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THE COPLEY MEDALIST OF 1871

DR. JULIUS ROBERT MAYER was educated for the medical profession. In the summer of 1840, as he himself informs us, he was at Java, and there observed that the venous blood of some of his patients had a singularly bright red colour. The observation riveted his attention; he reasoned upon it, and came to the conclusion that the brightness of the colour was due to the fact that a less amount of oxidation sufficed to keep up the temperature of the body in a hot climate than in a cold one. The darkness of the venous blood he regarded as the visible sign of the energy of the oxidation.

It would be trivial to remark that accidents such as this, appealing to minds prepared for them, have often led to great discoveries. Mayer's attention was thereby drawn to the whole question of animal heat. Lavoisier had ascribed this heat to the oxidation of the food. One great principle, says Mayer, of the physiological theory of combustion, is that under all circumstances the same amount of fuel yields by its perfect combustion the same amount of heat; that this law holds good for vital processes; and that hence the living body, notwithstanding all its enigmas and wonders, is incompetent to generate heat out of nothing.

But beyond the power of generating internal heat, the animal organism can also generate heat outside of itself. A blacksmith, for example, by hammering can heat a nail, and a savage by friction can warm wood to its point of ignition. Now unless we give up the physiological axiom that the living body cannot create heat out of nothing, "we are driven," says Mayer, "to the conclusion that it is the *total* heat generated within and *without* that is to be regarded as the true calorific effect of the matter oxidised in the body."

From this again he inferred that the heat generated externally must stand in a fixed relation to the work expended in its production. For, supposing the organic processes to remain the same; if it were possible, by the mere alteration of the apparatus, to generate different amounts of heat by the same amount of work, it would follow that the oxidation of the same amount of material would sometimes yield a less, sometimes a greater, quantity of heat. "Hence," says Mayer, "that a fixed relation subsists between heat and work, is a postulate of the physiological theory of combustion."

This is the simple and natural account given subsequently by Mayer himself of the course of thought started by his observation in Java. But the conviction once formed that an unalterable relation subsists between work and heat, it was inevitable that Mayer should seek to express it numerically. It was also inevitable that a mind like his, having raised itself to clearness on this important point, should push forward to consider the relationship of natural forces generally. At the beginning of 1842 his work had made considerable progress; but he had become physician to the town of Heilbronn, and the duties of his profession limited the time which he could devote to purely scientific inquiry. He thought it wise, therefore, to secure himself against accident, and in the spring of 1842 wrote to Liebig, asking him to publish in his "Annalen" a brief preliminary notice of the work then accomplished. Liebig did so, and Dr. Mayer's first paper is contained in the May number of the "Annalen" for 1842.

Mayer had reached his conclusions by reflecting on the complex processes of the living body; but his first step in public was to state definitely the physical principles on which his physiological deductions were to rest. He begins, therefore, with the forces of inorganic nature. He finds in the universe two systems of causes which are not mutually convertible;—the different kinds of matter, and the different forms of force. The first quality of both he affirms to be *indestructibility.* A force cannot become nothing, nor can it arise from nothing. Forces are convertible, but not destructible. In the terminology of his time, he then gives clear expression to the ideas of potential and dynamic energy, illustrating his point by a weight resting upon the earth, suspended at a height above the earth, and actually falling to the earth. He next fixes his attention on cases where motion is apparently destroyed without producing other motion; on the shock of inelastic bodies, for example. Under what form does the vanished motion maintain itself? Experiment alone, says Mayer, can help us here. He warms water by stirring it; he refers to the force expended in overcoming friction. Motion in both cases disappears, but heat is generated, and the quantity generated is the equivalent of the motion destroyed. Our locomotives, he observes with extraordinary sagacity, may be compared to distilling apparatus. The heat beneath the boiler passes into the motion of the train, and it is again deposited as heat in the axles and wheels.

A numerical solution of the relation between heat and work was what Mayer aimed at, and towards the end of his first paper he makes the attempt. It was known that a definite amount of air, in rising one degree in temperature, can take up two different amounts of heat. If its volume be kept constant, it takes up one amount; if its pressure be kept constant, it takes up a different amount. These two amounts are called the specific heat under constant volume and under constant pressure. The ratio of the first to the second is as 1 : 1·421. No man, to my knowledge, prior to Dr. Mayer, penetrated the significance of these two numbers. He first saw that the excess 0·421 was not, as then universally supposed, heat actually lodged in the gas, but heat which had been actually consumed by the gas in expanding against pressure. The amount of work here performed was accurately known, the amount of heat consumed was also accurately known, and from these data Mayer determined the mechanical equivalent of heat. Even in this first paper he is able to direct attention to the enormous discrepancy between the theoretic power of the fuel consumed in steam-engines and their useful effect.

Though this first paper contains but the germ of his further labours, I think it may be safely assumed that, as regards the mechanical theory of heat, this obscure Heilbronn physician in the year 1842 was in advance of all the scientific men of the time.

Having, by the publication of this paper, secured him-

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self against what he calls "Eventualitäten," he devoted every hour of his spare time to his studies, and in 1845 published a memoir which far transcends his first one in weight and fulness, and, indeed, marks an epoch in the history of science. The title of Mayer's first paper was, "Remarks on the Forces of Inorganic Nature." The title of his second great essay was, "Organic Motion in its Connection with Nutrition." In it he expands and illustrates the physical principles laid down in his first brief paper. He goes fully through the calculation of the mechanical equivalent of heat. He calculates the performances of steam-engines, and finds that 100 lbs. of coal in a good working engine produce only the same amount of heat as 95 lbs. in an unworking one; the 5 lbs. disappearing having been converted into work. He determines the useful effect of gunpowder, and finds 9 per cent. of the force of the consumed charcoal invested on the moving ball. He records observations on the heat generated in water when agitated by a pulping engine of a paper manufactory, and calculates the equivalent of that heat in horse-power. He compares chemical combination with mechanical combination—the union of atoms with the union of falling bodies with the earth. He calculates the velocity with which a body starting at an infinite distance would strike the earth's surface, and finds that the heat generated by its collision would raise an equal weight of water 17,356° C. in temperature. He then determines the thermal effect which would be produced by the earth itself falling into the sun. So that here, in 1845, we have the germ of that meteoric theory of the sun's heat which Mayer developed with such extraordinary ability three years afterwards. He also points to the almost exclusive efficacy of the sun's heat in producing mechanical motions upon the earth, winding up with the profound remark, that the heat developed by friction on the wheels of our wind and water-mills comes from the sun in the form of vibratory motion; while the heat produced by mills driven by tidal action is generated at the expense of the earth's axial rotation.

Having thus with firm step passed through the powers of inorganic nature, his next object is to bring his principles to bear upon the phenomena of vegetable and animal life. Wood and coal can burn; whence come their heat, and the work producible by that heat? From the immeasurable reservoir of the sun. Nature has proposed to herself the task of storing up the light which streams earthward from the sun, and of casting into a permanent form the most fugitive of all powers. To this end she has overspread the earth with organisms which, while living, take in the solar light, and by its consumption generate forces of another kind. These organisms are plants. The vegetable world indeed constitutes the instrument whereby the wave-motion of the sun is changed into the rigid form of chemical tension, and thus prepared for future use. With this prevision, as shall subsequently be shown, the existence of the human race itself is inseparably connected. It is to be observed that Mayer's utterances are far from being anticipated by vague statements regarding the "stimulus" of light, or regarding coal as "bottled sunlight." He first saw the full meaning of De Saussure's observation of the reducing power of the solar rays, and gave that observation its proper place in the doctrine of conservation. In the leaves of a tree, the carbon and oxygen of carbonic acid, and the hydrogen and oxygen of water, are forced asunder at the expense of the sun, and the amount of power thus sacrificed is accurately restored by the combustion of the tree. The heat and work potential in our coal strata are so much strength withdrawn from the sun of former ages. Mayer lays the axe to the root of many notions regarding the vital force which were prevalent when he wrote. With the plain fact before us that plants cannot perform the work of reduction, or generate chemical tensions, in the absence of the solar rays, it is, he contends, incredible that these tensions should be caused by the mystic play of the vital force. Such an hypothesis would cut off all investigation; it would land us in a chaos of unbridled phantasy. "I count," he says, "therefore, upon assent when I state as an axiomatic truth that during vital processes the *conversion* only and never the *creation* of matter or force occurs."

Having cleared his way through the vegetable world, as he had previously done through inorganic nature, Mayer passes on to the other organic kingdom. The physical forces collected by plants become the property of animals. Animals consume vegetables, and cause them to reunite with the atmospheric oxygen. Animal heat is thus produced, and not only animal heat but animal motion. There is no indistinctness about Mayer here; he grasps his subject in all its details, and reduces to figures the concomitants of muscular action. A bowler who imparts to an 8-lb. ball a velocity of 30 feet consumes in the act $\frac{1}{10}$ of a grain of carbon. A man weighing 150 lbs., who lifts his own body to a height of 8 feet, consumes in the act 1 grain of carbon. In climbing a mountain 10,000 feet high, the consumption of the same man would be 2 oz. 4 drs. 50 grs. of carbon. Boussingault had determined experimentally the addition to be made to the food of horses when actively working, and Liebig had determined the addition to be made in the case of men. Employing the mechanical equivalent of heat, which he had previously calculated, Mayer proves the additional food to be amply sufficient to cover the increased oxidation.

But he does not content himself with showing in a general way that the human body burns according to definite laws, when it peforms mechanical work. He seeks to determine the particular portion of the body consumed, and in doing so executes some noteworthy calculations. The muscles of a labourer 150 lbs. in weight, weigh 64 lbs.; when perfectly desiccated they fall to 15 lbs. Were the oxidation corresponding to that labourer's work. exerted on the muscles alone, they would be utterly consumed in 80 days. The heart furnishes a still more striking example. Were the oxidation necessary to sustain the heart's action exerted upon its own tissue, it would be utterly consumed in 8 days. And if we confine our attention to the two ventricles, their action would be sufficient to consume the associated muscular tissue in $3\frac{1}{2}$ days. Here, in his own words, emphasised in his own way, is Mayer's pregnant conclusion from these calculations:—"The muscle is only the apparatus by means of which the conversion of the force is effected; *but it is not the substance consumed in the production of the mechanical effect.*" He calls the blood "the oil of the lamp of life;" it is the slow-burning fluid whose chemical force in the furnace of the capillaries is sacrificed to produce animal motion. This was Mayer's conclusion twenty-six years ago. It was in complete opposition to the scientific conclusions of his time; but eminent investigators have since amply verified it.

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Thus, in baldest outline, I have sought to give some notion of the first half of this marvellous essay. The second half is so exclusively physiological that I do not wish to meddle with it. I will only add the illustration employed by Mayer to explain the action of the nerves upon the muscles. As an engineer, by the motion of his finger in opening a valve or loosing a detent, can liberate an amount of mechanical motion almost infinite compared with its exciting cause, so the nerves, acting upon the muscles, can unlock an amount of activity wholly out of proportion to the work done by the nerves themselves.

As regards these questions of weightiest import to the science of physiology, Dr. Mayer in 1845 was assuredly far in advance of all living men.

Mayer grasped the mechanical theory of heat with commanding power, illustrating it and applying it in the most diverse domains. He began, as we have seen, with physical principles; he determined the numerical relation between heat and work; he revealed the source of the energies of the vegetable world, and showed the relationship of the heat of our fires to solar heat. He followed the energies which were potential in the vegetable up to their local exhaustion in the animal. But in 1845 a new thought was forced upon him by his calculations. He then for the first time drew attention to the astounding amount of heat generated by gravity where the force has sufficient distance to act through. He proved, as I have before stated, the heat of collision of a body falling from an infinite distance to the earth, to be sufficient to raise the temperature of a quantity of water equal to the falling body in weight 17,356°C. He also found in 1845 that the gravitating force between the earth and sun was competent to generate an amount of heat equal to that obtainable from the combustion of 6,000 times the weight of the earth of solid coal. With the quickness of genius he saw that we had here a power sufficient to produce the enormous temperature of the sun, and also to account for the primal molten condition of our own planet. Mayer shows the utter inadequacy of chemical forces, as we know them, to produce or maintain the solar temperature. He shows that were the sun a lump of coal, it would be utterly consumed in 5,000 years. He shows the difficulties attending the assumption that the sun is a cooling body; for supposing it to possess the high specific heat of water, its temperature would fall 15,000° in 5,000 years. He finally concludes that the light and heat of the sun are maintained by the constant impact of meteoric matter. I never ventured an opinion as to the accuracy of this theory; that is a question which may still have to be fought out. But I refer to it as an illustration of the force of genius with which Mayer followed the mechanical theory of heat through all its applications. Whether the meteoric theory be a matter of fact or not, with him abides the honour of proving to demonstration that the light and heat of suns and stars *may* be originated and maintained by the collisions of cold planetary matter.

It is the man who from the scantiest data could accomplish all this in six short years, and in the hours snatched from the duties of an arduous profession, that the Royal Society has this year crowned with its highest honour. Dr. Mayer had never previously received any mark of recognition from the society.

It was not in my power to be present at our late president's last address; but Sir Edward Sabine has done me the honour of sending me a printed copy of it. It contains the reasons assigned by him for the award of the Copley medal. Briefly, but appreciatingly, he expresses his opinion of the merits of Dr. Mayer, committing to Prof. Stokes the task of drawing up a fuller statement of the case. This statement is marked by an evident desire to act fairly towards Mayer, and at the same time to qualify the award so that no erroneous inferences may be drawn from it. It will be observed that Prof. Stokes confines himself to Mayer's first paper, the real value of which, however, is best appreciated in connection with Mayer's subsequent work, as the soundness of the root is best demonstrated by the vigour of the tree. Prof. Stokes writes thus:—

"In a paper published in 1842, Mayer showed that he clearly conceived the convertibility of falling force, or of the *vis viva*, which is its equivalent or representative in visible motion, into heat, which again can disappear as heat by reconversion into work or *vis viva*, as the case may be. He pointed out the mechanical equivalent of heat as a fundamental datum, like the space through which a body falls in one second, to be obtained from experiment. He went further. When air is condensed by the application of pressure, heat, as is well known, is produced. Taking the heat so produced as the equivalent of the work done in compressing the air, Mayer obtained a numerical value of the mechanical equivalent of heat, which, when corrected by employing a more precise value of the specific heat of air than that accessible to Mayer, does not much differ from Joule's result. This was undoubtedly a bold idea, and the numerical value obtained by Mayer's method is, as we now know, very nearly correct." Prof. Stokes then qualifies the award in these words:—"Nevertheless it must be observed that an essential condition in a trustworthy determination is wanting in Mayer's method; *the portion of matter operated on does not go through a cycle of changes.* Mayer reasons as if the production of heat were the sole effect of the work done in compressing air. But the volume of the air is changed at the same time, and it is quite impossible to say *a priori* whether this change may not involve what is analogous to the statical compression of a spring, in which a portion or even a large portion of the work done in compression may have been expended. In that case the numerical result given by Mayer's method would have been erroneous, and *might* have been even widely erroneous. Hence the practical correctness of the equivalent obtained by Mayer's method must not lead us to shut our eyes to the merit of our own countryman Joule, in being the first to determine the mechanical equivalent of heat by methods which are unexceptionable, as fulfilling the essential condition that no ultimate change of state is produced in the matter operated upon."

The judgment of Prof. Stokes, regarding the possible error of Mayer's determination of the mechanical equivalent of heat, gives me occasion to cite another proof of the insight of this extraordinary man. His paper of 1845 contains the details of his calculation, which were omitted from his first brief paper. Mayer prefaces the calculation with these memorable words:—

"To prove this important proposition, we must fix our attention on the deportment of elastic fluids towards heat and mechanical effect.

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"Gay Lussac has proved by experiment that when an elastic fluid streams from one receiver into a second exhausted one of equal size, the first vessel is cooled, and the second one heated, by exactly the same number of degrees. This experiment, which is distinguished for its simplicity, and which, to other observers, has always yielded the same result, shows that a given weight and volume of an elastic fluid may expand to double, quadruple, in short, to several times its volume without experiencing, on the whole, any change of temperature; or, in other words, that for the expansion of the gas of itself (*an und für sich*), no expenditure of heat is necessary. But it is equally proved that a gas which expands *under pressure* suffers a *diminution* of temperature.

"Let a cubic inch of air at 1°, and under the pressure of 30 inches of mercury, be warmed by the quantity of heat x to 274° C., its volume being kept constant; this air, on being permitted to stream into a second exhausted vessel of the same size, will retain the temperature of 274°, and a medium surrounding the vessel will suffer no change of temperature. In another experiment, let our cubic inch of air be kept, not at constant *volume*, but under the constant *pressure* of the 30-inch mercurial column, and heated to 274°. In this case a greater quantity of heat is required; let it be $x + y$.

"In comparing these two processes, we see that in both of them the air is heated from 0° to 274°, and at the same time permitted to expand from one volume to two volumes. In the first case the quantity of heat necessary was $= x$, in the second case $= x + y$. In the first case the mechanical effect was $= 0$, in the second case it was equal to 15 lbs. raised one inch in height."

He then proceeds with his calculation.

Here it will be seen that Mayer was quite awake to the importance of the considerations dwelt upon by Prof. Stokes—that he knowingly chose for his determination a substance which, *an und für sich*, in expanding, consumes no heat. Hence, when by its expansion *against pressure* heat is consumed, no part of that heat is lost in producing "a change of state in the matter operated upon." The heat consumed is, therefore, the pure equivalent of the work done.

With regard to Dr. Joule, I have, to my regret, vainly endeavoured to find a mislaid document written a year ago, in which I ventured to describe his labours,[*] and to express the esteem I entertain for them. Supposing him to have derived his inspiration from Mayer's papers, that they had even caused him to prosecute his experiments on the mechanical equivalent of heat, he would still have rendered immortal service to science, and more than merited the honours bestowed upon him last year. For, wanting his work, the mechanical theory, however strong the presumptions, and however concurrent the evidence in its favour, could not be regarded as completely demonstrated. But Joule was not stimulated by Mayer. His work is his own, being practically contemporaneous with that of Mayer. He not only demonstrated experimentally the mechanical theory of heat, but in its completer form he was an independent creator of that theory. And so impressed was the Council of the Royal Society last year with the magnitude of his merits, that they actually added to the Rumford Medal already bestowed upon him, the final distinction of the Copley Medal. If England rated him as highly as I do, his reward would not be confined to mere scientific recognition.

As regards the latter, however, I do not think that the possibility suggested by Prof. Stokes represents any real danger. I do not imagine that the eyes of Science are in the least degree likely to be "shut to the merits of our own countryman." And I believe that the Royal Society, by stamping in two consecutive years these two men with the highest mark of its approval, will have strengthened that confidence in its impartiality which, throughout the whole scientific world, it has so long and so justly enjoyed.

JOHN TYNDALL

AIRY ON MAGNETISM

A Treatise on Magnetism. By G. B. Airy, Astronomer Royal. (Macmillan and Co.)

THIS is a book written upon the true scientific principle expressed by Newton when he said "Hypotheses non fingo." The elementary laws of magnetism are deduced by rigorous induction from particular cases and are then applied to explain phenomena. The book contains the substance of a series of lectures delivered by the Astronomer Royal at the University of Cambridge. One great element of excellence in the book is that the mathematics employed throughout are of a simple character, so that the first principles of magnetism are thus thrown open to one who has gone no great way in mathematical reading.

Formulæ having been obtained in the early sections for the action of one magnet on another, and the law of the inverse square having been established by a comparison of calculation with experiment, the great bulk of the volume is occupied in investigations which bear more directly on terrestrial magnetism and the magnetism of iron ships. The methods of determining the values of the magnetic elements at any place are carefully explained and illustrated, and the necessary formulæ deduced from the theory established in the preceding sections. We would especially recommend to the reader's attention the articles on the theory of the dipping needle. One chapter of extreme interest is devoted to "Theories of Terrestrial Magnetism," and the beautiful theory of Gauss is sketched out. We sincerely hope that that theory which was carried by Gauss to the fourth order of approximation will be before long carried to a higher order. Data now exist for this advance, as it requires accurate determinations of only eleven more elements.

The subject of the deviation of the compass in iron ships is one upon which the Astronomer Royal is peculiarly justified in speaking or writing. All the sections relating to the disturbance of compass needles are full of most important and suggestive matter. One section is devoted to the continuous registration of small changes in terrestrial magnetism, and the concluding section just touches on the subject of the relation between galvanic currents and magnetic forces, without entering into any calculations.

The book supplies a distinct want which has hitherto existed in the list of our mathematical text-books, and is a most valuable contribution to the diffusion of physico-mathematical science.

JAMES STUART

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[*] Thanks to the friendly efforts of Dr. Sharpey, this document reached my hands just as the proof of this paper was being returned for press. With the permission of the Editor of NATURE I will publish the document, with some additional matter, next week. J. T.

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