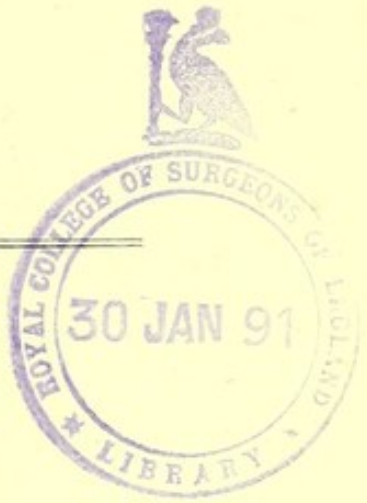
On some points in connection with animal nutrition : being an address delivered at South Kensington, in the Biological Section of the Science Conferences, May 26, 1876 / by J.H. Gilbert.

ON SOME POINTS IN CONNECTION WITH ANIMAL NUTRITION.

BEING AN ADDRESS DELIVERED AT SOUTH KENSINGTON, IN THE BIOLOGICAL SECTION OF THE SCIENCE CONFERENCES, MAY 26, 1876.

BY DR. J. H. GILBERT, F.R.S., F.L.S., F.C.S.



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On Some Points in connection with Animal Nutrition.

By Dr. J. H. Gilbert, F.R.S., F.L.S., F.C.S.

A few days ago Professor M. Foster wrote to me to say that he intended to bring the subject of nutrition forward on this occasion, and asked me if I would take part in the discussion afterwards ; and as he and I had had a good deal of correspondence and conversation some little time since about the important question of the sources of the fat of the animal body, I concluded it was probably to that subject he wished me to devote my attention. At any rate, I looked up hurriedly the materials which Mr. Lawes and myself have collected in relation to that subject, and some allied points, and propose, with your permission, to lay the facts before you shortly, although Professor Foster has not given you his paper.

Thirty-five years ago, or more, I believe the view generally accepted was, that the carnivora found the fat which existed in their bodies ready-formed in the herbivorous animals they consumed, and that the herbivora in their turn found all the fat of their bodies ready stored up in the plants they consumed. About that time Liebig, in reviewing the composition of vegetable food, came to the conclusion that this was simply impossible, taking into consideration the amount of fat which was stored up by many animals in proportion to the known quantities in the food. He put forward the view that the carbohydrates of the food—starch, sugar, and so on—were important sources of the fat of the herbivora. For a short time this view was opposed, but only for a short time, by Dumas and Boussingault, and some other experimenters in France, though they afterwards accepted it.

The investigations of Mr. Lawes and myself, it must be borne in mind, have always had an agricultural object, so that if they were not conducted exactly in the way which the physiologist will say they might have been, it has been because we had not the same object before us, that is a purely physiological one. Very soon our own experiments led us to believe that Liebig was right in his conclusion on this point, but that he must be wrong on some other points in relation to the feeding of animals which he so ably discussed. We found it was pretty certain, from the consideration of the feeding experiments, that some of the fat must have the source which he assumed.

On the other hand, he assumed that the value of food to the animal was measured by the amount of nitrogen which it contained ; that is to say, he maintained that, in the formation of meat, in the formation of milk, and in the exercise of force, the measure of the value of the food required, for these purposes, was the amount of nitrogen it contained ; and in the case of the exercise of force, the amount of urea which was eliminated. We found, however, that we could give twice or three times the quantity of nitrogen within a given time to one animal as to another, both at rest, and that the amount of nitrogen eliminated in urea was almost proportional to the amount of nitrogen in the food, and had no direct connection with the amount of force exercised.

The question of which of the constituents in the food, were of the most importance for the exercise of force, and for the making of fat, remained in this condition until the experiments instituted in Munich, about 16 or 17 years ago, with Pettenkofer's beautifully contrived respiration apparatus, a model and drawings of modifications of which are in the next room. I am glad that after very much trouble on my part to get such an apparatus brought to this Exhibition, and entirely failing, it has after all been sent by some one. It consists of a chamber in which an animal can be put, and by a water wheel, or by some other power, the air is gently aspirated through the apparatus, then it passes through guages, and through solutions, which absorb the carbonic acid, &c., and so the amount of air passing is gauged, and the products of respiration are determined. It is not the apparatus itself, but the results which it has brought out, which I wish to refer to on this occasion. In 1860, Bischoff and Voit published their first results. They kept a dog for many months without change as to movement, without giving it any special exercise, but varied its food immensely, and they found the urea eliminated was almost in proportion to the amount of nitrogen taken in the food. But inasmuch as the then existing view required this to be connected in some way with the exercise of force, they explained that so much more force was exercised in the actions within the body in dealing with the increased amount of nitrogenous substance consumed ; so that after all the amount of the urea eliminated was a measure of the exercise of force, although it was in these internal actions, and not in the voluntary exercise of muscular power. I was in Germany at the time that book came out, and went to Munich, hoping to see these gentlemen on the subject. In conversation with Professor Voit, I ventured to call in question the

conclusion at which they had arrived, and I think he considered I was entirely in error. But a few years afterwards it was found by others also that the amount of urea eliminated had no direct connection with the amount of force exercised, and that what is the most pronounced when there is an increased exercise of force, is an increased elimination of carbonic acid by the lungs. I believe there is now no doubt about that matter. Messrs. Fick and Wislicenus, Dr. Frankland, and Dr. E. Smith, brought that prominently forward, and I believe it is now accepted that the elimination of urea is no measure of the muscular force exerted within the body.

After putting forward these views, Messrs. Bischoff and Voit put their dog into a kind of tread-wheel, and they found that the amount of urea eliminated was not in proportion to the exercise of force, but the amount of carbonic acid was so, and eventually they themselves admitted the truth of this.

Then came the question of the sources of animal fat. On this point, again, Voit has worked almost exclusively with the dog, which is a carnivorous, or, at most, an omnivorous animal. He has found, which I do not wish to call in question, that in the case of the carnivora, and in some cases of the herbivora, the fat may be formed from the nitrogenous substance of the food. But from the results obtained with this carnivorous animal he has come to the conclusion, that not only in such cases, but in all, the fat formed within the animal is derived from the albuminous substance of the food or of the body. I have roughly noted a few of the experiments of Voit, which I believe are the strongest or most conclusive for his view of the question. He found that when a dog was fed on starch or sugar alone, or with albumin, or with fat and albumin, the carbon stored up, that is to say, the carbon which was not eliminated in any way from the body, was never more than that in the fat of the food, *plus* that in the albumin which was broken up, as indicated by the amount of urea eliminated. He concluded that this was a proof that fat was not formed from the carbohydrates. In another case, which perhaps was stronger, he fed the dog with starch and a little fat, but no albumin whatever, and the carbon stored up was equal to that of the fat in the food, *plus* that due to the oxidation of albuminous tissue, and when he gave more starch to this food the amount of carbon stored up was reduced ; that is to say, he argues that the carbohydrates in this case protected the albumin of the body from disintegration, and did not in any way serve for the production of fat ; and that there would have been a greater storing

up of carbon if this additional starch which he gave to the animal had been the source of the fat. He also argued, from a number of experiments, that starch and sugar are quite oxidised in the body, yielding carbonic acid, &c., within twenty-four hours. He maintains that the same must occur with herbivora as with carnivora. The carnivora are found absolutely to digest vegetable food, and take it into their system as an herbivorous animal; and he argues that, to establish a different source of fat, it must be shewn by experiment that fat is formed in excess of that in the food, *plus* that which can be formed from the oxidated albumin. Now this, I think, I shall be able to show you we have done. We have not accepted the challenge in the way of making new experiments for the purpose, but I think we have old experiments which are perfectly conclusive, and do meet exactly the requirement which Voit says is essential to disprove the view which he maintains with regard to the herbivora.

But before entering on our own experiments, I will just say what has happened in answer to the challenge in Germany. Weiske and Wildt conceived, as I shall be able to show afterwards was a very right thing to conceive, that the pig was the very best animal to experiment on for this purpose. He is certainly *the* fat-maker of all the animals that we feed; and there are other reasons why he is the best of all others to experiment upon in this particular. They had, from a theoretical point of view, a very good conception of what was necessary. They took four pigs, slaughtered two of them, and determined the fat and other constituents in those animals. Then they fed one on food very poor in nitrogenous substance, and one on food exceedingly rich in nitrogenous substance. It happened that the pig fed on food very rich in nitrogen had so much that it became unwell, and that experiment failed entirely. With regard to the one fed on food poor in nitrogen, the food was so poor that the experiment took too long a time; in fact, too much food was passed through the body in proportion to the increase produced; and when eventually they slaughtered that animal, and analysed it, so much nitrogen had passed through the body during the time, that they found the whole of the fat that had been formed might be derived from the nitrogenous substance consumed. Weiske and Wildt did not conclude therefrom that it was established that fat could only be produced from the nitrogenous substance, but they admit that the experiment was not conclusive.

In the experiments of Mr. Lawes and myself we have used a great many animals, and we have brought our results into calculation,

although the experiments were not at the time arranged with the special view of determining this question. The table shows some results of experiments with sixteen oxen, 249 sheep, and fifty-nine pigs. You will see that the proportion of stomachs and contents in the body is 11·6 per cent. with the oxen, 7·5 with sheep, whilst it is only 1·3 in the pig. The intestines and contents, on the other hand, shew in oxen only 2·7, in sheep 3·6, and in the pig 6·2 per cent.; with it, therefore, very much more than with either of the ruminant animals. We know that the character of the food is such in the case of the ruminants that they must pass an enormous quantity of very crude stuff through their bodies, and must elaborate it first in one stomach and then in another, and the result is they have not only a very large capacity of stomach, but also a very large proportion of contents in relation to the whole body. In the case of the pig, on the other hand, the stomach is exceedingly small; the natural food of the pig is starchy seeds or roots (which are the food of man also), it contains exceedingly little necessarily effete matter, their stomachs have comparatively manageable stuff to deal with, and they have a very small stomach, while on the other hand their intestines are very large. It is known that the transformation of the starch goes on almost throughout the intestinal canal, so that we can easily understand how it is that with such starchy food these animals have an enormous amount of intestines compared with either oxen or sheep. If we look at the proportion in the live weight of the, so to speak, further elaborating organs the heart, the liver, the lungs, the pancreas, and so on, their percentage by weight in the bodies of the three descriptions of animals is almost identical.

Now, for 100 lbs. of live weight the amount of dry substance consumed per week was 12·5 byoxen, 16 by sheep, and 27 by pigs; that is: 100 lbs. live weight of pig will consume much more dry substance of food, and, as I have stated, that food is of a more highly nutritive kind, and more easily digested, than that of oxen or sheep. Again, the increase per week was only 1·13 per cent. on the live weight of oxen, 1·76 of sheep, and 6·43 of pigs. So that the proportion of the increase to the weight of the body is much the greatest with the pig. Then, if we take the facts in relation to the amount of the food, for 100 lbs. of dry substance of food, the ox will give in increase only 5·2 of fat, the sheep 7, and the pig 15·7. Suffice it to say, that there is less effete matter in the food of the pig, and therefore its live weight and its increase indicate more nearly the real increase of the body, and

not the fluctuating matters in the alimentary canal. Its food is of a higher character, so that a larger proportion of it is stored up. That which passes through the system is more completely used, and the amount of fat which is produced is also very much higher. Therefore, I say the pig is by far the best animal to experiment upon for this purpose.

Whilst on this subject I may refer to a portion of the table which vegetarians will perhaps not be much pleased to see. If we are to judge that the size of the stomach indicates to some extent the character of the food, its crudeness or concentration, as no doubt is the case with the other animals, and if we compare oxen, sheep, pigs, and man, we find the proportion of stomach by weight per cent. is, approximately, in oxen 3·2, in sheep 2·44, in pigs about 0·88, and in man only 0·38; so that going from one animal to the other you should have more concentrated and more digestible food in the case of man, than of the pig; and you have animal food as well as starchy seeds, roots, &c.; and the indication is, I think, that man was not made to consume potatoes and cabbages by the bushel.

The next point is as to the indications of merely practical results. Without going into the chemistry of the subject, or discussing whether the food of the animal does contain enough or not enough of nitrogenous substances to yield all the fat produced, I will call attention to some results which will indicate the general relations of the food to the necessities of the body. On the coloured diagram you have the results of thirty separate experiments on pigs. The plan was this: we gave to a certain set a fixed amount of highly nitrogenous food, and let them take whatever they liked of less nitrogenous food. To another set we gave a fixed amount of food low in nitrogen and rich in starch and such matters, and let them make up whatever they wanted with highly nitrogenous food. So we rang the changes in a great many more cases than are here represented, but in this way it will be seen that the animal fixed its own diet according to the necessities of the case; and the question is, was it the nitrogenous substances, was it the non-nitrogenous substances, or was it the total dry substance, nitrogenous and non-nitrogenous together, which guided the amount consumed by a given live weight within a given time, or rather guided — for these were fattening animals — the amount of increase which was produced? The lowest amount of nitrogenous substances consumed by 100 lbs. of live weight of pig per week in any one experiment being taken as 100, in some cases 300 were taken, and

in most more than 200. In the same way the lowest amount of non-nitrogenous substance being taken as 100, in no case was nearly as much as 200 consumed, and the average was about 140 parts. When we come to the total dry substance, including both nitrogenous and non-nitrogenous, we find that the quantities ranged more closely together; that is to say, the total digestible organic substance seems to have been the measure of what was required, and that the nitrogenous might possibly act for the non-nitrogenous substances if there were not enough of them. But it is quite clear that the measure was either the non-nitrogenous substances or the total organic substances—certainly not the nitrogenous substances. Then the question arose, whether the same thing would hold in relation to the amount of increase in the weight of animal produced. It was always assumed, I think, until these experiments of Mr. Lawes and myself, that when animals were not fed on highly nitrogenous food the amount they stored up was comparatively small. These experiments show the amount of these three classes of constituents consumed in producing 100 pounds increase of live weight in the different cases. 100 pounds being the lowest amount of nitrogenous substance required, 282 was the highest, the animal fixing his own diet, and in many cases it was over 200; that is to say, more than twice as much as satisfied him when he had enough of other matters to make up. At any rate it would seem that fat can be formed from nitrogenous substances, provided there is a deficiency of non-nitrogenous substances in the food; and I may say that the nitrogenous substances are of a higher food capacity, irrespective of the nitrogen, containing more carbon, more hydrogen, and less oxygen; they have more useful matter in them than an equal weight of starch or any substance of that kind.

Now the question arises, what is the state of affairs when we attempt to calculate these results and to see whether or not the food did contain enough nitrogenous substances, or albuminous matter, to supply the whole of the fat produced? These experiments were not specially arranged to settle that question, but they were calculated afterwards. It was about twenty-six years ago that we took two pigs of the same litter, very carefully selected both by practical and scientific eyes as being as nearly as possible exactly alike. One was slaughtered, and the total amount of dry matter, fat, nitrogen, mineral matter, and so on, determined; and then the other animal was fed. At that time we had not arrived at such distinct conclusions as we did afterwards as to

the desirability of giving a greater proportion of starchy matters. We gave the animal a great deal more than the proportion of nitrogenous substances existing in what may be called the normal fattening food of the pig—barley meal. In the first column of the table, the results of that experiment are calculated out to show whether the food did contain enough nitrogenous matter to yield the fat produced. You will see the proportion of non-nitrogenous matter to one of nitrogenous is 3·6. Now, the proportion in barley meal, which is the best fattening food for the pig, is between 5 and 6 to 1; so that we gave too much nitrogen according to what we now know is the best proportion. There was a considerable amount of increase in ten weeks, eighty-eight pounds, or 85·4 per cent. on the original weight of the body.

The question is, how much fat was in the food? and that is shown in the second division of the table. It is calculated that for 100 pounds increase in the live-weight there were stored up 63·1 pounds of fat. There were of ready-formed fat in the food 15·6 pounds; leaving 47·5 pounds fat to be produced from some material or other. Out of 100 of nitrogenous substances consumed as food, there were stored up in increase 7·8, leaving 92·2 parts of nitrogenous substance which might be used for the production of fat or might not. If we calculate how much carbon there was in the produced fat, and how much there was left available in the nitrogenous substance for the production of fat, we find that there were 7·4 pounds more carbon possibly available from the nitrogenous substance than was necessary for the production of the fat; or, put in another way, there were 120 of carbon available from the nitrogenous substances for 100 required.

According to this mode of calculation, therefore, there was enough nitrogenous substance to justify the conclusion of Voit; or rather, the result does not in any way disprove his conclusion that fat has been produced from the disintegration of nitrogenous substances in the body. This table was calculated some years ago, and we have intentionally put the results in the worst aspect that we could for our own side of the case, that we might not exaggerate the conditions. For instance, we have assumed that the whole of the fat in the food would be taken up, which it certainly would not; and we have assumed that the whole of the nitrogenous substances of the food would be digested, and would come into play, which they certainly would not. If we assume in our own calculations the estimate adopted in Germany, that 100 pounds of nitrogenous substance cannot yield more than

fifty-one of fat, even this experiment shows a little deficiency of nitrogenous substances, and would in fact be in favour of our view.

The next two experiments given in the table show a still higher proportion of nitrogenous substance in the food; and there was, accordingly, a great deal more carbon available from the nitrogenous substance than was necessary for the formation of the amount of fat produced.

The next two experiments (four and five), were with more natural fattening food of the animal, one entirely Indian corn-meal, and the other entirely barley-meal. A pig requires for rapid fattening very little, if any, more nitrogenous substance than this represents. But here we have only 60 per cent. or a little over, 60·8 in one case, and 60·5 in the other, of the carbon of the fat produced in the animal, possibly derivable from the nitrogenous substance of the food. So that we have in those two cases nearly 40 per cent. of the carbon of the produced fat which could not possibly come from the nitrogenous substances, and must have come from the non-nitrogenous matter, in fact from the carbohydrates. But an objection may be raised to this calculation; the animals were larger to begin with; and the weights were heavier at the end; so that the composition of the lean animal, and of the fat animal, as derived from the direct analyses, does not absolutely apply; but we could not possibly thus get rid of this forty or more per cent. which the calculations would show to be derived from the non-nitrogenous substance of the food.

The remaining four experiments are also entirely in favour of our view. The animals were about the same weights as those analysed: the food was more nearly the proper food for fattening, being rather lower in nitrogenous substances, but much higher than in experiments 4 and 5. But even here we found 18·9, 18·8, 25·2, and 14·1 per cent. of the total carbon of the produced fat could not possibly have been derived, and certainly a great deal more was not derived, from the nitrogenous substances of the food.

I need not trouble you further with these results. But I should say that the contrary view has been adopted not only by some physiologists, but in Germany in some text books on agricultural chemistry. I hold in my hand one of these text books in which the evidence of these experiments is discarded, and it is assumed that if you cannot experiment with the respiration apparatus the results are good for nothing. I would not wish to depreciate the importance of the results obtained by the respiration apparatus in any way. I have taken the

greatest interest in them, and I think they lead to the most important conclusions; but I also think some observers have come to very erroneous conclusions from the results of such experiments. I submit that if you experiment with *the* fat-producer—the pig—and if you take two carefully selected animals (or more if you like) kill and analyse one, and feed the other as rapidly as possible, that is, let him take as much of the most appropriate food as he will take, you may, without any respiration apparatus, determine this point. It is most important that it should be definitely settled. Since the recent publications on the subject, Mr. Lawes and myself have gone thoroughly into the question, and re-calculated most of our results; those relating to oxen and sheep as well as pigs. They point to this: that the ruminant animals, which have such elaborate machinery, and do so little productive work, do pass so much nitrogenous substance through the body in relation to the amount of increase, that they do not show that fat can be derived from the non-nitrogenous substances of the food; but in the case of pigs the evidence is perfectly conclusive. Having re-calculated our own experiments in this way, and the results being absolutely conclusive so far as the pig is concerned, Mr. Lawes is unwilling to be at the trouble and expense of further experiments on the question; but it really is one of great importance, and one which public institutions might well take up. It is of importance, not only agriculturally, with reference to the proper way of feeding stock, but also in its bearings on the nutrition of man.

[For the tables and diagrams referred to above, see—"On the Sources of the Fat of the Animal Body," *Philosophical Magazine*, December, 1866; and—"On the Formation of Fat in the Animal Body," *Journal of Anatomy and Physiology*, Vol. xi., Part iv.; and for other points, and detail—"Food in its Relations to Various Exigencies of the Animal Body," *Philosophical Magazine*, July, 1866; and the papers therein referred to.]