Address

ON

THE RELATION OF FOOD TO WORK DONE BY THE BODY, AND ITS BEARING UPON MEDICAL PRACTICE.

Delivered at the Meeting of the British Medical Association at Oxford.

BY THE

REV. PROFESSOR HAUGHTON,

M.D., F.R.S., D.C.L. OXON., FELLOW OF TRINITY COLLEGE, DUBLIN.

(Continued from p. 211.)

Source of muscular work.—As soon as it was satisfactorily established by Lavoisier and his successors that the natural combustion of carbon and hydrogen in the blood was sufficient, and rather more than sufficient, to account for the animal heat, it became a matter of great interest to physiologists to ascertain, if possible, how much of the work developed in the blood by chemical changes is employed in producing animal heat, how much in mechanical work, external and internal, and how much in vital or mental operations.

At the outset of this inquiry it received a misdirection from the conjecture thrown out by Liebig, that the excretion of nitrogen (in the form of urea) gave necessarily the measure of the wear and tear of the muscular tissues themselves, which are composed of proteinic or nitrogenous compounds. This conjecture led to Liebig's celebrated classification of food into *heat-producing* and *flesh-forming* foods, which has been unhesitatingly received until lately, in this country, by physiologists and physicians.

Before investigating the truth or falsity of Liebig's theory, it is worth while to state the most recent results obtained as to the muscular work per day of which man is capable.

From numerous observations, of which some were made by myself, on the daily labour of hodmen, paviours, navvies, and pedlars, I have obtained the following mean :—

Daily labour of man = 353·75 ft. tons = 109549 kil. met.

This quantity of work is the exact equivalent of the work done by a man of 150 lbs. weight in climbing through one mile of vertical height, and is, as I have already shown, less than *one-eighth* part of the work expended in producing and maintaining animal heat. Even if we add to the external muscular work that of circulation and respiration, which may be estimated at 133 ft. tons, the whole work done by the muscles, voluntary and involuntary, of a hard-worked labourer will amount to less than *one-sixth* of the work expended in maintaining the temperature of his body at 100° F.

I was led to believe, from my investigations as to the quantity of urea excreted in various diseases, that a certain minimum quantity equivalent to 2 grs. per pound avoirdupois of body weight was excreted quite independently of muscular exertion; and I proved that death was preceded in many chronic diseases by a fall in the urea excreted to 2 grs. per pound. These investigations were made chiefly on patients dying of advanced kidney disease, in which the excretion of albumen had nearly or altogether ceased, and on patients dying of phthisis.

Pettenkofer and Voit found that the excretion of urea in a dog reduced to 29 kilos. by ten days' fasting was reduced to 8·6 grms. And, since

29 kilos. = 63·8 lbs.

8·6 grms. = 132·7 grs.

Excretion of urea = 2·08 grs. per lb. of body weight.

Ranke obtained a precisely similar result from observations made upon himself, after long fasting, continued for several days.

If these views be well founded, it is plain that part only of the urea excreted can be regarded as due to muscular exertion, for 2 grs. per lb. (or 300 grs. for a man weighing 150 lbs.) must be set aside as a constant due to vital work, independent of muscular work altogether. Hence it would follow, supposing the muscular exertion to be measured by the increased excre-

tion of urea produced by it, that the urea will not increase as fast as the muscular exertion, but it ought to increase regularly, although at a slower rate. With a view to settle this important question, I devised the following observations upon myself in the month of July, 1866, which proved conclusively that an increase of muscular exertion, amounting to fourfold, is not accompanied by any corresponding increase in the excretion of nitrogen, in the form of urea.

I had previously ascertained by repeated experiments, extending from 1860 to 1865, that my excretion of urea (under ordinary conditions as to exercise, which did not exceed five miles a day), ranged from 465·09 grs. to 537·47 grs. per day; mean, 501·28 grs. This quantity of urea I regarded as my natural physiological average, and it was so well established that I thought I should obtain an important result by comparing it with the average found from several days of unusual muscular exertion. I accordingly walked for five consecutive days in the hilly districts of Wicklow, noting carefully the horizontal distance traversed each day, and the vertical height traversed up and down. The vertical heights were reduced to horizontal distances, on the assumption, which is well founded, that 20 is the proper coefficient for converting one into the other, and on the assumption that the work of descent is half the work of ascent. During the five days of observation the work done, expressed in horizontal miles of walking, was as follows :—

	First day.	Miles.
Miles walked	...	11·4
Height ascended	1800 ft. = 10·2	— 21·6
	Second day.	
Miles walked	...	12·0
Height ascended	2400 ft. = 13·7	— 25·7
	Third day.	
Miles walked	...	11·6
Height ascended	1400 ft. = 8·0	— 19·6
	Fourth day.	
Miles walked	...	9·3
Height ascended	1400 ft. = 8·0	— 17·3
	Fifth day.	
Miles walked	...	10·4
Height ascended	1600 ft. = 9·1	— 19·5

From the preceding statement it follows that the average work done each day was 20·74 miles of horizontal walking—the result of which upon the urea excreted was to be compared with the result already mentioned, as a physiological constant determined under circumstances in which the daily muscular work never exceeded 5 miles of horizontal walking.

In order to determine the urea, I collected each day all the urine passed, and kept one-fifth part of it; and at the close of the fifth day examined the mixture formed from the five days' urine. It was found to contain 501·16 grains of urea per day; a result identical with the physiological quantity previously found by me under totally different conditions—namely, 501·28 grains. I was much surprised at this result, for I had previously believed in the theory laid down by Liebig, which attributed the excretion of urea to the disintegration of muscular tissue.

It might be objected to the preceding reasoning that there can be no doubt as to the capability of the combustion of proteinic compounds represented by 501·28 grains of urea excreted to produce mechanical force sufficient to explain the muscular exertion of walking 20 or 21 miles per day; as may be shown in the following manner :—

(1) The urea excreted bears to the proteine consumed the proportion of 24 to 79; as appears from their chemical compositions—namely,

Urea	...	C ₂ H ₄ N ₂ O ₂	...	60
Proteine	...	C ₃₆ H ₂₇ N ₄ O ₁₂	...	395

(2) In 100 parts of proteine there are 53·7 parts of carbon and 7 parts of hydrogen; the total heat due to the combustion of 1 grm. of proteine is, therefore,

	Heat units.
0·537 grm. of carbon	4·3389
0·070 grm. of hydrogen	2·4123
	6·7512

This number, 6·7512, represents the maximum quantity of

heat units* that could be produced by the combustion of 1 grm. of proteine; but the term depending on hydrogen in it should be reduced to five-ninths of its amount, in consequence of the hydrogen already combined with oxygen in the proteine. Hence we find—

Combustion of 1 grm. of proteine.

Carbon	4·3389 heat units.
Hydrogen	1·3402 „
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	5·6791

(3) In 100 parts of urea there are 20 parts of carbon and 6½ parts of hydrogen; the total heat, therefore, due to the combustion of 1 grm. of urea is

0·20 grm. carbon	1·6160
0·067 grm. hydrogen	2·3089
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	3·9249

The term depending on hydrogen in this result should be reduced to one-half, in consequence of the hydrogen already combined with oxygen in the urea. Hence we find—

Combustion of 1 grm. of urea.

Carbon	1·6160
Hydrogen	1·1544
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	2·7704

4th. From the three preceding statements it is easy to see that, for every gramme of proteine consumed, 0·8416 heat units are contained in the urea excreted; so that

The digestion of 1 gramme of proteine gives out 4·8375 heat-units.

It is easy to see that 501·28 grs. of urea excreted correspond to 1650 grs. of proteine in the food, or to 106·92 grammes; and the total work due to the digestion of this quantity of food may be found by multiplying it by the “digestion coefficient” already found, and by 423, which is Joule’s coefficient for converting heat-units into kilogrammeters. Hence we have—

Work due to production of 501·28 grs. of urea

$$= 106·92 \times 4·8375 \times 423$$

$$= 218786 \text{ k. m.}$$

$$= 704 \text{ ft. tons.}$$

This amount of theoretical work produced by nitrogenous food is more than double the work actually done during the walking excursion.

The average work was 20·74 miles horizontal per day, which may be considered as the exact equivalent of lifting my weight (knapsack and clothes included = 150 lb.) through one mile of vertical height. Hence the work actually done by me was—

$$\frac{150 \times 5280}{2240} = 354 \text{ ft. tons.}$$

I have elsewhere shown that the work done by the heart per day amounts to 122 ft. tons, which should be added to the external work, as well as the unknown work due to the muscles used in respiration, probably amounting to one-sixth of the heart’s work, or 21 ft. tons; adding these quantities we find

External work... ..	354 ft. tons.
Circulation and respiration	133 „
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	487 „

This quantity represents 69 per cent. of the applied work, which was shown to be 704 ft. tons.

The amount of muscular work accounted for is 354 ft. tons, which is almost exactly half the whole theoretical work supplied by the food that goes to form urea—viz., 704 ft. tons; but it has been already shown that 2 grs. of urea per pound of body weight is required to maintain the vital work, including circulation and respiration. This would give from 240 grs. to 260 grs. of urea out of the 501 grs. available for vital work, so that one half of the available work may be considered as expended on vital work, and the other half as expended on external muscular work. This supposition, however, requires us to believe that the muscles act without loss by friction. This is not admissible, for I have elsewhere endeavoured to show that there is a loss in the force applied by the muscles of various animals in consequence of the friction of their tendons amounting—

In man to	35 per cent.
In the mastiff to	41 „
In the kangaroo to	61 „

* Heat unit = 1 kilog. of water raised 1° c.

Hence it may be regarded as certain that the available force represented by 501 grs. of urea is not sufficient to account fully both for vital work and for the external mechanical work expended by me during the experiments just described.

The foregoing observations and calculations were made in the month of July, 1866, but I did not then publish them, as I found afterwards that I had been anticipated by Dr. Fick and Dr. Wisleceus, of Zurich, in a paper published in June in the *Philosophical Magazine*, on the urea excreted during an ascent of the Faulhorn. Professor Frankland, in a paper published in the same journal in September, 1866, corrected some erroneous reasoning that found its way into Fick’s and Wisleceus’ paper, and further supplied, from direct experiment, the digestion coefficient of proteine, which had been obtained by me from calculation. The actual value of this important constant was found by him to be—

Actual value of digestion coefficient of proteine ...	4·3155
Calculated value... ..	4·8375

My only object in now publishing an account of the independent experiment and calculation made by myself, is to confirm the certainty of the important fact first proved experimentally by Fick and Wisleceus, that the force due to the urea excreted in a given time is not sufficient to provide the actual work that may be done by the muscles in the same time.

Liebig and his followers, misled by a preconception of the simplicity of nature, assigned to nitrogenous food the duty of providing the force necessary for the production of muscular work, by supplying the waste of muscular tissue; while they supposed the farinaceous and fatty foods to provide the amount of animal heat required by the body.

The opponents of Liebig have fallen into the opposite error, and deny that nitrogenous food contributes any portion of the force employed in muscular work.

The truth, as is usual, lies between the two extreme hypotheses, and we are now compelled to admit that a given development of force, expressed in animal heat, muscular work, and mental exertion, may be the effect of several, perhaps many, supposable supplies of digested food, farinaceous, saccharine, fatty, and albuminous.

Just as a given algebraical function may be equated to a given constant, by the use of a certain definite number of values of its variable quantity, so may a given effect of work in the animal body be produced by certain definite, though very different, combinations of various kinds of food, the digestion of which follows each its own law, and develops its own amount of force. The number of roots in our equation of life increases the difficulty of solving it, but by no means permits the acceptance of the lazy assumption that it is altogether insoluble, or reduces a sagacious guess to the level of the prophecy of a quack.

Lavoisier supposed in his earlier investigations that animal heat was developed by the combustion of carbon and hydrogen in the lungs; just as in earlier times it was supposed to be produced spontaneously in the heart, which was imagined to be so hot as even to burn the hand that should imprudently venture to touch it.

In like manner, Liebig and his followers supposed the muscular work to be developed in the substance itself of the muscles that were its instruments.

Both of these doctrines are now justly repudiated by physiologists; and the view proposed in 1845 by Dr. Mayer, of Heilbronn, and recently developed with much ability by Mr. C. W. Heaton, of Charing-cross Hospital, in the *Philosophical Magazine* for May, 1867—that the blood itself is the seat of all the chemical changes that develop force in the body,—has gained favour among physiological chemists, and also met with acceptance among practical clinical observers.

Thus the human mind revolves in cycles, and the physicians of the nineteenth century are preparing to sit at the feet of Moses and learn that the blood of an animal really constitutes its life; while South African theologians reject his authority because he happened to confound a Rodent with a Ruminant.*

No two classes of animals can well differ more from each other than the cats and ruminants, one of which is intended by nature to eat the other. They differ in all respects as to food, the cats requiring a supply of fresh meat and blood for their health, and the ruminants being exclusively vegetable feeders; yet in both classes we find a great development of muscular power, and of rapid action of muscles, qualities alike

* No reasonable person can fail to perceive the ignorance of the great Law-giver who will apply to him the test first proposed by Swift for Homer. Moses, like the author of the “Iliad,” was totally unacquainted with the discipline and doctrines of the Church of England.

necessary to the pursuer and to the pursued. There can be no doubt that muscular work is developed in the cats from the combustion of flesh, and in the ruminants, mainly, if not exclusively, from farinaceous food. It is, however, worthy of remark, that the muscular qualities developed by the two kinds of food differ considerably from each other. The hunted deer will outrun the leopard in a fair and open chase, because the work supplied to its muscles by the vegetable food is capable of being given out continuously for a long period of time; but in a sudden rush at a near distance, the leopard will intallibly overtake the deer, because its flesh food stores up in the blood a reserve of force capable of being given out instantaneously in the form of exceedingly rapid muscular action.

In conformity with this principle, we find among ourselves an instinctive preference given to farinaceous and fatty foods, or to nitrogenous foods, according as our occupations require a steady, long-continued, slow labour, or the exercise of sudden bursts of muscular labour continued for short periods. Thus chamois hunters setting out for several days' chase provide themselves with bacon fat and sugar; the Lancashire labourers use flour and fat, in the form of apple dumplings; while the Red Indian of North America almost transforms himself into a carnivore, by the exclusive use of flesh food; he sleeps as long, and can fast as long, as the puma or jaguar; and possesses stored up in his blood a reserve of force which enables him, like a cat, to hold his muscles for hours in a rigid posture, or to spring upon his prey, like a leopard leaping from a tree upon the back of an antelope.

If the preceding view of the muscular qualities developed by the two kinds of food be correct, important inferences suggest themselves as to the food that should be employed in relation to several kinds of work. Of these inferences I shall select two examples:—

1. The nurses of one of our Dublin hospitals were formerly fed chiefly upon flesh food and beer, a diet that seemed well suited to their work in ordinary times, which was occasionally severe, but relieved by frequent intervals of complete rest. Upon the occasion of an epidemic of cholera, when the hospital duties of the nurses became more constant, although on the whole not more laborious, they voluntarily asked for bacon fat and milk, as a change of diet from the flesh meat and beer; this change was effected on two days in each week with the best results as to the health of the nurses, and as to their power of discharging the new kind of labour imposed upon them.

2. I have been informed, on competent authority, that the health of the Cornish miners breaks down ultimately, from failure of the action of the heart and its consequences, and not from the affection of the lungs called "miner's phthisis." The labour of the miner is peculiar, and his food appears to me badly suited to meet his requirements. At the close of a hard day's toil, the weary miner has to climb by vertical ladders through a height of 100 to 200 fathoms before he can reach his cottage, where he naturally looks for his food and sleep. This climbing of the ladders is performed hastily, almost as a gymnastic feat, and throws a heavy strain (amounting from one-eighth to one-quarter of the whole day's work) upon the muscles of the tired miner, during the half hour or hour that concludes his daily toil. A flesh-fed man (as a Red Indian) would run up the ladders like a cat, using the stores of force already in reserve in his blood; but the Cornish miner, who is fed chiefly upon dough and fat, finds himself greatly distressed by the climbing of the ladders—more so indeed than by the slower labour of quarrying in the mine. His heart, over-stimulated by the rapid exertion of muscular work, beats more and more quickly in its efforts to oxidate the blood in the lungs, and so supply the force required. Local congestion of the lung itself frequently follows, and lays the foundation for the affection so graphically, though sadly, described by the miner at forty years of age, who tells you that "his other works are very good, but that he is beginning to leak in the valves."

Were I a Cornish miner, and able to afford the luxury, I should train myself for the "ladder feat" by dining on half a pound of rare beefsteak and a glass of ale, from one to two hours before commencing the ascent.

The excretion of nitrogen by the cats and ruminants is very different, as might be expected from their food. I have ascertained that the urea discharged by a Bengal tiger and a sheep, daily, is as follows:—

Bengal tiger	...	...	...	...	4375 grs. of urea.
Sheep	...	...	...	...	256 "

It is worthy of remark, and serves to throw light on the

meaning of the excretion of nitrogen from the body, that causes but slightly connected with muscular exertion in the ruminants increase amazingly the excretion of urea. Thus I have found the following excretion of urea from a ram during the rutting season:—

Ram (rutting season) ... 1493 grs. of urea.

This amounts to a *sixfold* increase of urea, which cannot possibly be accounted for by the food consumed at the time, but requires us to assume a certain storing up of force, represented by nitrogenous compounds, which has been going on for a considerable period previous to the rutting season. A similar and equally remarkable storing up of phosphates and carbonates takes place, previous to the rutting season, in the ruminants that shed their horns, which in the *Cervus Megaceros* often weigh 90 lb.

These remarkable phenomena remind us of the maxim of the wise Hippocrates, who recommends moderation in the use of the gifts of the Golden Venus, as well as in those of Ceres and Bacchus—*πόντοι, σιτία, ποτά, ὕπνος, ἀφροδίσια μετρία*; with which may be compared its converse in the Latin proverb—"Sine Cerere et Baccho, friget Venus."

(To be concluded.)

REPORT OF THE

THIRTY-SIXTH ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION.

HELD IN OXFORD, AUGUST 4TH, 5TH, 6TH, & 7TH, 1868.

(Concluded from page 233.)

FRIDAY, AUGUST 7TH.

Many of the members started by the early train for London. At half-past nine a meeting of the Committee appointed to consider the mode of electing to the Council was held. The report was presented at a later part of the day.

At 10 o'clock the sixth general meeting was held in the Divinity School, for the adjourned debate on Dr. Hughes Bennett's report on the Action of Mercury, which stated the mineral in moderate doses exerted no action, and in larger ones greatly diminished the amount of bile secreted.

Dr. BENNETT scarcely thought his paper was exactly one fitted for discussion. So long as the action of the drug was a matter open to difference of opinion it was a different thing; but when facts had established a reliable result there was nothing more to be said. In the tables he had handed round the facts therein contained had reduced our knowledge of the action of the drug in one sense to almost mathematical precision. After explaining the mode of performing the experiments, and their nature, Dr. Bennett proceeded to say that the medical profession had been acting under a delusion in regard to the drug mercury. How it is that it has been so generally used under the idea that it was a cholagogue, and how it is that when persons are what is termed bilious a blue pill and black draught does good, was not his business to determine. The Committee had not been deputed to explain the opinions of medical men; their object was certainly indirectly to show that opinion is not always based upon exact data. He would be glad if medical men would give reasons why they used the drug. He believed it was to be explained by the fact that the idea of its being a cholagogue, taught in the schools, and handed down from book to book, originated from a statement of Paracelsus that hydrargyrum did good in icterus. This was the source of the popular belief. He appealed to Oxford and its literary magnates, and the frequenters of the Bodleian Library, with its classical lore, to give a better explanation. He wished to ask the meeting this question, Did they think that the evidence contained in the tables handed round sufficient to establish the truth of the proposition that mercury does not increase the secretion of bile? Are 33 experiments carefully made on dogs too little or enough? The Committee feel satisfied, but does the British Medical Association? The experimenters are willing to proceed farther if it be thought desirable. Were they (referring to the meeting) satisfied, or will they suggest any further experiment or observation which can elucidate or test the subject more completely? The Committee cannot suggest anything more, but are willing to undertake further inquiry. Do let us, continued Dr. Bennett, settle this question once and for all, and do not