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Energy Is Conserved!

There are no neutral narratives. A narrative presentation begins—assuming this is the intent—long before it is able to make explicit the perspective it is framing. I have laid out my interests, but the way I have constructed "The Invention of Mechanics: Power and Reason" has already engaged the reader in ways that harmonized with my own perspective. There, I contrasted two of Lagrange's successors: Hamilton, for whom mechanical energy is not just conserved when a mechanical, or dynamic, system undergoes change but becomes, through the Hamiltonian operator, a fulcrum for all the representational changes possible for the system, including the specific change known as movement; and Carnot, the inventor of a very different use of the concept of conservation, which no longer describes autonomous change but corresponds to the ideal of completely controlling the power of heat to produce mechanical work. In the case of the heirs of the Lagrangian event, I did not have to slow down the movement of history. Every physicist is familiar with it. The only difference might be in the insistence of my emphasis. Every physicist "knows" that Hamiltonian dynamics introduces only "conservative" forces that conserve mechanical energy. But some don't see anything remarkable in this restriction. All of Carnot's successors "know" that the change from state to state that constitutes the ideal cycle invented by Carnot, a cycle wherein the equivalence between "cause" and "effect" is ensured *against* the "natural" tendency of heat to pass spontaneously and without mechanical consequence from a hot body to a cold body, is merely a laborious "mimicry" of conservative dynamic change. But the majority accept that this mimicry acknowledges its subordination to the original model. We need to slow down, however, when we are no longer dealing with what customarily "goes without saying" but with the question today associated with the hierarchy of physics. Why do the requirements of dynamics, as I have presented them, no longer impose any limitation on its relevance? Why does the fact that a "force" or a phenomenon is "dissipative" today simply indicate that its definition is "approximate," marked by human finiteness and ignorance, in other words, a part of "phenomenological physics"? What has happened?

The reader knows that the next step will be to bring these two traditions into contact with one another. Maybe she is already anticipating the dramatic turn of events that transformed the perspective of physics in the mid-nineteenth century. From human respiration to the steam engine, from the burning candle to the electrochemical battery: all phenomena, whether part of nature or artificially produced, *conserve energy.* So, I have led the reader to "expect" to see the conservation of energy framed within the context of a very specific problem, one that establishes a very specific connection between the question of the Lagrangian heritage and an event that has affected the very identity of physics, that has transformed the evaluation of what the practice of physicists allows them to claim: the discovery of the conservation of energy.

The conservation of energy is the quintessential example of this type of knowledge, which for Planck established the physicist's vocation: faith in the ability of physics to reveal an intelligible world, independent of our interests and practices. To reveal a world and not construct an objective definition—herein lies the difference between Planck's vision of the world and the object of mechanics that made its way out of Galileo's laboratory and was consecrated by Lagrange's equations. The mechanical object had the power to dictate the way in which it would be defined, and this was the primary reason for its interest. It made it possible to bring together around it those who would invent the mathematical representation it sanctioned but not to gather together disparate phenomena, to represent the world. On the contrary, it had to be selected within that world, then isolated and purified. In other words, it is, with respect to its existence as an experimental object, radically dependent on our interests and practices: the ball has to be round, the inclined plane smooth, and it would be even better if air were absent when trying to satisfy the requirements on which the power of mechanical representation depends. This is not at all the case, however, for the energy conserved—at least after 1850—by any natural process, whether selected by practitioners or part of nature (which was soon to include the stars).

As we now know, the discovery of the conservation of energy was one of those "simultaneous discoveries" that draw the attention of historians of science. As if it were "in the air," so to speak. However, Thomas Kuhn has also shown that statements made after 1840 can only be assimilated retrospectively,[1] each author conferring a distinct meaning on what would become "energy" (and what, at the time, was usually called "force").[2] Contrary to Galilean acceleration, for example, where the inclined plane produced both the measurement and its interpretation, the devices that revealed the conservation of energy were unable to confer a determinate interpretation on their results. A scientist confronting such a result was not forced to see what the author of the device wanted her to see. The matter was open to discussion—of which there were many.

To understand the background of those discussions, we need first to distinguish conversion from conservation. The notion of conversion between "forces" was initially an aesthetic idea, which communicated with the presentation of an "indestructible force" that gave nature its permanent unity. As such, the idea did not, strictly speaking, have an author. We can trace it back to Leibniz's "live force" or to the post-Kantian philosophy of nature. The novelty that characterized the first decades of the nineteenth century is found in the ability to see older phenomena (a burning candle or the heat given off by a chemical reaction) and new (electrolysis, the electric battery, the steam engine) as unanimously confirming universal convertibility. A collection of dispersed "facts" from distinct practices, with distinct interpretations, can be unified if it is seen as a "network" ensuring the conversion of any kind of force (or energy) into another. This was not a "thesis" that would have been negotiated among the different protagonists but a "way of seeing," an aesthetic, that brought together precursors or authors of statements we judge to be "scientific"; and such statements came from physicians, engineers, meteorologists, and physicists specializing in motion, heat, electricity, or magnetism. Weren't they all involved in conversion processes?

The notion of conservation, however, implies measurement. It is not only a question of indestructibility, for what is now at stake is the creation of a device that can be used to *quantify* the conversion. In 1843, Joule identified the quantitative equivalence between heat and work by correlating the rise in the temperature of water in which a system of blades turns to the work needed to produce the movement of those blades. The conversion of mechanical work into heat can now be characterized by a "mechanical" equivalent of heat: this will be the amount of work necessary to increase the temperature of a kilogram of water by one degree.

While the device used to determine the quantity of what disappeared and the quantity of what appeared doesn't reject the egalitarian network of conversion processes, it certainly distorts it. In fact, measurement privileges mechanical work, which will become the common standard of reference. Correlatively, it privileges laboratory practitioners, for physicians and naturalists are incapable of submitting "their" energies to this type of measurement. But here we must proceed cautiously. Joule's measurement may indeed have taken place in the lab, but it is not, like the measurement of a Galilean body, objective in the strong sense, simultaneously creating the conditions for understanding the phenomenon. It is an engineering measurement, based on the concept of work where, as we saw in "The Invention of Mechanics," the price of generality is silence concerning the nature of what is being measured. Measurement requires that two phenomena be related by an equivalence, yes, but this relation is contingent upon the measurement device, unlike measurements involving the pendulum, where motion is converted "spontaneously" into potential energy and vice versa.

Measurement requires equivalence, but does equivalence provide relevant access to the intelligibility of natural processes? For what may be the first time since Newton's adversaries questioned the breakdown of light by a prism in the eighteenth century,[3] the question of what the laboratory does, of the relevance of the operations it makes possible, has become critical. As Friedrich Engels, a connoisseur in this matter, noted: "If we change heat into mechanical motion or vice versa, is not the quality altered while the quantity remains the same? Quite correct. But it is with change of form of motion as with Heine's vices; anyone can be virtuous by himself, for vices two are always necessary. Change of form of motion is always a process that takes place between at least two bodies, of which one loses a definite quantity of motion of one quality (e.g. heat), while the other gains a corresponding quantity of motion of another quality (mechanical motion, electricity, chemical decomposition)."[4]

In other words, quantitative equivalence cannot be used to contradict qualitative transformation, to reduce it to an underlying identity, because it is associated with a condition—*there must be two,* an interaction must take place—about which it remains silent. The apparatus used to show that what one gains the other loses subjects transformation to the imperative of measurement, but that measurement by itself is incapable of identifying what it makes equivalent. What is heat? How does it differ from mechanical work? How is chemical energy or electrical energy different from that work? The equality reveals nothing. The conflict over interpretation has begun.

The axis of the conflict is obviously the relationship between "mechanical" conservation and the new, "energy" conservation. This gives a central role to the notion of work, the shared reference for the two types of equivalence. Work is "mechanical currency" and, as such, has been used as the common unit of measurement, but, again as such, it is quite incapable of providing the "reason" for the energy transformation, being merely the standard. Does the conservation of energy nevertheless reflect the secret omnipotence of "mechanical reasons," justifying the reduction of qualitatively different forms of energy to mechanical energy alone? This is Helmholtz's thesis. Or, moving to the opposite extreme, does it allow us to question, on behalf of the logic of qualitative multiplicity, mechanical reason itself, that is, the privileged intelligibility of (rational) mechanics? This is Engels's thesis. Following Helmholtz's interpretation, force and work together characterize a world hidden from direct observation. The ideal pendulum triumphs over the imperfect pendulum, whose movement is gradually dampened, for the spontaneous dampening of mechanical motion has as its equivalent the release of heat, which is itself only a form of hidden mechanical motion, possibly analogous to the vibration of atoms in matter. Following Engels, even in the case of mechanics, there must, upon closer inspection, be "two parties": there must be an interaction so that the energy associated with motion is converted into potential energy, and vice versa. For Engels, work is, in all cases, a practical measurement that depends on artificial devices. Thus, the ideal pendulum becomes an unreliable witness in that it appears to justify making the equivalence measured by work the reason for its movement and, therefore, to assign a purely mechanical identity to cause and effect. But Joule's apparatus is now a reliable (modest) witness. It illustrates the instrumental nature of measurement and enables work, as well as the mechanical force corresponding to it, to be interpreted. These are purely operational notions, which are neutral about the identity of the terms whose quantification is made possible by reciprocal measurement.

The practice of the physicist is at stake here. For Engels it entailed a lucidity that questions the very meaning of mechanics: the Galilean object seemed to confer an objective character, dictated by the object, on categories of measurement, but this power appeared to be retroactively contingent, a nonrepresentative particular case of what we can require of nature.[5] For Helmholtz, on the other hand, the conservation of energy justified a universalization of the requirements of mechanics, which no longer defined only the ideal object of mechanics but the conditions of intelligibility of any natural phenomenon. All other fields of inquiry, therefore, had to accept the central importance of the quantitative equality of cause and effect.[6]

In making Helmholtz and Engels symmetrical proponents of the two most antagonistic interpretations of the conservation of energy, we indicate that we are outside the history of physics properly speaking, where this symmetry doesn't exist. In the context of that history, one is a respectable, if mistaken, protagonist, while the other is most often described as an ideological intruder. But this is a hasty judgment and needs to be slowed down to become interesting. A problem then arises. For it indicates that the invention of the "properly physical" issues of conservation is not defined in the arena that circumscribes the tension between Galileo's pendulum and Joule's system of moving blades. In fact, there was another protagonist, and this protagonist descends in a direct line not from the physics of forces but from the rational mechanics of changes of state—that is, the ideally reversible cycle of transformation introduced by Sadi Carnot.

After a complex history, the Carnot cycle would become the "arena" in which the relationship between mechanical energy and what was to be called "thermodynamic" energy, governed by two principles, would be determined. I will discuss that history and those principles in the following pages, but first I want to point out the contrast between the problems they introduced and the related problems the conservation of energy has led to (and which it will continue to engender in "scientific culture").

The conservation of energy introduced "important questions" and was able to attract the interest of a broad range of disciplines: philosophy, physics, biology, medicine, sociology, economy, psychology. Freud comes to mind, but also the physicist Wilhelm Ostwald, who retraced human history in terms of the energy resources made available by human technology and analyzed the psychopathological episodes that characterized the lives of "great men" in terms of energy output. The conservation of energy was a "cultural event" with indeterminate limits, and it is reasonable to assume that the "scientific event" that played out in the arena of the Carnot cycle cannot be disassociated historically from this cultural event any more than Galileo's laws can be disassociated from his confrontation with the Catholic church. But, like Galileo's laboratory, the arena defined by questions about the Carnot cycle was unique in the sense that the issues that arose there could only be understood and discussed by specialists. It is not so much a question of competence, even if the formulation of the issues makes it necessary to grasp the difference between energy transformations that can be associated with a series of changes of state and ordinary energy transformations, for example, those involved in Joule's system of blades.[7] Aside from competence, it is interest that drives the selection of the protagonists. Neither the biologist, nor the physician, nor for that matter the dialectical philosopher, has any reason to take an interest in the now crucial issue constituted by the concept of change of state. Nothing they are involved in adds any relevance to the concept.

Moreover, it is not simply a question of understanding how the Carnot cycle came to serve as the arena in which what we respectively refer to as the "cultural" and "properly scientific" issues of the conservation of energy were differentiated. We also need to understand its somewhat strange status within twentieth-century physics. The first-year student of physics or chemistry still learns the Carnot cycle in the way Clausius redefined it, but this "required chapter" in the curriculum most often inspires boredom and confusion. The student doesn't really understand why it is even necessary. Therefore, the arena did not have the ability to define its issues but only to serve as a framework in which questions whose answers were found elsewhere were formulated.

In any event, we can understand the student's puzzlement: the Carnot cycle reinterpreted by Clausius has become a very strange creature. Its invention by Carnot was part and parcel of a science whose demise the conservation of energy announced: the science of heat was identified with a fluid that was conserved and whose behavior could be used, in particular, to explain the experimental relationship between pressure, volume, and temperature that characterizes a gas.[8] The science of caloric was a cutting-edge science in the first half of the nineteenth century and it is this that Carnot not only worked with but connected with the great mechanical tradition of the conservation of a cause in the effect. This connection, invented by Carnot and realized in his cycle, lost its justification with the destruction of caloric heat. The great irony of the Carnot cycle, the one that often provokes the disgust of students but that also serves as the arena in which the "before" and "after" of the conservation of energy are measured, is that the cycle itself and the optimal yield it defines have survived. It is a connection that no longer connects, a bridge that ties together two banks whose highway systems have been modified so extensively that one has to wonder why it was built in the first place. It is as if the Carnot cycle, which captured energy transformations, was an abstraction that came from nowhere and to which no intuition leads.

The cycle appears as just such an abstraction because the caloric theory, that is, the conservation of a "heat-substance," which led to its invention and made it intuitively intelligible, is now forgotten. The amount of caloric contained in a body is obviously not directly measurable according to the terms of this theory. But one thing is certain: if a given quantity of gas, whose volume and pressure have changed, and which has received or given off heat, returns to the initial values of its pressure, volume, and temperature variables, it means the gas must have given up as much heat as it took in during the cycle of its transformations. That is why Carnot was able to define the states of his cycle in terms of pressure, volume, and temperature without having any way to determine how much heat was absorbed from the hot source and how much was transferred to the cold source. Regardless of the path taken in moving from one state to another, whether the system is compressed at constant temperature and then cooled or cooled at constant volume and then compressed, the conservation of caloric ensured that, from the moment the cycle becomes closed, all the heat absorbed has been restored. And this holds true even for "real," that is, nonideal, cycles. Also, any transition from one state to another, where both are characterized in terms of pressure, temperature, and volume, should imply that, regardless of the path between those two states, the same quantity of caloric is absorbed or released, the only difference between the paths—or between ideal and nonideal transitions—being the mechanical work produced or consumed at the time. In other words, the conservation of heat or caloric served as a fixed point, ensuring that the "same quantity" could be described, even if this quantity could not be measured. When the caloric theory gave way to the conservation of energy, when Carnot's apparatus stopped transmitting heat from a hot source to a cold source but converted the heat into work, the cycle no longer offered any obvious guarantee about anything. Quite the contrary, it now became a problem: why couldn't all the heat received from the hot source be converted into work?

The same holds true on the other "bank," where we find the conservation of cause and effect. Carnot had shown that because of its reversibility, the output of his ideal cycle was better than any other device could provide: for a given amount of caloric passing from a source of heat to a source of cold, it produced the maximum possible amount of mechanical work. However, Carnot's demonstration was based on a reductio ad absurdum that was traditional in mechanics. For if a hypothetical cycle had greater output, then by coupling it to an ideal Carnot cycle operating in reverse, as a heat pump, it would *freely* produce mechanical work. But if the heat is converted into work, the absurdity disappears: the result of the coupling is not the free production of work but the conversion of more heat into work. Once again we are forced to ask, why can't all the heat be converted into work? The optimal output defined by the ideal Carnot cycle has become an enigma.

If the Carnot cycle, part of a body of physics that died with the arrival of the conservation of energy, has survived, it is because it identified and implemented an *ideal operation,* whose reversibility in itself guaranteed that all loss had been eliminated. But the loss of what? That is the question we must now answer. Not energy, for this was both a certainty and the primary difficulty. Whether heat flows directly between two bodies at different temperatures or is "converted" into work, measured by a change in volume, *in all cases energy is conserved.* Because the cycle is reversible, it introduces a cause that is conserved in the effect it produces, but this "cause" must be completely distinct from energy, for the energy balance is completely indifferent to the ideal, conservative nature of the cycle. The cycle speaks of the impossibility of converting, for a given temperature difference, more heat than is defined by Carnot's optimal output, but the energy being conserved is silent about the question of possible and impossible conversions between distinct forms of energy. In other words, the conservation of energy doesn't have the ability to characterize the ideal invented by Carnot because it is indifferent to the reversibility of the cycle, just as it is indifferent to the distinction between the ideal pendulum and the real pendulum whose movement is slowed down by friction. So, in relation to what conservation is the loss eliminated by the cycle defined?

The challenge of defining what it is that reversible transformations conserve may well be what separated the new "scientific" thermodynamics from the "great questions" brought about by the conservation of energy. We know, from the history of mechanics, why reversibility is privileged. Mechanical movements that conserve energy, that do not gradually slow down as a result of friction, can be described by a state function, that is, expressed as a series of changes of state. And it is this possibility that has given mechanics its formidable inventive power and, at the same time, limited that power to the class of ideal movements alone, devoid of friction. The general conservation of energy seemed to have flattened this difference. Because a movement that is slowed by friction "conserves" energy, a part of the mechanical energy is "simply" converted into heat. But the energy that is conserved, precisely because it is always conserved, has lost its status as a state function. It can no longer distinguish between the ideal situation, where the “cause” is conserved in its “effect,” and the dissipative situation, where the cause is exhausted in producing a lesser and sometimes null effect (as is the case when heat is spontaneously transmitted from a hot body to a cold). This is the new message of the Carnot cycle: even if all energy transformations conserve energy, they are not all the same, and it is the definition of this nonequivalence that needs to be made explicit. In other words, the ideal output defined by Carnot, the determinate character of the relationships between heat energy consumed and mechanical energy produced at the conclusion of the ideal cycle, traces the enigmatic figure of a new *state function.*