool. If we wish to begin again an operation similar to the first, if we wish to develop a new quantity of motive power with the same instrument, with the same steam, it is necessary first to re-establish the original condition-to restore the water to the original temperature. This can undoubtedly be done by at once putting it again in contact with the body *A* ; but there is then contact between bodies of different temperatures, and loss of motive power.* It would be impossible to execute the inverse operation, that is, to return to the body A the caloric employed to raise the temperature of the liquid. This difficulty may be removed by supposing the difference of temperature between the body A and the body B indefinitely small. The quantity of heat necessary to raise the liquid to its former temperature will be also indefinitely small and unimportant relatively to that which is necessary to produce steam-a quantity always limited.

The proposition found elsewhere demonstrated for the case in which the difference between the temperatures of the two

*This kind of loss is **found in all steam-engines. In fact, the water destined to feed the boiler is always cooler than the water which it already contains. There occurs between them a useless re-establishment of equilibrium of caloric. We are easily convinced, a *posteriori,* that this re-establishment of equilibrium causes a loss of motive power if we reflect that it would have been possible to previously heat the feed-water by using it as condensing-water in a small accessory engine, when the steam drawn from the large boiler might have been used, and where the condensation might be produced at a temperature intermediate between that of the boiler and that of the principal condenser. The power produced by the small engine would have cost no loss of heat, since all that which had been used would have returned into the boiler with the water of condensation.**

bodies is indefinitely small, may be easily extended to the general case. In fact, if it operated to produce motive power by the passage of calorie from the body *A* to the body *Z,* the temperature of this latter body being very different from that of the former, we should imagine a series of bodies *B, C, D...* of temperatures intermediate between those of the bodies *A, Z,* and selected so that the differences from *A* to *B,* from *B* to *C,* etc., may all be indefinitely small. The caloric coming from *A* would not arrive at *Z* till after it had passed through the bodies *B, C, D,* etc., and after having developed in each of these stages maximum motive power. The inverse operations would here be entirely possible, and the reasoning of page 53 would be strictly applicable.

According to established principles at the present time, we can compare with sufficient accuracy the motive power of heat to that of a waterfall. Each has a maximum that we cannot exceed, whatever may be, on the one hand, the machine which is acted upon by the water, and whatever, on the other hand, the substance acted upon by the heat. The motive power of a waterfall depends on its height and on the quantity of the liquid; the motive. power of heat depends also on the quantity of caloric used, and on what may be termed, on what in fact we will call, the *height of its fall,* * that is to say, the difference of temperature of the bodies between which the exchange of caloric is made. In the waterfall the motive power is exactly proportional to the difference of level between the higher and lower reservoirs. In the fall of caloric the motive power undoubtedly increases with the difference of temperature between the warm

* The matter here dealt with being entirely new, we are obliged to employ expressions not in use as yet, and which perhaps are less clear than is desirable.

and the cold bodies; but we do not know whether it is proportional to this difference. We do not know, for example, whether the fall of caloric from 100 to 50 degrees furnishes more or less motive power than the fall of this same caloric from 50 to zero. It is a question which we propose to examine hereafter.

We shall give here a second demonstration of the fundamental proposition enunciated on page 56, and present this proposition under a more general form than the one already given.

When a gaseous fluid is rapidly compressed its temperature rises. It falls, on the contrary, when it is rapidly dilated. This is one of the facts best demonstrated by experiment. We will take it for the basis of our demonstration.

If, when the temperature of a gas has been raised by compression, we wish to reduce it to its former temperature without subjecting its volume to new changes, some of its caloric must be removed. This caloric might have been removed in proportion as pressure was applied, so that the temperature of the gas would remain constant. Similarly, if the gas is rarefied we can avoid lowering the temperature by supplying it with a certain quantity of caloric. Let us call the caloric employed at such times, when no change of temperature occurs, *caloric due to change of volume.* This denomination does not indicate that the caloric appertains to the volume: it does not appertain to it any more than to pressure, and might as well be called *caloric due to change of pressure.* We do not know what laws it follows relative to the variations in volume: it is possible that its quantity changes either with the nature of the gas, its density, or its temperature. Experiment has taught us nothing on this subject.

It has only shown us that this caloric is developed in greater or less quantity by the compression of the elastic fluids.

This preliminary idea being established, let us imagine an elastic fluid, atmospheric air for example, shut up in a cylindrical vessel, *abcd* (Fig. 1), provided with a movable diaphragm or piston Cd. Let there be also two bodies, *A* and *B,* kept each at a constant temperature, that of *A* being higher than that of *B.* Let us picture to ourselves now the series of operations which are to be described:
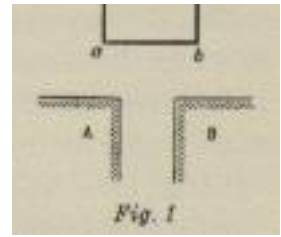

(1) Contact of the body *A* with the air enclosed in the space *abcd* or with the wall of this space -a wall that we will suppose to transmit the caloric readily. The air becomes by such
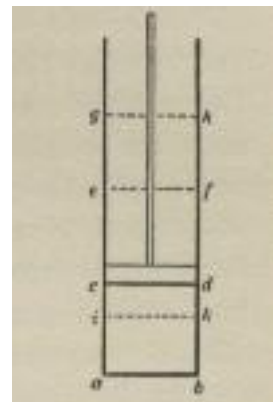
contact of the same temperature as the body *A; cd* is the actual position of the piston.

(2) The piston gradually rises and takes the position *ef.* The body *A* is all the time

Fig. 1

in contact with the air, which is thus kept at a constant temperature during the rarefaction. The body *A* furnishes the caloric necessary to keep the temperature constant.

(3) The body *A* is removed, and the air is then no longer in contact with any body capable of furnishing it with caloric. The piston meanwhile continues to move, and passes from the position *ef* to the position *gh*. The air is rarefied without receiving caloric, and its temperature falls. Let us imagine that it falls thus till it becomes equal to that of the body *B;* at this instant the piston stops, remaining at the position *gh.*

(4) The air is placed in contact with the body *B;* it is compressed by the return of the piston as it is moved from the position *gh* to the position *cd.* This air remains, however, at a constant temperature because of its contact with the body *B,* to which it yields its caloric.

(5) The body *B* is removed, and the compression of the air is continued, which being then isolated, its temperature rises. The compression is continued till the air acquires the temperature of the body *A.* The piston passes during this time from the position *cd* to the position *ik.*

(6) The air is again placed in contact with the body *A.* The piston returns from the position *ik* to the position *ef;* the ternperature remains unchanged.

(7) The step described under number 3 is renewed, then successively the steps 4, 5, 6, 3, 4, 5,6,3,4,5;and so on.

In these various operations the piston is subject to an effort of greater or less magnitude, exerted by the air enclosed in the cylinder; the elastic force of this air varies as much by reason of the changes in volume as of changes in temperature. But it should be remarked that with equal volumes, that is, for the similiar positions of the piston, the temperature is higher during the movements of dilatation than during the movements of compression. During the former the elastic force of the air is found to be greater, and consequently the quantity of motive power produced by the movements of dilatation is more considerable than that consumed to produce the movements of compression. Thus we should obtain an excess of motive power-an excess which we could employ for any purpose whatever. The air, then, has served as a heat-engine; we have, in fact, employed it in the most advantageous manner possible, for no useless re-establishment of equilibrium has been effected in the caloric.

All the above described operations may be executed in an inverse sense and order. Let us imagine that, after the sixth period, that is to say the piston having arrived at the position we cause it to return to the position *ik,* and that at the same time we keep the air in contact with the body *A.* The caloric furnished by this body during the sixth period would return to its source, that is, to the body *A,* and the conditions would then become precisely the same as they were at the end of the fifth period. If we now take away the body *A,* and if we cause the piston to move from *ef* to *cd*, the temperature of the air will diminish as many degrees as it increased during the fifth period, and

will become that of the body *B*. We may evidently continue a series of operations the inverse of those already described. It is only necessary under the same circumstances to execute for each period a movement of dilatation instead of a movement of compression, and reciprocally.

The result of these first operations has been the production of a certain quantity of motive power and the. removal of caloric from the body *A* to the body *B*. The result of the inverse operations is the consumption of the motive power produced and the return of the caloric from the body *B* to the body *A;* so that these two series of operations annul each other, after a fashion, one neutralizing the other.

The impossibility of making the caloric produce a greater quantity of motive power than that which we obtained from it by our first series of operations, is now easily proved. It is demonstrated by reasoning very similar to that employed on page

56*;* the reasoning will here be even more exact. The air which we have used to develop the motive power is restored at the end of each cycle of operations exactly to the state in which it was at first found, while, as we have already remarked, this would not be precisely the case with the vapor of water.*

We have chosen atmospheric air as the instrument which should develop the motive power of heat, but it is evident that the reasoning would have been the same for all other gaseous substances, and even for all other bodies susceptible of change of temperature through successive contractions and dilatations, which comprehends all natural substances, or at least all those which are adapted to realize the motive power of heat. Thus we are led to establish this general proposition:

The motive power of heat is independent of the agents employed to realize it; its quantity is fired solely by the temperatures of the bodies between which is effected, finally, the transfer of the caloric.

We must understand here that each of the methods of developing motive power attains the perfection of which it is susceptible. This condition is found to be fulfilled if, as we remarked above, there is produced in the body no other change of tem-

* **We tacitly assume in our demonstration, that when a body has experienced any changes, and when after a certain number of transformations it returns to precisely its original state, that is, to that state considered in respect to density, to temperature, to mode of aggregation-let us suppose, I say, that this body is found to contain the same quantity of heat that it contained at first, or else that the quantities of heat absorbed or set free in these different transformations are exactly compensated. This fact has never been called in question. It was first admitted without reflection, and verified afterwards in many cases by experiments with the calorimeter. To deny it would be to overthrow the whole theory of heat to which it serves as a basis. For the rest, we may say in passing, the main principles on which the theory of heat rests require the most careful examination. Many experimental facts appear almost inexplicable in the present state of this theory.**

MOTIVE POWER OF HEAT

perature than that due to change of volume, or, what is the same thing in other words, if there is no contact between bodies of sensibly different temperatures.

Different methods of realizing motive power may be taken, as in the employment of different substances, or the

use of the same substance in two different states-for example, of a gas at two different densities.

This leads us naturally to those interesting researches on the aeriform fluids-researches which lead us also to new results in regard to the motive power of heat, and give us the means of verifying, in some particular cases, the fundamental proposition above stated.*

We readily see that our demonstration would have been simplified by supposing the temperatures of the bodies A and B to differ very little. Then the movements of the piston being slight during the periods 3 and 5, these periods might have been suppressed without influencing sensibly the production of motive power. A very little change of volume should suffice in fact to produce a very slight change of temperature, and this slight change of volume may be neglected in presence of that of the periods 4 and 6, of which the extent is unlimited.

If we suppress periods 3 and 5, in the series of operations above described, it is reduced to the following:

(1) Contact of the gas confined in *abcd* (Fig. 2) with the body A, passage of the piston from *cd* to *ef.*

(2) Removal of the body A, contact of the gas confined in *abef* with tile body B, return of the piston from *ef* to *cd*.

- **We will suppose, in what follows, the reader to be *au courant* with the later progress of modern Physics in regard to gaseous substances and heat.**

(3) Removal of the body *B,* contact of the gas with the body *A,* passage of the piston from *cd* to *ef*, that is, repetition of the first period, and so on.

The motive power resulting from the *ensemble* of operations 1 and 2 will evidently be the difference between that which is produced by the expansion of the gas while it is at the temperature of the body *A*, and that which is consumed to compress this gas while it is at the temperature of the body *B*.

Let us suppose that operations 1 and 2 be performed on two gases of

different chemical natures but un-

der the same pressure- under at

mospheric pressure, for example.

These two gases will behave exactly

alike under the same circumstances,

Fig. 2

Fig. 3

that is, their expansive forces, originally equal, will remain always

equal, whatever may be the variations of volume and of temperature, provided these variations are the same in both. This results obviously from the laws of Mariotte and MM. Gay- Lussac and Dalton-laws common to all elastic fluids, and in virtue of which the same relations exist for all these fluids between the volume, the expansive force, and the temperature.

Since two different gases at the same temperature and under the same pressure should behave alike under the same circumstances, if we subjected them both to the operations above described, they should give rise to equal quantities of motive power.

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Now this implies, according to the fundamental proposition that we have established, the employment of two equal quantities of caloric; that is, it implies that the quantity of caloric transferred from the body *A* to the body *B* is the same, whichever gas is used.

The quantity of caloric transferred from the body *A* to the body *B* is evidently that which is absorbed by the gas in its expansion of volume, or that which this gas relinquishes during compression. We are led, then, to establish the following proposition:

When a gas passes without change of temperature from one definite volume and pressure to another volume and another pressure equally definite, the quantity of caloric absorbed or relinquished is always the same, whatever may be the nature of the gas chosen as the subject of the experiment.

Take, for example, 1 litre of air at the temperature of 100^0 and under the pressure of one atmosphere. If we double the volume of this air and wish to maintain it at the temperature of 100^0, a certain quantity of heat must be supplied to it. Now this quantity will be precisely the same if, instead of operating on the air, we operate upon carbonic-acid gas, upon nitrogen, upon hydrogen, upon vapor of water, or of alcohol, that is, if we double the volume of 1 litre of these gases taken at the temperature of 100^0 and under atmospheric pressure.

It will be the same thing in the inverse sense if, instead of doubling the volume of gas, we reduce it one half by compression. The quantity of heat that the elastic fluids set free or absorb in their changes of volume has never been measured by any direct experiment, and doubtless such an experiment would be

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very difficult, but there exists a datum which is very nearly its equivalent. This has been furnished by the theory of sound. It deserves much confidence because of the exactness of the conditions which have led to its establishment. It consists in this:

Atmospheric air should rise one degree Centigrade when by sudden compression it experiences a reduction of volume of 1/116.*

Experiments on the velocity of sound having been made in air under the pressure of 760 millimetres of mercury and at the temperature of 6^0, it is only to these two circumstances that our datum has reference. We will, however, for greater facility, refer it to the temperature 0^0, which is nearly the same.

Air compressed 1/116, and thus heated one degree, differs from air heated directly one degree only in its density. The primitive volume being supposed to be V, the compression of 1/116 reduces it to

V- V/116.

Direct heating under constant pressure should, according to the rule of M. Gay-Lussac, increase the volume of air 1/267 above what it would be at 0^0: 50 the air is, on the one hand, reduced to the volume

V - V/116; on the other, it is increased to V + V/267.

The difference between the quantities of heat which the air possesses in both cases is evidently the quantity employed to raise it directly one degree; so then the quantity of heat that the air would absorb in passing from the volume V - V/116 to

* **M. Poisson, to whom this figure is due, has shown that it accords very well with the result of an experiment of MM. Clement and Desormes on the return of air into a vacuum, or rather, into air slightly rarefied. It also accords very nearly with results found by MM. Gay-Lussac and Welter. (See note, p.78.)**

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the volume V + V/267 is equal to that which is required to raise it one degree.

Let us suppose now that, instead of heating one degree the air subjected to a constant pressure and able to dilate freely, we in-close it within an invariable space, and that in this condition we cause it to rise one degree in temperature. The air thus heated one degree will differ from the air compressed 1/116 only by its 1/116 greater volume. So then the quantity of heat that the air would set free by a reduction of volume of 1/116 is equal to that which would be required to raise it one degree Centigrade under constant volume. As the differences between the volumes V -V/116, V, and V + V/267 are small relatively to the volumes themselves, we may regard the quantities of heat absorbed by the air in passing from the first of these volumes to the second, and from the first to the third, as sensibly proportional to the changes of volume. We are then led to the establishment of the following relation:

The quantity of heat necessary to raise one degree air under constant pressure is to the quantity of heat necessary to raise one degree the same air under constant volume, in the ratio of the numbers 1/116 + 1/267 to 1/116; or, multiplying both by 116 X 267, in the ratio of the numbers 267 + 116 to 267.

This, then, is the ratio which exists between the capacity of air for heat under constant pressure and its capacity under constant volume. If the first of these two capacities is expressed by unity, the other will be expressed by the number267/ (267 + 116), or very nearly 0.700; their difference, 1 - 0.700 or 0.300, will evidently express the quantity of heat which will produce the

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increase of volume in the air when it is heated one degree under constant pressure.

According to the law of MM. Gay-Lussac and Dalton, this increase of volume would be the same for all other gases; according to the theory demonstrated on page 78, the heat absorbed by these equal increases of volume is the same for all the elastic fluids, which leads to the establishment of the following proposition:

The difference between specific heat under constant pressure and specific heat under constant volume is the same for all gases.

It should be remarked here that all the gases are considered as taken under the same pressure, atmospheric pressure for example, and that the specific heats are also measured with reference to the volumes.

It is a very easy matter now for us to prepare a table of the specific heat of gases under constant volume, from the knowledge of their specific heats under constant pressure. Here is the table:

TABLE OF THE SPECIFIC HEAT OF GASES

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The first column is the result of the direct experiments of MM. Delaroche and Berard on the specific heat of the gas under atmospheric pressure, and the second column is composed of the numbers of the first diminished by 0.300.

The numbers of the first column and those of the second are here referred to the same unit, to the specific heat of atmospheric air under constant pressure.

The difference between each number of the first column and the corresponding number of the second being constant, the relation between these numbers should be variable. Thus the relation between the specific heat of gases under constant pressure and the specific heat at constant volume, varies in different gases.

We have seen that air when it is subjected to a sudden compression of 1/116 of its volume rises one degree in temperature. The other gases through a similar compression should also rise in temperature. They should rise, but not equally, in an inverse ratio with their specific heat at constant volume. In fact, the reduction of volume being by hypothesis always the same, the quantity of heat due to this reduction should likewise be always the same, and consequently should produce an elevation of temperature dependent only on the specific heat acquired by the gas after its compression, and evidently in inverse ratio with this specific heat. Thus we can easily form the table of the elevations of temperature of the different gases for a compression of 1/116. (See table page 72.)

A second compression of 1/116 (of the altered volume), as we shall presently see, would also raise the temperature of these gases nearly as much as the first; but it would not be the same

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with a third, a fourth, a hundredth such compression. The capacity of gases for heat changes with their volume. It is not unlikely that it changes also with the temperature.

We shall now deduce from the general proposition stated on page 64 a second theory, which will serve as a corollary to that just demonstrated.

Table

Let us suppose that the gas enclosed in the cylindrical space abcd (Fig. 2) be transported into the space a'b'c'd' (Fig. 3) of equal height, but of different base and wider. This gas would increase in volume, would diminish in density and in elastic force, in the inverse ratio of the two volumes abcd, a'b'c'd'. As to the total pressure exerted in each piston Cd, c'd', it would be the same from all quarters, for the surface of these pistons is in direct ratio to the volumes.

Let us suppose that we perform on the gas inclosed in a'b'c'd' the operations described on page 65, and which were taken as having been performed upon the gas inclosed in abcd; that is, let us suppose that we have given to the piston c'd' motions equal

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to those of the piston cd, that we have made it occupy successively the positions c'd' corresponding to cd, and ef corresponding to ef, and that at the same time we have subjected the gas by means of the two bodies *A* and *B* to the same variations of temperature as when it was inclosed in abcd. The total effort exercised on the piston would be found to be, in the two cases, always the same at the corresponding instants. This results solely from the law of Mariotte.* In fact, the densities of the two gases maintaining always the same ratio for similar positions of the pistons, and the temperatures being always equal in both, the total pressures exercised on the pistons will always maintain the same ratio to each other. If this ratio is, at any instant whatever, unity, the pressures will always be equal.

As, furthermore, the movements of the two pistons have equal extent, the motive power produced by each will evidently be the same; whence we should conclude, according to the proposition on page 64, that the quantities of heat consumed by each are the same, that is, that there passes from the body A to the body B the same quantity of heat in both cases.

* The law of Mariotte, which is here made the foundation upon which to establish our demonstration, is one of the best authenticated physical laws. It has served as a basis to many theories verified by experience, and which in turn verify all the laws on which they are founded. We can cite also, as a valuable verification of Mariotte's law and also of that of MM. Gay-Lussac and Dalton, for a great difference of temperature, the experiments of MM. Dulong and Petit. (See *Annals* de *Chimie* et *Physique,* Feb. 1818, t. vii. p.122.)

The more recent experiments of Davy and Faraday can also be cited.

The theories that we deduce here would not perhaps be exact if applied outside of certain limits either of density or temperature. They should be regarded as true only within the limits in which the laws of Mariotte and of MM. Gay-Lussac and Dalton are themselves proven.

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The heat abstracted from the body A and communicated to the body B, is simply the heat absorbed during the rarefaction of the gas, and afterwards liberated by its compression. We are therefore led to establish the following theorem:

When an elastic fluid passes without change of temperature from the volume U to the volume V, and when a similar ponderable quantity of the same gas passes at the same temperature from the volume U' to the volume V', if the ratio of U' to V' is found to be the same as the ratio of U to V, the quantities of heat absorbed or disengaged in the two cases will be equal.

This theorem might also be expressed as follows: *When a gas varies in volume without change of temperature, the quantities of heat absorbed or liberated by this gas are in arithmetical progression, if the increments or the decrements of volume are found' to be in geometrical progression.*

When a litre of air maintained at a temperature of ten degrees is compressed, and when it is reduced to one half a litre, a certain quantity of heat is set free. This quantity will be found always the same if the volume is further reduced from a half litre to a quarter litre, from a quarter litre to an eighth, and so on.

If, instead of compressing the air, we carry it successively to two litres, four litres, eight litres, etc., it will be necessary to supply to it always equal quantities of heat in order to maintain a constant temperature.

This readily accounts for the high temperature attained by air when rapidly compressed. We know that this temperature inflames tinder and even makes air luminous. If, for a moment, we suppose the specific heat of air to be constant, in spite of the changes of volume and temperature, the temperature will in-

p. *75*

crease in arithmetical progression for reduction of volume in geometrical progression.

Starting from this datum, and admitting that one degree of elevation in the temperature corresponds to a compression of 1/116, we shall readily come to the conclusion that air reduced to 1/14 of its primitive volume should rise in temperature about 300 degrees, which is sufficient to inflame tinder.*

The elevation of temperature ought, evidently, to be still more considerable if the capacity of the air for heat becomes less as its volume diminishes. Now this is probable, and it also seems to follow from the experiments of MM. Delaroche and Berard on the specific heat of air taken at different densities. (See the Memoire in the *Annals de Chimie*, t. 'XXXV. pp.72,224.)

The two theorems explained on pp.67 and 74 suffice for the comparison of the quantities of heat absorbed or set free in the changes of volume of elastic fluids, whatever may be the density and the chemical nature of these fluids, provided always that they be taken and maintained at a certain invariable temperature.

* When the volume is reduced 1/116, that is, when it becomes 115/116 of what it was at first, the temperature rises one degree. Another reduction of 1/116 carries the volume to $(115/116)^2$:, and the temperature should rise another degree. After x similar reductions the volume becomes $(115/116)^x$, and the temperature should be raised x degrees. If we suppose $(115/116)^x$-=1/14 and if we take the logarithms of both, we find

x = about 300

If we suppose $(115/116)^x$ =1/2, we find

x = 80^0;

which shows that air compressed one half rises 80^0

All this is subject to the hypothesis that the specific heat of air does not change, although the volume diminishes. But if, for the reasons hereafter given (pp.77,79), we regard the specific heat of air compressed one half as reduced in the relation of 700 to

616, the number 80^0 must be multiplied by 700/616, which raises it to 90^0.

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But these theories furnish no means of comparing the quantities of heat liberated or absorbed by elastic fluids which change in volume at different temperatures. Thus we are ignorant what relation exists between the heat relinquished by a litre of air reduced one half, the temperature being kept at zero, and the heat relinquished by the same litre of air reduced one half, the temperature being kept at **100.** The knowledge of this relation is closely connected with that of the specific heat of gases at various temperatures, and to some other data that Physics as yet does not supply.

The second of our theorems offers us a means of determining according to what law the specific heat of gases varies with their density.

Let us suppose that the operations described on page 65, in-stead of being performed with two bodies, A, B, of temperatures differing indefinitely small, were carried on with two bodies whose temperatures differ by a finite quantity-one degree, for example. In a complete circle of operations the bodyA furnishes to the elastic fluid a certain quantity of heat, which may be divided into two portions: (1) That which is necessary to maintain the temperature of the fluid constant during dilation; (2) that which is necessary to restore the temperature of the fluid from that of the body B to that of the body A, when, after having brought back this fluid to its primitive volume, we place it again in contact with the body A. Let us call the first of these quantities a and the second b. The total caloric furnished by the body A will be expressed by a + b.

The caloric transmitted by the fluid to the body B may also be divided into two parts: one, b', due to the cooling of the gas

p. 77

by the body B; the other, a', which the gas abandons as a result of its reduction of volume. The sum of these two quantities is a' + b'; it should be equal to a + b, for, after a complete cycle of operations, the gas is brought back exactly to its primitive state. It has been obliged to give up all the caloric which has first been furnished to it. We have then

$$a + b = a' + b';$$

or rather,

$$a - a' = b' - b.$$

Now, according to the theorem given on page 74, the quantities a and a' are independent of the density of the gas,

provided always that the ponderable quantity remains the same and that the variations of volume be proportional to the original volume. The difference a - a' should fulfill the same conditions, and consequently also the difference b' - b, which is equal to it. But b' is the caloric necessary to raise the gas enclosed in abcd (Fig. 2) one degree; b' is the caloric surrendered by the gas when, enclosed in abef, **it** is cooled one degree. These quantities may serve as a measure for specific heats. We are then led to the establishment of the following proposition:

The change in the specific heat of a gas caused by change of volume depends entirely on the ratio between the original volume and the altered volume. That is, the difference of the specific heats does not depend on the absolute magnitude of the volumes, but only on their ratio.

This proposition might also be differently expressed, thus:

When a gas increases in volume in geometrical progression, its specific heat increases in arithmetical progression.

p. 78

Thus, a being the specific heat of air taken at a given density, and a + h the specific heat for a density one half less, it will be, for a density equal to one quarter, a + 2h for a density equal to one eighth, a + 3h; and so on.

The specific heats are here taken with reference to weight. They are supposed to be taken at an invariable volume, but, as we shall see, they would follow the same law if they were taken under constant pressure.

To what cause is the difference between specific heats at constant volume and at constant pressure really due? To the caloric required to produce in the second case increase of volume. Now, according to the law of Mariotte, increase of volume of a gas should be, for a given change of temperature, a determined fraction of the original volume, a fraction independent of pressure. According to the theorem expressed on page 70, if the ratio between the primitive volume and the altered volume is given, that determines the heat necessary to produce increase of volume. It depends solely on this ratio and on the weight of the gas. We must then conclude that:

The difference between specific heat at constant pressure and specific heat at constant volume is always the same, whatever may be the density of the gas, provided the weight remains the same.

These specific heats both increase accordingly as the density of the gas diminishes, but their difference does not vary.*

* MM. Gay~lussac and Welter have found by direct experiments, cited in the Mecanique Celeste and in the Annales & Chimie et de Physiquc, July, 1822, p.267, that the ratio between the specific heat at constant pressure and the specific heat at constant volume varies very little with the density of the gas, According to what we have just seen, the difference should remain constant,

p. 79

Since the difference between the two capacities for heat is constant, if one increases in arithmetical progression the other should follow a similar progression: thus one law is applicable to specific heats at constant pressure.

We have tacitly assumed the increase of specific heat with that of volume. This increase is indicated by the experiments of MM. Delaroche and Berard: in fact these physicists have found 0.967 for the specific heat of air under the pressure of 1 metre of mercury (see Memoire already cited), taking for the unit the specific heat of the same weight of air under the pressure of O^m.760.

According to the law that specific heats follow with relation to pressures, it is only necessary to have observed them in two particular cases to deduce them in all possible cases: it is thus that, making use of the experimental result of MM. Delaroche and Berard which has just been given, we have prepared the following table of the specific heat of air under different pressures:

and not the ratio. As, further, the specific heat of gases for a given weight varies very little with the density, it is evident that the ratio itself experiences but alight changes.

The ratio between the specific heat of atmospheric air at constant pressure and at constant volume is, according to MM. Gay~lussac and Welter, 1.3748, a number almost constant *for* all press'ures, and even for all temperatures. We

$267 + 116$

have come, through other considerations, to the number = 1.44,

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which differs from the former 1/20, and we have used this number to prepare a table of the specific heats of gases at constant volume. So we need not regard this table as very exact, any more than the table given on p.80. These tables are mainly intended to demonstrate the laws governing specific heats of aeriform fluids.

1. 80

Table

The first column is, as we see, a geometrical progression, and the second an arithmetical progression.

We have carried out the table to the extremes of compression and rarefaction. It may be believed that air would be liquefied before acquiring a density 1024 times its normal density, that is, before becoming more dense than water. The specific heat would become zero and even negative on extending the table beyond the last term. We think, furthermore, that the figures of the second column here decrease too rapidly. The experiments which serve as a basis for our calculation have been made within too contracted limits for us to expect great exactness m the figures which we have obtained, especially in the outside numbers.

Since we know, on the one hand, the law according to which heat is disengaged in the compression of gases, and on the other, the law according to which specific heat varies with volume, it will be easy for us to calculate the increase of temperature of a gas that has been compressed without being allowed to lose heat

p. 81

In fact, the compression may be considered as composed of two successive operations: (1) compression at a constant temperature; (2) restoration of the caloric emitted. The temperature will rise through the second operation in inverse ratio with the specific heat acquired by the gas after the reduction of volume, -specific heat that we are able to calculate by means of the law demonstrated above. The heat set free by compression, according to the theorem of page 74, ought to be represented by an expression of the form

I)

$$s = A + B\log v,$$

s being this heat, *v* the volume of the gas after compression, A and B arbitrary constants dependent on the primitive volume of the gas, on its pressure, and on the units chosen.

The specific heat varying with the volume according to the law just demonstrated, should be represented by an expression of the form

$$z = A' + B'\log v,$$

A' and B' being the different arbitrary constants of A and B.

The increase of temperature acquired by the gas, as the effect of compression, is proportional to the ratio *s/z* or to the relation $\underline{A + B \underline{\log} v}$ It can be represented by this ratio itself; thus call-$A' + B'\log v$

ing it *t,* we shall have

t= $\underline{A + B\log v}$

$$A' + B'\log v$$

If the original volume of the gas is 1, and the original temperature zero, we shall have at the same time $t = 0$, log $v = 0$, whence

p. 82

$A = 0$; t will then express not only the increase of temperature, but the temperature itself above the thermometric zero.

We need not consider the formula that we have just given as applicable to very great changes in the volume of gases. We have regarded the elevation of temperature as being in inverse ratio to the specific heat; which tacitly supposes the specific heat to be constant at all temperatures. Great changes of volume lead to great changes of temperature in the gas, and nothing proves the constancy of specific heat at different temperatures, especially at

temperatures widely separated. This constancy is only an hypothesis admitted for gases by analogy, to a certain extent verified for solid bodies and liquids throughout a part of the thermometric scale, but of which the experiments of MM. Dulong and Petit have shown the inaccuracy when it is desirable to extend it to temperatures far above **100.**

According to a law of MM. Clement and Desormes, a law established by direct experiment, the vapor of water, under whatever pressure it may be formed, contains always, at equal weights the same quantity of heat; which leads to the assertion that steam, compressed or expanded mechanically without loss of heat, will always be found in a saturated state if it was so produced in the first place. The vapor of water so made may then be regarded as a permanent gas, and should observe all the laws of one. Consequently the formula

$$\sim = \underline{A+B\log v}\; A' + B'\log v$$

should be applicable to it, and be found to accord with the table of tensions derived from the direct experiments of M. Dalton.

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We may be assured, in fact, that our formula, with a convenient determination of arbitrary constants, represents very closely the results of experiment. The slight irregularities which we find therein do not exceed what we might reasonably attribute to errors of observation.

We will return, however, to our principal subject, from which we have wandered too far-the motive power of heat.

We have shown that the quantity of motive power developed by the transfer of caloric from one body to another depends essentially upon the temperature of the two bodies, but we have not shown the relation between these temperatures and the quantities of motive power produced. It would at first seem natural enough to suppose that for equal differences of temperature the quantities of motive power produced are equal; that is, for example, the passage of a given quantity of caloric from a body, A, maintained at 100, to a body, B, maintained at 50, should give rise to a quantity of motive power equal to that which would be developed by the transfer of the same caloric from a body, B, at 50, to a body, C, at zero. Such a law would.doubtless be very remarkable, but we do not see sufficient reason for admitting it a priori. We will investigate its reality by exact reasoning.

Let us imagine that the operations described on page 65 be conducted successively on two quantities of atmospheric air equal in weight and volume, but taken at different temperatures. Let us suppose, further, the differences of temperature between the bodies A and B equal, so these bodies would have for example, in one of these cases, the temperatures 100 and 100 - h(h being indefinitely small), and in the other 1 and 1 - h. The

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quantity of motive power produced is, in each case, the difference between that which the gas supplies by its

dilatation and that which must be expended to restore its primitive volume. Now this difference is the same in both cases, as anyone can prove by simple reasoning, which it seems unnecessary to give here in detail; hence the motive power produced is the same.

Let us now compare the quantities of heat employed in the two cases. In the first, the quantity of heat employed is that which the body A furnishes to the air to maintain it at the temperature of 100 during its expansion. In the second, it is the quantity of heat which this same body should furnish to it, to keep its temperature at one degree during an exactly similar change of volume. If these two quantities of heat were equal, there would evidently result the law that we have already assumed. But nothing proves that it is so, and we shall find that these quantities are not equal.

The air that we shall first consider as occupying the space abcd (Fig. 2), and having one degree of temperature, can be made to occupy the space abef, and to acquire the temperature of 100 degrees by two different means:

(1) We may heat it without changing its volume, then expand it, keeping its temperature constant.

(2) We may begin by expanding it, maintaining the temperature constant, then heat it, when it has acquired its greater volume.

Let a and b be the quantities of heat employed successively in the first of the two operations, and let b' and a′ be the quantities of heat employed successively in the second. As the final

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result of these two operations is the same, the quantities of heat employed in both should be equal. We have then

a + b = a' + b'

whence

a' - a = b - b'.

a' is the quantity of heat required to cause the gas to rise from 10 to **1000** when it occupies the space abef.

a is the quantity of heat required to cause the gas to rise from 10 to **1000** when it occupies the space abcd.

The density of the air is less in the first than in the second case, and according to the experiments of MM. Delaroche and Berard, already cited on page 78, its capacity for heat should be a little greater.

The quantity a' being found to be greater than the quantity a, b should be greater than b'. Consequently, generalizing the proposition, we should say:

The quantity of heat due to the change of volume of a gas is greater as the temperature is higher.

Thus, for example, more caloric is necessary to maintain at **100** the temperature of a certain quantity of air the volume of which is doubled, than to maintain at **10** the temperature of this same air during a dilatation exactly equal.

These unequal quantities of heat would produce, however, as we have seen, equal quantities of motive power for equal fall of caloric taken at different heights on the thermometric scale; whence we draw the following conclusion:

The fall of caloric produces more motive power at inferior than at superior temperatures.

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Thus a given quantity of heat will develop more motive power in passing from a body kept at 1 degree to another maintained at zero, than if these two bodies were at the temperature of 101^0 and 100^0.

The difference, however, should be very slight. It would be nothing if the capacity of the air for heat remained constant, in spite of changes of density. According to the experiments of MM. Delaroche and Berard, this capacity varies little- so little even, that the differences noticed might strictly have been attributed to errors of observation or to some circumstances of which we have failed to take account.

We are not prepared to determine precisely, with no more experimental data than we now possess, the law according to which the motive power of heat varies at different points on the thermometric scale. This law is

intimately connected with that of the variations of the specific heat of gases at different temperatures-a law which experiment has not yet made known to us with sufficient exactness. We will endeavor now to estimate exactly the motive power of heat, and in order to verify our fundamental proposition, in order to determine whether the agent used to realize the motive power is really unimportant relatively to the quantity of this power, we will select several of them successively: atmospheric air, vapor of water, vapor of alcohol.

Let us suppose that we take first atmospheric air. The operation will proceed according to the method indicated on page 65. We will make the following hypotheses: The air is taken under atmospheric pressure. The temperature of the body A is 1/1000

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of a degree above zero, that of the body B is zero. The difference is, as we see, very slight-a necessary condition here.

The increase of volume given to the air in our operation will be $1/116 + 1/267$ of the primitive volume; this is a very slight increase, absolutely speaking, but great relatively to the difference of temperature between the bodies A and B.

The motive power developed by the whole of the two operations described (page 65) will be very nearly proportional to the increase of volume and to the difference between the two pressures exercised by the air, when it is found at the temperatures $0^0.001$ and zero.

This difference is, according to the law of M. Gay-Lussac, 1/267000 of the elastic force of the gas, or very nearly 1/267000 of the atmospheric pressure.

The atmospheric pressure balances at 10.40 metres head of water; 1/267000 of this pressure equals $1/267000 \times 10^m.40$ of head of water.

As to the increase of volume, it is, by supposition, $1/116 + 1/267$ of the original volume, that is, of the volume occupied by one kilogram of air at zero, a volume equal to $0^{me}.77$, allowing for the specific weight of the air. So then the product,

$$(1/116 + 1/267) \times 0.77 \times 1/267000 \times 10.40$$

will express the motive power developed. This power is estimated here in cubic metres of water raised one metre.

If we carry out the indicated multiplications, we find the value of the product to be 0.000000372.

Let us endeavor now to estimate the quantity of heat em-

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ployed to give this result; that is, the quantity of heat passed from the body A to the body B.

The body A furnishes:

(1) The heat required to carry the temperature of one kilogram of air from zero to $0^0.001$;

(2) The quantity necessary to maintain at this temperature the temperature of the air when it experiences a dilatation of

1/116 + 1/267

The first of these quantities of heat being very small in comparison with the second, we may disregard it. The second is, according to the reasoning on page 68, equal to that which would be necessary to increase one degree the temperature of one kilogram of air subjected to atmospheric pressure.

According to the experiments of MM. Delaroche and Berard on the specific heat of gases, that of air is, for equal weights, 0.267 that of water. If, then, we take for the unit of heat the quantity necessary to raise 1 kilogram of water 1 degree; that which will be required to raise 1 kilogram of air 1 degree would have for its value 0.267. Thus the quantity of heat furnished by the body A is

0.267 units.

This is the heat capable of producing 0.000000372 units of motive power by its fall from $0^0.001$ to zero.

For a fall a thousand times greater, for a fall of one degree, the motive power will be very nearly a thousand times the former, or

0.000372.

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If, now, instead of 0.267 units of heat we employ 1000 units, the motive power produced will be expressed by the proportion

0.267/0.000372 = 1000/x , whence x = 372/267 = 1,395.

Thus 1000 units of heat passing from a body maintained at the temperature of 1 degree to another body maintained at zero would produce, in acting upon the air,

1.395 units of motive power.

We will now compare this result with that furnished by the action of heat on the vapor of water.

Let us suppose one kilogram of liquid water enclosed in the cylindrical vessel abcd (Fig. 4), between the bottom ab and the piston cd. Let us suppose, also, the two bodies A, B

maintained each at a constant temperature, that of A being a very little above

that of B. Let us imagine now the following operations:

(1) Contact of the water with the body A, movement

of the piston from the position cd to the position ef, formation

of steam at the temperature of the body A to fill the vacuum

produced by the extension of volume. We will suppose the space abef *Fig. 4*

large enough to contain all the water in a state of vapor.

1. Removal of the body *A*, contact of the vapor with the body *B*, precipitation of a part of this vapor, diminution of its elastic force, return of the piston from ef to ab, liquefaction of the rest of the vapor through the effect of the pressure combined with the contact of the body B.

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(3) Removal of the body B, fresh contact of the water with the body A, return of the water to the temperature of this body, renewal of the former period, and so on.

The quantity of motive power developed in a complete cycle of operations is measured by the product of the volume of the vapor multiplied by the difference between the tensions that it possesses at the temperature of the body A and at that of the body B. As to the heat employed, that is to say, transported from the body A to the body B, it is evidently that which was necessary to turn the water into vapor, disregarding always the small quantity required to restore the temperature of the liquid water from that of B to that of A.

Suppose the temperature of the body A 100 degrees, and that of the body B 99 degrees: the difference of the tensions will be, according to the table of M. Dalton, 26 millimetres of mercury or $0^m.36$ head of' water.

The volume of the vapor is 1700 times that of the water. If we operate on one kilogram, that will be 1700 litres, or $1^{me}.700$.

Thus the value of the motive power developed is the product

1.700 x 0.36 = 0.611 units,

of the kind of which we have previously made use.

The quantity of heat employed is the quantity required to turn into vapor water already heated to 100^0. This quantity is found by experiment. We have found it equal to 550^0, or, to speak more exactly, to 550 of our units of heat.

Thus 0.611 units of motive power result from the employment of 550 units of heat. The quantity of motive power result-

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ing from 1000 units of beat will be given by the proportion

$550/0.611 = 1000/x$, whence $x = 611/550 = 1.112$.

Thus 1000 units of heat transported from one body kept at 100 degrees to another kept at 99 degrees will produce, acting upon vapor of water, 1.112 units of motive power.

The number 1.112 differs by about 1/4 from the number 1.395 previously found for the value of the motive power developed by 1000 units of heat acting upon the air; but it should be observed that in this case the temperatures of the bodies A and B were 1 degree and zero, while here they are 100 degrees and 99 degrees. The difference is much the same; but it is not found at the same height in the thermometric scale. To make an exact comparison, it would have been necessary to estimate the motive power developed by the steam formed at 1 degree and condensed at zero. It would also have been necessary to know the quantity of heat contained in the steam formed at one degree.

The law of MM. Clement and Desormes referred to on page 82 gives us this datum. The constituent heat of vapor of water being always the same at any temperature at which vaporization takes place, if 550 degrees of heat are required to vaporize water already brought up to 100 degrees, 550 + 100 or 650 will be required to vaporize the same weight of water taken at zero.

Making use of this datum and reasoning exactly as we did for water at 100 degrees, we find, as is easily seen,

$$1.290$$

for the motive power developed by 1000 units of heat acting upon the vapor of water between one degree and zero. This num-

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ber approximates more closely than the first to

1.395.

It differs from it only 1/13, an error which does not exceed probable limits, considering the great number of data of different sorts of which we have been obliged to make use in order to arrive at this approximation. Thus is our fundamental law verified a special case.*

We will examine another case in which vapor of alcohol is acted upon by heat. The reasoning is precisely the same as for the vapor of water. The data alone are changed. Pure alcohol boils under ordinary pressure at $78^0.7$ Centigrade. One kilogram absorbs, according to MM. Delaroche and Berard, 207 units of heat in undergoing transformation into vapor at this same temperature, $78.^0.7$.

The tension of the vapor of alcohol at one degree below the boiling-point is found to be diminished 1/25. It is 1/25 less than the atmospheric pressure; at least, this is the result of the experiment of M. Betancour reported in the second part of *l'Architecture hydraulique* of M. Prony, pp.180, 195.

If we use these data, we find that, in acting upon one kilogram of alcohol at the temperatures of $78^0.7$ and $77^0.7$, the motive power developed will be 0.251 units.

This results from the employment of 207 units of heat. For 1000 units the proportion must be

$$207/0.254 = 1000/x, \text{ whence } x = 1.230.$$

*We find *(Annales de Chimie et de Physique, July,* 1818, p.294) in a memoir of M. Petit an estimate of the motive power of heat applied to air and to vapor of water. This estimate leads us to attribute a great advantage to atmospheric air, but it is derived by a method of considering the action of heat which is quite imperfect.

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This number is a little more than the 1.112 resulting from the use of the vapor of water at the temperatures 100^0 and 99^0; if we suppose the vapor of water used at the temperatures 78^0 and 77^0 we find, according to the law of MM. Clement and Desorme, 1.212 for the motive power due to 1000 units of heat. This latter number approaches, as we see, very nearly to 1.230. There is a difference of ouly 1/50.

We should have liked to be able to make other approximations of this sort-to be able to calculate, for example, the motive power developed by the action of heat on solids and liquids, by the congelation of water, and so on; but Physics as yet refuses us the necessary data.*

The fundamental law that we propose to confirm seems to us to require, however, in order to be placed beyond doubt, new verifications. It is based upon the theory of heat as it is understood to-day, and it should be said that this foundation does not appear to be of unquestionable solidity. New experiments alone can decide the question. Meanwhile we can apply the theoretical ideas expressed above, regarding them as exact, to the examination of the different methods proposed up to date, for the realization of the motive power of heat.

It has sometimes been proposed to develop motive power by the action of heat on solid bodies. The mode of procedure which naturally first occurs to the mind is to fasten immovably a solid body-a metallic bar, for

example by one of its extremities; to attach the other extremity to a movable part of the machine;

* Those that we need are the expansive force acquired by Solids and liquids by a given increase of temperature, and the quantity of heat absorbed or relinquished in the changes of volume of these bodies.

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then, by successive heating and cooling, to cause the length of the bar to vary, and so to produce motion. Let us try to decide whether this method of developing motive power can be advantageous. We have shown that the condition of the most effective employment of heat in the production of motion is, that all changes of temperature occurring in the bodies should be due to changes of volume. The nearer we come to fulfilling this condition the more fully will the heat be utilized. Now, working in the manner just described, we are very far from fulfilling this condition: change of temperature is not due here to change of volume; all the changes are due to contact of bodies differently heated-to the contact of the mettallic bar, either with the body charged with furnishing heat to it, or with the body charged with carrying it off.

The only means of fulfilling the prescribed condition would be to act upon the solid body exactly as we did on the air in the operations described on page 82. But for this we must be able to produce, by a single change of volume of the solid body, considerable changes of temperature, that is, if we should want to utilize considerable falls of caloric. Now this appears impracticable. In short, many considerations lead to the conclusion that the changes produced in the temperature of solid or liquid bodies through the effect of compression and rarefaction would be but slight.

(1) We often observe in machines (particularly in steam-engines) solid pieces which endure considerable strain in one way or another, and although these efforts may be sometimes as great as the nature of the substances employed permits, the variations of temperature are scarcely perceptible.

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(2) In the action of striking medals, in that of the rolling-mill, of the draw-plate, the metals undergo the greatest compression to which we can submit them, employing the hardest and strongest tools. Nevertheless the elevation of temperature is not great. If it were, the pieces of steel used in these operations Would soon lose their temper.

(3) We know that it would be necessary to exert on solids and liquids a very great strain in order to produce in them a reduction of volume comparable to that which they experience in cooling (cooling from 100^0 to zero, for example). Now the cooling requires a greater abstraction of caloric than would simple reduction of volume. If this reduction were produced by mechanical means, the heat set free would not then be able to make the temperature of the body vary as many degrees as the cooling makes it vary. It would, however, necessitate the employment of a force undoubtedly very considerable.

Since solid bodies are susceptible of little change of temperature through changes of volume, and since the condition of the most effective employment of heat for the development of motive power is precisely that all change of temperature should be due to a change of volume, solid bodies appear but ill fitted to realize this power.

The same remarks apply to liquids. The same reasons may be given for rejecting them.*

We are not speaking now of practical difficulties. They will be numberless. The motion produced by the dilatation and com-

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● The recent experiments of M. Oerstedt on the compressibility of water have shown that, for a pressure of five atmospheres, the temperature of this liquid exhibits no appreciable change. (See Annals de *Chimie* et de *Physique,* Feb. 1823, p.192.)

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pression of solid or liquid bodies would only be very slight. In order to give them sufficient amplitude we should be forced to make use of complicated mechanisms. It would be necessary to employ materials of the greatest strength to transmit enormous pressure; finally, the successive operations would be executed very slowly compared to those of the ordinary steam-engine, so that apparatus of large dimensions and heavy cost would produce but very ordinary results.

The elastic fluids, gases or vapors, are the means really adapted to the development of the motive power of heat. They combine all the conditions necessary to fulfill this office. They are easy to compress; they can be almost infinitely expanded; variations of volume occasion in them great changes of temperature; and, lastly, they are very mobile, easy to heat and cool, easy to transport from one place to another, which enables them to produce rapidly the desired effects. We can easily conceive a multitude of machines fitted to develop the motive power of heat through the use of elastic fluids; but in whatever way we look at it, we should not lose sight of the following principles:

(1) The temperature of the fluid should be made as high as possible, in order to obtain a great fall of caloric, and consequently a large production of motive power.

(2) For the same reason the cooling should be carried as far as possible.

(3) It should be so arranged that the passage of the elastic fluid from the highest to the lowest temperature should be due to increase of volume; that is, it should be so arranged that the cooling of the gas should occur spontaneously as the effect of rarefaction. The limits of the temperature to which it is possible

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to bring the fluid primarily, are simply the limits of the temperature obtainable by combustion; they are very high.

The limits of cooling are found in the temperature of the coldest body of which we can easily and freely make use; this body is usually the water of the locality.

As to the third condition, it involves difficulties in the realization of the motive power of heat when the attempt is made to take advantage of great differences of temperature, to utilize great falls of heat. In short, it is necessary then that the gas, by reason of its rarefaction, should pass from a very high temperature to a very low one, which requires a great change of volume and of density, which requires also that the gas be first taken under a very heavy pressure, or that it acquire by its dilatation an enormous volume-conditions both difficult to fulfill. The first necessitates the employment of very strong vessels to contain the gas at a very high temperature and under very heavy pressure. The second necessitates the use of vessels of large dimensions. These are, in a word, the principal obstacles which prevent the utilization in steam-engines of a great part of the motive power of the heat. We are obliged to limit ourselves to the use of a slight fall of caloric, while the combustion of the coal furnishes the means of procuring a very great one.

It is seldom that in steam-engines the elastic fluid is produced under a higher pressure than six atmospheres-pressure corresponding to about 160^0 Centigrade, and it is seldom that condensation takes place at a temperature much under 40^0. The fall of caloric from 160^0 to 40^0 is 120^0, while by combustion we can procure a fall of 1000^0 to 2000^0.

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In order to comprehend this more clearly, let us recall what we have termed the fall of caloric. This is the passage of the heat from one body, A, having an elevated temperature, to another, B, where it is lower. We say that the fall of the caloric is 100^0 or 1000^0 when the difference of temperature between the bodies A and B is 100^0 or 1000^0.

In a steam-engine which works under a pressure of six atmospheres the temperature of the boiler is 160^0. This is the body A. It is kept, by contact with the furnace, at the constant temperature of 160^0, and continually furnishes the heat necessary for the formation of steam. The condenser is the body B. By means of a current of cold water it is kept at a nearly constant temperature of 40^0. It absorbs continually the caloric brought from the body A by the steam. The difference of temperature between these two bodies is 160^0 - 40^0, or 120^0. Hence we say that the fall of caloric is here 120^0.

Coal being capable of producing, by its combustion, a temperature higher than 1000^0, and the cold water, which is generally used in our climate, being at about 10^0, we can easily procure a fall of caloric of 1000^0, and of this only 120^0 are utilized by steam-engines. Even these 120^0 are not wholly utilized. There is always considerable loss due to useless re-establishments of equilibrium in the caloric.

It is easy to see the advantages possessed by high-pressure machines over those of lower pressure. *This superiority lies essentially in the power of utilizing a greater fall of caloric*. The steam produced under a higher pressure is found also at a higher temperature, and as, further, the temperature of condensation remains always about the same, it is evident that the fall of

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caloric is more considerable. But to obtain from high-pressure engines really advantageous results, it is necessary that the fall of caloric should be most profitably utilized. It is not enough that the steam be produced at a high temperature: it is also necessary that by the expansion of its volume its temperature should become sufficiently low. A good steam-engine, therefore, should not only employ steam under heavy pressure, but *under successive and variable pressures, differing greatly from one another, and progressively decreasing.*

In order to understand in some sort a posteriori the advantages of high-pressure engines, let us sup-pose steam to be formed under atmospheric pressure and introduced into the cylindrical vessel abcd (fig.5), under the piston cd, which at first touches the bottom ab. The steam, after having moved the piston from ab to cd, will continue finally to produce its results in a manner with which we will not concern ourselves.

Let us suppose that the piston having moved to cd is forced downward to ef, without the steam being allowed to escape, or any portion of its caloric to be lost. It will be driven back into the space abef, and will increase at the same time in density, elastic force, and temperature. If the steam, instead of being produced under atmospheric pressure, had been produced just when it was being forced back into abef, and so that after its introduction into the cylinder it had made the piston move from ab to ef, and had moved it simply by its extension of volume, from ef to cd, the motive power produced would have been more considerable than in the first case. In fact, the movement of the

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piston, while equal in extent, would have taken place under the action of a greater pressure, though variable, and though progressively decreasing.

The steam, however, would. have required for its formation exactly the same quantity of caloric, only the caloric would have been employed at a higher temperature.

It is considerations of this nature which have led to the making of double-cylinder engines-engines invented by Mr. Hornblower, improved by Mr. Woolf, and which, as regards economy of the combustible, are considered the best. They consist of a small cylinder, which at each pulsation is filled more or less (often entirely) with steam, and of a second cylinder having usually a capacity quadruple that of the first, and which receives no steam except that which has already operated in the first cylinder. Thus the steam when it ceases to act has at least quadrupled in volume. From the second cylinder it is carried directly into the condenser, but it is conceivable that it might be carried into a third cylinder quadruple the second, and in which its volume would have become sixteen times the original volume. The principal obstacle to the use of a third cylinder of this sort is the capacity which it would be necessary to give it, and the large dimensions which the opening for the passage of the steam must have. We will say no more on this subject, as we do not propose here to enter into the details of construction of steam-engines. These details call for a work devoted specially to them, and which does not yet exist, at least in France.*

*We find in the work called *De la Richesse Minerale,* by M. Heron de ville-fosse, vol. ii**.** p. 50 and following, a good description of the steam-engines actually in use in mining. In England the steam-engine has been very fully discussed in the *Encyclopedia Britannica.* Some of the data here employed are drawn from the latter work.

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If the expansion of steam is mainly limited by the dimensions of the vessels in which the dilatation must take place, the degree of condensation at which it is possible to use it at first is limited only by the resistance of the vessels in which it is produced, that is, of the boilers.

In this respect we have by no means attained the best possible results. The arrangement of the boilers generally in use is entirely faulty, although the tension of the steam rarely exceeds from four to six atmospheres. They often burst and cause severe accidents. It will undoubtedly be possible to avoid such accidents, and meantime to raise the steam to much greater pressures than is usually done.

Besides the high-pressure double-cylinder engines of which we have spoken, there are also high-pressure engines of one cylinder. The greater part of these latter have been constructed by two ingenious English engineers, Messrs. Trevithick and Vivian. They employ the steam under a very high pressure, sometimes eight to ten atmospheres, but they have no condenser. The steam, after it has been introduced into the cylinder, undergoes therein a certain increase of volume, but preserves always a pressure higher than atmospheric. When it has fulfilled its office it is thrown out into the atmosphere. It is evident that this mode of working is fully equivalent, in respect to the motive power produced, to condensing the steam at 100^0, and that a portion of the useful effect is lost. But the engines working thus dispense with condenser and air-pump. They are less costly than the others, less complicated, occupy less space, and can be used in places where there is not sufficient water for condensation. In such places they are of inestimable advantage, since no others could take their place. These engines are principally employed

in England to move coal-wagons on railroads laid either in the interior of mines or outside of them.

We have, further, only a few remarks to make upon the use of permanent gases and other vapors than that of water in the development of the motive power of heat.

Various attempts have been made to produce motive power by the action of heat on atmospheric air. This gas presents, as compared with vapor of water, both advantages and disadvantages, which we will proceed to examine.

(1) It presents, as compared with vapor of water, a notable advantage in that, having for equal volume a much less capacity for heat, it would cool more rapidly by an equal increase of volume. (This fact is proved by what has already been stated.) Now we have seen how important it is to produce by change of volume the greatest possible changes of temperature.

(2)Vapors of water can be formed only through the intervention of a boiler, while atmospheric air could be heated directly by combustion carried on within its own mass. Considerable loss could thus be prevented, not only in the quantity of heat, but also in its temperature. This advantage belongs exclusively to atmospheric sir. Other gases do not possess it. They would be even more difficult to heat than vapor of water.

    1. In order to give to air great increase in volume, and by that expansion to produce a great change of

temperature, it must first be taken under a sufficiently high pressure; then it must be compressed with a pump or by some other means before heating it. This operation would require a special apparatus, an apparatus not found in steam-engines. In the latter, water is in

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a liquid state when injected into the boiler, and to introduce it requires but a small pump.

(4) The condensing of the vapor by contact with the refrigerant body is much more prompt and much easier than is the cooling of air. There might, of course, be the expedient of throwing the latter out into the atmosphere, and there would be also the advantage of avoiding the use of a refrigerant, which is not always available, but it would be requisite that the increase of the volume of the air should not reduce its pressure below that of the atmosphere.

(5) One of the gravest inconveniences of steam is that it cannot be used at high temperatures without necessitating the use of vessels of extraordinary strength. It is not so with air for which there exists no necessary relation between the elastic force and the temperature. Air, then, would seem more suitable than steam to realize the motive power of falls of caloric from high temperatures. Perhaps in low temperatures steam may be more convenient. We might conceive even the possibility of making the same heat act successively upon air and vapor of water. It would be only necessary that the air should have, after its use, an elevated temperature, and instead of throwing it out immediately into the atmosphere, to make it envelop a steam-boiler, as if it issued directly from a furnace.

The use of atmospheric air for the development of the motive power of heat presents in practice very great, but perhaps not insurmountable, difficulties. If we should succeed in overcoming them, it would doubtless offer a notable advantage over vapor of water.

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As to the other permanent gases, they should be absolutely rejected. They have all the inconveniences of atmospheric air, with none of its advantages. The same can be said of other vapors than that of water, as compared with the latter.

If we could find an abundant liquid body which would vaporize at a higher temperature than water, of which the vapor would have, for the same volume, a less specific heat, which would not attack the metals employed in the construction of machines, it would undoubtedly merit the preference. But nature provides no such body.

The use of the vapor of alcohol has been proposed. Machines have even been constructed for the purpose of using it, by avoiding the mixture of its vapor with the water of condensation, that is, by applying the cold body externally instead of introducing it into the machine. It has been thought that a remarkable advantage might be secured by using the vapor of alcohol in that it possesses a stronger tension than the vapor of water at the same temperature. We can see in this only a fresh obstacle to be overcome. The principal defect of the vapor of water is its excessive tension at an elevated temperature; now this defect exists still more strongly in the vapor of alcohol. As to the relative advantage in a greater production of motive power, -an advantage attributed to it, -we know by the principles above demonstrated that it is imaginary.

It is thus upon the use of atmospheric air and vapor of water that subsequent attempts to perfect heat-engines should be based. It is to utilize by means of these agents the greatest possible falls of caloric that all efforts should be directed.

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Finally, we will show how far we are from having realized, by any means at present known, all the motive power of combustibles.

One kilogram of carbon burnt in the calorimeter furnishes a quantity of heat capable of raising one degree Centigrade about 7000 kilograms of water, that is, it furnishes 7000 units of heat according to the definition of these units given on page 88.

The greatest fall of caloric attainable is measured by the difference between the temperature produced by combustion and that of the refrigerant bodies. It is difficult to perceive any other limits to the temperature of combustion than those in which the combination between oxygen and the combustible may take place. Let us assume, however, that 1000^0 may be this limit, and we shall certainly be below the truth. As to the temperature of the refrigerant, let us suppose it $.0^0$ We estimated approximately (page 91) the quantity of motive power that 1000 units of heat develop between 100^0 and 99^0. We found it to be 1.112 units of power, each equal to 1 metre of water raised to a height of 1 metre.

If the motive power were proportional to the fall of caloric, if it were the same for each thermometric degree, nothing would be easier than to estimate it from 1000^0 to $\mathbf{0^0}.$ Its value would be

$$1.112 \text{ x } 1000 = 1112.$$

But as this law is only approximate, and as possibly it deviates much from the truth at high temperatures, we can only make a very rough estimate. We will suppose the number 1112 reduced one-half, that is, to 560.

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Since a kilogram of carbon produces 7000 units of heat, and since the number 560 is relatively 1000 units, it must be multiplied by 7, which gives

$$7 \text{ x } 560 = 3920.$$

This is the motive power of 1 kilogram of carbon. In order to compare this theoretical result with that of experiment, let us ascertain how much motive power a kilogram of carbon actually develops in the best-known steam-engines.

The engines which, up to this time, have shown the best results are the large double-cylinder engines used in the drainage of the tin and copper mines of Cornwall. The best results that have been obtained with them are as follows:

65 millions of lbs. of water have been raised one English foot by the bushel of coal burned (the bushel weighing 88 lbs.). This is equivalent to raising, by a kilogram of coal, 195 cubic metres of water to a height of 1 metre, producing thereby 195 units of motive power per kilogram of coal burned.

195 units are only the twentieth of 3920, the theoretical maximum; consequently 1/20 only of the motive power of the combustible has been utilized.

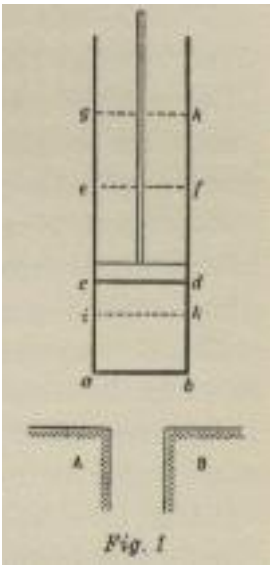We have, nevertheless, selected our example from among the best steam-cngines known.

Most engines are greatly inferior to these. The old engine of Charnot, for example, raised twenty cubic metres of water thirty-three metres, for thirty kilograms of coal consumed, which amounts to twenty-two units of motive power per kilogram, -a result nine times less than given above, and one hundred and eighty times less than the theoretical maximum.
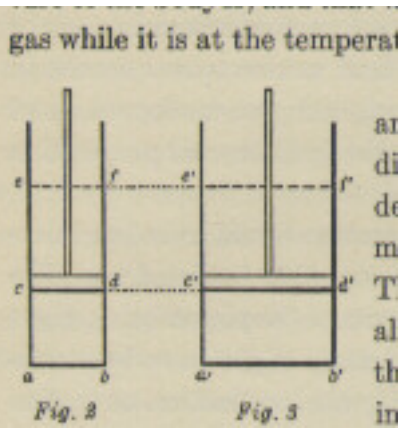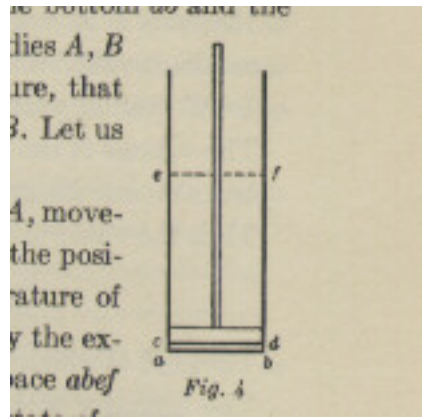
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We should not expect ever to utilize in practice all the motive power of combustibles. The attempts made to attain this result would be far more hurtful than useful if they caused other important considerations to be neglected. The economy of the combustible is only one of the conditions to be fulfilled in heat-engines. In many cases it is only secondary. It should often give precedence to safety, to strength, to the durability of the engine, to the small space which it must occupy, to small cost of installation, etc. To know how to appreciate in each case, at their true value, the considerations of convenience and economy which may present themselves; to know how to discern the more important of those which are only accessories; to balance them properly against each other, in order to attain the best results by the simplest means: such should be the leading characteristics of the man called to direct, to co-ordinate among themselves the labors of his comrades, to make them co-operate towards one useful end, of whatsoever sort it may be.
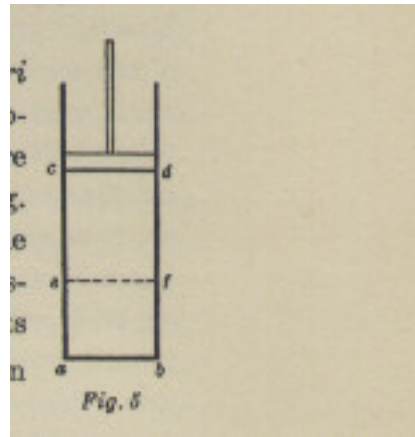
Fig. 1

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Fig. 2   Fig. 3

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Fig. 4

Fig. 5

TABLE OF THE SPECIFIC HEAT OF GASES

Names of Gases	Specific Heat under Const. Press.	Specific Heat at Const. Vol.
Atmospheric Air	1.000	0.700
Hydrogen Gas	0.903	0.603
Carbonic Acid	1.258	0.958
Oxygen	0.976	0.676
Nitrogen	1.000	0.700
Protoxide of Nitrogen	1.350	1.050
Olefiant Gas	1.553	1.253
Oxide of Carbon	1.034	0.734

TABLE OF THE ELEVATION OF TEMPERATURE OF GASES THROUGH THE EFFECT OF COMPRESSION

Names of Gases	Elevation of Temperature for a Reduction of Volume of 1/116
Atmospheric Air	1.000
Hydrogen Gas	1.160
Carbonic Acid	0.730
Oxygen	1.035
Nitrogen	1.000
Protoxide of Nitrogen	0.667
Olefiant Gas	0.558
Carbonic Oxide	0.965

SPECIFIC HEAT OF AIR

Pressure in Atmospheres	Specific Heat, that of Air under Atmospheric Pressure being 1	Pressure in Atmospheres	Specific Heat, that of Air under Atmospheric Pressure being 1
1/1024	1.840	1	1.000
1/512	1.756	2	0.916
1/256	1.672	4	0.832
1/128	1.588	8	0.748
1/64	1.504	16	0.664
1/32	1.420	32	0.580
1/16	1.336	64	0.496
1/8	1.252	128	0.412
1/4	1.165	256	0.328
1/2	1.084	512	0.244
1	1.000	1024	0.160