Croonian lectures on matter and force: given at the Royal College of Physicians in 1868 / by Henry Bence Jones.

CROONIAN LECTURES ON MATTER AND FORCE.

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CROONIAN LECTURES

ON

MATTER AND FORCE.

GIVEN AT THE ROYAL COLLEGE OF PHYSICIANS
IN 1868.

BY

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PREFACE.

THESE Lectures, since they were published in the *British Medical Journal,* have been corrected, and three Appendices have been added to show how the progress of Physiology depends now upon Physics and Chemistry.

The object of this republication will be obtained, if the idea of the inseparability of matter and force is made clearer to any one; or even if the confusion that exists in the use of the word force, or the indefiniteness in the use of the word matter, is lessened.

The confusion regarding the word force partly comes from its use in not less than three different senses: firstly, as the cause of motion;

secondly, as possible or actual motion; and thirdly, even as the result of motion. Thus the attractions that start the cannon-ball, the motion of the ball, and the blow it gives, have each been designated by the word force.

The indefiniteness in the use of the word matter has partly originated in the assumption of some stuff as the essence, or vehicle for light, heat, electricity, magnetism, the interstellar æther, and for life ; and these imaginary liquids, or gases, have been realized as if they had been proved to exist by analysis.

As these and similar confusions pass away, more and more clearness will be obtained regarding the inseparable union of matter and force.

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CROONIAN LECTURES

ON

MATTER AND FORCE.

LECTURE I.

ON THE THREE STAGES OF OUR IDEAS REGARDING THE UNION OF PONDERABLE MATTER AND FORCE IN THE ABIOLOGICAL SCIENCES.

> "But though the natural works of God can never by any possibility come in contradiction with the higher things which belong to our future existence, and must with everything concerning him ever glorify him, still I do not think it at all necessary to tie the study of the natural sciences and religion together."—FARADAY, MS.

MR. PRESIDENT,—

I must begin this course of lectures by assuring you that I am fully sensible of my rashness in venturing to bring before you a subject which may be thought to be more metaphysical than physical, and to be too difficult to be of any practical utility; but I venture to take the great chance of failure, because I hold that the clearness and breadth or dimness and narrowness of our ideas regarding matter and force must constitute a good or a bad foundation of all the knowledge we possess, not only in medicine, but in every other science.

In the history of the development of scientific knowledge, whether in mankind, nations, or individuals, ideas of matter and force are found to become gradually more clear and more broad; and, as soon as clearer and more connected ideas are obtained, they immediately lead to fresh investigations; and on this account, if for no other cause, it is desirable that from time to time we should ask ourselves what is the degree of clearness and breadth to which we have attained in our ideas of matter and force?

What are our present ideas? Whence have they come? Whither are they going?

Ideas that have become too old give no new lines of research, and always cause the grossest misinterpretation of new facts; whilst new ideas, even though far from the perfect representation of the ultimate truth, yet can gradually be made to come nearer and nearer to that perfect knowledge which is based upon reason and demonstration, and not on external or internal authority.

In this lecture, I intend to bring before you the history of our ideas regarding the union of ponderable matter and force in those sciences into which the idea of life does not enter.

Three distinct stages of ideas or epochs of thought may be clearly recognised.

The first may be considered as the authoritative stage, or that of complete separation between the ideas of ponderable matter and force. This stage may be summed up in two words, materialism, and immaterialism or spiritualism. It may be called primitive, because it implies a state of almost complete ignorance of the first principles of natural knowledge.

The second stage is founded partly on authority and partly on natural knowledge. It is marked by the incomplete separation between the ideas of ponderable matter and force. Force is held to be imponderable matter, or to be inseparably united with imponderable matter. This may be called the stage of imponderable materialism, and it may be marked as the transition or Newtonian stage.

The third stage rests solely on the advance of natural knowledge, and is characterised by the complete union or perfect inseparability between the ideas of ponderable matter and force. This may be called materialism, if, in the definition of matter, the definition of force is included; or it may be called spiritualism, if, in the definition of spirit, the definition of matter is contained. This stage, in which our ideas of ponderable matter and force are inseparably united, may be distinguished as the modern stage.

The first or primitive stage is that of complete separation between the ideas of matter and force.

Before bringing to your notice some of the records of this stage which exist, it is desirable that you should for a moment consider what the earliest ideas of matter and force were likely to be when no knowledge of science existed.

The further we go back, the more we must expect ideas of force and matter to be formed on superficial likenesses and distinctions. Scientific separations or relations must not be looked for. The different conditions of the same matter, as solid, liquid, or vapour, will strike the senses far more distinctively than the different elementary substances which are now known. So, also, variations of form and quantity will be far more noticeable than the action of the different forces which we now recognise in matter. Finally, we must expect no clear distinction between the highest and the lowest phenomena shown by animals and vegetables. The immortal soul, the feelings, the reason, the instinct, the life as it exists in animals and in vegetables, are all likely to be utterly confused together; and it will be as vain to look for separate ideas of different kinds of matter and force as to look for separate ideas of the soul, the reason, the animal life, and the vegetable life, to which separation we are so well accustomed.

Probably by far the oldest recorded ideas which exist on the matter and force of inorganic nature are those of the Jews, contained in a few words in the Book of Genesis. It is said, "The earth was created, and it was without form, and void." Afterwards, God said, "Let there be light, and there was light." The firmament, the waters, and the earth, were made as light was made ; that is, in its creation, light or force was regarded as perfectly separable from matter.

In other ancient nations, or in savage tribes, we may see how ideas of matter and force were distinctly separated, by observing how the different phenomena of nature were worshipped as separate personal deities, according to the distinctness of their action on the senses.

Wherever we come to the dawn of ideas, we find man raising the forms or forces of matter which he perceives around him until he makes them into heroes or deities, to which he gives a body and qualities like those he finds in himself. Thus time, or night, or day, the heaven, the earth, the sea, or light, darkness, fire, or life, become incarnate gods, capable of doing good or evil.

By reversing the process, we can descend from the deities to the ideas from which they came ; and everywhere we shall find that, in the earliest ideas, force is regarded as self-existing, and altogether separable from the idea of matter.

The Vedas were perhaps compiled in the

sixteenth or fourteenth century before Christ. The Hindu religion contained among its deities Indra, the god of the sky or light; Vritra, the demon of darkness or night; Agni, the god of fire; Savitri, the god of the sun; Ap, the god of waters; Prithri, the god of earth. Creation, preservation, and destruction were worshipped as Brahma, Vishna, and Seva. Brahma formed fire, water, and earth.*

In the sacred books of the Chinese, the seven gods deemed worthy of peculiar homage are the

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* "Wherever in the world around him the Hindu observed extraordinary manifestations of the brilliant or the beautiful, the barren or the prolific, the sombre or the terrible, wherever the action of the elements was such as to produce extraordinary effects upon himself, his children, or his property, he betrayed the consciousness of his dependence by some special act of homage. He acknowledged in such powers the presence of divinity. He called the influence which affected him or his a deva (*deus*). It was pregnant for a time with a divine or a diabolical efficacy, and therefore it became a fitting object of desire or dread, of adoration or of deprecation, according to the aspects it assumed in reference to the worshipper. Hence also every province of Creation was soon peopled by spiritual energies, all varying in their character with human hopes and fears, with human interests and passions. Nay, so far was the Hindu impelled in this direction, that he deified the sacrifice itself (the soma or milky juice of the moon-plant), from which he hoped to profit. He worshipped his own offering. He worshipped the solemn form of words by which his offering was accompanied."—*Hardwick,* vol. i., p. 178.

sun, moon, and five planets. The animated material heaven and the earth were father and mother of all things. To them the emperor might sacrifice; whilst the mandarins might only worship the lower deities — wind, rain, lightning. Confucius, who may have lived in the sixth century before Christ, directs that the blue firmament should be worshipped, especially at the equinox, as the central point of influence whence the cause of all things acts.

The old Egyptians "lived in adoration of the world above, beneath, and around them." The sun seems to have been the chief God and the source of all the other Gods. They were divided into pairs, masculine and feminine, and they represented cosmic principles.

In Persia, the nature-worship was similar to that of the Hindus. The sun, moon, fire, water, earth, and winds, etc., were divided into good and bad spirits. Zoroaster, the reformer, and supposed writer of the Zendavesta, taught the adoration of the sun and sacred fire as the best representative of the good eternal spirit, Ormazd or Ahuramazda. A corresponding evil spirit, Ahriman, furnished the second element of his dualistic creed.

The earliest Greek ideas of matter and force are given by Pherecides Syrius in the births and marriages of the Gods. He says Zeus was first of all. The second principle was a plastic matter, or earth. Light was the third. Amongst the deities were Uranus, Neptune, Vulcan, and Venus—sky, water, fire, and attraction. Empedocles first taught the doctrine of the four elements. To him, the force of heat was quite distinct from the three forms of matter, earth, water, and air.

If, instead of turning to the records of ancient nations, we take the mythology of the natives of Africa, America, or Polynesia, we find everywhere that ideas of matter and force are distinctly separated or entirely confused, according to the action of the different phenomena of nature on the senses. In New Zealand, the Maoris held that night was the first god, because they thought night produced the day. Heaven was next; then earth; then their children, the gods of light, the sea, etc. They thought that everything was the organ of a special god, or father, or demon, who made it, and kept it what it was. Thus they deified meteors, rainbows, sharks, ants, rats, or any image, to which they gave their own shape and properties.

Among the wild tribes of America, the Great Spirit is the highest member of a group, a personification of the mightiest of all natural energies; the sun, God. The living sun itself is not a symbol, but an actual God. The moon is the evil spirit, capricious and changeful. An animal or thing that is worshipped is not a symbol of this or that divine natural power, but becomes altogether a god, a fetish, or spell, or magical charm.

Even if we look to the origin of our own ideas, we shall find that the belief in the existence of four elements — earth, air, fire, water—which has lasted down to the infancy and perhaps beyond the maturity of some now alive, is quite sufficient to prove that we once had our ideas of force or fire entirely separable from our ideas of the three different conditions in which ponderable matter may occur. We have all gone through the primitive stage of ideas, in which ponderable matter is entirely separable from force.*

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* In the origin of some of the latest religions, the separability of matter and force is distinctly seen. "In 1792,

The second or transition stage of ideas consists in an incomplete separation between the ideas of matter and force.

In this stage, force is considered as altogether separable from ponderable matter, but actually to consist of, or to be perfectly inseparable from an imponderable æther, gas, or fluid, which is capable of being attached for a time to the ponderable matter.

The first trace of the idea that force is imponderable matter seems to have started from Kepler: he used it to account for the motions of the planets. He thought that a current of fluid matter circulated round the sun, carrying the planets with it like a boat in a stream. He

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Johann Schönherr felt that he had got the results of nature in his grasp. Light is the male vivifier; water the female, the nurse. These two arch-beings, the supreme male and supreme female, bound in eternal and in necessary wedlock, explain everything; for in this great wedlock of principles lies the only chance of the seed of things being brought to life. Schönherr felt that this sudden gift of insight was no accident of time and place. It must be more: a revelation from on high; a working of celestial love in his soul; a pouring of the divine will into his spirit. One day he went to Emmanuel Kant, to whom he wished to make known his grand secret, that all living things consist of light and water. 'Very well,' said the expounder of pure reason, 'have you tried to live on them?'"—*Dixon.*

says the vehicle of that virtue which urges the planets, circulates through the spaces of the universe after the manner of a river or whirlpool (*vortex*), moving quicker than the planets. He asserted these vortices to be an "immaterial species," capable, however, of overcoming the inertia of bodies.

Thence came the vortices of Descartes, by which he showed how the world must have been constructed, and thence the agitation of the æther of Leibnitz.

And then follows Newton, who may rightly give his name to this stage of our ideas on matter and force; for Newtonian ideas on matter and force have continued in consequence of his authority down to the present time. Even with regard to gravity, Newton refused to entertain the third stage of ideas. In his second letter to Bentley, 1693, he says: "You sometimes speak of gravity as essential and adherent to matter. Pray do not ascribe that notion to me. The cause of gravity I do not pretend to know, and would take more time to consider of it."

He seems to have been disposed to refer the tendency of bodies to a centre to the elasticity of an æther. It was, however, in his ideas of light that Newton held the imponderable materiality of force. Descartes said that light consisted of small particles emitted by the luminous body. He compares these particles to balls, and endeavours to explain, by means of this comparison, the laws of reflection and refraction. Hooke proposed the theory of undulations, and asserted that light consisted in a quick, short, vibratory motion propagated in a homogeneous medium.

Huygens says Whewell would have established the undulatory theory, but Newton, though at first by no means averse to the assumption of an æther as the vehicle of luminiferous undulations, and even to the last considering the assumption of an æther as highly probable, and its vibrations as important parts of the phenomena of light, yet made the emission ideas the leading part of his optical doctrines. "His disciples found it more easy to conceive the motions of a particle than the propagation of a wave, and the general ascendency of the Newtonian doctrines led to a belief in the materiality of light."

Newton's ideas of the actual emission of some substance as the cause of light lasted until the time of Dr. Young and Fresnel.

The materiality of light led directly to the assumption of the materiality of heat.

Lambert, in 1755, published an essay on the force of heat, in which he likens the communication of heat to the flow of a fluid. To account for the phenomena of heat, a combustible element, an igneous matter, capable of combining or separating itself from other substances, was assumed; this was phlogiston. It was considered, until the time of Black and Lavoisier, as just as much a chemical element as any ponderable element. It was assumed even to have a principle of lightness.

The imponderable materialism of heat, however, survived in the idea of caloric; material heat was an actual flow and emission of material particles. Leslie, in 1804, says, "What is this calorific and frigorific fluid?" It is merely the ambient air. But afterwards he says, "It is the same subtle matter that, according to its different modes of existence, constitutes either heat or light." Even as late as 1832, in the *Bibliothèque Universelle de Génève*, vol. 49, and again in 1834, in the *Annales de Chimie*, vol. 58,

M. Ampère published his views on heat and light considered as results of the vibratory motion of the imponderable æther.

The imponderable materialism of the electric and magnetic force commenced with the same stage of ideas regarding heat.

In 1733, Dufay proposed the idea of two electric fluids, each repelling its own parts or attracting those of the other. Franklin assumed only one fluid, repelling itself and attracting all other matter. In 1803, Dr. Thomas Young, in the *Journal of the Royal Institution*, vol. i., p. 103, gives his ideas regarding the imponderable material substance which, when present in different bodies, gives rise to electricity. " Perhaps," he says, " some antiphlogistian will soon give us a chemical analysis of the electric fluid. Might I be permitted such a doctrine, it should be that it consists of oxygen and hydrogen combined with caloric only." And in the *Annals of Philosophy*, new series, vol. ii., p. 196, Sept., 1821, Mr. Faraday says, " There are many arguments in favour of the materiality of electricity, and but few against it; but still it is only a supposition, and it will be as well to remember, while pursuing the subject of electromagnetism, that we have no proof of the materiality of electricity, or of the existence of any current through the wire."

With regard to the imponderable materiality of the magnetic force. Descartes considered magnetic curves to be the traces of currents of ætherial matter, which are thus rendered sensible even to the eye. According to Æpinus, the phenomena of the opposite poles arose from an excess or defect of a magnetic fluid. Coulomb assumed an austral and a boreal fluid instead of a single fluid; Whewell says the hypothesis of magnetic fluids, as physical realities, was never so widely or strongly embraced as that of two electric fluids. There was no spark, shock, discharge, or mechanical effects. He continues, "if we doubt regarding electric fluids, we cannot help pronouncing upon the magnetic fluids as having still more insecure claims to a material existence. They must be regarded as different effects of one common cause. No philosopher would dream of assuming electric fluids and magnetic fluids as two distinct material agents."

Thus, then, in the second stage of ideas of matter and force, force is made quite inseparable from imponderable matter, although this is considered to be perfectly separable from ponderable matter.

In other words, there is an incomplete separation between the ideas of matter and force.

We come, then, now to the third, or modern stage, in which there is complete union, and no possibility of any separation between the ideas of matter and force.

The absolute union—the complete inseparability—of our ideas of matter and force is most apparent in our ideas of matter and chemical force. Molecules have been endowed with forces which give rise to various chemical qualities, and these never change either in their nature or in their amount. Mr. Faraday says, "A particle of oxygen is ever a particle of oxygen—nothing can in the least wear it. If it enter into combination, and disappear as oxygen—if it pass through a thousand combinations, animal, vegetable, and mineral—if it lie hid for a thousand years, and then be evolved, it is oxygen with its first qualities. Neither more nor less. It has all its original force, and only that; the amount of force which it disengaged when hiding itself has again to be employed in a reverse direction when it is set at liberty. (Faraday, *Researches in Chemistry*, p. 454.)

If it were possible to take the ultimate atom of any one of the elements, we should find that the chemical force which constitutes and determines its nature would be absolutely inseparable from the matter of which the element consists.

For example, the ultimate atom of carbon would have the same kind of force as any mass of carbon, and it would differ in kind, and always from the ultimate atom of any other element, because of the peculiarity of its force. If the chemical force could be separated from the atom of carbon, the matter might cease to be carbon, and might become some other element, and the transmutation of metals might then be possible.

The union, also, between matter and gravity is just as inseparable as the union between matter and chemical force. Matter without weight is not matter at all; the weight belongs to the matter, and cannot be taken from it. The gravity can no more be destroyed than the matter itself can be destroyed. However small the matter may be divided, yet each part will have a part of the force, and there can be no more of the force lost than of the matter. We cannot think that the matter can exist without the force of gravity being always acting or ready to act in each atom of it. Nor can we think that any portion of the force of gravity can be separated from the matter. If we mentally attempt to divide any amount of the force into its constituent portions, then every portion, however minute, of the force must have a corresponding portion of matter to which it is inherent, and without which the force cannot be thought to exist.

Newton's great discovery consisted in determining the existence of force in each particle of the matter of the earth and the planets; and Adams and Leverrier, recognising the action of a force not accounted for in the matter that was known, predicted the existence of unknown matter in an undiscovered planet which was looked for and found.

The great advance, however, towards the modern stage of ideas regarding the inseparability of matter and force is owing to Young and Fresnel, who overthrew the second stage by their discoveries regarding light. Dr. Whewell says, "By their ideas on the interference of undulations, of double refraction (or the passage of undulations through substances in which the resistance to the undulations is different in different directions). By their ideas of transverse vibration or polarisation and dipolarisation, they did away with the most material part of Newton's ideas on the nature of light, and now the only vestige that remains is the luminiferous æther, the imponderable material substance which is still assumed to carry the motions which constitute light."

As the question of the existence of this luminiferous æther is of great interest in connection with our ideas of matter and force, I must bring before you as shortly as possible the opposite views that now exist regarding it, whether it be regarded simply as the carrier, or as essential to the origination of that form of motion called light.

The Rev. T. R. Birks, in his work on matter and æther, published in 1862, most powerfully defends its existence and defines its properties. He assumes that it exercises a strong repulsive power among its own particles, and a strong attraction and power of combining with all other elements. Every monad of ordinary matter, he considers, is combined with a monad of æther, forming a double conjugate compound atom or unit. Compounds of several monads may combine in several ways, forming different chemical elements. Every chemical element is not a solid sphere, either in motion or at rest, but consists of a definite number of units, duads, centres of force, or monads of matter and æther inseparably joined together, arranged in some definite order, revolving usually round some axis of rotation, and parted from the nearest elements by a certain amount of attached æther. Every such element must in its very structure have four or five different sources of contrast, from which a kind of polarity may arise: contrasts of matter, æther, and rotation, and these contrasts may be included under the general term of polarity. For example, positive or vitreous electricity may be an excess of attached æther; negative or resinous, a deficiency.

Professor Clark Maxwell, in the *Philosophical Transactions*, 1865, Part I., p. 460, says: "We have, therefore, some reason to believe, from the phenomena of light and heat, that there is an ethereal medium filling space and permeating bodies, capable of being set in motion and of transmitting that motion from one part to another, and of communicating that motion to gross matter, so as to heat it and affect it in various ways."

Mr. Grove, on the other side, says (p. 163): "In a lecture delivered in January, 1842, I stated that it appeared to me more consistent with known facts to regard light as resulting from a vibration or motion of the molecules of matter itself rather than from a specific æther pervading it, just as sound is propagated by the vibration of wood, or as waves are by water. I am not here speaking of the character of the vibrations of light, sound, or water, which are doubtless very different from each other, but am only comparing them so far as they illustrate the propagation of force by motion in the matter itself.

"To the main objection of Dr. Young, that all bodies would have the properties of solar phosphorus if light consisted in the undulations of ordinary matter, it may be answered that so many bodies have this property, and with so great variety in its duration, that *non constat* all may not have it, though for a time so short that the eye cannot detect its duration.

"If it be said that there is not sufficient elasticity in ordinary matter for the transmission of undulations with such velocity as light is known to travel with, this may be so if the vibrations be supposed exactly analogous to those of sound; but that molecular motion can travel with equal, and even greater, velocity than light, is shown by the rapidity with which electricity traverses a metal wire when each particle of metal is undoubtedly affected."

Mr. Brooke, in the *Proceedings of the Royal Society*, 1867 (p. 412), says: "The known enormous velocity (of probably not less than 250,000 miles in a second) at which electricity travels through a copper conductor is complete evidence that ordinary matter is capable of transmitting something at a considerably greater velocity than the waves of light and heat. Why should not appropriate kinds of matter be assumed capable of transmitting these also? and if so, the need of the interstitial presence of æther ceases altogether."

Mr. Grove sums up the arguments in favour of and against the luminiferous æther thus: (p. 186, ed. 1867): "At the utmost our assumption, on the one hand, is that whenever light, heat, etc., exist, ordinary matter exists, though it may be so attenuated that we cannot recognise it by the test of other forces such as gravitation; and that to the expansibility of matter no limit can be assigned. On the other hand, a specific matter without weight must be assumed, of the existence of which there is no evidence but in the phenomena, for the explanation of which its existence is supposed. To account for the phenomena the æther is assumed, and to prove the existence of the æther the phenomena are cited. For these reasons, and others above given, I think the assumption of the universality of ordinary matter is the least gratuitous."

Mr. W. K. Clifford, in a lecture at the Royal Institution, says: "It has been supposed for a long time that light consists of waves transmitted through an extremely thin ethereal jelly that pervades all space; it is easy to see the very rapid tremor which spreads through a jelly when you shake it at one point. From this hypothesis we can deduce the laws of the propagation of light, and of the way in which different rays interfere with one another, and the laws so deduced are abundantly confirmed by experiment. But here also science kicks down the ladder by which she has risen. In order to explain the phenomena of light, it is not necessary to assume anything more than a periodical oscillation between two states at any given point of space. What the two states are nobody knows, and the only thing we can assert with any degree of probability is that they are not states of merely mechanical displacement, like the tremor of a jelly, for the phenomena of fluorescence appear to negative this supposition."

Of the existence of this æther, spectrum analysis has given us no evidence, yet this most sensitive test has proved that some of the substances that are on our earth exist also in the stars, nebulæ, and comets. As yet no great opportunity for analysing the matter of comets has occurred. It is probable that nitrogen and the aerolitic metals exist in them. When we are told that the tail of the comet of 1811 extended 112 million miles through space, and that the nature of the matter composing it may be determined, it is evident that ultimately we shall know much more regarding the interstellar medium than we do at present.

However much our knowledge may extend, even if it should be proved that an æther exists for the conveyance of light through interstellar space, probably it will be found to be as unessential for the production of light itself, as it is unessential for the production of heat.

The ideas of the union of matter and force followed the same course with regard to heat as they did with regard to light.

"The discovery of polarisation of heat by Professor Forbes, and its confirmation and extension to dipolarisation by Melloni, almost at a single blow ruined the emission theory" (Whewell). It made the doctrine of emission as untenable regarding heat as it had before been found regarding light.

As early as 1798, Count Rumford read a paper before the Royal Society concerning the heat which is excited by friction. "It appears to me," he says, "to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in these experiments, except motion."

In 1799, Davy published a paper on heat and light, in which he said: "I conclude that heat or the power of repulsion is not matter." "Again," he says, "heat, then, or that power which prevents the actual contact of the corpuscles of bodies, and which is the cause of our peculiar sensations of heat and cold, may be defined as a peculiar motion, probably as a vibration of the corpuscles of bodies tending to separate them. It may, with propriety, be called the repulsive motion."

"I am obliged," says Mr. Grove, "in order to be intelligible, to talk of heat as an entity, and of its conduction, radiation, etc.; yet these expressions are, in fact, inconsistent with the dynamic theory, which regards heat as motion, and as nothing else. Thus conduction would be simply a progressive dilatation or motion of the particles of the conducting substance." "The phenomena depend upon the molecular structure of the matter affected; and, although these facts are not absolutely inconsistent with the theory which supposes them to be fluids or entities, it will, I think, be found to be far more consistent with that which views them as motion.

"Heat, which we are at present considering, cannot be insulated. We cannot remove the heat from a substance, and retain it as heat. We can only transmit it to another substance, either as heat, or as some other mode of force. We only know certain changes of matter, for which changes heat is a generic name. The thing heat is unknown."

In 1863, Professor Tyndall says: "The dynamical theory, or, as it is sometimes called, the mechanical theory of heat, discards the idea of materiality as applied to heat. The supporters of this theory do not believe heat to be matter, but to be an accident or condition of matter—namely, a motion of its ultimate particles. From the direct contemplation of some of the phenomena of heat, a profound mind is led almost instinctively to conclude that heat is a kind of motion."

The theory, then, which Rumford so powerfully advocated, and Davy so ably supported, was, that heat is a kind of motion; and that by friction, percussion, and compression, this motion may be generated, as well as by combustion.

Mr. Clifford, in his lecture at the Royal Institution, says: "Fourier, in trying to find the laws of the spread of heat from one part of a body to another, took the hypothesis that heat was a fluid which flowed from the hot end into the cold as water flows through a pipe. From this hypothesis the laws of conduction were deduced; but, in the process, it was found that the very same laws would flow from other hypotheses. In fact, whatever can be explained by the notion of a fluid, can be equally well explained either by the attraction of particles or by the strains of a solid substance. The very same mathematical calculations result from three distinct hypotheses."

With regard to the third stage of ideas regarding the union of matter and electric and magnetic force, Dr. Whewell says, the discovery of the polarisation of heat shook the idea of the electric fluids as physical realities. Material caloric and material electricity fell together.

Mr. Grove says: "Electricity is that affection of matter or mode of force which most distinctly and beautifully brings into relation other modes of force, and exhibits to a great extent, in a qualitative form, its own relation with them, and their reciprocal relations with it and with each other. From the manner in which the particular force called electricity is seemingly transmitted through certain bodies, such as metallic wires, the term current is commonly used to denote its apparent progress.

"It is very difficult to present to the mind any theory which will give a definite conception of its *modus agendi.* The early theories regard its phenomena as produced either by a single fluid, idio-repulsive, but attractive of all other matter; or else as produced by two fluids, each idio-repulsive, but attractive of the other. No substantive theory has been proposed other than these two. But, although this is the case, I think I shall not be unsupported by many who have attentively studied electrical phenomena, in viewing them as resulting, not from the action of a fluid or fluids, but as a molecular polarisation of ordinary matter, or as matter acting by attraction and repulsion in a definite direction."

Whenever we come to the third stage of ideas regarding electricity and magnetism, like light and heat, they will be regarded as peculiar vibratory motions of ponderable matter, having quantitative relations to all other forms of motion. A clear idea of the nature of a complex transverse double polar motion of a particle of gaseous, liquid, or solid matter, is, however, far beyond our attainment now.

Such, then, is a sketch of the history of the three stages of ideas regarding the separability of matter and force.

It must, however, be remembered, that these three stages pass insensibly into one another; and that, in the history of these ideas in individual minds, some ideas regarding the separability of matter and force may be found in the third stage, while other ideas may still halt at the second, or never get beyond even the first stage which I have described.

Thus every one may admit the inseparability of matter from the causes of gravitation and chemical action; whilst some may hardly be prepared even now to allow that heat is equally inseparable from matter. Still less readily may the cause of light be admitted to be inherent in ponderable matter; and few at present are able to believe that electricity and magnetism are peculiar motions, excited only in the molecules of ponderable matter.

In other words, the third stage of ideas regarding the union of matter and force may to some extent be received by every one; but, in its full extent, it is only gradually becoming recognised as undoubted truth.

As soon as it is admitted that force is absolutely inseparable from matter, whether gas, liquid, or solid, it will become as impossible to think that matter can consist only of centres of force, as to think that matter can be inert or void of all force.

Matter, if it be inseparable from force, must always be in a state of motion, or of tendency to, or of resistance to, motion; it can never be in a state of perfect rest.

Immediately connected also with the idea of the inseparability of matter and force is another modern idea regarding force, the history of which in England must be given here.

This idea was first represented by the term conversion of force, then by correlation of forces, then by conservation of force, and now by conservation of energy.

In 1839, Mr. Faraday, in a paper on the Improbability of Contact exciting Voltaic

Electricity, wrote (*Exp. Researches,* vol. ii., p. 103): "We have many processes by which the form of the power may be so changed that an apparent *conversion* of one into another takes place." "But in no case is there a pure creation—a production of power, without a corresponding exhaustion of something to supply it."

Mr. Grove, in a lecture on the Progress of Physical Science in 1842, says: "The present tendency of theory seems to lead to the opinion that all these affections (of matter) are resolvable into one—namely, motion;" and: "Light, heat, electricity, magnetism, motion, and chemical affinity, are all convertible material affections. Assuming either as the cause, one of the others will be the effect. Thus heat may be said to produce electricity, electricity to produce heat, magnetism to produce electricity, electricity magnetism, and so of the rest. . . . We must humbly refer their causation to one omnipresent influence, and content ourselves with studying their effect, and developing by experiment their mutual relations."

In 1843, in a course of lectures on the Correlation of Forces, he says: "The view which I venture to submit to you in these lectures is, that force cannot be annihilated, but is merely subdivided, and altered in direction and character." "I will venture as an opinion, founded after much consideration, that science is rapidly progressing towards the establishment of immediate or direct relations between all these forces."

In 1843, Mr. Joule (*Phil. Mag.*, vol. xxiii., p. 442), on the Mechanical Value of Heat, says that he shall repeat and extend his experiments, being satisfied that the grand agents of Nature are, by the Creator's fiat, indestructible, and that, wherever mechanical force is expended, an exact equivalent of heat is always obtained; and he mentions the practical conclusions which may be drawn from the convertibility of heat and mechanical powers into one another.

Mr. Faraday, in a lecture on the Conservation of Force in 1857 (*Researches*, p. 454), says the idea we have of the indestructibility of individual matter is one case, and a most important one, of the conservation of force; and, in a manuscript, he says: "Force cannot be annihilated or created at pleasure. It cannot act, then cease to act, then act, then cease to act, without being otherwise disposed of."

In another manuscript, he says: "What I mean by the word force is the source or sources of all possible actions of the particles or materials of the universe; being often called the powers of Nature, when spoken of in respect of the different manners in which these effects are shown."

In order to avoid the indistinctness and confusion which come from using the word force at one time as the cause of an effect, and at another time as the effect itself; and, in order to confine ideas to the effect which alone can be the object of experimental research, the term conservation of energy is now adopted, instead of the term conservation of force.

Thus, force, which we assume to be indestructible and inseparable from matter, is the cause of energy, and energy is the effect of force.

This diagram may perhaps render the use of these terms more clear:—

$$\overbrace{\left.\begin{array}{l}\text{Force or power, total}\\ \text{quantity of which is}\\ \text{as constant as the total}\\ \text{quantity of matter.}\end{array}\right\}}^{\text{CAUSE.}} = \overbrace{\left\{\begin{array}{l}\text{Actual energy} = \text{Motion}\\ \text{Potential energy} = \text{Tension}\end{array}\right\}}^{\text{EFFECTS.}} = \text{Work.}$$

The idea of the conservation of energy is, that the sum of the actual and potential energy in the world is constant.

The force, or, as it is better called, the actual energy, that can be put into the cannon ball, is exactly equal to the latent or potential energy in the gunpowder, and this depends on the chemical force in the oxygen, hydrogen, and carbon. The ball gains only what the powder loses. As the potential energy, or tension, decreases, the actual energy, or motion, increases, and no loss or gain in the total amount of motion and tension can occur.

Whatever the form of motion may be, it can only come either from some other form of motion or from some form of tension.

It has been Dr. Joule's great work to exhibit experimentally the measurable relation between heat and gravity. He has determined that a certain amount of one kind of motion does produce an equivalent quantity of another kind. He proved that 772 pounds weight of matter falling one foot gives rise to enough heat to raise a pound of water one deg. Fahr. in temperature. He called the unit of mechanical motion one pound weight falling one foot=one foot-pound, and the unit of heat that quantity which raises one pound of water one deg. Fahr.; then the mechanical equivalent of heat is 772 foot-pounds.

The equivalence of other forms of energy has yet to be determined by experiment.

Meanwhile, according to modern ideas, the different forms are so related to one another, that none can be lost, and none can be produced except by passing into or out of some other form of energy.

"But," says Mr. Faraday (*Researches in Chemistry and Physics*, p. 459), "after all, the principle of the conservation of force may by some be denied. Well, then, if it be unfounded, even in its application to the smallest part of the science of force, the proof must be within our reach, for all physical science is so. In that case, discoveries as large, or even larger, than any yet made, may be anticipated. I do not resist the search for them; for no one can do harm, but only good, who works with an earnest and truthful spirit in such a direction. But let us not admit the destruction or creation of force without clear and constant proof.

"Just as the chemist owes all the perfection of his science to his dependence on the certainty of gravitation applied by the balance, so may the physical philosopher expect to find the greatest security and the utmost aid in the principle of the conservation of force. All that we have that is good and safe, as the steam-engine, the electric telegraph, etc., witness to that principle. It would require a perpetual motion, a fire without heat, heat without a source, action without reaction, cause without effect, or effect without a cause, to displace it from its rank as a law of Nature."

Finally, he says: "By admitting no hypothesis, and believing in no assertion of any fact opposed to the principle of the conservation of force, the natural philosopher is prepared to look for effects and conditions as yet unknown, and the way for him is open to any degree of development of the consequences and relations of power. By denying this principle, he opposes a dogmatic barrier to improvement; whilst, by admitting it, he has a fresh stimulus to investigation and a pilot to human science." (P. 450.)

In my next lectures, I shall show you how far the three stages of ideas on the separability of matter and force can be recognised in the history of sciences into which the idea of life enters; and I shall try to make some application of the doctrine of the conservation of energy to physiology, pathology, and therapeutics.

LECTURE II.

ON THE FIRST AND SECOND STAGES OF OUR IDEAS REGARDING THE UNION OF PONDERABLE MATTER AND FORCE IN THE BIOLOGICAL SCIENCES.

> "And matter, whatever it is, must be held to be so adorned, furnished, and formed, that all virtue, essence, action, and natural motion, may be the consequence and emanation thereof."—Lord Bacon.

Mr. President,—

I ought to repeat the apology which I offered to you in my first lecture, because I shall begin this lecture with some words of wisdom which might not be expected in this theatre, and yet they are words to which I must ask you to give the utmost weight as regards what I am about to say. Mr. Faraday was the greatest experimental philosopher, judging from the quantity and quality of his work, that England, or I might say the world, ever produced; and, at the same time, perhaps he was one of the most religious men that ever lived. Among his manuscripts, that have been entrusted to me since his death, I have found this passage: "But, though the natural works of God can never by any possibility come into contradiction with the higher things which belong to our future existence, and must, with everything concerning Him, ever glorify Him, still I do not think it at all necessary to tie the study of the natural sciences and of religion together."

It appears, from what I have said in my first lecture, that a certain fixed direction of development of knowledge is found to obtain in our ideas of the union of matter and force in the sciences from which the idea of life is excluded.

Whether we judge from the general history of science, or from the history of the knowledge of individual minds, the same succession of ideas marks the advance which is gradually being made.

First, the ideas of matter and force are considered to be completely separable; secondly, they are held to be incompletely separable; and, lastly, these ideas are found to be completely inseparable the one from the other.

When all idea of life is excluded, the present state of our ideas regarding the union of matter and force may be summed up in the following formulæ. Where matter is, there force must be, showing itself in motion, or tension, or in resistance. Without matter, no kind of motion, nor tension, nor resistance, occurs.

Turning now to the matters and forces in those sciences in which the idea of life is concerned, the question arises, What is the history of the progress of our ideas regarding the union of living matter and vital force?

As regards the biological sciences in general, or as regards the biological knowledge which each individual acquires, are these three stages here also recognisable? First the complete, and secondly the partial separation of our ideas regarding living matter and living force; and is there a third stage, in which there is a perfect inseparability of the ideas of matter and force? Has our biological knowledge passed, or is it passing, through these three stages? Will the biological sciences follow in the footsteps of all other sciences? Will they share with them in the extent and rate of progress which will result from clearer and more connected ideas regarding matter and force?

Before I proceed, let me state most distinctly, that what I shall say regarding Life will not apply only to man, but will be just as truly applicable to every other class of animals down to the infusoria and protozoa, and, beyond these, to the whole extent of the vegetable creation, in which no reason, no mind, no soul, can by any stretch of the imagination be supposed to exist.

In this theatre, at least, there can be no fear of misapprehension, no danger of mistake, from any confusion of our ideas of life with our ideas of the immortal soul. And, however much these ideas may be generally confounded and indistinct, to us at least the question whether, first, a peculiar vital spirit can temporarily lodge in the matter of the body; or whether, secondly, a peculiar imponderable vital fluid can temporarily be diffused through the matter of the body; or whether, thirdly, a peculiar form of motion of the matter of the body is all we can recognise as constituting life;— each and all of these questions, I say, can never be considered by us to have anything whatever in common with any question regarding the existence of the immortal soul.

Let us, then, proceed to the primitive stage of ideas regarding the union of matter and vital force.

In the earliest Jewish record, the same separation may be traced between vegetable and animal matter and life as between earthy and watery matter and light.

The separation between matter and force is most distinctly stated in the highest of all organised beings. We read that man was formed of the dust of the ground; and, *after* he was formed, the breath of life was breathed into his nostrils. This is probably the oldest idea in existence regarding the nature of the vital force. It was a something added to the matter of the full-formed man. The body was without force until the life was given. The idea of the force is distinctly separable from the idea of the body, just as the idea of light was distinctly separable from the idea of the sun. This, then, is the earliest statement of the first stage of ideas regarding the union of matter and force. In it, perfect separation is supposed to be possible between matter and living force.

If the Book of Genesis be a revelation of physical science by the Almighty to man, then the existence of vital force separate from the full-formed body is true, and must be believed; but if this book, so far as regards science, represents only the existing state of knowledge at the time when it was written, as is shown by the facts mentioned in it contradicting the revelation which the Almighty has made in his works,* then, whatever may be the interest we feel in the earliest record of scientific knowledge, still it cannot be allowed to possess any scientific authority in determining what is the true relation of matter and vital force.

By the early Hindus, the human soul, or life, was thought to be a portion of the Supreme Ruler; as a spark of the fire; as a part of the

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* The contradictions between the Book of Genesis and the revelation given in God's works, are seen in the statement —1, that day and night and light existed before the sun was made; 2, that darkness was as much an entity as light; 3, that the moon had a light of its own like the sun; 4, that the firmament divided the waters from the waters—in other words, that there was water compared to the sea above the heavens; and 5, in the particulars regarding the order and time of creation of inorganic and organic things. Similar or identical ideas are met with among other nations and tribes before the dawn of natural knowledge. The supposition that an untrue revelation was willingly made by omniscience in consequence of the ignorance of the Jews is far beyond all possibility of belief.

whole. After transmigration, the spirit was thought to be utterly reabsorbed into the divine essence. The vital powers, and the elements of which the body consists, were both considered to be absorbed absolutely and completely. Both name and form ceased. Immortality without members or parts began.*

The Brahman of the old religion and the Buddhist of the later religion rejected entirely the idea of the personal immortality of the matter of the body; but they differed completely in the idea of the state in which the separated spirit ultimately existed. In the later religion, Nirvana, or utter annihilation, was the ultimate aim of the human spirit.

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* "From the doctrine of emanation sprung up the doctrine of transmigration. The human spirit may be united with the lowest species of organic life, may ascend in successive births into the bodies of spiders, snakes, and chameleons, until deemed worthy of inhabiting a human tenement. Then an opportunity is given it of achieving its own liberation; and, according to the present quality of its actions, it will mount directly upwards through the ranks of demigods and gods, or plunge again into the lower region of existence, and commence a fresh series of births.

"The great object of Brahminical or Buddhist religion is the discovery of the means of putting a stop to further transmigration; the discontinuance of corporeal being; the liberation of the soul from the body."—*Archdeacon Hardwick*, vol. ii., p. 295.

The Egyptians took a different view of matter. "The conservation of the body was essential to the vigour and felicity of the soul; hence embalming, in order that the spirit may have something on which to lean for help. In virtue of the strength afforded to the spirit by this union with the former cause of her vitality, she continued to exist in some analogous condition—disembodied, but still associated with her previous tenement, and still in some mysterious fashion living by its life." (Hardwick, vol. ii., p. 297.)

The tombs and pyramids were made to protect the body while the spirit went to Amenti, to be judged by Osiris, either when acquitted to pass into a state of permanent felicity in the sun, or when condemned to return to some animal shape on earth.

One chief part of the religion of the Egyptians consisted in the worship of the dead. They prayed to the authors of their bodies. They buried with them food, dress, and implements of war, business, and pleasure. They even made the spirit into a peculiar material substance, and did not consider it as a purely spiritual essence.

Confucius taught the Chinese duty to parents while they lived, and to give sacrifices to them when dead. They worshipped their ancestors, as beings capable of giving aid and counsel to the deserving, and inflicting vengeance on the unworthy. They gave them food, clothes, and even gilt paper-money.

Confucius said the body consisted of two principles—the one light, invisible, and ascending; and the other gross, palpable, and descending; and that, on the separation of these two principles, the light or spiritual part ascended into the air, whilst the heavy or corporeal part sank into the earth.

The Greeks personified their idea of life, and confused it with motion, matter, and mind.

Thales thought that there must be a living soul, a vital principle, in amber and the magnet, because they have a moving force.

Anaximenes thought the air was an ensouled and ensouling force, and that it was the life of the body.

Heraclitus of Ephesus admitted no distinction between fire, life, and the soul. Man's soul or life was an emanated portion of the universal fire or universal reason which encompasses the heaven and rules all.

Democritus maintained that the vital principle and mind were absolutely identical.

If we look at the belief of savages, we find that the Maoris worship departed ancestors, because their spirits could exercise vengeance and inflict injuries (after the fashion of witches). The chief had his bow and arrows buried with him, that he might go forth at night to his old calling. He was made a god, and went to battle for his kinsmen. He was hovering near them in the hours of gloom, of peril and privation. He was spoken to as a familiar friend, and was given offerings of betal and tobacco, and appeared personally in the sacred house to the living chief of the tribe.

The extent to which they separated and personified their ideas of force is seen in the fact that they supposed each disease was caused by a different god, who resided in the part affected.

Livingstone says (p. 642) that some of the southern tribes of Africa not only believe in the transmigration of souls, but think that, while persons are still living, they may enter into lions and alligators, and then return again to their own bodies.

Thus, then, in Hindustan, Egypt, and China, and among the wild tribes of New Zealand and Africa, the idea of the separation between body and spirit was distinctly held. The spirit of the deceased ancestor was believed to linger for a certain period near the place of burial, and to be pleased or displeased by the offerings made to it. After a time, it began to pass through the different forms of matter to which it was to be temporarily attached.

At the time of the revival of knowledge, the perfect separation of the ideas of life and body prevailed universally.

Thus Van Helmont considered that the body contained a presiding spirit or *archæus.*

Paracelsus considered this *archæus* lived in the stomach.

Stahl taught that the body, as body, has no power to move itself, but must be always put into motion by an immaterial substance. The cause of all activity, he said, was an immaterial being, which he called the soul.

Harvey (ed. 1766, p. 294) has a chapter headed "Ovum non esse opus uteri sed animæ."

Looking at the great operations which go on in the egg, he says we must agree with the poet—

> "Spiritus intus alit, totamque infusa per artus
> Mens agitat molem."

He carries us, in the idea of life, far beyond cells, nuclei, and germinal granular matter, even as far as flatulence, when he says, "Alibique, etiam flatibus vitam quandam atque ortum et interitum inesse, arbitrantur."

He ends this chapter on the egg thus: "Quibus rite pensitatis, propriam illi inesse animam concludimus."

At the present time, the popular idea is, that no distinction exists between the life, the mind, and the soul of man; all are confused together; and all are thought to compose a single immaterial spirit, which comes at birth and goes at death, being perfectly separable from the matter of which we are made.

Thus, in the earliest ideas of all nations, and probably of all individuals, no doubt whatever exists as to the entire separability of the ideas of ponderable matter and of vital force; and the first stage of ideas in the biological sciences is identically the same as the first stage in those sciences into which the idea of life does not enter.

As the second stage of ideas in the abiological sciences consisted in the assumption of an imponderable matter, which was considered to be separable from the ponderable matter, and diffusible through it, giving rise to the phenomena of force in every part; so the second stage, or partial separation in the ideas of living force and matter, is marked by the assumption of a living imponderable gas or fluid pervading the ponderable matter.

This stage, from the renown of him who gave it the greatest support in England, may well be called the Hunterian stage.

Even Asclepiades and Galen spoke of a subtle humour or spirit existing in the body, which they compared to air.

The chemical tendencies of the seventeenth century led to the supposition that it was sulphureous or nitrous acid; and, soon afterwards, it was compared to an æther. Even Newton, in the twenty-third query, says: "Is not vision performed by the vibrations of this medium æther at the bottom of the eye, propagated by the solid capillamenta of the nerves to the place of sensation?" and in the twenty-fourth query: "Is not animal motion performed by the vibrations of this medium æther?"

The idea of a vital fluid, a material substance of extreme subtlety, volatility, and energy—in other words, a subtle ætherial fluid diffused through the frame, enabling it to show all the phenomena of life—was taught by Hofmann.

This imponderable fluid was even so far personified or incarnated, that each particle of the vital fluid was believed to have a determined idea of the whole mechanism and organism; and, according to this idea, it formed the body, and preserved it by its motion.

As the idea of an electric fluid or fluids became stronger, so the idea of the vital fluid was made more scientific by imagining it to be similar to or even identical with the electric fluid.

John Hunter said: "The living principle of the blood is the *materia vitæ diffusa*, of which every part of an animal has its portion. It is as it were diffused through the whole solids and fluids, making a necessary constituent part of them, and forming with them a perfect whole, giving to both the power of preservation and the susceptibility of impression, and, from their construction, giving them consequent reciprocal action." (Hunter's *Works*, vol. iii., p. 115.)

Again, he says: "The blood has as much the *materia vitæ* as the solids, which keeps up that harmony between them." (P. 115.)

Instead of a single vital fluid capable of being an imponderable of all work, many different vital fluids, each capable of doing its own work and no other, were assumed. The most remarkable of all these was the nervous fluid.

Glisson describes the vital fluid residing in the nerves as resembling the spirituous part of white of egg. Whether this was carbonate of ammonia or sulphuretted hydrogen, must be undetermined.

Even Cuvier, in the introduction to the *Règne Animal*, p. 30, says: "There is great probability that it is by an imponderable fluid that the nerve acts upon the fibre; and that this nervous fluid is drawn from the blood and secreted by the medullary matter."

Johannes Müller taught that the nervous agent differed entirely from electricity or light, but was either an imponderable matter, or the undulations of a fluid capable, like electricity through copper, of travelling at the rate of nearly three hundred thousand miles a second, or, like light through the interstellar æther, at two hundred thousand miles a second.

The researches of Helmholtz* show that the rate of passage of that motion through the particles of matter in a nerve which gives rise to sensation or to motion is only between twenty-eight and thirty-three feet in a second.

So late as 1866, Professor Bain, in a lecture at the Royal Institution on the Correlation of Force in its Bearing on Mind, speaks "of a certain flow of the influence circulating through the nerves."

Another very remarkable vital fluid was considered to be a chemical agent, an elementary principle, or highly-attenuated substance, which, among its other singular and remarkable properties, had that of imparting to the constituent matters of the animal frame new chemical affinities between it and the surrounding elements, and thereby protecting the living fibre from dissolution, or giving them other properties which, without this vitalisation, they would not possess.

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* Appendix I.

Out of many other examples, I will take two most interesting chemical discoveries, not yet fully completed, which show in a striking way how many so-called vital actions, as knowledge advances, may be included among ordinary chemical or physical actions.

The first of these discoveries is respecting the coagulation of the blood. Hunter said (vol. iii., p. 113): "As the coagulation of the blood appears to be that process which may be compared with the action of life in the solids, we shall examine this process a little further, and see if this power of coagulation can be destroyed. If it can, we shall next inquire if by the same means life is destroyed in the solids, and if the phenomena are nearly the same in both."

"Coagulation," again he says, "I conceive to be an operation of life corresponding to the convulsion of muscles which takes place at the moment of death." "The blood loses the principle of coagulation, and, I suppose, life." (P. 115.)

Alexander Schmidt has found that fibrine is formed by the contact of two albuminous matters. One he calls fibrinoplastic, and the other fibrinogenic. The first is especially plentiful in the red corpuscles, the serum of the blood, the cellular tissue, and the cornea. The second is found in exudations, especially in the pericardium and fluid of hydrocele, in lymph and chyle. When these two substances come into contact in any fluid, they combine quickly or slowly, according to the greater or less quantity of each substance, to form fibrine. The action takes place more quickly at a high temperature, more slowly at a low temperature.

You see these two substances here. The paraglobulin or plastic substance has been obtained from serum or blood-corpuscles; the fibrinogenic substance from pericardium-fluid. Apart, they never form fibrine. When mixed together, they form it quickly, as you see, although they have been already many hours in these flasks.

All idea, then, of a vital action in the coagulation of the blood must be given up, although much more has to be made out of the chemistry of these substances, and especially of the paraglobulin, which is, according to Professor Brücke (*Proceedings of the Imperial Academy of Sciences,* May 23, 1867), a mixture of two substances, one of which acts as the fibrinogenic matter.

Those who still think that vital action must be concerned in the coagulation of the blood will probably say that it is the principle of life that stops the mutual action of these two substances in healthy circulating blood.

The chemist will before long solve the problem why these two substances do not always act upon one another, since they can both be got out of the blood. The most probable answer is, that there is some slight difference of composition between these substances as they exist in circulating blood and as we obtain them from blood that has been drawn from the body. The fibrinoplastic substance is rapidly acted on by ozone. The fibrinogenic substance also is rapidly changed after it is formed, so that possibly the necessary quantity of these substances is not given the necessary time to act upon one another for the production of coagula.

The second of the unfinished discoveries which I wish to bring before you relates to the subject of digestion.

In Dr. Prout's *Bridgewater Treatise* (p. 493), he says: "The stomach must have the power of organising and vitalising the different alimentary substances. It is impossible to imagine that this agency of the stomach can be chemical. This agency is vital, and its nature is completely unknown."

In his work on *Stomach and Renal Disease* (5th ed., p. 490), he says: "The third or vitalising function may in some instances be suspended or otherwise deranged. Thus, when more food is taken by healthy individuals than is required for the purposes of the animal economy, there is reason to believe that, however perfectly the superfluous portion of the aliments may, for the sake of enabling them to pass through the system without producing great disorder, be dissolved and converted, the vitalising function is withheld; and that such superfluous matters are finally eliminated either with the bile or in the form of lithate of ammonia in the urine." In short, excess of urates in the urine after a large meal may depend on a want of vital action.

Some late researches of Kühne on the action of the pancreas (Virchow's *Archiv*, vol. xxxix., p. 130) appear to me to promise the solution of the problem why a portion of the albuminous food passes off as urates in the urine, whilst the larger portion remains as albumen in the blood.

Kühne shows that albuminous and fibrinous substances when subjected to the continuous action of the pancreatic juice give rise to tyrosine, leucine, and an aniline-like substance in much greater quantity than happens when albumen is fused with potassa, or boiled with sulphuric acid, or allowed to decompose.

Here are tyrosine and leucine thus prepared by the action of the pancreatic juice.

It seems in the highest degree probable that, when an excess of albuminous food is taken, then, in consequence of the slower absorption, a portion of it remains long exposed to the action of the pancreatic fluid, and this continued action of the pancreatic fluid on albumen produces substances which are able, more or less immediately, to form uric acid.

The relation of some of these substances is seen in the following table:—

Tyrosine $= C^9 H^{11} N O^3$
Leucine $= C^6 H^{13} N O^2$
Uric acid $= C^5 H^4 N^4 O^3$
Creatine $= C^4 H^9 N^3 O^2$
Urea $= C H^4 N^2 O$

Measured by the carbon it contains, tyrosine is a higher product of albumen than leucine, uric acid, or urea.

Whether it, or some other product from albumen in the blood or in the kidney, gives rise to uric acid, has to be determined.

Certain it is that our chemical knowledge now entirely does away with all idea of any want of vital action, causing excess of urates in the urine, and a very slight advance will probably enable us to prepare uric acid in the laboratory from the albuminous substances.

The progress of animal chemistry proves more and more clearly that the matter in the body has no special chemical properties peculiar to life; but that matter within possesses the same chemical energy which it possesses out of the body.

Life has no power to create or to destroy any chemical force in the matter of living things; but the very slightest difference in the circumstances under which any chemical action occurs, produces a variation in the effects that are produced.

Ultimately, when all the circumstances under which vital chemical actions occur are fully made out, there will be found to be no difference between these actions and those which can be made to take place where no influence of life can be supposed to exist.

Another vital fluid, or principle, has been assumed to account for the phenomena displayed at the beginning of life in animal and vegetable bodies.

Dr. Prichard says: "This vital principle assumes the character of a plastic or formative power. It presides over, and sets in action, the different processes by which growth and organisation are effected; and gives form and modification to the component parts of the animal and vegetable body, and contributes afterwards by a preserving influence to the maintenance of its existence for a definite portion of time. This doctrine ascribes to a thing which is supposed to be merely a species of matter highly attenuated, properties and agencies which belong to the highest power and the highest intelligence."

It appears, then, that, in the biological as in the abiological sciences, ideas of the union of matter and force have gone, or are going, through two identical phases.

As in the abiological sciences the earliest ideas of matter were quite separate from the ideas of force, so vital force, in the first stage of our ideas, was originally pure spirit, void of all materiality, perfectly separable from matter.

In the second or transition stage in the abiological sciences, force was imperfectly separable from matter. Force was itself a highly-attenuated volatile substance, an imponderable elementary principle; or the force was inseparably united with an imponderable fluid, gas, or æther, which gave to ponderable matter its energies whilst the union lasted.

In the second, or transition, or Hunterian stage in the biological sciences, the vital force is either itself an imponderable solid, fluid, gas, spirit (*geist*), or ghost, or it is inseparably attached to an imponderable matter which can temporarily be diffused through ponderable matter.

In my next lecture I shall show you that the abiological sciences are far in advance of the biological sciences in the extent to which the third stage of ideas of the perfect union of ponderable matter and force is held to obtain.

I shall try, also, by some examples in physiology, pathology, and therapeutics, to show you that in this third stage of perfect inseparability of matter and force, and in the principle of the conservation of energy, "we have," in the words of Mr. Faraday, "a new stimulus to investigation and a pilot which will lead to discoveries far beyond the region to which our present (biological) knowledge extends."

LECTURE III.

ON THE THIRD STAGE OF OUR IDEAS REGARDING THE UNION OF PONDERABLE MATTER AND FORCE IN THE BIOLOGICAL SCIENCES.

MR. PRESIDENT,—

I come now to the object for which I undertook this course of lectures.

It is to show you that, even in this country in which John Hunter's great authority makes the *materia vitæ diffusa* a reality, and I might almost say a part of the *religio medici*, yet even here, the third stage of ideas regarding the union of ponderable matter and force is beginning to grow up amongst us, and gives the promise of fruit far beyond any we have hitherto gathered in physiology, pathology, and therapeutics.

The general ideas regarding the union of ponderable matter and force in the biological sciences have not as yet got beyond the second or Hunterian stage; and there are many who think that the first stage, which consists in the complete separability between matter and vital force, still represents the whole truth.

The third stage, or complete union between the ideas of matter and vital force—that is, the utter inseparability of one particle of vital force from the matter in which it has been placed, is as yet in its infancy, and cannot be yet subjected to the examination it will have to undergo.

We are only just entering upon the inquiry how far our ideas of conservation and correlation of energy can be extended to the biological sciences.

We are only just beginning to ask how far a balance can be struck between the income and outgoing of all the energy which vegetables and animals possess in health and in disease.

The quantity and quality of the energy that comes in, and the quantity and quality of the energy that goes out of the living thing, are questions in every respect as inseparable from the progress of biology, as the quantity and quality of the ponderable matter that enters into or is thrown out of any system.

The slight progress that has hitherto been made is seen in the fact that there are many persons at present able to rest in the belief that the inseparability and conservation of matter and force are our main principles and our best guides in the abiological sciences; whilst most persons think that in the biological sciences the perfect or imperfect separability of matter and force, and the constant creation and annihilation of energy, are most likely to be the true foundations on which the sciences that include life must be built.

But, if this be so, then the creation and annihilation of force must be established by positive proof, and must not rest only on assumptions which are directly opposed to the most certain knowledge we possess regarding ponderable matter and force in other branches of science.

We know now that in all living things no separate or peculiar matter is present.

The stuff which takes part in the living actions, and the forces which are inherent in that stuff are there, and indestructible and inseparable. Inorganic matter and inorganic force always exist together in living things; so that, if a separable living force be also present, then we must admit that two totally different relations of ponderable matter and force must obtain in the same matter at the same time.

The unity of nature will at least be preserved by our hesitation to admit the assumption of a force capable of creation and annihilation, until some very conclusive evidence is obtained that there actually is in living things such a force or forces capable of being separated entirely from the matter of which they are made.

Until this be proved, let us inquire how far the third stage of ideas regarding the union of matter and force and the principle of the conservation of energy opens new paths for investigation, and gives us new glimpses of truth.

What are the actual and potential energies which enter into the body? To what motions do these energies give rise there? How do these motions leave the body?

Matters in a state of tension, and ready for chemical motion, are constantly going into the body in the food and air. The quantity of active and latent energy which goes in, ought exactly to balance the quantity which comes out, deducting that which remains latent in the chemical substances, or becomes active in the actual warmth of the body itself.

The chemical changes in the matter within the body (that is, the decrease of potential energy or tension) give rise to different forms of motion. These motions appear as the functions or work of the body.

Two chief kinds may be distinguished—first, motion in great masses, as mechanical work, etc.; secondly, motion in little masses, or molecular motion, as heat, electricity, nutrition, etc. A little mass of carbon and of oxygen contains a certain amount of tendency to move; that is, of potential energy. These molecules, as soon as they can, move together, and the motion produced must continue in some form until a state of tension or potential energy again results.

The latent force of the nourishment is most easily measured by determining the amount of latent heat which the food contains. In order to do this a known amount of substance is burnt, and the amount of heat produced is determined; and as the mechanical equivalent of heat is known, the equivalent amount of work which the substance can do may be calculated.

By far the greater part of the potential energy or tension which goes in is ultimately changed into warmth. Other modes of motion, as electricity and mechanical work, take but a small part of the total income. The balance-sheet at present can give an idea of the form which the account will ultimately take; but it cannot tell the items with any approach to accuracy.

The difficulties which surround the determination of these items at present may be seen by looking for a few moments where our knowledge now is regarding the origin of muscular motion.

According to the earliest ideas, muscular motion was produced by the soul.

According to transition ideas, the motion was considered to be created by a definite something, a vital force or fluid, capable of being increased or diminished according as stimulants (nervous or other) acted on it or not; and finally, capable of quitting the matter of the muscle a short time after the body was dead.

The Hallerian irritability was the best representative of this stage of ideas.

According to the latest ideas, the origin of the motion must be some antecedent equivalent motion. This is looked for in the chemical changes in the nitrogenous or the non-nitrogenous matter of the contractile texture or surrounding blood.

"No one," says Dr. Frankland, "possessing any knowledge of physical science, would now venture to hold that vital force is the source of muscular power. An animal, however high its organisation, can no more generate an amount of force capable of moving a grain of sand than a stone can fall upwards, or a locomotive drive a train without fuel."

Professors Liebig and Playfair, and others, say that the chemical changes in the nitrogenous matter of the muscles are the cause of motion.

Professors Frankland, Fick,* and others, say that the mechanical work is much greater than can be accounted for by the amount of change in this matter, as measured by the urea produced. They determine the amount of

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* Appendix II.

mechanical work done in a given time, and they translate it into its equivalent of heat, weighing also the urea produced in that time. By burning a known weight of muscle out of the body, they determine how much heat it can produce; and from this they can calculate how much muscle must be burnt in the body to give an amount of heat equivalent to the mechanical work done in the given time. They then calculate what amount of urea this weight of muscle would produce. By comparing the actual amount of urea produced with the calculated amount, it appears that only one-fifth of the work can come from chemical change in the nitrogenous texture of the muscles. Four-fifths of the work must arise from the chemical action going on in the non-nitrogenous matters in the muscles or in the surrounding blood.

The experiments made by Dr. Parkes most completely confirm the view that the motion of the muscle during exercise does not bear any relationship to the amount of chemical disintegration in the albuminous substance of the muscle. Indeed, he suggests the opinion that the action of the muscle is not connected with disintegration, but with formation; that, when it is in exercise, it increases; and, when it is quiescent, it lessens in bulk; that is, that it more rapidly disintegrates during rest than during exercise. This view can only be proved by a vast amount of further experimental research.

When this question, which regards the value of this one item in the expenditure and income of matter and force, is solved by experiment, there will be left still more difficult problems—as, for example, how this conversion of heat into contraction of muscular fibre takes place; how the nerves are made to be able to increase or lessen the conversion of latent into active energy at will.

This most extensive and important inquiry, which the doctrine of the conservation of energy has opened to our work, is good evidence that the third stage of ideas on the union of matter and force is beginning to be received in physiology.

What the worth of these ideas is may be seen in the comparative simplification of the problem of the source of animal heat, when the doctrine of the conservation of energy is admitted.

Whether animal heat arises from nervous force or from chemical action has been a point as disputed as whether voltaic electricity arose from metallic contact or from chemical action.

The dispute is ended in both instances by the admission of the doctrine of the conservation of energy.

If the animal heat comes from nervous force, or the electricity from metallic contact, then what is the equivalent of potential energy that gives these their actual energy? Unless the creation of force be assumed, the equivalent in actual or potential energy must be forthcoming.

In the case of animal heat, it may be said that the nervous force comes from an equivalent energy of nutrition; but then the further question must be answered, Whence does this energy of nutrition come? And this brings us ultimately to the chemical force which gives the potential energy to the matter that enters the body. This is the prime origin of the motion which we call animal heat.

The different kinds of apparatus or organs which the animal possesses for the conversion of energy determine in what form of motion the expenditure of energy can take place.

The brain, the nerves, the muscles, the electric organs, the textures in general, all these are machines set in action chiefly by the potential energy or tension in the food, textures, and air; the supply of oxygen, hydrogen, and carbon being the first necessary condition.

The mechanical, chemical, nutritive, muscular, and nervous motions are so related, that it is most difficult to separate the action of any one motion, even the simplest which can occur in the human body.

When these motions are so increased or diminished as to constitute disease, then the difficulty of isolating any one motion becomes by no means lessened. Still, even in disease, the doctrine of the conservation of energy can enable us to make at least as great an advance as was made in our ideas and language when the doctrine of phlogiston, or the hypothetical inflammable principle which was thought to possess a power of levity, was given up.

Vital force in disease must cease to be regarded as an imponderable material capable of varying in quantity and quality. We must cease to think that it can be made more active by stimulants, or be kept up by whips, whether ponderable as alcohol, æther, ammonia; or imponderable as heat, light, electricity, friction; or that it can be made less active, or let down or untoned by excessive use or by withdrawal of the stimulants.

When any excess of motion takes place, we must have an answer where the equivalent of that motion comes from, and whither it will go. Our ponderable and imponderable whips and stimulants are bound up with the matter, and produce more active motion according to the latent energy which the matter itself possesses.

When any want of motion occurs, we must ask whence the deficiency of motion comes. Is there a want of matter possessing latent energy? or is there increased resistance to the conversion of latent energy into active motion?

It may be said that this is only a verbal alteration; but it is, in truth, an alteration in the foundation on which all our knowledge is based.

It is an alteration which represents the direction in which science is advancing; and it is an alteration which not only marks the advance, but renders further progress more certain and more easy.

I will take the first and greatest question in pathology to illustrate the importance of the change in our views regarding the union and conservation of ponderable matter and force.

Inflammation, according to our unchanged ideas, is an increase of heat, redness, swelling, and pain, caused by an excess amounting to tumult in the nutritive changes in the inflamed part.

The dilatation of the smallest arteries and veins; the retardation of the current of the blood; the disappearance of the layer of serum which lines the vessels; the piling up of the colourless globules in the veins; the stoppage in the capillaries,—all these are produced by a direct or reflex paralysis of the contractile textures of the small arteries and veins. (Cohnheim, *Virchow's Archiv*, Sept. 1867.)

In other words, excess of nutrition in the inflamed part is considered to arise from a loss of energy.

This is directly opposed to the principle of the conservation of energy. Let us apply this principle to inflammation in parts free from blood-vessels and nerves, as the cartilage and cornea, where, at least, no question of a direct or reflex paralysis of the blood-vessels can arise.

In the non-vascular textures, the increased action is limited to increased heat and nutrition. These two forces are directly related, so that a certain quantity of heat can produce a certain quantity of nutrition, and *vice versâ;* but neither can arise from nothing. Either requires some other mechanical, chemical, electrical, or thermal motion to be given to the matter in the inflamed part, to increase the healthy motions which are always there.

This increased motion may come from without by injuries and accidents, or from within by diffusion of active substances into the cornea and cartilage. According to the amount of increase of motion, will be the amount of inflammation. In the vascular textures, the increased motions of oxidation and nutrition outside the vessels act directly on the contents of the nearest vessels, and, from the increased action between the vessels and textures, the dilatation of the vessels, the disappearance of the serous layer, and the heaping up of the red and white corpuscles arise.

The principle of the conservation of energy requires that increased oxidation and nutrition in the inflamed part cannot arise except from an equivalent quantity of some form of motion, or some form of potential energy reaching the part.

The same principle also requires that the inflammation must continue until the increased motion passes off in heat, or in forming pus, or in some other form of motion or nutrition. Thereby the increased motions subside to the amount which is called health.

The doctrine of the conservation of energy and the inseparability of matter and force will lead to an entire change not only in physiology and pathology, but also in that most practical part of medicine, therapeutics.

At present, our knowledge is very confused and uncertain as to how and where medicines act. We almost believe that our medicines have the power not only of creating, but of annihilating force; and we almost think that they are able to select the part on which they will act, whilst they leave other parts of the body entirely free from their presence.

From our want of knowledge how and where the effects are produced, the greatest possible difference of opinion exists as to the action and work of all the different classes of medicines—stimulants, sedatives, tonics, alteratives, specifics, evacuants.

We are almost led to say that stimulants create force, because they increase contractility and sensibility; that sedatives destroy force, because they diminish contractility and sensibility; that tonics create force, and specifics destroy force, and alteratives change force.

But the law of the conservation of energy requires us to believe that no food and no medicine can cause the creation or the destruction of the slightest particle of energy. The amount of conversion of potential into actual energy may be made more or less. The conversion of one kind of motion into another kind may be diminished or increased, but no annihilation nor creation of force is possible.

We must cease to regard the so-called imponderable heat, or its negation cold; friction, or its negation rest; electricity; light, etc., as entities that can be increased or diminished as remedies; and we must begin to look at them as modes of motion of the particles of matter in the different parts where they act. These modes of motion, by their correlations with the motions of oxidation and nutrition which are going on in the affected part, determine whether more motion or less motion of matter should result.

The medicines which are taken into the body have the same incapability as food to create or annihilate force; but they possess chemical energies by which, wherever they go, they take part in the motions of oxidation and nutrition which are going on there; and, according to their chemical properties, they add to the motions, or increase the resistance to the motions, that constitute disease.

The questions, then, which must be answered before we can obtain clear ideas of the actions of medicines in the body are: 1. What are the different motions which occur in the body? and how are these different motions related to one another? and 2. How do different agents or medicines increase or diminish these different motions which occur in the different organs and textures?

Assuming that any energy which shows itself in any of the motions of the body must arise from some other form of energy, and that this must ultimately be traced up to the chemical energy which enters with the food and air, then the action of medicines in increasing or diminishing or altering the action of oxygen in the different textures must be at least one of the most important questions in therapeutics.

Let me for a moment take the most directly chemical of all medicines, alkalies and acids, and let me try to show you that they are capable of taking a part in oxidation; and, from this experiment regarding the actions of acids and alkalies out of the body, perhaps you will be inclined to agree with me that they are capable of playing opposite parts in the oxidation which is going on everywhere in the body.

I have here an organic matter, grape-sugar, and oxygen in the oxide of copper, precipitated from the sulphate by alkali, which I can bring into contact with each particle of the organic matter. In each of these glasses, the same amount of sugar and oxygen and alkali is present. I will leave one glass as a standard of comparison regarding the rate of oxidation of the grape-sugar. To the other glasses I will add different amounts of alkali and acid; and even in a few minutes, without raising the fluids to the temperature of the body, you will see that the most alkaline is most rapidly oxidised, and that the least alkaline or acid fluid will have the oxidation retarded or altogether stopped.

That alkalies and acids have the same action in the body, and that they can promote or retard oxidation, must be determined by direct experiments; but meanwhile these views will lead us to think more of the alkalescence of the blood, and of the extent of the evil that acidity may occasion, even if we do not use sugar and acids more freely in inflammations and fevers, and urge our alkalies with greater perseverance where fatty and gouty thickenings and deposits exist.

If time permitted, I might here mention the wonderful experiments on the production of acute fatty disease of the liver, spleen, heart, etc., by phosphorus-poison. The formation of phosphoric acid in the textures probably is the direct cause of the accumulation of fat by arrest of chemical action.

Inseparable from the action of oxygen in the system are the other correlated actions—heat, electricity, nutrition, contractility, and sensibility. These must increase or diminish as the action of oxygen increases or diminishes; and the questions which arise as to the mode and extent of the convertibility of any one of these motions into other modes of motion have to be determined by further investigation.

Stimulants, tonics, and evacuants, may, perhaps, not only take part directly in the motions of any part, but they may also promote or retard the conversion of one motion into other motions.

Specifics and alteratives may, directly as well as indirectly, change the motions in the system.

And sedatives and narcotics may have the same double action in retarding or stopping the motions that take place.

This view will almost lead us to consider all medicines as alteratives, and, if so, we may perhaps place stimulant and sedative medicines at the two extremes of the alterative actions; the stimulants giving rise to the greatest increase of motion, and the sedatives allowing the least motion or the nearest approach to rest. Hence rest and motion may be taken as the two great aims of therapeutical actions.

The two substances which perhaps I might name as best showing these opposite actions, are laughing gas, NO^2, which, in small quantity, rapidly increases involuntary and voluntary motion; and prussic acid, HCN, which almost immediately stops all conversion of energy in the heart, diaphragm, and other respiratory muscles, by paralysing the excito-motory ganglia in the heart itself, and by the same action on the eighth pair.*

These are some of the questions which are opened to therapeutical inquiry by the doctrine of the conservation of energy and the inseparability of matter and force.

When these questions can be answered, most

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* Some late experiments of Dr. Preyer, of Bonn, show that when an animal is made for some time to breathe oxygen, and then is given a small poisonous dose of prussic acid, the poison takes no effect whatever. (*Die Blausäure*, W. Preyer, M.D., Bonn, p. 67.)

"Prussic acid, he says, acts as if it suddenly robbed the blood of oxygen. If we were able to take away from an animal all the oxygen of its blood in a few seconds, then, without the least doubt, we should have a perfect representation of poisoning by prussic acid." P. 69.

of the differences of opinion on the action of medicines will disappear.

Perhaps we shall ultimately be able to estimate the increase or diminution of any one motion which, by affecting all other motions in a part or in the whole body, constitutes disease. When the disease arises from increased action, we shall restore that normal quantity and quality of motion in the body on which the health depends, by decreasing the motion or adding to the resistance to conversion; and, when the disease arises from diminished action, we shall attain the same result by increasing the motion or lessening the resistance to conversion.

With regard to the question where medicines act, the idea of the inseparability of matter and force will probably lead to far broader views than we now hold.

For want of more definite knowledge, we are almost tempted to assume that medicines have an elective affinity which determines the place where they will act.

If, however, it can be shown that most medicines pass into every texture of the body, and carry with the matter whatever force they possess, then it follows that our present ideas of the local action of medicines are far too restricted.

The experiments which I have published in the *Proceedings of the Royal Society*, June 15th, 1865,* on the rate of passage of lithium into and out of the vascular and non-vascular textures, show the rate at which chloride of lithium passes into the textures through the stomach of the guinea-pig:—

1½	grain in	3	days	plenty found everywhere.	
3	grains in	15	minutes	everywhere except in the lens.	
,,	,,	30	,,	,,	,,
,,	,,	30	,,	,,	traces in the lens.
,,	,,	30	,,	,,	outer part of the lens.
,,	,,	60	,,	,,	,,
,,	,,	60	,,	,,	except in the lens.
,,	,,	2¼	hours	,,	and throughout the lens.
,,	,,	4	,,	,,	,,
,,	,,	8	,,	,,	,,
,,	,,	24	,,	,,	,,
,,	,,	26	,,	,,	,,
¼	,,	5¼	,,	,,	except in the lens.

Hence, in four minutes, in a guinea-pig, after three grains of chloride of lithium it may be found everywhere by the spectrum-analysis; and this quantity was found to give traces in the lens of the eye after thirty-three days; and it appeared in the urine for thirty more

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* See Appendix III.

days, the guinea-pig probably re-dosing itself from its own skin.

The following table shows the rate of passage of carbonate of lithia into and out of cataracts in men :—

20 grains of carbonate of lithia were taken

25 minutes		before the operation	—No trace of lithium was found in the cataract.
2½ hours		”	—Lithium in the watery extract of the cataract.
3½ ”		”	—Lithium in each particle.
4 ”		”	” ”
4¼ ”		”	” ”
4½ ”		”	” ”
5 ”	(old man)	”	” ”
5 ”		”	” ”
7 ”		”	” ”
4 days	spontaneous soft cataract, 25 to 30	”	—Traces in alcoholic extract of ash.
7 ”		”	—In alcoholic extract not the slightest trace of lithium.
7 ”	5 hours	”	” ”
7 ”		”	—Slightest trace in the alcoholic extract.

In man after taking five grains, lithium was found in the urine in from ten to twenty minutes, and continued to pass out for eight days. After a twenty-grain dose of carbonate of lithia, the crystalline lens showed lithia in two and a half hours. After seven days none was found.

Sulphate of thallium was also shown, by spectrum-analysis, to pass into all the vascular and non-vascular textures, and even sulphate of silver was traced in almost every texture.

In the *Proceedings of the Royal Society*, April 12, 1866, Dr. Dupré and I further showed that four grains of sulphate of quinine passed in fifteen minutes into every texture of a guinea-pig, and was detectable for forty-eight hours.

If we had sufficiently delicate modes of detecting other substances, it can hardly be doubted that they would follow the same law, and be found to pass rapidly into every texture.

With the matter, whatever energy that matter possesses must pass also; and wherever the matter finds motions going on in which it can take part, there the medicine will act. It may be present in equal quantity elsewhere, and yet give no sign of its presence, because no increase or diminution of the motions in those parts may occur. Let me illustrate this for an instant by supposing a shower or sheet of any substance fell in every part of this room—for example, alcohol. In many places it would apparently not be present, no action would occur. Where there was varnish, it would act chemically on the resin; where the fire was burning, it would burn; and in our eyes it would act chemically, increasing the actions there.

From these glimpses into the physiology, pathology, and therapeutics of the future, each of which might well have been the subject of many lectures, it is evident that a great change will take place in our ideas in the biological sciences when we follow the progress of knowledge in the abiological sciences.

As the abiological sciences have passed, or are passing, through three different stages of perfect and imperfect separation and perfect union of ideas regarding ponderable matter and force, so it is reasonable to expect that ultimately we shall pass through the same three stages of ideas regarding the union of matter and force in the biological sciences; and, if so, we shall arrive at an idea of life which will be based on the perfect union of ponderable matter and force.

If biology be disposed to profit by the progress of the other sciences, we must expect that all we shall know of vital force will be got by looking for its effect as the most peculiar of all the motions of which matter is capable. This must be able to arise from other motions and be able to give rise to other motions; and its cause must be altogether as incapable of destruction and creation as the abiological forces themselves.

It may well be said, what can be the nature of this motion? how can it be understood? The answer must be the same as that which we must give even now regarding the nature of the motion of the simple or compound atoms of matter which we call electricity or magnetism, or even light or heat.

How many among us at present have a clear idea of the motion of the simple molecules of matter which we call heat, or even gravitation and crystallisation? Whilst we are unable to grasp the marvellously complex double polar motion of compound molecules which constitutes electricity, can we expect to be able to form an idea of the most complex of all the motions of matter from which the mind turns away to the image of some aërial or ætherial spirit, with large wings and larger powers; or rests satisfied with an imponderable gaseous or liquid matter diffused through living liquids, or temporarily attached to more solid granular matter?

Notwithstanding our ignorance, let us attempt to gain knowledge by following in the footsteps of the abiological sciences.

Whatever form of motion or tension proceeds from the body, let us regard it not as created or destroyed, but as the representative and equivalent of that energy which went into the body.

Let us consider that the balance of in-going and out-coming energy of the body is exactly comparable with the balance of the in-going and out-coming ponderable matter; and let us remember that these balances are inseparably dependent the one on the other.

The views which I have endeavoured to bring before you will be at once condemned as materialism by those who think that they know more of matter than as the fixed abode of force, and more of force than as that which gives energy to matter.

Those who are so ready to use the word materialism as a reproach, should remember that they can give no definition of matter which does not involve the definition of force, and that they know nothing whatever of matter except as that which can exert or resist force; and they should not forget that there is no proof whatever that force is something without weight which the Creator has made separably to adhere to matter.

Those also who are tempted to oppose spiritualism to materialism must remember that, in the progress of science, this something without weight, this fluid, air, or æther, has ceased to be regarded as the actual force itself, but is considered only to be an imponderable matter to which the force is inseparably united.

From this point of view, the question between materialism and spiritualism is, in fact, only a question between ponderable and imponderable materialism. Hence the scientific spiritualist of the present day differs from the materialist of the present day only as far as imponderable differs from ponderable matter.

The spiritualist who still holds the primitive idea of the perfect separation of matter and force may find full occupation for his reason in weighing the evidence on which his belief or internal conviction rests; but he must leave the investigation of the foundations of natural knowledge to those who can see no reason for faith in witches, ghosts, transmutations, and transmigrations.

There are some who think little of scientific truth, but, comparatively speaking, care much to recognise the Almighty Will as the primary cause of all things.

Such persons will find that this power and will are shown in the inseparable union of ponderable matter and force quite as much as if he had willed to make them completely separable.

We, who search for truth above all things, are compelled by our belief in the inseparability of matter and force in the abiological sciences to work out the inquiry how far this inseparability holds true in the biological sciences also.

Our ideas of the grandeur, the unity, and the power of the first cause will surely not be lessened if it can be shown that one law of the union of force and matter and of the conservation of energy obtains throughout the organic as well as throughout the inorganic creation.

APPENDICES

APPENDICES.

APPENDIX I.

APPENDIX II.

APPENDIX III.

APPENDIX I.

ON THE TIME REQUIRED FOR THE TRANSMISSION OF VOLITION AND SENSATION THROUGH THE NERVES.

A LECTURE GIVEN AT THE ROYAL INSTITUTION,

BY EMIL DU BOIS-REYMOND,
Professor of Physiology in the University of Berlin.

Introduction.—The speaker first pointed out a certain similarity of action between the nerves and telegraph-wires. Just as little as telegraph-wires, do the nerves betray by any external symptom that any or what news is speeding along them; and, like those wires, in order to be fit for service, they must be entire. But, unlike those wires, they do not, once cut, recover their conducting power when their ends are caused to meet again; in fact, every injury by which the organic structure of the nerve is impaired, such as bruising it between the blades of a forceps or by a ligature, or burning it, or corroding it by some chemical substance, will stop the transmission of either the influence of the will upon tis central, or of the impressions of external objects upon its peripheric end.

This was illustrated by placing the sciatic nerve of a frog, still attached to the gastrocnemius muscle, on three electrodes, A, B, C—A being the remotest from, and C the nearest to, the muscle. A being connected with one end of a self-acting induction apparatus, and either B or C with the other end, a strong tetanus of the muscle ensued, and was rendered visible by the raising of a little mica flag;* but after a ligature had been tightened around the nerve between B and C, which was simply done by pressing down a lever,† the tetanus was observed to arise only on making the connection with C, because only then a portion of the nerve between the ligature and the muscle was stimulated, whereas, on making the connection with B, the ligature intercepted the effect produced by the stimulation of the portion A B.

These facts, the speaker said, are calculated to impress us with a notion of something moving along the nerves in the act of volition and in that of sensation, which, to preclude any hypothetical view regarding its nature, we will term the *nervous agent*. As this agent for its conveyance requires an un-

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* See the description and drawing of the apparatus in *Beschreibung einiger Vorrichtungen und Versuchsweisen zu elektrophysiologischen Zwecken.* Von E. du Bois-Reymond. Abhandlungen der K. Akademie der Wissenschaften zu Berlin, 1862, S. 141.

† The apparatus used is described and figured in *Untersuchungen über thierische Elektricität.* Von E. du Bois-Reymond. Bd. ii., Abth. i. Berlin, 1849. S. 341; Taf. iii., Fig. 109, 110.

injured state of the nerve, being checked by so gross an obstacle as a cut or a ligature, it must necessarily be something material, not, however, the electricity such as it moves along a telegraph-wire, for this would readily overleap those impediments. But, whatever the nervous agent may be, it obviously must proceed in the nerves with a certain definite velocity. In other words, however great this velocity may be, a certain definite time will be required for the messages of the brain to reach the muscles and for those of the senses to reach the brain.

In common life, it is true, we never notice any phenomenon indicative of such a delay in the transmission of despatches in our nerves. Certainly our limbs do not instantaneously carry out the orders of our will; but this is rather owing to the circumstance of time being required for motion. On the other hand, we fancy we see the light, we hear the sound, we feel the prick on the toes as well as on the cheek at the very instant the corresponding organs of the senses have been affected. But a little reflection shows that this is altogether a delusion; in fact, if we only had one sense, an indefinite time might elapse between its organ being affected and the sensation taking place in the brain, without our perceiving it; we simply should always be so much behind the real time, just as we are when listening to distant music, or when looking at the stars. The same thing would also happen if we had two senses, both equally slow in working. We should only perceive that something was wrong, if the two senses had a different rate of working, and if the difference of time resulting therefrom for the perception of messages conveyed by the two senses were large enough to become apparent, as is the case when we see the flash of the gun before hearing its report. Now we cannot well distinguish intervals of time smaller than one-tenth of a second at the most; so that within that limit any irregularity in the working of our organs of sense may occur, from any cause whatever—hence from a delay of the messages in the nerves—without our becoming aware of it.

The problem thus suggests itself, to ascertain whether any perceptible time is required for the transmission of volition and sensation through the nerves, and if so, what is the rate of propagation of the nervous agent.

Historical Remarks.—This problem is by no means a new one; for a hundred and fifty years it has engaged the attention of physiologists, and many an adventurous hypothesis has been broached in order to approach to at least a pretence of a solution.

One of the early Iatro-mathematicians preposterously conceived that the velocity of the nervous agent ought to bear the same proportion to that of the blood in the aorta as the width of the aorta to that of the nerve-tubes, and he thus inferred the velocity of the former to be one hundred and twenty millions of miles in one second, rather more than six hundred times the velocity of light.*

Haller himself tried, in reading the Æneid aloud, how many letters he could pronounce in one minute.

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* Haller, *Elementa Physiologiæ Corporis humani.* Tom. iv. Lausann, 1762. 4°. P. 372.

Finding that they were fifteen hundred, among which the R, according to him, requires for its formation ten successive contractions of the M. styloglossus, he states that in one minute a muscle may contract and relax fifteen thousand times, and as the relaxing lasts as long as the contracting, each contraction would have lasted only $\frac{1}{30000}$ of a minute, or $\frac{1}{500}$ of a second. Hence Haller argues that the nervous agent requires $\frac{1}{500}$ of a second for travelling from the brain to the M. styloglossus, say a distance of about four inches, which makes about 160 feet in one second. Now this result is not a little remarkable. In Haller's reasoning every single step is erroneous, and the whole rests on a perfectly absurd basis. Nevertheless, the result to which Haller has thus been led wonderfully coincides with that which has recently been arrived at by the methods which it is the object of this lecture to explain; so that in this case the Æneid really has proved a book of oracles.*

John Müller, of Berlin, hardly seventeen years ago, used in his lectures to dwell upon the apparent impossibility of ever solving the problem under consideration, on account of the enormous rate of propagation, comparable to that of light and electricity, which he ascribed to the nervous agent, while the small compass of the animal body did not offer sufficient range for its measurement.†

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* Haller, *Elementa Physiologiæ Corporis humani.* Tom. iv. Lausann, 1762. 4°. Pp. 373, 483.

† *Handbuch der Physiologie des Menschen, u. s. w.* Bd. i., 4 Aufl. Coblenz, 1844. S. 581.

These historical details, perhaps, will not be deemed superfluous, inasmuch as they are calculated to bring out more fully the beauty and the high scientific value of the following researches.

Description of M. Pouillet's Chronoscope.—More than twenty years ago, M. Pouillet suggested a very ingenious plan for measuring the velocity of projectiles. If an electric current flows constantly through the coil of a galvanometer, the needle is deflected to an amount which depends upon the intensity of the current, and upon the sensitiveness of the galvanometer. But if the current be sent through the coil only for a time so short that it vanishes when compared to the duration of one oscillation of the needle, things happen differently. The needle then receives as it were a single impulse, yielding to which it slowly recedes, till its velocity has been annihilated by the magnetic force of the earth, which draws it back to zero. And the initial velocity imparted to the needle by the current, provided this be of constant intensity, will be proportional to its duration, so that from the velocity, which can be calculated from the deflection of the needle, the duration of the current may be inferred. The galvanometer is thus transformed into a *chronoscope*, which can be used for measuring the duration of rapidly transient processes, whenever there is a possibility of making the beginning and the end of the process coincide with the beginning and the end of the *chronoscopic current*,—for so we will style the current, which, by its action on the galvanometer, becomes an indicator of time. And similarly we will style *chronoscopic circuit*, the circuit of the chronoscopic current.

If, *e. g.*, the velocity of a bullet, within the very barrel of a gun, were to be measured, the chronoscopic circuit, in addition to the battery and the galvanometer, should comprise a wire stretched out just before the muzzle of the gun, the cock, which in some way or other should be insulated from the gun, and lastly the gun itself. Then at the moment when the cock strikes the percussion-cap, the circuit is made and remains so till the bullet tears the wire; and so the current will only have lasted during the time required for the explosion of the percussion-cap, for that of the gunpowder in the gun, and for the moving of the bullet along the barrel. This time has been found to be from $\frac{1}{150}$ to $\frac{1}{140}$ of a second. By repeating the same experiment with the wire or a net of wires at any greater distance from the muzzle, and by taking the difference of the times required in both cases, the time elapsed during the flight of the bullet through the space comprised between the two positions of the wire can be ascertained.*

Application of this Method to our Problem by Professor Helmholtz.—The same method was, a few years later (1850), successfully applied by Professor Helmholtz to the solution of our problem.

Suppose that the chronoscopic current, when its circuit is made, causes the muscle to contract by stimulating a motor nerve at a point A. It will be easy to arrange things so as to cause the muscle by

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* *Comptes Rendus*, &c., 1844, t. xix., p. 1384.

its contraction to break the circuit. The current will then have lasted the time necessary for the transmission of the nervous agent from the point A of the nerve to the muscle, and, moreover, that necessary for the muscular contraction to break the circuit. But by repeating the experiment, with the sole difference that the chronoscopic current is made to act upon a point B of the nerve farther from the muscle than A, and by taking the difference of the times required in both cases, the time which elapses while the nervous agent is travelling from point B to point A will be found.

Fig. 1.

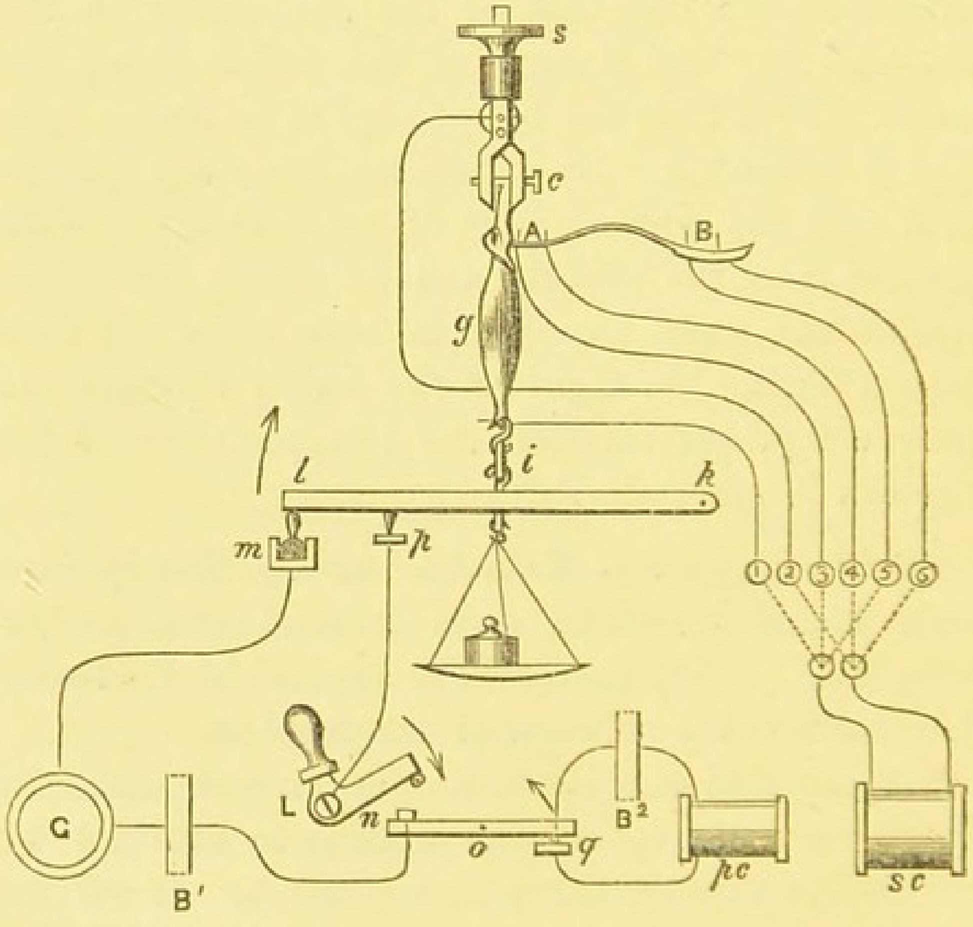

Fig. 1 shows the experiment, not exactly as made by Professor Helmholtz, but with some slight modifications, that the speaker has introduced for the sake of convenience. *g* is the gastrocnemius muscle of a frog, fastened by the thigh-bone in the clamp *c*, which can be raised or lowered by the screw *s*. Through the *tendo-Achillis* a hook is thrust, and to this, by an insulating piece *i*, a brass lever *l k* is attached, turning on the axis *k*, and, near its end *l*, supported by a platinum point resting on a platinum plate *p*. Just underneath the muscle, a scale-pan is suspended from the lever, on which any suitable weight may be placed. At the end *l* of the lever an amalgamated copper point dips into a mercury cup *m*. G is the galvanometer-coil; and it is hardly necessary to mention that the readings are made with mirror, telescope, and scale. B^1 is the battery belonging to the chronoscopic circuit. This circuit is formed by B^1, G, *m*, *p*, L, and the place of contact *n*, where for the present it is interrupted. For several reasons, which it would take too long here to explain, the chronoscopic current cannot be employed for directly stimulating the nerve by its beginning, and this has to be done indirectly, in the following way:

n o q is an insulating lever turning on the axis *o*, and bearing at *n* a platinum plate connected with B^1. This plate corresponds to a platinum point at the end of the brass lever L, which in its turn is connected with *p*, so that by pressing down L, the chronoscopic circuit is made. But by pressing down the extremity *n* of the lever *n o q*, its extremity *q* is simultaneously raised, and thus another circuit is broken. This circuit comprises a battery B^2, and the primary coil *p c* of an induction apparatus, from whose secondary coil *s c* wires extend to the part of the nerve which is to be stimulated, or to the muscle. No perceptible time elapses between the breaking of the primary circuit and the generation of the induced current, and the duration of the latter does not exceed a few ten-thousandths of a second. Hence the stimulation of the nerve can be assumed to happen at the very instant the chronoscopic circuit is made.

By means of the screw *s*, it is easy to make the muscle support the lever *l k* so that the platinum point just rests on the plate *p*. This is done by lowering the screw, till on percussing the lever above the platinum point no clattering is heard. If now the tension of the muscle be ever so little increased, the lever will be lifted, and the chronoscopic circuit broken at *p*.

After the contraction is over, as the lever sinks back into its original position, the chronoscopic circuit would be made again, and the experiment spoilt in consequence of the new and overpowering action exercised on the needle, unless some measure were taken to prevent it. This very serious difficulty has been met by Professor Helmholtz with singular felicity. The mercury cup into which the amalgamated copper point dips is lowered before the experiment, till by capillary attraction the mercury is drawn up in a cone connected with the point by a thread. The slightest upward motion of the point then will cause the mercury thread to break, after which the liquid metal speedily reassumes its spheroidal surface. So that when the amalgamated point comes down again, it cannot reach the mercury, and the chronoscopic circuit remains open.

In order to obtain a clear insight into the conditions of the experiment, it should first be made by stimulating, not the nerve, but the muscle itself. For this purpose, the ends of the secondary coil should be united with the wires 1 and 2 in the diagram. As, on doing so, all the parts of the muscle are acted upon simultaneously at the very instant the chronoscopic circuit is made, no delay in the nerves can occur; nevertheless, a deflection of the needle is thus obtained, which shows that after the stimulation there is an interval of time of about ·01 of a second, during which the tension of the muscle still remains unaltered. This interval has been styled by Professor Helmholtz the *stage of latent stimulation*.

If, after having made the muscle support the lever so that it just rests on the plate *p*, a weight be placed upon the scale-pan, the deflection obtained on stimulating the muscle as before is increased, and, up to a certain limit, the more so the heavier the weight. It thus appears that after the stage of latent stimulation is over, the muscle does not at once acquire its whole energy, but that its tension (at equal length) gradually increases, and reaches its maximum after about ·05 of a second.

This result (which the speaker illustrated by two experiments, one without, and the other with an additional charge on the scale-pan) is in itself of considerable interest, as it shows that the difference which, up to the time of Professor Helmholtz's researches, had been admitted between the mode of contracting of the striped and of the unstriped muscles is only one of degree. Of the latter class of muscles, in contradistinction to the first class, it was said that their contraction did not immediately follow stimulation, and that their energy rose, and subsided again, gradually. But it is now obvious that all this holds equally with the striped muscles, only that the whole process here lasts but a few hundredths of a second, whereas with the unstriped muscles every stage of the contraction may take up as many seconds, so as to be easily perceptible without artificial means.

The values obtained in repeated trials for the duration of the latent stimulation do not accord very well; but those which represent the time required by the muscle to raise an additional weight remain almost identical as long as the muscle does not become exhausted, provided the stimulation be capable of producing the maximum of contraction which can be called forth by instantaneous stimulation. This also happens when, instead of stimulating the muscle itself, the induced current, in successive trials, is made to act with sufficient energy upon always the same part of the nerve, only in this case the time of latent stimulation is found to be a little longer than in the case of the muscle itself being stimulated. But on making the current pass alternately through the part A of the nerve nearer to the muscle, by means of the wires 3 and 4 in the diagram, and through the part B farther from the muscle, by means of the wires 5 and 6, two sets of observations are obtained, the corresponding figures of which differ from each other by a small but constant quantity, independent, moreover, of the additional charge placed upon the scale-pan. This difference obviously indicates the time required by the nervous agent for travelling from B to A, and the distance between A and B being known, the rate of propagation of the stimulation or the velocity of the nervous agent can easily be calculated.

This velocity, in Professor Helmholtz's experiments, has been found to be 26·4 mètres (86·6 feet) in one second.* The reason why on stimulating the nerve the time of latent stimulation appears a little longer than on stimulating the muscle itself, is now also evident. It is owing to the time which elapses during the passage of the nervous agent from the stimulated part of the nerve-tubes to their termination in the muscle.

Professor Helmholtz's Experiments with the Myographion.—Soon after Professor Helmholtz had thus, for the first time, succeeded in determining the velocity of the nervous agent, he devised another method for obtaining the same result in a manner more simple and more adapted to a variety of purposes.

As before, the gastrocnemius muscle is caused to lift a lever by its contraction, but this time with the view of making the muscle itself register the

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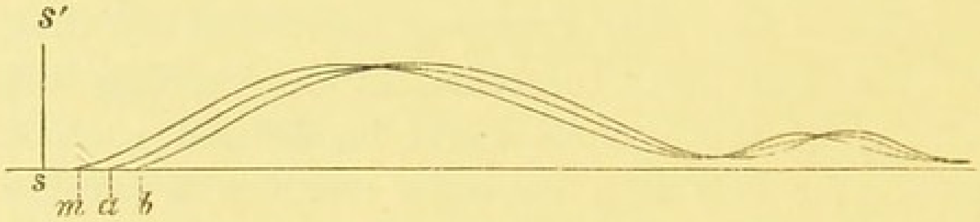
* Joh. Müller's *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin.* Berlin, 1850. S. 276.

beginning, and the successive stages, of the contraction; wherefore the apparatus employed is termed a *myographion*. To this end, opposite a steel point or style suspended from the lever is placed a rotating cylinder of glass, blackened over a lamp. The cylinder is moved by clockwork at an increasing rate, and, when it has acquired the proper speed, the centrifugal force developed releases a piece of mechanism which breaks the primary circuit of the induction apparatus connected with the muscle or nerve. In consequence of a very ingenious arrangement, which will hereafter be explained, this always happens when the cylinder is in exactly the same position to the style; so that always the same point of the datum-line traced by the style during the quiescent state of the muscle corresponds to the instant of stimulation.

The contracting muscle thus traces a curve on the cylinder, like those in Fig. 2. The point *s* of the datum-line marks the instant of stimulation.

FIG. 2.



The curve does not start from this point, but a little later, from *m*, the distance *s m* corresponding to the stage of latent stimulation, if we suppose the muscle to have been acted upon directly by the current. The muscle then, slowly and gradually, begins to act, in due time reaches a climax of energy, and then again gradually relaxes. The results obtained by M. Pouillet's method regarding the successive stages of contraction are thus confirmed by the graphic method. The curve traced by the contracting muscle of course becomes the more stretched the quicker the cylinder rotates, but, at an equal rate of rotation, the curves traced successively by the same muscle, stimulated by a current capable of giving rise to the maximum of contraction, coincide so as almost entirely to coalesce, so long as the muscle does not become exhausted. This also happens when in repeated trials the current, instead of on the muscle itself, is made to act with sufficient energy upon always the same part of the nerve, say the part A; only all the curves thus obtained are found to start a little further from *s* on the datum-line, say at *a*.

But, on stimulating the nerve alternately at a place A nearer to, and B farther from, the muscle, the curves obtained no longer coincide. They separate, and the curves traced on stimulating B will again be found to lie farther off on the datum-line, starting at *b* instead of at *a*, and keeping everywhere the same horizontal distance *a b* from the curve traced when A is stimulated.

This horizontal distance of the two curves, or sets of curves, evidently corresponds to the time the nervous agent has spent in travelling along the nerve from B to A. It is, in fact, the displacement of the surface of the cylinder which, according to its angular velocity, has occurred during the same time. Determining this angular velocity by means of an indicator, the velocity of the nervous agent was found by Professor Helmholtz to be 27·25 mètres (89·4 feet) in one second, which agrees very well with the figure formerly obtained by M. Pouillet's method.* It is hardly necessary to mention that the difference *s a—s m* corresponds to the time spent by the nervous agent in travelling from A to the termination of the nerve-tubes in the muscle.

Different kinds of Myographion proposed; description of the Apparatus used by the speaker on the present occasion.—The myographion, as it was originally designed by Professor Helmholtz, is a rather complicated and expensive apparatus. Various modifications have been proposed in order to simplify it. For the clockwork moving the cylinder, Thiry has substituted a reaction-engine moved by air, like that in M. Foucault's apparatus for comparing the velocity of light in air and in water. The rotation of the cylinder in this myographion is uniform, and its rate is ascertained by the pitch of a siren connected with it.† Harless and Professor Fick altogether abandoned the rotating scheme, which, in fact, involves great and, perhaps, unnecessary difficulties, and for the rotating cylinder substituted a glass plate moving in its own plane. Harless allowed the plate to drop in an apparatus constructed upon the principle of Atwood's machine, and thus obtained an uniform velocity, which could easily be determined

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* Joh. Müller's *Archiv, u. s. w.*, 1852. S. 199.

† Henle und Pfeuffer, *Zeitschrift für rationelle Medicin*, 3. R., B. xxi., 1864, S. 300.

theoretically.* Professor Fick attached the plate to a heavy pendulum, and although the velocity then was no longer uniform, yet the law of its variation was known.†

The speaker himself had formerly devoted much attention to the improvement of the rotating myographion, and had suggested some alterations of it, which, executed by that most skilful instrument-maker, M. Sauerwald, of Berlin, answer remarkably well, and have done good service in some very important researches, hereafter to be mentioned.‡ For the present occasion, however, he contrived a new myographion which in simplicity far exceeds every previous design, and which may be styled the *spring-myographion.* As in Harless's and in Professor Fick's myographion, in the spring-myographion the cylinder is replaced by a glass plate moving in its own plane. But instead of being impelled by gravity, the plate, on pulling a trigger, is, as it were, shot by a spiral spring along two horizontal steel wires which act as supports and guides. The curve is traced on the plate, after this has reached its greatest velocity, *i.e.*, after the spring has passed through its position of equilibrium. On account of the friction, however, the velocity of the plate cannot be taken as uniform, nor can the law of its variation and its amount at any given place

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* *Abhandlungen der K. Bayer. Akademie der Wiss.* II. Cl., Bd. ix., Abth. ii. München, 1862. S. 361.

† *Vierteljahrschrift der Naturforschenden Gesellschaft in Zürich*, 1862, S. 307.

‡ A. v. Bezold, *Untersuchungen über die elektrische Erregung der Nerven und Muskeln.* Leipzig, 1861. S. 85.

theoretically be ascertained with any degree of precision. But, if required, this could easily be done experimentally, by recurring to Dr. Thomas Young's original plan*, *viz.*, by causing a tuning-fork to record its vibrations upon the plate, together with the curve traced by the contraction of the muscle. On the present occasion this would be useless, as it is only intended to show that there is a larger interval between the instant of stimulation and the beginning of contraction, if a part of the nerve farther from, than if a part of it nearer to, the muscle be acted upon. For this purpose it is only requisite that the motion of the plate should always follow the same law and attain the same velocity, and that the point of the datum-line, corresponding to the instant of stimulation, should always be the same.

In the spring-myographion the latter condition is fulfilled in the same manner as in Professor Helmholtz's, and every other myographion. The frame carrying the plate has a projecting piece or tooth, which, just when the frame has reached its highest speed, strikes a lever, and thus breaks the primary circuit of the induction apparatus. This, of course, always happens in exactly the same position of the plate, whatever may be its velocity; so that in order to find the point of the datum-line corresponding to the instant of stimulation, it is only necessary to move the plate with the hand along its guides so slowly that the contraction of the muscle, instead of

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* *A Course of Lectures on Natural Philosophy and the Mechanical Arts.* London, 1807. Vol. i., p. 190.

a curve, traces a vertical line ($s\ s'$ in Fig. 2) corresponding to the two limbs of the curve, the ascending and the descending one, which have coalesced.

A very essential precaution in these, as well as in all experiments on nerves or muscles, is that these textures, especially the nerves, should be carefully preserved from becoming dry. This can be done by covering the part of the apparatus containing them with a shade, the inner walls of which are lined with moist bibulous paper; or, as was the case on the present occasion, by enclosing the nerve in a tube of vulcanite, in which it rests on two pairs of platinum electrodes, one as near to, the other as far from, the muscle as possible. This tube of vulcanite is also provided with another contrivance, which will be more fully mentioned presently.

The speaker now proceeded to show a complete experiment with the spring-myographion. After two curves had been traced, one by stimulating the nerve near to, the other by stimulating it far from, the muscle, the magnified image of the curves was exhibited to the audience by means of the electric light.

Conditions upon which the rate of Propagation of the Nervous Agent has been hitherto found to depend.— 1. The figures given above (see pages 104 and 110) for the rate of propagation of the nervous agent were obtained at a temperature of from 11° to 21° C. At a lower temperature, Professor Helmholtz found the velocity was greatly diminished.* This can easily

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* Joh. Müller's *Archiv, u. s. w.*, 1850, S. 358.

be shown by means of the tube of vulcanite belonging to the speaker's new myographion. The nerve, inside the tube and between the two pairs of electrodes, rests on a varnished copper-plate, which forms part of a small vessel, lodged in the wall of the vulcanite tube. Through this vessel water of any temperature may be passed. By thus cooling the nerve, a wider interval is obtained between the two curves; in fact, this artifice was put into practice in the above-mentioned experiment to make the result more striking. Plates were also exhibited with curves traced on them at a higher and a still lower temperature, thus showing that the horizontal distance of the two curves is in a measure proportional to the lowering of the temperature.

2. Dr. H. Munk, by delicate researches made in the speaker's laboratory, has succeeded in demonstrating that the velocity of the nervous agent is not the same in different parts of the same nerve. According to him, this velocity in the motor nerves increases as the nerve approaches the muscle.*

3. Professor v. Bezold, also in the speaker's laboratory, has undertaken to answer the question whether and how the velocity of the nervous agent may be altered by the *electrotonic state* of the nerve —as sixteen years ago the speaker ventured to term the remarkable condition into which he found the whole nerve thrown when any part of it is pervaded by an electric current; a condition making itself manifest by a most striking change in the electromo-

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* Reichert's und du Bois-Reymond's *Archiv. für Anatomie, u. s, w.*, 1860, S. 798.

tive action of the nerve,* and, as afterwards demonstrated chiefly by Professor Pflüger, also by a change in the excitability of its different points.† These changes, which only last as long as the current itself, are found to be the more considerable the nearer to the electrodes the nerves are examined. In addition to these facts, Professor v. Bezold found the velocity of the nervous agent to be diminished in the electrotonic state, and that also the more, the nearer to the electrodes the nerves were examined. These researches were made with the speaker's improved rotating myographion, alluded to above. (See p. 113.)‡

Rate of Transmission of Sensation in the Nerves and the Spinal Cord of Man.—All the preceding experiments were made upon the motor fibres in the sciatic nerve of the frog. Similar results, however, have been arrived at with regard to the nerves of sensation in the living body of man, in the following way.

An induced current is made to impinge alternately upon two different places on the skin, so as to cause just a slight sensation of pain. The two places should be such as are supplied, like the big toe and the inguinal region, for instance, with sensory fibres

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* *Untersuchungen über thierische Elektricität*, Bd. ii., Abth. i. Berlin, 1849, S. 289. *On Animal Electricity, &c.*, by H. Bence Jones. London, 1852. P. 174.

† *Untersuchungen über die Physiologie des Electrotonus.* Berlin, 1859.

‡ A. v. Bezold, *Untersuchungen über die elektrische Erregung der Nerven und Muskeln.* Leipzig, 1861.

emanating from neighbouring roots, but of very unequal length. The person experimented on is desired, as soon as he becomes aware of the shock, or *sensation-signal*, as it may be termed, to answer it by another signal, *volition-signal*, which generally consists in making or breaking a circuit at the instant when the sensation-signal is given; and means are provided for measuring the time, that we shall designate by T, which elapses between the two signals. The time T comprises, firstly, the time required for the transmission of sensation to the brain—for its perception there—for volition—for the transmission of volition to the muscles—for muscular contraction; secondly, T comprises the time lost in the purely mechanical and physical process of signaling. This last time, of course, depends upon the experimental method employed, and by suitable arrangement may even be reduced to nothing. Now, should the value of T, when the more distant place on the skin is stimulated, always, and by nearly the same amount, exceed its value when the nearer place is stimulated, then, as everything else in both experiments remains the same, the difference of time must obviously depend upon the unequal distance which in both cases the nervous agent has to travel from the place stimulated to the brain.

This scheme also, in the main, was suggested by Professor Helmholtz, as early as 1850;* the first correct results, however, were thus arrived at eleven

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* *Königsberger Naturwissenschaftliche Unterhaltungen.* Bd. ii., Hft. ii. 1851. S. 169.

years later by Dr. Hirsch, the able astronomer of Neuchâtel in Switzerland, by means of Hipp's chronoscope, which he so regulated as to obtain the first part of the time T, *i. e.*, that merely occupied by physiological processes, entirely free from the second.* For that physiological part of the time T, with reference to the part it plays in modern astronomical observation, he proposed the name of *physiological time.* The subject has since been taken up, and as regards physiology more fully treated by Dr. Schelske, of Berlin, whose experiments were made at the Utrecht Observatory, by means of the chronograph now in use among astronomers for registering their observations.

In this chronograph, as constructed by Krille, of Hamburg, there is again a rotating cylinder, on whose blackened surface two points or styles, *a* and *b*, mark a spiral track. Each of them is fixed to the keeper of an electro-magnet, and, when the keeper is attracted, deviates a little from its path, so as to trace a broken line. The circuit of the magnet A is alternately made and broken by the pendulum of a clock, in accordance wherewith the style *a* traces a broken line like *a a′* in Fig. 3, every horizontal segment of which corresponds to one second. The circuit of the magnet B contains the primary coil of the induction apparatus, whose secondary coil is connected with the skin of the person experimented on. Two *keys*, moreover, form part of this circuit: a *lever-key*, which, when pressed down, keeps the

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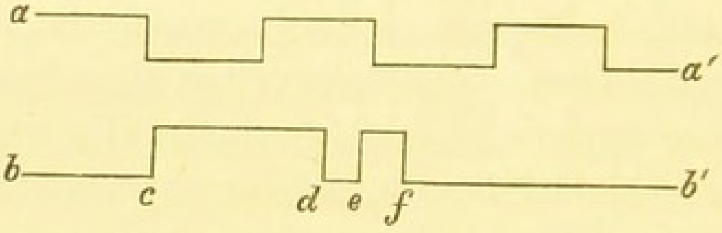
* Moleschott's *Untersuchungen zur Naturlehre des Menschen und der Thiere*, Bd. ix., S. 183.

circuit closed by establishing a bridge between two points of it; and a *spring-key*, which the person holds in his hand, ready, by it, to make the circuit again after it has been broken by the lever-key.

All the rest is very simple, and easily understood

Fig. 3.



when looking at the broken line *b b′* in Fig. 3, which is the record of an experiment as written down by the style *b*. At *c*, the lever-key has been pressed down in order to prepare the experiment. At *d*, the circuit of the magnet B, and of the primary coil of the induction apparatus, has been broken, and the induction current passed through the skin, all by again lifting the lever-key. At *e*, the person, having perceived the shock, has made the circuit again by means of the spring-key, and at *f* has broken it once more, in order to render another experiment possible. So that the distance *d e* here would correspond to the time we have designated by T, and would be found to be greater or smaller according to the distance the nervous agent had to travel from the stimulated place on the skin to the brain.

The velocity of the nervous agent in the nerves of common sensation, in the living body of man, has thus been found by Dr. Schelske to be 29·6 mètres (97·1 feet) in one second, which is only very little more than the result obtained for the motor nerves of the frog. (See pages 109 and 112.)*

By similar experiments, Dr. Schelske has ascertained that the transmission of sensation through the spinal cord of man takes place at very nearly the same rate as in the nerves. This is the more remarkable, as in other respects the nerve-tubes undergo a material change on entering the spinal cord, where, according to Professor van Deen, of Groningen, and others, they are no longer capable of stimulation by electricity, chemical substances, mechanical injuries, &c.

Considerations suggested by the foregoing Results.—The following table affords an opportunity of comparing the velocity of the nervous agent, as it has been established by the foregoing researches, with that of several other agents, or bodies in motion, from which comparison some interesting conclusions may be drawn :—

VELOCITY OF	Mètres in one second.
Electricity in Mr. Wheatstone's experiment	464,000,000
Light	300,000,000
Sound in Iron	3,485
,, Water	1,435
,, Air	332
Shooting Star	64,380
Earth in Orbit round Sun . .	30,800
Earth's Surface at Equator . .	465

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* Reichert's und du Bois-Reymond's *Archiv, u. s. w.*, 1864, S. 151.

VELOCITY OF	Mètres in one second.
Cannon-ball	552*
Wind	1—20
Eagle's flight	35†
Locomotive	27
Greyhound, Racehorse	25
Nervous Agent	26—30
Hand throwing stone 24m·5 high	21·9
Muscular contraction	0·8—1·2
Arterial wave	9·25
Blood in dog's carotid	0·2—0·3
„ capillaries	0·0006—0·0009
Particles moved by cilia	0·00007

A glance at this table shows that the velocity of the nervous agent, far from being enormously great, as most physiologists formerly supposed, is, on the contrary, wonderfully small. Not only is it beyond any comparison smaller than the velocity of electricity and light, and the so-called planetary velocities, but it is even small when compared to the velocity of sound in different media, or to the initial velocity of a cannon-ball.

In order better to realize how slowly sensation and volition are transmitted through the nerves, let us suppose that a large whale, which may be thirty mètres long (98½ feet), has its tail struck by a harpoon. It will then take about one whole second before the pain reaches the huge creature's brain; and, neglecting even the time required for the

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* Rev. Samuel Haughton, in the *Proceedings of the Royal Irish Academy*, 1862, vol. viii., p. 113.

† Simmler, *Poggendorff's Annalen, u. s. w.*, 1864. Bd. cxxi., S. 331.

processes in the brain, nearly another second will elapse before the order can be telegraphed to the muscles of the tail to capsize the boat.

Again, suppose the engine-driver on the locomotive of an express-train running a mile a minute holds his arm extended towards the tender, and moves his fingers, then the nervous agent in the motor fibres of his arm will rest in space or nearly so, because its motion is destroyed by that of the train, just as a cannon-ball, fired at the equator due west, has its motion destroyed by that of the earth around its axis, and does not strike the wall or the ship's side, but is struck by them. And the same thing will happen with the nervous agent in the sensory fibres of the fireman's arm, if, standing on the tender, he should burn his hand at the locomotive.

But also in the racehorse and greyhound, when they are running at full speed, the nervous agent will nearly rest in space, and in the flying eagle it will even be carried in the opposite direction.

As in these cases the whole body of the animal is darted through space at a rate equal or even superior to that of the nervous agent, it will be less a matter of surprise that a man should be able to move his hand almost as quickly as that agent moves along his nerves. This can be made apparent by the height to which a heavy body, a stone, for instance, may be thrown. Throwing, in fact, is nothing else than imparting the greatest possible velocity to the hand, together with the projectile, and letting this fly when the tangent to the curve, wherein the hand necessarily moves, points in the proper direction. The initial velocity of the stone, then, can never exceed that of the hand, or the velocity of the hand, on throwing the stone vertically, must have been equal to that with which the falling stone passes through the horizontal plane from which it started. This reasoning leads to the result put down in the table, and it may safely be conjectured that the same figure also applies with tolerable accuracy to the motion of the fist when striking a blow.

The *arterial wave*, which in more superficial arteries is felt as pulse, travels only about three times more slowly than the nervous agent, according to Professor E. H. Weber's observations.*

By the *velocity of the muscular contraction*, we understand the rate at which the wave of contraction runs along the fibre, when one end of the muscle is stimulated. This rate has been ascertained by Professor Aeby, now of Berne, by experiments made on the muscles of frogs poisoned with Curare, in order to exclude the co-operation of the nerves, and by means of a myographion, on whose cylinder two styles marked their track. The lever bearing each style rested on the muscle with a sort of crutch, and, when the wave of contraction passed under the crutch, it was raised by the thickening of the muscle. Two curves were thus obtained, whose horizontal distance was equal to the distance of the styles, *plus* the distance corresponding on the

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* *De pulsu, resorptione, auditu et tactu. Annotationes anatomicæ et physiologicæ, &c.* Lipsiæ, 1831. 4°, p. 2.

cylinder to the time spent by the wave of contraction in travelling from the first to the second crutch. The velocity of the contraction thus obtained, falls as much short of that anticipated by theory, as did that of the nervous agent; being, in fact, even in muscles fresh from the body of the frog, only about one yard in one second.*

Time required for Reflex Action in the Spinal Cord.—The conduction of sensation in the spinal cord, according to Dr. Schelske's above-mentioned experiments, takes place at nearly, perhaps quite, the same rate as in the nerves; but a great delay is occasioned by the so-called reflex action, when the sensation, impinging upon the spinal cord, starts an involuntary motion by the intervention of the ganglionic cells. By experiments performed on frogs poisoned with strychnia, Professor Helmholtz has found that the reflex contraction happens from $\frac{1}{30}$ to $\frac{1}{10}$ of a second later than the contraction directly produced by the same induced current; from which observation it may be concluded, that the reflex action in the spinal cord takes more than twelve times the time required for the transmission of the stimulation through the sensory and the motor nerves.† This is doubly interesting, because it was chiefly by the impossibility of catching with

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* Dr. Ch. Aeby, *Untersuchungen über die Fortpflanzungsgeschwindigkeit der Reizung in der quergestreiften Muskelfaser*. Braunschweig, 1862.

† *Monatsberichte der Berliner Akademie der Wissenschaften*, 1854. S. 332.

the eye the slightest lapse of time between the stimulation and the reflex contraction that John Müller had been induced to admit, as we have pointed out, so enormous a velocity for the nervous agent.*

Time required for Sensation and Volition in the Brain.—More recently researches of this kind have been extended even to the time required for one of the simplest operations of the brain, *viz.*, the act of sensation and subsequent volition. Dr. de Jaager, in Professor Donders's laboratory, in Utrecht, first repeated Dr. Schelske's experiment on the transmission of sensation in the human body; but with this modification, that the shock could at will be given either on the right or on the left side. In one set of experiments, the person had to answer the right-side shock with a spring-key in his right hand, the left-side shock with a similar key in his left hand; and he knew, beforehand, on which side he was going to be stimulated, and, therefore, would have to answer. In another set of experiments the side was not known beforehand, and the person, after having received the shock, had first to consider which side had been struck, and with which hand, accordingly, he had to act. Now the mean result in the first case was— . . $T_1 = \cdot 205$ sec.

in the second $T_2 = \cdot 272$

and the difference $T_2 - T_1 = \cdot 067$

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* L. c., p. 583.

is obviously the time spent in the operation of the brain required in the second, and not required in the first case.

The physiological time necessary for answering a signal given to the eye has also been determined by Dr. Hirsch by means of Hipp's chronoscope, regulated so as to eliminate the error in time resulting from the working of the apparatus. The sensation-signal being an electric spark, ·204 of a second elapsed before the volition-signal could be given; but if, while watching the rapidly-moving hand of the chronoscope, the observer proposed to stop it at a given position on the dial (by throwing it out of gear, which with this chronoscope is the mode of making a volition-signal), only ·077 of a second were required for doing so.

Dr. de Jaager repeated these experiments with his chronograph and with variously coloured light, and he found that with red as well as with white light, ·200 of a second on the average were required for answering the signal. This figure cannot be compared directly with Dr. Hirsch's figures, with the first of which it so closely agrees, because Dr. de Jaager's observations represent the time T, those of Dr. Hirsch the physiological time only. In these trials the colour of the light to appear was known to the person, and he simply had to answer the signal with the right or with the left hand, according to what had been agreed upon beforehand. In other experiments, however, the colour of the light was not known, and it was agreed that red light should be answered with the right, white light with the left hand. Here the time required for answering was found = ·355 sec., that is, ·154 of a second longer than in the first case, the latter figure again representing the time spent in the operation of the brain.

The physiological time intervening between a signal received by the ear and answered by Hipp's chronoscope has been found by Dr. Hirsch to be ·149 sec. Dr. de Jaager, in company with Professor Donders, has had recourse to quite another method for determining the same time. He has applied to this purpose Mr. Scott's phonautograph, as constructed by M. König, of Paris. In this apparatus a membrane partakes of the vibrations of any sounding body placed opposite the mouth of a paraboloid cavity over whose bottom that membrane is stretched; and a point connected with the membrane traces curves, indicative of its vibrations, on a blackened cylinder, on which simultaneously a tuning-fork records the time. Two persons, A and B, separated by a screen, sit down before the phonautograph. A utters some sharp, explosive syllable, such as *ka*, *ke*, *ki*, or the like. B endeavours to repeat the syllable as quickly as possible. The instant at which both persons have begun to utter the sound is plainly discernible in the track of the style on the cylinder. Now, if the syllable to be repeated has been agreed upon before, the time required varies from ·180 sec. to ·250 sec., according to the person experimented upon, and to practice; but if the syllable be unknown, about ·088 sec. more are necessary for answering the signal.* It thus appears that "*quick as thought*" is, after all, not so very quick.

Concluding Remarks.—The preceding results are not entirely without a practical bearing. In medicine, it is true, they have not yet been applied, although, perhaps, in cases of paralysis, the velocity of the nervous agent will be found diminished, and diagnostic signs, more or less important, might be derived therefrom. But as, strange to say, the warlike art of gunnery first supplied physiology with the means of measuring the velocity of the nervous agent, so again it is a science apparently far remote from this field of inquiry, astronomy, which is the first to benefit by the progress achieved in it. Astronomical observations, as regards the exact determination of time, have hitherto attained only a limited perfection. Till very recently, indeed, though the observations of the same astronomer might agree ever so closely, still, between the observations of several, even first-rate astronomers, discrepancies occurred of one second and more, resulting from the different estimate formed by the observers of the time between the passage of the star at the cross-wire and the next beat of the clock. This uncertainty as to the real time of an astronomical event has been greatly diminished by the introduction into observatories of the electric chronograph. It still subsists, however, and the only way of removing it as completely as possible seems to

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* *De physiologische Tijd bij psychische Processen, Academisch Proefschrift*, &c. Utrecht, 1 Julij, 1865.

be that adopted by Dr. Hirsch, *viz.*, to ascertain by appropriate experiments the time required for registering the passage at the cross-wire of an artificial star moving at a rate similar to that of the real stars, and thus, for the knowledge of the *personal equation* of two observers, to substitute that of their *personal corrections*, the difference of which corresponds to their personal equation.*

As to the theory of the nervous agent, now that we know that this agent moves more than ten times less quickly than sound in air, every attempt to identify it with the electric current as it circulates in a telegraph-wire must appear hopeless, even if a circuit, such as would be necessary for the supposed nerve current to circulate in, were anatomically demonstrated. Thus to the other arguments against this view of the nervous agent—that the resistance of the nerve-tubes would be far too great for any battery to send an available current through them—that the physiological insulation of the nerve-tubes from each other would be impossible to explain—that the effect of ligature or of cutting the nerve and causing its ends to meet again, would be equally obscure—to these arguments, unanswerable as they are in themselves, the researches sketched in this lecture have added corroborative evidence of the highest order. What we have termed the nervous agent, if we look upon its very small velocity, in all probability is some internal motion, perhaps even

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* Plantamour et Hirsch, *Détermination télégraphique de la différence de longitude entre les Observatoires de Genève et de Neuchâtel.* Genève et Bâle, 1864. 4°, p. 89.

some chemical change, of the substance itself contained in the nerve-tubes, spreading along the tubes, according to the speaker's experiments, both ways from any point where the equilibrium has been disturbed; being capable of an almost infinite number of variations or gradations, and of so peculiar a character as to require the unimpaired condition of the nervous structure. It would be out of place on the present occasion to venture upon any more definite statement regarding the nature of the nervous agent, although the progress recently made in this line, chiefly by the modern school of German physiologists, already affords the means of doing so, and of discussing the merits and defects of several solutions proposed. It may not, however, be inappropriate to remark, that although the electric theory of the nervous agent, in the sense just alluded to, cannot any longer be thought of (as little, indeed, as with any degree of probability it ever could), yet it would be rash, as the matter stands, entirely to dismiss the notion of electricity being concerned, and even playing a prominent part, in the internal mechanism of the nerves. The electric currents discovered in the nerves by the speaker four-and-twenty years ago; the remarkable changes which these currents undergo whenever the nervous agent is called into action; the wonderful phenomena of electric fish; those of the muscles and glands; finally, the extreme sensitiveness of the nerves for electricity however applied, and the beautiful and peculiar laws which govern their reaction thereupon, all these are facts which certainly ought not to be disregarded as long as the close connection which they seem to indicate between the two agents has not otherwise proved a fallacy. Nor is it by any means impossible to frame an electrical hypothesis of the nervous agent such as would embrace that newly-discovered feature of it, which has formed the subject of this lecture—its comparative sluggishness. If the *electro-motive molecules*, by which the speaker has tried to account for the electro-motive effects of the nerves, muscles, and electrical organs—*viz.*, minute centres of chemical action, all arranged regularly so as to turn homologous sides the same way—are conceived to act upon each other electrically, mutually determining their position of equilibrium and controlling their deviations from it, in such a system, though electricity were the connecting link of the whole and the means of transmitting power through it, the rate of transmission would be independent of that of electricity, and might in proportion to it be almost infinitesimal; it might, indeed, be what the rate of transmission of the nervous agent really is.

The speaker concluded by exhibiting a model of such a system in which the electro-motive molecules were represented by astatic needles; a more detailed account of which, however, cannot well be given here.

APPENDIX II.

ON THE SOURCE OF MUSCULAR POWER.

A LECTURE GIVEN AT THE ROYAL INSTITUTION,

BY EDWARD FRANKLAND, Ph.D., F.R.S.
Professor of Chemistry, R.I.

Introduction.—What is the source of muscular power? Twenty years ago, if this question had been asked, there were but few philosophers who would have hesitated to reply, "The source of muscular power is that peculiar force which is developed by living animals, and which we term the *vital force!*" But the progress of scientific discovery has rendered the view implied in such an answer so utterly untenable that, at the present moment, no one possessing any knowledge of physical science would venture to return such a reply. We now know that an animal, however high its organization may be, can no more *generate* an amount of force capable of moving a grain of sand, than a stone can fall upwards or a locomotive drive a train without fuel. All that such an animal can do is to liberate that store of force, or *potential energy*, which is locked up in its food. It is the *chemical change* which food suffers in the body of an animal that liberates the previously pent-up forces of that food, which now make their appearance in the form of *actual energy*—as heat and mechanical motion.

From food, and food alone, comes the *matter* of which the animal body is built up; and from food alone come all the different kinds of *physical force* which an animal is capable of manifesting.

The two chief forms of force thus manifested are *Heat* and *Muscular motion* or *mechanical work*, and these have been almost universally traced to two distinct sources—the *heat* to the oxidation of the *food*, and the *mechanical work* to the oxidation of the *muscles*.

This doctrine, first promulgated, the speaker believed, by Liebig, occupies a prominent position in that philosopher's justly celebrated *Chemico-Physiological Essays*.

In his work entitled *Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie, Braunschweig*, 1842, Liebig says: "All experience teaches that there is only one source of mechanical power in the organism, and this source is the transformation of the living parts of the body into lifeless compounds. . . . This transformation occurs in consequence of the combination of oxygen with the substance of the living parts of the body." And again, in his *Letters on Chemistry*, 1851, p. 366, referring to these living parts of the body, he says: "All these organized tissues, all the parts which in any way manifest force in the body are derived from the albumen of the blood; all the albumen of the blood is derived from the plastic or sanguineous constituents of the food, whether animal or vegetable. It is clear, therefore, that the plastic constituents of food, the ultimate source of which is the vegetable kingdom, are the conditions essential to all production or manifestation of force, to all these effects which the animal organism produces by means of its organs of sense, thought, and motion." And again, at page 374, he says: "The sulphurized and nitrogenous constituents of food determine the continuance of the manifestations of force; the non-nitrogenous serve to produce heat. The former are the builders of organs and organized structures, and the producers of force; the latter support the respiratory process, they are *materials for respiration.*"

This doctrine has since been treated as an almost self-evident truth in most physiological text-books; it has been quite recently supported by Ranke;* and, in his lecture *On the Food of Man in relation to his Useful Work*, 1865, Playfair says, page 37: "From the considerations which have preceded, we consider Liebig amply justified in viewing the non-nitrogenous portions of food as mere heat-givers. . . . While we have been led to the conclusion that the transformation of the tissues is the source of dynamical power in the animal." At page 30 he also says: "I agree with Draper and others in considering the contraction of a muscle due to a disintegration of its particles, and its relaxation to

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* *Tetanus eine Physiologische Studie.* Leipzig. 1865.

their restoration. . . . All these facts prove that transformation of the muscle through the agency of oxygen is the condition of muscular action." Finally, in a masterly review of the present relations of chemistry to animal life, published in March last,* Odling says, page 98: "Seeing, then, that muscular exertion is really dependent upon muscular oxidation, we have to consider what should be the products, and what the value of this oxidation." And again, page 103: "The slow oxidation of so much carbon and hydrogen in the human body, therefore, will always produce its due amount of heat, or an equivalent in some other form of energy; for while the latent force liberated by the combustion of the carbon and hydrogen of fat is expressed *solely in the form of heat*, the combustion of an equal quantity of the carbon and hydrogen of voluntary muscle is expressed *chiefly in the form of motion.*"

Nevertheless, this view of the origin of muscular power has not escaped challenge. Immediately after its first promulgation, Dr. J. R. Mayer wrote:† "A muscle is only an apparatus by means of which the transformation of force is effected, *but it is not the material by the chemical change of which* mechanical work is produced." He showed that the 15 lbs. of dry muscles of a man weighing 150 lbs. would, if their mechanical work were due to their chemical change, be completely oxidized in 80 days, the heart

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* *Lectures on Animal Chemistry.*

† *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel*, 1845.

itself in 8 days, and the ventricles of the heart in 2½ days. After endeavouring to prove by physiological arguments that not one per cent. of the oxygen absorbed in the lungs could possibly come into contact with the substance of the muscles, Mayer says: "The fire-place in which this combustion goes on is the interior of the blood-vessels, the *blood* however—a slowly-burning liquid—is the oil in the flame of life. . . . Just as a plant-leaf transforms a given mechanical effect, *light*, into another force, *chemical difference*, so does the muscle produce mechanical work at the cost of the chemical difference consumed in its capillaries. Heat can neither replace the sun's rays for the plant, nor the chemical process in the animal: every act of motion in an animal is attended by the consumption of oxygen and the production of carbonic acid and water; every muscle to which atmospheric oxygen does not gain access ceases to perform its functions."

But Mayer was not the first to conceive this view of muscular action. Nearly 200 years ago, a Bath physician, Dr. John Mayow,* distinctly stated that for the production of muscular motion two things are necessary—the conveyance of combustible substances to the muscle by the blood, and the access of oxygen by respiration. He concluded that the chief combustible substance so used was fat. A century before Priestley isolated oxygen, Mayow was aware of its existence in the air, in nitre, and in nitric acid; he knew that combustion is supported by the

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* *De Motu musculari*, 1681. Mayow was born in 1645, and died 1679.

oxygen of the air, and that this gas is absorbed in the lungs by the blood, and is absolutely necessary for muscular activity.

For two centuries this doctrine sank into oblivion; and it is only within the last two years that it has been again advanced, chiefly by Haidenhain,[*] Traube, and, to a limited extent, by Donders.[†]

Experimental evidence was, however, still wanting to give permanent vitality to the resuscitated doctrine; for although the laborious and remarkable investigations of Voit[‡] and of Edward Smith[§] point unmistakably in the direction of Mayow and Mayer's hypothesis, yet the results of these physiologists were not sufficiently conclusive to render the opposite view untenable. This want of data of a sufficiently conclusive character has been supplied by a happily conceived experiment undertaken by Fick and Wislicenus in the autumn of 1865, and described in the *Philosophical Magazine*, vol. xxxi., p. 485. In the application of these data, however, to the problem now under consideration, one important link was found to be wanting, *viz.*, the amount of actual energy generated by the oxidation

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[*] *Mechanische Leistung Wärmeentwickelung und Stoffumsatz bei der Muskelthätigkeit*, 1864.

[†] As this is passing through the press, the speaker has become aware that Messrs. Lawes and Gilbert advocated this doctrine in 1852, and repeatedly since; their opinions being founded upon experiments on the feeding of cattle.

[‡] *Untersuchungen über den Einfluss des Kochsalzes, des Kaffeés und der Muskel-bewegungen auf den Stoffwechsel*, p. 150, Munich, 1860.

[§] *Phil. Trans.*, 1861, p. 747.

of a given weight of muscle in the human body. Fick and Wislicenus refer to this missing link in the following words:—"The question now arises what quantity of heat is generated when muscle is burnt to the products in which its constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question, for neither the heat of combustion of muscle nor of the nitrogenous *residue* (urea) of muscle is known." Owing to the want of these data, the numerical results of the experiment of Fick and Wislicenus are rendered less conclusive against the hypothesis of muscle combustion than they otherwise would have been, whilst similar determinations, which have been made by Edward Smith, Haughton, Playfair, and others, are even liable to a total misinterpretation from the same cause.

The speaker stated that he had supplied this want by the calorimetrical determination of the actual energy evolved by the combustion of muscle and of urea in oxygen. Availing himself of these data he then proceeded to the consideration of the problem to be solved, the present condition of which might be thus summed up:—It is agreed on all hands that muscular power is derived exclusively from the mutual chemical action of the food and atmospheric oxygen; but opinions differ as to whether that food must first be converted into the actual organized substance of the muscle, before its oxidation can give rise to mechanical force, or whether it is not also possible that muscular work may be derived from the oxidation of the food, which has only arrived at the condition of blood and not of organized muscular tissue.

The importance of this problem can scarcely be overrated; it is a corner-stone of the physiological edifice, and the key to the phenomena of the nutrition of animals. For its satisfactory solution the following data require to be determined:—

1st. The amount of force or actual energy generated by the oxidation of a given amount of muscle in the body.

2nd. The amount of mechanical force exerted by the muscles of the body during a given time.

3rd. The quantity of muscle oxidized in the body during the same time.

If the total amount of force involved in muscular action, as measured by the mechanical work performed, be greater than that which could possibly be generated by the quantity of muscle oxidized during the same time, it necessarily follows that the power of the muscles is not derived *exclusively* from the oxidation of their own substance.

1stly. As regards the first datum to be determined, it is necessary to agree upon some unit for the measurement of mechanical force. The unit of force most commonly adopted is that represented by the lifting of a kilogram weight to the height of one metre. The researches of Joule and Mayer have connected this standard unit with heat;—they prove that the force required to elevate this weight 425 times will, when converted into heat, raise the temperature of an equal weight of water 1° C. If this weight were let fall from a height of 425 metres, its collision with the earth would produce an amount of heat sufficient to raise the temperature of 1 kilogram of water 1° C. The same heating effect would also of course be produced by the fall of 425 kilograms through 1 metre. This standard of force is termed a *metrekilogram* ;* and 425 metrekilograms are equal to that amount of heat which is necessary to raise the temperature of 1 kilogram of water through 1° C. If then it be found that the heat evolved by the combustion of a certain weight of charcoal or muscle, for instance, raises the temperature of 1 kilogram of water through 1° C., this means, when translated into mechanical power, 425 metrekilograms. Again, if a man weighing 64 kilograms climbs to a height of 1,000 metres, the ascent of his body to this height represents 64,000 metrekilograms of work; that is, the labour necessary to raise a kilogram weight to the height of 1 metre 64,000 times.

In order to estimate the amount of actual energy generated by the oxidation of a given amount of muscle in the body, it is necessary to determine, first, the amount of actual energy generated by the combustion of that amount of muscle in oxygen, and then to deduct from the number thus obtained the amount of energy still remaining in the products of the oxidation of this quantity of muscle which leave the body. Of these products, urea and uric and

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* I follow the example of the Registrar-General in abbreviating the French word *gramme* to gram.

hippuric acids are the only ones in appreciable quantity which still retain potential energy on leaving the body, and of these the two latter are excreted in such small proportions that they may be considered as urea without introducing any material error into the results.

These determinations were made in Lewis Thompson's calorimeter, which consists of a copper tube to contain a mixture of chlorate of potash with the combustible substance, and which can be enclosed in a kind of diving-bell, also of copper, and so lowered to the bottom of a suitable vessel containing a known quantity (2 litres) of water. The determinations were made with this instrument in the following manner :—19·5 grams of chlorate of potash, to which about $\frac{1}{8}$th of peroxide of manganese was added, was intimately mixed with a known weight (generally about 2 grams) of the substance whose potential energy was to be determined, and the mixture being then placed in the copper tube above mentioned, a small piece of cotton thread previously steeped in chlorate of potash and dried was inserted in the mixture. The temperature of the water in the calorimeter was now carefully ascertained by a delicate thermometer ; and the end of the cotton thread being ignited, the tube with its contents was placed in the copper bell and lowered to the bottom of the water. As soon as the combustion reached the mixture a stream of gases issued from numerous small openings at the lower edge of the bell and rose to the surface of the water—a height of about 10 inches.

At the termination of the deflagration, the water was allowed free access to the interior of the bell, by opening a stopcock connected with the bell by a small tube rising above the surface of the water in the calorimeter. The gases in the interior of the bell were thus displaced by the incumbent column of water, and by moving the bell up and down repeatedly a perfect equilibrium of temperature throughout the entire mass of water was quickly established. The temperature of the water was again carefully observed, and the difference between this and the previous observation determines the calorific power or potential energy, expressed as heat, of the substance consumed.

The value thus obtained is, however, obviously subject to the following corrections :—

1. The amount of heat absorbed by the calorimeter and apparatus employed, *to be added.*

2. The amount of heat carried away by the escaping gases, after issuing from the water, *to be added.*

3. The amount of heat due to the decomposition of the chlorate of potash employed, *to be deducted.*

4. The amount of heat equivalent to the work performed by the gases generated in overcoming the pressure of the atmosphere, *to be added.*

Although the errors due to these causes to some extent neutralize each other, there is still an outstanding balance of sufficient importance to require that the necessary corrections should be carefully attended to.

The amount of error from the first cause was once for all experimentally determined, and was added to the increase of temperature observed in each experiment.

The amount of heat carried away by the escaping gases after issuing from the water may be divided into two items, *viz.* :—

a. The amount of heat rendered latent by the water which is carried off by the gases in the form of vapour.

b. The amount of heat carried off by these gases by reason of their temperature being above that of the water from which they issue.

It was ascertained that a stream of dry air when passed through the water of the calorimeter, at about the same rate and for the same period of time as the gaseous products of combustion, depressed the temperature of the water by only 0°·02 C.

By placing a delicate thermometer in the escaping gases, and another in the water, no appreciable difference of temperature could be observed. Both these items may therefore be safely neglected.

The two remaining corrections can be best considered together, since a single careful determination eliminates both. When a combustible substance is burnt in gaseous oxygen, the conditions are essentially different from those which obtain when the same substance is consumed at the expense of the combined or solid oxygen of chlorate of potash. In the first case the products of combustion, when cooled to the temperature of the water in the calorimeter, occupy less space than the substances concerned in the combustion, and no part of the energy developed is therefore expended in external work, that is, in overcoming the pressure of the atmosphere. In the second case, both the combustible and the supporter of combustion are in the solid condition, whilst a considerable proportion of the products of combustion are gases. The generation of the latter cannot take place without the performance of external work, for every cubic inch produced must obviously, in overcoming atmospheric pressure, perform an amount of work equivalent, in round numbers, to the lifting of a weight of 15 lbs. to the height of one inch. In performing this work the gases are cooled, and consequently less heat is communicated to the water of the calorimeter. Nevertheless, the loss of heat due to this cause is but small. Under the actual conditions of the experiments detailed below, its amount would only have increased the temperature of the water in the calorimeter by 0°·07 C. Even this slight error is entirely eliminated by the final correction which we have now to consider.

It is well known that the docomposition of chlorate of potash into chloride of potassium and free oxygen is attended with the evolution of heat. If a few grains of peroxide of manganese, or better, of peroxide of iron, be dropped into an ounce or two of fused chlorate of potash which is slowly disengaging oxygen, the evolution of gas immediately proceeds with great violence, and the mixture becomes visibly red hot, although the external application of heat be discontinued from the moment when the metallic peroxide is added. The latter remains unaltered at the close of the operation. It is thus obvious that chlorate of potash, on being decomposed, furnishes considerably more heat than that which is necessary to gasify the oxygen which it evolves. It was therefore necessary to determine the amount of heat thus evolved by the quantity of chlorate of potash (9·75 grams) mixed with one gram of the substance burnt in each of the following determinations. This was effected by the use of two copper tubes, the one placed within the other. The interior tube was charged with a known weight of the same mixture of chlorate of potash and peroxide of manganese as that used for the subsequent experiments, whilst the annular space between the two tubes was filled with a combustible mixture of chlorate and spermaceti, the calorific value of which had been previously ascertained. The latter mixture was ignited in the calorimeter as before, and the heat generated during its combustion effected the complete decomposition of the chlorate in the interior cylinder, as was proved by a subsequent examination of the liquid in the calorimeter, which contained no traces of undecomposed chlorate. The following are the results of five experiments thus made, expressed in units of heat, the unit being equal to 1 gram of water raised through 1° C. of temperature— :

	Units of Heat.
1st experiment	340
2nd ,, ...	300
3rd ,, ...	375
4th ,, ...	438
5th ,, ...	438
	5)1891
Mean	378

This result was confirmed by the following experiments :—

1. Starch was burnt, firstly, in a current of oxygen gas, and secondly, by admixture with chlorate of potash and peroxide of manganese.

Heat units furnished by one gram of starch burnt with 9·75 grams chlorate of potash	4290
Heat units furnished by the same weight of starch burnt in a stream of oxygen gas	3964
Difference	326

2nd. Phenylic alcohol was burnt with chlorate of potash, and the result compared with the calorific value of this substance as determined by Favre and Silbermann.

Heat units furnished by one gram of phenylic alcohol burnt with 9·75 grams chlorate of potash	8183
Heat units furnished by one gram of phenylic alcohol when burnt with gaseous oxygen (Favre and Silbermann)	7842
Difference	341

These three determinations of the heat evolved by the decomposition of 9·75 grams of chlorate of potash, furnishing the numbers 378, 326, and 341, agree as closely as could be expected, when it is considered that all experimental errors are necessarily thrown upon the calorific value of the chlorate of potash.

The mean of the above five experimental numbers was, in all cases, deducted from the actual values read off in the following determinations.

It was ascertained by numerous trials that all the chlorate of potash was decomposed in the deflagrations, and that but mere traces of carbonic oxide were produced.

Joule's mechanical equivalent of heat was employed, *viz.*, 1 kilogram of water raised 1° C. = 423 metrekilograms of force, or one unit of heat.

The following results were obtained :—

Actual energy developed by one gram of each substance when burnt in oxygen.

Name of Substance dried at 100° C.	Heat Units.					Metre-kilograms of force (Mean.)*
	1st Experiment	2nd Experiment	3rd Experiment	4th Experiment	Mean.	
Beef Muscle purified by repeated washing with ether . . .	5174	5062	5195	5088	5103	2161
Purified Albumen	5009	4987	..	..	4998	2117
Beef Fat . . .	9069	..	..	..	9069	3841
Hippuric Acid .	5330	5437	..	..	5383	2280
Uric Acid . .	2645	2585	..	..	2615	1108
Urea † . . .	2121	2302	2207	2197	2206	934

It is evident that the above determination of the actual energy developed by the combustion of muscle in oxygen represents more than the amount of actual energy produced by the oxidation of muscle within the body, because, when muscle burns in oxygen its carbon is converted into carbonic acid, and its hydrogen into water; the nitrogen being, to a great extent, evolved in the elementary state;

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* Got by multiplying units of heat by ·423.

† The speaker showed the combustibility of urea by burning it upon asbestos in a jar of oxygen gas.

whereas, when muscle is most completely consumed in the body, the products are carbonic acid, water and urea; the whole of the nitrogen passes out of the body as urea—a substance which still retains a considerable amount of potential energy. Dry muscle and pure albumen yield, under these circumstances, almost exactly one-third of their weight of urea, and this fact, together with the above determination of the actual energy developed on the combustion of urea, enables us to deduce with certainty the amount of actual energy developed by muscle and albumen respectively when consumed in the human body. It is as follows:—

Actual energy developed by one gram of each substance when consumed in the body.

Name of substance dried at 100° C.	Heat units (Mean.)	Metrekilograms of force. (Mean.)
Beef Muscle purified by ether . .	4368	1848
Purified Albumen	4263	1803

2ndly. We have thus ascertained the first of our three data, *viz.*, the amount of force or actual energy generated by the oxidation of a given amount of muscle in the body; and we now proceed to ascertain the second, *viz.*, the amount of mechanical force exerted by the muscles of the body during a given time. For this purpose we have only to avail ourselves of the details of Fick and Wislicenus's conclusive experiment already referred to, and which consisted in the ascent of the Faulhorn in Switzerland from the lake of Brienz. This mountain can be ascended by a very steep path from Iseltwald, which was of course favourable for the experiment, and there is an hotel on the summit which allowed the experimenters to pass the following night under tolerably normal circumstances. The following is their own description and estimate of the amount of work performed in the ascent.*

"Let us now inquire how much work was really really done by our muscles. One item necessary for the reply is already at hand, *viz.*, the height of the summit of the Faulhorn above the level of the lake of Brienz multiplied by the weight of the body; the former reckoned in metres, the latter in kilograms. The weight of the body with the equipments (hat, clothes, stick) amounted to 66 kilograms in Fick's case, and 76 in Wislicenus's. The height of the Faulhorn above the level of the lake of Brienz is, according to trigonometric measurements, exactly 1956 metres. Therefore Fick performed 129,096 (1956 × 66) and Wislicenus 148,656 (1956 × 76) metrekilograms of muscular work."

But in addition to this measurable external work there is another item of force "which can be expressed in units of work; and though its value cannot be quite accurately calculated, yet a tolerable approximation can be made. It consists of the force consumed in respiration and the heart's action. The work performed by the heart has been estimated, in a healthy full-grown man, at about 0·64

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* *Phil. Mag.*, vol. xxxi., p. 496, 1866.

metrekilogram* for each systole. During the ascent, Fick's pulse was about 120 per minute. That gives for the 5·5 hours of the ascent an amount of work which may be estimated at 25,344 metrekilograms, entirely employed in the maintenance of the circulation. No attempt has yet been made to estimate the labour of respiration. One of us has shown, however, in the second edition of his *Medical Physics* (p. 206), that Donders' well-known investigations concerning the conditions of pressure in the cavity of the thorax give sufficient data for such an estimate. He has there shown that the amount of work performed in an inspiration of 600 cubic centims. may be rated at about 0·63 metrekilogram. Fick breathed during the ascent at an average rate of about 25 respirations per minute, which gives, according to this estimation, an amount of respiratory work for the whole ascent of 5,197 metrekilograms. If we add this, and the number representing the work of the heart, to the external work performed by Fick, we obtain a total of 159,637 metrekilograms.† If we suppose that Wislicenus's respiratory and circulatory work bore the same proportion to Fick's as his bodily weight did to Fick's, *i.e.*, 7 : 6, we obtain for Wislicenus's amount of work, as far as it is possible to calculate it, a total of 184,287 metrekilograms.‡

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* 0·43 is here assigned as the work of the left, and 0·21 as that of the right ventricle.

† 129,096
25,344
5,197
159,637

‡ 148,656
29,568
6,063
184,287

"Besides these estimated (and certainly not overestimated) items, there are several others which cannot be even approximately calculated, but the sum of which, if it could be obtained, would probably exceed even our present large total. We will try to give at least some sort of an account of them. It must first be remembered that in the steepest mountain path there are occasional level portions, or even descents. In traversing such places the muscles of the leg are exerted as they are in ascending, but the whole work performed is transformed back into heat. The same force-producing process, however, must be going on in the muscles as if work were being performed which did not undergo this transformation. In order to make this point yet clearer we may take into consideration that the whole work of the ascent only existed temporarily as work. On the following day the result was reversed; our bodies approached the centre of the earth by as much as they had receded from it the day before, and, in consequence, on the second day an amount of heat was liberated equal to the amount of work previously performed. The two parts of the action, which in this case were performed on two separate days, take place in walking on level ground in the space of a footstep.

"Let us observe, besides, that in an ascent it is not only those muscles of the leg specially devoted to climbing which are exerted; the arms, head, and trunk are continually in motion. For all these movements force-generating processes are necessary, the result of which cannot, however, figure in our total of work, but must appear entirely in the form of heat, since all the mechanical effects of these movements are immediately undone again. If we raise an arm, we immediately let it drop again, &c.

"There was besides a large portion of our muscular system employed during the ascent, which was performing no external work (not even temporary work, or mechanical effects immediately reversed), but which cannot be employed without the same force-generating processes which render external work possible. As long as we hold the body in an upright position, individual groups of muscles (as, for instance, the muscles of the back, neck, &c.) must be maintained in a state of continual tetanus in order to prevent the body from collapsing. We may conceive of a tetanized muscle as holding up a weight which would immediately fall if the supply of actual energy were to cease. It is active but it performs no work, and therefore all the force produced is liberated in the form of heat."

Thus the total amount of measured and estimable work performed in 5·5 hours in the experiments before us was 159,637 metrekilograms for Fick, and 184,287 metrekilograms for Wislicenus. This is our second datum.

3rdly. The third, *viz.*, the amount of muscle oxidized in the body during the performance of this work has been carefully determined by the same experimenters, as well as the rate of muscle consumption before and after the ascent. For the details of these determinations the speaker referred his hearers to the *Philosophical Magazine* for 1866, vol. xxxi., page

488; but the following is a condensed summary of the results:—

Ascent of the Faulhorn.

		Fick.	Wislicenus.
		Gram.	Gram.
Amount of Nitrogen secreted in Urine per hour before ascent		·63	·61
Weight of dry Muscle corresponding to Nitrogen		4·19	4·05
Amount of Nitrogen secreted per hour during ascent		·41	·39
Weight of dry Muscle corresponding to Nitrogen		2·70	2·56
Amount of Nitrogen secreted per hour during 6 hours after the ascent		·40	·40
Weight of dry Muscle corresponding to Nitrogen		2·63	2·63
Amount of Nitrogen secreted per hour during the following night		·45	·51
Weight of dry Muscle corresponding to Nitrogen		3·06	3·39
Total amount of Nitrogen secreted during ascent		3·31	3·13
Ditto during 6 hours after ascent		2·43	2·42
		5·74	5·55
Weight of dry Muscle corresponding to Nitrogen secreted	During ascent	20·98	20·89
	During 6 hours after ascent	16·19	16·11
		37·17	37·00

The results of these determinations add a new link to the chain of experimental evidence, that muscular exertion does not necessarily increase the excretion of nitrogen through the urine. From midday before the ascent (August 29th, 1865) to the following evening at seven o'clock (August 30th) both gentlemen abstained from all nitrogenous food. During these thirty-one hours they had nothing in the way of solid food except starch, fat, and sugar. The two former were taken in the form of cakes. Starch was made up with water into a thin paste, which was then made into small cakes and fried with plenty of fat. The sugar was taken dissolved in tea. In addition to this there was the sugar contained in the beer and wine, which were taken in quantities usual in mountain excursions. It was doubtless owing to this absence from food containing nitrogen that the amount of this element secreted through the urine declined tolerably regularly from the 29th of August till the evening of the 30th. Even in the night of the 30th to the 31st, in spite of the plentiful meal of albuminous food on the evening of the 30th, the secretion of nitrogen was less than on the preceding night. The reason of this is probably to be sought for in the circumstance that during the period of abstinence the secretion of nitrogen was carried on at the expense of tissues, and now these tissues required reparation.

It is perhaps scarcely worthy of record that during the ascent neither of the experimenters perspired perceptibly, since it has been proved by Ranke that no appreciable amount of nitrogen leaves the system in the matter of perspiration; and as Thiry has also shown that no nitrogen is got rid of by respiration, it follows that, in addition to the nitrogen contained in the urine, the only other mode of exit for this element is through the fæces. Now the proportion secreted through the fæces has been estimated by Ranke at about one-twelfth of that in the urine; but inasmuch as all experiments on the subject tend to show that this alvine nitrogen is, as voided, a constitutent of un-oxidized compounds, that is, of compounds that have not yielded up their force, it has no claim upon our attention.

4thly. There is still another circumstance which requires to be taken into consideration before we proceed to apply our three data to the solution of the problem before us. It is this:—Is it possible that at the termination of the ascent of the Faulhorn there might be a considerable quantity of the nitrogenous products of decomposition retained in the body? Considering the physiological effect of the retention of urea in the system, as exemplified whenever the secretion of urine is interrupted, it is difficult to imagine the possibility of any considerable quantity of urea being retained in the system of a healthy man. It is, however, otherwise with creatin, another of the products of the metamorphosis of tissue; for it has been repeatedly shown that a muscle which has been hard worked contains more creatin than one that has been at rest. Thus the quantity of creatin contained in the heart of an ox was found to be ·14 per cent. (Gregory), and that in other ox-flesh only ·06 per cent. (Staedeler). Now the muscles which extend the leg in walking, and which do the essential work in ascending, have been estimated by Weber to weigh in both legs 5·8 kilograms, and if we assume that before the ascent these muscles contained ·06 per cent. of creatin, whilst after the ascent the percentage had increased to ·14 per cent., then the amount of creatin thus exceptionally retained would amount to 4·64 grams, which would be derived from 8·4 grams of muscle.

The speaker had been unable to determine the calorific effect of creatin, and consequently the actual energy developed by the transformation of muscle into creatin; for, although he was kindly furnished with an ample supply of this material by Dr. Dittmar, yet all attempts to burn it in the calorimeter were fruitless. Even when mixed in very small proportions with chlorate of potash and other combustibles of known value, the mixture invariably exploded violently on ignition. Although actual determination thus fails us, there can be no doubt that the transformation of muscle into creatin and other non-nitrogenous products must be attended by the liberation of far less actual energy than its transformation into urea, carbonic acid, and water. To be convinced of this, it is only necessary to compare (under equal nitrogen value) the formulæ of muscle, creatin, and urea, remembering at the same time that the nitrogen probably possesses no thermal value, and that each atom of oxygen destroys approximately the thermal effect of two atoms of hydrogen:—

	Comparable formulæ.	Powerful or unburnt matter.
Muscle . . .	$C_{24}\ H_{37}\ N_6\ O_7$	$C_{24}\ H_{23}$
Creatin . . .	$C_8\ H_{18}\ N_6\ O_4$	$C_8\ H_{10}$
Urea . . .	$C_8\ H_{12}\ N_6\ O_3$	$C_3\ H_6$

Thus it is evident that the amount of creatin exceptionally retained in the system could not greatly affect the result of the experiment as regards the possible amount of actual energy derivable from the metamorphosed tissues during the ascent; firstly, on account of the small quantity of creatin so retained, and, secondly, because creatin still contains about one-third of the potential energy of the muscle from which it is derived. But as this point cannot be experimentally demonstrated, the speaker followed the example of Fick and Wislicenus, and made a very liberal allowance on this score. He allowed, as they had done, that the whole of the nitrogen secreted during the six hours after the ascent was exceptionally retained in the system *as urea* during the ascent. This is equivalent to an admission that the muscles of the legs contained at the end of the ascent eleven times as much creatin as was present in them before the ascent. In the above tabular statement of results provision has been made for this allowance by adding together, on the one hand, the amounts of nitrogen secreted during the ascent and six hours after it, and, on the other, the weights of dry muscle corresponding to these two amounts of nitrogen.

5thly. Having thus far cleared the ground, let us now compare the amount of measured and calculated work performed by each of the experimenters during the ascent of the Faulhorn with the actual energy capable of being developed by the maximum amount of muscle that could have been consumed in their bodies, this amount being represented by the total quantity of nitrogen excreted in each case during the ascent and for six hours afterwards :—

	Fick.	Wislicenus.
	Grams.	Grams.
Weight of dry Muscle consumed	37·17	37·00
	Metrekilograms.	Metrekilograms.
Actual energy capable of being produced by the consumption of 37·17 and 37·00 grams of dry Muscle in the body	68,690	68,376
Measured work performed in the ascent (external work)	129,096	148,656
Calculated circulatory and respiratory work performed during the ascent (internal work)	30,541	35,631
Total ascertainable work performed . .	159·637	184,287

It is thus evident that the muscular power expended by these gentlemen in the ascent of the Faulhorn could not be exclusively derived from the oxidation, either of their muscles, or of other nitrogenous constituents of their bodies, since the maximum of power capable of being derived from this source even under very favourable assumptions is, in both cases, less than one-half of the work actually performed. But the deficiency becomes much greater if we take into consideration the fact, that the actual energy developed by oxidation or combustion cannot be wholly transformed into mechanical work. In the best-constructed steam-engine, for instance, only $\frac{1}{10}$th of the actual energy developed by the burning fuel can be obtained in the form of mechanical power ; and in the case of man, Helmholtz estimates that not more than $\frac{1}{5}$th of the actual energy developed in the body can be made to appear as external work. The experiments of Haidenhain, however, show that, under favourable circumstances, a muscle may be made to yield in the shape of mechanical work as much as one-half of the actual energy developed within it, the remainder taking the form of heat. Taking then this highest estimate of the proportion of mechanical work capable of being got out of actual energy, it becomes necessary to multiply by 2 the above numbers representing the ascertainable work performed, in order to express the actual energy involved in the production of that work. We then get the following comparison of the actual energy capable of being developed by the amount of muscle consumed, with the actual energy necessary for the performance of the work executed in the ascent of the Faulhorn :—

	Fick.	Wislicenus.
	Metrekilograms.	Metrekilograms.
Actual energy capable of being produced by Muscle metamorphosis	68,690	68,376
Actual energy expended in work performed .	319,274	368,574

Thus, taking the average of the two experiments, it is evident that *scarcely $\frac{1}{5}$th of the actual energy required for the work performed could be obtained from the amount of muscle consumed.*

6thly. Interpreted in the same way, previous experiments of a like kind prove the same thing, though not quite so conclusively. To illustrate this I will here give a summary of three sets of experiments: the first, made by Dr. E. Smith, upon prisoners engaged in treadmill labour; the second, by the Rev. Dr. Haughton, upon military prisoners engaged in shot drill; and the third, adduced by Playfair, and made upon pedestrians, piledrivers, men turning a winch, and other labourers.

TREADWHEEL EXPERIMENTS.

A treadwheel is a revolving drum with steps placed at distances of 8 inches, and the prisoners are required to turn the wheel downwards by stepping upwards. Four prisoners, designated below as A, B, C, and D, were employed in these experiments, and each worked upon the wheel in alternate quarters of an hour, resting in a sitting posture during the intervening quarters. The period of actual daily labour was 3½ hours. The total ascent per hour 2160 feet, or per day 1·432 mile. The following are the results:—

Treadwheel Work.—(E. Smith.)

	Weight in Kilograms	Ascent in Mètres.	Days occupied in Ascent	External work performed in Metrekilograms.	Total Nitrogen evolved.	Weight of dry Muscle corresponding to Nitrogen.
					Grams	Grams.
A	47·6	23,045	10	1,096,942	171·3	1101·2
B	49	23,045	10	1,129,205	174·5	1121·7
C	55	20,741	9	1,140,755	168·0	1080·1
D	56	20,741	9	1,161,496	159·3	1024·3

In these experiments the measured work was performed in the short space of 3½ hours, whilst the nitrogen estimated was that voided in the shape of urea in 24 hours. It will, therefore, be necessary to add to the measured work, that calculated for respiration and circulation for the whole period of 24 hours. This amount of internal work was computed, from the estimates of Helmholtz and Fick, to be as follows:—

Internal Work.—(Helmholtz and Fick.)

	Work performed.	Actual energy required.
	Metrekilo-grams.	Metrekilo-grams.
Circulation of the blood during 24 hours, at 75 pulsations per minute . . .	69,120*	138,240
Respiration for 24 hours, at 12 respirations per minute	10,886	21,772
Statical activity of muscles	not determined	not determined
Peristaltic motion	,, ,,	,, ,,
	80,006	160,012

Taking this estimate for internal work, the average results of the treadwheel experiments may be thus expressed:—

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[*] Since making use of this number, I find that Donders estimates the work of the heart alone, for 24 hours, at 86,000 metrekilograms, a figure which is higher than that above for the combined work of circulation and respiration.

Treadwheel Work.

Average external work per man per day	119,605 mks.
Average nitrogen evolved per man per day	17·7 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day	114 ,,
Actual energy producible by the consumption of 114 grams of dry muscle in the body	210,672 mks.
Average actual energy developed in the body of each man, *viz.*—	
External work 119,605 × 2 = 239,210 mks.	
Circulation 69,120 × 2 = 138,240 ,,	
Respiration 10,886 × 2 = 21,772 ,,	
	399,222 ,,

In these experiments the conditions were obviously very unfavourable for the comparison of the amount of actual energy producible from muscle metamorphosis with the quantity of actual energy expended in the performance of estimable work; since, during that portion of the twenty-four hours not occupied in the actual experiment, a large amount of unestimable internal work, such as the statical activity of the muscles, peristaltic motion, &c., was being performed. Nevertheless, these experiments show that the average actual energy developed in producing work in the body of each man was nearly twice as great as that which could possibly be produced by the whole of the nitrogenous matter oxidized in the body during 24 hours. It must also be remarked that the prisoners were fed upon a nitrogenous diet containing six ounces of cooked meat, without bone; a diet which, as is well known, would favour the production of urea.

Shot-drill Experiments.

7thly. The men employed for these experiments were fed exclusively upon vegetable diet, and they consequently secreted a considerably smaller amount of nitrogen than the flesh-eaters engaged in the treadwheel work. The other conditions were, however, equally unfavourable for showing the excess of work performed, over the amount derivable from muscle metamorphosis.

In shot-drill, each man lifts a 32-lb. shot from a tressel to his breast, a height of 3 feet; he then carries it a distance of 9 feet, and lays it down on a similar support, returning unloaded. Six of these double journeys occupy one minute. The men were daily engaged with—

Shot drill	3 hours.
Ordinary drill . .	1¼ ,,
Oakum picking . .	3½ ,,

The total average daily external work was estimated by Haughton at 96,316 metrekilograms per man.

The following is a condensed summary of the results of these experiments :—

Military Vegetarian Prisoners at Shot Drill.—(Haughton.)

Average external work per man per day .	96,316 mks.
Average nitrogen evolved per man per day	12·1 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day . .	77.9 ,,
Actual energy producible by the consumption of 77·9 grams of dry muscle in the body	143,950 mks.

Military Vegetarian Prisoners at Shot Drill—continued.

Average actual energy developed daily in the body of each man, *viz.*, External work, 96,316 × 2 =	192,632 mks.	
Internal work	160,012 „	
	———	352,644 mks.

Owing chiefly to the vegetable diet of these prisoners, the result is more conclusive than that obtained upon the treadwheel, the amount of work actually performed being considerably more than twice as great as that which could possibly be obtained through the muscle metamorphosis occurring in the bodies of the prisoners.

Playfair's Determinations.

8thly. In these determinations the number 109,496 metrekilograms was obtained as the average amount of daily work performed by pedestrians, pile-drivers, porters, paviours, &c.; but, as the amount of muscle consumption is calculated from the nitrogen taken in the food, the conditions are as unfavourable as possible with regard to the point the speaker was seeking to establish; for it is here assumed, not only that all the nitrogen taken in the food enters the blood, but also that it is converted into muscle, and is afterwards oxidized to carbonic acid, water, and urea.

The following are the results expressed as in the previous cases:—

Hard-worked Labourer.—(Playfair.)

	Work performed.	Actual energy required.
Daily labour (external work)	109,496 mks.	218,992 mks.
Internal work	80,006 „	160,012 „
	189,502 mks.	379,004 mks.
Actual energy capable of being produced from 5·5 oz. (155·92 grms.) of flesh-formers contained in the daily food of the labourer	..	288,140 mks.

Thus, even under the extremely unfavourable conditions of these determinations, the actual work performed exceeded that which could possibly be produced through the oxidation of the nitrogenous constituents of the daily food by more than 30 per cent.

9thly. We have seen, therefore, in the above four sets of experiments, interpreted by the data afforded by the combustion of muscle and urea in oxygen, that the transformation of tissue alone cannot account for more than a small fraction of the muscular power developed by animals; in fact, this transformation goes on at a rate almost entirely independent of the amount of muscular power developed. If the mechanical work of an animal be doubled or trebled there is no corresponding increase of nitrogen in the secretions; whilst it was proved on the other hand by Lawes and Gilbert, as early as the year 1854, that animals, under the same conditions as regarded exercise, had the amount of nitrogen in their secretions increased twofold by merely doubling the amount of nitrogen in their food. Whence then comes the muscular power of animals? What are the substances which, by their oxidation in the body, furnish the actual energy, whereof a part is converted into muscular work? In the light of the experimental results detailed above, can it be doubted that a large proportion of the muscular power developed in the bodies of animals has its origin in the oxidation of non-nitrogenous substances? For whilst the secretion of nitrogen remains nearly stationary under widely different degrees of muscular exertion, the production of carbonic acid increases most markedly with every augmentation of muscular work, as is shown by the following tabulated results of E. Smith's highly important experiments regarding the amount of carbonic acid evolved from his own lungs under different circumstances.*

Excretion of carbonic acid during rest and muscular exertion:—

	Carbonic acid per hour.
During sleep	19·0 grams.
Lying down and sleep approaching .	23·0 „
In a sitting posture	29·0 „
Walking at rate of 2 miles per hour .	70·5 „
„ „ 3 „ „ .	100·6 „
On the treadwheel, ascending at the rate of 28·65 feet per minute . .	189·6 „

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* *Phil. Trans.* for 1859, p. 709. (Pettenhoffer's experiments on the different amounts of carbonic acid thrown out during the day and during the night do not affect the three last numbers in this table.)

It has been already stated as a proposition upon which all are agreed, that food, and food alone, is the ultimate source from which muscular power is derived; but the above determinations and considerations, the speaker believed, prove conclusively, firstly, that the non-nitrogenous constituents of the food, such as starch, fat, &c., are the chief sources of the actual energy, which becomes partially transformed into muscular work; and secondly, that the food does not require to become organized tissue before its metamorphosis can be rendered available for muscular power; its digestion and assimilation into the circulating fluid—the blood—being all that is necessary for this purpose. It is, however, by no means the non-nitrogenous portions of food alone that are capable of being so employed, the nitrogenous also, inasmuch as they are combustible, and consequently capable of furnishing actual energy, might be expected to be available for the same purpose; and such an expectation is confirmed by the experiments of Savory upon rats,* in which it is proved that these animals can live for weeks in good health upon food consisting almost exclusively of muscular fibre. Even supposing these rats to have performed no external work, nearly the whole of their internal muscular work must have had its source in the actual energy developed by the oxidation of their strictly nitrogenous food.

It can scarcely be doubted, however, that the chief use of the nitrogenous constituents of food is for the renewal of muscular tissue; the latter, like

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* *The Lancet*, 1863, pages 381 and 412.

every other part of the body, requiring a continuous change of substance, whilst the chief function of the non-nitrogenous is to furnish by their oxidation the actual energy which is in part transmuted into muscular force.

The combustible food and oxygen coexist in the blood which courses through the muscle, but when the muscle is at rest there is no chemical action between them. A command is sent from the brain to the muscle; the nervous agent determines oxidation. The potential energy becomes active energy, one portion assuming the form of motion, another appearing as heat. *Here is the source of animal heat, here the origin of muscular power!* Like the piston and cylinder of a steam-engine, the muscle itself is only a machine for the transformation of heat into motion; both are subject to wear and tear and require renewal, but neither contributes in any important degree by its own oxidation to the actual production of the mechanical power which it exerts.

10thly. From this point of view it is interesting to examine the various articles of food in common use, as to their capabilities for the production of muscular power. The speaker had therefore made careful estimations of the calorific value of different materials used as food, by the same apparatus and in the same manner as described above for the determination of the actual energy in muscle, urea, uric acid, and hippuric acid.

The results are embodied in the following series of tables, but it must be borne in mind that it is only on the condition that the food is digested and passes into the blood that the results given in these tables are realized. If, for instance, sawdust or paraffin oil had been experimented upon, numbers would have been obtained for these substances, the one about equal to that assigned to starch, and the other surpassing that of any article in the table; but these numbers would obviously have been utterly fallacious, inasmuch as neither sawdust nor paraffin oil is, to any appreciable extent, digested in the alimentary canal. Whilst the force values experimentally obtained for the different articles in these tables must therefore be understood as the maxima assignable to the substances to which they belong, yet it must not be forgotten that a large majority of these substances appear to be completely digestible under normal circumstances.

Actual Energy developed by One Gram of various Articles of Food when burnt in Oxygen.

NAME OF FOOD.	Heat Units.		Metrekilograms of Force.		Per cent. of Water.
	Dry.	Natural Condition.	Dry.	Natural Condition.	
Cheese (Cheshire)	6114	4647	2589	1969	24·0
Potatoes	3752	1013	1589	429	73·0
Apples	3669	660	1554	280	82·0
Oatmeal	..	4004	..	1696	..
Flour	..	3941	..	1669	..
Pea-meal	..	3936	..	1667	..
Ground Rice	..	3813	..	1615	..
Arrowroot	..	3912	..	1657	..
Bread Crumb	3984	2231	1687	945	44·0
Ditto Crust	..	4459	..	1888	..

Actual Energy developed by One Gram of various Articles of Food, &c.—continued.

Name of Food.	Heat Units.		Metrekilograms of Force.		Per cent. of Water.
	Dry.	Natural Condition.	Dry.	Natural Condition.	
Beef (lean)	5313	1567	2250	664	70·5
Veal „	4514	1314	1912	556	70·9
Ham „	4343	1980	1839	839	54·4
Mackerel	6064	1789	2568	758	70·5
Whiting	4520	904	1914	383	80·0
White of Egg	4896	671	2074	284	86·3
Hard-boiled Egg	6321	2383	2677	1009	62·3
Yolk of Egg	6460	3423	2737	1449	47·0
Gelatin	4520	. .	1914	. .	. .
Milk	5093	662	2157	280	87·0
Carrots	3767	527	1595	223	86·0
Cabbage	3776	434	1599	184	88·5
Cocoa Nibs	. .	6873	. .	2911	. .
Beef Fat	9069	. .	3841	. .	. .
Butter	. .	7264	. .	3077	. .
Cod-liver Oil	. .	9107	. .	3857	. .
Lump Sugar	. .	3318	. .	1418	. .
Commercial Grape Sugar .	. .	3277	. .	1388	. .
Bass's Ale (alcohol reckoned)	3776	775	1599	328	88·4
Guinness's Stout	6348	1076	2688	445	88·4

Actual Energy developed by One Gram (15·43 grs.) *of various Articles of Food when oxidized in the Body. The quantity of Carbon and Hydrogen in Urea (indicated by the Nitrogen) being deducted from the total Carbon and Hydrogen of the substance.*

NAME OF FOOD.	Metrekilograms of Force.	
	Dry.	Natural Condition.
Butter	..	3077 = 1000
Beef Fat	3841	.. ..
Cod-liver Oil	3857	.. ..
Cocoa Nibs	..	2902 = 943
Cheshire Cheese	2129	1846 = 599
Oatmeal	..	1665 = 541
Arrowroot	..	1657 = 538
Flour	..	1627 = 522
Pea-meal	..	1598 = 519
Ground Rice	..	1591 = 517
Lump Sugar	..	1418 = 460
Yolk of Egg	2641	1400 = 455
Commercial Grape Sugar . .	..	1388 = 450
Hard-boiled Egg	2562	966 = 313
Bread Crumb	1625	910 = 295
Lean of Ham, boiled . . .	1559	711 = 231
Gelatin	1550	.. ..
Mackerel	2315	683 = 221
Lean of Beef	2047	604 = 196
Ditto Veal	1704	496 = 161
Guinness's Stout	2688	455 = 147
Potatoes	1563	422 = 137
Whiting	1675	335 = 109
Bass's Ale, bottled . . .	1559	328 = 106
Apples	1516	273 = 88
Milk	2046	266 = 86
White of Egg	1781	244 = 79
Carrots	1574	220 = 71
Cabbage	1543	178 = 58

Weight and Cost of various Articles of Food required to be oxidized in the Body in order to raise 140 *lbs.* (10 *stone*) *to the height of* 10,000 *ft.* (1·89 *mile*)

External work = ⅕th actual energy.

Name of Food.	Weight in lbs. required.	Price per lb.	Cost.
		s. *d.*	*s.* *d.*
Cheshire Cheese	1·156	0 10	0 11½
Potatoes	5·068	0 1	0 5¼
Apples	7·815	0 1½	0 11¾
Oatmeal	1·281	0 2¾	0 3½
Flour	1·311	0 2¾	0 3¾
Pea-meal	1·335	0 3¼	0 4½
Ground Rice	1·341	0 4	0 5½
Arrowroot	1·287	1 0	1 3½
Bread	2·345	0 2	0 4¾
Lean Beef	3·532	1 0	3 6½
„ Veal	4·300	1 0	4 3½
„ Ham, boiled	3·001	1 6	4 6
Mackerel	3·124	0 8	2 1
Whiting	6·369	1 4	9 4
White of Egg	8·745	0 6	4 4½
Hard-boiled Egg	2·209	0 6½	1 2½
Isinglass	1·377	16 0	22 0½
Milk	8·021	5*d.* per quart.	1 3½
Carrots	9·685	0 1½	1 2½
Cabbage	12·020	0 1	1 0¼
Cocoa-nibs	0·735	1 6	1 1¼
Butter	0·693	1 6	1 0½
Beef Fat	0·555	0 10	0 5½
Cod-liver Oil	0·553	3 6	1 11¼
Lump Sugar	1·505	0 6	1 3
Commercial Grape Sugar	1·537	0 3½	0 5½
Bass's Pale Ale (bottled)	9 bottles	0 10	7 6
Guinness's Stout	6¾ „	0 10	5 7½

Weight of various Articles of Food required to sustain Respiration and Circulation in the Body of an average Man during 24 hours.[*]

NAME OF FOOD.	Weight in oz. 437½ grs.	NAME OF FOOD.	Weight in oz. 437½ grs.
Cheshire Cheese . . .	3·0	Whiting	16·8
Potatoes	13·4	White of Egg	23·1
Apples	20·7	Hard-boiled Egg . . .	5·8
Oatmeal	3·4	Gelatin	3·6
Flour.	3·5	Milk	21·2
Peameal	3·5	Carrots	25·6
Ground Rice	3·6	Cabbage	31·8
Arrowroot	3·4	Cocoa-nibs.	1·9
Bread	6·4	Butter	1·8
Lean Beef	9·3	Cod-liver Oil	1·5
„ Veal	11·4	Lump-Sugar	3·9
„ Ham, boiled . . .	7·9	Commercial Grape Sugar	4·0
Mackerel	8·3		

These results are in many instances fully borne out by experience. The food of the agricultural labourers in Lancashire contains a large proportion of fat. Besides the very fat bacon which constitutes their animal food proper, they consume large quantities of so-called apple dumplings, the chief portion of which consists of paste in which dripping and suet are large ingredients; in fact these dumplings frequently contain no fruit at all. Egg and bacon

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[*] P. 162. On Helmholtz and Fisk's supposition, actual energy required equals 160,012 metrekilograms for internal work for twenty-four hours, and by page 172 one gram (= 15·43 grs.) of different substances give in natural condition so many metrekilograms of force.

pies and potatoe pies are also very common *pièces de résistance* during harvest-time, and whenever very hard work is required from the men. The speaker well remembers being profoundly impressed with the dinners of the navigators employed in the construction of the Lancaster and Preston Railway: they consisted of thick slices of bread surmounted with massive blocks of bacon, in which mere streaks of lean were visible. Dr. Piccard states that the chamois hunters of Western Switzerland are accustomed, when starting on long and fatiguing expeditions, to take with them, as provisions, nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. They doubtless find that in fat and sugar they can most conveniently carry with them a store of force-producing matter. The above tables affirm the same thing. They show that ·55 lb. of fat will perform the work of 1·15 lb. cheese, 5 lbs. potatoes, 1·3 lb. of flour or peameal, or of 3½ lbs. of lean beef.* Donders, in his admirable pamphlet *On the Constituents of Food and their Relation to Muscular Work and Animal Heat*, mentions the observations of Dr. M. C. Verloren on the food of insects. The latter remarks: "Many insects use during a period in which very little muscular work is performed food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen." He also mentions

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* 10 parts fat = 115 parts cheese = 900 parts potatoes = 114 parts flour = 804 parts lean beef.

bees and butterflies as instances of insects performing enormous muscular work, and subsisting upon a diet containing but the merest traces of nitrogen.

CONCLUSION.

11thly. We thus arrive at the following conclusions :—

1. The muscle is a machine for the conversion of potential energy into mechanical force.

2. The mechanical force of the muscles is derived chiefly, if not entirely, from the oxidation of matters contained in the blood, and not from the oxidation of the muscles themselves.

3. In man the chief materials used for the production of muscular power are non-nitrogenous ; but nitrogenous matters can also be employed for the same purpose, and hence the greatly increased evolution of nitrogen under the influence of a flesh diet, even with no greater muscular exertion.

4. Like every other part of the body, the muscles are constantly being renewed ; but this renewal is not perceptibly more rapid during great muscular activity than during comparative quiescence.

5. After the supply of sufficient albuminized matters in the food of man to provide for the necessary renewal of the tissues, the best materials for the production, both of internal and external work, are non-nitrogenous matters, such as oil, fat, sugar, starch, gum, &c.

6. The non-nitrogenous matters of food, which find their way into the blood, yield up all their potential energy as actual energy ; the nitrogenous

matters, on the other hand, leave the body with a portion (amount in urea one-seventh) of their potential energy unexpended.

7. The transformation of potential energy into muscular power is necessarily accompanied by the production of heat within the body, even when the muscular power is exerted externally. This is, doubtless, the chief and, probably, the only source of animal heat.

APPENDIX III.

ON THE RATE OF PASSAGE OF CRYSTALLOIDS INTO AND OUT OF THE VASCULAR AND NON-VASCULAR TEXTURES OF THE BODY.

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IT occurred to me that possibly, by means of the spectrum, I might trace the rate of passage of medicines into the vascular and non-vascular textures, and prove their presence, and determine the time during which they remain in action in some of the tissues, far more accurately than had yet been done.

I was fortunate enough to obtain the assistance of Dr. A. Dupré, who had already published a paper in the *Philosophical Magazine* on the presence of lithium and strontium in the waters of London; and I am greatly indebted to him for carrying out all the suggestions which I thought requisite for proving how soon the salts of lithia pass into the different vascular and non-vascular textures of animals and of man, and how quickly these salts again pass out and cease to be detectable in the different parts of the body.

I shall divide this paper into five sections:—

1. On the method of analysis, and its delicacy.

2. Experiments on animals to which salts of lithium were given, upon the rate of their passage into the textures.

3. On the rate of the passage of lithia-salts out of the textures.

4. On experiments on healthy persons, and on cases of cataract.

5. On the presence of lithium in liquid and solid food.

1. *On the Method of Analysis, and its Delicacy.*

Three methods of preparing the substance to be analyzed were followed, according as much or little lithia was present.

When plenty of lithia was present, it was immediately detected in the spectrum by simply touching the substance containing lithia with a red-hot platinum wire. In the case of liquids, a portion of a drop was taken up on the end of the wire, and it was then put into the gas-flame.

If no lithia was thus detectable, a larger or smaller portion of the substance was extracted by distilled water twice or thrice, and the liquid was evaporated to dryness, and the residue was then tested.

If very little lithia was present, it was necessary to incinerate a larger or smaller portion of the substance, and to treat the ash with sulphuric acid, to exhaust the resulting sulphates with absolute alcohol and evaporate the alcohol extract to dryness, and to test the residue for lithia.

Kirchhoff and Bunsen state that less than $\frac{9}{1,000,000}$ of a milligramme of carbonate of lithia = to about $\frac{1}{8,000,000}$ of a grain can be detected by the spectrum analysis.

To determine the delicacy of the test for the chloride of lithium, the following experiment was made :—One grain of chloride of lithium was dissolved in one litre of water. Of this solution 100 cub. centims. were taken and again diluted to one litre, this latter solution containing 0·1 grain of chloride of lithium to the litre.

When further diluted to five times its bulk, the lithium reaction was still seen faintly on a wire taking up 0·06 grain of solution. The line is most distinctly visible in the evening, in a somewhat dark room.

This dilution is equal to 0·1 grain of chloride of lithium in 5 litres of water, or 1 grain in 50 litres. One litre = 15,440 grains, or 50 litres = 772,000 grains. In 0·06 grain of this solution there are therefore 0·00000008 grain chloride of lithium, or about $\frac{1}{12,000,000}$th of a grain of chloride of lithium. This contains only $\frac{1}{6}$ part of lithium, so that the $\frac{1}{72,000,000}$th of a grain of metallic lithium, when pure, gives the spectrum reaction.

When the chloride of lithium was dissolved in urine, the test was from twice to six times less delicate than in distilled water.

2. *Experiments on Animals to which Salts of Lithium were given, upon the Rate of the Passage into the Textures.*

Experiment 1.—Two guinea-pigs were fed for several days on the same food. One was killed, and the urine, the nails, hair, blood, bones, muscles, nerves, cornea, and crystalline lens were examined by the spectrum, and no trace of lithium was found anywhere. The other was given half a grain of chloride of lithium for seven days, and for two days one grain. It was then killed, and the lithium was found everywhere, even in the cornea, crystalline lens, hair, and toe-nails. In these it was more distinctly present than anywhere else, so that it probably came from the urine.

Experiment 2.—Another guinea-pig, fed on the same food as the first two, was given only half a grain of chloride of lithium for three days. The third morning the lithium was detectable, by analysis, in the hair; the fourth day it was killed, and the lithium was found everywhere, as in the last instance.

Experiment 3.—Another, after the hair and nails had been examined for four days and no lithium found, was given three grains of chloride of lithium. In two hours and a half lithium was detected in the hair of the belly, though in six hours none was found in the hair of the back; much more was then in the hair of the belly. In twenty-six hours it was killed. Lithium was found everywhere—both in the outer and inner part of the lens very distinctly, and in the cartilage of hip- and knee-joints. The spleen and liver seemed to have less lithium than the vitreous and aqueous humour and the lens.

Experiment 4.—A guinea-pig was given three grains of chloride of lithium, and in twenty-four hours it was killed. Lithium was found in the cartilage of the hip- and knee-joints, in the centre of the lens, in the nails, and in the outer moisture of the eye.

Experiment 5.—To another, the hair of which gave no trace of lithium, were given three grains of chloride of lithium, and it was killed in eight hours; as usual, lithium was found in all the organs—by far the most in the kidneys. Little was found in the blood. It was quite evident in the cartilage of the hip-joint, and very distinct in the outer layer of the crystalline lens, but none at all could be found in the centre of the lens. Both lenses were examined more than six different times with the same result.

Experiment 6.—In a guinea-pig, much younger than the last, which was killed eight hours after three grains of chloride of lithium, the whole lens was penetrated—the smallest particle, even one-twentieth the size of a pin's-head, taken from each part of the lens, showing the lithium distinctly. The whole lens of another pig that had taken no lithium was burnt, and did not show the slightest trace of lithium.

Experiment 7.—Another guinea-pig was given three grains of chloride of lithium, and it was killed in four hours. Lithium was found in the fibrin, serum, and corpuscles of the blood, in the cartilage of the hip-joint, and in the lens, even in its most central part. There was scarcely any difference between the inner and outer part. The vitreous and aqueous humours showed much more evidence of lithium than the lens itself did.

Experiment 8.—A guinea-pig, the urine of which gave no trace of lithium, had three grains of chloride of lithium, and was killed in two and a half hours. The lithium was found in the cartilage of the hip-joint distinctly but faintly. The blood showed the lithium very distinctly, much more so than in any of the previous experiments. The outer portion of the lens showed lithium, though but slightly. The inner portions of the lens showed more. The vitreous and aqueous humours showed lithium very distinctly.

Experiment 9.—A large guinea-pig was given three grains of chloride of lithium, and it was killed in an hour. Lithium was found in the blood, urine, and nails very distinctly; in the cartilage of the hip- and knee-joints very faintly; in the vitreous and aqueous humours of the eye very distinctly. No lithium was found in the lens, not even when half the lens was taken for a single experiment. The stomach contained food.

Experiment 10.—Another guinea-pig was killed an hour after the same dose. The lithium was found strongly in the blood, bile, liver, and kidney. Traces occurred in the brain and in the cartilage of the hip-joint. It was present distinctly in the humours of the eye and in the lens. The difference between the inner and outer part of the lens was very marked. The second eye was not examined for more than fourteen hours after the first eye. After this time the centre of the lens contained as much lithium as the outer part did. The stomach contained water.

Experiment 11.—A young guinea-pig, fasting, was given three grains of chloride of lithium, and thirty-two minutes afterwards it was killed. Lithium showed faintly in the cartilage of the hip-joint; very distinctly in the humours of the eye; distinctly in the outer part of the lens, very faintly in the inner part; nearly the whole of the inner part had to be burnt to give the appearance. Lithium was very distinct in the blood, and remarkably so in the nails.

Experiment 12.—Another young guinea-pig, fed in the same way, and bought at the same place as the two former, was killed without taking any lithia. No lithium was found anywhere. The whole of the spleen, one kidney, and one lens were incinerated, and each ash was used for a single experiment, and in no instance was lithium found. There was no lithium in the cartilage of the hip-joint, nor in the blood, nor in the nails.

Experiment 13.—A very young and small guinea-pig that had been kept fasting for thirty-six hours, was given three grains of chloride of lithium, and it was killed in half an hour, the urine having been previously examined, and no lithium found in it. Very much lithium was found in the blood and in the urine; very slight traces in the cartilage and in the brain. The lens showed no lithium when incinerated entire, but the aqueous extract of the lens showed minute traces of lithium.

Experiment 14.—An old guinea-pig, also fasting for about thirty-six hours, was given the same quantity of chloride of lithium, and was also killed in half an hour. No lithium could be detected before the dose, in the urine, nor in the toe-nail of one leg. After taking the lithium, the animal was wrapped up in a cloth, the leg only being left out to prevent it from licking the toe; after death, the nails of this leg showed that some lithium was there. The sciatic nerve showed traces of lithium. The cartilage of the hip-joint, when touched with red-hot wire, showed no lithium, but scrapings from the surface showed traces of lithium. The humours of the eye showed traces of lithium, but the lens showed no lithium even in the watery extract. The brain showed only exceedingly faint traces of lithium. The stomach was almost completely empty.

Experiment 15.—A guinea-pig was kept fasting for twenty-four hours; it was then given three grains of chloride of lithium, and it was killed in a quarter of an hour. Lithium was found in the bile, liver, kidney, and blood, very distinctly; very faintly in the brain and in the cartilage of the hip-joint, and in the humours of the eye. None was found in the lens. The stomach contained only some water, no solid food.

Experiment 16.—Three fresh guinea-pigs were taken; one was killed without taking any lithium.

The urine showed no lithium in one drop, but the ash of the urine showed traces of lithium. No lithium could be detected in any of the organs, not even by treating the ash of the kidney with sulphuric acid and alcohol.

The two remaining animals were each given one quarter of a grain of chloride of lithium.

The first was killed in five and a quarter hours afterwards. All the organs, except the lens of the eye, showed lithium by simply touching them with the red-hot wire. The urine and the bile showed the lithium very distinctly. The blood showed lithium faintly. The vitreous and aqueous humours showed traces of lithium. An aqueous extract of the lens showed no lithium. The animal was a large and old one, and the stomach was nearly empty.

The second was killed twenty-four hours after one quarter of a grain.

None of the organs showed any lithium by simply touching them with a red-hot wire. The ash of the kidney showed traces of lithium, and so did the ash of part of the liver. No lithium could be detected either in the vitreous and aqueous humour, or in the lens; the urine and the bile showed lithium in one drop, but only faintly. Possibly the lithium had not been absorbed in this case. The state of the stomach, as regards food, was not recorded.

It follows from these experiments, that when no lithium is taken no lithium can be found in the different textures, but that even in a quarter of an hour three grains of chloride of lithium given on an empty stomach may diffuse into the cartilage of the hip-joint and into the aqueous humour of the eye. In very young and very small guinea-pigs the same quantity of lithium in thirty or thirty-two minutes may give traces of lithium in the lens; but in an old animal in this time it will have got no further than the aqueous humour. If the stomach be empty, in an hour the lithium may be very evident in the outer part of the lens. and very faintly in the inner part; but if the stomach be full of food, the lithium does not in an hour reach the lens. Even in two hours and a half, lithium may be more marked in the outer than in the inner part of the lens. In four hours the lithium may be in every part of the lens, but less evidence of its presence will be obtained there than from the humours of the eye. In eight hours even, the centre of the lens may show less than the outer part. In twenty-six hours the diffusion had taken place equally through every part of the lens. Even one quarter of a grain in twenty-four hours showed lithium everywhere except in the lens.

Experiment 17.—To endeavour to determine the different rate of absorption and excretion in young and old animals, four guinea-pigs were taken; two were young, and two were old. The four, after fasting for fifteen hours, were each given two grains of chloride of lithium. Two of them, one young and one old, were killed in six hours.

The young animal showed lithium distinctly in the outer and inner part of the lens, and also in the cartilage of the hip-joint, when touched with a red-hot wire. The stomach was about half full of food.

The old one showed lithium distinctly in the outer part of the lens, but scarcely the faintest trace in the inner part. The cartilage of the hip-joint showed lithium quite as distinctly as the cartilage of the young pig.

The other two guinea-pigs were kept. After forty-eight hours, the urine of both showed lithium very distinctly in one drop. Six days afterwards, the urine of the young animal still showed lithium faintly in each drop. The urine of the old one found in the bladder after its death showed lithium faintly in each drop.

Both were killed on the sixth day, and no lithium could be detected in the alcoholic extracts of the kidneys, livers, or lenses of either.

A short series of experiments were made with the view of determining the rate at which the salts of lithium diffuse into the textures when the lithium is injected into the skin instead of passing through the stomach.

Three grains of chloride of lithium in solution were injected into the skin of the back of the neck of a guinea-pig, and the animal was killed in twenty-four minutes.

The urine, bile, kidney, and liver showed lithium very distinctly. The cartilage of the hip-joint showed lithium distinctly when touched with a red-hot wire. The aqueous humour showed lithium very distinctly, but the lens, when washed, showed only a very minute trace of lithium when the entire lens was taken at one time on the wire. The toe-nails showed lithium very distinctly.

Another had three grains injected under the skin of the neck, and it was killed in ten minutes.

The humours of the eye showed lithium distinctly, but the aqueous humour showed decidedly more than the vitreous humour. The incinerated aqueous extract of the lenses showed lithium very faintly. The large nerves of the leg also showed lithium very faintly.

Another guinea-pig had a grain and a half of chloride of lithium injected under the skin of the neck, and in five minutes it was killed. The aqueous humour showed lithium distinctly. The vitreous humour showed none. The blood and bile showed lithium very distinctly. The kidney and urine showed lithium faintly, and the liver very faintly.

In another pig three grains of chloride of lithium were injected into the skin of the neck, and the animal was killed in four minutes.

The blood and the bile showed lithium very distinctly; the blood showed it rather more than the bile. The bladder contained only a few drops of urine, which showed lithium distinctly. The kidney showed lithium fairly well. The liver showed the lithium only very faintly, and in some parts not at all.

The aqueous humour showed lithium distinctly The vitreous humour showed no lithium.

So that, when injected under the skin—

3 grains in twenty-four minutes gave lithium in the lens and everywhere.

3 grains in ten minutes gave lithium in the lens and everywhere.

1½ grain in five minutes gave lithium in the aqueous humour and in the bile.

3 grains in four minutes gave lithium in the aqueous humour and in the bile.

3. *Experiments on the Rate of Passage of the Lithium out of the Textures.*

Experiment 18.—Five guinea-pigs were given two grains of chloride of lithium each. They were killed at different periods; the first in six hours. The smallest particle of the lens showed the lithium very distinctly; a decided difference, however, was detectable between the inner and the outer part. The cartilage of the hip-joint showed lithium very distinctly when touched with the red-hot wire. All the organs and the blood showed lithium very abundantly. The stomach contained very little solid food, but was half full of liquid. The second and third were killed in twenty-four hours. The lenses of both showed the lithium very distinctly; no difference was perceptible between the inner and the outer part. The cartilage of the hip-joint showed no lithium when touched with the red-hot wire; but a small portion taken off the surface showed lithium distinctly.

The fourth guinea-pig was killed in forty-eight hours. The lens showed lithium very distinctly. A small piece taken from the cartilage of the hip-joint showed only traces of lithium.

The fifth was killed in ninety-six hours. The lens showed no lithium even when a considerable proportion of it was taken for one experiment. The aqueous extract of half one lens showed no lithium. A small portion of the cartilage of the hip-joint showed no lithium. The urine showed lithium very distinctly even in one drop.

Experiment 19.—Six fresh guinea-pigs were taken. The first was killed and examined, having had no lithium. The two lenses, incinerated and treated with sulphuric acid and alcohol, showed no lithium. The ash of the kidney showed no lithium directly, but when treated with sulphuric acid and alcohol, showed a distinct trace of lithium.

The five others were given each one grain of chloride of lithium.

The first was killed five and a half hours after the dose. The cartilage of the hip-joint showed lithium faintly when merely touched with a red-hot wire. The lens showed lithium distinctly in the outer part, scarcely a trace in the inner part. The vitreous and aqueous humours showed lithium very distinctly. The stomach was quite full.

The second was killed twenty-four and a half hours after the lithium was taken. The cartilage of the hip-joint showed no lithium even in a small particle scraped off the surface. The lens still showed lithium distinctly, though less so than in the first; no difference was perceptible between the inner and the outer portion.

The third was killed in forty-eight hours. The lens showed no lithium in a small particle taken on a loop of the wire. The aqueous extract of the lens showed only faint traces of lithium. The urine showed lithium very distinctly in a single drop.

The fourth was killed in seventy-two and a half hours. The lens showed no lithium when the ash was treated with sulphuric acid and alcohol. The ash of the kidney showed no lithium directly, but when treated with sulphuric acid and alcohol, showed traces of lithium. The urine still showed lithium distinctly in one drop.

The fifth guinea-pig: on the seventh day after the dose, the urine showed lithium in one drop; ninth day, still faint traces of lithium in the urine; eleventh day, urine directly shows no lithium, but the ash still shows faint traces; thirteenth day, ash of urine shows no lithium, but alcoholic extract shows lithium distinctly; fourteenth day the same; sixteenth day the same; thirty-sixth day, when killed, no lithium could be detected in the bones, nerves, lens, or vitreous or aqueous humours, nor in the urine, kidney, or liver.

Experiment 20.—Two guinea-pigs, in the urine of which no lithium could be detected, were given each half a grain of chloride of lithium.

In three hours and fifty minutes afterwards one was killed. The cartilage of the hip-joint showed no lithium when simply touched with a red-hot wire. Scrapings from the surface of the cartilage showed faint traces of lithium. The sciatic nerve, humours of the eye, and the brain showed faint traces. The muscles of the thigh showed the lithium much more distinctly than the sciatic nerve. The lens showed lithium very distinctly in the aqueous extract, but not otherwise. The blood and bile were very rich in lithium. The stomach was moderately full of food.

The other animal, which was given half a grain, was kept until the lithium ceased to appear in the urine.

Fourth day. Lithium distinctly in the urine.

Tenth day. Urine showed exceedingly minute traces of lithium.

Eleventh day. Still traces.

Thirteenth day. Urine shows no lithium in the quantity adhering to the wire.

Fourteenth day. Still lithium in the alcoholic extract.

Twenty-seventh day. Still traces of lithium.

Thirtieth day. The animal was found dead.

The ash of the urine found in the bladder (about a quarter of an ounce) showed no lithium. The alcoholic extract of the ash showed lithium faintly. The alcoholic extract of the ash of one kidney showed no lithium. And the alcoholic extract of the two lenses showed no lithium.

Experiment 21.—Two guinea-pigs, the urine of which contained no lithium, were each given one quarter of a grain of chloride of lithium.

One was killed in four hours and thirty-five minutes. Lithium was found very faintly in the spleen, very distinctly in the blood, in the urine, and in the bile. Faintly in the sciatic nerve and in the brain. Very faintly in the scrapings of the cartilage. Pretty distinctly in the vitreous and aqueous humours, but very faintly in the aqueous extract of the lens. The stomach was moderately full.

The other was kept until the lithium ceased to appear in the urine.

Second day. Lithium very distinctly.

Fourth day. Minute traces of lithium.

Sixth day. A drop or two of urine shows no lithium, but on evaporating and incinerating one-twelfth of an ounce, the ash shows lithium very distinctly.

Seventh day. Lithium still distinct in the ash.

Eighth day. Still in the ash.

Tenth day. Ash of urine shows only the merest trace.

Eleventh day. Ash of urine shows no lithium; but when treated with sulphuric acid and alcohol, lithium is still distinct.

Thirteenth day. Still lithium in one quarter of an ounce.

Fourteenth day. Alcoholic extract from one-eighth of an ounce shows no lithium.

Sixteenth day. The animal was killed. The fluids and organs were incinerated, the ash treated with sulphuric acid, excess of acid driven off and the dry residue extracted with absolute alcohol, alcoholic extract evaporated, and dry residue tested. The two lenses gave extremely feeble traces of lithium. One-eighth of an ounce of urine and bile gave traces of lithium. Ninety grains of liver gave traces. One quarter of an ounce of blood gave no lithium. An entire kidney, weighing ninety grains, distinctly contained lithium.

Experiment 22.—Two guinea-pigs, the hair and nails of which showed no lithium, were given each three grains of chloride of lithium.

In the first, in two hours no lithium was in the hair. In four hours lithium was in the hair of the belly, but scarcely perceptible in the hair of the head. In twenty-four hours it was very distinct in the hair of the belly and the head, and in the nails. For five days it was detected in each drop of the urine. Ten days afterwards the urine showed lithium very distinctly; only after thirty-two days was lithium absent from a few drops of the urine. The thirty-third day after the dose the animal was killed. No lithium was found in the bile, liver, blood, lens, kidneys, spleen, or other parts, by simply taking a small piece of the organ on a red-hot wire. The evaporated aqueous extract of the two lenses showed no trace of lithium; when, however, the two kidneys were incinerated, the ash treated with sulphuric acid, the resulting sulphates exhausted with absolute alcohol and the alcoholic extract evaporated to dryness, lithium was easily detected in the residue. A portion of the liver, treated in the same manner, also yielded lithium.

The second guinea-pig gave traces of lithium in the urine when one-eighth of an ounce was evaporated and treated with sulphuric acid and alcohol, thirty-nine days after the lithium was taken.

It follows from these experiments on the rate of passage of lithium into and out of the body, that—

With three grains of chloride of lithium, a young guinea-pig in half an hour had lithium in the watery extract of the lens. An old guinea-pig in the same time had no lithium in the lens.

With two grains, a young guinea-pig in six hours had lithium distinctly in all parts of the lens. An old guinea-pig had in the same time scarcely any lithium in the inner part, but some in the outer part of the lens.

With the same quantity, in six days neither a young nor an old guinea-pig gave any trace of lithium in the alcoholic extract of the kidney, liver, or lenses.

When two grains of chloride of lithium were taken, after six hours the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours no difference in the different parts of the lens was detectable. In forty-eight hours still no difference was observed. In ninety-six hours (four days) no lithium was detectable in the lens or in a cartilage of a joint; still the urine showed lithium very distinctly even in one drop.

After one grain of chloride of lithium, in five hours and a half the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours and a half there was no difference throughout the lens. In forty-eight hours the watery extract of the lens showed faint traces of lithium. In seventy-two hours and a half (three days) the alcoholic extract of the lens showed no lithium. The urine still showed lithium distinctly in one drop, and continued to do so for seventeen days in the alcoholic extract.

After a quarter of a grain, in five hours and thirty-five minutes lithium was distinct in the vitreous and aqueous humours, and very faintly in the lens. After sixteen days, the minutest traces of lithium were detected in the lens, the liver, and the kidneys, but no trace could be found in the blood. (This animal had perhaps somehow eaten the minutest quantity of lithia in the food.)*

After half a grain of chloride of lithium, in three hours and fifty minutes traces of lithium could be found in the lens, and for thirty-seven or thirty-eight days traces of lithium could be found in the urine.

After three grains of chloride of lithium, in four hours lithium was in the hair of the belly, and for thirty-two days the urine showed lithium very distinctly. The thirty-third day after the lithium the lens was found to contain minute traces of lithium, and after thirty-nine days the lithium was in the alcoholic extract of the urine.

4. *Experiments on the Rate of Passage of Lithium through the Human Body, and into and out of the Crystalline Lens.*

Experiment 1.—A man took ten grains of carbonate of lithia dissolved in water, four hours after his midday food.

In five minutes no lithium could be detected in the urine.

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* The skin of guinea-pigs throws off lithium, and it collects on the hair and nails, so that it is possible for the animal to redose itself with lithium from its own body, and thus to keep lithium passing in and out of the textures much longer than if a single dose only were taken.

In ten minutes lithium was evident.

In eighteen hours it was present in the nails of the hands and feet, and in the hair of the beard and body; apparently most where there was most perspiration. No lithium could be found in the hair of the head or whiskers.

In forty-two hours very perceptible.

In sixty-six hours another dose of ten grains was taken.

In ninety hours lithium was detectable in the hair of the head.

For three days after the second dose it was perceptible in one drop of the urine, but rather doubtful in the hair and in the nails.

For six days after the second dose lithium was detectable in the urine.

For eight days after, no lithium could be detected when the eighth of an ounce of urine was evaporated.

Twelve days afterwards, though no lithium was in the urine or the hair of the head or whiskers, it was detectable in the hair of the body.

Experiment 2.—The same man three hours after breakfast took ten grains of carbonate of lithia.

In five minutes lithium was just perceptible in the urine.

In ten minutes extremely distinct in one drop of the urine.

In twenty-four hours very distinct in the urine.

Fourth day. Traces of lithium when the urine was concentrated by evaporation.

Fifth day. Less perceptible in evaporated residue.

Sixth day. No lithium could be detected in evaporated urine.

Experiment 3.—The same man took ten grains of carbonate of lithia after fasting for seven hours. The urine was passed every second minute after taking the lithia.

Second, fourth, and sixth minute, no lithium.

Eighth minute, traces of lithium very slight.

Tenth minute, lithium distinctly present.

Third day afterwards lithium very distinct.

Fourth day. Lithium faintly found in each drop of urine.

Fifth day. Lithium very faint in each drop.

Sixth day. Only the merest trace.

Seventh day. No lithium in the eighth of an ounce evaporated to a few drops.

Eighteenth day. Two ounces of urine incinerated, and the ash heated with sulphuric acid and alcohol, showed no lithium.

Twenty-first day. Nails of the hands and feet still show lithium.

Experiment 4.—The same man, two hours and a half after a little food, took ten grains of chloride of lithium: one nail on the hand and one on the foot were varnished before taking the lithium.

Second and fourth minute afterwards no lithium was in the urine.

Sixth minute, traces of lithium.

Eighth minute, distinctly present.

Tenth minute, very distinctly.

Twenty-five hours afterwards none of the nails showed any lithium.

Forty-four hours. Scrapings of the unvarnished nails on the hands and feet showed lithium distinctly. The further the scraping was carried the less lithium was found. The scrapings of the varnished nail of the hand shows only traces of lithium. The varnished nail of the foot has no lithium. A small particle of the skin from the hand or foot shows lithium distinctly. Perspiration shows lithium distinctly. The hair of the head or whiskers shows no lithium.

Third day. Nails the same as yesterday. The unvarnished nails show lithium; the varnished, none. Urine shows lithium most distinctly.

Fourth day. Urine shows lithium in one drop.

Fifth day. Urine still shows minute traces of lithium in one drop.

Sixth day. Urine shows no lithium. Ash of the urine shows faint traces of lithium.

Seventh day. Ash of urine shows no lithium. Alcoholic extract shows traces.

Eighth day. Alcoholic extract from one ounce of urine still shows traces of lithium.

Ninth day. Alcoholic extract from one ounce of urine shows no lithium.

Experiment 5.—A boy, aged sixteen years, took five grains of chloride of lithium, and the urine was passed every second minute. Half an hour previously he had eaten some bread and butter. No lithium could be detected in the urine previous to the taking of the dose.

Second minute, no lithium in the urine.

Fifth minute, none.

Ninth minute, none.

Tenth minute, very faint traces of lithium.

Thirteenth minute, lithium very distinctly present.

After twenty-four hours lithium still very distinct.

Second, third, fourth, and fifth day. Still very distinct.

Seventh day. No lithium was found in the evaporated residue. The ash of the residue shows very slight traces.

Eighth day. The alcoholic extract from one ounce of urine shows no lithium.

Experiment 6.—The same boy had his hair, nails, and urine examined, and no lithium was found.

Five grains of carbonate of lithia were then given to him.

In two minutes, five minutes, ten minutes, no lithium was found in the urine.

In twenty minutes lithium was distinctly present.

In eighteen hours the lithium was found in the nails, none in the hair of the head.

In thirty-two hours, still none in the hair of the head. Very distinctly in the root and tip of the nails.

Another five grains of carbonate of lithia were then given.

In nineteen hours lithium was detected in the hair of the head.

In four days a drop of the urine showed lithium very distinctly, as did the hair and nails.

In five days the same.

In seven days one drop of the urine shows no lithium, but if the urine is slightly concentrated by evaporation, lithium is still perceptible.

In eight days one-eighth of an ounce evaporated still shows slight traces.

In nine days no lithium in one-eighth of an ounce of urine.

Experiment 7.—The same boy took five grains of carbonate of lithia, but he omitted previously to empty his bladder.

In five minutes, lithium not yet detectable in the urine.

In ten minutes lithium very distinctly present in one drop of the urine.

Four days afterwards traces of lithium still in the urine.

Five days afterwards slight traces in one-eighth of an ounce of urine when evaporated.

In six days afterwards no lithium perceptible in the urine.

Experiment 8.—Twenty-five grains of chloride of lithium were dissolved in one gallon of water, and the feet and ankles of a man were kept in the solution for two hours; at the end of this time the urine was passed and examined for lithium, and no trace could be found in the aqueous extract of the ash of one quarter of an ounce of urine.

These experiments agree very closely with some which I made many years since on a full-grown German who had an open bladder, admitting the urine to be caught as it came from the kidneys.

Feb. 24, 1852. At 8.45 A.M. he took two cups of black coffee and nothing else.

9.30 to 9.50. Urine was caught, and it contained no trace of iron.

9.50. Protosulphate of iron, 6·7 grains, free from persulphate was taken in two ounces of distilled water.

9.55. Urine caught and contained no iron.

10. No iron as peroxide. Present as protoxide.

10.5. Slightest trace of peroxide. Protoxide distinct.

10.10. Slightest trace of peroxide. Protoxide distinct.

10.20. Slightest trace of peroxide. Protoxide less distinct.

10.30. Slightest trace of peroxide. Protoxide less distinct.

10.40. Slightest trace of peroxide. Slightest trace of protoxide.

11. Slightest trace of peroxide. No trace.

11.10. Slightest trace of peroxide. No trace.

Feb. 26. The same patient. At 8 A.M. two cups of black coffee and nothing else.

10.20 to 10.30. Urine caught and no trace of iron found.

10.30 Sulphate of protoxide of iron four grains, given in one ounce of distilled water.

10.31. No trace of iron in the urine.

10.34. No trace.

10.35. No trace.

10.36. No trace.

10.37. Slightest trace of protoxide of iron in the urine. No peroxide.

10.39. No trace.

10.40. No trace.

March 2nd. The same patient. At 8 A.M. two cups of black coffee and nothing else.

9.34 to 9.40. Urine collected and no trace of iron found.

9.40. Sulphate of protoxide given, seven grains in two ounces of distilled water.

9.42½. None.
9.45. None.
9.47½. None.
9.50½. A trace.
9.52½. A trace.
9.55. Good.
9.57½. Doubtful.
10. Doubtful.
10.5. More distinct.
10.10. Doubtful.
10.15. Doubtful.

March 19. At 8 A.M. two cups of black coffee without milk, nothing else taken.

Urine from 9.50 to 9.56 collected; contained no iodine. One grain of iodide of potassium dissolved in one ounce of water was then taken.

9.58. No iodine.
9.59. No iodine.
10. None.
10.1. None.
10.2. None.
10.3. None.
10.4. None.
10.5. None.

10.6. None.
10.8. Trace of iodine.
10.10. Very marked iodine.
10.15. Very marked.

So that one grain of iodide of potassium in one ounce of water was detected in the urine in twelve minutes, and was very marked in fourteen minutes. Iron was detected once in seven minutes and twice in ten minutes, and it was very distinct in fifteen minutes.

Professor Mulder also made many experiments on this patient, but I am unable to find any account of his results.

In the *Medical Gazette* for 1845, pp. 363 & 410, Mr. Erichsen gives some experiments he made on a boy of thirteen who had an open bladder. He states that twenty grains of ferrocyanide of potassium were detected in one minute in the urine. The stomach was fasting, and the salt was dissolved in three ounces of water. Forty grains taken three quarters of an hour after a full meal were only detected after thirty-nine minutes.

Forty grains in four ounces of water were twice detected in two minutes, and no trace could be found after twenty-four hours; once in two minutes and a half; once in six minutes and a half; once in fourteen minutes; once in twenty-seven minutes; and once in thirty-nine minutes.

Twenty grains of ferrocyanide he once detected for twenty-eight hours.

It follows from these experiments that ten grains of carbonate or chloride of lithium, taken two and a half, three, or four hours after food by a man, require between five and ten minutes to pass from the stomach to the urine, and this quantity of carbonate or chloride of lithium will continue to produce traces of lithium in the urine from six to seven, or even eight days.

Five grains of chloride or carbonate of lithia, taken shortly after food by a boy, gives no appearance in the urine until from ten to twenty minutes, and this quantity continues to pass out for five, seven, or eight days.

Experiments made by the ordinary mode of analysis showed that—

Four grains of sulphate of the protoxide of iron, taken almost fasting by a man, gave a trace in the urine in seven minutes.

Seven grains gave distinct appearance in ten minutes and ten minutes and a half.

One grain of iodide of potassium, taken by the same man fasting, appeared in the urine in twelve minutes.

Experiments on the Rate of Passage of Lithium into and out of the Crystalline Lens.

Through the kindness of Mr. Bowman and Mr. Critchett at the Moorfields Ophthalmic Hospital, lithia water, containing variable quantities of lithia, was given to different patients about to be operated on for cataract.

Experiment 1.—The hard cataracts from two patients who had taken no lithia water were examined; no trace of lithium could be found in either lens.

Experiment 2.—The hard cataracts from two other patients who had no lithia water were examined; an aqueous extract of each lens was made; one showed the most excessively feeble lithium line; the other lens did not give the slightest indication.

Experiment 3.—The hard cataracts of two other patients who had taken no lithia water were examined; the alcoholic extract of the ash, after treatment with sulphuric acid, showed no lithium in either lens.

The lens of a third patient was examined when no lithia water had been taken, and the alcoholic extract showed no lithium.

Experiment 4.—The lens of a man aged seventy was extracted twenty-five minutes after he had taken twenty grains of carbonate of lithia in water on an empty stomach; no lithium could be detected in the lens.

Experiment 5.—A woman, æt. sixty-four, at 9 A.M. took twenty grains of carbonate of lithia in water; both lenses were extracted at 11½ A.M. the same day. Neither of the lenses showed any lithium when touched with a red-hot wire, but the aqueous extract of one lens showed lithium faintly, and the aqueous extract of the other lens showed lithium distinctly.

Experiment 6.—An eye was removed three hours after twenty grains of carbonate of lithia had been taken; the lens was removed half an hour afterwards, and on examination every portion of the lens contained lithium. The circulation through the eye had been healthy, and the lens itself was clear.

Experiment 7.—The soft lens of a girl aged fourteen was examined after ten grains of carbonate of lithia in water had been taken five hours before the operation, and the same quantity four hours before extraction.

The smallest fraction of the lens showed the lithium distinctly.

Experiment 8.—Another patient with two soft cataracts took twenty grains of carbonate of lithia seven hours before one operation; but the capsule of the lens had been previously broken, so as to expose the cataract to the aqueous humour.

Lithium was found very distinctly even in the smallest particle of the cataract.

Four days after the first operation the capsule of the other lens was broken, so as to expose the cataract to the aqueous humour; and seven days after the first operation the second operation was performed.

In this cataract not the slightest trace of lithium could be found.

A woman with diseased heart drank some lithia water, containing fifteen grains of citrate of lithia, thirty-six hours before her death; and six hours before death she drank the same quantity.

After death the crystalline lens, the blood, and the cartilage of one joint were examined for lithium.

The cartilage showed lithium very distinctly; the blood showed lithium very faintly; and when the entire lens was taken for a single examination, the faintest possible indications of lithium were obtained.

Another patient five and a half hours before death drank lithia water containing ten grains of carbonate of lithia.

After death the cartilage of one joint and the crystalline lens were examined.

The cartilage showed lithium very distinctly. When half the lens was taken for a single analysis, only very faint traces of lithium could be found.

When no lithia had been taken, seven cataracts were examined most carefully, and one only showed an exceedingly feeble trace of lithium.

When twenty grains of carbonate of lithia were taken twenty-five minutes before the operation, the lens showed no lithium; the same quantity taken two and a half hours before, showed lithium in the watery extract; three and a half hours before, showed lithium in each particle; between four and five hours before, the same; seven hours before, the same; seven days before, not the slightest trace of lithium.

Thirty grains of carbonate of lithia, taken between six and thirty-six hours before death, showed the faintest indications of lithium in the lens.

Ten grains of carbonate of lithia, taken five and a half hours before death, gave only faint traces of lithium in the lens.

On the Passage of Solutions of Lithium through the Textures after Death.

A sheep's eye was examined after death and no lithium could be detected in any part. Two other eyes were placed in a solution of chloride of lithium containing one grain to one litre of water. Twenty-three hours afterwards the lithium was found to have penetrated through the entire eye. There was, however, a perceptible difference between the amount of lithium in the inner and outer part of the lenses.

Two other sheep's eyes had a small portion of the cornea in front and the sclerotic removed at the back; they were then placed in a moderately strong solution of chloride of lithium, and the aqueous humour was examined from time to time.

After eighteen hours the aqueous humour showed lithium distinctly, and when the lens was extracted the lithium was found throughout its substance.

Two other eyes were placed whole in a solution containing one tenth of a grain of chloride of lithium to one litre of water.

In twenty-four hours the lithium had penetrated the entire eye. No difference was perceptible in different parts of the lens.

The rate at which a solution of chloride of lithium diffused through the stomach of a fresh-killed guinea-pig which had taken no lithium was determined.

A solution of one grain of chloride of lithium in twenty grains of water was put into the stomach, and it was hung up so that the the solution gravitated to the lowest part. The outer side of the stomach opposite the solution was touched from time to time with a loop of platinum wire, which was afterwards tested for lithium.

In first minute. No lithium came through the stomach.

In second minute. No lithium.

In third minute. No lithium.

In fourth minute. No lithium.

In fifth minute. No lithium.

In sixth minute. Traces of lithium.

In seventh minute. Traces of lithium.

In eighth minute. Lithium was very distinct.

The stomach of another guinea-pig was filled with a solution of lithium containing one grain of lithium in about half an ounce of water. The stomach was entirely filled and laid flat on a plate. The ends of the stomach and round the side showed lithium coming through in four minutes. The upper part of the stomach showed the lithium coming through in fifteen minutes.

5. *On the Presence of Lithium in Solid and Liquid Food.*

An ounce of each substance was taken. It was dried or evaporated, and incinerated carefully at a low red heat in a muffle on a platinum tray. The ash was tested for lithium first by taking a small fraction on a loop of a platinum wire into the flame of the spectroscope. When no lithium was thus detected, the ash was treated with sulphuric acid, and heated to expel the excess of acid; the dry residue was extracted with absolute alcohol, the solution filtered, evaporated to dryness, and the residue taken up in a drop of water and tested by the spectroscope.

Potatoes.	In Ash direct.	In Alcoholic Extract.
No. 1.	No lithium.	No lithium.
2.	"	"
3.	"	"
4.	"	Lithium distinctly.
5.	"	No lithium.
Apples.		
No. 1.	"	"
2.	"	"
3.	"	Lithium distinctly.
4.	"	Trace of lithium.
Carrots.		
No. 1.	"	No lithium.
2.	"	"
Bread.		
No. 1.	"	Slight traces of lithium.
2.	"	Traces of lithium.
3.	"	Lithium distinctly.
Savoy Cabbage.		
No. 1.	Lithium distinctly.	
2.	No lithium.	Lithium shown distinctly.
Tea.		
No. 1.	"	"
2.	"	Lithium very faintly.
3.	"	Lithium very distinctly.
4.	"	Faintly.
5.	"	"
6.	"	"
7.	"	No lithium.

Tea.	In Ash direct.	In Alcoholic Extract.
No. 8.	No lithium.	Faintly.
9.	"	No lithium.
10.	"	Very distinctly.
Coffee.		
No. 1.	"	Very faintly.
2.	"	"
3.	"	No lithium.
4.	"	Lithium distinctly.
5.	"	Very distinctly.

Wines: in almost all cases the ash gave direct evidence of the presence of lithium.

Port Wines.		
No. 1.	Small traces of lithium.	
2.	Faintly.	
3.	Very faintly.	Lithium exceedingly distinct.
4.	"	"
5.	No lithium.	Very faintly.
6.	"	Very distinctly.

Sherry.

No. 1. Lithium extremely brightly.

2. Faintly.

3. Exceedingly faintly.

4. Very faintly.

5. Faintly.

6. Distinctly.

French Wines.		
No. 1.	(red).	Lithium very distinctly.
2.	(white).	Extremely distinctly.
3.	(champagne).	Very brightly.
4.	"	"

Rhine Wines.

No. 1. Lithium exceedingly faintly.

2. "

3. Lithium distinctly.

Rhine Wines.	In Ash direct.	In Alcoholic Extract.
No. 4.	Very faintly.	
5.	Distinctly.	
6.	Faintly.	
7.	Distinctly.	
8.	Faintly.	
Ale.		
No. 1.	No lithium.	Lithium faintly.
2.	,,	No lithium.
3.	,,	Lithium faintly.
Porter.		
No. 1.	,,	,,
2.	,,	No Lithium.
3.	,,	Lithium distinctly.

In the *Philosophical Magazine*, vol. xx., Messrs. A. and F. Dupré gave the spectrum analysis of London waters. All the different waters examined gave lithium. The shallow waters appear to be richer in lithium than the deep-well waters. The different waters examined were: Thames water at high and low tide at Westminster Bridge; the water from Chelsea and Lambeth Water-Companies; New River water; Duck Island well, in St. James's Park; Pump in Lincoln's-Inn. These were above the London clay.

Burnett's Distillery and Whitbread's Brewery: from the sand above the chalk.

Guy's Hospital well and Trafalgar Square well: from the chalk.

	In Ash direct.	In Alcoholic Extract.
Entire sheep's kidney.	No lithium.	Very faint traces.
One ounce of kidney.	,,	No lithium.
One ounce of mutton.	,,	,,
One ounce of beef.	,,	,,

It appears from these experiments that

Potatoes showed lithium once in five trials.
Apples „ „ twice in four trials.
Carrots „ no lithium in two trials.
Bread „ lithium thrice in three trials.
Cabbage „ „ twice in two trials.
Tea „ „ eight times in ten trials.
Coffee „ „ four times in five trials.
Port wine „ „ six times in six trials.
Sherry „ „ six times in six trials.
French wine „ „ four times in four trials.
Rhine wine „ „ eight times in eight trials.
Ale „ „ twice in three trials (traces).
Porter „ „ twice in three trials (traces).

Mutton, beef, and sheep's kidney showed no lithium: one kidney had a slight trace of lithium.

I hope in a future paper, with the help of Dr. Dupré, to show that Thallium, Rubidium, and Cæsium, by spectrum analysis, can be traced even into the crystalline lens, and to determine the rate at which they pass in, if not out of, the textures; and by other means we shall endeavour to trace the passage of other crystalloids throughout the textures.

Conclusions.

1. *On the Rate of Passage of Solutions of Lithium into the Textures of Animals.*

In guinea-pigs, even in a quarter of an hour after three grains of chloride of lithium are taken into the stomach, the lithium may be found not only in all the vascular textures, but even in the cartilage of the hip-joint, and in the humours of the eye. If the same quantity is injected into the skin, in ten minutes it can be detected in the lens and everywhere; and even in four minutes the lithium may be detected everywhere except in the lens.

In half an hour after the same quantity is taken into the stomach, lithium may be found in the crystalline lens.

After it has been taken eight hours, it may not have passed completely into the inner part of the lens.

In twenty-six hours it will be found in every part of the lens.

When half a grain only of chloride of lithium was taken, in less than four hours traces were found in the lens. And even when only a quarter of a grain was taken, faint traces of lithium were found in five and a half hours.

2. *On the Rate of Passage of Solutions of Lithium out of the Textures of Animals.*

After two grains of chloride of lithium, in six days neither a young nor an old guinea-pig gave any lithium in the kidney, liver, or lenses.

After two grains, in four days no lithium could be found in the lens, nor in the cartilage of a joint.

After one grain, in three days the alcoholic extract of the lens showed no lithium.

After a quarter of a grain of chloride of lithium, in sixteen days the minutest traces of lithium were detected in the liver, kidneys, and lens.

After half a grain, for thirty-seven or thirty-eight days traces of lithium could be found in the urine.

After three grains, traces were found in the lens for thirty-three days, and for thirty-nine days the smallest quantity could be found in urine.

The skin of guinea-pigs throws off lithium, and it collects on the hair and nails; so that it is possible for the animal to redose itself with lithia from its own body, and thus to keep lithia passing in and out of the textures much longer than if a single dose only were taken.

3. *On the Rate of Passage of Solutions of Lithium in and out of the Human Body.*

In ten-grain doses, lithium may be found in the urine in from five to ten minutes, and continue to pass out for six or seven days.

In five-grain doses it may be in the urine in from ten to twenty minutes, and continue to pass out even for eight days.

In twenty-grain doses, it may be found in small quantity in the crystalline lens in two and a half hours, and be present in every particle of the lens in three and a half, five, and seven hours; and no trace of lithium may be detectable in the lens after seven days, when twenty grains of the carbonate of lithia had been taken.

4. *Results of the Examination of Solid and Liquid Food.*

Although almost every kind of vegetable food, and almost every fluid which we drink, contains infinitesimal quantities of lithia, yet rarely, if ever, can lithium be detected in any part of the body of man or animals, unless some larger quantity is taken than ordinarily occurs in the food or drink.

APPENDIX.—Received July 8, 1865.

On the Passage of Chloride of Rubidium into the Textures.

A guinea-pig was given three grains of chloride of rubidium at 11 A.M. At 6.30 P.M. it was killed. Rubidium was not detectable anywhere; not even satisfactorily in the urine.

Another guinea-pig was given ten grains of chloride of rubidium at 11.20 A.M. At 3 P.M. scarcely any rubidium could be detected in the urine. The following day, at 11 A.M., it was given five grains more. At 2 P.M. rubidium was just detectable in the urine. The next day, at 2 P.M., it was again given five grains, the rubidium being just perceptible in the urine. Twenty-five hours afterwards it was killed.

Extremely minute traces of rubidium were found in the kidney and in the blood; somewhat more, but still very faint traces, in the liver. In the cartilages no rubidium could be found, nor in the aqueous humour of the eye. When the whole lens was incinerated at once the smallest possible trace of rubidium was found. The urine showed traces of rubidium.

An elderly man took nineteen grains of chloride of rubidium four hours before he was operated on for cataract. The most careful search could not find any rubidium in the lens after its removal.

Another patient, with a double cataract, was given twenty grains of chloride of rubidium. One lens was extracted ten hours afterwards, and the other seven days afterwards, but in neither could traces of rubidium be found.

It was found by experiment that $\frac{1}{16000}$ of a grain of chloride of rubidium in water was detectable by the spectrum analysis. $\frac{1}{8000}$ of a grain in urine could be distinctly observed.

On the Passage of Chloride of Cæsium into the Textures.

Delicacy of the reaction for Cæsium.—One grain of chloride of cæsium in 400 cub. centims. of water just gives the blue cæsium lines in a quantity of solution that can adhere to the loop of a platinum wire which took up 0.05 of solution. The $\frac{1}{125000}$ part of a grain of chloride of cæsium in water can be detected. If potassium is present in the same solution the test is much less delicate.

In urine, one grain of chloride of cæsium in 200 cub. centims. is the limit of the reaction for a quantity remaining on the loop of the same wire as was previously used. Hence $\frac{1}{62500}$ of a grain of chloride of cæsium in urine can be detected.

A guinea-pig was given three grains of chloride of cæsium, and twenty hours afterwards another three grains. Twenty hours after the second quantity it was killed. The ash of the urine showed cæsium slightly. No cæsium could be detected in the two lenses taken for one examination; nor in the liquid humours of the eyes. A small portion of the ash of the kidneys and liver showed no cæsium, but aqueous extracts, after concentration, showed cæsium faintly. No cæsium could be detected in the blood, nor in the bile.

A guinea-pig was given six grains of chloride of cæsium, and six grains more nineteen hours afterwards; twenty-four hours after the second dose it was killed. No cæsium could be found in the lenses, nerves, aqueous humour, blood, or bile. Urine, kidney, and liver showed cæsium slightly in the aqueous extract of the ash.

A guinea-pig was given ten grains of chloride of cæsium, and twenty hours afterwards ten grains more. Twenty-seven hours after the second dose it was killed.

The evaporated and incinerated extract of the two lenses showed the cæsium only faintly. The aqueous humour of the eye showed cæsium faintly. The evaporated and incinerated extract of the two large nerves of the legs showed cæsium pretty distinctly.

On the Passage of Sulphate of Thallium into the Textures.

A rabbit was given one grain of sulphate of thallium. The urine, passed two hours after the first dose, gave the reaction very clearly.

Another rabbit was given three grains of sulphate of thallium, and it was killed twenty-one hours and a half afterwards. This rabbit took no food after the dose of thallium, but the stomach was found completely full of dry food. Thallium was found in the kidneys, liver, and spleen, by simply touching with a red-hot wire and bringing the small quantity of substance adhering to the wire into the flame. The blood, lens and cartilage showed none in this manner. The aqueous extract, however, of the coagulated blood and lens showed thallium distinctly. The cartilage of the hip could not be thus examined, owing to the small quantity to be got.

Another rabbit was given three grains of sulphate of thallium, and it was killed in six hours and a half. The aqueous extract of the lens showed thallium distinctly.

A guinea-pig was given two grains of sulphate of thallium, and twenty hours afterwards it took two grains more; twenty-two hours after the second dose it was killed. The urine showed thallium only after concentration. Small pieces of the liver, kidney, cartilage of the short ribs, and large nerve of the leg showed thallium distinctly. Humours of the eye showed thallium distinctly. Aqueous extract of the lenses together showed it distinctly. The blood showed no thallium directly, but the aqueous extract of a small quantity of coagulated blood showed the thallium very faintly. The brain showed the thallium also very faintly. The toe-nails showed the thallium very distinctly; and the hair of the belly also showed it very distinctly.

Another guinea-pig was given two grains of sulphate of thallium, and it was killed in six hours. The aqueous extract of the lens showed thallium faintly. The urine showed the thallium distinctly. The aqueous extract of the two large nerves showed no thallium.

On the Passage of Sulphate of Silver into the Textures.

A guinea-pig was given one-eighth of a grain of sulphate of silver. Twenty-three hours afterwards it was given another eighth of a grain. Twenty-seven hours afterwards a third eighth of a grain was given; and the same dose on the third, fourth, fifth, sixth, seventh, ninth, and tenth days; on the eleventh day the animal died. One grain and a quarter of sulphate of silver in twelve days was taken. The ashes of the liver, kidney, and stomach showed silver fairly, by means of galvanic precipitation of silver or copper. The ash of the bile showed silver rather less distinctly. The ash of the urine showed the silver only very slightly. The ash of the lenses showed only very slight traces of silver, and the ash of the brain showed none.

On the Passage of Chloride of Strontium into the Textures.

Two guinea-pigs, which had been given no strontium, had the whole kidney, liver, and lenses examined for strontium, but no trace of it could be found.

A guinea-pig was given four grains of chloride of strontium; in seven hours it was killed. The urine showed strontium distinctly in a single drop. No strontium could be detected in the kidney, liver, or lens, though a whole lens was taken for the examination by the spectrum analysis.

Another guinea-pig was given ten grains of chloride of strontium; in fourteen hours and a half it was killed. A small quantity of urine showed no strontium, and no strontium was found in the ashes of the kidney or liver.

To a third guinea-pig half a grain of chloride of strontium was given. Nineteen hours afterwards the urine showed traces of strontium, and then half a grain more was given. Twenty-four hours and a half afterwards another grain was given; and twenty-four hours after this half a grain more. Twenty-seven hours afterwards another half-grain of chloride of strontium was given. At this time the urine showed strontium very distinctly. On the sixth day another half-grain, and again on the seventh, eighth, ninth, tenth, and eleventh days, until five grains and a half were taken. The twelfth day it was killed. The urine showed strontium distinctly. No strontium could be detected in the lens, humours, or blood; and minute traces only in the ash of the kidneys and liver.

THE END.

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