shall have to give an account of some changes which have not found a place under the previous heads."

Space will only permit us to quote a few of the questions raised. It is now generally admitted that pressure is the principal cause which determines the widening of lines, but it is not generally known that a different appearance of the lines may be presented according as the pressure is due to the impact of *similar or dissimilar* molecules; thus a molecule of sodium will widen its lines more easily in an atmosphere of sodium than in another atmosphere. Mr. Lockyer has observed that the lines of oxygen or nitrogen may be obtained sharp at atmospheric pressure by mixing a small quantity of one gas with the other. The gas which is present in small quantities has its lines sharp.

The curious fact is mentioned that when a line widens unsymmetrically it widens in nearly all cases more towards the red, and then towards the violet end.

In that part of the report which relates to multiple spectra an account is given of the gradual spreading of the opinion that these spectra are due to different molecular groupings. The question of long and short lines is next discussed, and great stress is laid on the fact that the longest lines are by no means always the strongest. An abstract of Mr. Lockyer's work on the subject is given, and of the confirmation which his results have found in later work. Thus Mr. Lockyer found that the longest lines were always the first to be reversed. Professors Liveing and Dewar have since examined the absorption-spectra of many metallic vapours. The lines which they have seen reversed were nearly in all cases those which are longest in the spark, though not always those which are strongest. Results obtained by M. Lecoq de Boisbaudran with sparks, the temperature of which was lower than in the ordinary jar-discharge, also confirm Mr. Lockyer's results. Discussing the attempts which have been made to explain these and other facts, it is again mentioned that we must assume the impacts of a similar molecule to produce a greater effect than the impacts of a dissimilar one. The last part of the report treats of some other changes in the relative intensities of lines. We only mention the experiments in which Mr. Lockyer found sometimes the green sodium line to be present without the well-known yellow double line. The report concludes as follows:—

"We have here again two hypotheses, that of molecular shocks and that of molecular combinations. Both explain the facts satisfactorily, and I do not think that one of them necessarily excludes the other. I believe, on the contrary, that a line can be drawn, and that while the regular changes observed chiefly in band-spectra may be due to one cause, the often irregular changes in metallic spectra, where one set of lines disappears and another appears often on the violet side, but sometimes towards the red, may be due to another cause.

"It is often said that we must not ascribe the same phenomenon to two different causes, when one of them is sufficient to explain it; but the point at issue is whether the phenomena are the same in all cases. An advance of science has constantly led to the separation of phenomena which were formerly considered to be connected together, and we believe that the further development of the different points we have attempted to discuss, in which different observers have strongly taken up opposite opinions, will lead to the blending together of different views rather than the entire elimination of one of them."

Prof. Hartley, in his part of the report, gives us an account of our knowledge on emission spectra in the ultra-violet region. He treats especially of the researches on the solar spectrum by Mascart, Draper, and Cornu.

Prof. A. K. Huntington reports on the absorption spectra in the ultra-violet region. The results obtained by Prof. Stokes and Dr. Miller are given in detail. Amongst the results obtained by Dr. Miller, it seems especially interesting to notice the connection which apparently exists between the absorbing properties of a liquid and that of its vapour. When one of them is transparent to the ultra-violet rays the other is also, and *vice versâ*.

Prof. Soret, it is well known, constructed a few years ago a spectroscope with a fluorescent eyepiece, and has by means of it carried researches in the ultra-violet parts of the spectrum. We notice especially the examination of absorption-spectra of the bases of gadolinite, and the conclusions drawn from it on the existence of new elementary bodies. Prof. Cornu has given much attention to the absorption power of our atmosphere, and we find a full account of his experiments in Prof. Huntington's report. In conclusion we have a short abstract of the work done by Professors Hartley and Huntington on absorption-spectra in the ultra-violet region. They obtained the following results:—

1. The normal alcohols of the series $C_4H_{2n-1}OH$ are remarkable for transparency to the ultra-violet rays of the spectrum, pure methylic alcohol being nearly as much so as water.

2. The normal fatty acids exhibit a greater absorption of the more refrangible rays of the ultra-violet spectrum than the normal alcohols containing the same number of carbon atoms.

3. There is an increased absorption of the more refrangible rays corresponding to each increment of CH_2 in the molecule of the alcohols and acids.

4. Like the alcohols and acids, the ethereal salts derived from them are highly transparent to the ultra-violet rays, and do not exhibit absorption-bands.

Interesting results were also obtained by the examination of substances containing the benzene nucleus, and in a separate paper the absorption-spectra of essential oils were examined and discussed. Prof. Hartley has still further extended the researches jointly begun with Prof. Huntington, and has arrived at the conclusion that no molecular arrangement of carbon atoms causes selective absorption, unless three pairs are doubly linked together in a closed chain.

It will be seen that a few only of the branches of spectrum analysis have been discussed in the present report, and next year no doubt will bring us a further instalment of a work which we hope will prove useful to those who are interested in spectroscopic investigations.

AGRICULTURAL CHEMISTRY[1]

III.

I HAVE thus far directed attention to some points of importance in connection with the sources of the constituents of our crops, and I must now briefly refer to some in connection with the composition, and to some relating to the uses, of the crops themselves.

As to composition, I must confine myself to indicating something of what is known of the condition of the nitrogen in our various crops; though I had intended to say something respecting the carbohydrates, and especially respecting the various members of the cellulose group.

As to the nitrogen—in our first experiments on the feeding of animals, made in 1847, 1848, and 1849, the results of which were published in the last-mentioned year—we found that, in the case of succulent roots used as food, not only were they not of value as food in proportion to their richness in nitrogen, but when the percentage of it was higher than a certain normal amount, indicating relative succulence and immaturity, they were positively injurious to the animals. So marked was the variation of result according to the condition of maturity or otherwise of of the foods employed, that, when reviewing the results of the experiments which had up to that time been conducted, in a paper read before this Section of the British Association at the Belfast meeting in 1852 (and which was published in full in the annual volume[2]), we stated that the mode of estimating the amount of proteine compounds by multiplying the percentage of nitrogen by 6·3 was far from accurate, especially when applied to succulent vegetable foods, and that the individual compounds ought to be determined. The Rothamsted laboratory staff was however much smaller then than it is now, and with the pressure of many other subjects upon us, it was at that time quite impossible to follow up the inquiry in that direction.

It is indeed only within the last ten years or so that the question has been taken up at all systematically; but we are already indebted to E. Schulze, A. Urick, Church, Sachsse, Maercker, Kellner, Vines, Emmerling, and others, for important results relating to it.

Our knowledge in regard to the subject is however still very imperfect. But it is in progress of investigation from two distinctly different points of view—from that of the vegetable physiologist and that of the agricultural chemist. The vegetable physiologist seeks to trace the changes that occur in the germination of the seed, and during the subsequent life-history of the plant, to the production of seed again. The agricultural

[1] Opening Address in Section B (Chemical Science), at the Swansea meeting of the British Association, by J. H. Gilbert, Ph.D., F.R.S., V.P.C.S., F.L.S., President of the Section. Continued from p. 499.

[2] "On the Composition of Foods in relation to Respiration and the Feeding of Animals."

chemist takes the various vegetable products in the condition in which they are used on the farm, or sold from it. And as a very large proportion of what is grown, such as grass, hay, roots, tubers, and various green crops, are not matured productions, it comes to be a matter of great importance to consider whether or not any large proportion of the nitrogenous contents of such products is in such condition as not to be of avail to the animals which consume them in their food?

We cannot say that the whole of the nitrogen in the seeds with which we have to deal exists as albuminoids. But we may safely assume that the nearer they approach to perfect ripeness the less of non-albuminoid nitrogenous matters will they contain; and in the case of the cereal grains at any rate, it is probable that if really perfectly ripe they will contain very nearly the whole of their nitrogen as albuminoids. With regard to some leguminous and other seeds, which contain peculiar nitrogenous bodies, the range may however be wider.

But whatever the condition of the nitrogenous bodies in the seeds we grow or sow, with germination begins a material change. Albuminoids are transformed into peptones, or peptone-like bodies, or degraded into various amido- or other compounds. Such change into more soluble and more diffusible bodies is, it is to be supposed, essential to their free migration, and to their subserviency to the purposes of growth. In the case of the germination, especially of some leguminous seeds, asparagine has been found to be a very prominent product of such degradation of the albuminoids; but it would seem that this disappears as the green parts are developed. But now the plant begins to receive supplies of nitrogen from the soil, as nitrates or ammonia, and it would seem that amides constitute a considerable proportion of the produced nitrogenous bodies, apparently as an intermediate stage in the formation of albuminoids. At any rate, such bodies are found to exist largely in the immature plant; whilst the amount of them diminishes as the plant, or its various parts, approach to maturity.

But not only have we thus, in unripened vegetable productions, a greater or less, and sometimes a very large, proportion of the nitrogenous bodies formed within the plant, existing as amido-compounds, but we may have a large amount existing in the juices as nitric acid, and some as ammonia, &c. Thus, E. Schulze determined the nitric acid in various "roots;" and he found that, in some mangels, more than one-third of the total nitrogen existed in that form, and about one-tenth as much as ammonia. In a considerable series at Rothamsted, we have found an extremely variable proportion existing as nitric acid, according to the size, succulence, or degree of maturity, of the roots; the amount being, as a rule, the least with the ripest and less highly nitrogenous roots, and the most with the most succulent, unripe, and highly nitrogenous ones. In some cases it reached as much as from 20 to nearly 30 per cent. of the total nitrogen. In many other immature vegetable products nitric acid and ammonia have been found; but, so far as I remember, in none in anything like so large a proportion as in the so-called "root-crops," especially mangels. In many, however, the quantity appears to be immaterial; and it is remarkable that whilst there is so much in the "roots," little or none is found in potatoes.

No wonder that, in the experiments already referred to, we found the feeding result to be the worse the more succulent and immature the roots, and the higher their percentage of nitrogen, accordingly.

But it is to the difference in amount of the albuminoid bodies themselves, in different descriptions of vegetable produce, that I wish specially to direct attention, making, however, some reference to what is known of the proportion of the nitrogen existing as amido-compounds.

In some mangels E. Schulze found only from about 20 to 22 per cent. of their total nitrogen to exist as insoluble and soluble albumin. But he found in one case 32·5, and in the other 40·8 per cent. of the total nitrogen as amides. In a large series of determinations at Rothamsted, by Church's method, we found a variation of from under 20 to over 40 per cent. of the total nitrogen of mangels to exist as albuminoids; or, in other words, from nearly 60 to over 80 per cent. of it in the non-albuminoid condition.

In potatoes Schulze found from under 50 to 65 per cent. of the total nitrogen as soluble and insoluble albumin, and from 27·7 to 49·1 per cent. as neutral and acid amides. In a series of potatoes grown at Rothamsted, under very various conditions as to manuring, and in two different seasons, we found the nitrogen as albuminoids to range from little over 50 to more than 71 per cent. of the total nitrogen, leaving, of course, from less than 30 to nearly 50 per cent. to be accounted for in other ways.

Kellner determined the amount of nitrogen as albuminoids, and as amido-compounds, in a considerable series of green foods, both leguminous and gramineous, cut at different stages of their growth. The proportion of the total nitrogen not as albuminoids was, upon the whole, greater in the leguminosæ than in the gramineæ. In both, however, the proportion as albuminoids increased as the plants approached to maturity. The proportion as albuminoids was in all these products very much larger than in roots, and generally larger than in potatoes. In the case of first-crop meadow-hay we found in the separated gramineous herbage 76·4, in the leguminous herbage 82, and in the miscellaneous herbage 80·3 per cent. of the nitrogen as albuminoids; and in the second crop 86·2 per cent. in the gramineous, 88·3 per cent. in the leguminous, and 88·1 per cent. in the miscellaneous herbage. How far the higher proportion of the nitrogen as albuminoids in the second crops is to be taken as any indication of the characteristics of the autumn growth, or how far it is to be attributed to the accidental condition of the weather, may be a question.

These illustrations are sufficient to give some idea of the range and proportion of the nitrogen in different feeding crops which does not exist as albuminoids; and they are sufficient to show that a very large proportion of the non-albuminoid matter exists as various amido-compounds. The question arises, therefore, whether these bodies contribute in any way to the nutrition of the animals which feed upon them? We have but little experimental evidence on this point. As green herbage is the natural food of many descriptions of animal, we might suppose that characteristic constituents of it would not be without some value as food; but the cultivated root crops are much more artificial productions, and it is in them that we find such a very large proportion of non-albuminoid nitrogen. With respect to some of the amido compounds, at any rate, direct experiments seem to show that they are digested in the animal body, and increase the elimination of urea. Weiske and Schrodt found that rabbits receiving, as their only nitrogenous food, either asparagine or gelatin, wasted and died; but a rabbit receiving both asparagine and gelatin increased in weight and survived to the end of the experiment, which lasted seventy-two days. From the results of other experiments made with sheep, they concluded that both asparagine and gelatin protect the albuminoids of the body from oxidation.

These considerations lead me, in conclusion, to refer briefly, and I promise it shall be as briefly as is consistent with clearness, to the two very much disputed questions of *the origin of muscular power*, and *the sources of the fat of the animal body*. These subjects Mr. Lawes and myself have frequently discussed elsewhere; but as the controversy has assumed a new phase quite recently it seems desirable and appropriate that I should recur to it on the present occasion.

With regard to the question of the sources in the food of the fat of the animal body, Liebig originally maintained that although fat might be formed from the nitrogenous compounds within the body, the main source of it in the herbivora was the carbohydrates. In his later writings he sharply criticised the experiments and arguments of those who have maintained the formation of fat chiefly from the proteine compounds, but he at the same time seems to attach more importance to that source than he formerly did. He gives it as his opinion that the question cannot be settled by experiments with herbivora. He adds that what we know with certainty is that, with these animals, albuminates and carbohydrates work together to produce fat; but whether the non-nitrogenous product, fat, has its origin in the albumin or in the carbohydrate he considers it not easy to determine.

At the time when we commenced our experiments on the feeding of animals in 1847 the question whether the fat of the animals fed for human food was mainly derived from albuminoids or from carbohydrates had been scarcely raised, or at least it was not prominent. The question then was rather—whether the herbivora received their fat ready formed in their food, or whether it was produced within the body—the latter view being that which Liebig had so forcibly urged, at the same time maintaining that at any rate its chief source was the carbohydrates. Accordingly our experiments were not specially arranged to determine whether or not the whole of the fat produced could or could not be derived from the albuminoids.

For each description of animal, oxen, sheep, and pigs, such foods as had been established by common experience to be appropriate were selected. The general plan of the experiments was to give to one set a fixed amount of a recognised good food, containing known quantities of nitrogen, fatty matter, &c., to another set the same amount of another food, of different characters in these respects; to other sets also fixed amounts of other foods in the same way: and then there was given to the whole series the same complementary food *ad libitum*. Or, to one set was supplied a uniform food rich in nitrogen, and to others uniform foods poorer in nitrogen, and so on, in each case *ad libitum*.

It will be seen that in this way a great variety of dietaries was arranged, and it will be observed that in each case the animals themselves fixed their consumption according to the requirements of the system.

As already indicated, the individual nitrogenous and non-nitrogenous compounds of the foods were not determined. As a rule, the constituents determined were—the total dry matter, the ash, the fatty matter, and the nitrogen; from which last the amount of nitrogenous compound it might represent was calculated by the usual factor. But, as already said, the results so obtained were only used with considerable reservation, especially in the case of all immature vegetable produce. Nor was the crude fibre determined; but, as in the case of the estimated nitrogenous substance, when interpreting the results, it was always considered whether or not the food contained much or little of probably indigestible woody matter.

The animals being periodically weighed, we were thus able to calculate the amounts of the so-estimated nitrogenous substance, and of the total non-nitrogenous substance, including and excluding fat, consumed—*for a given live-weight within a given time*, and *to produce a given amount of increase in live-weight*.

Experiments were made with a large number of sheep and a large number of pigs. And, even without making allowance for the different condition of the nitrogenous or of the non-nitrogenous constituents, in comparable foods, the results so uniformly indicated that, both the amount consumed by a given live-weight of animal within a given time, and that required to produce a given amount of increase, were determined much more by the amount of the non-nitrogenous than by that of the nitrogenous constituents which the food supplied. And when allowance was made for the different condition of the nitrogenous constituents, and for the greater or less amount of the non-nitrogenous ones which would probably be indigestible and effete, the indications were still more remarkable and conclusive.

In very many cases the animals were slaughtered, and carefully examined as to whether the tendency of development had been more that of growth in frame and flesh, or in fatness. Here, again, the evidence was clear, that the tendency to growth in frame and flesh was favoured by a high proportion of nitrogen in the food, and that to the production of fat by a high proportion of digestible non-nitrogenous constituents.

In a few cases the actual amount of fat in the animals in the lean, and in the fat condition, was determined; and the results admitted of no doubt whatever that a very large proportion of the stored-up fat could not have been derived from the fatty matter of the food, and must have been produced within the body.

So decisive and consistent were the very numerous and very varied results in regard to these points, that we had no hesitation in concluding—not only that much of the fat stored up was produced within the body, but that the source of much, at any rate, of the produced fat must have been the non-nitrogenous constituents of the food—in other words, the *carbohydrates*.

As already stated, however, as the question whether the source of the produced fat was the proteine compounds or the carbohydrates was not then prominent, we had not so arranged the experiments as to obtain the largest possible increase in fat with the smallest possible supply of nitrogenous compounds in the food, nor did we then even calculate whether or not there was sufficient nitrogenous matter consumed to be the source of the whole of the fat produced.

This question, indeed, excited very little interest, until, at a meeting of the Congress of Agricultural Chemists held at Munich in 1865 (at which I happened to be present), Prof. Voit, from the results of experiments made in Pettenkofer's respiration apparatus with dogs fed on flesh, announced his conclusion that fat must have been produced from the nitrogenous substance, and that this was probably the chief, if not the only, source of the fat, even of herbivora—an opinion which he subsequently urged much more positively.

In the discussion which followed the reading of Prof. Voit's paper, Baron Liebig forcibly called in question his conclusions; maintaining not only that it was inadmissible to form conclusions on such a point in regard to herbivora, from the results of experiments made with carnivora, but also that direct quantitative results obtained with herbivorous animals had afforded apparently conclusive evidence in favour of the opposite view.

Voit's paper excited considerable controversy, in which Mr. Lawes and myself joined. We maintained that experiments to determine such a question should be made not with carnivora or omnivora fed on flesh, but with herbivora fed on their appropriate fattening food, and on such herbivora as common experience showed to be pre-eminently fat-producers. We pointed out[1] that the pig comprised, for a given live-weight, a comparatively small proportion of alimentary organs and contents; that, compared with that of the ruminants, his food was of a high character, yielding, for a given weight of it, much more total increase, much more fat, and much less necessarily effete matter; that, in proportion to his weight, he consumes a larger amount of food, and yields a larger amount, both of total increase and of fat, within a given time; and, lastly, that he contains a larger proportion of fat, both in a given live-weight and in his increase whilst fattening.

It is obvious that with these characteristics there is much less probable range of error in calculating the amount and the composition of the increase in live-weight in relation to the amount and composition of the food consumed, than in the case of ruminants; and that therefore the pig is very much more appropriate for the purpose of experiments to determine the sources in its food of the fat it produces.

Accordingly we calculated a number of our early experiments made with pigs, to determine whether or not the nitrogenous substance they consumed was sufficient for the formation of the fat they produced. For simplicity of illustration, and to give every possible advantage to the view that nitrogenous substance might have been the source of the produced fat, we assumed the whole of the crude fat of the food to have been stored up in the animal—thus estimating a minimum amount to be produced. Then again we supposed the whole of the nitrogenous substance of the food to be perfectly digested, and to become available for the purposes of the system. Lastly, after deducting the amount of nitrogenous substance estimated to be stored up as such, the whole of the remainder was reckoned to be so broken up that no other carbon-compounds than fat and urea would be produced.

The result was that, even adopting these inadmissible assumptions in all the cases in which, according to common experience, the food was admittedly the most appropriate for the fattening of the animal, the calculation showed that a large amount of fat had been produced which could not have been derived from the nitrogenous substance of the food, and must therefore have had its source in the carbohydrates. Such a result is moreover entirely accordant with experience in practical feeding.

Reviewing the whole subject in great detail in 1869, Prof. Voit refers to these results and calculations. He confesses that he has not been able to get a general view of the experiments from the mass of figures recorded, and from his comments he shows that he has on some points misunderstood them. He admits, however, that as the figures stand, it would appear that fat had, in some instances, been derived from the carbohydrates. Still, he says, he cannot allow himself to consider that a transformation of carbohydrates into fat has thus been proved.

Prof. Emil von Wolff again in his "Landwirthschaftliche Fütterungslehre," referring to the same experiments, admits that they are almost incomprehensible unless we assume the direct concurrence of the carbohydrates in the formation of fat. He nevertheless seems to consider that evidence of the kind in question is inconclusive; and he suggests that experiments with pigs should be made in a respiration apparatus to determine the point.

Mr. Lawes and myself entertained, however, the utmost confidence that the question was of easy settlement without any such apparatus, provided only suitable animals and suitable foods were selected. I, accordingly, gave a paper on the subject in the *Section für Landwirthschaft- und Agricultur-Chemie*, at the Naturforscher Versammlung, held at Hamburg

[1] "On the Sources of the Fat of the Animal Body," *Phil. Mag.*, December, 1866.

in 1876.[1] The points which I particularly insisted upon were—that the pig should be the subject of experiment; that he should be allowed to take as much as he would eat of his most appropriate fattening food, so that his increase, and the fat he produced, should bear as large a proportion as possible to his weight, to the total food, and to the total nitrogenous substance consumed. Finally, it was maintained that, if these conditions were observed and the constituents of the food determined, and those of the increase of the animal estimated according to recognised methods, the results could not fail to be perfectly conclusive without the intervention either of a respiration apparatus or of the analysis of the solid and liquid matters voided.

Results so obtained were adduced in proof of the correctness of the conclusions arrived at. We at the same time admitted that although, for reasons indicated, we had always assumed that fat was formed from the carbohydrates in the case of ruminants as well as of pigs, yet, as in our experiments with those animals we had supplied too large amounts of ready-formed fat, or of nitrogenous matter, or of both, it could not be shown so conclusively, by the same mode of calculation in their case as in that of pigs.

In the discussion which followed, Prof. Henneberg agreed that it seemed probable that fat could be formed from the carbohydrates in the case of pigs. In the case of experiments with other animals, however, the amount of fat produced was too nearly balanced by the amount of fat and albuminous matters available, to afford conclusive evidence on the point.

Quite recently Prof. Emil von Wolff (*Landwirthschaftliche Jahrbücher*, Band viii. 1879, Supplement) has applied the same mode of calculation to results obtained by himself with pigs some years ago. He concluded that the whole of the body fat could not have been formed without the direct co-operation of the carbohydrates of the food. But what is of greater interest still is that he also calculated in the same way the results of some then quite recent experiments of Henneberg, Kern, and Wattenberg, with sheep. He thus found that, even including the whole of the estimated amides with the albumin, there must have been a considerable production of fat from the carbohydrates; and, excluding the amides, the amount reckoned to be derived from the carbohydrates was of course much greater.

I will only add, on this point, that on recalculating some of our early results with sheep, which did not afford sufficiently conclusive evidence when the whole of the nitrogen of the food was reckoned as albumin, show a very considerable formation of fat from the carbohydrates if deduction be made for the probable amount of non-albuminoid nitrogenous matter of the food.

We have now, then, the two agricultural chemists of perhaps the highest authority, both as experimenters and writers on this subject on the continent, giving in their adhesion to the view, that the fat of the herbivora, which we feed for human food, may be, and probably is, largely produced from the carbohydrates. I dare say, however, that some physiologists will not change their view until Voit gives them sanction by changing his, which, so far as I know, he has not yet done.

The question which has been currently entitled that of "The Origin of Muscular Power," or "The Sources of Muscular Power," has also been the subject of much investigation, and of much conflict of opinion, since the first publication of Liebig's views respecting it in 1842.

As I have already pointed out, he then maintained that the amount of muscular tissue transformed, the amount of nitrogenous substance oxidated, was the measure of the force generated in the body. He accordingly concluded that the requirement for the nitrogenous constituents of food would be increased in proportion to the increase of the force expended. In his more recent writings on the subject, he freely criticises those who take an opposite view. He nevertheless grants that the secretion of urea is not a measure of the force exerted; but, on the other hand, he does not commit himself to the admission that the oxidation of the carbohydrates is a source of muscular power.

The results of our own early and very numerous feeding experiments were, as has been said, extremely accordant in showing that, provided the nitrogenous constituents in the food were not below a certain rather limited amount, it was the quantity of the digestible and available non-nitrogenous constituents, and not that of the nitrogenous substance, that determined—both *the amount consumed by a given live-weight within a given time*, and *the amouut of increase in live-weight produced*. They also showed that one animal, or one set of animals, might consume two or three times as much nitrogenous substance in proportion to a given live-weight within a given time as others in precisely comparable conditions as to rest or exercise. It was further proved that they did not store up nitrogenous substance at all in proportion to the greater or less amount of it supplied in the food, but that the excess reappeared in the liquid and solid matters voided.

So striking were these results, that we were led to turn our attention to human dietaries, and also to a consideration of the management of the animal body undergoing somewhat excessive labour, as, for instance, the hunter, the racer, the cab-horse, and the foxhound, and also pugilists and runners. Stated in a very few words, the conclusion at which we arrived from these inquiries (which were summarised in our paper given at Belfast in 1852) was, that unless the system were overtaxed, the demand induced by an increased exercise of force was more characterised by an increased requirement for the more specially respiratory, than for the nitrogenous, constituents of food.

Soon afterwards, in 1854, we found by direct experiments with two animals in exactly equal conditions as to exercise, both being in fact at rest, that the amount of urea passed by one feeding on highly nitrogenous food was more than twice as great as that fed on a food comparatively poor in nitrogen.

It was clear therefore that the rule which had been laid down by Liebig, and which has been assumed to be correct by so many writers, even up to the present time, did not hold good—namely, that "The sum of the mechanical effects produced in two individuals in the same temperature is proportional to the amount of nitrogen in their urine; whether the mechanical force has been employed in voluntary or involuntary motions, whether it has been consumed by the limbs or by the heart and other viscera"—unless, indeed, as has been assumed by some experimenters, there is, with increased nitrogen in the food, an increased amount of mechanical force employed in the "involuntary motions" sufficient to account for the increased amount of urea voided.

The question remained in this condition until 1860, when Bischoff and Voit published the results of a long series of experiments made with a dog. They found that, even when the animal was kept at rest, the amount of urea voided varied closely in proportion to the variation in the amount of nitrogenous substance given in the food—a fact which they explained on the assumption that there must have been a corresponding increase in the force exercised in the conduct of the actions proceeding within the body itself in connection with the disposal of the increased amount of nitrogenous substance consumed. Subsequently, however, they found that the amount of urea passed by the animal was, with equal conditions as to food, &c., no greater when he was subjected to labour than when at rest; whilst, on the other hand, the carbonic acid evolved was much increased by such exercise. They accordingly somewhat modified their views.

In 1866 appeared a paper by Professors Fick and Wislicenus, giving the results obtained in a mountain ascent. They found that practically the amount of urea voided was scarcely increased by the labour thus undertaken. Prof. Frankland gave an account of these experiments in a lecture at the Royal Institution in the same year; and he subsequently followed up the subject by an investigation of the heat developed in the combustion of various articles of food, applying the results in illustration of the phenomena of the exercise of force.

Lastly, Kellner has made some very interesting experiments with a horse at Hohenheim, the results of which were published last year. In one series the experiment was divided into five periods, the same food being given throughout; but the animal accomplished different distances, and drew different weights, the draught being measured by a horse-dynamometer. The changes in live-weight, the amount of water drunk, the temperature, the amount of matters voided, and their contents in nitrogen, were also determined.

The result was that with only moderate labour there was no marked increase in the nitrogen eliminated in the urine; but that with excessive labour the animal lost weight and eliminated more nitrogen. Kellner concluded, accordingly, that, under certain circumstances, muscular action can increase the transformation of albumin in the organism in a direct way; but that, nevertheless, in the first line is the oxidation of the non-nitrogenous matters—carbohydrates and fat, next comes in requisition the circulation-albumin, and finally the organ-albumin is attacked.

In reference to these conclusions from the most recent experi-

[1] The substance of that communication is given in the *Journal of Anatomy and Physiology*, vol. xi. part iv.

ments relating to the subject, we may wind up this brief historical sketch of the changes of view respecting it, with the following quotation from our own paper published in 1866:[1]—". . . all the evidence at command tended to show that by an increased exercise of muscular power there was, with increased requirement for respirable material, probably no increased production and voidance of urea, unless, owing to excess of nitrogenous matter in the food, or a deficiency of available non-nitrogenous substance, or diseased action, the nitrogenous constituents of the fluids or solids of the body were drawn upon in an abnormal degree for the supply of respirable material."

In conclusion, although I fully agree with Voit, Zuntz, Wolff, and others, that there still remains much for both Chemistry and Physiology to settle in connection with these two questions of "The Sources of the Fat of the Animal Body" and "The Origin of Muscular Power," yet I think we may congratulate ourselves on the re-establishment of the true faith in regard to them, so far at least as the most important practical points are concerned.

[1] Food in its relations to various exigencies of the animal body.—*Phil. Mag.*, July,

THE GERMAN ASSOCIATION

THE fifty-third congress of the Association of German Naturalists and Physicians has been held at Danzig during the past week. At the first general meeting on Saturday, September 18, Dr. H. Abegg, who filled the post of president, in a brief speech of welcome to his colleagues expressed his pleasure at finding that the congress was so numerously attended. There had been fears that Danzig, owing partly to its somewhat isolated position, would have kept many from visiting it who would otherwise have come, had the point of meeting been fixed in a more southerly part of Germany. But these fears were wholly groundless; from far and near he was rejoiced to see additions to their body; and to all and each of his esteemed colleagues he bade hearty welcome.

Herr von Ernsthausen, Prof. Bail, and the Chief Burgomaster of Danzig, also gave short addresses, in which they confirmed the sentiments of the President.

So far as the reports in the admirable *Tageblatt* go, the following are some of the principal papers and lectures:—

The first paper read was by Prof. Hermann Cohn of Breslau, "On Writing, Type, and the Increase of Shortsightedness." Myopia, *i.e.*, shortsightedness, or the inability to distinguish objects at a distance, was, as he said, rarely or never born with the subject; it is generally induced by an injurious method of study which strains the eye during childhood. In 1865 the Professor began to collect statistics such as the schools in his own native town offered to him, and from these he was able to establish the following facts:—

1. That cases of shortsightedness occur rarely in village schools; their frequency increases in proportion to the demand made upon the eye in higher schools and colleges; so that in gymnasia myopia is most prevalent.

2. That the number of shortsighted scholars in all schools and colleges increases in proportion as one examines the higher grades or classes.

3. That the average of myopia increases from class to class; *i.e.*, those who are shortsighted become more and more so.

These conclusions have since met with universal confirmation. Among the causes which tend to increase the malady, the Professor specified school desks constructed regardless of hygienic principles, lesson-books of which the typography is cramped and indistinct, and badly and insufficiently lighted schoolrooms. All these as they now existed were more or less unsatisfactory, and could bear alteration with perceptible benefit to the scholar. Indeed to make reforms in this direction was, as he showed, the duty of the State; and he hoped that a Government commission might ere long be appointed to regulate the construction of school-desks, the typography of lesson-books, and the lighting of class-rooms. By this means the evil which was so rapidly increasing might be met, and the percentage of shortsightedness thereby reduced to a far lower minimum than was at present the case.

The next address was given by Prof. Eduard Strasburger of Jena, "On the History and the present State of the Cell Theory." Having sketched at some length the growth and the development of this theory, the learned professor remarked in conclusion:—

"The results of research into cell-structure are well adapted to teach us a great deal about the complicated nature of the fundamental substance of life; and complicated this must be, to produce such a series of phenomena in constant succession. We have merely to accustom ourselves to regard protoplasm, not as a simple substance, but, on the contrary, as a highly organised body, or we have otherwise no means of explaining the phenomena of life. It is at any rate a fact that a lump of protoplasm, the ovum, is capable, after union with another particle of protoplasm, of reproducing the entire parent organism in its complicated structure. That the properties of an egg are not essentially different from those of other protoplasm, but that rather only one part of the protoplasm in the egg is specially suited for reproduction is proved by the fact that other masses of protoplasm in the organism become often capable of reproducing it in a perfect form. The behaviour of *Begonia* leaves is specially striking; and I therefore submit a specimen of them to you. It is well known that new plants are engendered from such leaves. Microscopical investigation shows us that in these leaves there are separate epidermal cells which reproduce the whole plant; the protoplasm of a single such cell affords, therefore, the basis for an entirely new organism. Thus the process does not differ in principle from the formation of a germ from the egg.

"The attributing of all the functions of life to protoplasm is to be looked upon as a great advance in science; although it is impossible for us, so far, even to form hypotheses with regard to the forces which are at work in the protoplasm. It will be the task of the future to throw light upon this side of the question. Shall we ever be able to gain a deeper insight into the final, the invariable causes of life? At the present it were futile to attempt this. The progress which science has made in the last ten years, often yielding quite unexpected results, leads us to hope for yet further advance; and in the seeking for knowledge, rather than in its final acquisition, it is that our highest pleasure lies."

In the sectional sitting for Mathematics and Astronomy held on the following Monday, September 20, Director B. Ohlert read a paper "On the Rapid Motion of the inner Moon of Mars in the light of Laplace's Theory." He pointed out that the fact that the inner moon of Mars passes round the planet in a far shorter time than the latter needs for rotation on its own axis would seem to be in contradiction to the hypothesis of Laplace on the origin of our planetary system. The lecturer further showed that there was nothing very remarkable in the *rapidity of the motion of this moon*, which, owing to the slight distance from Mars, was wholly in agreement with the third law of Kepler; but rather that an explanation was needed of the *slow axial motion of the planet itself*, and similarly of the other planets. And hereupon Prof. Ohlert adduced proofs from which, according to his view, and in conformity with the assumption of Laplace, the rapidity of the axial motion of the planets in the final period of their formation would of necessity become diminished.

Dr. Franz then followed with a paper "On the Observation of Double-Stars made at the Königsberg Observatory, and on certain Peculiarities of the Königsberg heliometer."

The Section for Anthropology and Prehistoric Research held a sitting on the same day, with Dr. Stieda in the chair. Dr. Anger of Elbing exhibited a rich collection of anthropological specimens, chiefly illustrating the antiquity of the district.

In the Botanical Section Prof. Bail read a valuable paper "On Underground Fungi," in which he stated that the several species and varieties of these in Germany must certainly exceed the usually accepted number.

Prof. Moebius of Kiel, in the Section for Zoology and Comparative Anatomy, read (also on the same day) an interesting monograph "On the Importance of the Foraminifera for the Doctrine of Descent."

He began by quoting Dr. Carpenter's view that the genera and species of the Foraminifera cannot be determined after the usual method, but that the only natural classification of the great mass of different forms is to arrange them in accordance with their degree of relationship. Prof. Moebius himself had come to the conclusion from his researches among the Foraminifera which he had collected in Mauritius in 1874 that the repeatedly occurring peculiarities among the Foraminifera may serve and must serve us in forming an idea of their nature and zoological position.

The sarcode of the Foraminifera behaves with regard to the formation of the skeleton and shell just as does the protoplasm of the eggs of the Metazoa to the formation of the germs and of all organs proceeding from them. Like the protoplasm of the egg, it possesses a quite definite and hereditary capacity for self-development.