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Joule's Scientific Outlook

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In commemorating Joule's pioneering work to-day, I should like to discuss it from two different points of view: the one more general and also more historical, the other more specifically concerned with its scientific aspect.

The first point I want to make is that we cannot really understand how a career like Joule's was possible if we do not put it in its proper social background. If we try to consider Joule as an isolated personality, without regard to the environment in which he worked and lived, we get a very distorted view of his significance in the development of physics.

It is a striking fact that the science of thermodynamics (the science of energy in its various forms and transformations) is hardly more than a century old; yet, if we go back in history we find that the Greeks had already some knowledge of the motive power of heat. However, so far as we know, they only used this knowledge to make a sort of toy, whose description we find in Heron's work on Pneumatics. Quite another use was made of steam power in the 18th century in the Cornish mines, where steam engines were employed to pump water out of the mines. From these beginnings steam engines gradually developed, in the course of the 18th and beginning of the 19th century, into a major source of motive power for large industrial machinery. Why this difference in outlook between Hellenistic society and the kind of society that existed in this country in the 18th century? I cannot attempt to tackle this problem to-day; but the contrast that I have just pointed out brings to mind very forcibly the importance of the economic and social background for the development of science.

Scientific ideas and principles are in a sense eternal and universal: they are lying ready to be discovered and contemplated at all times and in all circumstances. But historically things do not happen that way. The laws of Nature are only discovered when there is an incentive to search for them, most often in order to put them to some kind of social use. Science is not at all what it is sometimes represented to be—some contemplation of Nature which could go on in temples erected for the purpose. It is essentially a social activity and one of the most important of all the activities that go to build human society.

The birth and growth of thermodynamics offers a classic illustration of this social aspect of science, because in this case the link between social and economic motives on the one hand and scientific ideas on the other appears

in a particularly clear and simple way. Joule's case, moreover, shows the rather subtle nature of this link. It would be quite wrong to say that Joule started his investigation of the connection between heat and mechanical power with a view to perfecting the engines providing such power. That could have been so, but as a historical fact Joule was primarily animated by pure scientific curiosity. His interest was first aroused by the production of heat by the passage of an electric current through a conductor. This phenomenon had been somewhat neglected by Davy and Faraday, who had concentrated their attention on other perhaps more immediate aspects of voltaic electricity. There was thus hope for an alert young man of independent mind to reap some new results by following up this line of research. It was only at the conclusion of the long series of experiments which led him to the formulation of the law of conservation of energy that he gave consideration to the implications of this law for the utilization of natural forces as the source of mechanical power.

The socially significant circumstance in Joule's attitude to a purely scientific problem is, however, the direction that his curiosity took. The attention that he paid to the relations of equivalence between heat and other physical phenomena was an entirely novel outlook at the time. It was a kind of curiosity quite different from that which inspired Davy and Faraday in looking at the same phenomenon. In fact, we have here one of the most striking examples of a physical phenomenon looked upon by great physicists from two quite different points of view. Joule's way of looking at it was so strange, so novel, that it was not immediately understood, even by the most prominent scientists of the time. This is convincingly demonstrated by the fact that the first paper submitted by Joule to the Royal Society was not judged of sufficient interest to be published in full.

It is said that Joule, when asked many years later what he thought of this rejection of his first paper by the Royal Society, answered: "I was not surprised. I could imagine those gentlemen in London sitting round a table and saying to each other, 'what good could come out of a town where they dine in the middle of the day?'" This remark shows, at any rate, that Joule was keenly aware of the real motive underlying this lack of understanding between the gentlemen in London and the lonely young man in Manchester. It is clear, in the first place, that this misunderstanding is not of a scientific nature, since, of course, the phenomenon is there for anybody to see and study. It is a misunderstanding based on a difference of social outlook.

The scientists who then composed the membership of the Royal Society had received the inspiration of their work in the 18th century, and the scientific effort of the 18th century was directed towards quite different problems from those that were in the centre of interest in a growing manufacturing centre such as Manchester in the early 19th century. The scientific tradition of the 18th century derived from Newton's work, and, like the latter, was determined by problems concerning not an industrial community but a commercial one. The problems of navigation focussed attention especially upon astronomy and optics. Considerations of mechanical power and of the utilization of the forces of Nature as sources of such power were not foremost in people's minds in the 18th century because production then was of a very limited scope and was mainly on an artisanal level.

It was only when the accumulation of wealth in the closing years of the 18th century created a demand for increased production that the need was felt of replacing hand tools by machine tools, and then of supplying power to drive the machines. These changes in the methods of production brought

about a revolution in the social set-up of society. A new class of people arose whose activity centred on manufacturing rather than on commerce. The interest of those people in science was naturally directed towards the utilization of the forces of Nature in obtaining mechanical power. It is in just such a community that Joule grew up. The son of a wealthy brewer, he could look forward to a life of leisure. Thus, on the one hand, he was able to indulge in investigations inspired by pure scientific curiosity; but, on the other, the background of this curiosity was nevertheless determined by the interests of the people in whose environment he was brought up. He was led to study those aspects of the phenomena which might throw light on the possibilities of transformation of various natural agencies into each other and into mechanical power.

Joule is not, of course, the only example of the influence exerted by the new industrial class. At about the same time as he engaged in his researches, Grove, a distinguished amateur of science, gave his famous lecture "On the correlation of the forces of Nature" at the London Institution. In this lecture the various aspects of the transformation of natural agencies into each other are treated at length, though only in a qualitative manner, and the novelty of this way of looking at natural phenomena is emphasized. It is significant that Grove's audience was not the Royal Society but the members of an Institution newly founded by the industrialists for the purpose of discussing scientific problems of special interest to them. It took quite a long time before the new way of looking at Nature advocated by Grove and Joule was incorporated into the scientific tradition. This was not achieved before the middle of the century. There is a lapse of about eight years between the time when Joule formulated his main conclusion regarding the conservation of energy and his election in 1850 as a Fellow of the Royal Society, which indicates the complete change of outlook of the leading scientific circles.

This, then, is the first lesson that we may derive from an effort to understand Joule's work and personality. Let us now turn to a more technical analysis of this work and consider the methods which he used with such mastery in his investigations. It is often said—and unfortunately Osborne Reynolds' biography of Joule, so excellent in other respects, has added authority to that view—that Joule is typical of the experimental physicist who bases his conclusions only on the results of experiments without allowing himself to be biased by preconceived theoretical views. He is further contrasted with Robert Mayer, the German physicist who at the same time as Joule—and, of course, unknown to him—arrived at exactly the same discovery of the general law of the conservation of energy, without making a single experiment but, it is stated, by "pure" reasoning, or even by "metaphysical" reasoning, which is, I imagine, the worst that can be said of anybody!

Now, I should like to try and convince you that this view is quite superficial and mistaken. If we look more closely at the actual arguments developed by both Joule and Mayer, we find that these two lines of argument are essentially identical. As a theoretical physicist, it gives me pleasure to say that the argument in both cases is mainly theoretical, with, of course, as in all good theory, some reference to experiment.

Let us first examine Mayer's work. It is true that Mayer did not make any experiment himself, but he was quite clear from the start that the decisive conclusion must be based on facts that could only be learned from experiment. His brilliant achievement was just to make an extremely ingenious use of experiments carried out by others, in order to extract from them the data which he needed. In 1806 Gay-Lussac, the great French chemist, had investi-

gated the heating effects, that occur when air compressed in a container is allowed to escape into another container which had been evacuated. He was trying to find out whether the specific heat of a gas depends on its density. This was a question that had arisen in connection with the discussions about the nature of heat. He found that no heat is evolved in the phenomenon and left it at that. Gay-Lussac's paper appeared in a very out-of-the-way publication, the "Journal de la Société d'Arcueil", in which were published the proceedings of informal meetings of a little group of French physicists at the summer house of one of them at Arcueil. It therefore remained generally unknown. Joule, for instance, made exactly the same experiment in 1845 without ever having heard of Gay-Lussac's previous attempt. Mayer, a young physician, was led to reflect on the connection between heat and mechanical work by his interest in Lavoisier's theory of respiration. It was in the course of this study that he had a first intuition of a general relation of equivalence between heat and mechanical power. But he saw (and here his genius appears) that he could only arrive at a clear understanding of such a relation by tracing it to phenomena much simpler than the physiological processes which he was studying; he must go down to a quantitative analysis of elementary physical phenomena. When he accordingly started to learn physics with this aim in view, he came across a description of Gay-Lussac's experiment, probably in a French text book which he was using extensively at that time.

Now, the remarkable event occurred that Mayer, because his mind was oriented in the right direction, was able to read the hidden message contained in that experiment—a message which Gay-Lussac himself failed to understand. He knew, of course, that a gas takes up more heat when it is allowed to expand against a fixed pressure than when it is kept in the same volume. He suspected that the additional heat in the first case was related to the mechanical work accompanying the expansion; but in order to be sure that the relation between heat and work in this case was one of equivalence, one had to enquire whether the expansion by itself, when it was not accompanied by any mechanical work, did not involve any exchange of heat. The answer to this question, as Mayer instantly recognized, was given by Gay-Lussac's experiment. In its cogency and simplicity, Mayer's reasoning is one of the most beautiful examples of the kind of argument which leads to great discoveries in physics. As you see, there is no metaphysics about it! Mayer picked out the significant facts revealed by the experiment, put them together, and the conclusion fell, like ripe fruit, into his hands. Just to collect the right facts and to squeeze the conclusion out of them required, however, not only the keenest insight, but above all the focusing of the attention in the right direction. All the facts had been in the possession of Gay-Lussac, but he failed to draw from them the momentous conclusion that they implied—not because his insight was less penetrating than Mayer's, but because his interest was not fixed on the decisive aspect of the phenomenon.

Now, what about Joule? Joule's train of thought is much more intricate than Mayer's, and looking at it superficially it seems that the way by which he came to his discovery was extremely devious—that he, so to speak, stumbled on it; but a more careful study leads to a very different conclusion. At the very beginning of the paper in which he describes his first experiments about the heat produced by the passage of current through a conductor, he shows that he is already concerned with the idea of equivalence. He explains that in checking by new measurements the generally accepted assertion that the heat produced by the passage of the current is exactly proportional to the resistance, his concern is to ascertain "whether resistance to conduction is the

sole cause of the heating effects". The only possible meaning of that sentence, with the word "sole" underlined, is to indicate that the idea at the back of his mind was that of equivalence between the phenomena of heat production and of the passage of the current through the resistance. If he were not looking for an exact balance, there would have been no point in asking whether there were one or more causes of heat production. Here the "book-keeping" mentality of Joule's environment is conspicuous, albeit hardly conscious to Joule himself.

The next step was to compare the heat produced by the passage of the current with the chemical changes occurring in the battery which produced this current. Again, it is clear that Joule could only have been led to such a comparison by some suspicion of a possible equivalence between the chemical changes on the one hand and the heat produced on the other. However, he is admirably cautious in the analysis of his results. He does not rashly jump to the conclusion that the chemical change in the battery is the cause of the heat produced. He keeps in mind the two possible theories about the nature of heat: the theory that heat is a substance which would simply be transported by the current but not created, and the rival theory according to which heat is a form of motion, in which case it could be generated by suitable agencies. At this stage he appears to be non-committal about the theory of heat, but in a later paper he declares that he was actually convinced that the dynamical theory was the true one. Here, then, we have his own word for stating that he was guided from the start, in the orientation which he gave to his researches, by a definite theoretical view about the nature of heat. But he rightly kept his theoretical convictions in reserve and did not venture to express them before he had collected empirical evidence which he judged sufficiently convincing.

From such clear and unambiguous hints we are, therefore, justified in concluding that Joule was inspired from the very beginning by theoretical ideas: the idea of the dynamical nature of heat and a perhaps not quite articulate idea that there might be an equivalence between heat and other physical agencies. The experiments that he undertook on the connection between heat and electricity were made with a view to testing those ideas.

A striking confirmation of this inference is offered by Joule's next experiment, which is probably the most extraordinary in the whole history of physics. He encloses the revolving armature of an electromagnetic engine in a cylindrical container, carefully protected against heat losses, and filled with a known amount of water. After ascertaining the temperature of the water, he rotates the coil and water-filled container during a certain time between the poles of the fixed electromagnet, and measures the current induced. He then again takes the temperature of the water, and derives the quantity of heat evolved in the process. The qualitative fact that heat appears in this case in the revolving coil was the decisive proof he was looking for that heat is not simply transferred by the current from one place on the circuit to the other, but is actually generated. This was therefore a crucial experiment which disposed of the view of the caloric theory of heat and definitely confirmed the dynamical theory.

The quantitative implications of the experiment, however, were of still more decisive importance in guiding Joule to the recognition of the universal law of equivalence between various physical agencies. The way in which he proceeded further with the analysis of electromagnetic heat production bears testimony, not only to Joule's ingenuity as a natural philosopher, but also to his intellectual integrity, all the more remarkable when one sees him as a self-taught youth grappling single-handed with one of the most funda-

mental laws of Nature. He did not—as one might, after the event, imagine he could have done—immediately link the heat produced by the rotation of the coil with the mechanical work spent in this operation. He first proved by a striking argument that the heat production must be quantitatively related to the work done in rotating the coil. For this purpose he superposed upon the induced current an additive or subtractive current supplied by a battery, and he showed that in every case the heat produced was proportional to the square of the total current. Now, he argued, the chemical changes in the battery are proportional to the current intensity itself; therefore they cannot account for all the heat produced, and the balance can only be related to the mechanical work spent in producing the induced current. This piece of reasoning is assuredly one of the most beautiful examples of a penetrating and rigorous analysis of a physical phenomenon. Only after he had thus established the necessity of assuming a relation between heat production and mechanical work did he take the last and comparatively easy step in the investigation, viz. to measure the amount of work needed to produce in the apparatus a known quantity of heat.

It was necessary to remind you of the details of this beautiful work of Joule in order to make clear how the view that Joule was “a pure experimenter” without any “theoretical bias” could gain ground. Certainly, the way in which he carried out the series of experiments which I have just tried to sketch very briefly and incompletely is, as I have emphasized, a model of rigorous experimental induction; but it would be quite wrong, and lead us to a quite distorted picture of Joule’s achievement, to ignore the fact that the conception and planning of these experiments was essentially directed by the wish to test definite theoretical speculations. If we disregard this fact, the complication and strangeness of Joule’s experimental arrangements become utterly incomprehensible. If, on the contrary, simply listening to what Joule himself tells us,* we judge the adequacy of his apparatus for the purpose of putting to test the dynamical theory of heat and the general idea of equivalence between the physical agencies, everything becomes clear and the admirable cogency of Joule’s procedure comes into full light. If you ask me how it was ever possible to overlook Joule’s own unambiguous statements about his being guided by theoretical ideas and create the impossible legend of his merely experimenting, I can only remark that the history of science seems to be the last refuge of some people of otherwise sober judgment for satisfying a primeval urge for mythical creation.

When Joule had thus established on a firm empirical foundation his conceptions of the nature of heat and the conservation of energy, he set up a new series of painstaking experiments with a view to a more direct, and accordingly more accurate, determination of the mechanical equivalent of heat. From the point of view of experimental technique these famous experiments are again masterpieces which set us wondering about his independence of judgment and self-confidence. Even now it is hard to imagine how he could have the boldness to devise apparatus for such delicate measurements and trusted their reliability as he did. This, however, is another fascinating aspect of Joule’s work upon which I have no time to enter. The study of Joule as an experimenter is actually still to be made. Perhaps the facilities

* The especially significant passages in Joule’s papers revealing his theoretical preoccupations are the introduction to the fundamental paper analysed above (*Joule’s Scientific Papers*, Vol. 1, p. 123) and also the fifth observation at the end of the preceding paper (*ibid.* p. 120) where he boldly predicts the quantitative relation between heat and work in the electromagnetic machine and announces in a footnote that he is “preparing for experiments to test the accuracy of this proposition”.

for such a study provided by the Joule Museum may incite somebody to tackle it. I will just mention a small but characteristic detail. Several pages of Joule's laboratory books are filled with notes on the painstaking comparison of the readings of his different thermometers. It is really touching to see that he gave those thermometers individual names—the "Old Standard", the "Old Sensible", and "Fastré Sensible". The thermometers lived for him: they were, so to speak, his faithful helpers.

There is now a last remark about Joule's achievement which I should like to make very briefly. It concerns Joule's position with respect to the scientific philosophy of his time. Joule, as we have seen, pursued concurrently two great ideas: the dynamical theory of heat and the equivalence between heat and mechanical work. In his mind, these two ideas were united by a quite essential connection: the picture of heat as a mode of molecular motion was the justification of the law of equivalence, the atomic and mechanistic model of the universe was the *raison d'être* of the conservation of energy. This conviction is strongly expressed in the remarkable lecture which Joule delivered in Manchester in 1847. It was shared by all the leading physicists—Thomson, Clausius, Maxwell, Boltzmann—who brought to completion the new science significantly called "thermodynamics" or "mechanical theory of heat". Strictly speaking, however, the principle of conservation of energy is not logically tied to any particular view on the nature of heat. This was always emphasized by Robert Mayer, who insisted on the fact that the establishment of a quantitative equivalence between different classes of phenomena does not at all mean that those phenomena are in any sense identical. Mayer stressed the qualitative differences between various phenomena and their continual transformation into each other.

In this attitude of Mayer we recognize, of course, the influence of the German Romantic philosophy which, in spite of all its shortcomings, had at any rate the merit of reminding physicists of the importance of qualitative changes, which they are sometimes prone to forget. It is quite typical of the "book-keeping" mentality of Joule and the British physicists that they never understood Mayer's point of view. In one of Joule's notebooks there is a translation of Mayer's first paper which is very revealing. It is, I must say, a rather bad translation; the more abstruse sentences especially have proved to be quite unintelligible to Joule. And he was obviously so impatient of Mayer's views that he could not help inserting in the course of his translation various disparaging remarks. This curious irritation shows that Joule's own mechanistic philosophy was not the result of cool, logical thinking but was rooted in deeper emotions.

I think that the reason why Joule and his followers believed so strongly in a completely mechanical picture of the material world must again be sought in their social environment. The second half of the 19th century witnessed a rapid growth and concentration of the production process, calling for an enormous increase in the development of the sources of motive power, and for a bolder use of materials in building lofty bridges and powerful machinery. Scientists interested in such developments were influenced by their intimate contact with matter in its more solid and bulky aspects. This unwittingly reflected itself, so to speak, in their philosophical views. An extreme example of this kind of mechanical philosophy is afforded by Joule's friend, William Thomson, later Lord Kelvin, who did not shrink from imagining intricate models of the ether involving gyroscopes and strings, and who even identified the understanding of a phenomenon with the construction of a mechanical model for it.

Yet those people would have been grievously shocked if anybody had described them as "materialists". Joule himself was deeply religious. We have, for instance, the draft of his presidential address to the British Association in 1873, which he was prevented by illness from delivering, in which he makes a case in favour of scientific education. The chief purpose and justification of the study of science, according to him, is that it is a way of adoring God by the contemplation of His works. Of course, he also deals at great length with the practical uses of science; but he presents them as secondary advantages. Nevertheless, the essential component of his scientific attitude must not be sought in such utterances—too obviously conditioned by uncritical acceptance of social conventions—but in his spontaneous scientific thinking. There he behaved in exactly the same way as the free-thinker Tyndall, that outspoken exponent of "scientific materialism".

Towards the end of the century, there occurred a curious reaction. Ostwald took up Mayer's views again, or rather misrepresented them, in the form of what he called "Energetik"; he endeavoured to do away with atoms and all materialistic interpretations of Nature and to reduce the interpretation of all physical phenomena to the two fundamental laws of thermodynamics. This attempt, which was indeed quite unsound, failed completely. It must be said, incidentally, that as soon as Ostwald saw that he was on the wrong track he honestly acknowledged it and proclaimed his allegiance to the atomic view of Nature. This turn in the development of natural philosophy at the end of the 19th century is not an accident originating in Ostwald's personality: it is again, a sort of echo in the realm of physics of the turmoil that was going on in the social sphere at that time and which gave rise to a general reaction against the so-called materialistic philosophy. It is a very significant fact that apart from the short-lived and rather trivial episode of the Energetik, social reaction could never get any hold on scientific thought. The tremendous advances of science in our time have been achieved in the same spirit as Joule's pioneering work.

Richard Bradley, an Early Eighteenth Century Biologist

BY DR. H. HAMSHAW THOMAS, M.B.E., F.R.S.

ABSTRACT of Paper read on 28th May, 1951

Historians of Science have paid little attention to the development of plant biology at the end of the 17th and in the first third of the 18th century. At this time horticulture was becoming very popular, and many were interested in experiments on plants and in the attempts to discover the principles of their physiology and reproduction, but apart from Stephen Hales few of them have been remembered. During the following hundred years the powerful influence of Linneus, and the arrival in Europe from all parts of the world of new types of plants, occupied men's minds so fully that problems of plant life were sadly neglected.

Richard Bradley, the first titular Professor of Botany in the University of Cambridge, was one of those particularly interested in the study of how plants grow and reproduce themselves. His early works reflect the spirit of experimental enquiry characteristic of his times, and this theme constantly recurs in his later books. He was a most prolific writer, responsible for not less than twenty-four books or pamphlets published between 1714 and 1730, some of them running through several editions. Most of them dealt with horticulture or agriculture; they must have had much influence in spreading knowledge about some of the main facts of plant biology. For a long time