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LXX. *On the Origin of Muscular Power.* *By* Dr. A. Fick, *Professor of Physiology, Zurich; and* Dr. J. Wislicenus, *Professor of Chemistry, Zurich**.

IT is now a universally acknowledged fact that muscular action is brought about by chemical changes alone; and the proposition, that it is by processes of oxidation that the muscles are rendered capable of performing work, would be just as little likely to meet with contradiction. But all are not agreed as to what the substance is, which, by oxidation, furnishes the store of actual energy which is capable of being in part transmuted into mechanical work. Most physiologists and chemists appear to think that the oxidation of albuminoid substances alone can generate muscular power. Even quite recently Playfair† has published a special treatise to prove this hypothesis. The beautiful investigations of J. Ranke‡ seem also to point indubitably in the same direction. In many manuals of physiology the proposition in question is laid down as a self-evident principle. The chief reason why it numbers so many adherents may lie in the following reflection made, more or less consciously, by most of them:—The action of the muscle is connected with the destruction of its substance, by far the greater part of which is of an albuminoid nature; therefore the destruction by oxidation of albuminoid bodies is the essential condition of the muscle's mechanical action. The fallacy of this line of argument will be immediately apparent if, for instance, we apply it to a locomotive. "This machine consists essentially of iron, steel, brass, &c.; it contains but little coal; therefore its action must depend on the burning of iron and steel, not on the burning of coal." In like manner it is by no means self-evident that it is

* Communicated by the Authors, and translated by Professor Wanklyn.

† "On the Food of Man in relation to his useful Work," read to the Royal Society of Edinburgh, April 3, 1865.

‡ Tetanus, a Physiological Study. Leipzig, 1865.

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specially the oxidation of albuminous compounds which produces muscular force. It is quite possible that the non-nitrogenous substances in muscle play the part of combustibles, although but a small quantity of those substances is to be found in muscle at any particular moment. It is even conceivable that these compounds pass, as it were, through the muscle in a rapid stream, each particle which enters it being immediately oxidized and carried away again. If we examine this supposition more closely, we find that, even from the most general points of view, there is much to be said for the hypothesis that non-nitrogenous compounds form the fuel or oxidizable materials in the muscles.

Liebig long ago pointed out that the non-nitrogenous organic compounds of the food, particularly the hydrates of carbon and the fats, are the sources of heat. He, indeed, could hardly have thought of fuel generating mechanical power, chiefly because our present problem was far beyond the chemists and physiologists of that time. But from the point to which science has at present attained, if once a certain group of materials of food are found to be heat-producers, it is easy to conceive the derivation from the oxidation of these substances, not merely of heat, but also of the mechanical work of the organism, since, as is now well known, heat and mechanical work are only two manifestations of the same force. In fact it would be very strange if, in the animal economy, one particular group of food-constituents was used merely for the production of heat, in order that the temperature of the body might be maintained above that of the surrounding medium. No doubt this animal warmth is a necessary condition of existence for mammals and birds; but the mechanical theory of heat teaches that the almost inevitable secondary result of the production of muscular work must be heat, and that therefore no special processes are necessary to raise the temperature of the organism, the latter, in fact, going hand in hand with the production of mechanical power. If the non-nitrogenous compounds were exclusively heat-producers in the narrower sense, and the albuminoid bodies, on the contrary, only force-producing material, nature would have proceeded as uneconomically as a manufacturer who should put up a stove near a steam-engine, although a sufficient amount of heat was given out by the steam-engine itself. Although at present, in the light of Darwin's theory, the employment of teleological arguments in a certain order of cases has once more become admissible, yet we are far from being of opinion that a chemico-physiological question like the above is capable of being decided by such arguments. Nevertheless considerations of this nature may serve to shake other hypotheses which rest only on a teleological basis.

The doctrine that muscular force is produced only by the oxidation of albuminoid compounds, is, however, much more seriously shaken by the important investigations of Edward Smith, who has shown, in the most convincing manner, that the production of carbonic acid in the human body may be increased tenfold by muscular exertion, whilst the excretion of urea proceeds with tolerable uniformity. The latter fact has also been frequently observed by other inquirers, viz. by Bischoff and Voit (in part before the investigations of E. Smith). The numbers given by Smith do not, however, furnish quite a direct disproof of the doctrine in question. If any adherent of that doctrine felt inclined to retain it on any terms, he might reply to Smith, "Probably muscular action necessarily excites the process of oxidation of non-nitrogenous substances without these compounds having anything to do with the production of that action." On the other hand, the objection might be made to Smith, that perhaps, when there is violent exertion of the muscles, though the metamorphosis of compounds containing nitrogen is increased, yet the secretion of urea is not larger, because the results of the metamorphosis of these compounds leave the body in other forms.

There is one way in which the question whether muscular force can be generated only by the oxidation of albuminoid compounds might be decisively negatived, and that possibly by a single experiment. It is suggested by the following simple line of thought. Granting that a person might accomplish a certain measurable amount of external labour, say m metre-kilogrammes, and that in so accomplishing it he oxidized p grammes of albumen in his muscles; granting also that we know the amount of heat which is liberated when a gramme of albumen is changed by oxidation into the products of decomposition in which the constituents of albumen leave the human body; then if the thermic equivalent of the manual labour m be greater than the amount of heat which could possibly be produced by the oxidation of p grammes of albumen, the question may be negatived with the most complete certainty. But if, on the contrary, the thermic equivalent of m metre-kilogrammes is less than that of the heat arising from the oxidation of p grammes of albumen, the question has by no means received an affirmative answer. It is only in the former case that the experiment has a decisive result.

Such an experiment has been made by us conjointly. It is true that the quantities which require to be determined, with the exception of the mechanical work, cannot have any exact numerical value assigned to them; but we can confine their values within certain limits, so that a satisfactory conclusion may nevertheless be obtained. As measurable external labour we chose the ascent of a mountain-peak, the height of which was known. We preferred the mountain to a *treadmill*, not merely because the ascent is a more entertaining employment, but chiefly for the reason that we had no suitable treadmill at our disposition. Of the numerous peaks of the Swiss Alps, the one most suitable for our purpose appeared to be the Faulhorn, near the lake of Brienz, in the Bernese Oberland. It was necessary that the mountain which was to serve for our experiment should be as high as possible, and nevertheless should permit of our passing a night on its summit under tolerably normal circumstances; for had we been obliged immediately to descend again, the measurable amount of work would have been at once followed by an undeterminable but violent exertion of the muscles, in which much metamorphosis would occur, the thermic equivalent of which would be, however, entirely liberated as heat. The Faulhorn satisfies all these requirements; for although its height is very considerable, rising to about 2000 metres above the lake of Brienz, yet there is an hotel on its summit. Besides, it can be ascended by a very steep path, which was, of course, favourable for our experiment, because the amount of muscular action which is lost and not calculable (being reconverted into heat) is thus reduced to a minimum. We chose the steepest of the practicable paths; it starts from a little village on the lake of Brienz called Iseltwald, and at a moderate pace the summit may be reached in less than six hours.

In order to diminish as far as possible the unnecessary consumption (*Luxusconsumtion*) of albumen during our experiment, we took no albuminoid food from midday on August 29 until 7 o'clock in the evening of August 30. During these thirty-one hours we had nothing in the way of solid food except starch, fat, and sugar. The two former, following F. Ranke's directions, were taken in the form of small cakes. Starch was rubbed up with water, and the thin paste thereby produced was fried with plenty of fat. The sugar was taken dissolved in tea. There was also the sugar contained in the beer and wine, which were taken in the quantity usual on foot tours.

The experiment proper began on the evening of the 29th of August, at a quarter past 6 P.M., by a complete evacuation of the bladder. The urine from this time till 10 minutes past 5 on the morning of the 30th was collected and measured; we will call it *night urine*; we took a specimen of it with us for examination. Then the urine secreted in the time between 10 minutes past 5 in the morning and 20 minutes past 1 in the afternoon of the 30th of August was also measured. During this time the ascent of the mountain was performed, it may therefore be designated

the *work urine*. Again, the amount of urine secreted between 20 minutes past 1 and 7 in the evening was determined; this we will call simply the *after-work urine*. During this time we remained, for the most part, in the house without any muscular exertion worth mentioning. After 7 o'clock a plentiful meal, consisting chiefly of meat, was taken, and lastly the urine excreted during the night spent in the hotel of the Faulhorn (that is to say, from 7 o'clock in the evening of the 30th till half-past 5 in the morning of the 31st of August) was measured and again designated *night urine*. Arrived at the hotel on the summit of the Faulhorn, determinations of the quantity of urea in the specimen of the *night urine* from the 29th to the 30th, and of the *work urine* excreted between half-past 5 in the morning and 20 minutes past 1, shortly after the end of the ascent, were proceeded with. The method followed was precisely that given by Neubauer for absolute determinations of urea.

Phosphoric and sulphuric acids were first precipitated from 40 cubic centims. of urine by 20 cubic centims. of solution of baryta, which quantity was proved to be sufficient; and 15 cubic centims. of the filtrate (=10 cubic centims. of urine) were employed for the determination of chlorine by Liebig's method, with a solution of nitrate of mercury of which 1 cubic centim. corresponded to 0·01 grm. of common salt. From 30 cubic centims. of the filtrate the chlorine was then precipitated with the exact amount of silver solution necessary, as indicated by the amount of mercury solution previously employed. A portion of the filtrate (= to 5 cubic centims. of urine) was then employed in making an approximative determination of the amount of urea, and with another portion (= the 10 cubic centims. of urine) an exact determination of urea was made with the necessary mercury solution (1 cubic centim. =0·01 grm. of urea). These operations were performed with every possible precaution.

On the morning of the 30th of August we made similar determinations of the *after-work urine* of the preceding day, collected between 20 minutes past 1 P.M. and 7 o'clock in the evening. A specimen of each kind of urine was also sealed up in a perfectly filled and well-corked and sealed bottle, in order that on our return to Zurich the yet more important determination of the absolute quantity of nitrogen might be undertaken; the same thing was done with the second *night urine*, collected and measured between 7 o'clock in the evening of the 30th of August, and 6 o'clock in the morning of the 31st; but the quantity of urea in this, owing to want of time, was not determined. The chlorine and urea determinations gave the following results:—

I. Night urine from the 29th to the 30th of August. In both cases coloured light yellow, perfectly clear, decided acid reaction.

(*a*) Fick: total quantity of urine 790 cubic centims. In 10 cubic centims. we found 0·0619 grm. of common salt, and, regard being had to the necessary corrections*, 0·1580 grm. of urea. The total quantity of the latter amounted, consequently, to 12·4820 grms.

(*b*) Wislicenus: quantity of urine 916 cubic centims. In 10 cubic centims. 0·03 grm. of salt and 0·1284 grm. of urea; in the whole quantity, therefore, 11·7614 grms. of the latter.

II. Work urine, from half-past 5 in the morning of the 30th of August till 20 minutes past 1.

(*a*) Fick: quantity of urine 396 cubic centims.; pale yellow, clear, acid. In 10 cubic centims. 0·0395 grm. of salt and 0·1776 grm. of urea. Total quantity of urea 7·0330 grms.

(*b*) Wislicenus: quantity of urine 261 cubic centims.; pale yellow, turbid after cooling, acid. In 10 cubic centims. 0·0460 grm. of salt and 0·2566 grm. of urea. Total quantity of urea 6·6973 grms.

III. The after-work urine, from 20 minutes past 1 on the 30th of August till 7 in the evening. In both cases darkish yellow, a sediment being deposited on cooling, acid.

(*a*) Fick: quantity of urine 198 cubic centims. In 10 cubic centims. 0·007 grm. of salt and 0·2612 grm. of urea. Total quantity of urea 5·1718 grms.

(*b*) Wislicenus: quantity of urine 200 cubic centims. In 10 cubic centims. 0·018 grm. of salt and 0·2551 grm. of urea. Total quantity of urea 5·1020 grms.

It is evident that the amount of urea found cannot be the correct measure for the quantity of oxidized albuminoid substances, particularly as a portion of nitrogen, which must not be omitted, must always have been present in the sediment, which consisted almost exclusively of acid urate of soda. It therefore became necessary to ascertain the total quantity of nitrogen present. The urea determinations were undertaken principally in order that, if any accident should happen to any of the specimens of urine on their way to Zurich, there might be at least some data saved for an approximate calculation of the nitrogen.

The absolute nitrogen determinations were made between the 4th and 6th of September, in the laboratory of the Zurich University, with the urine which had remained perfectly fresh. 5 cubic centims. of it were distilled with excess of soda-lime, from a suitable apparatus, into pure hydrochloric acid, the residue heated till it became colourless, and air drawn through the apparatus and through the acid. The determination of the am-

* Neubauer and Vogel, *Analyse des Harnes*, 4 Aufl. p. 143–146.

monia absorbed by the acid was made as usual with perchloride of platinum, the double salt so obtained ignited and weighed, the quantity of nitrogen being calculated from the weight of the metallic platinum found.

I. Night urine from the 29th to the 30th of August.

(*a*) Fick: 5 cubic centims. of urine gave 0·3095 grm. platinum = 0·043768 grm. nitrogen. Total quantity of nitrogen 6·915344 grms.

(*b*) Wislicenus: 5 cubic centims. of urine gave platinum 0·2580 grm. = 0·036485 grm. nitrogen. Total quantity of nitrogen 6·684052 grms.

II. Work urine.

(*a*) Fick: 5 cubic centims. of urine gave platinum 0·2958 grm. = 0·418303 grm. nitrogen. Total quantity of nitrogen 3·312960 grms.

(*b*) Wislicenus: 5 cubic centims. of urine gave platinum 0·4245 grm. = 0·060030 grm. nitrogen. Total quantity of nitrogen 3·133566 grms.

III. The after work urine.

(*a*) Fick: 5 cubic centims. of urine gave platinum 0·4338 grm. = 0·06134545 grm. nitrogen. Total quantity of nitrogen 2·4293 grms.

(*b*) Wislicenus: 5 cubic centims. of urine gave platinum 0·4272 grm. = 0·0604121 grm. nitrogen. Total quantity of nitrogen 2·416484 grms.

IV. Night urine from the 30th to the 31st of August.

(*a*) Fick: 5 cubic centims. of urine gave platinum 0·6601 grm. = 0·0933475 grm. nitrogen. Total quantity of nitrogen in the 258 cubic centims. 4·816731 grms.

(*b*) Wislicenus: 5 cubic centims. of urine gave platinum 0·7001 grm. = 0·099004 grm. nitrogen. Total quantity of nitrogen in the 270 cubic centims. of urine 5·346216 grms.

From these figures result the following comparative Tables, for which the amount of nitrogen in the total quantity of the urea has been calculated:—

Fick.

	Urea.	Nitrogen in the urea.	Total nitrogen.	
Night urine, 29th to 30th of August	12·4820	5·8249	6·9153	
Work urine	7·0330	3·2681	3·3130	5·7423
After-work urine	5·1718	2·4151	2·4293	
Night urine, 30th to 31st of August	....	....	4·8167	

Wislicenus.

	Urea.	Nitrogen in the urea.	Total nitrogen.	
Night urine, 29th to 30th of August .	11·7614	5·4887	6·6841	
Work urine . . .	6·6973	3·1254	3·1336	5·5501
After-work urine .	5·1020	2·3809	2·4165	
Night urine, 30th to 31st of August .	....	....	5·3462	

It is evident that the calculation of the amount of the protein substances in question must be based upon the numbers for the total amount of nitrogen, and therefore that, in order to render the result as convincing as possible, the most unfavourable case (that is, the one which leads to the greatest quantity of protein bodies) must be selected. All real tissue-forming protein bodies, with the exception of permanent cartilage yielding chondrin, contain more than 15 per cent. of nitrogen: we are therefore entitled to make this proportion the basis of our calculation, and we thereby obtain the following numbers for the protein bodies consumed:—

	Fick. grms.	grms.	Wislicenus. grms.	grms.
For night urine, 29th to 30th of August	46·1020		44·5607	
For work urine . .	22·0867	38·2820	20·8907	37·0007
For after-work urine	16·1953		16·1100	
For night urine, 30th to 31st of August	32·1113		35·6413	

Let us, in the first place, use the figures we have obtained, in order to gain an idea of the course of the excretion of nitrogen through the urine during the period of the experiment. For this purpose we divide the amounts of nitrogen given in the first two Tables by the number of hours during which they were secreted, and we obtain,

Average quantity of Nitrogen excreted per hour.

	By Fick. grm.	By Wislicenus. grm.
During the night, 29th to 30th .	0·63	0·61
During the time of work . . .	0·41	0·39
During rest after work	0·40	0·40
During the night, 30th to 31st .	0·45	0·51

A glance at these Tables furnishes a new testimony to the fact, which has often been before experimentally proved, that muscular exertion does not notably increase the excretion of nitrogen through the urine. It declined in our experiment tolerably regularly from the 29th of August till the evening of the 30th*, evidently in consequence of abstinence from food containing nitrogen. In the night of the 30th to the 31st, in spite of the plentiful meal of albuminous food on the evening of the 30th, the secretion of nitrogen was less than on the preceding night. The reason of this perhaps was that during abstinence the secretion of nitrogen (never entirely discontinued) was carried on at the expense of tissues, and now these tissues had first of all to be repaired. We will not pursue such considerations further, but will apply our figures to other conclusions.

We must, it is true, in the first place take our stand upon an hypothesis, which, however, has been well established by many recent investigations. We assume, namely, that the nitrogen of the oxidized albumen leaves the body entirely through the urine. In fact it has lately been proved, on the one hand by Ranke, on the other by Thiry, that neither by the perspiration nor by the breath is any perceptible quantity of nitrogen disposed of. Fortunately we are further in a position to state that during the ascent of the mountain we neither of us perspired to a perceptible degree. During the whole ascent we were enveloped in a cold mist, which prevented us from becoming overheated. Even if any noticeable amount of nitrogen were excreted with the fæces, we should yet be justified in the preceding experiment in neglecting it; for the nitrogenous products of the transmutation of albumen which might possibly be contained in the fæces are doubtless not highly oxidized compounds, and no heat worth mentioning is liberated in their production.

We have now to consider, on the preceding assumptions, what is the largest amount of albumen that can have been oxidized in our bodies during the ascent of the mountain. We think we should be justified in not estimating the albumen oxidized during the hours of work higher than was calculated from the quantity of nitrogen excreted in the work urine (viz. 22·09 for Fick, 20·89 for Wislicenus). In fact the rate of the nitrogen excretion seems to be so entirely regulated by the supply of food, and so completely independent of muscular action, that we can reasonably suppose it to depend only upon the decomposition of protein substances. If any one were to maintain that at the

* The slight apparent deviation of the numbers under Wislicenus ought hardly to come into consideration, as they may easily have been affected by the retention of some urine in the bladder, or by other similar disturbances.

end of the time of work any considerable quantity of the nitrogenous products of decomposition remained in the body, we might reply that at least an equal quantity of such products must have been in the body at the commencement of the time of work.

We will not, however, insist upon this point; nay, we will even concede to the opponents of our hypothesis that there was an exceptionally large quantity of the peculiar nitrogenous products of muscular action retained in the body. We will also not avail ourselves of the consideration that this curious phenomenon, if it really existed, would only indicate that the products of decomposition resulting from muscular action were not so highly oxidized as ordinarily, and that therefore comparatively little heat was liberated during their formation. We will, as we have said, put aside all these considerations; but we may, without danger of meeting with any opposition, assume that in the six hours following the time of work an amount of the nitrogenous products of decomposition was discharged, at least, as great as the difference between the quantity in the body at the end of the time of work and that which was in it at the commencement of that period; in fact, actual data for this assumption are not altogether wanting. Among the products of the decomposition of protein substances, the one which alone suggests itself as likely to be retained in noticeable quantities in muscle is creatin. Now observation, it must be admitted, goes to prove that a muscle which has been hard worked contains more creatin than one which has been at rest. Thus the quantity of creatin contained in the heart of an ox was found to be 0·0014 (Gregory), and that in other ox-flesh only 0·0006 (Städeler). Let us now suppose that in our case the extensor muscles of the thigh, which really do the essential work in ascending, contained, previously to that exertion, the same quantity of creatin, 0·0006, as ordinary ox-flesh, but after it as much as is found in an ox's heart; then the difference between these two should be added to the amount excreted through the urine during the time of work. Now the weight of the muscles which extend the leg in walking is estimated in a full-grown and powerful man at 2913 grms. (see Weber, *Mechanik der Gehwerkzeuge*, p. 218); and the muscles of the two legs would therefore weigh 5·8 kilogs. According to these data the surplus of creatin exceptionally retained after the work will be 5·8 kilogs. $(0{\cdot}0014-0{\cdot}0006)=4{\cdot}64$ grms.; this indicates 8·4 grms. of albumen.

From the products of decomposition discharged during the six hours following the time of work we have reckoned over 16 grms. of albumen; we may therefore certainly assume that, during those six hours, at least as much of the decomposition products

of protein substances appeared in the urine as the surplus which might possibly have been retained after work, over and above that normally contained in the tissues. This being granted, we have the data for further calculations, in the total amount of nitrogen contained in the work urine and after-work urine (see page 492). This is for Fick 38·28 grms., and for Wislicenus 37·00 grms. of decomposed albumen. To the first of these two figures we may apply a slight correction. For obvious reasons, we were compelled to measure the night urine at our hotel at Interlaken at 10 minutes past 5. But the work really began two hours later, after one hour passed on the steamboat, and another spent over breakfast at Iseltwald; therefore the urine secreted during these two hours ought not properly to have been reckoned with the work urine. In the case of one of us, however (Fick), this error may be in some measure repaired; just before the ascent he had discharged a quantity of urine into the vessel destined to receive the work urine. This quantity could not indeed be measured, because the graduated apparatus had been packed up at Interlaken, but, by the eye, it must have been at least as much as 20 cubic centims. If we assume that this quantity of urine contained the same proportion of nitrogen as the work urine, it would correspond to 1·11 grm. of albumen, which we ought without doubt to deduct from the number 38·28. We thus obtain for the greatest possible quantity of albumen oxidized in Fick's body during the ascent 37·17 grms.

The question now arises, what quantity of heat is generated when 37·17 and 37·00 grms. of albumen are respectively burnt to the products in which their constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question; for neither the heat of combustion of albumen nor of the nitrogenous *residue* of albumen is known. We are able, however, to assign a limit which the quantity of heat in question will not overstep. In fact it is quite certain that the amount of heat which 1 grm. of albumen will give when completely burnt must be less than the amount of heat which would be obtained if the combustible elements contained in a gramme of albumen were burnt separately; that is, in other words, the heat of combustion of albumen is less than that of a mixture of the elements in the same proportions, and not in combination with oxygen. Now the latter number may be easily calculated; it is only requisite to determine how much heat would be produced by the combustion of the amount of carbon and hydrogen contained in a gramme of albumen. The nitrogen may be neglected, since it is known to be set free in the combustion of albumen. Very probably also nitrogen has no heat of combustion at all; that is to say, in the combination of an atom of nitrogen with oxygen there is not so much heat developed as is consumed in the separation of the molecules of oxygen into the atoms of which they are composed. A gramme of albumen contains 0·535 grm. of carbon, which, computed at the highest rate (8080) for the heat of combustion of carbon, gives 4·32 units of heat. For the combustion of the 0·07 grm. of hydrogen contained in 1 grm. of albumen, putting 34,462 for the heat of combustion of this element, there are 2·41 units of heat; altogether for 1 grm. of albumen 6·73 units of heat. The true heat of combustion of a gramme of albumen is therefore in any case considerably less than this number; and still smaller must the amount of heat be which is liberated by the imperfect oxidation of a gramme of albumen as it actually occurs in the human body. Conceding this point to our opponents, and making the almost absurd assumption that 6·73 units of heat are produced by the consumption of 1 grm. of albumen in the human body; then there would arise from the consumption of albumen, in the case of Fick, $37{\cdot}17 \times 6{\cdot}73 = 250$, and in that of Wislicenus $37{\cdot}00 \times 6{\cdot}73 = 249$ units of heat, available for the muscular work done in ascending the mountain. Expressed in units of work, this gives for Fick 106,250, and for Wislicenus 105,825 metre-kilogrammes.

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249
3825
00
50
5825

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

The question we put at the outset is in this manner definitively answered. *The burning of protein substances cannot be the only source of muscular power; for we have here two cases in which men performed more measurable muscular work than the equivalent of the amount of heat, which, taken at a most absurdly high figure, could be calculated to result from the burning of the albumen.* But our proof becomes far stronger when we consider that a much greater amount of heat must have been given forth by the force-producing chemical processes than is equivalent to the measurable external work performed. In the

first place we may add to the data already obtained of 129,096 and 148,656 metre-kilogrammes another item which can be expressed in units of work; and though its value cannot be quite accurately calculated, yet a tolerable approximation can be made. It consists of the force consumed in respiration and the heart's action. The work performed by the heart has been estimated, in a healthy full-grown man, at about 0·64 metre-kilogramme* for each systole. During the ascent, Fick's pulse was about 120 per minute. That gives for the 5·5 hours of the ascent an amount of work which may be estimated at 25,344 metre-kilogrammes, entirely employed in the maintenance of the circulation. No attempt has yet been made to estimate the labour of respiration. One of us has shown, however, in the second edition of his 'Medical Physics' (p. 206), that Donder's well-known investigations concerning the conditions of pressure in the cavity of the thorax give sufficient data for such an estimate. He has there shown that the amount of work performed in an inspiration of 600 cubic centims. may be rated at about 0·63 metre-kilogramme. Fick breathed during the ascent at an average rate of about 25 respirations per minute, which gives, according to this estimation, an amount of respiratory work for the whole ascent of 5197 metre-kilogrammes. If we add this, and the number representing the work of the heart, to the external work performed by Fick, we obtain a total of 159,637 metre-kilogrammes, which is already half as much again as the amount of heat to be obtained by the burning of the albumen. In the case of Wislicenus, the relative amounts are still more striking. If we suppose that his respiratory and circulatory work bore the same proportion to Fick's as his bodily weight did to Fick's, *i. e.* 7 : 6, we obtain for Wislicenus's amount of work, as far as it is possible to calculate it, a total of 184,287 metre-kilogrammes, which exceeds that calculated from the oxidation of the proteine substances by more than three-fourths.

Besides these estimated (and certainly not over-estimated) items, there are several others which cannot be even approximately calculated, but the sum of which, if it could be obtained, would probably exceed even our present large total. We will try to give at least some sort of an account of them. It must first be remembered that in the steepest mountain path there are occasional level portions, or even descents. In traversing such places the muscles of the leg are exerted as they are in ascending, but the whole work performed is transformed back into heat. The same force-producing process, however, must be going on in the

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

muscles as if work were being performed which did not undergo this transformation. In order to make this point yet clearer, we may take into consideration that the whole work of the ascent only existed temporarily as work. On the following day the result was reversed; our bodies approached the centre of the earth by as much as they had receded from it the day before, and, in consequence, on the second day an amount of heat was liberated equal to the amount of work previously performed. The two parts of the action, which in this case were performed on two separate days, take place in walking on level ground in the space of a footstep.

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

There was besides a large portion of our muscular system employed during the ascent, which was performing no external work (not even temporary work, or mechanical effects immediately reversed), but which cannot be employed without the same force-generating processes which render external work possible. As long as we hold the body in an upright position, individual groups of muscles (as, for instance, the muscles of the back, neck, &c.) must be maintained in a state of continual tetanus in order to prevent the body from collapsing. This point seems to have been much misunderstood; and we will therefore discuss it, shortly but fundamentally. The reason of this misunderstanding is, that in many treatises on the subject such employment of the muscles is denominated "statical work," although there is really no *work* when a tetanized muscle holds a burden in equilibrium. It is only at the commencement of this condition, when the burden is raised, that there is any work. We might therefore, in order to correct this misunderstanding, and shortly to designate the condition in question, propose the expression "statical activity." While the tetanized muscle is maintaining a burden in equilibrium, it is certainly in an active state. As long as it supports the burden, the force-generating processes must be active in it; but the whole of the actual energy thereby produced must necessarily be liberated in the form of heat; for no work is really performed. It may assist in comprehending this if we put before the reader a perfectly similar and more simple case. Let us imagine a cylinder standing perpendicularly and closed at the bottom; it is filled with some kind of gas, and the whole is in

equilibrium with the surrounding air. In the cylinder is a piston fitting air-tight, but moveable without friction. The piston has a certain weight, which we will imagine at first to have balanced the elasticity of the gas underneath it: now let us apply to the gas in the cylinder a certain amount of heat so that its temperature rises; then the balance between the elasticity of the gas and the weight of the piston will be destroyed. The latter will rise. Here is a certain amount of external work done, and a certain corresponding portion of the added heat disappears. If we now leave the apparatus alone, the rest of the added heat will gradually be abstracted by the surrounding air, and the piston will sink to its old place. It may be observed, in passing, that during this process the heat which had been transmuted into work again appears as heat. We might now propose to ourselves the task of keeping the piston up in the cylinder. To do this the increased temperature of the gas must be maintained. This can only be done by a continual addition of heat to it; for, according to the conditions of the experiment, it would always be losing heat. Evidently then no more than an exact compensation for the loss is needed to maintain the elevation *ad infinitum*. No more heat will be changed into work, because no more work is done. Suppose, for instance, we produced the warmth necessary to maintain the position of the piston by burning carbon, the whole of the heat produced by its burning would now be liberated, and diffused into the medium surrounding the cylinder. We may conceive of a tetanized muscle (like the heated gas in the cylinder) as holding up a weight which would immediately fall if the supply of actual energy were to cease. It is active, but it performs no work, and therefore all the force produced is liberated in the form of heat.

If we return from this digression to our subject, we shall find that we have another and a last item to add to the total of actual energies which must be provided by force-generating processes in muscles. It is hardly conceivable, on the principles of the mechanical theory of heat, that these processes, even in the case of real muscular work, produce only just the quantity of actual energy needed for the mechanical work in question. It is, on the contrary, tolerably certain that only a part of the actual energy developed by those force-generating processes can be transmuted into mechanical work. This exceedingly probable conclusion, deduced from the most general physical considerations, has been confirmed by experiment. From Heidenhain's* beautiful investigations concerning the connexion of the development of heat with muscular activity, it is possible to estimate, at least ap-

* *Mechanische Leistung und Wärmeentwickelung im Muskel.* Leipzig, 1864.

proximately, the value of the collective amounts of actual energy developed in single experiments; at least it is possible to fix a point below which it will not descend. This lowest possible value is almost always considerably higher than that of the equivalent of actual energy transmuted during the experiment into mechanical work. It may be casually remarked that in almost all Heidenhain's experiments, the mechanical work was changed back again into heat; for in every case the muscle allowed the weight it had raised to fall again, so that no mechanical work was really performed, and therefore the whole actual energy generated could appear only in the form of heat. It appears from Heidenhain's investigations, that the relation of that portion of the actual energy transmuted into work to the total energy produced in the contraction of muscle, varies very much according to the tension of the muscle while working. But we shall not be taking too high a figure if we assume that this proportion can never, under normal circumstances, be greater than as $\frac{1}{2}$ to 1. According to this we must at once double the numbers found above for the total amount of work permanently or temporarily performed, and should thus obtain a number which would give us an approximation to the sum total of actual energy (expressed in units of work) which must have been supplied by the force-generating processes in the muscle, in order that such work might be performed. The number which ought still to be added to these figures in order to represent the work which had been actually performed, and the statical activity of the muscles, would certainly be considerable; but it must be omitted from the calculation, because, as we said above, we have no data from which to compute it. We must therefore abide by the 319,274 metre-kilogrammes for Fick, and 368,574 for Wislicenus.

It may be thought that by making use of Helmholtz's well-known conclusion, we might much more easily have calculated a minimum for the total actual energy provided by the processes for the generation of muscular force during the ascent. By ingeniously combining the results of Smith's experiments on respiration with Dulong's determinations of animal heat, and with the well-established hypothesis concerning capability of work in man, that physiologist inferred that in the human body not more than one-fifth of the heat produced by the oxidation of metamorphosed substances is transmuted into muscular work. It would seem, according to this, that we might obtain the minimum value which we require simply by multiplying the estimated amount of muscular work by five. But this is not the case; for Helmholtz does not separate the processes which generate muscular power from the somewhat different ones which produce heat. He looks upon the whole body as an apparatus working mechanically, and comes to the conclusion that this apparatus can only utilize one-fifth of the whole amount of heat generated in it by oxidation. By multiplying our external work by five, we obtain a minimum of the total amount of actual energy generated by all processes of oxidation during the ascent; but among these processes there might be some, such as the oxidation of the constituents of the circulating blood, which have nothing to do with the production of muscular power.

Let us therefore content ourselves with the figures which we previously obtained, and which will afford sufficient proof for our purpose. We had by their means arrived at the following result:—During our ascent, force-generating processes must have been carried on in our muscles sufficient to afford 751 units of heat in Fick's case, and 820 in Wislicenus's. But the actual amount of albumen oxidized could not, as we calculated, have afforded even a third of this quantity of heat. We therefore repeat, on far more satisfactory grounds, our former conclusion, viz. that the oxidation of albuminous substances cannot be the only source of muscular action. We can now go further, and assert that *the oxidation of albuminous bodies contributes at the utmost a very small quota to the muscular force.* From this assertion it is but a step, which we cannot avoid taking, to the doctrine which has already been frequently proclaimed more or less clearly *, and which has lately been advanced in a most decided manner by Traube, viz. *that the substances by the burning of which force is generated in the muscles, are not the albuminous constituents of those tissues, but non-nitrogenous substances, either fats or hydrates of carbon.*

We might express this doctrine by the following simile:—A bundle of muscle-fibres is a kind of machine consisting of albuminous material, just as a steam-engine is made of steel, iron, brass, &c. Now, as in the steam-engine coal is burnt in order to produce force, so in the muscular machine, fats or hydrates of carbon are burnt for the same purpose. And in the same manner as the constructive material of the steam-engine (iron, &c.) is worn away and oxidized, the constructive material of the muscle is worn away, and this wearing away is the source of the nitrogenous constituents of the urine. This theory explains why, during muscular exertion, the excretion of the nitrogenous constituents of urine is little or not at all increased, while that of carbonic acid is enormously augmented; for in a steam-engine, moderately fired and ready for use, the oxidation of iron, &c.

* For the last three years one of us has in his lectures brought forward this doctrine as an hypothesis, but he was not willing to present it to the public until he could bring undeniable facts to prove it.

would go on tolerably equably, and would not be much increased by the more rapid firing necessary for working, but much more coal would be burnt when it was at work than when it was standing idle. We therefore conclude that since the burning of albumen cannot be the only source of muscular power, it is not in any way concerned in its production; and to this conclusion we are impelled by the consideration that in so delicate an apparatus as a muscle-fibre must be, it is not likely that different kinds of chemical processes should be employed to produce the same effect. Even steam-engines are not indifferent as to the material burnt in them; in one made to burn wood, it would not do to use coal. How, then, is it possible to conceive that the muscle-machine was constructed especially for albumen, and that when enough albumen is not to be had, it puts up contentedly with non-nitrogenous fuel? That it does make use of non-nitrogenous material, we have by our experiment proved beyond a doubt. We therefore conclude that *since the muscle-machine can undoubtedly be heated by means of the non-nitrogenous fuel, this fuel is in all cases that best suited for it.*

In conclusion we may be permitted to return to the more general considerations touched upon in the opening of our paper. By the light of our hypothesis, the great efforts for the digestion of the hydrates of carbon which we meet with in the animal world are perfectly intelligible. We see, for instance, among the ruminants, most complicated apparatus, constructed for the purpose of saccharifying at least a little of the cellulose, hard as it is to dissolve, and of thus gaining something for the animal economy. This becomes immediately comprehensible on the assumption that hydrates of carbon subserve the most important functions of muscular work. These substances do not lose any of their importance as heat-producers in the ordinary sense of the phrase, because in muscular work a great part of the heat produced by oxidation is liberated as such, and because even the heat converted into work is always at last changed back into heat in the body of the animal; for it is only exceptionally that the animal is methodically employed by man for the production of external mechanical work.

There is another consideration connected with the preceding one, which is well calculated to make our conclusion appear *à priori* extremely probable. Among those animals whose muscles have enormous strength, there are several whose nourishment contains very little albumen, but, on the other hand, large quantities of the hydrates of carbon,—for instance, the swift ruminants, the goat, the chamois and gazelle, and many flying insects. Is it conceivable that the great muscular exertions of these creatures are really sustained by the oxidation of albumen?

We will here quote a remarkable fact bearing upon our theory, which was lately communicated to us by Dr. Piccard. The chamois-hunters of western Switzerland are accustomed, when starting on long and fatiguing expeditions, to take with them as provisions nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. What they mean by this expression is, that they have learnt by experience that, in the form of fat and sugar, they can most conveniently carry with them a rich provision of force-producing oxidizable matter. With regard to this point, we can assert, from our own experience in the ascent of the Faulhorn, that in spite of the amount of work and the abstinence for thirty-one hours from albuminous food, we neither of us felt in the least exhausted. This could hardly have been the case if our muscular force had not been sustained by the non-nitrogenous food of which we partook.

LXXI. *On* Mr. Cooke's *Observations of the Solar Spectrum.* *By* Balfour Stewart, *M.A., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In Silliman's Journal for March 1866, there appears a communication from Mr. Josiah Cooke, Jun., Cambridge, U.S., "On the Aqueous Lines of the Solar Spectrum," which communication has been republished in the Philosophical Magazine of last month.

The author, who has kindly forwarded me a copy of his paper, alluded to his having visited Kew Observatory in the summer of 1864, when he was surprised to find my spectroscope less powerful than the one he was then using. He considers that "the facts stated in his paper fully account for the discrepancies in the representations which different observers have given of the D lines," and that "the moist climate of England is the evident explanation of the additional lines."

Mr. Balfour Stewart, the Superintendent of Kew Observatory, has forwarded me the following letter. You will observe that the observations of Mr. Stewart and of the other Kew observers are somewhat at variance with those of Mr. Cooke.

Believe me,
Yours truly,
J. P. Gassiot.

Clapham Common, May 8, 1866.

Kew Observatory, May 7, 1866.

My dear Sir,—It has occurred to me that perhaps you might like to know how far the observations made with your spectroscope at Kew corroborate the remarks of Mr. J. P. Cooke as to the influence of aqueous vapour on that small but interesting region of the spectrum embraced by the two D lines.

The observations made at Kew, in which your most powerful arrangement of prisms (the sulphuret-of-carbon set) was used, may be considered comparable with any of the observations of Mr. Cooke. There may possibly be a small preponderance of power of the one instrument over the other, although I do not know which possesses it; but evidently, from the sketches given, these two arrangements are of the same order as regards power. The sulphuret-of-carbon prisms were in operation at Kew for about four months, until it was found that a glass train of inferior power was really more useful for your main object of obtaining an accurately-measured map of the solar spectrum; and during these four months the region between the two D lines was repeatedly observed, especially by Mr. Loewy.

These observations were made under varying atmospheric conditions, but in all these the same number of fine lines between the two broad D lines was invariably observed. The appearance presented by this region was that you have sketched in a paper communicated to the Royal Society on March 17, 1864, and it differs very little from fig. 4. given by Mr. Cooke in the Philosophical Magazine for May 1866, as representing an observation of this region made by him when the weight of vapour in one cubic foot of air was 6·57 grains.

When, however, we analyze the state of the atmosphere at Kew at the time (from midday to 3 P.M. of March 12, 1864) when the observation recorded and sketched by you was made, we find that the weight of vapour in one cubic foot of air was only 1·98 grain. In as far as we can judge of the vaporous condition of the whole atmosphere by a hygrometrical observation, the vapour of the atmosphere was much less at Kew at the time of this sketch than it was at the time when Mr. Cooke observed the similar appearance which he has represented in fig. 4; indeed the air at Kew at this moment probably contained less vapour than at the moment when he observed the appearance given in fig. 2, where there are only two lines between the lines D. But your sketch exhibits thirteen intervening lines.

Our Kew experience therefore does not quite accord with Mr. Cooke's results; for we have obtained—

(1) In all our observations, extending over the period of perhaps one month, the same number of lines between the two D lines.

(2) One of these observations was made at a time when there was probably little vapour in the atmosphere, and yet the spectrum obtained at Kew is similar to that obtained by Mr. Cooke at a time when there was much vapour.

Our experience is therefore against a decrease of vapour producing a decrease of lines in this region, but not perhaps against a decrease of vapour producing a diminution in the intensity of the group of lines near the most refrangible D line. This is the group that Mr. Cooke has found to vanish altogether when the amount of vapour is small. We have never found them to vanish, but we have observed a change of intensity, which may perhaps be due to vapour.

I shall only make one further remark. An observation of the spectrum of ignited sodium (made, I think, in the presence of Mr. Huggins) discovered the existence of at least two sodium lines between the two D lines. In this region, therefore, there are at least four lines due to sodium; and it might be natural to suppose that the absorption lines due to the presence of sodium vapour in the sun's atmosphere should be at least four in number for the same region. Besides these there is probably a nickel line; so that on these grounds I should hesitate in believing that Mr. Cooke's fig. 1, in which there are only three lines in all, namely the two D lines and one intervening line, is the ultimate representation of this region by a very powerful instrument.

I remain,

Yours very truly,

J. P. Gassiot, Esq., V.P.R.S. B. STEWART.

LXXII. *On the Action of Carbonic Oxide on Sodium-ethyle.* By J. ALFRED WANKLYN, *F.R.S.E., Professor of Chemistry at the London Institution**.

NEITHER carbonic acid nor carbonic oxide acts upon zinc-ethyle; but both of these gases attack sodium-ethyle. In the case of carbonic acid, the product of the reaction upon sodium-ethyle is, as I showed some years ago, propionate of soda. This reaction is very energetic, evolving much heat, and taking place without the application of external heat when the gas is simply passed over the sodium-compound.

The action of carbonic oxide is much less energetic. When the compound containing sodium-ethyle and zinc-ethyle, which I have described on a former occasion†, is sealed up with carbonic oxide, there is no perceptible change at first, but after a time a black deposit makes its appearance. If the apparatus be kept at ordinary temperatures, a considerable time is required for the production of this deposit; but if it be heated to temperatures approaching 100° C., then the blackening takes place immediately.

The black deposit is not carbonaceous, for it dissolves in hy-

* Communicated by the Author.

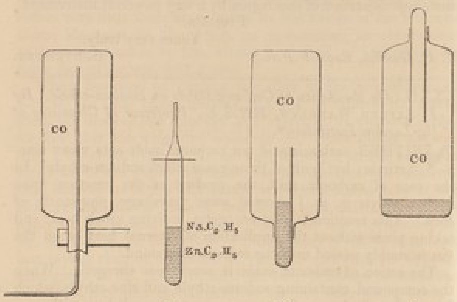
† *Ann. der Chem. und Pharm.* (1858), vol. cviii. p. 67.

drochloric acid. It is metallic, consisting of metallic zinc, and most probably also of metallic sodium. Absorption of the gas and formation of a fragrant oil accompany this deposit of metal.

The following is a description of the method adopted for the preparation of this oil in sufficient quantity to admit of an examination of it.

The carbonic oxide was prepared by Fownes's process, viz. by heating ferrocyanide of potassium with excess of sulphuric acid. It was collected in a large gas-holder, and after being passed through potash-lye and sulphuric acid, filled into Winchester quart bottles (stoppered bottles holding from 2½ to 3 litres) by displacement (see fig. 1).

The sodium-ethyle was prepared in small tubes, each tube being charged with about 12 grms. of zinc-ethyle and 1 grm. of sodium, which was made to act very completely upon the zinc-ethyle by being shaken and gently warmed with it. The sodium-ethyle having been fully formed, the tube containing it had its top cut off (see fig. 2), and was then introduced into the bottle filled with carbonic oxide (see fig. 3). The apparatus was then reversed,



1 2 3 4

and the sodium-ethyle shaken out into the bottle containing the carbonic oxide. Lastly the bottle containing the carbonic oxide and the sodium-ethyle was stoppered and placed in warm water, and repeatedly shaken. After a short time the contents of the bottle became very black, and the digestion in warm water was stopped. The bottle was allowed to cool, then a little mercury and water poured in, and shaken up well. The aqueous solution was then introduced into a retort and distilled. Along

with the first few drops of the aqueous distillate an oil distilled over.

The amount of oil yielded by a Winchester quart of carbonic oxide and a charge of sodium-ethyle from 12 grms. of zinc-ethyle and 1 grm. of sodium was about 1 grm. The residue in the retort after the oil had been distilled off was examined and found to be very alkaline.

The product from a good many Winchester quarts of carbonic oxide was employed in the following experiments.

The oil, after being dried, was rectified and found to consist essentially of two portions—one boiling at 100° to 110° C., and the other at 150°, or a little higher. The portion with the lower boiling-point was freed from the rest by careful fractionation. Combustion of it gave these results:—

I. ·2076 grm. gave ·5218 CO^2 and ·2300 grm. H^2O.
II. ·1029 grm. gave ·2597 CO^2 and ·1155 grm. H^2O.

	I.	II.
Carbon	68·55	68·83
Hydrogen . . .	12·31	12·47
Oxygen	19·14	18·70
	100·00	100·00

Deducing the formula by the well-known method, the following results are arrived at:—

C . . . 68·55÷12= 5·71 ÷1·196= 4·79
H . . . 12·31÷ 1 =12·31 ÷1·196=10·30
O . . . 19·14÷16= 1·196÷1·196= 1·00

Therefore I. gives C=4·79; H=10·30; O=1·00.

C . . . 68·83÷12= 5·736÷1·15= 4·988
H . . . 12·47÷ 1 =12·47 ÷1·15=10·84
O . . . 18·70÷16= 1·150÷1·15= 1·00

Therefore II. gives C=4·988; H=10·84; O=1·00.

From which it follows that the formula is $C^5H^{10}O$, which requires

C^5	60	69·77
H^{10} . . .	10	11·63
O	16	18·60
	86	100·00

It will be observed that both analyses point decisively to C^5, but that, whilst I. requires H^{10}, II. gives a result approximating more nearly to H^{11} than H^{10}. The fact that the estimations of hydrogen are usually in excess, and that H^{11} would be in opposition to the law of saturation, fully justifies the selection of the formula $C^5H^{10}O$. There is, moreover, as will presently appear, a special reason why the hydrogen should be in excess.

The resulting oil has therefore the composition of a compound containing two atoms of ethyle and one atom of carbonic oxide,

$$C^5 H^{10} O = CO + (C^2 H^5)^2 ;$$

and all the facts observed are in harmony with the account of the reaction given by this equation,

$$CO + 2 NaC^2 H^5 = Na^2 + CO (C^2 H^5)^2 .$$

There is absorption of gas and precipitation of metal. Obviously sodium liberated in presence of zinc-ethyle will act upon the zinc-ethyle, precipitating the zinc.

The oil having the composition $CO (C^2 H^5)^2$ appears to be identical with propione. It has the proper kind of smell, about the right boiling-point, and does not combine with bisulphite of soda, in this respect agreeing with the propione of Morley prepared from propionate of baryta, and the propione of Freund obtained by the action of zinc-ethyle on chloride of propionyle. Further reasons for concluding that it is identical with propione will be given directly.

The ketones are able to take up nascent hydrogen, giving secondary alcohols; it is therefore to be expected that the propione should have been partially converted into amylene-hydrate during the treatment of the mass of reduced metal and propione with mercury and water. This will fully account for the analyses exhibiting the hydrogen rather in excess over the formula required for propione. Further support is furnished by a peculiarity observed in the distillation of the substance. It commenced to distil with great precision at 99° and 100°, and at 110° the retort was quite dry; but the boiling-point had no tendency to remain constant at any point intermediate between these two extremes.

Now propione boils at 101° C., and amylene-hydrate at 108° C. No attempt was made to effect a separation by fractional distillation.

With the view of further ascertaining the nature of the compound $CO (C^2 H^5)^2$, it was submitted to the oxidizing action of a mixture of bichromate of potash and dilute sulphuric acid. For this purpose a quantity of the product was prepared, and rectified ten times. It was also heated in a stream of dry carbonic acid gas, so as to expel all traces of ether, and then sealed up with excess of bichromate of potash and dilute sulphuric acid, and heated in the water-bath for many hours. The action takes place very slowly; and indeed one attempt to effect the oxidation in a retort utterly failed.

After the oil had disappeared, the digestion-tube was opened; there was no escape of gas, proving the non-formation of carbonic acid during the oxidation. The contents of the digestion-

tube had the well-known smell of the lower members of the fatty acid series, and after being diluted with water were distilled, and gave an acid distillate. This distillate was redistilled, and the distillate converted into a baryta-salt. The baryta-salt was carefully tested for formiates, and contained none.

It was dried at 110° C. and analyzed.

I. ·3922 grm. of baryta-salt was ignited, and evolved abundance of organic matter, leaving ·2804 grm. carbonate of baryta. Therefore Ba per cent. =49·77.

II. ·6266 grm. was precipitated with dilute sulphuric acid; and the resulting sulphate of baryta weighed ·5295 grm. Therefore Ba per cent. =49·74.

These numbers are intermediate between those required by propionate and acetate of baryta—a result which clearly shows that the liquid is broken up on oxidation, and, since neither carbonic acid nor formic acid is produced, indicates pretty clearly that the results of the oxidation are propionic and acetic acids.

Confirmation of this fact was obtained by applying Liebig's method of fractional saturation to the mixed acid. Some of the acid was partially saturated with carbonate of soda and distilled, and the distillate made into a baryta-salt.

·2066 grm. of this baryta-salt gave ·1728 grm. of sulphate of baryta. ∴ Ba per cent. =49·24.

Propionate of baryta contains 48·48 per. cent. of barium. If higher acids than propionic had been there, an utterly different result would have been obtained.

The residue in the retort was then supersaturated with sulphuric acid and distilled, when it gave a distillate which presented all the characters of acetic acid.

It is therefore established that the oxidation-products are propionic and acetic acids, and that neither carbonic acid nor formic acid is given.

Bearing in mind the origin of the compound, this is a very interesting result. Formed by the union of carbonic oxide with ethyle, it gives the characteristic oxidation-product of the propyle series, and the characteristic oxidation-product of the ethyle series,

$$C \begin{cases} O \\ C^2 H^5 + O^3 = C^3 H^6 O^2 + C^2 H^4 O^2 . \\ C^2 H^5 \end{cases}$$

According to theory, normal propione should fall into the groups C^3 and C^2 when oxidized; this reaction therefore indicates that the product obtained from carbonic oxide and sodium-ethyle is identical with normal propione.

In conclusion, I cannot refrain from referring to Kolbe's speculations on the nature of the ketones. Many years ago he described these compounds as consisting of carbonic oxide in union with the alcohol-radicals. In the reaction which forms the subject of this paper, carbonic oxide is seen to unite with the alcohol-radicals and to form a genuine ketone: a more remarkable confirmation of the theory is hardly conceivable.

LXXIII. *On Aqueous Vapour and Terrestrial Radiation.* *By* M. G. Neumayer.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I TRANSMIT to you a communication from M. Neumayer, which I trust will have the effect of stimulating other meteorologists to enter upon and pursue the line of inquiry which he has so ably and zealously begun. There is, in my estimation, no result of physical science more certain than that the aqueous vapour of our air exerts a powerful influence upon the radiation from our earth; and there is none, I imagine, more likely to guide the really scientific meteorologist to results of permanent value.

John Tyndall.

To Professor Tyndall, F.R.S., &c.

Frankenthal, Pfalz a. Rhein,
May 4, 1866.

My dear Sir,

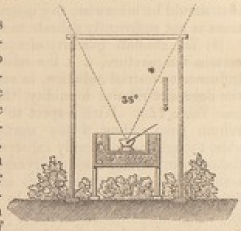
You will perhaps remember that some two years ago I sent you some original observations made at the Flagstaff Observatory, Melbourne, bearing on the absorption of heat by aqueous vapour, and its relation to terrestrial radiation. At the time when I did myself the pleasure to call upon you at London, you stated that great pressure of other matters did not admit of your undertaking the task of discussing those observations; and in consequence of this I had the extensive labour of classifying and condensing so large a number of figures done in my bureau of computation, established with the object of publishing discussions on the magnetic and meteorological observations made by me during the years 1858–63. In the annexed Tables, four in number, you receive the results of my labours in this direction, which I feel great pleasure in putting before you.

In order that you may be able to form an opinion as to the manner in which the observations were made, I subjoin the following remarks.

(1) The observations were made at all times of the day and the night whenever the sky was clear, *i. e.* the zenith perfectly free from clouds.

(2) The tension of aqueous vapour and the degree of relative humidity were observed by August's psychrometer, continually checked, however, by Döbereiner-Regnault's aspirator hygrometer. The reductions were effected by Regnault's Tables, the thermometers being Kew standards.

(3) The radiation-instrument consisted of a spirit minimum-thermometer of Kew, the bulb of which was placed and carefully adjusted in the focus of a parabolic reflector nicely polished and silvered, 6·4 inches wide and 2·4 inches deep*. This reflector was put in a box filled with cotton and placed in a little house, keeping out the rays of the sun, but in such a manner that the zenith over the instrument remained perfectly free for a space of some 38°. The focus of the reflector was about 1½ feet above the surrounding ground (see introduction to 'Results,' page 4.)



(4) The psychrometer was placed close to this radiation-apparatus; and in order to have a control, a thermometer was placed within the little house (see annexed figure), the same being frequently compared with the Kew standard.

(5) Table I. contains in its first column the number of single simultaneous observations, the second column shows the mean pressure of aqueous vapour, the next the mea ntemperature of the air, and the last the mean difference between temperature of air and radiation, obtained by the instruments described. The results have been arranged according to the value of the aqueous tension.

(6) Table II. shows the relation between the degree of relative humidity of air and the radiation-difference. For every single set of observations the relative humidity was computed.

(7) It remains yet to be mentioned that the elaborate task of classifying and reducing the 4376 single sets of observations has been twice executed, both computations being quite independent of each other.

(8) With a view to show that the decrease in radiation with an increase in humidity, as evident from Table II., is not due to the decrease in temperature, as may be suspected when glancing over Table II., two more Tables were constructed illustrative of the influence the temperature of the air alone exerts on the radiation-instrument (Tables III. and IV.). For, although fully aware of the principles generally adopted with regard to the influence of the temperature of the air on radiation, and also of the results of your investigations (§ XII. p. 406), to the

* In strict conformity with the instructions of the Royal Society.

effect that the temperature of the surrounding air does not sensibly influence the difference of temperature between the radiating body and the temperature of the air, I thought one might be inclined to suspect undue radiation influencing the reading of the parabolic thermometer, and I took for this reason the trouble of classifying the observations merely according to temperature of the air.

The results at which I arrive may be summed up in the following lines:—

(*a*) The absolute quantity of aqueous vapour in the air is in itself alone not sufficient as a criterion for the degree of radiation; or if it should be otherwise, the instrument used for the observations is not sufficiently delicate to show the degree of influence.

(*b*) The absolute quantity of aqueous vapour, together with a certain temperature, *i. e.* the relative humidity of air, greatly influence terrestrial radiation, in such a manner that, the greater the degree of relative humidity, the less radiation is noticeable (see Table II.). With respect to the latter remark, I cannot refrain from adding a few words. The law is even noticeable, though less distinctly expressed, in the lower per cents. of humidity in extreme cases of dryness—although in all these cases a thin veil of cirrostratus clouds covers the sky, so delicate as frequently not to be perceptible by the eye. It is further to be remarked that, during hot winds, when the air is extremely dry, quantities of dust are floating in the air, seriously interfering with radiation, although the sky is apparently quite clear. All these cases have been included in deriving the above results; and considering all, we must be surprised that, notwithstanding all this, an increase of radiation of between 15 and 30 per cent is perceptible.*

It is scarcely necessary for me to dwell on the apparent connexion between the increase of temperature of the air and the amount of radiation shown by Table II., such being fully accounted for by the relation between temperature and relative humidity. Tables III. and IV. sufficiently prove that the radiation-instrument was not influenced by the surrounding air in any undue manner so as to interfere with the recognition of the general law.

I hastened to put these facts before you, in order you might be able to convince yourself of the reliability of the observations and the method of discussion; and further, because I was convinced you would find it important to receive proofs that by the aid of meteorological instruments generally in use we are able to recognize a function of aqueous vapour in our atmosphere which

* I have shown that the smoke of the London air is feeble in comparison with its aqueous vapour.—J. T.

hitherto had only been demonstrated by your own important experiments (as far as my knowledge on this point goes).

You would oblige me exceedingly by giving me your ideas on the various points raised in this letter. According to your opinion, I purpose either turning the results to account by publishing them in my next publication, or hand them over to you with no other condition but that I shall be supplied with whatever you shall write upon this special subject.

Requesting you kindly to excuse the liberty I have taken in thus troubling you, I remain, my dear Sir,

Yours very truly,

G. Neumayer.

Table I.

No. of obs.	Mean			No. of obs.	Mean			No. of obs.	Mean		
	Pressure of vapour.	Temp. of air.	Temp. of air minus temp. radiation.		Pressure of vapour.	Temp. of air.	Temp. of air minus temp. radiation.		Pressure of vapour.	Temp. of air.	Temp. of air minus temp. radiation.
	inch.				inch.				inch.		
4	0·1712	32°·1	1°·35	29	0·2703	55°·1	3°·56	45	0·3483	64°·8	4°·03
43	·1929	37·7	2·73	44	·2704	50·1	3·82	44	·3485	56·3	3·37
10	·1956	46·9	3·38	44	·2869	54·7	3·74	48	·3493	65·0	3·17
42	·2100	39·9	2·62	41	·2878	56·4	3·73	45	·3497	66·9	3·93
34	·2114	45·3	2·88	43	·2879	54·0	4·31	49	·3677	58·7	3·24
43	·2120	46·4	2·88	41	·2884	56·8	4·00	45	·3680	70·2	3·55
44	·2133	49·5	3·52	43	·2886	52·7	3·39	44	·3687	64·5	3·72
45	·2293	43·2	2·98	43	·2888	52·5	3·85	48	·3687	63·4	3·23
40	·2302	43·6	3·34	45	·2892	59·6	3·56	24	·3692	63·0	3·69
40	·2304	46·2	3·10	40	·2894	57·2	4·00	44	·3692	64·4	4·34
43	·2307	45·4	3·23	45	·2899	50·7	3·13	43	·3701	60·5	3·52
43	·2307	51·3	4·60	40	·2900	56·5	3·91	44	·3876	66·3	4·50
44	·2313	52·9	2·69	49	·2915	53·3	3·97	49	·3888	61·8	3·60
29	·2322	52·6	3·51	45	·2940	61·9	3·70	43	·3894	63·2	3·76
44	·2484	47·3	3·37	44	·3076	53·0	3·21	45	·3900	69·2	3·77
44	·2493	46·5	2·81	40	·3080	61·1	4·58	19	·3909	59·0	3·32
43	·2494	47·1	3·32	40	·3088	57·5	3·38	44	·4081	65·9	3·84
41	·2496	52·6	3·41	43	·3091	57·6	3·95	49	·4087	63·5	3·60
44	·2497	48·2	2·32	43	·3093	58·1	4·10	43	·4092	62·3	4·00
43	·2497	49·4	3·60	44	·3095	54·2	3·83	3	·4100	64·4	3·07
43	·2502	54·9	3·60	24	·3095	55·9	4·18	48	·4280	68·1	4·24
45	·2504	46·3	4·52	44	·3096	55·0	3·95	9	·4290	64·0	3·29
40	·2510	59·8	4·12	45	·3097	64·4	3·47	49	·4295	67·1	4·13
40	·2512	48·2	3·09	44	·3111	57·7	3·82	32	·4360	67·5	3·56
6	·2533	54·9	3·10	4	·3232	55·1	3·30	35	·4476	66·7	3·77
41	·2523	52·1	3·53	44	·3280	57·7	4·03	48	·4497	69·2	4·25
43	·2683	48·5	3·51	48	·3286	55·5	3·82	48	·4692	70·6	3·66
45	·2687	52·7	3·71	43	·3287	59·1	3·80	26	·4698	71·7	3·84
44	·2687	50·2	3·60	41	·3290	59·4	3·90	3	·4886	68·7	2·86
40	·2688	56·3	4·05	44	·3291	55·2	3·90	48	·4893	73·6	3·04
44	·2689	50·2	3·58	45	·3294	59·3	3·64	48	·5096	73·1	3·47
41	·2691	56·4	3·74	40	·3298	66·7	4·33	27	·5284	76·8	3·58
44	·2692	47·7	3·58	40	·3320	68·8	3·68	12	·5460	77·0	4·55
43	·2694	50·3	3·98	43	·3468	62·1	3·85	12	·5715	79·4	3·50
43	·2695	51·8	2·55	49	·3472	56·3	3·48	19	·5908	77·9	3·72
40	·2696	61·8	4·10	44	·3478	60·0	4·23	13	·6061	72·5	2·58
43	·2699	53·2	3·54	34	·3481	61·0	4·41	4	·6292	78·3	2·23

TABLE II.

Groups.	Relative humidity.	Number of observations.	Temperature of air.	Temperature of air minus radiation.
	per cent.			
1	22·417	190	81°·83	4°·79
2	40·220	406	71·43	4·69
3	47·479	244	68·03	4·43
4	52·455	286	66·46	4·37
5	57·521	322	61·99	3·92
6	62·580	301	59·70	3·70
7	67·441	322	58·01	3·71
8	72·485	357	55·12	3·52
9	77·427	438	52·67	3·48
10	82·598	458	51·43	3·36
11	87·397	426	49·63	3·21
12	92·348	403	46·26	3·11
13	96·643	223	43·33	2·55

TABLE III.

Number of		Mean pressure of vapour.	Mean temperature of air.	Temperature of air minus radiation.
Observations.	Groups.			
		inch.		
220	7	0·2009	42°·54	2°·77
284	7	·2307	47·89	3·35
259	6	·2493	48·52	3·14
215	6	·2514	52·70	3·57
257	6	·2687	52·38	3·70
213	7	·2698	53·30	2·59
385	9	·2885	54·95	3·74
258	6	·3000	57·22	3·79
243	6	·3000	57·53	3·91
268	7	·3253	57·10	3·64
295	7	·3401	62·03	3·95
276	6	·3552	63·65	3·55
296	7	·3746	63·40	3·80
384	11	·4117	64·93	3·69
343	13	·5229	73·5	3·46

In this Table are represented the means of the groups, irrespective of the number of single observations.

TABLE IV.

Number of observations.	Temperature of air.	Pressure of vapour.	Temperature of air minus radiation.
251	41°·46	0·2108	2°·96
747	48·30	0·2502	3·39
881	53·70	0·2799	3·58
919	58·01	0·3163	3·73
740	63·40	0·3628	3·76
522	68·15	0·4081	3·86
135	72·72	0·5187	3·23
74	77·88	0·5732	3·52

N.B. I need scarcely mention that the values arrived at only possess a relative worth with respect to the instruments used. An apparatus measuring terrestrial radiation absolutely and in a manner *which renders it practical*, is, I think, yet to be contrived.

LXXIV. *On the Spectra of some of the Fixed Stars.* By WILLIAM HUGGINS, *F.R.A.S., and* W. A. MILLER, *M.D., LL.D., Treas. and V.P.R.S., Professor of Chemistry, King's College, London.*

[Concluded from p. 425.]

§ V. *General Observations.*

20. ON *the Colours of the Stars.*—From the earliest ages it has been remarked that certain of the stars, instead of appearing to be white, shine with special tints; and in countries where the atmosphere is less humid and hazy than our own, this contrast in the colour of the light of the stars is said to be much more striking. Various explanations of the contrast of colours, by Sestini and others, founded chiefly on the difference of the wave-lengths corresponding to the different colours, have been attempted, but as yet without success. Probably in the constitution of the stars as revealed by spectrum analysis, we shall find the origin of the differences in the colour of stellar light*.

Since spectrum analysis shows that certain of the laws of terrestrial physics prevail in the sun and stars, there can be little doubt that the immediate source of solar and stellar light must be solid or liquid matter maintained in an intensely incandescent state, the result of an exceedingly high temperature. For it is from such a source alone that we can produce light even in a feeble degree comparable with that of the sun.

The light from incandescent solid and liquid bodies affords an

* In connexion with this subject we quote the following passage from Smith's *Speculum Hartwellianum*, 4to, 1860, p. 315:—"Sir David Brewster observes that there can be no doubt that in the spectrum of every coloured star certain rays are wanting which exist in the solar spectrum; but we have no reason to believe that these defective rays are absorbed by any atmosphere through which they pass. And in recording the only observation perhaps yet made to analyze the light of the coloured stars, he says, 'In the orange-coloured star of the double star ζ Herculis, I have observed that there are several defective bands. By applying a fine rock-salt prism, with the largest possible refracting angle, to this orange star, as seen in Sir James South's great achromatic refractor, its spectrum had the annexed appearanc [in the Campden Hill Journal], clearly showing that there was one defective band in the red space, and two or more in the blue space. Hence the colour of the star was orange, because there was a greater defect of blue than of red rays.' "

unbroken spectrum containing rays of light of every refrangibility within the portion of the spectrum which is visible. As this condition of the light is connected with the state of solidity or liquidity, and not with the *chemical* nature of the body, it is highly probable that the light when first emitted from the photosphere, or light-giving surface of the sun and of the stars, would be in all cases identical.

The source of the difference of colour, therefore, is to be sought in the difference of the constituents of the investing atmospheres*. The atmosphere of each star must vary in nature as the constituents of the star vary; and observation has shown that the stars do differ from the sun and from each other in respect of the elements of which they consist. The light of each star therefore will be diminished by the loss of those rays which correspond in refrangibility to the bright lines which the constituents of each atmosphere would, in the incandescent state, be capable of emitting. In proportion as these dark lines preponderate in particular parts of the spectrum, so will the colours in which they occur be weaker; and consequently the colours of other refrangibilities will predominate.

Of this the spectrum of α Orionis affords a good example. The green and blue parts of the spectrum are comparatively dark, from the numerous and close groups of dark lines. In the orange they are less strong. Hence it might be anticipated that the light of the star would be characterized by "an orange tinge," as noted by Smyth. β Pegasi is described by Smyth as "deep yellow;" and the appearance exhibited by its spectrum, which closely resembles that of α Orionis, though much fainter, supports the same view.

Aldebaran is recorded by Smyth as of a "pale rose tint." In the spectrum of this star, with the exception of the hydrogen line C, there are but few strong lines in the red, whilst the orange portion is considerably subdued by dark lines, which are less numerous in the green and blue. Sirius, on the contrary, is "brilliant white" (Smyth); and the continuous brightness of the spectrum, with the exception of five strong lines, is, as compared with Aldebaran and α Orionis, unaffected by the dark lines which cross it. The spectrum is indeed crowded with numerous fine lines; but the intensity of these lines is extremely feeble as

* The presence in the atmospheres of Aldebaran and α Orionis of metals, such as iron, which require an exceedingly high temperature to convert them into vapour, renders untenable the supposition, which might otherwise have been entertained, that the orange and red tints of the light of these stars might be due to an inferior degree of incandescence of the photosphere as compared with the temperature of the stars the light of which is white.

contrasted with those of the stars just mentioned. It may be that the length of the stellar atmosphere through which the light passes is less, relatively to the intensity of radiation from the photosphere, and so is insufficient to produce lines of the same degree of blackness as would be produced if the atmosphere were denser. The great intensity, however, of the light of Sirius would rather lead to the conclusion that the atmosphere of vapours is itself highly incandescent. If so, might it not to some extent replace with its own light the light which it has absorbed from the photosphere behind it? It matters little, however, for the present purpose, whether or not either of these suppositions be adopted. There is at all events a most striking difference between the effect on the colour of the star of the closely grouped and very dark lines in the green and blue portions of the spectrum of α Orionis and of the corresponding portion of the spectrum of Sirius, in which the dark lines are faint and wholly unequal to produce any noticeable subduing of the blue and green rays.

We have not yet had an opportunity of testing by experiment whether this hypothesis of the origin of the colours of the light of the stars is also applicable to the remarkable exceptional class of stars the light of which is of a decided green, blue, or violet colour. Such stars are usually very small, and they are always so closely approximated to other more brilliant stars, that it is scarcely possible, with the apparatus which we employ, to obtain separate images of the two spectra: and even were such separation easily practicable, the light of the strongly-coloured star is usually so feeble that its satisfactory prismatic analysis would be a matter of great difficulty.

[One of the objects proposed in the construction of the spectrum apparatus with which the additional observations on Jupiter, Saturn, and Mars were made, and which has been described (p. 415) in connexion with those observations, was to make it available for the prismatic observation of some double and multiple stars.

Before commencing the observation of the spectra of the components of a double star, it is necessary that the position-angle of the stars should be approximatively known. The spectrum apparatus has then to be rotated upon the end of the telescope until the direction of the slit becomes perpendicular to a line joining the stars. When the instrument is in this position, the images of the stars are elongated by the cylindrical lens into two short lines of light parallel with the slit, and separated from each other by a small interval. If the telescope be now moved in a direction at right angles to that of the slit, either of the elongated stellar images can, at pleasure, be made to fall upon the slit and form its spectrum in the instrument. By adopting this method of observation, the spectra of the components of β Cygni were separately examined. These spectra, especially that of B, are so faint that the lines are seen with difficulty, and scarcely admit of being measured. Since, however, on account of the strongly-contrasted colours of these stars, considerable interest attaches to a comparative examination of their spectra, we have represented in fig. 4, Plate V., the appearances which these spectra present to the eye, though we have not yet measured the lines and bands in them. These figures must be regarded as eye estimations only of the general features of the two spectra. The spectra contain, doubtless, many other lines; and the positions of the lines inserted in the drawings, with the exception of *b* and D, were not measured, but only roughly estimated. The distinctive characteristics of these spectra are in accordance with the theory of the origin of the colours of the stars proposed in the foregoing paragraphs. In the case of both stars, the portions of the spectrum which correspond to the colours which are deficient in the light of the star, are those which are most strongly shaded with bands of absorption. Thus in the spectrum of A, the light of which is yellow tinted with orange, the absorption is greatest in the violet and blue; for the strong lines in the orange and red, since they are narrow, would diminish in a much smaller degree the light of these refrangibilities. The yellow and part of the green are free from *strong* lines.

The light of the star B appears to us to be blue, though in some states of the atmosphere the star becomes greenish blue, green, and even greenish white. These changes are probably due to the comparatively greater absorptive action of the vapours in the air upon the more refrangible portions of the spectrum; in proportion to which absorption the other parts of the spectrum become relatively exalted, and thus predominate more or less in the eye.

This inequality of the absorptive action of the vapours of the atmosphere upon different parts of the spectrum becomes very evident if the eyepiece of the telescope be put out of focus (outside of the focus) so as to bring the blue and red rays to a focus in the centre of an expanded image of the star. In the case of B of β Cygni, the centre appears purple, surrounded with a margin of green. In proportion to the changes in the atmosphere, by the passage of masses of vapour or thin cloud, will be the variations of these colours. The green becomes greener; but the blue and the violet are affected in a much greater degree, at times fading almost completely; then the colours resume their former tints and brightness. Several such changes may sometimes occur during one observation.

The spectrum B, observed under conditions of atmosphere in

which the colour of the star was blue, was remarkable for the faintness of the orange and yellow portions, compared with the rest of the spectrum. The diminished brightness of these parts appears to be produced by several groups of closely set fine lines, while towards the more refrangible limit of the spectrum a few strong lines separated by considerable intervals are seen.

The observation of this star, on account of the faintness of its spectrum, is so difficult and fatiguing to the eye, that we have not been able to examine it more accurately or in greater detail.

We have by the same method of observation examined the spectra of the components of α Herculis. The spectrum of A is remarkable for the great strength of the groups of lines in the green, blue, and violet; fainter bands are visible in the yellow and orange, also two strong bands in the red. This arrangement of the bands of absorption agrees with the orange colour which strongly predominates in the light of this star.

B is bluish green in colour. The more refrangible portions of its spectrum are very bright in consequence of the absence of any strong bands. The yellow and the orange parts are crossed by several groups of lines.—August 31, 1864.]

The suggestive fact that stars of these more highly refrangible colours are always observed in close contiguity with much brighter stars, generally of an orange or red tint, would afford countenance to the supposition that these exceptional colours are due to some special physical conditions essentially connected with the stellar systems of which they seem to form a part.

Arago* remarks, "Among the sixty or eighty thousand *isolated* stars, the positions of which are to be found in the catalogues of astronomers, there are none, I think, inscribed with any other indications in regard to colour, than white, red, and yellow. The physical conditions which determine the emission of blue and green light appear, then, to exist only in *multiple* stars."

These stars are without exception feeble in the intensity of their light. The explanation is not admissible, that the faint blue or violet light is due to a less intense incandescence of the radiating surface, since it is precisely these more refrangible rays which would be the first to fail as the temperature diminished, and upon this supposition the star should be dull red. It is of course to be supposed that in the process of gradual cooling some bodies which are less volatile than others would cease to exist in the atmosphere at an earlier period than others, or that they might enter into new combinations more readily than others, and so modify the tint of the light emitted.

The existence around these blue stars of an extended atmo-

* Popular Astronomy, translated by Smith and Grant, vol. i. p. 295.

sphere of "fog" will not explain the absorption of the *less* refrangible portion of the luminous spectrum.

21. These spectrum observations are not without interest also when viewed in connexion with the *nebular hypothesis* of the cosmical origin of the solar system and fixed stars. For if it be supposed that all the countless suns which are distributed through space, or at least those of them which are bright to us, were once existing in the condition of nebulous matter, it is obvious that, though certain constituents may have been diffused throughout its mass, yet the composition of the nebulous material must have differed at different points; otherwise, during the act of agglomeration, each system must have collected and condensed equal proportions of similar materials from the mass around. It cannot be supposed that similarity in physical properties has caused the association of the different elements: we find, for example, some of the least volatile of the metals, such as iron, associated with highly volatile elements, such as mercury and tellurium, in the same star.

If we may so say, there seems to be some analogy between this irregular distribution of the elements in different centres in space, and the manner in which the components of the earth's crust are distributed. Upon the earth there are certain very generally diffused elements, such as oxygen, hydrogen, carbon, silicon, iron, aluminium, and calcium, which occur in all parts; whilst there are others which, like silver, tin, lead, and other metals, are accumulated at particular points only. Whatever may have been the physical causes which may have produced this separation, we see abundant evidence of the advantage of this distribution in their application to the purposes of man—smallness in relative amount being compensated for by the accumulation of the material in denser deposits, which allow of their comparatively easy extraction to supply the wants of mankind. If this arrangement be admitted as designed in the case of the earth, is it going beyond the limits of fair deduction to suppose that, were we acquainted with the economy of those distant globes, an equally obvious purpose might be assigned for the differences in composition which they exhibit?

22. The additional knowledge which these spectrum observations give us of the nature and of the structure of the fixed stars, seems to furnish a basis for some legitimate speculation in reference to the great plan of the visible universe, and to the special object and design of those numerous and immensely distant orbs of light.

The closely marked connexion, in similarity of plan and mode of operation, in those parts of the universe which lie within the range of experiment, and so of our more immediate knowledge, renders it not presumptuous to attempt to apply the process of reasoning from analogy to those parts of the universe which are more distant from us.

Upon the earth we find that the innumerable individual requirements which are connected with the present state of terrestrial activity, are not met by a plan of operation distinct for each, but are effected in connexion with the special modifications of a general method embracing a wide range of analogous phenomena. If we examine living beings, the persistence of unity of plan observable amidst the multiform varieties of special adaptation of the vertebrate form of life may be cited as an example of the unity of operation referred to. In like manner the remarkably wide range of phenomena which are shown to be reciprocally interdependent and correlative of each other, by the recent great extension of our knowledge in reference to the relation of the different varieties of force and their connexion with molecular motion, exhibits a similar unity of operation amidst the changes of the bodies which have not life.

The observations recorded in this paper seem to afford some proof that a similar unity of operation extends through the universe as far as light enables us to have cognizance of material objects. For we may infer that the stars, while differing the one from the other in the kinds of matter of which they consist, are all constructed upon the same plan as our sun, and are composed of matter identical, at least in part, with the materials of our system.

The differences which exist between the stars are of the *lower order*, of differences of *particular adaptation*, or special modification, and not differences of the *higher order* of distinct *plans of structure*.

There is therefore a probability that these stars, which are analogous to our sun in structure, fulfil an analogous purpose, and are, like our sun, surrounded by planets, which they by their attraction uphold, and by their radiation illuminate and energize. And if matter identical with that upon the earth exists in the stars, the same matter would also probably be present in the planets genetically connected with them, as is the case in our solar system.

It is remarkable that the elements most widely diffused through the host of stars are some of those most closely connected with the constitution of the living organisms of our globe, including hydrogen, sodium, magnesium, and iron. Of oxygen and nitrogen we could scarcely hope to have any decisive indications, since these bodies have spectra of different orders. These forms of elementary matter, when influenced by heat, light, and chemical force, all of which we have certain knowledge are radiated from the stars, afford some of the most important conditions which we know to be indispensable to the existence of living organisms such as those with which we are acquainted. On the whole we believe that the foregoing spectrum observations on the stars contribute something towards an experimental basis on which a conclusion, hitherto but a pure speculation, may rest, viz. that at least the brighter stars are, like our sun, upholding and energizing centres of systems of worlds adapted to be the abode of living beings.

Table of Stellar Spectra.

Aldebaran.				α Orionis.				β Pegasi.	
822·5	H	1107		840		1139·5		896	
855·5		1112	Te	860		1144		923	
872·5		1117·5	Sb	870		1145·5		1000	Na
880		1143 *d*		881		1148		1002	
893·5		1158		887		1151		1014	
900		1164	Hg	890		1158·5		1165	
903·5		1171·5		899		1167		1220	
907·5		1178		911		1169·5		1276·5	
915		1187		918	Ca	1176·5		1291·5	
918	Ca	1192		920		1183·5		1297·5	Mg
923	Hg	1202		929		1187		1300·5	
933	Ca	1210		933	Ca	1191·5		1350·5	
945·5	Sb	1224·5		936		1198		1392·5	
951·5		1240		946		1201·5		1425	
954·5		1241·5		966		1210	Te	1515	
956		1250		968·5		1214		1732	
966·5	Sb	1252	Fe	976		1220·5		1835	
972·5		1269·5	Fe	983		1225			
976	Te	1272		992		1237			
982		1277	Bi	1000	Na	1243			
986·5		1282		1002		1252	Fe		
993		1291·5		1010·5		1262			
1000	Na	1297·5	Mg	1013	Ca	1269·5	Fe		
1002		1300·5		1030		1277	Bi		
1013	Ca	1314	Bi	1040		1280·5			
1023		1323		1050·5		1285·5			
1028		1328		1062	Bi	1291·5			
1031		1351		1069·5		1297·5	Mg		
1036·5	Hg	1420	Fe	1079·5		1300·5			
1040		1442·5	Fe	1085·5		1303			
1044	Hg	1483	H	1090		1314	Bi		
1058				1091·5		1334			
1062	Bi			1099		1350			
1067	Te			1105	Ca	1356			
1076				1109·5		1361			
1086·5	Te			1116·5		1416			
1095				1123·5		1420	Fe		
1100				1132		1442·5	Fe		
1105	Ca			1135·5		1557			

LXXV. *On the Spectra of some of the Nebulæ.* By William Huggins, *F.R.A.S. A Supplement to the Paper "On the Spectra of some of the Fixed Stars. By* William Huggins, *F.R.A.S., and* W. A. Miller, *M.D., LL D., Treas. and V.P.R.S."**

[With a Plate.]

THE concluding paragraphs of the preceding paper refer to the similarity of essential constitution which our examination of the spectra of the fixed stars has shown in all cases to exist among the stars, and between them and our sun.

It became therefore an object of great importance, in reference to our knowledge of the visible universe, to ascertain whether this similarity of plan observable among the stars, and uniting them with our sun into one great group, extended to the distinct and remarkable class of bodies known as nebulæ. Prismatic analysis, if it could be successfully applied to objects so faint, seemed to be a method of observation specially suitable for determining whether any essential physical distinction separates the nebulæ from the stars, either in the nature of the matter of which they are composed, or in the conditions under which they exist as sources of light. The importance of bringing analysis by the prism to bear upon the nebulæ is seen to be greater by the consideration that increase of optical power alone would probably fail to give the desired information; for, as the important researches of Lord Rosse have shown, at the same time that the number of the clusters may be increased by the resolution of supposed nebulæ, other nebulous objects are revealed, and fantastic wisps and diffuse patches of light are seen, which it would be assumption to regard as due in all cases to the united glare of suns still more remote.

Some of the most enigmatical of these wondrous objects are those which present in the telescope small round or slightly oval disks. For this reason they were placed by Sir William Herschel in a class by themselves under the name of Planetary Nebulæ. They present but little indication of resolvability. The colour of their light, which in the case of several is blue tinted with green, is remarkable, since this is a colour extremely rare amongst single stars. These nebulæ, too, agree in showing no indication of central condensation. By these appearances the planetary nebulæ are specially marked as objects which probably present phenomena of an order altogether different from those which characterize the sun and the fixed stars. On this account, as well as because of their brightness, I selected these nebulæ as the most suitable for examination with the prism.

* From the Philosophical Transactions for 1864, Part II. Communicated, with additional notes, by the Author.

The apparatus employed was that of which a description was given at page 415. A second eyepiece was used in these observations, having a magnifying-power of nine diameters. For the greater part of the following observations on the nebulæ, the cylindrical lens is not necessary, and was removed from the instrument. The numbers and descriptions of the nebulæ, and their places for the epoch 1860, January 0, included within brackets, are taken from the last Catalogue of Sir John Herschel*.

[No. 4373. 37 H. IV. R.A. $17^h\,58^m\,20^s$. N.P.D. $23°\,22'\,9''\cdot5$. A planetary nebula; very bright; pretty small; suddenly brighter in the middle, very small nucleus.] In Draco.

On August 29, 1864, I directed the telescope armed with the spectrum-apparatus to this nebula. At first I suspected some derangement of the instrument had taken place; for no spectrum was seen, but only a short line of light perpendicular to the direction of dispersion. I then found that the light of this nebula, unlike any other ex-terrestrial light which had yet been subjected by me to prismatic analysis, was not composed of light of different refrangibilities, and therefore could not form a spectrum. A great part of the light from this nebula is monochromatic, and after passing through the prisms remains concentrated in a bright line occupying in the instrument the position of that part of the spectrum to which its light corresponds in refrangibility. A more careful examination with a narrower slit, however, showed that, a little more refrangible than the bright line, and separated from it by a dark interval, a narrower and much fainter line occurs. Beyond this, again, at about three times the distance of the second line, a third exceedingly faint line was seen. The positions of these lines in the spectrum were determined by a simultaneous comparison of them in the instrument with the spectrum of the induction spark taken between electrodes of magnesium. The strongest line coincides in position with the brightest of the air lines. This line is due to nitrogen, and occurs in the spectrum about midway between *b* and F of the solar spectrum. Its position is seen in Plate VI.†

The faintest of the lines of the nebula agrees in position with the line of hydrogen corresponding to Fraunhofer's F. The other bright line was compared with the strong line of barium 2075‡: this line is a little more refrangible than that belonging to the nebula.

Besides these lines, an exceedingly faint spectrum was just perceived for a short distance on both sides of the group of bright

* Philosophical Transactions, Part I. 1864, pp. 1–137.

† See also Philosophical Transactions, 1864, p. 156, and plate 1.

‡ Ibid. p. 156.

lines. I suspect this is not uniform, but is crossed with dark spaces. Subsequent observations on other nebulæ induce me to regard this faint spectrum as due to the solid or liquid matter of the nucleus, and as quite distinct from the bright lines into which nearly the whole of the light from the nebula is concentrated.

In the diagram (fig. 5, Plate V.) the three principal lines only are inserted, for it would be scarcely possible to represent the faint spectrum without greatly exaggerating its intensity.

The colour of this nebula is greenish blue.

[No. 4390. 2000 h. Σ 6. R.A. $18^h\,5^m\,17^s\cdot8$. N.P.D. $83°\,10'\,53''\cdot5$. A planetary nebula; very bright; very small; round; little hazy.] In Taurus Poniatowskii.

The spectrum is essentially the same as that of No. 4373.

The three bright lines occupy the same positions in the spectrum, which was determined by direct comparison with the spectrum of the induction-spark. These lines have also the same relative intensity. They are exceedingly sharp and well defined. The presence of an extremely faint spectrum was suspected. In connexion with this it is important to remark that this nebula does not possess a distinct nucleus.

The colour of this nebula is greenish blue.

[No. 4514. 2050 h. 73 H. IV. R.A. $19^h\,41^m\,7^s\cdot5$. N.P.D. $39°\,49'\,41''\cdot7$. A planetary nebula with a central star. Bright; pretty large; round; star of the 11th magnitude in the middle.] In Cygnus.

The same three bright lines were seen. Their positions in the spectrum were verified by direct comparison with the induction-spark. In addition to these a spectrum could be traced from about D to about G of the solar spectrum. This spectrum is much stronger than the corresponding spectrum of 4373. This agrees with the greater brightness of the central star, or nucleus. The opinion that the faint continuous spectrum is formed alone by the light from the bright central point was confirmed by the following observation. When the cylindrical lens was removed, the three bright lines remained of considerable length, corresponding to the diameter of the telescopic image of the nebula; but the faint spectrum became as narrow as a line, showing that this spectrum is formed by light which comes from an object of which the image in the telescope is a point.

Lord Rosse remarks of this nebula, "A very remarkable object, perhaps analogous to H. 450"*.

The colour of this nebula is greenish blue.

[No. 4510. 2047 h. 51 H. IV. R.A. $19^h\,36^m\,3^s\cdot0$. N.P.D.

* Philosophical Transactions, Part III. 1861, p. 733. For a figure of H. 450 see Philosophical Transactions, 1850, plate 38. fig. 15.

104° 28′ 52″·5. A planetary nebula. Bright; very small; round.] In Sagittarius.

This nebula is less bright than those which have been described. The two brighter of the lines were well defined, and were directly compared with the induction-spark. The third line was seen only by glimpses. I had a suspicion of an exceedingly faint spectrum.

The colour of this nebula is greenish blue.

Lord Rosse remarks, "Centre rather dark. The dark part is a little north preceding the middle" *.

(No. 4628. 2098 h. 1 H. IV. R.A. $20^h\ 56^m\ 31^s{\cdot}2$. N.P.D. 101° 55′ 4″·8. An exceedingly interesting object. Planetary; very bright; small; elliptic.] In Aquarius.

The three bright lines very sharp and distinct. They were compared for position with the induction-spark. Though this object is bright, an indication only of the faint spectrum was suspected. This nebula contains probably a very small quantity of matter condensed into the liquid or solid state.

The colour of the light of this nebula is greenish blue.

Lord Rosse has not detected any central star, nor any perforation as seen in some of the other planetary nebulæ. He represents it with ansæ, which probably indicate a nebulous ring seen edgeways†.

[No. 4447. 2023 h. 57 M. R.A. $18^h\ 48^m\ 20^s$. N.P.D. 57° 8′ 57″·2. An annular nebula; bright; pretty large; considerably elongated.] In Lyra‡.

The apparent brightness of this nebula, as seen in the telescope, is probably due to its large extent; for the faintness of its spectrum indicates that it has a smaller intrinsic brightness than the nebulæ already examined. The brightest of the three lines was well seen. I suspected also the presence of the next in brightness. No indication whatever of a faint spectrum. The bright line looks remarkable, since it consists of two bright dots corresponding to sections of the ring; and between these there was not darkness, but an excessively faint line joining them. This observation makes it probable that the faint nebulous matter occupying the central portion is similar in constitution to that of the ring. The bright line was compared with the induction-spark*.

[No. 4964. 2241 h. 18 H. IV. R.A. $23^h\ 19^m\ 9^s{\cdot}9$. N.P.D. 48° 13′ 57″·5. Planetary; very bright; pretty small, round, blue.]

With a power of 600 this nebula appears distinctly annular. The colour of its light is greenish blue†. The spectrum formed by the light from this nebula corresponds with that of 37 H. IV. represented in fig. 5, Plate V.

In the spectrum of this nebula, however, in addition to the three bright lines, a fourth bright line, excessively faint, was seen. This line is about as much more refrangible than the line agreeing in position with F as this line is more refrangible than the brightest of the lines, which coincides with a line of nitrogen.

[No. 4532. 2060 h. 27 M. R.A. $19^h\ 53^m\ 29^s{\cdot}3$. N.P.D. 67° 39′ 43″. Very bright; very large; irregularly extended. Dumb-bell.] In Vulpecula.

The light of this nebula, after passing through the prisms, remained concentrated in a bright line corresponding to the brightest of the three lines represented in fig. 5, Plate V. This line appeared nebulous at the edges. No trace of the other lines was perceived, nor was a faint continuous spectrum detected.

The bright line was ascertained, by a simultaneous comparison with the spectrum of the induction-spark, to agree in position with the brightest of the lines of nitrogen.

* Philosophical Transactions, 1861, Part III. p. 732.

† Ibid. 1850, p. 507, and plate 38. fig. 14.

‡ Lord Rosse, in his description of this nebula, remarks, "The filaments proceeding from the edge become more conspicuous under increasing magnifying-power within certain limits, which is strikingly characteristic of a cluster; still I do not feel confident that it is resolvable."—Philosophical Transactions, 1844, p. 322, and plate 19. fig. 29.

In 1850 Lord Rosse further remarks, "I have not yet sketched it with the 6-feet instrument, because I have never seen it under favourable circumstances: the opportunities of observing it well on the meridian are comparatively rare, owing to twilight. It was observed seven times in 1848, and once in 1849. The only additional particulars I collect from the observations are that the central opening has considerably more nebulosity, and there is one pretty bright star in it, s. f. the centre, and a few other very minute stars. In the sky round the nebula and near it there are several very small stars which were not before seen; and therefore the stars in the dark opening may possibly be merely accidental. In the annulus, especially at the extremities of the minor axis, there are several minute stars, but there was still much nebulosity not seen as distinct stars."—Philosophical Transactions, 1850, p. 506.

"Nothing additional since 1844, except a star s. f. the middle."—Philosophical Transactions, 1861, p. 732.

* Already in 1850 Lord Rosse had discovered a connexion in general plan of structure between some of the nebulæ which present small planetary disks in ordinary telescopes, and the annular nebula in Lyra. His words are, "There were but two annular nebulæ known in the northern hemisphere when Sir John Herschel's Catalogue was published; now there are seven, as we have found that five of the planetary nebulæ are really annular. Of these objects, the annular nebula in Lyra is the one in which the form is the most easily recognized."—Philosophical Transactions, 1850, p. 506.

† For Lord Rosse's observations of this nebula, see Philosophical Transactions, 1844, p. 323; ibid. 1850, p. 507 and plate 38. fig. 13; ibid. 1861, p. 736 and plate 30. fig. 40.

Minute points of light have been observed in this nebula by Lord Rosse, Otto Struve, and others; the spectra of these bright points, especially if continuous like those of stars, are doubtless invisible from excessive faintness.

By suitable movements given to the telescope, different portions of the image of the nebula formed in the telescope were caused successively to fall upon the opening of the slit, which was about $\frac{1}{10}$ inch by $\frac{1}{300}$ inch. This method of observation showed that the light from different parts of the nebula is identical in refrangibility, and varies alone in degree of intensity *.

In addition to these objects the following were also observed :—

[No. 4294. 92 M. R.A. $17^h\ 12^m\ 56^s{\cdot}9$. N.P.D. $46°\ 43'\ 31''{\cdot}2$.] In Hercules. Very bright globular cluster of stars. The bright central portion was brought upon the slit. A faint spectrum similar to that of a star. The light could be traced from between C and D to about G.

Too faint for the observation of lines of absorption.

[No. 4244. 50 H. IV. R.A. $16^h\ 43^m\ 6^s{\cdot}4$. N.P.D. $42°\ 8'\ 38''{\cdot}8$. Very bright; large; round.] In Hercules. The spectrum similar to that of a faint star. No indication of bright lines.

[No. 116. 50 h. 31 M. R.A. $0^h\ 35^m\ 3^s{\cdot}9$. N.P.D. $49°\ 29'\ 45''{\cdot}7$.] The brightest part of the great nebula in Andromeda was brought upon the slit.

The spectrum could be traced from about D to F. The light appeared to cease very abruptly in the orange; this may be due to the smaller luminosity of this part of the spectrum. No indication of the bright lines.

[No. 117. 51 h. 32 M. R.A. $0^h\ 35^m\ 5^s{\cdot}3$. N.P.D. $49°\ 54'\ 12''{\cdot}7$. Very very bright; large; round; pretty suddenly much brighter in the middle.]

This small but very bright companion of the great nebula in Andromeda presents a spectrum apparently exactly similar to that of 31 M.

The spectrum appears to end abruptly in the orange, and,

* The author found, on November 23, 1864, that the light of the great nebula in Orion is resolved by the prism into three bright lines.—Proceedings of the Royal Society, vol. xiv. p. 39.

During 1865 he discovered that the nebulæ which follow have similar spectra, consisting of one, two, or three bright lines, in addition to which, in the case of some of them, a continuous spectrum was visible.

2102....	27 H. IV.	4499....	38 H. VI.
4234....	5 Σ.	4827....	705 H. I.
4403....	17 M.	4627....	192 H. I.
4572....	16 H. IV.		

—Proceedings of the Royal Society, vol. xv. p. 18.

throughout its length, is not uniform but is evidently crossed either by lines of absorption or by bright lines.

[No. 428. 55 Androm. R.A. $1^h\ 44^m\ 55^s{\cdot}9$. N.P.D. $49°\ 57'\ 41''{\cdot}5$. Fine nebulous star with strong atmosphere.] The spectrum apparently similar to that of an ordinary star*.

[No. 826. 2618 h. 26 IV. R.A. $4^h\ 7^m\ 50^s{\cdot}8$. N.P.D. $103°\ 5'\ 32''{\cdot}2$. Very bright cluster.] In Eridanus. The spectrum could be traced from the orange to about the blue. No indication of the bright lines.

Several other nebulæ were observed, but of these the light was found to be too faint to admit of satisfactory examination with the spectrum-apparatus †.

It is obvious that the nebulæ 37 H. IV., 6 Σ., 73 H. IV., 51 H. IV., 1 H. IV., 57 M., 18 H. IV., and 27 M. can no longer be regarded as aggregations of suns after the order to which our own sun and the fixed stars belong. We have in these objects to do no longer with a special modification only of our own type of suns, but find ourselves in the presence of objects possessing a distinct and peculiar plan of structure.

In place of an incandescent solid or liquid body transmitting light of all refrangibilities through an atmosphere which intercepts by absorption a certain number of them, such as our sun appears to be, we must probably regard these objects, or at least their photo-surfaces, as enormous masses of luminous gas or vapour. For it is alone from matter in the gaseous state that light consisting of certain definite refrangibilities only, as is the case with the light of these nebulæ, is known to be emitted.

It is indeed *possible* that suns endowed with these peculiar conditions of luminosity may exist, and that these bodies are clusters of such suns. There are, however, some considerations, especially in the case of the planetary nebulæ, which are scarcely in accordance with the opinion that they are clusters of stars.

Sir John Herschel remarks of one of this class, in reference to the absence of central condensation, "Such an appearance would not be presented by a globular space uniformly filled with stars or luminous matter, which structure would necessarily give rise to an apparent increase of brightness towards the centre, in proportion to the thickness traversed by the visual ray. We might therefore be inclined to conclude its real constitution to

* "Looked at eight times, but saw no nebulous atmosphere."—Lord Rosse, Philosophical Transactions, 1861, p. 712.

† The author has since observed 31 nebulæ and clusters, each of which gives a continuous spectrum.—Proceedings of the Royal Society, vol. xiv. p. 39; vol. xv. p. 18.

be either that of a hollow spherical shell or of a flat disk presented to us (by a highly improbable coincidence) in a plane precisely perpendicular to the visual ray"*. This absence of condensation admits of explanation, without recourse to the supposition of a shell or of a flat disk, if we consider them to be masses of glowing gas. For supposing, as we probably must do, that the whole mass of the gas is luminous, yet it would follow, by the law which results from the investigations of Kirchhoff, that the light emitted by the portions of gas beyond the surface visible to us, would be in great measure, if not wholly, absorbed by the portion of gas through which it would have to pass; and for this reason there would be presented to us a *luminous surface* only†.

Sir John Herschel further remarks‡, "Whatever idea we may form of the real nature of the planetary nebulæ, which all agree in the absence of central condensation, it is evident that the intrinsic splendour of their surfaces, if continuous, must be almost infinitely less than that of the sun. A circular portion of the sun's disk subtending an angle of 1′ would give a light equal to that of 780 full moons, while among all the objects in question there is not one which can be seen with the naked eye." The small brilliancy of these nebulæ is in accordance with the conclusions suggested by the observations of this paper; for, reasoning by analogy from terrestrial physics, glowing or luminous gas would be very inferior in splendour to incandescent solid or liquid matter§.

* Outlines of Astronomy, 7th edit. p. 646.

† Sir William Herschel in 1811 pointed out the necessity of supposing the matter of the planetary nebulæ to have the power of intercepting light. He wrote:—"Admitting that these nebulæ are globular collections of neulous matter, they could not appear equally bright if the nebulosity of which they are composed consisted only of a luminous substance perfectly penetrable to light. . . . . Is it not rather to be supposed that a certain high degree of condensation has already brought on a sufficient consolidation to prevent the penetration of light, which by this means is reduced to a superficial planetary appearance?"

"Their planetary appearance shows that we only see a superficial lustre such as opake bodies exhibit, and which could not happen if the nebulous matter had no other quality than that of shining, or had so little solidity as to be perfectly transparent."—Philosophical Transactions, 1811, pp. 314, 315.

‡ Outlines of Astronomy, 7th edit. p. 646.

§ The author has made an attempt to determine approximately the *intrinsic* brightness of three of the gaseous nebulæ. It is probable that these bodies consist of continuous masses of material. In the telescope they present surfaces subtending a considerable angle. As long as a distant object is of sensible size in the telescope, its original brightness remains unaltered. By a suitable method of observation, the intensities of these

Such gaseous masses would be doubtless, from many causes, unequally dense in different portions; and if matter condensed into the liquid or solid state were also present, it would, from its superior splendour, be visible as a bright point or points within the disk of the nebula. These suggestions are in close accordance with the observations of Lord Rosse.

Another consideration which opposes the notion that these nebulæ are clusters of stars is found in the extreme simplicity of constitution which the three bright lines suggest, whether or not we regard these lines as indicating the presence of nitrogen, hydrogen, and a substance unknown.

It is perhaps of importance to state that, except nitrogen, no one of thirty of the chemical elements the spectra of which I have measured has a strong line very near the bright line of the nebulæ. If, however, this line were due to nitrogen, we ought to see other lines as well; for there are specially two strong double lines in the spectrum of nitrogen, one at least of which, if they existed in the light of the nebulæ, would be easily visible*. In my experiments on the spectrum of nitrogen, I found that the character of the brightest of the lines of nitrogen, that with which the line in the nebulæ coincides, differs from that of the two double lines next in brilliancy. This line is more nebulous at the edges, even when the slit is narrow and the other lines are thin and sharp. The same phenomenon was observed with some of the other elements†. We do not yet know the origin of this difference of character observable among lines of the same element. May it not indicate a physical difference in the atoms,

nebulæ have been obtained in terms of the light of a sperm candle burning at the rate of 158 grains per hour.

The light of nebula 4628, 1 H. IV. = $\frac{1}{[illegible]}$ part of that of candle.
" annular nebula, Lyra. . = $\frac{1}{[illegible]}$ " "
" Dumb-bell nebula. . . . = $\frac{1}{[illegible]}$ " "

These values are too small by the unknown corrections for the possible power of extinction of space, and for the absorptive power of the earth's atmosphere.—Proceedings of the Royal Society, vol. xv. p. 18.

* Philosophical Transactions, 1864, p. 154 and plate 1.

For the purpose of ascertaining whether the absence of the other bright lines of nitrogen might be connected with the presence of hydrogen, I arranged an apparatus in which, while the spectrum of the induction-spark in a current of nitrogen was being observed, a current of hydrogen could be introduced, and the proportion of the two gases to each other easily regulated. With this apparatus the fading out of the bright lines of nitrogen, as the proportion of this gas to hydrogen was diminished, and again their increase in brilliancy when the current of nitrogen was made stronger, were carefully observed, but without detecting any marked variation in the relative brightness of the lines.

† Philosophical Transactions, 1864, pp. 143, 150.

in connexion with the vibrations of which the lines are probably produced? The speculation presents itself, whether the occurrence of this one line only in the nebulæ may not indicate a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect*.

Observations on other nebulæ, which I hope to make, may throw light upon these and other considerations connected with these wonderful objects.

* On January 9, 1866, the author observed the spectrum of comet I. 1866. The comet appeared in his telescope as an oval nebulous mass surrounding a small and dim nucleus. The prism showed that the nucleus was self-luminous, that it consisted of matter in the state of ignited gas, and that *this matter is similar in constitution to the gaseous material of some of the nebulæ*. The coma shines by reflected solar light.—Proceedings of the Royal Society, vol. xv. p. 5.

LXXVI. *On the Level of the Sea during the Glacial Epoch.* By Archdeacon Pratt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SINCE I sent you a paper on the above subject, which was published in your Number for March, I have seen several letters which Mr. Croll's first letter has drawn forth; and I perceive that I have omitted to consider the effect of the drain upon the ocean which the formation of Mr. Croll's ice-sheet would cause. My calculation of the amount of rise in the level of the ocean in passing from the equator to northern latitudes may be all right. But the drain upon the ocean would reduce the level so as to make my starting-point (the equator) much lower, I conceive (probably 2000 feet), than the rise caused by the attraction of the ice-sheet. This therefore most materially affects the result.

This demand for water to form the ice-sheet would be supplied in part (but not sufficiently) from the southern hemisphere; for the attraction of the ice-sheet in the north would produce a depression in the ocean at the south pole and parts thereabout, and *by no means* an elevation.

I say "by no means," in reference to the way in which some of the writers and calculators in this problem have supposed that they see an analogy between the perturbation of the ocean by the moon and by the ice-sheet. But the cases are quite different. The moon is a free body, and as it draws the ocean so it draws the earth's centre; and the *difference* of effects must be taken to find the state of the ocean with reference to the earth's centre.

Not so with the ice-sheet. The ice-sheet is rigidly attached to the earth, which the moon is not. It may produce a strain in the solid materials of the earth by its attraction, but cannot draw the earth's centre to it. Hence the ice-sheet will have its whole effect on the ocean, which must not be diminished by theoretically applying a force equal to the attraction on the centre in the opposite direction.

It may be said, But the moon does *not* actually draw the earth's centre to it. If this be true, it is because the earth-and-moon's revolving round their common centre of gravity produces a centrifugal force in each, so as to keep each in its place; and this centrifugal force affects not only the centre, but the ocean too.

It is not, however, strictly true that the moon does not draw the earth's centre to it. This would be the case if the orbit were a circle; but it is more like an ellipse, the moon sometimes receding, sometimes approaching the earth's centre. This of itself might perhaps be a sufficient answer to the writer who says that the moon's action on the earth and ocean would not be affected if it were rigidly connected with the earth, and therefore why may not the ice-sheet be considered to be like the moon in its effect in raising the water opposite to it as well as under and near it? But the proper answer is, that the ice-sheet's being rigidly connected with the earth's centre produces a force which keeps them at the same distance from each other, *and* that force has no effect whatever (as the centrifugal force between the moon and earth has) on the ocean. In fact, to return to what I said at first, the moon is a free body with reference to the earth, but the ice-sheet is a fixed body.

J. H. Pratt.

Calcutta, April 20, 1866.

LXXVII. *On the Observations and Calculations required to find the Tidal Retardation of the Earth's Rotation.* By Professor W. Thomson, F.R.S.*

THE first *publication* of any definite estimate of the possible amount of the diminution of rotatory velocity experienced by the earth through tidal friction is due, I believe, to Mr. William Ferrel, and is to be found in the Number for December 8, 1853, of the Astronomical Journal of Cambridge, United States. It is founded on calculating the moment round the earth's centre of the attraction of the moon, on a regular spheroidal shell of water symmetrical about its longest axis, this being (through the influence of fluid friction) kept in a position inclined backwards

* From the Rede Lecture, Cambridge, May 23, 1866, "On the Dissipation of Energy." Communicated by the Author.

at an acute angle to the line from the earth's centre to the moon. One of the simplest ways of seeing the result is this:—First, by the known conclusions as to the attractions of ellipsoids, or still more easily by the consideration of the proper "spherical harmonic"* (or Laplace's coefficient) of the second degree, we see that an equipotential surface lying close to the bounding surface of a nearly spherical homogeneous solid ellipsoid is approximately an ellipsoid with axes differing from one another by three-fifths of the amounts of the differences of the corresponding axes of the ellipsoidal boundary. From this it follows† that a homogeneous prolate spheroid of revolution attracts points outside it approximately as if its mass were collected in a uniform bar having its ends in the foci of the equipotential spheroid. If, for example, a globe of water of 21,000,000 feet radius (this being nearly enough the earth's radius) be altered into a prolate spheroid with longest radii exceeding the shortest radii by two feet, the equipotential spheroid will have longest and shortest radii differing by $\frac{6}{5}$ of a foot. The foci of this latter will be at 7100 feet on each side of the centre; and therefore the resultant of gravitation between the supposed spheroid of water and external bodies will be the same as if its whole mass were collected in a uniform bar of 14,200 feet length. But by a well-known proposition‡, a uniform line F F′ (a diagram is unnecessary) attracts a point M in the line M K bisecting the angle F M F′. Let C Q be a perpendicular from C, the middle point of F′ F, to this bisecting line M K. If C M be $60 \times 21 \times 10^6$ (the moon's distance), and if the angle F C M be 45°, we find, by elementary geometry, $CQ = \cdot 02$ of a foot (about $\frac{1}{4}$ inch). The mass of a globe of water equal in bulk to the earth is $1\cdot 1 \times 10^{21}$ tons §. And, the moon's mass being about $\frac{1}{75}$ of the earth's, the attraction of the moon on a ton at the earth's distance is $\frac{1}{75} \times \frac{1}{60^2}$, or $\frac{1}{270{,}000}$ of a ton force, if, for brevity, we call a ton force the ordinary terrestrial weight of a ton—that is to say, the amount of the earth's attraction on a ton at its surface. Hence the whole force of the moon on the earth is $\frac{1\cdot 1 \times 10^{21}}{270{,}000}$, or $4\cdot 1 \times 10^{15}$ tons force. If, then, the tidal disturbance were exactly what we have supposed, or if it were (however irregular) such as to have the same resultant effect, the retarding influence of the moon's attraction would be that of $4\cdot 1 \times 10^{15}$ tons force acting in the plane of the equator and in a line passing the centre at $\frac{1}{50}$ of a foot distance. Or it would be the same as a simple frictional resistance (as of a friction-brake) consisting of $4\cdot 1 \times 10^{15}$ tons force acting tangentially against the motion of a pivot or axle of about $\frac{1}{2}$ inch diameter. To estimate the retardation produced by this, we shall suppose the square of the earth's radius of gyration, instead of being $\frac{2}{5}$, as it would be if the mass were homogeneous, to be $\frac{1}{3}$ of the square of the radius of figure, as it is made to be, by Laplace's probable law of the increasing density inwards, and by the amount of precession calculated on the supposition that the earth is quite rigid. Hence (if we take $g = 32\cdot 2$ feet per second generated per second, and the earth's mass as $6\cdot 1 \times 10^{21}$ tons) the loss of angular velocity per second, on the other suppositions we have made, will be

$$\frac{32\cdot 2 \times 4\cdot 1 \times 10^{15} \times \cdot 02}{6\cdot 1 \times 10^{21} \times \frac{1}{3}(21 \times 10^6)^2}, \text{ or } 2\cdot 94 \times 10^{-21}.$$

The loss of angular velocity in a century would be $31\frac{1}{2} \times 10^8$ times this, or $\cdot 93 \times 10^{-11}$, which is as much as $\frac{1\cdot 28}{10^7}$ of $\frac{2\pi}{86400}$, the present angular velocity. Thus in a century the earth would be rotating so much slower that, regarded as a time-keeper, it would lose about one second and a quarter in ten million, or four seconds in a year. And the accumulation of effect of uniform retardation at that rate would throw the earth as a time-keeper behind a perfect chronometer (set to agree with it in rate and absolute indication at any time) by 200 seconds at the end of a century, 800 seconds at the end of two centuries, and so on. In the present very imperfect state of clock-making (which scarcely produces an astronomical clock two or three times more accurate than a marine chronometer or good pocket-watch), the only chronometer by which we can check the earth is one which goes much worse—the moon. The marvellous skill and vast labour devoted to the lunar theory by the great physical astronomers Adams and Delaunay, seem to have settled that the earth has really lost in a century about ten seconds of time on the moon corrected for perturbations. M. Delaunay has suggested that the true cause may be tidal friction, which he has proved to be probably sufficient by some such estimate as the preceding*. But the many disturbing influences to which

* Thomson and Tait's 'Natural Philosophy,' § 536 (4).

† Ibid. § 501 and § 480 (*e*).

‡ Ibid. § 480 (*b*) and (*a*).

§ In stating large masses, if English measures are used at all, the ton is convenient, because it is 1000 kilogrammes nearly enough for many practical purposes and rough estimates. It is 1016·047 kilogrammes; so that a ton diminished by about 1·6 per cent. would be just 1000 kilogrammes.

* It seems hopeless, without waiting for some centuries, to arrive at

the earth is exposed render it a very untrustworthy time-keeper. For instance, let us suppose ice to melt from the polar regions (20° round each pole, we may say) to the extent of something more than a foot thick, enough to give 1·1 foot of water over those areas, or ·066 of a foot of water if spread over the whole globe, which would in reality raise the sea-level by only some such almost undiscoverable difference as $\frac{3}{4}$ of an inch, or an inch. This, or the reverse, which we may believe might happen any year, and could certainly not be detected without far more accurate observations and calculations for the mean sea-level than any hitherto made, would slacken or quicken the earth's rate as a time-keeper by one-tenth of a second per year*.

Again, an excellent suggestion, supported by calculations which show it to be not improbable, has been made to the French Academy by M. Dufour, that the retardation of the earth's rotation indicated by M. Delaunay, or some considerable part of it, may be due to an increase of its moment of inertia by the incorporation of meteors falling on its surface. If we suppose the previous average moment of momentum of the meteors round the earth's axis to be zero, their influence will be calculated just as I have calculated that of the supposed melting of ice. Thus meteors falling on the earth in fine powder (as is in all probability the lot of the greater number that enter the earth's atmosphere and do not escape into external space again) enough to form a layer about $\frac{1}{20}$ of a foot thick in 100 years, if of twice the density of water, would produce the supposed retardation of

any approach to an exact determination of the amount of the actual retardation of the earth's rotation by tidal friction, except by extensive and accurate observation of the amounts and times of the tides on the shores of continents and islands in all seas, and much assistance from *true* dynamical theory to estimate these elements all over the sea. But supposing them known for every part of the sea, the retardation of the earth's rotation is easily calculated by quadratures.

* The calculation is simply this. Let E be the earth's whole mass, a its radius, k its radius of gyration before, and k' after the supposed melting of the ice, and W the mass of ice melted. Then, since $\frac{2}{3}a^2$ is the square of the radius of gyration of the thin shell of water supposed spread uniformly over the whole surface, and that of either ice-cap is very approximately $\frac{1}{2}a^2(\sin 20°)^2$, we have

$$Ek'^2 = Ek^2 + Wa^2[\tfrac{2}{3} - \tfrac{1}{2}(\sin 20)^2].$$

And, by the principle of the conservation of moments of momentum, the rotatory velocity of the earth will vary inversely as the square of its radius of gyration. To put this into numbers, we take, as above, $k^2 = \frac{1}{3}a^2$ and $a = 21 \times 10^6$. And as the mean density of the earth is about $5\frac{1}{2}$ times that of water, and the bulk of a globe is the area of its surface into $\frac{1}{3}$ of its radius,

$$E : W :: \frac{5 \cdot 5a}{3} : \cdot 066.$$

10^s on the time shown by the earth's rotation. But this would also accelerate the moon's mean motion by half the same proportional amount; and therefore a layer of meteor-dust accumulating at the rate of $\frac{1}{30}$ of a foot per century, or 1 foot in 3000 years, would suffice to explain Messrs. Adams and Delaunay's result. I see no other way of directly testing the probable truth of M. Dufour's very interesting hypothesis than to chemically analyze quantities of natural dust taken from any suitable localities (such dust, for instance, as has accumulated in two or three thousand years to depths of many feet over Egyptian, Greek, and Roman monuments). Should a considerable amount of iron with a large proportion of nickel be found or not found, strong evidence for or against the meteoric origin of a sensible part of the dust would be afforded.

Another source of error in the earth as a time-keeper, which has often been discussed, is its shrinking by cooling. But I find by the estimates I have given elsewhere* of the present state of deep underground temperatures, and by taking $\frac{1}{100000}$ as the vertical contraction per degree Centigrade of cooling in the earth's crust, that the gain of time by the earth, regarded as a clock, would not in a century amount to more than $\frac{1}{30}$ of a second, or $\frac{1}{6000}$ of the amount estimated above as conceivably due to tidal friction.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 477.]

March 15, 1866.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

"On a possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust." By John Evans, F.R.S., Sec. G.S.

At a time when the causes which have led to climatal changes in various parts of the globe are the subject of so much discussion, but little apology is needed for calling the attention of this Society to what possibly may have been one of these causes, though it has apparently hitherto escaped observation.

That great changes of climate have taken place, at all events in the northern hemisphere of the globe, is one of the best-established facts of geology, and that corresponding changes have not been noticed to the same extent in the southern hemisphere may possibly be considered due rather to a more limited amount of geological observation than to an absence of the phenomena indicative of such alterations in climatal conditions having occurred.

The evidence of the extreme refrigeration of this portion of the

* "Secular Cooling of the Earth." Transactions of the Royal Society of Edinburgh, 1862; and Philosophical Magazine, January 1863.

earth at the Glacial Period is constantly receiving fresh corroboration; and various theories have been proposed which account for this accession of cold in a more or less satisfactory manner.

Variations in the distribution of land and water, changes in the direction of the Gulf-stream, the greater or less eccentricity of the earth's orbit, the passage of the Solar System through a cold region in space, fluctuations in the amount of heat radiated by the sun, alternations of heat and cold in the northern and southern hemispheres, as consequent upon the precession of the equinoxes, and even changes in the position of the centre of gravity of the earth, and consequent displacements of the polar axis, have all been adduced as causes calculated to produce the effects observed; and the reasoning founded on each of these data is no doubt familiar to all.

The possibility of any material change in the axis of rotation of the earth has been so distinctly denied by Laplace* and all succeeding astronomers, that any theory involving such a change, however tempting as affording a solution of certain difficulties, has been rejected by nearly all geologists as untenable.

Sir Henry James†, however, writing to the 'Athenæum' newspaper in 1860, stated that he had long since arrived at the conclusion that there was no possible explanation of some of the geological phenomena testifying to the climate at certain spots having greatly varied at different periods, without the supposition of constant changes in the position of the axis of the earth's rotation. He then, assuming as an admitted fact that the earth is at present a fluid mass with a hardened crust, showed that slaty cleavage, dislocations, and undulations in the various strata are results which might be expected from the crust of the earth having to assume a new external form, if caused to revolve on a new axis, and advanced the theory that the elevation of mountain-chains of larger extent than at present known produced these changes in the position of the poles.

The subject was discussed in further letters from Sir Henry James, the Astronomer Royal, Professors Beete Jukes and Hennessy, and others; but throughout the discussion the principal question at issue seems to have been whether any elevation of a mountain-mass could sensibly affect the position of the axis of rotation of the globe as a whole; and the general verdict was in the negative.

At an earlier period (1848) the late Sir John Lubbock, in a short but conclusive paper in the 'Quarterly Journal of the Geological Society'‡ pointed out what would have been the effect had the axis of rotation of the earth not originally corresponded with the axis of figure, and also mentioned some considerations which appear to have been absent from Laplace's calculations.

Sir John Lubbock, however, in common with other astronomers, appears to have regarded the earth as consisting of a solid nucleus with a body of water distributed over a portion of its surface; and there can be but little doubt that, on this assumption of the solidity of the earth, the usually received doctrines as to the general persistence of the direction of the poles are almost unassailable.

* Mécanique Céleste, vol. v. p. 14. † Athenæum, Aug. 25, 1860, &c.
‡ Vol. v. p. 5.

Directly, however, that we argue from the contrary assumption, that the solid portion of the globe consists of a comparatively thin but to some extent rigid crust, with a fluid nucleus of incandescent mineral matter within, and that this crust, from various causes, is liable to changes disturbing its equilibrium, it becomes apparent that such disturbances may lead, if not to a change in the position of the general axis of the globe, yet at all events to a change in the relative positions of the solid crust and the fluid nucleus, and in consequence to a change in the axis of rotation, so far as the former is concerned.

The existence in the centre of the globe of a mass of matter fluid by heat, though accepted as a fact by many (if not most) geologists, has no doubt been called in question by some, and among them a few of great eminence. The gradual increase of temperature, however, which is found to take place as we descend beneath the surface of the earth, and which has been observed in mines and deep borings all over the world, the existence of hot springs, some of the temperature of boiling water, and the traces of volcanic action, either extinct or still in operation, which occur in all parts of the globe, afford strong arguments in favour of the hypothesis of central heat.

And though we are at present unacquainted with the exact law of the increment of heat at different depths, and though, no doubt, under enormous pressure the temperature of the fusing-point of all substances may be considerably raised, yet the fact of the heat increasing with the depth from the surface seems so well established that it is highly probable that at a certain depth such a degree of heat must be attained as would reduce all mineral matter with which we are acquainted into a state of fusion. When once this point was attained, it seems probable that there would be no very great variation in the temperature of the internal mass; but whether the whole is in one uniform state of fluidity, or whether there is a mass of solid matter in the centre of the fluid nucleus, are questions which do not affect the hypothesis about to be considered.

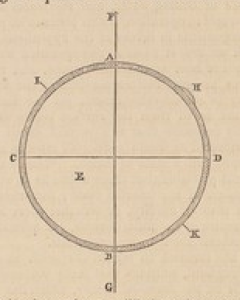
Those who are inclined to regard the earth as a solid or nearly solid mass throughout, consider that many volcanic phenomena may be accounted for on the chemical theory, which has received the support, among others, of Sir Charles Lyell. But apart from the consideration that such chemical action must of necessity be limited in its duration, the existence of local seas of fluid matter, resulting from the heat generated by intense chemical action, would hardly account for the increase of heat at great depths in places remote from volcanic centres; and the rapid transmission of shocks of earthquakes and the enormous amount of upheaval and subsidence as evidenced by the thickness of the sedimentary strata, seem inconsistent either with the general solidity of the globe or any very great thickness of its crust.

The supposition that the gradual oscillations of the surface of the earth, of which we have evidence all over the world as having taken place ever since the formation of the earliest known strata up to the present time, are due to the alternate inflation by gas and the subsequent depletion of certain vast bladdery cavities in the crust of the earth, can hardly be generally accepted.

Those who wish to see the arguments for and against the theory of there being a fluid nucleus within the earth's crust, will find them well and fairly stated in Naumann's 'Lehrbuch der Geognosie'*. My object is, not to discuss that question, but to point out what, assuming the theory to be true, would be some of the effects resulting from such a condition of things, more especially as affecting climatal changes. The agreement or disagreement between these hypothetical results and observed facts may ultimately assist in testing the truth of the assumption.

The simplest form in which we can conceive of the relations to each other of a solid crust and a fluid nucleus in rotation together is that of a sphere.

Let A C B D be a hollow sphere composed of solid materials and of perfectly uniform thickness and density, and let it be filled with the fluid matter E, over which the solid shell can freely move, and let the whole be in uniform rotation about an axis F G, the line C D representing the equator. It is evident that in such a case, the



hollow sphere being in perfect equilibrium, its axis and that of its fluid contents would perpetually coincide. If, however, the equilibrium of the shell or crust be destroyed, as, for instance, by the addition of a mass of extraneous matter at H, midway between the pole and the equator, not only would the position of the axis of rotation be slightly affected by the alteration in the position of the centre of gravity of the now irregular sphere, but the centrifugal force of the excess of matter at H would gradually draw over the shell towards D until, by sliding over the nucleus, it attained its greatest possible distance from the centre of revolution by arriving at the equator. The resultant effect would be that though the whole sphere continued to revolve around an axis as nearly as possible in the line F G, yet the position of the pole of the hollow shell would have been changed by 45°, as by the passage of H to

* 2nd edit., 1858, vol. i. p. 36.

the equator the points I and K would have been brought to the poles by spirals constantly decreasing in diameter, while A and B, by spirals constantly increasing, would have at last come to describe circles midway between the poles and the equator.

The axis of rotation of the hollow sphere and that of its fluid contents would now again coincide, and would continue to do so perpetually unless some fresh disturbance in the equilibrium of the shell took place.

If instead of the addition of fresh matter at H we had supposed an excavation or removal of some portion of the shell, a movement in the axis of rotation of the shell would also have ensued, since from the diminished centrifugal force of that portion of the hollow sphere where the excavation had taken place, it would no longer equipoise the corresponding portion on the opposite side at I, and the excavated spot would eventually find its way to the pole.

In order more clearly to exhibit these effects, I have prepared a model in accordance with a suggestion of Mr. Francis Galton, F.R.S., in which a wheel representing a section of a hollow sphere has its axis, upon which it can freely turn, fixed in a frame, which is itself made to revolve in such a manner that the axis of its rotation passes through one of the diameters of the wheel, and coincides with what would be the axis of the sphere of which the wheel is a section.

In the periphery of the wheel are a number of adjustable screws with heavy heads, so that, by screwing any of them in or out, the addition of matter or its abstraction at any part of the sphere may be represented.

If by adjusting these screws the wheel could be brought into perfect equilibrium, its position upon its own axis would remain unchanged in whatever position it was originally placed, notwithstanding any amount of rotation being given to the frame in which it is hung; but practically it is found that with a certain given position of the screws a certain part of the wheel coincides with the axis of the frame, or becomes the pole around which the sphere revolves. The rim of the wheel is graduated so as to show the position of the poles in all cases, and generally speaking the wheel always settles down after rotation with the pole within three or four degrees of the same spot, if no alteration has been made in the adjustment of the screws, though of course what was the uppermost pole may become the lower one; and in some cases the wheel may be *in æquilibrio* with a projecting screw either above or below the equator, in which case there may be four readings on the circle at the index-point, according as the one pole or the other is uppermost, and the projecting screw is above or below the equator.

With the screws on the wheel evenly balanced, a slight alteration in the adjustment of any of them immediately tells upon the position of what, for convenience sake, may be called the poles, except, indeed, in such cases as screwing outwards those already at the equator, or making similar alterations in the adjustment of two screws at equal distances on either side of one of the poles. If a screw be turned outwards so as notably to project at any spot, no matter how near to the pole, it will be found, after the machine has been a short time in revolution, in the region of the equator. Or again, if one or, better still, two opposite screws at the equator be turned inwards, they will be found after a short period of revolution at the poles.

Now let us assume for a moment that, though the crust was partially covered by water, the earth, instead of being a spheroid, was a perfect sphere, consisting of a hardened crust of moderate thickness supported on a fluid nucleus over which the crust could travel freely in any direction, but both impressed with the same original rotatory motion, so that without some disturbing cause they would continue to revolve for ever upon the same axis, and as if they were one homogeneous body. Let us assume, moreover, that this crust, though in perfect equilibrium on its centre of rotation, was not evenly spherical externally, but had certain projecting portions, such as would be represented in Nature by continents and islands rising above the level of the sea.

It is evident that so long as those continents and islands remained unaltered in their condition and extent, the relative position of the crust to the enclosed fluid nucleus would remain unaltered also. But supposing those projecting masses were either further upheaved from some internal cause, or worn down and ground away by the sea or by subaërial agency and deposited elsewhere, it seems impossible but that the same effects must ensue as we see resulting upon the model from the elevation and depression of certain screws, and that the axis of rotation of the crust of the sphere would be changed in consequence of its having assumed a fresh position upon its fluid nucleus, though the axis of the whole sphere might have retained its original direction, or have altered from it only in the slightest degree.

An irregular accumulation of ice at one or both of the poles, such as is supposed by M. Adhémar, would act in the same manner as an elevation of the land; and even assuming that the whole land had disappeared from above the surface of the sea, yet if by marine currents the shallower parts of the universal ocean were deepened and the deeper parts filled up, there would, owing to the different specific gravity of the transported soil and the displaced water, be a disturbance in the equilibrium of the crust, and a consequent change in the position of its axis of rotation.

Now if all this be true of a sphere, it will also, subject to certain modifications, be true of a spheroid so slightly oblate as our globe.

The main difference in the two cases is, that in a sphere the crust may assume any position upon the nucleus without any alteration in its structure, while in the case of the movement of a spheroidal crust over a similar spheroidal nucleus, every portion of its internal structure must be more or less disturbed, as the curvature at each point will be slightly altered.

The extent of the resistance to an alteration of position arising from this cause will depend upon the oblateness of the spheroid and the thickness and rigidity of the crust; while the thicker the latter is, the less also will be the proportionate effect of such elevations, subsidences, and denudations as those with which we are acquainted. The question of friction upon the nucleus is also one that would have to be considered, as the internal matter though fluid might be viscous.

It will of course be borne in mind that the elevations and depressions of the surface of the globe are not, on the theory now under consideration, regarded according to the proportion they bear to the earth's radius, but according to their relation to the thickness of the earth's crust; and that, even assuming Mr. Hopkins's extreme estimate to be true, yet elevations or depressions, such as we know to have taken place, of 8000 or 10,000 feet, bear an appreciable ratio to the 800 or 1000 miles which he assigns as the thickness of the earth's crust.

It is, however, to be remarked that the extremely ingenious speculations of Mr. Hopkins are based on the phenomena of precession and nutation, and that if once the possibility of a change in the position of the axis of rotation of the earth's crust be admitted, it is not improbable that the value of some of the data upon which the calculations of these movements are founded may be affected.

The supposition of the thickness of the crust being so great seems also not only entirely at variance with observed facts as to the increase of heat on descending beneath the surface of the earth, but to have been felt by Mr. Hopkins himself to offer such obstacles to any communication between the surface of the globe and its interior, that he has had recourse to an hypothesis of large spaces in the crust at no great depth from the surface, and filled with easily-fusible materials, in order to account for volcanic and other phenomena.

But though it may be possible to account for volcanoes upon such an assumption, yet, as already observed, the phenomena of elevation and depression, such as we find to have taken place, and more especially the existence of vast geological faults, cannot without enormous difficulty be reconciled with such a theory.

Taking the increment of heat as 1° Fahrenheit for every 55 or 60 feet* in descent, a temperature of 2400° Fahr. would be reached at about 25 miles, sufficient to keep in fusion such rocks as basalt, greenstone, and porphyry; and such a thickness appears much more consistent with the fluctuations in level, and the internal contortions and fractures of the crust which are everywhere to be observed. Sir William Armstrong, on the assumption of the temperature of subterranean fusion being 3000° Fahr., considers that the thickness of the film which separates us from the fiery ocean beneath would be about 34 miles.

Even assuming a thickness of 50 miles, so as to make still greater allowance for the increased difficulty of fusion under heavy pressure, the thickness of the crust would only form one-eightieth part of the radius of the earth; or if we represent the earth by a globe 13 feet in diameter, the crust would be one inch in thickness, while the difference between the polar and equatorial diameters would be half an inch.

In such a case, the elevation or wearing away of continents such as are at present in existence, rising, as some of them do, nearly a quarter of a mile on an average above the mean sea-level, would cause a great disturbance in the equilibrium of the crust, sufficient

* Page, 'Advanced Text-book of Geology,' p. 30.

to overcome considerable resistance in its attempts to regain a state of equilibrium by a movement over its fluid nucleus.

Whether the thickness of the earth's crust was not in early geological times less than at present, so as to render it more susceptible of alterations in position—whether the spheroid of the fluid mineral nucleus corresponds in form with the spheroid of water which gives the general contour of the globe—whether or no there are elevations and depressions upon the nucleus corresponding to some extent with the configuration of the outer crust, and whether the motion of the crust upon it, besides effecting climatal changes, might not also lead to some elevations and depressions of the land, and produce some of the other phenomena mentioned by Sir Henry James, are questions which I will leave for others to discuss.

My object is simply to call attention to what appears to me the fact, that if, as there seems reason to suppose, our globe consists of a solid crust of no great thickness resting on a fluid nucleus, either with or without a solid central core, and if this crust, as there is abundant evidence to prove, is liable to great disturbances in its equilibrium, then it of necessity follows that changes take place in the position of the crust with regard to the nucleus, and an alteration in the position of the axis of rotation, so far as the surface of the earth is concerned, ensues.

Without in the slightest degree undervaluing other causes which may lead to climatal changes, I think that possibly we may have here a *vera causa* such as would account for extreme variations from a Tropical to an Arctic temperature at the same spot, in a simpler and more satisfactory manner than any other hypothesis.

The former existence of cold in what are now warm latitudes might, and probably did in part, arise from other causes than a change in the axis of rotation, but no other hypothesis can well account for the existence of traces of an almost tropical vegetation within the Arctic circle.

Of the former existence of such a vegetation, the evidence, though strong, is not conclusive. But if the fossil plants of Melville Island, in lat. 75° N.*, which appear to agree generically with those from the English coal-measures, really grew upon the spot where they were now discovered, they seem to afford conclusive evidence of a change in the position of the pole since the period at which they grew, as such vegetation must be considered impossible in so high a latitude.

The corals and Orthoceratites from Griffiths Island and Cornwallis Island, and the liassic Ammonites from Point Wilkie, Prince Patrick's Island, tell the same story of the former existence of something like a subtropical climate at places at present well within the Arctic circle.

To use the words of the Rev. Samuel Haughton†, in describing the fossils collected by Sir F. L. M'Clintock, "The discovery of such fossils *in situ*, in 76° N. latitude, is calculated to throw considerable doubt upon the theories of climate, which would account for all past changes of temperature by changes in the relative position

* Lyell, 'Principles of Geology,' 1853, p. 88.

† Journal of the Royal Dublin Society, vol. i. p. 244.

of land and water on the earth's surface;" and I think that all geologists will agree with this remark, and feel that if the possibility of a change in the position of the axis of rotation of the crust of the earth were once admitted, it would smooth over many difficulties they now encounter.

That some such change is indeed taking place at the present moment may not unreasonably be inferred from the observations of the Astronomer Royal, who, in his Report to the Board of Visitors for 1861, makes use of the following language, though "only for the sake of embodying his description of the observed facts," as he refers the discrepancies noticed to "some peculiarity of the instrument. . . . . The Transit Circle and Collimators still present those appearances of agreement between themselves and of change with respect to the stars which seem explicable only on one of two suppositions—that the ground itself shifts with respect to the general Earth, or that the Axis of Rotation changes its position."

GEOLOGICAL SOCIETY.

[Continued from p. 482.]

April 25, 1866.—Warington W. Smyth, Esq., President, in the Chair.

The following communications were read:—

1. "Additional documents relating to the Volcanic Eruptions at the Kaimeni Islands." By Commander Brine, of H.M.S. 'Racer.'

In these documents it was stated that the active volcano now forming part of Neo Kaimeni Island continues to increase in size by the addition of volcanic matter ejected from the crater, and that the rate of increase of the new island situated to the south-west, near St. George's Bay, is considerably less than at first. The new island contains the crater of a second volcano, 30 feet in height, with a circular base of 300 yards; and, judging from the soundings obtained at Paleo Kaimeni and St. George's Bay, it is probable that the island will eventually fill up the bay.

2. "Report to the Eparch of Santorino on the Eruptions at the Kaimeni Islands." By M. Fouqué.

Since the eruptions at Santorino earthquakes have become much less violent in the surrounding country, and the fears of the inhabitants have been unnecessarily great. A new fissure has been opened between George Island and Aphroessa; and lava and torrents of steam have issued from this vent, as well as much gas. The non-existence of a crater was considered by M. Fouqué to be due to the small quantity of ejected matter and the feebleness of the eruption. M. Ste.-Claire Deville has shown that there exists a certain relation between the degree of intensity of a volcano in action and the nature of the volatile elements ejected; and M. Fouqué has been enabled to establish the truth of this law. Thus, in an eruption of maximum intensity, the predominant volatile product is chloride of sodium, accompanied by the salts of soda and potash; an eruption of the second order gives hydrochloric acid and chloride of iron; in the third degree, sulphuric acid and salts of ammonia; and in the fourth or most feeble phase, steam only, with carbonic acid and combustible gases. The eruption at Neo Kaimeni has never exceeded the third degree of intensity; and when it excited the greatest alarm, it gave off only sulphuric acid, steam, and combustible gases.

3. "Remarks upon the Interval of Time which has passed between the formation of the Upper and Lower Valley-gravels of part of England and France; with notes on the character of the Holes bored in rocks by Mollusca." By A. Tylor, Esq., F.G.S.

The difficulties attending investigations into the relative ages of gravel-deposits having been stated, and a *résumé* given of the steps by which the opinions now current on the subject had been arrived at, Mr. Tylor proceeded to combat the view that the Upper and Lower Valley-gravels are separated from each other by a long interval of time. The conclusion that man had existed on the earth from so distant a date as is required by Mr. Prestwich's interpretation of the phenomena exhibited in the valleys of the Somme and other rivers was also considered untenable, and to prove that the theory requiring it is erroneous.

Accepting Mr. Godwin-Austen's theory of the Pleistocene age of the English Channel, the author inferred from it that the excavation of the transverse valleys of the south-east of England was similar to that of the valleys of Devonshire, which he considers to have been excavated in remote geological periods, and to have been filled with gravel prior to the period of the valley-gravels, at which time the valleys were re-excavated. He then brought forward evidence to show that, in the case of the small valley in which Kent's Hole (180 feet above the sea-level) is situated, the gravel has been swept away from the valley during an epoch immediately preceding the historic period, and without any appearance of great denudation of the older rocks, leaving what may be called High- and Low-level Valley-gravels on its slopes as remanié deposits; and in support of this view he mentioned the presence of human implements in these gravels, the existence of Pholas-perforations on the face of the rock in which are the two openings of Kent's Hole (showing that little weathering had taken place since), as well as the occurrence of a bed of red clay, or loess, 80 feet thick, and 220 feet at its base above the sea-level.

The age of the Kent's Hole Valley was identified with that of the Valley of the Somme, on account of the similar position of the gravels and of the raised beaches at the coast-line, as well as the similarity of levels and of the organic contents of the detritus in the two valleys.

In conclusion Mr. Tylor gave a note on the character of holes bored in rocks by Mollusca, with especial reference to the bored rocks at Kent's Hole and Marychurch, about 200 feet above the present sea-level, coming to the conclusion that they have probably been formed by *Pholas dactylus*.

May 9.—Warington W. Smyth, Esq., M.A., F.R.S., Pres., in the Chair.

The following communications were read:—

1. "On a new species of *Acanthodes* from the Coal-shales of Longton." By Sir Philip de M. Grey Egerton, Bart., M.P., F.R.S., F.G.S.

Owing to the kindness of Mr. Ward, of Longton, the author had been enabled to examine a considerable collection of specimens of the Acanthodean fishes of the North Staffordshire Coal-field. The specimens were all imperfect, the anterior parts of the fish being rarely preserved, and even when present being in a very mutilated condition; but Sir Philip Egerton had been able to determine the distinctness of at least one species, which he now described as *Acanthodes Wardi*. This species was far less bulky and more elongated than *A. Bronni* from the Saarbrück Coal-field; but it was not so slender as *A. gracilis* from the Permian beds of Klein Neudorf.

2. "A sketch of the Gravels and Drift of the Fenland." By Harry Seeley, Esq., F.G.S.

By the Fenland was understood the flat country west of the Chalk Hills of Norfolk, from Hunstanton to Cambridge, thence to Bedford, and northwards to Peterborough. Three kinds of Drift were described as occurring in this region—namely, Boulder-clay covering the high land, a coarse gravel which caps the hills, and the fine gravel of the plains. Mr. Seeley gave first a sketch of their distribution over the area under consideration, and then described some of their most important exposures, especially the sections at March, Barnwell, and Hunstanton. He also gave lists of the marine shells found at March, occurring between Boulder-clays, and those found at Hunstanton, which are of much later date; also of the bones and land and freshwater shells found at Barnwell, including one bone described as having been cut by man previous to deposition in the gravel.

Comparing the drift of the Fenland with that of the Eastern Counties, Mr. Seeley inferred that the brown clay of the latter district corresponds with the brown boulder-clay, which is the oldest drift-deposit in the former, and that the hill-gravel, the blue boulder-clay, and perhaps the shell-bed of March, correspond to the Contorted Drift.

3. "Additional Observations on the Geology of the Lake-country." By Prof. R. Harkness, F.R.S., F.G.S., and H. Nicholson, Esq. With a Note on the Trilobites; by J. W. Salter, Esq., F.G.S.

The authors having first communicated the following additions to the fauna of the Skiddaw slates—namely, from the lower strata, *Phacops Nicholsoni*, n. sp., *Æglina binodosa*, and *Lingula brevis*; and from the upper beds, *Diplograpsus teretiusculus* and *Agnostis morea*—they stated that fossiliferous rocks had been discovered by them among the "ash-beds" of the Lake-country on the same horizon as those associated with the purely igneous rocks of the eastern parts of Cumberland and Westmoreland, which underlie the Coniston Limestone, and are of Caradoc age. This discovery has thus placed the green rocks of the Lake-country in the same position.

The Caradoc formation of the Lake-country was stated to embrace three divisions—namely, the Coniston Flags and Grits, the Coniston Limestone, and the Igneous rocks and ash-beds; and the following organic remains were enumerated as having been obtained from the Coniston Flags and Grits, the uppermost division of the formation:—*Graptolithus Ludensis, Diplograpsus pristis, Phacops obtusicordatus, Orthis crispa, Cardiola interrupta, Orthoceras filosum, O. tenuistriatum*, and *O. subannulatum*.

4. "On the Lower Silurian Rocks of the Isle of Man." By Prof. R. Harkness, F.R.S., F.G.S., and H. Nicholson, Esq.

The older sedimentary deposits, which occupy the greater part of the island, have been regarded by previous observers as Lower Silurian. These slates were described by the authors as forming an anticlinal axis which traverses the island in a north-east and south-west direction, and to be conformably overlain at Douglas Head and Banks How on the south-eastern part of the island, by green ash-beds (slates and porphyries).

The only fossil of the slates is the *Palæochorda major* of the Skiddaw slates; and from the circumstance that the Lower Silurian rocks of the Isle of Man are in the exact line of strike of the Skiddaw slates of the Lake-country, the authors regarded these beds as corresponding with them; and the "green ash-beds" were considered to be the equivalents of the ash-beds and porphyries which succeed the Skiddaw slates.

LXXIX. *Intelligence and Miscellaneous Articles.*

ON THE LAW OF THE UNION OF SIMPLE SUBSTANCES, AND ON ATTRACTIONS AT SMALL DISTANCES. BY MM. ATHANASE AND PAUL DUPRÉ.

THE use of weighings in measuring work and molecular forces leads to a precision which had not been hitherto attained. It has become possible to commence the study of attractions at small distances; and there is reason to hope for an early and considerable progress in those branches of the physical sciences which are more directly connected with molecular mechanics.

When two bodies, terminated by plane parallel faces, approach till they are in contact, a work is effected proportional to the surface, and independent of the thickness, provided this exceeds the radius ϵ of the sphere of sensible attraction. We have shown that the same number represents this work, the force of union, and the force of contraction of the superficial layer; the apparatus for determining it have been described (memoir addressed to the Academy in October 1865, and *Annales de Chimie et de Physique*, February, March, and April 1866). If reduced by calculation to the unit of surface and unit of specific gravity, the result obtained for each body becomes comparable with analogous numbers. The two plates which unite may moreover be of the same nature, or may have a different chemical composition; it is convenient to represent the force of union by a symbol f accompanied by a chemical indication of the bodies in question; thus f^{Hg}_{Hg} represents the force of the union of mercury with itself after reduction to unity, and f^{O}_{H} that of hydrogen with oxygen. By the aid of this notation the force of union of a compound body may be calculated as a function of the union of elements which it is often impossible to obtain in a direct manner. By equating the values found by experiment, an equation is obtained which contains in fact several unknown quantities; but other compounds containing the same simple substances in different proportions furnish new equations; and provided their number is sufficiently great, not only the numbers sought, but also very valuable verifications are obtained. This research, which will be communicated completely to the Academy when finished, has already given a most important law.

The forces of union of simple substances reduced to the unit of surface and of specific gravity are inversely proportional to their equivalents.

For hydrogen we have obtained 27 milligrammes per millimetre, and that by four methods:

(1) By means of mercury.
(2) By means of bromine.
(3) By means of oil of turpentine and benzole.
(4) By means of water, wood-spirit, and benzole.

But we only propose this figure as the result of a primary study of the fundamental number; it will probably undergo some corrections after the experiments have been renewed, and the products completely purified. The experiments to be made are long and delicate; great precautions must be taken to avoid serious errors.

The facts already confirmed refer to definite integrals; hence it is probable, though not quite certain, that

At the same very small distance, and with an equal specific gravity, two elements of volume of a simple substance exert on each other an attraction the value of which must be simply multiplied by the ratio of the equivalents if it is to be applied to another simple substance.

This law of attraction evidently reproduces the experimental law of the forces of union. It leads directly to several other laws which we shall examine experimentally, of which the following are the enunciations:—

In simple substances the attractions on contact, reduced to the unit of surface and of specific gravity, are inversely as the equivalents.

In simple substances the ratio of attraction on contact to the force of union is—

(1) Independent of the specific gravity.
(2) Independent of the chemical nature.
(3) Independent of the molecular grouping.

For all simple substances taken in the fluid state, a constant product is obtained when the following four numbers, referred to the same temperature and pressure, are multiplied together:—

(1) The chemical equivalent.
(2) The coefficient of expansion for constant pressure.
(3) The inverse of the coefficient of compressibility.
(4) The inverse square of the density.

The attractions on contact of two different simple substances acting one upon another, and therefore of any two compounds, are obtained by calculations resembling those we have used for the forces of union; the attractions of very close-lying parts enter into it evidently in great measure.

When a simple substance unites with another element, the force of union is not always attractive; and thus we have an explanation of facts hitherto difficult to understand—for instance, the heat produced by the decomposition of binoxide of nitrogen. The atoms of oxygen and those of nitrogen attract at distances greater than e with a force (universal gravity) which is only appreciable for large masses. At distances less than a certain quantity e', very small as compared with e, they still attract; but in the interval $e\,e'$ they repel one another, and the work of chemical union consists of one part negative and one part positive which is less. They do not combine directly, because they repel one another; but when by any means they are made to traverse the interval $e\,e'$, they may remain united. These latter views are partly hypothetical; hence we shall not fail to submit them to all the verifications of which they are capable. The principal difficulties arise from the circumstance that chemical products must be used which are very pure, and capable of furnishing the best determinations. With ternary compounds, if the choice is not good, the errors to be feared accumulate very rapidly in the calculation.

We have shown, by the known values of the forces of union and attraction on contact, that the general law of attraction would not be expressed by several terms obtained by multiplying whole powers of the inverse of the distance by constant coefficients. It would appear to be represented by the sum of three functions of the distance, of which the first (that is, the astronomical function) would predominate completely at great distances, and could be entirely neglected at small ones. The second, which might be called the physical function, would predominate in the interval $e\,e'$; it would almost completely determine the force of union, and would be common to all simple substances, provided it were preceded by the inverse of the equivalent as a factor. The third, finally (that is, the chemical function), would predominate in turn from zero to e'.—*Comptes Rendus*, April 2, 1866.

ON A NEW METHOD OF MEASURING THE LENGTHS OF LUMINOUS WAVES. BY PROF. STEFAN.

If light be allowed to fall on a column of quartz with polished faces parallel to the optic axis, each ray is resolved into the ordinary and the extraordinary ray, if the faces of entrance and of emergence are parallel. If both are again brought into a common direction of vibration, all those rays are extinguished the difference of whose path amounts to an uneven number of semi-wavelengths. If the spectrum of the light is formed, dark interference-bands appear, which are the more numerous and the finer the thicker the quartz. The difference of phase between two rays may be calculated from the thickness of the quartz, and from the quotients of refraction,—and with great accuracy, since only the differences, and not the absolute values of the latter are required. Twice the difference of phase divided by the wave-length is an uneven number for each dark band, and for each succeeding one towards violet is two units

greater. From the number of bands from one Fraunhofer's line to another, the wave-length of the latter may be calculated when that of the first is known.

To determine a wave-length directly, independent of another, the difference of phase for the place of the spectrum in question must be successively increased or diminished. Therefore a removal of the interference-bands sets in. From the number of the bands which have passed through the cross wires, and the change of the difference of phase thus produced, the wave-length can be calculated for the place fixed upon. The successive change of the difference of phase could be obtained by pushing over each other two quartz wedges. Such an apparatus was not at hand, and the following method was therefore used. The column of quartz was turned slowly out of its position at right angles to the incident rays; the angle of incidence, and therefore also the difference of phase between the ordinary and extraordinary ray successively increased, and the bands which simultaneously passed through the cross wire were counted. As the change in the difference of phase which occurs with the measured alteration of the incident angle can be calculated, these are the data needed for the absolute determination of the wave-length of the place fixed upon.

For the wave-lengths of Fraunhofer's lines B, C, D, E, F, G, H, the following numbers were found, in ten-millionths of a millimetre: 6873, 6578, 5893, 5271, 4869, 4291, 3959. These values agree very accurately with those deduced from the diffraction phenomena of fine gratings, and are therefore at the same time a proof of the accuracy of our theory of the diffraction of light.—*Berichte der Wiener Akademie*, April 26, 1866.

ON THE INFLUENCE OF INTERNAL FRICTION IN THE AIR ON THE MOTION OF SOUND. BY PROF. STEFAN.

The results of the analytical investigation are as follows. Friction increases the velocity of sound, and to a greater extent the higher the tone. Yet even for the highest tones this increase is very small, about 0·001 millim. in a second.

The amplitudes decrease in plane-progressive waves in geometrical progression. The exponent of this progression increases with the height of the tone, and, indeed, proportionally to the square of the number of vibrations. The diminution of amplitude is only perceptible in high tones. With a tone of 10,000 vibrations the amplitude is diminished by $\frac{1}{8}$ at a distance of 1000 metres; at 2000 metres by $\frac{1}{67}$: with a tone of 30,000 vibrations by $\frac{1}{8}$, even at 100 metres.

Standing vibrations are only possible if the length of the wave exceeds a certain value. Yet this is very small, equal to four times the mean way which, according to the new theory of gases, a molecule makes from one impact to the next.

In a standing wave also the amplitudes decrease with the time in geometrical progression, whose exponent is proportional to the square of the number of vibrations. The amplitudes of the tones of 1000, 10,000, and 30,000 vibrations sink to one-half before the lapse of 100 seconds, 1, and 0·1 second respectively.—*Berichte der Wiener Akademie*, April 16, 1866.

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END OF THE THIRTY-FIRST VOLUME.

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WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua ..... Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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NUMBER CCXII.—SUPPLEMENT.

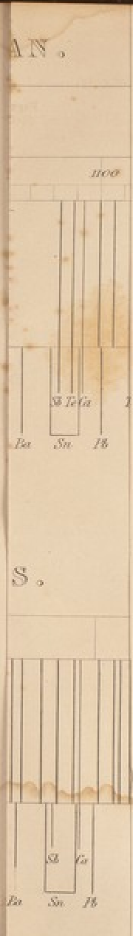
PLATES.

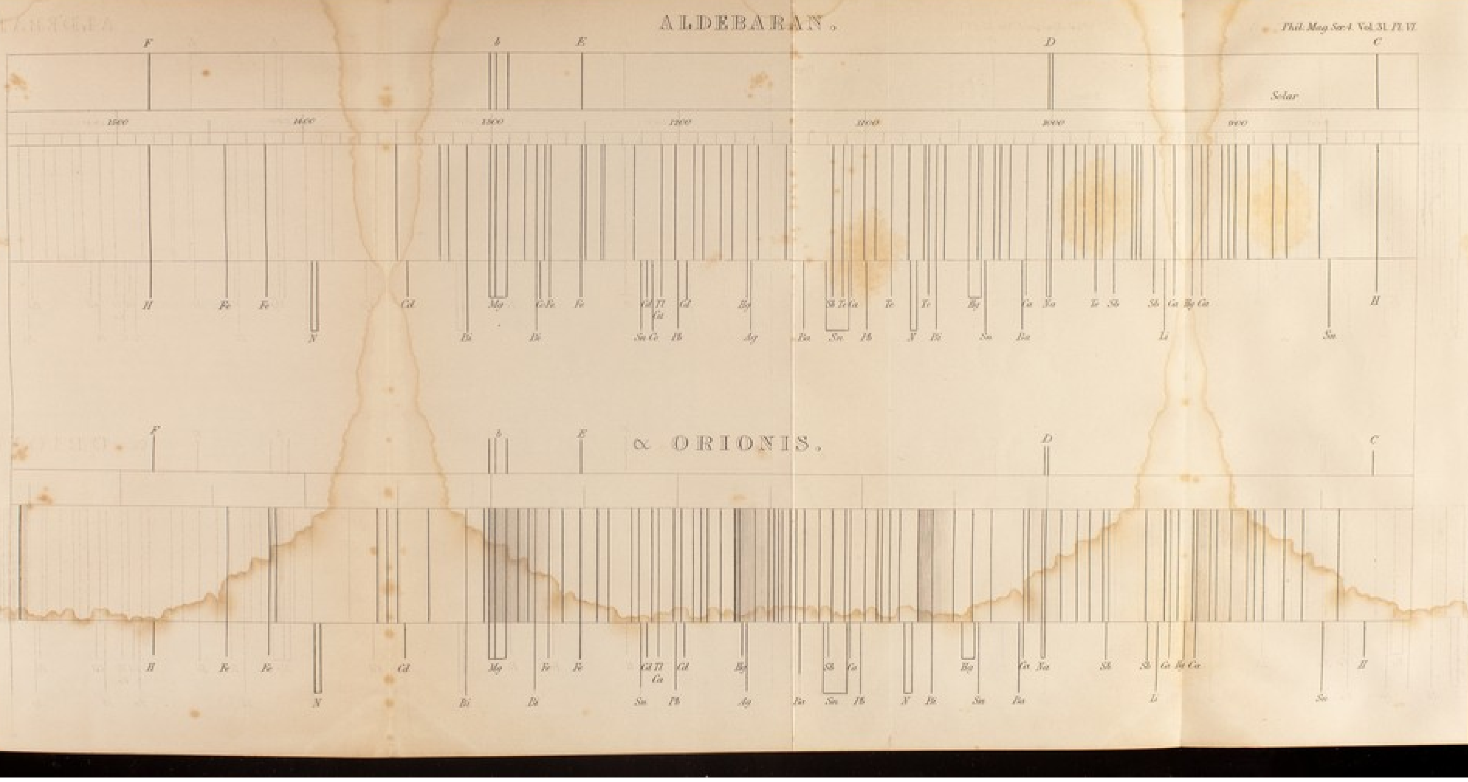
ERRATA.

Vol. 30. Page 410, lines 30 and 31, transpose the words "common oxygen" and "ozone."

" — 439, line 12, *for* $\left(\frac{2500}{2499}\right)$ *read* $\left(\frac{2500}{2499}\right)^{60}$.

Vol. 31. — 305, line 2 from bottom, *for* simple proportion to the latitude *read* simple proportion to the sine of the latitude.

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LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1866.

XXII. *Reply to* Dr. Werner Siemens's *Paper "On the Question of the Unit of Electrical Resistance."* *By* FLEEMING JENKIN, *F.R.S.**

IN the Philosophical Magazine for May, there appeared a translation of a paper "On the Unit of Electrical Resistance" by Dr. Werner Siemens, containing views much at variance with the conclusions of the British-Association Electrical-Standard Committee, and referring frequently to the part I have taken in supporting those conclusions. Dr. Siemens enjoys so high a reputation, that I am anxious to answer his arguments directly, instead of simply referring, as I might do, to the Reports of the Committee, where most of the points have, I think, been already met.

Dr. Siemens has treated of several very distinct questions, which might be divided as follows:—

(1) What is the best unit of electrical resistance?

(2) What is the best method of making and reproducing any unit?

(3) Have Dr. Siemens's proposals and labours met with fair consideration and full acknowledgment by the Committee and by myself?

As to the first point, Dr. Siemens supports as unit the resistance of a prism of mercury 1 metre long and 1 square millimetre section at 0° C., or a million times the resistance of a cubic metre of mercury at 0°.

The British-Association Committee propose as unit ten million times the absolute electromagnetic unit, or metre per second, as

* Communicated by the Author.

determined by a Subcommittee consisting of Professor Thomson, Professor Maxwell, Mr. Balfour Stewart, and myself.

The arguments in favour of one or the other of these units should be kept quite distinct from those in favour of the best materials for the reproduction or manufacture of material standards. If mercury be the best material for a permanent standard, any unit can be made of mercury. If mercury be the best material for reproducing a standard, then when the specific resistance of pure mercury in any unit has been determined, mercury can be used for the reproduction of that unit. Conversely, if Dr. Siemens's be the best definition for a unit of resistance upon some independent grounds, then the fact that mercury was a bad substance for constructing or reproducing the unit might not even weaken the arguments in favour of his definition.

Now, apart from the question of manufacture and reproduction, why should a mercury unit be chosen? I see no arguments in Dr. Siemens's paper in favour of his own definition, except perhaps the statement that "mercury is the conductor which is without doubt the best suited to serve as unit of conducting-power."

Dr. Siemens here separates conducting-power from resistance, and seems to think that the specific conducting-powers of materials should be referred to some one standard material, and that the unit of conducting-power may be distinct from the unit of resistance. But conducting-power is naturally defined in calculations as the reciprocal of resistance, the unit of resistance and of conducting-power being the same; there is therefore no need whatever to select any substance as a unit of conducting-power, which would indeed simply introduce confusion into mathematical expressions of electrical relations. The Committee have therefore adopted Professor Thomson's definition of the specific conducting-power of metals as the conducting-power of the wire of unit length and weight (being the reciprocal of the resistance of the same wire). In this way the numbers expressing the specific properties are rendered independent of comparison with any material whatever, whereas, on the other plan, we should always have to ask on whose determination of mercury, silver, or gold &c. the values given were based. Moreover Professor Thomson's plan is infinitely more convenient in calculating from the specific values the actual resistance or conducting-power of any given wire. These considerations absolve me from the necessity of even considering whether, if a unit-material must be selected, mercury would be the best—a question which I nevertheless think might not be decided affirmatively.

Dr. Siemens also claims practical advantages, but does not state their nature. Mercury is little used in telegraphy as compared with copper, German silver, or iron.

A metre long and square millimetre section has a symmetrical sound; but, except in calculating the resistance of prisms of mercury, I know of no practical advantage which it has over any other magnitude. A metre of pure copper weighing a gramme would be more practically useful, for it would serve as an immediate term of comparison with the copper supplied for conducting-wires. A mile of iron wire, of the size generally used in telegraphy, would have a practical advantage in finding the distance of faults. I could give other definitions with practical advantages, but do not know of any practical advantage which has resulted, or could result, from a definition in terms of mercury.

Then even in a mercury unit, no reason is assigned for preferring the particular definition given to any other, such as a metre of mercury, weighing a gramme, or contained in a tube of say 1 millimetre diameter. I do not ask these questions as contending that these definitions are better than Dr. Siemens's, but simply to point out the completely arbitrary nature of his definition.

I have heard in favour of that definition two arguments which, so far as they go, are really in its favour. First, that it was coming into extensive use when the Standard Committee was appointed; secondly, that it was at least an intelligible definition. The first argument is based on truth, though the use of Dr. Siemens's unit was by no means so general as some of his friends suppose. No large English telegraph company used it, nor has it, I believe, been at all adopted in France. Nevertheless I admit that, owing to the undoubtedly excellent manufacture of Messrs. Siemens's resistance-coils, and the convenient arrangements of those coils, his unit was largely used; but this extended use was not, I think, caused by the excellence of his definition. People ordered coils from the most celebrated firm in Europe and took what was given them—the miles of copper wire before 1860, and the mercury units afterwards.

This argument, however, was fully considered by the Committee, but was overthrown by the considerations that changes were yearly being made in the coils supplied, that the definition, being wholly arbitrary and having no intrinsic merit, could not be compared in value with an approximation to the natural unit adopted, and that the use was by no means so general as to outweigh the two previous arguments.

I next come to the question of intelligibility. No doubt most people think they know what a metre of mercury of 1 millimetre section means, and comparatively few understand the definition adopted by the Committee. But who in practical life, or in the use of standards, refers to their definition? What Frenchman, measuring the contents of a brick wall, thinks of the earth's diameter? what Englishman, using a foot, thinks of pendulums? For practical use the material standard, not the definition, is the important point.

Further than this, the apparently simple definition might lead to gross errors. It requires all Dr. Siemens's skill to produce a mercury unit, if even he can do it. The attempt on the part of any ordinary optician to produce a mercury unit would result in gross discrepancies, which unfortunately might not be discovered until coils had been in use for years. The Committee, therefore, did not think the difficulty of explaining the definition of their unit of any serious importance.

Hitherto I have endeavoured to show that there is no reason to adopt Dr. Siemens's definition more than any other; I have now to show why any other definition should have been preferred to Dr. Siemens's by the Committee.

First to be considered were the practical units—the foot of copper wire, or the mile of iron wire, &c. One objection to these is, that to make them exact is to make them unpractical. Pure metals or specific alloys must have been adopted, and in practice these are never found. Practically no two feet of copper wire or miles of iron wire are alike. There are other arguments on this point which will be found in previous Reports.

I now come to the definition adopted. The reason of this adoption was that the absolute or natural units are those which must necessarily be used in all mathematical calculations of the relations between currents and magnets, according to the existing system of the measurement of magnets. They are, further, most convenient in expressing the relations between all electrical magnitudes themselves. How this is done I have already explained very fully in the Reports which, as Secretary, I had the honour to write for the Committee, and which I am happy to find meet with some approval by Dr. Siemens. Weber, Thomson, and Clerk Maxwell's writings are full of examples: the absolute unit is as natural an expression of electrical resistance as the cubic metre is of capacity when the lineal metre is the unit of length. It is the unit which necessarily expresses resistance, just as the cube necessarily expresses capacity. It would as certainly have been used in all scientific investigations (even if Dr. Siemens's unit had been in universal practical use) as the cubic metre or cubic foot are used in calculation even where the practical measures in use may be the chopin or the gallon. The Committee, therefore, considered that they had no choice in the matter so far as definition was concerned. On the one hand there was a number of arbitrary definitions, on the other the natural unit inevitably used in calculations: could their decision be doubtful?

So far I have argued the matter simply on the ground of definition. Dr. Siemens says that "the resistance-unit must consist of a definition, or be an absolute measure, which can be at any time and in any place reconstructed." I think I am right in understanding these words to mean that Dr. Siemens prefers a mere definition to any material standard whatever, even his own; and I am borne out in this view by his remark, that future and more complete determinations of his unit would become necessary as the exactness of physical measurements progresses; though he anticipates that no practical inconvenience could result from these corrections, as they would be very small. The Committee were of a diametrically opposite opinion: they thought that the one essential quality of a standard was its invariability, that no possible inconvenience would be equal to the continual variation of the standard, making, as it were, the 12 inches of today the 13 inches of tomorrow. No doubt we all believe that the corrections would not be so great or so frequent as this; but what man of science could recommend a periodical tampering with the lengths of the standard metre to bring it more nearly in accordance with some abstract definition? Such changes would on each occasion render almost useless an inconceivable mass of scientific labour. For instance, all Dr. Matthiessen's specific resistances of metals, recorded in Siemens's units, would, since their first issue, have required two or three revisions, and would even now be far from fixed. If, therefore, we can make a permanent unit, I think any permanent unit will be better than any definition; but if a definition alone is to be adhered to, the definition adopted by the Committee is much preferable to that of Dr. Siemens.

Dr. Siemens acknowledges with perfect candour the scientific importance of the determination of Weber's dynamic unit, but thinks that, as the determination of resistances combined with dynamic values rarely occurs, its general use is not of much practical importance. But although the combination of resistances with dynamic values rarely occurs in practice, the combination of resistance with the measurement of quantity, capacity, and currents is of daily occurrence. The unit proposed by Dr. Siemens stood alone and unconnected with any of those measurements. The Committee have adopted a coherent system, in which the current will be equal to the electromotive force divided by the resistance, the quantity to the quantity conveyed by the unit of current in the second, and the capacity to the capacity which, when electrified by the unit electromotive force, will contain the real quantity. Standards for the determination or measurement of all these magnitudes will be issued by the Committee. Dr. Siemens himself very fairly admits that in the British Association Reports the argument in favour of this coherent system of measurement is very convincingly put forward. The mercury stands alone an arbitrary definition, while every one of the Committee's units will be an approximation to the natural or mathematical unit. Now this relation between the several units is of practical convenience in the daily testing of submarine cables, and in all other electrical measurements.

But Dr. Siemens says, *first*, your so-called permanent unit does not represent your definition even with reasonable accuracy; and *secondly*, it will not be permanent.

With reference to the first point, Dr. Siemens has fairly pointed out the great difficulties of the determinations; but I think he also admits that the names of those who actually conducted the experiments are some guarantee that care would be taken, and that the names of the Committee at large are a further guarantee that the results would not be adopted without a strong conviction that they were trustworthy. But even if they were not, which I entirely disbelieve, the B.A. unit would not be at all more arbitrary than Dr. Siemens's unit, although I allow there would then be no reason for its adoption except its permanency.

Will it then be permanent? Time alone will show whether any of the ten material standards will remain perfectly permanent; but we have at least as good a guarantee for their permanence as Dr. Siemens has for his.

If his 1864 issue were correct, and I heartily wish we could be sure of this, the B.A. unit would be equal to 1·0486 metre of mercury at 0° C. of 1 square millimetre section; and this resistance can be obtained just as easily as the resistance of one metre. If, therefore, all our material standards vary, we are no worse off than Dr. Siemens. But we have what, while the present discrepancies in mercury-determinations exist, I venture to think is an extremely important addition to our security, in the probability of the permanence of the standards already made. We have all Dr. Siemens has, and a great deal more.

Every additional metal of which the specific resistance is accurately determined gives a fresh guarantee; and it is possible that some day this method of ensuring permanence may be more important than the mere conservation of material standards. As yet I think this is not so.

To sum up the arguments hitherto used.

The mercury unit as defined is arbitrary, varying, has no practical advantage, and forms part of no coherent system of general electrical measurements.

The British-Association unit is based on the natural mathematical unit, is permanent, and has the practical advantage of forming part of a coherent system for other electrical measurements.

Hitherto I have argued wholly on definitions, keeping in reserve all the questions as to how far Dr. Siemens had been successful in producing or reproducing his unit. For instance, when above I say that the mercury unit is varying, I mean that Dr. Siemens intends that it shall vary as better and better determinations are made; and when I say that the B.A. unit is permanent, I mean that the Committee intend it to be permanent. I will now consider how far some of the proposals for the manufacture have been, and are likely to be, practically successful. In arguing this point I am quite willing to abandon all the discrepancies hitherto pointed out in the units supplied by Dr. Siemens. The important question undoubtedly is, not whether Dr. Siemens has from first to last supplied faultless mercury units, but whether any unit can be reproduced by a given length and section or weight of mercury, or any other material. I think I have shown that there is no reason to adopt a metre of mercury of 1 millimetre section as unit of resistance; but mercury may nevertheless be the best substance of which to make units, or by which to reproduce them. I am at a loss to conceive how the two subjects can have been confused as they have been; if platinum be the best metal for a standard weight, we are not therefore obliged to take a cubic metre of platinum as the unit weight.

It is undoubtedly of great importance to have some means of reproducing a unit, in case the original be lost or altered; and Dr. Siemens's experiments are exceedingly valuable, although they can hardly yet be said to be conclusive. Dr. Siemens believes that he can reproduce his unit (and therefore the B.A. unit) at least to within 0·1 per cent. of accuracy, and, when the greatest possible care is used, probably with an almost unlimited exactness. It would, I am sure, give every member of the Committee great pleasure if this fact could be fully established; but it is not yet established. I admit that the discrepancies between the coils exhibited in 1862 have little to do with this point; although I was informed, as I now believe mistakenly, by Dr. Esselbach, that the difference was due to a change in Dr. Siemens's standard. Mr. C. W. Siemens himself at the Royal Society stated that inconvenience had been caused by a premature issue of coils based on an imperfect standard; but after Dr. Siemens's statements, I can feel no doubt that no material alteration has knowingly occurred in this standard. Moreover I admit that errors in early determinations would only show that the reproduction was not very easy, as no one now contends that it is. I further allow that commercial coils cannot be accepted as standards; also that an error in the specific gravity of mercury in no way affects this part of the question, also that an error in the coefficient of correction for the German-silver copies does not affect the argument; but I do maintain that when Dr. Siemens and Dr. Matthiessen obtain discrepant values after taking all possible care, we must be allowed to suspend our judgment as to which of the two have obtained the value which is most near to truth. According to Dr. Siemens's paper, making a correction in his 1864 standards, as he desires, of 0·287 per cent., the B.A. unit = 1·0486 millimetre of mercury of 1 millimetre section at 0° C. According to Dr. Matthiessen the value is 1·0396. These values are too different to allow us yet to use mercury as a certain means of reproduction.

As soon as one or more independent observers shall obtain really concordant results, they will be accepted by all as a means of checking the permanency of the material standards already made. Till then, I believe the best check will be found in the comparison of the standards themselves, which can be measured with an accuracy approximating at least to within 0·01 per cent., and are correct within this amount, although, owing to a misunderstanding and possible ambiguity of the language employed, Dr. Siemens believed they were not even intended to be so.

I shall not enter into the controversy between Dr. Matthiessen on the one hand, and Mr. Sabine and Dr. Siemens on the other*. Dr. Matthiessen can take good care of himself, and his reputation is too high to allow us simply to accept Dr. Siemens's results, differing as they do from those of the only other observer who has made researches with similar objects in a distinct laboratory.

Dr. Matthiessen places greater reliance on reproductions by

* I should, however, be glad to have an explanation upon one point. Dr. Siemens gives a Table showing that the determinations made with two tubes in 1859, and four tubes in 1860 and 1863, differed less than 0·1 per cent., and the most concordant results were obtained in the latest measurements. I venture to point out that, as I understand this statement, it does not prove that the determinations in 1859, 1860, and 1863 were concordant and gave the same unit. It shows that the relative measurements of the several tubes on the three occasions were correctly made within 0·1 per cent., and the relative resistances agreed with these measurements. But this result would have been obtained, however different the mercury might have been on the three occasions, provided of course the three tubes during any comparison were filled from one and the same source. But if by any chance impure mercury was used in 1859 and pure mercury in 1860, the unit as determined by the apparently concordant observations would be very different, and the difference could only be detected by comparison with a material standard assumed as permanent. I should be glad to know whether this comparison was made or not; and if made, with what result. Unless some such comparison has been made, we have no guarantee whatever that the results of the three determinations were really concordant. I do not find any mention of this comparison in Mr. Sabine's or Dr. Siemens's papers; but I may have overlooked it.

lead, or by a gold-silver alloy, than on reproductions by mercury; but until Dr. Siemens, or some other competent observer, also obtains thoroughly concordant results with these materials, I can place no reliance on any of these means of reproduction.

Yet it cannot be denied that the ten so-called permanent standards *may* all vary, and that unless some means of reproduction be discovered before that variation takes place, the unit may be lost. It would therefore be of immense importance to be able to say with certainty that the unit is equal to a certain length and weight of mercury, or lead, or platinum, or any other material; only I protest that if lead, platinum, or mercury be found the best material for this purpose, I will not therefore take the unit length and weight or section of that material as the unit of resistance.

It is this fallacy which I have always endeavoured to expose. No one in the Committee has ever underrated the value of a chemical means of reproduction; on the contrary, money has been freely voted, and committees yearly appointed, having this sole object. Dr. Matthiessen has been opposed to mercury, and to support his views has argued that at any rate hitherto the value of the mercury unit remained unfixed—a perfectly justifiable argument, supported by discrepancies in coils issued, though certainly not conclusive against the possibility of using mercury hereafter. But until this question is settled, the Committee have thought that material standards of very different materials, which remained unaltered in their equality, afforded a fair guarantee of permanency. Should they not remain equal, they can fall back on the length and weight of fresh mercury, lead, or gold-silver alloy which have been or may be found equal to the B.A. unit. Thus, according to Dr. Matthiessen, the ten standards are equal to wires or prisms weighing 1 gramme per metre, and of the following lengths for the various materials:—

Lead	0·44307 metre.
Gold-silver alloy . .	0·59952 ,,
Mercury	0·076505 ,,

We have therefore, as before said, all the guarantees Dr. Matthiessen can have for permanency, with the addition of ten material standards.

Thus, to resume, the reproduction of a standard, whatever that may be, by mercury or any other metal is recognized by the Committee as of great importance, both as an additional guarantee of permanency, and in case of accidental injury; but hitherto they recognize no means of reproduction by mercury or otherwise as established with such authority as to justify its formal adoption, and they see no reason to adopt a unit length and weight or section of any reproducing material as the unit of electrical resistance.

I now come to the third and personal point, and I enter upon it with great regret. I feel that I must have unconsciously written in a manner calculated to give annoyance, or Dr. Siemens could never have accused me of attacking his propositions "in a way not customary in scientific critiques." I am the more pained by this, as although I have only a slight acquaintance with Dr. Siemens, I may acknowledge with pleasure the friendship of his brother, Mr. C. W. Siemens of London. Certainly, though I have had occasion to differ from Dr. Siemens, and have urged my arguments as forcibly as I could, I never supposed that he or any one would have suspected me of "an intention to throw a false colouring upon the value of his work;" indeed I hardly know in what terms I ought to answer such an imputation.

Meanwhile I prefer to believe that Dr. Siemens will express regret at having in the warmth of controversy made such an accusation. I myself regret that the Report, which as Secretary I had the honour to write and present in 1865, had not been printed before Dr. Siemens wrote his article. The following extract will show the feeling which animated the Committee and myself:—"Thus a difference exists in Dr. Siemens's and Dr. Matthiessen's reproduction of a unit by means of mercury, as pointed out in last year's Report. It is of course probable that differences of this kind will in time disappear, and Dr. Siemens fairly points out that the discrepancy mentioned in last year's Report, between coils made from a very old and those made from a new determination of the mercury unit, affords no criterion of the accuracy with which mercury can now be used as a means of reproduction. Dr. Siemens was the first person who produced numerous sets of coils accurately adjusted; and although unable to recommend the adoption of his unit of resistance, the Committee once more take an opportunity of expressing their sense of the high value of Dr. Siemens's researches on the reproduction of units by means of mercury."

Probably if Dr. Siemens had seen this Report, his article would have been in some respects different. He should remember that if many of our arguments have been directed against his proposition, there are two good reasons for this, inasmuch as his units alone have been able to challenge any comparison with those of the Committee, and he himself has hitherto alone opposed our proposals.

Dr. Siemens refers specially to my Report to the Royal Society on the New Unit of Electrical Resistance. He blames me for using a single determination published in my Report on the

Exhibition of 1862, to force into Tables columns headed "Siemens, Berlin" and "Siemens, London," throws doubt on the accuracy of my observation in 1862, and says that, even if I made no mistake, the coils were adjusted when the art of copying resistances was scarcely known.

First, as to the accuracy of my observation, I may state that the whole of my Report on Dr. Siemens's instruments was submitted in manuscript long before publication to his Firm, in order that any mistakes might be corrected. The discrepancy between the coils was specially pointed out by me verbally, with regret that it should have been found to exist. Messrs. Siemens very kindly sent me a number of valuable corrections, but were unable to explain to my satisfaction the discrepancy between the coils.

I received more than one verbal explanation of that discrepancy. Dr. Esselbach said the standard had been altered; another gentleman said the coils had been touched on their return from the Red Sea; and it was suggested they might have altered with time.

In Mr. Loeffler's written reply on behalf of the firm, a suggestion was made that I might have observed the coils at different temperatures. I pointed out that this would have required 45° Fahrenheit as the difference between the coils, and that repeated experiments were made, all with the same result. Messrs. Siemens had the coils returned to them long before the publication of the Report, and did not then deny that a discrepancy existed.

I confess that I believed that they had been made from different standards; nor do I now know the true cause of their difference. In my belief I was strengthened by Mr. Siemens's statement (when the report was read at the Royal Society) that much trouble had been caused by the premature issue of the coils. But I entirely abandon that opinion since I understand that Dr. Siemens states that the mercury standard has never been sensibly changed.

Dr. Siemens throws doubt on the accuracy of my observations by saying that the difference given by me between the sets of coils as 1·2 per cent. was really 1·8 per cent.; but on examination he will see that the difference reported between the two 1862 coils was really 1·2 per cent., though 1·8 per cent. is nearly the difference between one of those coils and the 1864 issue; errors of 0·1 per cent. certainly did not exist in the coils themselves, and therefore the discrepancy could not have resulted from the rudeness of the methods employed to adjust them.

Next, as to the time at which those coils were made, Messrs. Siemens in 1862 stated that both sets exhibited were made at nearly the same time; they were both extremely well adjusted, as I stated at that time in my Report; and I am not aware that since 1862 any material advance has been made in the adjustments.

I do not quite understand whether Dr. Siemens means to state that the coils called by me "Siemens, London," had been only roughly adjusted by the method of weighing in 1859 and then readjusted in 1862 before exhibition; but whatever be the case, the two sets were equally well adjusted when exhibited, and as good in themselves as any set of coils I have since seen. The charge therefore of unfairness in publishing observations on rude coils, made by an imperfect method, arises from some mistake of Dr. Siemens as to the condition in which the coils were when exhibited by the London firm. Moreover let me point out that in the Report referred to I give the values of coils adjusted by Mr. White of Glasgow in 1859, under Prof. Thomson's supervision, and also exhibited in 1862. These are, and were, in excellent adjustment, with the exception of one coil. They were subjected to the same treatment as to travelling and use as Messrs. Siemens's coils; and I will not do Messrs. Siemens the injustice to suppose that in 1859, when the coils exhibited were first made, they knew less of the art of copying resistances than we did in England.

This value of Prof. Thomson's old unit, based on the old 1859 coils, has been repeated by me in every Table published, although it tells against the absolute unit as a standard, about as much as Messrs. Siemens's old coil tells against the mercury unit. Similarly I have repeated Weber's various discrepant determinations.

To resume. No serious error could have occurred in my experiment without being corrected by Messrs. Siemens at the time, for which they had every opportunity. The coils were not rudely adjusted when exhibited, nor were they even originally made at the time when the art of copying resistances was scarcely known. No explanation of the difference which could be substantiated was given me, and I believed that the difference was probably due to bad reproduction; but I abandon that opinion since I understand Dr. Siemens to say that no change in the standard has been made. I have only repeated their value in Tables in which I repeated still worse discrepancies in so-called absolute determinations, and in which the value of coils as old as those of Dr. Siemens were also given.

Dr. Siemens has himself allowed that the standard in 1864 did not, even by his experiments, truly represent his definition within 0·287 per cent. Surely it was no unfair criticism to point this out; and this is the only point urged by me in the

1864 Report to the Royal Society: and even if it be conceded that the old discrepancies gave no measure of the accuracy of the plan of reproduction, they greatly weaken any argument founded on the priority of Dr. Siemens's unit, and its extended use. One reason for repeating these various values of all units, besides that of showing the discrepancies in those hitherto issued, is that results have been published expressed in each unit.

Dr. Siemens is in error in supposing that Dr. Matthiessen's calculation of the value of the 1862 mercury coils, as compared with the others, rests on the measurement of a copper wire. He had been led into this error, I suppose, by some ambiguity of language; but Dr. Matthiessen's mile of copper wire exhibited in 1862 was a German-silver resistance-coil, as stated in my Juror's Report, and in the Report to the Royal Society. This German-silver coil Dr. Matthiessen still has, and I have the set of German-silver coils called "Thomson's units" in 1862. Both of these have remained constant in their ratio, and the value of all Dr. Siemens's coils has been determined by comparison with them.

A difference of $1\frac{1}{2}°$ C. would therefore by no means account for the discrepancy of 0·5 per cent., which we believe exists between Dr. Siemens's 1862 coils and 1864 standards.

I have now concluded all the remarks I have to make on those parts of Dr. Siemens's paper which are strictly relevant to the matter at issue; but Dr. Siemens, at the end of his paper, makes some criticisms on the historical sketch I gave in the paper above referred to, of the various units of resistance which have been proposed.

I think none of these criticisms would have been made by him if he had observed that I did not profess to give a history of the "progresses in the field of resistance-measurements," as he supposed, but only of the units proposed. A whole book would have to be written to do justice to a history of resistance-measurements, whereas my sketch occupies only three pages.

First, Dr. Siemens complains that I did not mention the sets of resistance-coils made since 1848 in Berlin. I presume these are what I mistakenly called the German mile of No. 8 iron wire. This mistake of mine was not corrected by Messrs. Siemens in 1862, and appears again in the historical sketch. Unfortunately Dr. Siemens's letter to me, giving information for my historical sketch in answer to mine of the 28th February, did not come till the 21st April, while the Report was sent in on the 16th of March, and read on the 6th of April. The value of the coils he refers to is given in the Table annexed to the sketch, and I am sorry that I did not add a note to the reprint giving the exact date of their introduction and their true definition. Dr. Siemens may, however, observe that I did not think it necessary to give the exact dates at which any of the mile or kilometre units were introduced after those which I was informed were used in 1847 by the Electric and International Company.

Dr. Siemens further criticises my statement that, until about the year 1850, measurements of resistance were chiefly confined to the laboratory; but I confess I think his statement that in 1848 they began to make resistance-coils rather proves than disproves my assertion, especially as I had mentioned that the Electric and International Company had coils in use in 1847.

Next he complains that I do not mention the complete set of resistance-coils made in 1859. But as resistance-coils were then common, I do not see why they should have been mentioned. Does Dr. Siemens mean that he then weighed the coils, instead of using Wheatstone's balance or other electrical adjustment? I had in 1859 one set of coils adjusted by the Wheatstone's balance to within 0·1 per cent., which I use to this day, made by White, under Professor Thomson's directions; and many other sets were in use at the time. These were the coils I used as standards at Birkenhead (not Dr. Siemens's coils, of which I had only very rough specimens of the so-called German mile).

I did not mention this set of practical coils, or any others, my object being to mention new units. I only speak of them now to show that my silence cannot possibly have proceeded from any hostile animus.

Dr. Siemens says my historical sketch is very incomplete; but, beyond the correction in the description of one comparatively unimportant unit, he does not add one definition to those I enumerated, although I quite admit that in all probability some omissions must have occurred. As to Marié Davy's prior proposal for a mercury standard, my own statement is that, though not first in order of time, Dr. Siemens merits especial recognition, owing to the manner in which his proposal was carried out. Is not this what Dr. Siemens claims? The Report was not specially on his unit; and he may observe that Ohm, Wheatstone, Weber, and Thomson, and others of equal rank, have necessarily each but a few lines in the short sketch I wrote.

Next Dr. Siemens complains that I mentioned neither the fact that in 1859 the relative resistances of the conductor and insulator were given in mercury units, nor the method followed to measure the resistance which the insulating covering offered to the electric current and to compare it with that calculated from the specific resistance of the insulated material.

This subject was unconnected with the introduction of any new unit, and was therefore omitted as irrelevant.

If I had mentioned the subject at all, I should first have

mentioned Professor Thomson, who in a lecture in 1857 to the British Association gave the relative specific resistances of the copper and gutta percha of the Atlantic cable. In a note below is an extract from a printed report published in a local paper*. Next to Professor Thomson I should have mentioned myself, who, acting not under Dr. Siemens's direction, but at the suggestion of Professor Thomson, made in the spring and summer of 1859 experiments on this subject, more complete, I think, than are contained in Dr. Siemens's Red-Sea Report. These results were published at the Meeting of the British Association in 1859, and communicated in a more complete form to the Royal Society; whereas it is not till 1860 that I find any publication by Dr. Siemens on the subject, in a paper read to the British Association in 1860, the very body which had received the two previous communications. He is silent as to both of these. He gives less complete results than those given in the previous year; he gives no experiments on the difference between positive and negative currents, nor any account of the curious effect of electrification due to the time the current has been applied, and without which any record of resistance-measurements is nearly useless. Experiments on both these points are given in my paper. Surely, then, it cannot have been from any hostile feeling to Dr. Siemens that I said nothing about the measurement of the resistance of insulators.

* Extract from Professor W. Thomson's Lecture before the Members of the British Association at Dublin, 1857, taken from the Glasgow 'North British Daily Mail' of the 4th of September, 1857:—

" . . . . . . He had now described the material and the process of manufacture. He would like to say something of the relative qualities of gutta percha and copper as conductors, for they were both conductors—the distinction between non-conductors and conductors being not an absolute distinction, but only a relative distinction. Gutta percha is not a non-conductor, but a very powerful resister of electricity. Gutta percha and every known substance conducted electricity through it. (The lecturer proceeded to explain that, when tested by the galvanometer, there was very little difference in the force of a current sent into 2500 miles of the Atlantic cable, whether the circuit was or was not completed.) This seemed rather hopeless for telegraphing (he continued), where there was so much leakage that the difference could not be discovered between want of insulation and insulation at the remote end. But if there were 49-50ths lost by defective insulation, it would only make the difference between sending a message in nine minutes instead of in eight. The explanation of this was simple, but must be reserved. He then proceeded to allude to the variations of the conducting-power of gutta percha in different temperatures, and gave several comparisons, the result of experiments by Mr. Whitehouse and himself on this subject, and showed that the variations observed in portions of the same material were caused by difference of temperature. At hot temperatures gutta percha resisted twenty million million million times as much as copper; at cold temperatures one hundred million million million. . . . . ."

Dr. Siemens also speaks of his methods, read to the British Association in 1860, as forming the foundation of the rational system of cable-testing now in use. When writing of what is done in a foreign country, we are often very imperfectly informed as to the literature and progress of that country. I cannot doubt Dr. Siemens never heard of Professor Thomson's lecture or of my papers, or he would in 1860 have mentioned them.

It is equally clear that he has not paid much attention to the mass of evidence given in 1859 before the Board-of-Trade Committee on submarine cables. I have no doubt that he never heard of Professor Thomson's paper published in 1860 in the *Encyclopædia Britannica*, giving all the methods which he claims. If he had seen any of these papers, he never could have thought that he had taught us much by a paper in 1860. The methods described in his interesting paper were quite familiar to Professor Thomson, Latimer Clark, C. F. Varley, myself, and others. The methods are so treated by Professor Thomson in the above article, which deserves to be better known even in England than it is.

Can Dr. Siemens still think that I avoided mention of all these papers, familiar as they are to me, in order wilfully to be silent as to his work?

Be it well understood that I am making no counter accusation of unfairness. I am sure some of the papers I have referred to were unknown to Dr. Siemens; and probably, with reference to the Report of the Committee of the Board of Trade, as it was only published in 1861, Dr. Siemens may not have observed that the evidence to which I refer was given in 1859.

I can well understand that Dr. Siemens, who has undoubtedly invented these methods independently and has carried them out successfully in important works by a large and able staff, may not have been fully informed as to the progress independently made in England; but to one who is familiar with the papers referred to, his claim to have founded the rational system now in use reads a little strangely. I will not be led into a controversy as to every little improvement in arrangements, or every mathematical formula in use: these improvements are often made independently by many men; and the formulæ are often obvious and necessary deductions from perfectly well known principles: but I do claim for Professor Thomson the honour of having been the first to insist on a measurement of the conducting-power of the copper in submarine cables, and to express the quality of the insulation in terms of resistance, though I said nothing of these things in my Royal-Society Report because they were irrelevant.

I wish in conclusion to say that I believe no English electrician is more fully persuaded of the great services rendered to telegraphy by Dr. Siemens than I am. I know the immense

difference between devising theories in the cabinet, or even trying isolated experiments, and actually carrying out those methods on a large scale by the aid of an organized staff. I concede both merits to Dr. Siemens; and if I have urged my arguments forcibly as to the independence of the English school of electricians of that of Germany, I beg Dr. Siemens to believe that I have done so from no desire to diminish his claims, which indeed I could not do, but only to vindicate myself from what I felt to be a very unmerited suspicion, that of having wilfully omitted to mention his discoveries.

XXIII. *On the Phenomena observed in the Absorption-spectrum of Didymium.* By R. Bunsen*.

[With Two Plates.]

IN a paper which Professor Bahr and I published together, "Upon the Compounds of Erbium and Yttrium"†, we showed that slight differences were observed in the absorption-spectrum of sulphate of didymium, according as the light was allowed to pass through a crystal or through a solution of the salt. Since that time I have found that the erbium- and the didymium-spectrum undergo alteration if polarized light be employed and either the ordinary or the extraordinary ray be allowed to pass through the crystal. I have also found that whilst, when spectroscopes with one prism and with a telescope of moderate power are employed, the spectra of the various didymium compounds do not show any difference, yet most undoubted differences are noticed when more powerful instruments are used.

The alterations which the absorption-spectra exhibit under these circumstances form the subject of the present communication.

In the following experiments two of Steinheil's spectroscopes were used. In the one, which I call the smaller, was placed one flint-glass prism having a refracting angle of 60°, and a refracting surface of 30 millims. in diameter, whilst the telescope had a magnifying power of 8: in the other, which I call the larger, were four large prisms of flint glass, one of which had a refracting angle of 60°, and the other three each of 45°; the magnifying power of the telescope was ·40. The observations with the smaller instrument are reduced to the same scale of measurement as that used in the Table figured in the Philosophical Magazine, S. 4. vol. xxvi. p. 241. With the larger instrument the scale was employed which Kirchhoff adopted in his researches on the solar spectrum.

* Translated and communicated by Professor Roscoe, from Poggendorff's *Annalen*, vol. cxxviii. p. 100.

† *Ann. der Chem. und Pharm*, vol. cxxxvii. p. 1.

The oxide of didymium employed was specially prepared from cerite. The oxalates of the cerite metals were separated from cerium-oxide by heating in the air, dissolving in nitric acid, and boiling with magnesite, and repeating this mode of separation three times with the reprecipitated oxalates.

The separation of the didymium-salt was accomplished in the usual way, by recrystallizing the sulphate twelve times, according to the methods best adapted to effect a complete separation. The crystals exhibited a bluish-red colour; and the oxide prepared from them was of a similar or rather redder tint, but was not the least brown-coloured.

The solution of the salt in water deposited well-formed crystals when allowed slowly to evaporate in a beaker covered with filter-paper during the hot months of summer. They gave on analysis the following numbers:—

Didymium-oxide . . .	46·27
Sulphuric acid	33·73
Water	20·00
	100·00

This corresponds to the formula $3(\mathrm{DiO}, \mathrm{SO}^3)\,8\,\mathrm{HO}$, and agrees closely with the following analysis, by Marignac, of the extremely pure salt from which he determined the atomic weight of didymium:—

Didymium-oxide . . .	46·50
Sulphuric acid	33·30
Water	20·20
	100·00

The crystals belong to the monoclinic system. The inclination of the oblique axes was found by measurement to be L=61° 45′, or close upon the former determinations of Marignac; whilst the relation between the orthodiagonal (a), the klinodiagonal (b), and the principal axis (c) is

$$a : b : c = 0{\cdot}3283 : 0{\cdot}6786 : 1.$$

These relations of the axes give the following angles, observed and calculated:—

	Observed.	Calculated.
O P upon ∞ P ∞ . . .	118° 15′	
+ P ∞ „ O P . . .	103 7	
O P „ − P . . .	119 40	
∞ P ∞ „ + P ∞ . . .	138 55	138° 38′
O P „ + ½ P . . .	114 0	113 36
+ P „ + ½ P . . .	162 33	162 12
− P „ + P . . .	144 30	144 41
− P „ − P upon O P .	77 30	77 24

The crystal (Plate II. fig. 11) employed in these experiments exhibited a strongly-marked tabular habitus, owing to the growth of the surfaces O P. It was cemented between two thin microscopic glasses with Canada balsam, and was strongly coloured and perfectly clear and transparent.

The plane of polarization of the ordinary and extraordinary rays which pass perpendicular to the surface O P makes with the klinodiagonal and with the orthodiagonal an angle of 20°. Which is the plane of polarization of the ordinary, and which of the extraordinary ray, cannot be settled, as the positions of the optic axes have not been determined. In order to distinguish the spectra of the two rays, a Nicol's prism was placed in front of the crystal, which was so arranged that the light was transmitted in a direction perpendicular to the surface O P; and the crystal so turned round that the plane of polarization of the Nicol coincided with one or other of the two planes. The position of the crystal when the plane of polarization of the Nicol made an angle of nearly 20° with the orthodiagonal I shall call the orthodiagonal position; the other I call the klinodiagonal position.

In the investigation of the spectra of different salts of didymium, a difficulty occurs, in the fact that absorption-spectra, otherwise perfectly similar, assume a different appearance according to the degree of intensity, the breadth of the absorption-bands varying with the thickness and with the proportion of salt contained in the absorbing medium. Such comparative observations, therefore, can only be of value when, in all the comparisons, the light has been acted upon by the same quantities of the absorbent body in passing through the absorbing medium. This condition is fulfilled when the amount of didymium contained in the absorbing medium is inversely proportional to the length of the column through which the light passes. In the comparison of the crystallized and dissolved sulphate with the solutions of other didymium salts this condition was most carefully taken into consideration.

Let the thickness of the layer of crystal through which the light passes be l, the thickness of the column of solution l_1; let the amount of didymium oxide contained in the unit of volume of the crystal be d, and that in the unit of volume of the solution be d_1; then in all the experiments d_1 was so chosen that

$$d_1 l_1 = dl.$$

The crystals used in the experiments had a specific gravity of 2·7153 at 8° C. as a mean of two well-agreeing determinations. 1 cubic centim. of the crystalline mass therefore contains 1·2563 grm. of didymium oxide. The layer of crystal lying between the faces O P, and through which the light passed, had a thickness of 1·55 millim. as measured by a spherometer. Hence the values of d and l are

$$d=1{\cdot}2563, \quad l=1{\cdot}55.$$

Solutions of three didymium salts were examined; one of sulphate, one of acetate, and one of chloride. In all three the values of d_1 and l_1 were

$$d_1=0{\cdot}03414 \text{ grm.}, \quad l_1=57{\cdot}1 \text{ millim.}$$

The tube (Plate II. fig. 10) used to contain these solutions consists of a thick-walled glass tube of 6 millims. internal diameter, into the ends of which two glass stoppers with plane parallel terminal surfaces were ground. These stoppers were cut out of a piece of plate glass, and fitted by accurate grinding into the tube. In order to prevent the tube from cracking when completely full of liquid, from expansion caused by rise of temperature, a side-tube was melted into the middle of the tube, and this was closed at the end and filled with air. This side-tube also served as a handle to hold the tube horizontal in the cork of the stand, so that the tube could be turned upon the axis of the side-tube and then taken out to be cleaned or refilled. Fig. 1 (Plate II.) gives the position of Fraunhofer's lines, to which the three first spectra are to be reduced. Fig. 2 represents the spectrum obtained by polarized lamplight with the small apparatus when the crystal is placed in the orthodiagonal position; and fig. 3 gives the spectrum by polarized light when the crystal is placed in the klinodiagonal position. Non-polarized light ought to give a spectrum the mean of these two; but it cannot be distinguished from that represented by fig. 3. The differences between figs. 2 and 3 are best seen in the three chief groups of bands near Fraunhofer's lines D, E, and F, as in these groups the bright spaces between the well-defined sharp absorption-bands in fig. 3 become darker in fig. 2, and thus each group of bands attains an indistinct and totally different appearance.

In order to investigate more accurately the changes which occur when polarized light is used, the single groups of bands at D, E, and F were examined by sunlight in the larger instrument. Fig. 5 represents the group of bands near D when the crystal is examined by polarized light in orthodiagonal position; fig. 6 the same group in klinodiagonal position. The spectrum fig. 6 differs from that of fig. 5, inasmuch as the spaces between the single dark bands become brighter, and the band at 1100 disappears, and in its place a new band appears between 1090 and 1095. The group of bands near E undergoes a still more remarkable change. The form of spectrum seen by polarized light in orthodiagonal position (fig. 12, Plate III.) changes to the form represented by fig. 13 when the position of the crystal is

altered to the klinodiagonal one. The five weaker bands disappear almost entirely, whilst only a slight alteration is noticed in the shade of the bands between 1557 and 1570; but at 1476 a new band appears. The alterations produced in the group of bands near the line F are the least visible. Fig. 17 represents this group: it exhibits such slight alterations in the relative brightness of the absorption-bands viewed with polarized light, either in the orthodiagonal or klinodiagonal position of the crystal, that these differences cannot be distinctly made out, especially when viewed by the somewhat weak light which the solar spectrum possesses near F. The same observation was made with several other bands; and their description, for a similar reason, is not given.

All these differences in the absorption-spectra occurring with polarized light resemble the absorption-phenomena which tourmaline exhibits under similar circumstances. The property which crystallized didymium-sulphate possesses, of absorbing differently the ordinary and extraordinary rays, is in its nature identical with that which makes tourmaline so valuable in optical experiments.

The solution of the crystals in water gives another spectrum, differing, again, from both the foregoing. This is represented on Plate II. fig. 4. Here also the three chief groups of bands are situated near D, E, and F, but they are still more confused than in the spectrum obtained when the crystal is placed in the orthodiagonal position (fig. 2).

Some of the maxima of absorption do indeed show a small but perceptible change of form: thus, for instance, the band 27 to 31 of the crystal-spectrum disappears, whilst a new but faint band becomes visible at 30 in the spectrum of the solution.

Very remarkable and noteworthy are the small alterations in position which occur in the minima of brightness in the didymium-spectrum, dependent upon the nature of the compound in which the metal occurs. These changes are too minute to be seen with the small, though seen with the larger instrument; I have as yet only investigated them completely in the case of three didymium-salts, viz. the chloride, sulphate, and acetate. It is, however, more than probable that these same phenomena will also be found to occur with other solutions, and with the absorption-spectra of other crystals of didymium-salts, and perhaps may be exhibited with the luminous spectra of the oxide and other compounds of didymium. The difficulty of obtaining a variety of well-crystallized didymium-salts of sufficient transparency has prevented me from pursuing the subject further in this direction. Plate II. figs. 7, 8, and 9 represent the group of bands near D of didymium chloride, sulphate, and acetate, in the order in which these salts are mentioned. Plate III. figs. 14, 15, and 16 represent the group of bands near E for these salts in the same order, and figs. 18, 19, and 20 the same for the group near F.

The atomic weight of didymium-chloride is 95·9, and that of the anhydrous acetate 106·9. It will be noticed that all the groups of bands in the case of the salts under examination approach the red end of the spectrum in the order of their increasing atomic weights.

These differences here noticed in the absorption-spectra of different didymium-compounds cannot, in our present complete state of ignorance of any general theory for the absorption of light in absorptive media, be connected with other phenomena. They remind one of the slight and gradual alterations in pitch which the notes from a vibrating elastic rod undergo when the rod is weighted, or of the change of tone which an organ-pipe exhibits when the tube is lengthened.

XXIV. *On the Origin of Muscular Power.*

By E. Frankland, *F.R.S.**

UNDER this title there appeared in a recent Number of the Philosophical Magazine an able article by Professors Fick and Wislicenus†, in which these gentlemen describe the results of experiments made upon themselves before, during, and after an ascent of the Faulhorn in Switzerland. In these experiments the amount of measured work performed in the ascent of the mountain was shown to exceed, by more than three-fourths, the amount which it would be theoretically possible to realize from the maximum amount of muscle-oxidation indicated by the total quantity of nitrogen in the urine.

The data afforded by these experiments appear to me to render utterly untenable the theory that muscular power is derived from muscle-oxidation. Nevertheless, in the application of these data to the problem under consideration, one important link was found to be wanting, viz. the amount of actual energy generated by the oxidation of a given weight of muscle in the human body. Fick and Wislicenus refer to this missing link in the following words:—"The question now arises, what quantity of heat is generated when muscle is burnt to the products in which its constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question; for neither the heat of combustion of muscle, nor of the nitrogenous *residue* of muscle (urea), is known."

* Communicated by the Author.

† Phil. Mag. vol. xxxi. p. 485.

Owing to the want of these data, the numerical results of the experiment of Fick and Wislicenus are rendered less conclusive against the hypothesis of muscle-oxidation than they otherwise would have been; whilst similar determinations which have been made by Edward Smith, Haughton, Playfair, and others are even liable to a total misinterpretation from the same cause.

I have endeavoured to supply this want by the calorimetrical determination of the actual energy evolved by the combustion of muscle and of urea in oxygen: but, inasmuch as uric and hippuric acids frequently appear in the urine as products of a less perfect muscle-oxidation, I have also determined the calorific value of these substances, and have added purified albumen and beef fat to the list. Creatin would also have been included; but, although I was furnished with an ample supply of this substance through the kindness of Dr. Dittmar, all attempts to burn it in the calorimeter were fruitless. In numerous trials under varied conditions it always exploded violently on ignition.

The determination of the actual energy developed by the combustion of the above-named substances is surrounded by formidable difficulties, which have probably prevented their previous execution. It is impossible to effect their complete combustion in oxygen gas, under conditions which permit of the accurate measurement of the heat evolved; but preliminary experiments showed that complete oxidation could be secured by deflagration with potassic chlorate; and, although this method is doubtless inferior in accuracy to the calorimetrical methods usually employed, it is hoped that, with the corrections described below, the results obtained merit sufficient confidence to render them useful in subsequent discussions of this and allied subjects. The determinations were made in a calorimeter devised some years ago by Lewis Thompson, and which I have repeatedly used with satisfaction in other determinations of a like kind. This instrument consists of a copper tube made to contain a mixture of potassic chlorate with the combustible substance, and which can be enclosed in a kind of diving-bell, also of copper, and so lowered to the bottom of a suitable vessel containing a known quantity (2 litres) of water. The experiments were conducted in the following manner:—19·5 grams* of chlorate of potash, to which about one-eighth of manganic oxide was added, were intimately mixed with a known weight (generally about 2 grams) of the substance whose thermal value was to be determined; and the mixture being then placed in the copper tube above mentioned, a small piece of cotton thread, previously

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

steeped in potassic chlorate and dried, was inserted in the mixture. The temperature of the water in the calorimeter was now carefully ascertained by a delicate thermometer, and, the end of the cotton thread being ignited, the tube with its contents was placed in the copper bell and lowered to the bottom of the water. As soon as the combustion reached the mixture, a stream of gases issued from numerous small openings at the lower edge of the bell and rose to the surface of the water—a height of about 10 inches. At the termination of the deflagration, the water was allowed free access to the interior of the bell, by opening a stopcock connected with the bell by a small tube rising above the surface of the water in the calorimeter. The gases in the interior of the bell were thus displaced by the incumbent column of water; and by moving the bell up and down repeatedly, a perfect equilibrium of temperature throughout the entire mass of water was quickly established. The temperature of the water was again carefully observed; and the difference between this and the previous observation gives the calorific power, or the potential energy, of the substance consumed, expressed as heat.

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

1. The amount of heat absorbed by the calorimeter and apparatus employed: *to be added.*
2. The amount of heat carried away by the escaping gases after issuing from the water: *to be added.*
3. The amount of heat due to the decomposition of the chlorate of potash employed: *to be deducted.*
4. The amount of heat equivalent to the work performed, by the gases generated, in overcoming the pressure of the atmosphere: *to be added.*

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

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

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

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

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

It was ascertained that a stream of dry air passed through the water of the calorimeter at about the same rate and for the

same period of time as the gaseous products of combustion, depressed the temperature of the water by only 0°·02 C.

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

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

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

First experiment	340
Second experiment . . .	300
Third experiment . . .	375
Fourth experiment , . .	438
Fifth experiment . . .	438
	1891
Mean	378

This result was confirmed by the following experiments:—

(1) Starch was burnt, first, in a current of oxygen gas, and secondly by admixture with potassic chlorate and manganic oxide.

Heat-units furnished by 1 grm. of starch burnt with 9·75 grms. of potassic chlorate . .	4290
Heat-units furnished by the same weight of starch burnt in a stream of oxygen gas . . .	3964
Difference	326

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

Heat-units furnished by 1 grm. of phenylic alcohol burnt with 9·75 grms. potassic chlorate.	8183
Heat-units furnished by 1 grm. of phenylic alcohol when burnt with gaseous oxygen (Favre and Silbermann)	7842
Difference	341

These three determinations of the heat evolved by the decomposition of 9·75 grms. of potassic chlorate, furnishing the num-

bers 378, 326, and 341, agree as closely as could be expected when it is considered that all experimental errors are necessarily thrown upon the calorific value of the potassic chlorate.

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

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

Joule's mechanical equivalent of heat was employed, viz. 1 kilog. of water raised 1° C. = 423 metrekilogs.

The following results were obtained:—

Actual Energy developed by 1 grm. of each substance when burnt in Oxygen.

Name of substance (dried at 100° C.).	Heat-units.					Metre-kilogs. of force. (Mean.)
	1st Exp.	2nd Exp.	3rd Exp.	4th Exp.	Mean.	
Beef muscle purified by repeated washing with ether ...	5174	5062	5195	5088	5103	2161
Purified albumen...	5009	4987	......	......	4998	2117
Beef fat	9069	......	......	......	9069	3841
Hippuric acid	5330	5437	......	......	5383	2280
Uric acid	2645	2585	......	......	2615	1108
Urea..................	2121	2302	2207	2197	2206	934

It is evident that the above determination of the actual energy developed by the combustion of muscle in oxygen represents more than the amount of actual energy produced by its oxidation within the body, because when muscle burns in oxygen its carbon is converted into carbonic acid, and its hydrogen into water, the nitrogen being to a great extent evolved in the elementary state; whereas when muscle is most completely consumed in the body the products are carbonic acid, water, and urea: the whole of the nitrogen passes out of the body as urea, a substance which still retains a considerable amount of potential energy. Dry muscle and pure albumen yield, under these circumstances, almost exactly one-third of their weight of urea; and this fact, together with the above determination of the actual energy developed on the combustion of urea, enables us to deduce with certainty the amount of actual energy developed by muscle and albumen respectively when consumed in the human body. It is as follows:—

Actual Energy developed by 1 grm. of each substance when consumed in the body.

Name of substance (dried at 100° C.).	Heat-units. (Mean.)	Metrekilogs. of force. (Mean.)
Beef muscle purified by ether ...	4368	1848
Purified albumen	4263	1803

Interpolating the data thus obtained into the results of Fick and Wislicenus's experiments, let us now compare the amount of measured and calculated work performed by each of the experimenters during the ascent of the Faulhorn, with the actual energy capable of being developed by the maximum amount of muscle that could have been consumed in their bodies, this amount being represented by the total quantity of nitrogen excreted in each case during the ascent and for six hours afterwards.

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

The actual energy capable of being produced by the consumption of 37·17 and 37·00 grms. of dry muscle in the body was *estimated* by Fick and Wislicenus at 106,250 and 105,825 metrekilogs.

The experimental determination of the actual energy developed by muscle-oxidation renders it now abundantly evident that the muscular power expended by these gentlemen in the ascent of the Faulhorn could not be exclusively derived from the oxidation either of their muscles or of other nitrogenous constituents of their bodies, since the maximum of power capable of being derived from this source, even under very favourable assumptions, is in both cases less than one-half of the work actually

performed; but the deficiency becomes much greater if, as Fick and Wislicenus have done, we take into consideration the fact that the actual energy developed by oxidation or combustion cannot be wholly transformed into mechanical work. In the best-constructed steam-engine, for instance, only one-tenth of the actual energy developed by the burning fuel can be obtained in the form of mechanical power; and in the case of man, Helmholtz estimates that not more than one-fifth of the actual energy developed in the body can be made to appear as external work. The experiments of Heidenhain, however, show that under favourable circumstances a muscle may be made to yield, in the shape of mechanical work, as much as one-half of the actual energy developed within it, the remainder assuming the form of heat. Taking, then, this highest estimate of the proportion of mechanical work capable of being got out of actual energy, it becomes necessary to multiply by 2 the above numbers representing the ascertainable work performed, in order to express the actual energy involved in the production of that work. We then get the following comparison of the actual energy capable of being developed by the amount of muscle consumed, with the actual energy necessary for the performance of the work executed in the ascent of the Faulhorn.

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

Thus, taking the average of the two experiments, it is evident that scarcely one-fifth of the actual energy required for the work performed could be obtained from the amount of muscle consumed.

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

Treadwheel Experiments.

A treadwheel is a revolving drum with steps placed at distances of 8 inches, and the prisoners are required to turn the wheel downwards by stepping upwards. Four prisoners, designated x003220 to bring metre kilogs to ft tons

below as A, B, C, and D, were employed in these experiments; and each worked upon the wheel in alternate quarters of an hour, resting in a sitting posture during the intervening quarters. The period of actual daily labour was 3½ hours. The total ascent per hour 2160 feet, or per day 1·432 mile. The following are the results:—

Treadweel Work. (E. Smith.)

	Weight in kilogs.	Ascent in metres.	Days occupied in ascent.	External work performed in metre-kilogs.	Total nitrogen evolved.	Weight of dry muscle corresponding to nitrogen.
					grms.	grms.
A	47·6	23,045	10	1,096,942	171·3	1101·2
B	49	23,045	10	1,129,205	174·5	1121·7
C	55	20,741	9	1,140,755	168·0	1080·1
D	56	20,741	9	1,161,496	159·3	1024·3

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

Internal Work. (Helmholtz and Fick.)

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

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

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

+ Circulation 223.1 ft tons
** Respiration 35.1
258.2

Treadwheel Work.

Average external work per man per day	119,605 metrekilogs.	= f.t. 346.2
Average nitrogen evolved per man per day	17·7 grms.	
Weight of dry muscle corresponding to average nitrogen evolved per day	114 "	
Actual energy producible by the consumption of 114 grms. dry muscle in the body	210,672 metrekilogs.	

Average actual energy developed in the body of each man, viz.

External work	119,605 × 2 = 239,210	metrekilogs.
Circulation	69,120 × 2 = 138,240	"
Respiration	10,886 × 2 = 21,772	"
	399,222	"

In these experiments the conditions were obviously very unfavourable for the comparison of the amount of actual energy producible from muscle-metamorphosis with the quantity of actual energy expended in the performance of estimable work, since, during that portion of the twenty-four hours not occupied in the actual experiment, a large amount of unestimable internal work, such as the statical activity of the muscles, peristaltic motion, &c., was being performed. Nevertheless these experiments show that the average actual energy developed in producing work in the body of each man was nearly twice as great as that which could possibly be produced by the whole of the nitrogenous matter oxidized in the body during twenty-four hours. It must also be remarked that the prisoners were fed upon a nitrogenous diet containing 6 ounces of cooked meat without bone—a diet which, as is well known, would favour the production of urea.

Shot-drill Experiments.

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

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

Shot drill	3 hours.	
Ordinary drill	1¼	"
Oakum-picking	3½	"

The total average daily external work was estimated by Haughton at 96,316 metrekilogs. per man. The following is a condensed summary of the results of these experiments:—

Military Vegetarian Prisoners at Shot Drill. (*Haughton.*) 7.7

Average external work per man per day	96,316 metrekilogs. 311
Average nitrogen evolved per man per day	12·1 grms.
Weight of dry muscle corresponding to average nitrogen evolved per day	77·9 „
Actual energy producible by the consumption of 77·9 grms. of dry muscle in the body	143,950 metrekilogs.

Average actual energy developed daily in the body of each man, viz.

External work 96,316 × 2 . . .	=192,632 metrekilogs.
Internal work „	=160,012 „
	352,644 „

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

Playfair's Determinations.

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

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

Hard-worked Labourer (Playfair).

	Work performed.	Actual energy required.
	metrekilogs.	metrekilogs.
Daily labour (external work)....	109,496	218,992
Internal work	80,006	160,012
	189,502	379,004

Actual energy capable of being produced from 5·5 oz. (155·92 grms.) of flesh-formers contained in the daily food of the labourer ...... metrekils. 288,140

* 352

= 928·1

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

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

Excretion of carbonic acid during rest and muscular exertion:—

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

It is admitted on all hands that food, and food alone, is the ultimate source from which muscular power is derived; but the above determinations and considerations prove conclusively, first, that the non-nitrogenous constituents of the food, such as

* Phil. Trans. for 1859, page 709.

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

It can scarcely be doubted, however, that the chief use of the nitrogenous constituents of food is for the renewal of muscular tissue—the latter, like every other part of the body, requiring a continuous change of substance; whilst the chief function of the non-nitrogenous is to furnish, by their oxidation, the actual energy which is in part transmuted into muscular force.

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

From this point of view it is interesting to examine the various articles of food in common use, as to their capabilities for the production of muscular power. I have therefore made careful estimations of the calorific value of different materials used as food, with the same apparatus and in the same manner asdescribed above for the determination of the actual energy in muscle, urea, &c. The results are embodied in the following series of Tables;

* The Lancet, 1863, pages 381 and 412.

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

Table I.—Results of Experiments with Food dried at 100° C., in heat-units.

Name of food.	Heat-units. 1st Exp.	Heat-units. 2nd Exp.	Heat-units. 3rd Exp.	Heat-units. (Mean.)
Cheshire cheese	6080	6149	......	6114
Potatoes	3752	......	......	3752
Apples	3776	3562	......	3669
Mackerel	5994	6134	......	6064
Oatmeal (not dried)	4143	4018	3857	4004
Lean beef	5271	5260	5410	5313
White of egg	4823	4940	4927	4896
Carrots	3776	3759	......	3767
Pea-meal (not dried)	3866	4006	......	3936
Flour (not dried)	3941	3931	......	3936
Arrowroot (not dried)	3923	3902	......	3912
Butter	7237	7291	......	7264
Ham boiled and lean	4188	4498	......	4343
Lean veal	4459	4595	4488	4514
Hard-boiled egg	6455	6187	......	6321
Yelk of egg	6460	......	......	6460
Isinglass	4520	4520	......	4520
Cabbage	3809	3744	......	3776
Whiting	4520	4520	......	4520
Ground rice (not dried)	3802	3824	......	3813
Cod-liver oil	9134	9080	......	9107
Cocoa nibs (not dried)	6809	6937	......	6873
Residue of milk	5066	5120	......	5093
Bread crumb	3984	3984	......	3984
Bread crust (not dried)	4459	......	......	4459
Lump sugar (not dried)	3403	3294	......	3348
Commercial grape-sugar (not dried)	3277	3277	......	3277
Residue from bottled ale	3776	3744	......	3760
Residue from bottled stout	6348	6455	......	6401

TABLE II.—Actual Energy developed by 1 gram of various articles of Food when burnt in Oxygen.

Name of food.	Heat-units.		Metrekilograms of force.		Per cent. of water.
	Dry.	Natural condition.	Dry.	Natural condition.	
Cheese (Cheshire)	6114	4647	2589	1969	24·0
Potatoes	3752	1013	1589	429	73·0
Apples	3669	660	1554	280	82·0
Oatmeal	......	4004	......	1696	
Flour	......	3936	......	1669	
Pea-meal	......	3936	......	1667	
Ground rice	......	3813	......	1615	
Arrowroot	......	3912	......	1657	
Bread crumb	3984	2231	1687	945	44·0
Bread crust	......	4459	......	1888	
Beef (lean)	5313	1567	2250	664	70·5
Veal	4514	1314	1912	556	70·9
Ham (boiled)	4343	1980	1839	839	54·4
Mackerel	6064	1789	2568	758	70·5
Whiting	4520	904	1914	383	80·0
White of egg	4896	671	2074	284	86·3
Hard-boiled egg	6321	2383	2677	1009	62·3
Yelk of egg	6460	3423	2737	1449	47·0
Isinglass	4520	......	1914		
Milk	5093	662	2157	280	87·0
Carrots	3767	527	1595	223	86·0
Cabbage	3776	434	1599	184	88·5
Cocoa-nibs	......	6873	......	2911	
Beef fat	9069	......	3841		
Butter	......	7264	......	3077	
Cod-liver oil	......	9107	......	3857	
Lump sugar	......	3348	......	1418	
Commercial grape-sugar	......	3277	......	1388	
Bass's ale (alcohol reckoned)	3760	775	1599	328	88·4
Guiness's stout „	6401	1076	2688	455	88·4

TABLE III.—Actual Energy developed by 1 gram of various articles of Food when oxidized in the Body.

Name of food.	Metrekilograms of force.	
	Dry.	Natural condition.
Cheshire cheese	2429	1846
Potatoes	1563	422
Apples	1516	273
Oatmeal	......	1665
Flour	......	1627
Pea-meal	......	1598
Ground rice	......	1591
Arrowroot	......	1657
Bread crumb	1625	910
Lean of beef	2047	604

TABLE (*continued*).

Name of food.	Metrekilograms of force.	
	Dry.	Natural condition.
Lean of Veal	1704	496
Lean of ham (boiled)	1559	711
Mackerel	2315	683
Whiting	1675	335
White of egg	1781	244
Hard-boiled egg	2562	966
Yelk of egg	2641	1400
Gelatin	1550	
Milk	2046	266
Carrots	1574	220
Cabbage	1543	178
Cocoa-nibs	......	2902
Butter	......	3077
Beef fat	3841	
Cod-liver oil	3857	
Lump sugar	......	1418
Commercial grape-sugar	......	1388
Bass's ale (bottled)	1559	328
Guiness's stout „	2688	455

TABLE IV.—Weight and Cost of various articles of Food required to be oxidized in the body in order to raise 140 lbs. to the height of 10,000 feet.

External Work = one-fifth of Actual Energy.

Name of food.	Weight in lbs. required.	Price per lb.	Cost.
		s. *d.*	*s.* *d.*
Cheshire cheese	1·156	0 10	0 11¼
Potatoes	5·068	0 1	0 5¼
Apples	7·815	0 1½	0 11¾
Oatmeal	1·281	0 2¾	0 3½
Flour	1·311	0 2¾	0 3½
Pea-meal	1·335	0 3½	0 4½
Ground rice	1·341	0 4	0 5½
Arrowroot	1·287	1 0	1 3½
Bread	2·345	0 2	0 4¾
Lean beef	3·532	1 0	3 6½
Lean veal	4·300	1 0	4 3½
Lean ham (boiled)	3·001	1 6	4 6
Mackerel	3·124	0 8	2 1
Whiting	6·369	1 4	9 4
White of egg	8·745	0 6	4 4½
Hard-boiled egg	2·209	0 6½	1 2½
Isinglass	1·377	16 0	22 0½
Milk	8·021	5*d.* per qrt.	1 3¾
Carrots	9·685	0 1½	1 2½
Cabbage	12·020	0 1	1 0¼
Cocoa-nibs	0·735	1 6	1 1¼
Butter	0·693	1 6	1 0½
Beef fat	0·555	0 10	0 5½
Cod-liver oil	0·553	3 6	1 11¼
Lump sugar	1·505	0 6	1 3
Commercial grape-sugar	1·537	0 3½	0 5½
Bass's pale ale (bottled)	9 bottles.	0 10	7 6
Guiness's stout „	6⅔ „	0 10	5 7½

Table V.—Weight of various articles of Food required to sustain Respiration and Circulation in the Body of an average Man during twenty-four hours.

Name of food.	Weight in ozs.	Name of food.	Weight in ozs.
Cheshire cheese	3·0	Whiting	16·8
Potatoes	13·4	White of egg	22·1
Apples.......................	20·7	Hard-boiled egg..........	5·8
Oatmeal	3·4	Gelatin	3·6
Flour	3·5	Milk	21·2
Pea-meal....................	3·5	Carrots	25·6
Ground rice	3·6	Cabbage	31·8
Arrowroot	3·4	Cocoa-nibs	1·9
Bread	6·4	Butter	1·8
Lean beef	9·3	Cod-liver oil	1·5
Lean veal	11·4	Lump sugar	3·9
Lean ham (boiled)......	7·9	Commercial grape-sugar	4·0
Mackerel	8·3		

These results are fully borne out by experience in many instances. The food of the agricultural labourers in Lancashire contains a large proportion of fat. Besides the very fat bacon which constitutes their animal food proper, they consume large quantities of so-called apple dumplings, the chief portion of which consists of paste in which dripping and suet are large ingredients; in fact these dumplings frequently contain no fruit at all. Egg and bacon pies and potatoe pies are also very common *pièces de résistance* during harvest time, and whenever very hard work is required from the men. I well remember being profoundly impressed with the dinners of the navigators employed in the construction of the Lancaster and Preston Railway; they consisted of thick slices of bread surmounted with massive blocks of bacon in which mere streaks of lean were visible. These labourers doubtless find that from fat bacon they obtain at the minimum cost the actual energy required for their arduous work. The above Tables affirm the same thing. They show that ·55 lb. fat will perform the work of 1·15 lb. cheese, 5 lbs. potatoes, 1·3 lb. of flour or pea-meal, or of $3\frac{1}{2}$ lbs. of lean beef. Donders, in his admirable pamphlet 'On the Constituents of Food, and their relation to Muscular Work and Animal Heat,' mentions the observations of Dr. M. C. Verloren on the food of insects. The latter remarks, "many insects use, during a period in which very little muscular work is performed, food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen." He also mentions bees and butterflies as instances of insects performing enormous muscular work, and subsisting upon a diet containing but the merest traces of nitrogen. The following conclusions may therefore be drawn from the foregoing experiments and considerations:—

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

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

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

4. Like every other part of the body, the muscles are constantly being renewed; but this renewal is scarcely perceptibly more rapid during great muscular activity than during comparative quiescence.

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

6. The non-nitrogenous matters of food which find their way into the blood yield up all their potential energy as actual energy; the nitrogenous matters, on the other hand, leave the body with a portion (at least one-seventh) of their potential energy unexpended.

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

XXV. *On a Problem in the Calculus of Variations.* *By* I. Todhunter, *M.A., F.R.S.**

In the Philosophical Magazine for July 1866, Professor Challis has communicated some additional observations respecting the problem in the Calculus of Variations which had been discussed in various preceding Numbers of the Magazine. The problem may be thus enunciated: To determine the greatest solid of revolution, the surface of which is given, and which cuts the axis at two fixed points. I have stated in the Philosophical Magazine for June what I consider to be the solution of the pro-

* Communicated by the Author.

blem as thus enunciated; and I think it will be admitted that this solution does give the solid of greatest volume, and is in harmony with the recognized principles of the Calculus of Variations.

In the Philosophical Magazine for July, a condition is attached to the enunciation, as will be seen from the following words which occur towards the beginning of the article:—"That a solid exists, the largest of all solids of revolution whose surfaces are of given area and extend *continuously* from one extremity of the axis to the other, there can, I think, be no reason to doubt. . . . ." The result which is obtained is that the required solid is that which is generated by the revolution of a segment of a circle round its chord, the chord being the straight line which joins the two fixed points. This result is called the absolute maximum.

My present design is briefly to test the accuracy of this result, and the method by which it is obtained.

I may remark that the word *absolute* does not seem very appropriate, because it naturally suggests freedom from any restriction, whereas the result is only maintained with the restriction that the surfaces considered shall be *continuous*; but this is not a matter of great importance in connexion with my design.

I will first show, by examining a particular case, that the asserted result is erroneous.

The particular case I take is that in which the distance between the two fixed points is indefinitely small, so that, in other words, the generating curve is only required to meet the axis at one point. Then the assertion is that the solid of greatest volume with a given surface is that formed by the revolution of a circle round a tangent; on the contrary, I maintain that by taking an ellipse of very small excentricity, a solid can be formed of greater volume with an equal surface.

Let $2a$ and $2b$ be the axes of the ellipse, and let it revolve round the tangent at one end of the axis minor.

Then the volume generated is

$$2\pi b \times \pi ab,$$

and the surface is

$$2\pi b \times 4a \int_0^{\frac{\pi}{2}} \sqrt{(1-e^2\cos^2\phi)}\, d\phi,$$

where e is the excentricity.

Let r be the radius of a circle; then the corresponding volume and surface are $2\pi^2 r^3$ and $4\pi^2 r^2$ respectively.

Now I shall show that if the volumes of the two solids are

equal, the surface of the former solid is less than the surface of the latter; this is equivalent to the statement I have made above.

We have, by equating the volumes,

$$ab^2 = r^3,$$

that is,

$$a^3(1-e^2) = r^3;$$

thus

$$a = r(1-e^2)^{-\frac{1}{3}}.$$

Hence the surface formed by the revolving ellipse is

$$8\pi r^2(1-e^2)^{-\frac{1}{6}} \int_0^{\frac{\pi}{2}} \sqrt{(1-e^2\cos^2\phi)}\, d\phi;$$

and we have to show that this is less than $4\pi^2 r^2$; that is, we have to show that

$$(1-e^2)^{-\frac{1}{6}} \int_0^{\frac{\pi}{2}} \sqrt{(1-e^2\cos^2\phi)}\, d\phi \text{ is less than } \frac{\pi}{2},$$

the excentricity e being supposed very small.

This may be shown in more than one way: the following will be sufficient. If e is very small, so that we may reject e^4 and higher powers of e, the left-hand expression becomes

$$\left(1+\frac{e^2}{6}\right)\frac{\pi}{2}\left(1-\frac{e^2}{4}\right), \text{ or } \frac{\pi}{2}\left(1-\frac{e^2}{12}\right).$$

This example is a special case of a general proposition which was enunciated in the Philosophical Magazine for March, and to which attention was again invited in July. The general proposition is the following: a ring having a circular transverse section is larger than any other ring having the same superficies, and the same radius either interior or exterior, but a different form of transverse section. Now I have just taken a special case of this general proposition, and shown that in this case the result is erroneous.

Moreover the general proposition itself is erroneous. For it may be shown in nearly the same manner that by taking, instead of the circle, an ellipse of very small excentricity with its major axis parallel to the axis of revolution, a solid can be formed of greater volume with an equal surface when the interior radius of the ring is given.

Again, the result which immediately follows in the March Number is also erroneous. A figure formed of a rectangle and a semicircle generates a solid which, under certain circumstances, is asserted to have the greatest volume with a given surface. It will be found that by changing the semicircle into a semiellipse, a solid can be formed of greater volume with an equal surface.

In the first example which I brought forward, I supposed the length of the axis of the required solid of revolution to be indefinitely small, and I showed that the result which I am examining is incorrect. If the length of the axis be *finite*, it may be shown that, by changing the segment of a circle into a segment of an ellipse, a solid can in many cases be formed greater than that which is erroneously said to be the maximum, but having an equal surface. I do not say that this is possible in *all* cases; but the fact that it is possible in *any* case establishes the opinion which I am maintaining.

Of course I do not assert that in the cases I have noticed I have here assigned the greatest solid, or a maximum solid; I have only professed to show that the statements on which I am commenting are erroneous.

The foregoing examples will enable a person who has not studied the Calculus of Variations, but is acquainted with the elements of the Differential and Integral Calculus, to form an opinion on the subject I am discussing. I shall proceed, in the second place, to show that the method by which the erroneous results are obtained is essentially unsound.

Let

$$u=\int v\,dx,$$

where v is a function of x, y, and the differential coefficients of y with respect to x. Then by the Calculus of Variations we obtain

$$\delta u=\int \mathrm{A}(\delta y-p\delta x)dx+\mathrm{B},$$

where B stands for certain terms which are free from the integral sign, and A is a function of x, y, and the differential coefficients.

Now if u is to be a maximum or minimum, we must have, according to the received theory, $\mathrm{A}=0$; and this is admitted in the article in the July Number. And if there are more than one value of the ordinate corresponding to a given abscissa, the relation $\mathrm{A}=0$ must in general be satisfied at each point thus assigned.

Suppose that y' and y'' represent two values of y which correspond to one abscissa x; and let A' and A'' denote the corresponding values of A. Then we must have $\mathrm{A}'=0$ and $\mathrm{A}''=0$. This is also admitted in the article in the July Number; the equations $\mathrm{A}'=0$ and $\mathrm{A}''=0$ are expressed at full on page 52. Then from these the equation $\mathrm{A}'-\mathrm{A}''=0$ is deduced, and the following words are added:—"This last equation, inasmuch as it takes account of both values of A, is the one which the form of the curve is required to satisfy. To draw any inference from

one value of A and exclude the other would be nothing short of error."

I admit of course that the equation $\mathrm{A}'-\mathrm{A}''=0$ must be satisfied; but I do not admit that this is *the one* equation which the form of the curve is required to satisfy.

We have to satify *both* $\mathrm{A}'=0$ and $\mathrm{A}''=0$. We may of course try to assist ourselves by discussing the equation $\mathrm{A}'-\mathrm{A}''=0$; but any result which we deduce from the last equation will not be applicable to the problem we are solving, unless it makes *both* $\mathrm{A}'=0$ and $\mathrm{A}''=0$. It is therefore unnecessary to examine the validity of the process which is applied to the equation $\mathrm{A}'-\mathrm{A}''=0$, so long as it is obvious that the result does not make both $\mathrm{A}'=0$ and $\mathrm{A}''=0$.

That the equations $\mathrm{A}'=0$ and $\mathrm{A}''=0$ are not satisfied when we take for the required curve a segment of a circle, can be immediately ascertained by trial.

Or we may establish this assertion by referring the curve to polar coordinates. In this case we shall have only *one* value of r, corresponding to *one* value of θ; so that we have no occasion to consider two values of A. The differential equation in polar coordinates, as given in the Magazine for March, is

$$\frac{r\sin\theta\,(r+r'')}{(r^2+r'^2)^{\frac{3}{2}}}+\frac{r'\cos\theta-3r\sin\theta}{r(r^2+r'^2)^{\frac{1}{2}}}=\frac{\sin\theta}{\lambda},$$

where accents denote differential coefficients. Now the equation corresponding to a segment of a circle is $r=\mathrm{C}_1\cos(\theta-\mathrm{C}_2)$, where C_1 and C_2 are constants. It will be found immediately, on trial, that so long as C_2 is not zero the equation is *not* satisfied, whatever sign we give to the radical.

Thus it follows that the method which I am examining is opposed to the fundamental principles of the Calculus of Variations.

I wish to advert to one of the results enunciated in March, because I am uncertain whether it is still maintained, or is abandoned as erroneous. The result was enunciated thus:—"The solid consisting of a cylinder and two hemispherical ends of the same radius, is larger than any other solid of revolution having the same amount of surface and the same length of axis."

I urged two objections against this, one of them being that the fundamental equation $\mathrm{A}=0$ is not satisfied. I cannot agree with Professor Challis that he sufficiently meets the objection. It seems to me that there are only three ways in which the objection may be combated: (1) by showing that the equation $\mathrm{A}=0$ is satisfied; (2) by denying that it is necessary to satisfy the equation $\mathrm{A}=0$; (3) by showing that although it is in general necessary to satisfy the equation $\mathrm{A}=0$, yet there are special reasons which remove the necessity in the present case.

But the objection is not removed by reasoning which does not bear on these points: it is not removed, for example, by showing that the proposed solution satisfies $A=0$ at some points, and satisfies $p=0$ at all the other points.

It is almost superfluous to advance another argument against the untenable result; but I may just mention that, by changing the hemispherical ends into semispheroids, we can form a solid having the same surface and the same length of axis as that which is erroneously called the greatest, but having a greater volume.

Having shown by examples and by theory that the results given in the Numbers for March and for July are inadmissible, I shall proceed in the third place to offer some remarks as to the possibility of solving the problem with the condition of *continuity*.

The word *continuous* may have more than one meaning; but I think that the following remarks will apply with any meaning which is likely to be assigned.

The figure which by its revolution round the axis generates the solid of *greatest* volume with a given surface, is a figure formed of an arc of a semicircle and a straight line which coincides in direction with the bounding diameter. This figure will be regarded as non-continuous by those who seek for a continuous solution. But we know that we can in general draw a continuous curve through any assigned number of points, however large. Hence we can in effect make a continuous curve *coincide as nearly as we please* with the non-continuous curve which gives the greatest solid. The best method of conceiving this to be done is to employ the theorems which serve as the foundation for the expansion of functions in terms of sines and cosines of multiple angles.

It seems to follow from this consideration that it is in vain to seek for any solution, continuous or non-continuous, which differs from that determined by the semicircle and straight line.

Again, whether a solution be continuous or non-continuous, it must satisfy the fundamental equation of the Calculus of Variations which I have denoted by $A=0$; and it does not seem possible to satisfy this equation except in the manner indicated in the Magazine for June.

The very interesting investigation respecting the course of a ship, to which Professor Challis refers, was unknown to me when I published my 'History of the Calculus of Variations.' I regret this, because the subject of discontinuous solutions of problems in the Calculus of Variations appears to me important; I have given several examples, and I should have been glad to have included in my work a notice of every case which had been discussed. I venture to suggest, without, however, laying much

stress on the suggestion, that if there are reasons for asserting that the problem of the greatest solid of revolution ought to have a *continuous* solution, there will also be reasons for making a similar assertion for the problem respecting the course of a ship.

I will advert to one fact which, although not essential, will be of use in studying what has been written on the problem under discussion.

I have quoted above the ordinary formula

$$\delta u = \int A(\delta y - p\delta x)dx + B$$

as that which will probably be most familiar to readers of the Magazine. It has, however, been shown by some of the most eminent writers on the subject, that we may use the formula

$$\delta u = \int A\delta y \, dx + B;$$

A is the same in the two formulæ, but B is not. I retain the opinion that I have elsewhere expressed in favour of the second formula: it seems to me to be obtained in a more simple and intelligible manner than the first, and to be better adapted to the higher investigations by which we discriminate between a maximum and a minimum.

Although I do not admit that the articles in the Magazine for March and for July have contributed directly to the solution of the problem discussed, yet I am glad that attention has been again drawn to the subject. My own conviction is that the problem is no longer perplexing, but that its true solution is that which was stated and supported in the Magazine for June.

Cambridge, August 2, 1866.

XXVI. *On Molecular Physics.* *By* Prof. W. A. Norton*.

[Continued from vol. xxxi. p. 282.]

Magnetic Condition of the Sun.—The intimate magnetic relation subsisting between the earth and sun enforce, even in the present general exposition of terrestrial magnetism, a brief consideration of the probable magnetic condition of the sun. We have seen that the sun's surface must be traversed by magnetic currents developed in two ways,—(1) by reason of the sun's rotation about an axis; (2) by reason of the combined effect of its motion of rotation and its motion of translation through space (vol. xxxi. p. 280). According to the most reliable determinations the sun's progressive motion is directed toward a point whose longitude is 253° 16′, and north latitude 57° 27′, and with a velocity of $4\frac{2}{3}$ miles per second, while his velocity of rotation at the equator is 1·3 mile per second. Accordingly the currents developed from the second cause must originate at the parts of

* From Silliman's Journal for March 1866.

the sun's surface that have a heliocentric longitude of about 163°, and with a gradually decreasing intensity on both sides of that point. The north pole of this system of currents, as developed at any moment of time, will lie in the heliocentric latitude 33°, and longitude 73°. In the course of one complete rotation of the sun (25 days), this pole will be carried around the parallel of latitude which contains that point. The resultant of the currents thus developed that will traverse any locality at the end of one or more rotations will therefore run parallel, or nearly so, to the equator, like the currents that originate in the simple rotation. In all this we neglect the small inclination of the sun's equator to the ecliptic (about 7°); or suppose the equator and ecliptic to coincide. It appears, therefore, that the poles of all the permanent currents should coincide with the poles of rotation.

But it is important to observe that at every moment of time there will be, coexisting with the permanent currents, a system of new currents originating as above mentioned; and that therefore there will be a *secondary magnetic equator*, crossing the ecliptic in 163° and 343° of longitude, and a *secondary magnetic pole*, in longitude 73° and north latitude 33°. Or rather the nodes and pole should be somewhat to the east of these positions, since the currents developed must decline gradually and the rotation carry them forward. The individual parallel currents of this system must decrease in intensity in both directions from their equator, by reason of the increasing distance from the equator of rotation of the points of tangential action of the impulses of the æther and cosmical matter, and the decreasing size of the magnetic parallels followed by the currents. It will be readily seen, in view of the fact that the sun derives its magnetism in part from its motion toward a point in the northern celestial hemisphere, that the magnetic intensity of its northern must be greater than that of its southern hemisphere.

Origin of the Sun's Spots.—The systematic observations upon the sun's spots made by Carrington, Schwabe, Wolf, Secchi, and others, and especially the detailed discussion to which all the observations have been subjected by Professor Wolf, have served conclusively to establish that the sun's spots have their immediate origin in some action of the planets Jupiter, Saturn, Venus, and the Earth upon the photosphere of the sun, or in such action cooperating with some other cause. (See *Astronomische Nachrichten*, Nos. 839, 1043, 1091, 1137, 1150, 1160, 1173, 1181, 1185, 1223, 1234, 1270, 1289, 1294, 1355, 1526, &c.)

Professor Wolf has determined the epochs of maxima of the sun's spots for a period comprising 100 years; and finds that the period of the spots varies from 8 to 16 years, and that its

mean value is 11·15 years. He gives a formula for determining the spot-condition of the sun at different dates, in which the several terms represent the specific actions of the four planets just mentioned, dependent upon their masses, distances, and annual motions, and which gives results in close correspondence with the results of the observations made between 1826 and 1848 (*Astronomische Nachrichten*, No 1181). He has more recently extended his investigations so as to include, but with less certainty, a much longer period.

The epochs of maxima and minima from 1750 to 1856 are given in the following Table, to which we have added the corresponding mean heliocentric longitudes of Jupiter and Saturn, the two planets upon which the varying number of spots developed during a year chiefly depend.

Epochs of maxima.	Jupiter.	Saturn.	Epochs of minima.	Jupiter.	Saturn.
1750·0	4·42°	231·6°	1755·7	177·4°	301·4°
1761·5	353·4	12·2	1766·5	145·1	73·5
1770·0	251·4	116·2	1775·8	67·4	187·3
1779·5	179·7	232·4	1784·8	340·5	297·3
1788·5	92·8	342·4	1798·5	36·3	104·9
1804·0	203·3	172·0	1810·5	40·5	251·5
1816·8	231·8	328·5	1823·2	66·0	46·8
1829·5	257·2	123·9	1833·8	27·7	176·4
1837·2	130·9	218·0	1844·0	337·3	301·2
1848·6	116·9	357·5	1856·2	347·6	90·4

It will be seen that (omitting the results answering to the first two epochs of the Table, which will be separately considered) at the epochs of maxima Jupiter was in some position intermediate between the point toward which the progressive motion of the sun is directed (long. 253°) and the diametrically opposite point (long. 73°, which is the longitude of the secondary magnetic pole), reckoning from east to west from the first point to the second. The average position is 183°, or *in the vicinity of the descending node of the currents of the secondary magnetic equator* (p. 206). Again, omitting the epochs 1755·7 and 1766·5, at all the epochs of minima the positions of Jupiter fell in the other half of the ecliptic, and his average position was in long 23°. This is in the vicinity of the *ascending node* of the same equator, which lies somewhat to the east of 343° (p. 206). If we consider now the case of Saturn, we find that his average position at the epochs of minima was 182°, or 183° if we take the first two epochs into account. If we separate the positions that fall in the two halves of the ecliptic, lying on opposite sides of the line from 253° to 73°, we obtain the average positions 162° and 333°. If we include the first two epochs, the latter average becomes 348°. The average position of Saturn at the epochs of maxima was 236°.

If we now direct our attention to the first two epochs of maxima, we shall perceive that Jupiter was in that part of the ecliptic in which his ordinary action is the least; and if we refer to the first two epochs of minima, it will be seen that he was in or near the part of the ecliptic in which his ordinary action is the greatest. We must conclude, then, that from 1750 to 1766 the normal condition of things at the two nodes was greatly changed, and that the action of Saturn conspired with that of Jupiter to produce the anomalous results.

In view of the general results that have now been obtained, we may infer (1) that in general the action of a planet to produce spots is greatest in the portion of the ecliptic which contains the descending node of the secondary magnetic equator, and least in the opposite portion; (2) that the action is approximately the same at corresponding points on one side and on the other of either node. But the indications are that a somewhat greater liability to epochs of maxima exists when the planet is on the side of the line of the magnetic nodes toward which the progressive motion of the sun is directed than on the opposite side. This is most observable in the case of Saturn; for his average position at the epochs of maxima was 236°, while that of Jupiter was 183°.

By an examination in detail of the diverse positions of the operating planets at all the different epochs, and following their motions from one epoch to another, it may further be shown (1) that a planet operates more effectively before and after it has passed either magnetic node than at the very node; (2) that the normal positions of the two nodes are not far from 0° and 180° of longitude, the sun's rotation having the effect to displace them about 17° toward the east (p. 206); (3) that the principal maxima of planetary action occur at about the positions 135° and 230° on opposite sides of the descending node, and that under certain circumstances other positions of inferior maxima manifest themselves, lying in the vicinity and on opposite sides of the ascending node; (4) that the principal minimum falls at about 0°, or at the ascending node, and a secondary minimum at 180° (or the descending node); but in the normal state of things the effect of the planet appears to experience small changes in the space from 320° to 70°. These positions of maxima and minima are given here only as first approximations.

These results of observation, deducible for the most part from the Table we have given (p. 207), and confirmed by a detailed examination of Professor Wolf's entire series of determinations, are all decided intimations of a dependence of the sun's spots upon the varying magnetic or electric condition of the sun, and indicate that they are closely connected with the varying intensity of the magnetizing or demagnetizing action of the system of secondary magnetic currents developed by the sun's progressive motion, in conjunction with the cooperative action of the electric waves that proceed from the region (long. 253° and lat. 57°) upon which the impulses of the æther and cosmical matter fall normally. Of these two operative causes, the first should augment on both sides of the secondary magnetic equator, and for considerable distances. It should be much greater on the side of the descending than on that of the ascending node of this equator, because the new magnetic currents originate on the former side. The electric currents developed at any instant upon the ascending-node side of the sun run in the opposite direction, and tend to weaken the prevailing currents. The residual excess of the latter over the former at any point, and at any instant, constitutes the new effective magnetic current at that point and moment of time (*l. c.* p. 266). Each of these two sets of currents may also play a certain part in conjunction with the magnetic currents that result from them. It will be seen, then, that the results of observation which we have signalized (pp. 207–208) are, in general, such as should ensue if the operative causes here considered cooperate with some action of the planets to develope the spots, each tending to enhance the effect of the other.

The tendency of the second operative cause, if it conspires in this manner with the planetary action, should be to make that action apparently the greatest when the planet is about in long. 253°; and therefore the region of the spots developed by the planet (which lies near the ecliptic on the side of the sun turned toward the planet) is nearest the central point of origination of the waves in question. On the other hand, the tendency of the same general cause should be to make the effect of the planet least at the diametrically opposite point (long. 73°).

It may be seen that if the three causes here supposed to be continually in operation—and to be cooperating more or less according to the positions of the planets with respect to the special points of maxima and minima alluded to, at the same time that the individual planets are conspiring more or less with each other according to their relative positions—should determine spots by their conjoint action; their effects, in respect to the connexion of the epochs of maxima and minima with certain positions of the planets, and the lengths of the periods comprised between these epochs should correspond, in the main, with the results of observation.

From the point of view we have now reached we may gain an insight into the probable nature of the process of origination of the sun's spots. Conceiving the luminous matter of the sun's photosphere to be endued with the properties we have recognized in those emanations of solar matter that enter the earth's photosphere (*l. c.* pp. 275–280), we may regard it as inductively magnetized by the sun's magnetic currents, and disposed in the lines of magnetic polarization, and probably also distributed into separate masses having in the various latitudes all the diverse directions of the sun's directive force. We have reason to suppose that in the upper portions of such masses the molecules on each line of polarization will be so widely separated as to be subject to an effective force of molecular repulsion from the sun (*l. c.* p. 272), and that in the state of equilibrium this is neutralized by the magnetic attraction between contiguous molecules. Now such a state of equilibrium may be disturbed, and the matter expelled to an indefinite distance in three ways,—(1) by demagnetization; (2) by electric discharges along the lines of polarization; (3) by both of these causes operating together. A demagnetization may result, as we have seen, from the new currents developed in the upper photosphere by the tangential action of the æther and cosmical matter; and electric discharges may ensue in consequence of the propagation of electric waves in every direction from the region (in long. 253°, and N. lat. 57°) that receives the impulses from the æther and cosmical matter normally. The points of greatest and least demagnetization should be such as have been already indicated (p. 209). But the photospheric matter should also be subject to the molecular repulsion of the masses of the different planets; and one effect of the impulses of this force should be to develope electric waves or currents proceeding from the region normally exposed to them. Such waves are of the same character, and originate essentially in the same manner as the "radial currents" that we have recognized as playing a conspicuous part in terrestrial magnetic phenomena (*l. c.* pp. 271–272). They should be most energetic, as in the case of these radial currents, at a certain moderate distance from the point directly under the planet, *i. e.* in low latitudes. On the other hand, it is to be observed that for a certain distance from this point the repulsive force of the planet may check the expulsion of the solar matter by its direct action.

The tendency to the formation of spots should be wanting at the permanent magnetic equator (or rotation equator), because the lines of polarization are there parallel to the surface and the induced magnetism feeble. Again, the effect of demagnetization should be greatest in low latitudes, where the total magnetic intensity is the least, and where also the new demagnetizing cur-

rents are most energetic. To this we may add, that as we have reason to believe that the temperature of the *mass* of the sun decreases toward the poles (*l. c.* pp. 269–270), the molecules of the photospheric matter may be less widely separated there, or be combined in groups so as to be subject to a less energetic force of repulsion from the sun. It is conceivable, too, that in special localities the electric waves or currents originated by the planets may operate to disunite molecules in the act of combining or condensing at the surface of the photosphere (the state of things supposed by Faye), and so bring them into the condition to be repelled and completely dispersed by the repulsive force of the sun. The process of dispersion having once begun, from any cause, may extend indefinitely downward.

There is a special mode of origination of spots, connected with the sun's motion in space, that has not yet been noticed. It is that the cosmical matter as it flows away from the region of normal impact toward the equator will become demagnetized, and thus initiate the process of dispersion of certain portions of the photospheric matter. The effect will be especially produced on the opposite side of the pole from the point of normal impact, as the changes of the induced magnetism will there be the greatest. A similar effect may ensue from a flow toward the equator of the matter at the very surface of the photosphere, produced by the impulses of the cosmical matter or æther. The phenomena are precisely similar to the auroral phenomena that light up occasionally the earth's photosphere (*l. c.* p. 280). This is undoubtedly the origin of the annual maximum of spots after the autumnal equinox detected by Professor Wolf. It is the result of the effects previously noticed, augmented by that here considered. An inferior maximum manifests itself in December, which is the direct result of this cause alone. (See *Astronomische Nachrichten*, Nos. 1043 and 1223.) It is important to observe here that when a planet is on that side of the sun, or in the vicinity of the ascending magnetic node before alluded to, it tends to check this southerly flow of matter at the surface of the photosphere, and so prevent the development of spots from the cause in question. We may add that we have doubtless another revelation of its operation in the predominating irregular disturbances of the magnetic needle in the autumnal months (or from August to December inclusive; see Professor Bache's Reports). The secondary maximum of the annual inequality of spots and magnetic disturbances near the vernal equinox is to be chiefly attributed to the demagnetization in the vicinity of the descending magnetic node already considered.

The spots are more numerous in the northern than in the southern hemisphere of the sun, because the low latitudes at which they chiefly originate lie, in the northern hemisphere, nearest the region (long. 253°, N. lat. 57°) from which the electric and material currents radiate, and because in the northern hemisphere alone is the flow of the material currents in the direction to be attended with a demagnetization. Again, the spots are confined to a narrower belt in the northern than in the southern hemisphere, because, the magnetic intensity of the northern hemisphere being the greatest (p. 206), the ordinary limiting parallel of the spots (which is the circle at which the demagnetizing action from either of the two causes specified, together with the electric discharges along the photospheric columns, cease to be sufficient to effect the dispersion of these columns) must lie nearest the equator in that hemisphere.

It is to be observed that spots may arise from electric discharges along the photospheric lines of polarization, although no demagnetizing action may come into operation; and it is even possible that indirectly an increase of magnetic intensity may, in special cases, cooperate with such currents.

Note.—Since the foregoing was written, the line of investigation here entered upon has been followed up, and new and important general results obtained. One of the principal results is, that the *density or quantity of matter of the sun's photosphere, in the region of the spots, experiences periodical augmentations*, in consequence of certain effects produced by Jupiter and Saturn while passing by the first quadrant of heliocentric longitude and the sun's north magnetic pole, and that these augmentations of density, in connexion with the subsequent diminutions, are one of the determining causes of the great variations that occur in the length of the period of the sun's spots (viz. from eight to sixteen years). Another cooperative cause consists in the diverse positions of the planets, *especially of Venus*, with respect to positions of favourable action, at the epochs of heliocentric conjunction with the earth. The other causes have been intimated.

[To be continued.]

XXVII. *Optics of Photography.—On a new Process for Equalizing the Definition of all the Planes of a Solid Figure represented in a Photographic Picture.* *By* A. Claudet, *F.R.S.**

ONE of the greatest deficiencies of photography in the representation of solid figures is the impossibility of obtaining a well-defined image of all the various parts situated on different planes; for it is well known that the best object-glasses can give a sharp image only for the plane in focus; the images of the objects situated before and behind are more and more confused as they are more and more distant from that plane.

* Communicated by the Author, having been read at the British Association, Nottingham Meeting, 1866.

To obviate this, or rather to equalize the effect to a certain degree, it is customary to reduce as much as possible, by means of diaphragms, the aperture of the lens. The object of such diaphragms is to cut off all the oblique rays, and to employ only the rays which emerge from the lens at the least angle possible. It is evident that when the rays are emerging from the centre of the lens, they follow a course so nearly parallel that any equal points of the object on various planes are included between spaces not varying much in size; so that although these points are distant from the plane which is represented at the mathematical focus with the greatest definition, they form their image within a circle of confusion so near the circle of definition that the eye cannot easily detect the difference, and the image of the solid figure appears well defined in its various planes.

But this result cannot be obtained without sacrificing a great amount of the light which falls on the lens and is stopped by the diaphragm; consequently the time of exposure for the formation of the image is to be increased as much as the surface of the lens has been reduced. It is then obvious that, in the case of portrait-taking, the advantage which would be gained in point of definition is lost by the unavoidable unsteadiness of the sitter, and at all events is more than counterbalanced by the constrained expression resulting from a long sitting.

Even supposing that the person could sit sufficiently long without moving, and preserve all the while the same expression, it is a question not difficult to decide in an artistical point of view, whether a photographic portrait showing all the pores and asperities of the skin, with the smallest of its wrinkles, would ever be an agreeable or artistic production.

Excessive minuteness is the greatest reproach which has been made by artists to the best photographic portraiture; and in order to obviate it, some have gone so far as to suggest that it would be desirable that photographers should take their portraits *a little out of focus*. But these artists, forgetting certain laws of optics, failed to observe that it was impossible to represent the whole of the figure in *the same degree* out of focus. If, for example, the nose was a little out of focus, the eyes would be considerably more so, and the ear still more; in fact some parts of the figure would be quite indistinct and confused, whilst one part only would be a little softened down by a slight deviation from the plane of sharp focus.

Although such a method is therefore unavailable, this suggestion, being made in a true spirit of progress, was worthy of consideration; and a very useful lesson was to be learned from the well-meant recommendation that photographic portraits, to be agreeable and artistic in effect, should not partake too much of the mathematical truth which is inherent to the action of the most perfect lenses, and which is particularly observable in the part of the image situated in the plane of the exact focus of the lens.

Convinced myself of the advantage which, in an artistical point of view, would result from photographic portraits being taken in such a manner that they should as much as possible resemble a work of art, in which all the features are marked by light touches of the brush or pencil, softly blending from light to shade, such an important subject has for a long time occupied my attention. My precise object has been to discover a method of removing, if possible, from photographic portraiture that mechanical harshness which is due to the action of the most perfect lenses.

In the best works of art all the effects are produced by a soft and harmonious treatment; nothing is hard or dry, nothing is too minutely delineated: in fact the hand of the artist is not capable of microscopic correctness—and fortunately so, for its work is not intended to be examined by a magnifying lens; still the general effect may be sufficiently minute for the artistic purpose.

Notwithstanding its defects, photography is the great teacher to artists: they find in it the true reflex of nature; it shows the correct distribution of light and shade with all its delicate half tints; its perspective drawings are perfect, and it represents the folds and texture of draperies in the most exquisite manner. But if art derives a great advantage from the imitation of photographic productions, art is in its turn a very competent and valuable teacher of photographers. Their works indeed have no value if they do not partake of a certain character which distinguishes the best works of art. And therefore photographers must not despise the recommendations of true artists; for in trying to imitate art they will often improve their own productions. Therefore as artists have nothing better to do than to imitate photography, so photography has nothing better to do than to be guided by art.

By the laws which regulate the action of lenses, it happens, as has been already pointed out, that in the representation of a solid figure there is strictly only one plane of that solid which can be taken in perfect focus. The image therefore of that plane is not in harmony with the images of the other planes, which are not so sharply defined. This inequality in the texture of the image cannot but be considered a defect; and it would be a great advantage if it were possible to equalize the effect, even at the cost of losing the mathematical accuracy of the plane in focus. I hope to show indeed that such a loss would be really a considerable gain. If photographic portraits should not exhibit all the pores, wrinkles, and defects of the skin, it is still less desi-

rable that only one part of the face should be in that condition, while all the others should gradually lose their sharpness as they are more and more distant from the plane of definition.

We can at will bring the focus upon any plane of the figure. In taking portraits not smaller than miniature size, we may choose the nose, the eyes, or the ears; but we cannot have the three equally sharply defined; and photographers endeavour to focus upon a middle plane, for example upon the eyes, in order to have the nose and the ears in the same degree of less perfect definition, not very far from that of the eyes.

Perfection in the portrait would be attained, were it possible to do so, first by taking the image of the nose, then, after having altered the focus, the image of the eyes, and finally, after again altering the focus, the image of the ear, and then, from these various images, forming a collective portrait. Such an idea may appear impracticable, possibly even absurd, and it is sure on first thoughts to be rejected and condemned. Yet I seriously, and after mature consideration both of the practice and of the theory of such a scheme, propose its adoption as one of the greatest improvements which will have been introduced in photography since its discovery. I beg to be allowed to explain the method in which I conceive I have solved the difficulty I have above alluded to.

Let me premise a few words upon the effect produced by the experiment of taking the photographic image of the focimeter. This instrument, I may be permitted to remark, was invented by me upwards of twenty years ago, and has been constantly used in my operating-room in order to test in what degree the chemical and visual foci of lenses coincided or differed. Until it came into use, nobody had ever dreamt that they did not exist in the same plane when the object-glasses were as much achromatic as those of the best telescopes. This fact being demonstrated by the instrument I refer to, was the cause of a complete change in the construction of lenses for photographic purposes; and from that time opticians have endeavoured to calculate, and succeeded in discovering curvatures the combination of which, to invent a phrase, achromatize, with the visible rays of light, the invisible rays which are exclusively endowed with the chemical action. The use of the focimeter I have found indispensable since the further discovery I have made that the two foci undergo continual changes from various atmospheric influences; and no photographic studio, therefore, should be without this instrument; for no optical combination is capable of preserving an invariable coincidence of foci, and the photographer must have the means at any moment of testing the then state of the elements, and of the light itself, in order to ascertain any change in its refraction and to act accordingly.

My focimeter, a model of which is on the table, is made of eight separate segments of a disk, mounted spirally on a horizontal axis of 12 inches, corresponding with the optical axis of the lens; the segments are all separated and distant about $1\frac{1}{2}$ inch from each other. In a front view they form on the ground glass the image of a complete and regular disk. The segments are covered with some uniform and well-defined devices; and the centre of each is marked with its number, from 1 to 8. The first segment is the nearest, and the last the furthest from the lens.

By moving slightly and slowly forward and backward the focusing or ground glass, any one of these segments, and all in succession, may be brought into focus. If we focus upon No. 4, for example, we see that the segments before and behind gradually lose their sharpness, in a greater or less degree, according to the quality of the lens; and from that experiment we may judge of what is empirically called the depth of focus of the lens. By comparing at the same time the photographic image with the image we had on the ground glass, we see if the visual and chemical foci agree, or to what extent they differ. But our present object not being to test whether the chemical and visual foci agree, we will take a lens in which we know that they coincide.

Now, supposing that we focus upon No. 1, we shall find that the photographic image of that segment will be very well defined, No. 2 a little less, No. 3 and all the others until No. 8 gradually losing their sharpness, so that No. 8 will be the most indistinct. In the same way, if we take a portrait so that the nose is on the plane of No. 1, this part of the face will be well defined; the eyes, which are on the plane of No. 2, will be a little less well defined; the ear, on the plane of No. 3, still less defined; and if the body is obliquely turned, the shoulder, which corresponds with the plane of No. 8, will be considerably confused.

Experimenting again upon the focimeter, let us suppose that, after having operated with No. 1 in focus, we move the frame holding the plate to a point previously marked on the camera-board where No. 8 is in perfect focus. If we then expose the plate a second time, or rather continue the exposure, we shall find that upon the first confused image of No. 8 a new image well defined has been impressed, and at the same time a confused image of No. 1 will have been impressed upon the first image of No. 1 which was well defined.

In examining the result, we shall find it better than if the second impression on both segments, No. 1 and No. 8, had not been taken. In the middle of a confused image of No. 1 and No. 8 we shall have one perfectly defined, the whole having the appearance of the shadow of a pin not quite in contact with the surface; that shadow being slightly blended from dark to light, but still sufficiently defined to show the exact form and size of the pin.

Now what has been done for the two extreme segments of the focimeter Nos. 1 and 8, can consecutively be done for the intermediate segments Nos. 2, 3, 4, 5, 6, 7, and 8; and in fact it is unavoidably done during the movement of the plate from No. 1 to No. 8; and the result is that every segment has the image of any small spot delineated upon it as if that spot was seen through a thin vapour.

This being well understood, let us apply the same mode of operating to the taking of a portrait; and while the person is sitting, let us move the frame holding the plate from the point of the focus of the nose to the focus of the furthest point of the figure. It is evident that during the movement of the plate the various planes of the figure will have been consecutively *in* focus and *out of* focus during one part of the exposure, and all in the same degree. Thus we have by a very simple contrivance found the means to realize the wish of true artists, viz. to take a photographic portrait without hard lines, but with the light and shades blended in the most artistic harmony.

We now arrive at the most important part of the discovery. The result may be obtained in greater perfection without having to move the frame holding the plate in order to adapt it consecutively to the focus of each of the planes of the figure. In moving the frame, it is evident that in one direction we increase, and in the other we reduce the size of those parts of the image which are consecutively brought into focus. The result is to exhibit more conspicuously than when these parts were out of focus the exaggeration of perspective which is inherent to all photographic representations taken by lenses not very distant from the figure—an exaggeration, I may remark, so disagreeably apparent in all large portraits taken by too short-focus lenses. To obviate this increase or reduction of the size of the image of the various planes of the figure, it would be necessary, if this were practicable, during the operation to change the lens and rapidly to substitute another having a focus appropriate to the distance of the new plane without altering the distance of the plate, so that the plate should not have to be moved forward or backward for the adaptation of the various foci according to each distance of plane.

It happens fortunately that this change of foci may be effected with the same object-glass when that object-glass is a double combination of lenses. The focus and power of such double combination being the result of the distance which separates the two lenses, it may be increased or reduced merely by altering that distance. Now if during the operation we bring nearer or further the two lenses, by this simple means we adapt the focus of every plane to the immoveable frame holding the plate; and we are enabled thus to represent consecutively on the plate an image of every plane, with a less reduction or increase of size than when the power of the double combination remains the same; for it happens fortunately that, to reduce the focus, we must separate the lenses, by which the power is increased. The alteration of the distance which separates the two lenses is effected by a rack and pinion acting upon a tube containing the back lens, that tube sliding into another containing the front lens, which remains fixed during the adaptation of the focus to the distance of every plane by means of a gradual movement communicated to the back lens during the sitting. The inspection of the apparatus, which I submit to the Meeting, will enable any visitor interested in the question to understand its action.

It is marvellous when we reflect that there is nothing to wish for in the shape of contrivances having for their object the perception of vision, and that from time to time man invents, or thinks he invents, what nature had done in the most perfect manner. The eye is supplied with a lens in the same way as the camera obscura; the retina is the screen on which, like the ground glass of the camera, the light reflected by all the natural objects form their image. By various humours through which the light is refracted, the spherical aberration is corrected and the most perfect achromatism is produced; the eye is endowed with muscles which enable it to alter the focal distance of the lens according to the various distances of the objects. Optics is able to imitate all these beautiful contrivances except the last, which is available only on account of the way in which we exercise the perception of vision. We see at once only a very small part of the image—that part which is projected on the centre of the retina; and the eye can adapt its focus to the distance of that part, and, as rapidly as thought, when directing its attention to another part it adapts its focus to that new distance. Therefore it matters not whether the other parts are in focus; we have only the perception of what we want to see, and, by the proper adaptation, that sensation conveys to our mind only a well-defined image. It cannot be so with the camera obscura, because, the photographic image produced by it being at once permanently fixed entire by the same exposure, we cannot change it in changing the focus; the only thing we can do is to impress a stronger image on a fainter image. The artificial optical instrument being destitute of a self-acting changing adaptation to the focus of all the other planes, can represent only one plane *in focus*; but if it had that adaptation, the surface receiving the impression of the image in a permanent manner (not like the retina, which does not retain the impression), that impression would consist of a number of

images superposed one upon another. For this reason nobody would ever have thought of proposing to employ a lens which, moving during the exposure, would adapt itself consecutively to the foci of all the other planes of the image. But from the fact that the eye can easily and usefully alter its focus according to the distance of the plane it wants to examine, and unconsciously discard the image of the other planes while they are out of focus, it is possible to learn what may be a very useful modification of the artificial optical instrument called the camera obscura. If we cannot discard the superposed images out of focus, and see only among them the one in focus, it happens fortunately that the image in focus is stronger, better defined, and consequently more conspicuous than all the others. If we cannot discard entirely the images out of focus, they at all events appear only like a number of blended shades of the principal image. Therefore in this process for changing the power and the focus of the double combination of lenses according to the distance of the various planes, we do nothing but imitate one of the most beautiful and indispensable of natural contrivances, by which the eye is so wonderfully well calculated to perform all the exigencies of perfect vision, and is one of the most marvellous and splendid works of the Creator.

This new plan of operating not requiring a longer sitting than the old process, the interposition of the usual diaphragms will, by cutting off the oblique rays, increase the definition of the compound image. It follows that, as much as the intensity of light will allow, the smaller the aperture of the diaphragm is, the more perfect will be the result.

One of the great advantages of the method I have described is that the various planes of the figure are represented with the same intensity of light, which is not the case when the rays are more condensed on the plane of exact focus than on the other planes. For it is obvious that the difference of intensities of light on the various planes produces an unnatural effect, and destroys so far the harmony of the picture.

I have felt justified in bringing this matter before the Association, from the confident hope that, by the examination of scientific photographers, a new era may henceforth begin in the art of photography. If the plan I propose is in its present state deficient in many practical points, as must be the case in almost all new inventions, I am sure that, with the cooperation of so many ingenious and active minds which are constantly engaged in the task of progress, the science of optics will be able to supply photographers with a camera obscura which in its working will approach as near as an artificial instrument can approach the beautiful instrument which gives to man the most perfect perception of all the wonders and beauties of nature.

XXVIII. *Notes on Mineralogy. By the* Rev. Samuel Haughton, *M.D., Fellow of Trinity College, Dublin**.

[Continued from vol. xxiii. p. 52.]

No. XII. *Analysis of Sombrerite.*

THIS rock-mineral is found at Sombrero and other isles of the Antilles, and is imported into Liverpool as a mineral manure. It is formed from the atmospheric decomposition of Guano, and contains numerous casts of land shells.

Sombrerite.

	Per cent.
Silica	0·08
Phosphate of lime and alumina	89·64
Carbonate of lime	5·00
Chloride of potassium	2·81
Fluoride of calcium	0·10
Water	0·60
	98·23
Loss	1·77
	100·00

No. XIII. *Analysis of Pitchy Iron Ore from Kilbride, Glenasplinkeen, co. Wicklow.*

This ore is associated with Limonite and Psilomelane, and occurs in considerable quantity. It forms one of the large group of hydrated peroxides of iron, with a larger amount of water than is commonly found.

It contains no sulphur, no protoxide of iron, and no organic matter.

Pitchy Iron Ore, Kilbride.

	Per cent.
Peroxide of iron	77·15
Water	20·43
Silica	0·30
Alumina	trace
Phosphoric acid	1·60
	99·48

The result of this analysis is

$$Fe^2O^3 + 2\tfrac{1}{3}HO.$$

That of Göthite is

$$Fe^2O^3 + HO;$$

and of Limonite

$$Fe^2O^3 + 1\tfrac{1}{2}HO.$$

* Communicated by the Author.

No. XIV. *Analysis of Green Orthoclase from Greenland.*

	Per cent.	Atomic constitution.		
Silica	64·40	1·431	1·431	4
Alumina	18·96	0·364	0·377	1
Peroxide of iron	1·04	0·013		
Lime	0·45	0·016	0·377	1
Magnesia	0·14	0 007		
Soda	2·35	0·076		
Potash	13·07	0·278		
	100·41			

This orthoclase was brought to Dublin from Greenland by Sir Charles Giesecké, and is beautifully crystallized in flat tabular masses. It gives the usual formula,

$$RO, SiO^3 + Al^2O^3, 3SiO^3.$$

No. XV. *Analyses of Volcanic Rocks from New Zealand.*

I am indebted to Dr. Lauder Lindsay for the opportunity of making the following analyses of volcanic rocks from Dunedin in New Zealand.

1. *Basalt*, with visible crystals of Augite and Chrysolith. Dunedin, New Zealand.

	Per cent.
Silica	46·60
Alumina	16·80
Peroxide of iron	7·28
Protoxide of iron	5·76
Protoxide of manganese	0·72
Lime	9·65
Magnesia	6·89
Soda	6·78
Potash	2·08
Titanic acid	trace
	102·56

2. *Vesicular Augitic Lava*, with cavities destitute of lining crystals. Mount Eden, Auckland, New Zealand.

This lava is divisible into two portions, soluble and insoluble in muriatic acid :—

Soluble	38·2
Insoluble	61·8
	100·0

It contains only a small quantity of carbonates, and seems to have undergone but little metamorphic action.

	Insoluble. grs.	Soluble. grs.
Silica	33·20	13·50
Titanic acid	1·10	0·31
Alumina	8·80	2·90
Peroxide of iron	2·14	0·60
Protoxide of iron	2·70	5·70
Protoxide of manganese	0·16	0·10
Lime	5·40	2·52
Magnesia	2·76	8·55
Soda	3·74	2·23
Potash	0·54	0·23
	60·54	36·64
Loss	1·26	1·56
Total	61·80	38·20

3. *Vesicular Augitic Lava*, with cavities lined with white crust. Dunedin, New Zealand.

This lava, like the last, is divisible into two portions, soluble and insoluble in muriatic acid:—

	Per cent.
Soluble	40·40
Insoluble	59·60
	100·00

It contains a large quantity of carbonates, due to metamorphic action.

	Insoluble. grs.	Soluble. grs.
Silica	33·00	9·24
Titanic acid	0·80	0·30
Alumina	9·00	4·44
Peroxide of iron	2·09	4·07
Protoxide of iron	1·15	4·43
Protoxide of manganese	0·16	0·10
Lime	8·08	2·13
Magnesia	3·04	6·09
Soda	1·76	0·83
Potash	0·88	0·21
	59·96	31·84
Gain	0·36	
	59·60	
Carbonic acid, water, and loss		8·56
		40·40

The portion of a lava that is soluble in muriatic acid consists of carbonates, magnetic oxide, and an unknown silicate of alumina and soda; it therefore varies considerably according to the

degree of metamorphism the lava has undergone subsequent to its emission. On reducing the insoluble parts of the preceding lavas to percentages, we find—

Insoluble portion of Dunedin Lava.

	No. 1.	No. 2.
Silica	53·72	55·37
Titanic acid	1·78	1·34
Alumina	14·24	15·10
Peroxide of iron	3·46	3·51
Protoxide of iron	4·37	1·93
Protoxide of manganese	0·26	0·27
Lime	8·74	13·56
Magnesia	4·46	5·10
Soda	6·05	3·00
Potash	0·87	1·47
Loss or gain	+2·05	−0·65
	100·00	100·00

This seems to be a mixture of Labradorite and Augite, and is very constant in the two specimens examined.

No. XVI. *On the Chemical Composition of four Zeolites, presented by* Colonel Montgomery *to the Geological Museum of Trinity College, Dublin.*

Some months ago, Colonel Montgomery presented to the Geological Museum of Trinity College some fine specimens of Zeolites found by him in the Bombay Presidency, four of which seemed to me worthy of chemical analysis and of being recorded.

No. 1. *Apophyllite.*

This mineral occurs in fine clear crystals coating the foliated Stilbite No. 2.

These crystals occur in the dimetric system.

Its chemical analysis gave the following results:—

		Oxygen.	
Silica	51·60	26·791	26·902
Alumina	0·24	0·111	
Lime	25·08	7·130	8·175
Magnesia	0·08	0·031	
Soda	0·63	0·160	
Potash	5·04	0·854	
Water	16·20	14·399	
Fluorine	0·97		
	99·84	49·476	

This analysis agrees very well with those of Apophyllite given in the books, but it is very difficult to assign its rational formula. It has been proposed to borrow as much oxygen from the water as, added to that of the protoxides, would give the formula

$$3RO, 2SiO^3 + 2HO;$$

and such a substitution is mathematically possible in this example; I have no faith, however, in such imaginary combinations.

The Stilbite (No. 2) occurred in flat radiated crystals, readily distinguishable by the eye as those of Stilbite. Its analysis gave—

No. 2. *Stilbite*.

		Oxygen.	
Silica	58·20	30·217	
Alumina	15·60	7·291	
Lime	8·07	2·294	
Magnesia	none		2·574
Soda	0·49	0·125	
Potash	0·92	0·155	
Water	18·00	16·000	
	101·28	56·082	

This analysis gives very well the usual formula of Stilbite, regarded as an hydrated lime orthoclase,

$$RO, SiO^3 + Al^2O^3, 3SiO^3 + 6HO.$$

The foregoing analysis may be compared with that of very finely-crystallized Stilbite, found by Mr. Jacob in the Nerbudda Valley, which was published by me in Note V. on Mineralogy, in the Philosophical Magazine for July 1857.

The next mineral to be described is especially interesting, because it seems to set at rest the controversy as to the specific identity of Hypostilbite, and fully establishes the title of that mineral to be regarded as a distinct species, and not a degradation of Stilbite produced by hydrous metamorphism. It occurs in large, fibrous, transparent masses, radiated like Natrolite or Thomsonite, and filling globular cavities in green trap.

No. 3. *Hypostilbite*. Spec. grav. = 2·180.

		Oxygen.	
Silica	52·80	27·414	
Alumina	17·12	8·001	
Lime	7·89	2·242	
Magnesia	trace		2·854
Soda	2·35	0·601	
Potash	0·07	0·011	
Water	18·52	16·462	
	98·75	54·731	

This analysis is very like that published by me, of a specimen of Hypostilbite from Skye (Phil. Mag. July 1857), and also resembles the original analysis of Beudant. It may be regarded as an hydrated lime oligoclase, with the formula

$$RO, SiO^3 + Al^2O^3, 2SiO^3 + 6HO.$$

The fourth specimen of Zeolite to be described occurs in large massive nodules, filling cavities in trap, of feathery structure, and apparently metamorphic in origin.

No. 4. *Harringtonite*. Spec. grav. = 2·174.

		Oxygen.	
Silica	45·60	23·676	
Alumina	27·30	12·760	
Lime	12·12	3·444	
Magnesia	trace		4·257
Soda	2·76	0·707	
Potash	0·63	0·106	
Water	12·99	11·545	
	101·40	52·238	

This mineral is identical with the Harringtonite of the books; but it seems difficult to assign its true formula.

No. XVII. *On the Geometrical Forms of Gall-Stones.*

The gall-stones examined by me, whose geometrical figures are here noticed, were taken, after death, from six individuals; and I am indebted, for the opportunity of describing them, to Dr. Banks, Dr. Fleming, Dr. Foot, and Mr. Duffey.

The dihedral angles were measured by the goniometer, and may be classified as follows:—

No. I. *Angles nearly equal to* 90°.	°	′	No. II. *Angles nearly equal to* 70° 31′·7.	°	′
1.	90	0	1.	74	15
2.	90	15	2.	75	0
3.	90	0	3.	72	30
4.	90	0	4.	69	30
5.	94	30	5.	71	0
6.	90	0	6.	70	0
7.	90	0	7.	71	30
8.	90	0	8.	72	30
9.	91	0	9.	67	0
10.	90	0	10.	66	0
11.	89	15	11.	67	0
12.	88	0	12.	72	0
13.	86	9	13.	72	30
14.	89	30	14.	72	0
15.	92	0	15.	73	0
16.	94	0	16.	72	0
17.	94	0	17.	70	0
18.	90	0	18.	68	0
Mean	90	28¼	19.	73	0
			Mean	70	59

No. III. *Angles nearly equal to* 109° 28′·3.		No. IV. *Angles nearly equal to* 116° 26′.	
1.	109° 0′	1.	117° 0′
2.	104 0	2.	122 30
3.	108 30	3.	118 0
4.	104 0	4.	117 30
5.	105 0	5.	118 0
6.	102 0	6.	123 0
7.	112 0	7.	122 0
Mean . .	106 21	Mean .	119 42

The angles already described are those belonging to four of the geometrical solids inscribable in a sphere, viz.—

1. Hexahedron . . .	90°	18 angles.
2. Tetrahedron . . .	70 31·7	19 „
3. Octahedron . . .	109 28·3	7 „
4. Dodecahedron . . .	116 26	7 „
	Total . . .	51 „

The remaining angles observed cannot be reduced to any of the preceding, but may be classified empirically as follows:—

No. V. *Angles observed.*		No. VI. *Angles observed.*	
1.	60° 0′	1.	99° 0′
2.	59 30	2.	97 0
3.	52 0	3.	99 30
4.	59 30	4.	101 0
5.	60 0	5.	99 0
6.	64 0	6.	97 0
Mean .	59 9	7.	101 0
		Mean .	99 4

No. VII. *Angles observed.*	
1.	82° 0′
2.	82 0
3.	80 0
4.	78 0
5.	77 0
6.	81 0
7.	79 0
8.	83 0
Mean	80 15

The total number of observations in Nos. V., VI., VII. are—

No. V. . . .	59° 9′	6 angles.
No. VI. . . .	99 4	7 „
No. VII. . . .	80 15	8 „
		21 „

The foregoing investigation warrants the belief that gall-stones owe their geometrical forms to mutual pressure in the gall-bladder and duct, and not to crystalline forces.

[To be continued.]

XXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 152.]

May 3, 1866.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

"The Calculus of Chemical Operations, being a Method for the Investigation, by means of Symbols, of the Laws of the Distribution of Weight in Chemical Change.—Part I. On the Construction of Chemical Symbols." By Sir B. C. Brodie, Bart., F.R.S.

In chemical transformations the absolute weight of matter is unaltered, and every chemical change, as regards weight, is a change in its arrangement and distribution. Now this distribution of weight is subject to numerical laws; and the object of the present method is to facilitate the study of these laws, by the aid of symbolic processes. The data of the chemical calculus, as indeed of every other application of symbols to the investigation of natural phenomena, are supplied by observation and experiment; and its aim is simply to deduce from these data the various consequences which may be inferred from them. The province of such a method commences where that of experiment terminates.

This part comprises the consideration of the fundamental principles of symbolic expression in chemistry, and also the application of the method to the solution of perhaps the most important of all chemical problems, namely, the question of the true composition, as regards weight, of the units of chemical substances.

Section I. In the first section certain definitions are given of those weights, and relations of weight, of which the symbols are subsequently considered. It may be regarded as containing an analysis of the subject of chemical investigation. The definitions are, of "a chemical substance," "a weight," "a single weight," "a group of weights," "identical weights," "a compound weight," "a simple weight," and "an integral compound weight."

The unit of a chemical substance is defined as that weight of the substance which at 0° Centigrade and 760 millims. pressure, and in the condition of a perfect gas, occupies the volume of 1000 cubic centimetres. This volume is termed the unit of space.

Section II. The second section treats of symbolic expression in chemistry. A "chemical operation" is defined as an operation of which the result is a weight. These operations are symbolized by letters, x, y, &c. An interpretation is assigned to the symbols $+$ and $-$ as the symbols of aggregation and segregation—that is, of the mental operations by which groups are formed. The symbol $=$ is selected as the symbol of chemical identity; the symbol 0 as the symbol of the absence of a weight, this symbol being identical with $x-x$. The symbol $(x+x)$ is the symbol of two weights collectively considered and as constituting a whole.

The symbols xy and $\frac{x}{y}$ are selected as the symbols of compound weights; and it is proved that with this interpretation these symbols are subject to the commutative and distributive laws

$$xy=yx,$$

$$x(y+y_1)=xy+xy_1,$$

and also to the index law

$$x^p x^q=x^{p+q}.$$

Section III. treats of the properties and interpretation of the chemical symbol 1, which is selected as the symbol of the subject of chemical operations, namely, the unit of space. With this interpretation the chemical symbol 1 has the property of the numerical symbol 1 given in the equation $x1=x$.

Section IV. Chemical symbols are here shown to be subject to a special symbolic law, given in the equation

$$xy=x+y.$$

This property, by which chemical symbols are distinguished from the symbols employed in other symbolic methods, is termed the "logarithmic" property of these symbols. A consequence of this property is that $0=1$, and that any number of numerical symbols may be added to a chemical function without affecting its interpretation as regards weight.

Section V. relates to the special properties of the symbols of simple weights, which are termed prime factors, from their analogy to the prime factors of numbers. These symbols differ, however, from

these factors in that, like the numerical symbol 1, they are incapable of partition as well as of division, which is a consequence of the condition $xy=x+y$.

The symbol of the unit of a chemical substance, expressed as a function of the simple weights of which it consists, is identical with the symbol of a whole number expressed by means of its prime factors, a^n, b^{n_1}, c^{n_2}. . . . A general method is given for discovering the prime factors of chemical symbols.

Section VI. is on the construction of chemical equations from experimental data.

Section VII. On the expression of chemical symbols by means of prime factors in the actual system of chemical equations. The object of this section is to prove that the units of weight of chemical substances are integral compound weights, and to discover the simplest expression for the symbols which is consistent with this assumption.

Such an expression cannot be effected unless some one symbol be determined from external considerations. The unit of hydrogen, therefore, is assumed to consist of one simple weight, its symbol being expressed by one prime factor, α, which is termed the *modulus* of the symbolic system. This assertion is the expression of an hypothesis which may be proved or disproved by facts, and the consequences of which are here traced.

The symbols of the elements are considered in three groups:—1. The symbols of the elements of which the density in the gaseous condition can be experimentally determined, and which form with one another gaseous combinations. 2. The symbols of carbon, boron, and silicon. 3. The symbols of other elements, which are determined with a certain probability by the aid of the law of Dulong.

For the method of constructing these symbols, which depends upon the solution in whole numbers of certain simple indeterminate equations, we must refer to the memoir itself.

The following symbols (p. 230) may serve as an illustration of the general results.

Prime factors.	Absolute weight in grammes.	Relative weight.
α	0·089	1
ξ	0·715	8
χ	1·542	17·25
ν	0·581	6·5
ϕ	1·305	15
κ	0·536	6

Name of substance.	Symbol.	Absolute weight in grammes.	Relative weight.
Hydrogen	α	0·089	1
Oxygen	ξ	1·430	16
Water	$\alpha\xi$	0·804	9
Chlorine	$\alpha\chi^2$	3·173	35·5
Hydrochloric acid	$\alpha\chi$	1·631	18·25
Oxide of chlorine	$\alpha\chi^2\xi$	3·888	43·5
Hypochlorous acid	$\alpha\chi\xi$	2·346	26·25
Teroxide of chlorine...	$\alpha\chi^2\xi^3$	5·319	59·5
Chlorous acid	$\alpha\chi\xi^2$	3·062	34·25
Chloric acid	$\alpha\chi\xi^3$	3·777	42·25
Nitrogen	$\alpha\nu^2$	1·251	14
Ammonia	$\alpha^3\nu$	0·760	8·5
Protoxide of nitrogen	$\alpha\nu^2\xi$	1·966	22
Nitrite of ammonium	$\alpha^2\nu^2\xi^2$	2·860	32
Chloride of ammonium	$\alpha^2\nu\chi$	2·391	26·75
Phosphorus	$\alpha^2\phi^2$	5·541	62
Phosphide of hydrogen	$\alpha^2\phi$	1·519	17
Pentachloride of phosphorus	$\alpha^2\phi\chi^5$	9·319	104·25
Terchloride of phosphorus	$\alpha^2\phi\chi^3$	6·145	68·75
Oxychloride of phosphorus	$\alpha^2\phi\chi^3\xi$	6·869	76·75
Carbon	κ^y	0·536+y	6+y
Acetylene	$\alpha\kappa^2$	1·161	13
Marsh-gas	$\alpha^2\kappa$	0·704	8
Alcohol	$\alpha^3\kappa^2\xi$	2·056	23
Ether	$\alpha^5\kappa^4\xi$	3·308	37
Acetic anhydride	$\alpha^3\kappa^4\xi^3$	4·559	51
Acetic acid	$\alpha^2\kappa^2\xi^2$	2·682	30
Trichloracetic acid	$\alpha^2\chi^3\kappa^2\xi^2$	6·146	68·75
Hydrocyanic acid	$\alpha\nu\kappa$	1·207	13·5
Cyanogen	$\alpha\nu^2\kappa^2$	2·324	26

Section VIII. Certain apparent exceptions are considered, in which it is not found possible to express the symbols of chemical substances by means of an integral number of prime factors, consistently with the assumption of the modulus α.

GEOLOGICAL SOCIETY.

[Continued from p. 155.]

June 20, 1866.—Warington W. Smyth, Esq., M.A., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Structure of the Red Crag." By S. V. Wood, Esq., F.G.S.

The Rev. O. Fisher having lately published a paper in which he

endeavoured to show that the Chillesford beds were beneath the Fluvio-marine Crag, Mr. S. V. Wood in this paper first drew attention to certain facts which appeared to him to prove the contrary view, especially the relations of the deposits as exhibited in pits at Wangford and at Thorpe, near Aldborough. The author then drew attention to the character of the fossils of the Red Crag as affording evidence of one of the most rapid changes in fauna that Geology affords; and he showed that this deposit contains the evidence of a transition by stages, from the oldest—where the affinities of the fossils are to a great extent with those of the Coralline Crag, and to a greater extent with the existing fauna of the Mediterranean—to the newer stages, in which the shells are very few, and confined to types peculiarly northern.

2. "Note on supposed Remains of the Crag on the North Downs, near Folkestone." By H. W. Bristow, Esq., F.R.S., F.G.S.

An examination of these sands at Paddlesworth had convinced the author of their similarity to certain ferruginous clayey sands with masses of ferruginous grit, which occur in the Hampshire Basin and belong to the Woolwich and Reading series; and he therefore concluded that if the Kentish beds can be proved to belong to any other member of the Tertiary series, it is only to be done by the evidence of the fossils.

3. "On the Warp of Mr. Trimmer, its age and probable connexion with the latest geological events and changes of climate." By the Rev. O. Fisher, M.A., F.G.S.

The author commenced by referring to the opinion of the late Mr. Trimmer respecting the origin of soils, that they are composed of the débris of the underlying rocks, together with transported materials. He then showed that the adventitious matter usually occurs filling furrows in the subjacent rock, and appears to have been carried forward in a plastic state, and not water-drifted. The author named it "trail," and explained that the variation of soils arises from its incorporation with the disintegrated matter. The furrows were considered to be indications of the last denudation of the surface, and it was suggested that they may have been formed by land-ice. The ice-sheet having finally disappeared, the formation of the warp with its basal pebbles was considered to be due to meteoric action. The warp was then stated to be older than the last depression of the land, and to underlie the *Scrobicularia*-clay, while the gravels beneath the submarine forests at the mouths of many valleys were also supposed to be trail.

In conclusion, Mr. Fisher discussed the theories of M. Adhémar and Mr. Croll, showing that the events as traced in the former part of the paper agree with their views, and that their determination of the date of the commencement of the alluvial period (the period of the retirement of the sea from our lower valleys) coincides remarkably with that assigned to it on totally different grounds by Mr. Prestwich, of from 8000 to 10,000 years.

4. "On Faults in the Drift-gravel at Hitchin, Herts." By J. W. Salter, Esq., F.G.S.

The author described some faults exhibited in a cutting of the Great Northern Railway, passing through the Chalk and Boulder-clay gravel, and remarked that whatever system of movements affected Tertiary rocks disturbed also the deeper-seated strata, and assigned this as a reason why the older rocks are more faulted and jointed than the newer.

5. "On some Flint Implements lately found in the Valley of the Little Ouse river, near Thetford." By J. W. Flower, Esq., F.G.S.

The sands and flint-gravel on the right bank of the river Ouse at Thetford form a terrace 8 to 10 yards above the river, and about 40 yards distant from it; at a spot called Red Hill a large number of flint implements have lately been obtained from this gravel, at from 12 to 15 feet below the surface, and within a foot or less of the chalk on which the gravel rests; and some were found in the same gravel filling pot-holes in the chalk.

The author pointed out the exact correspondence, as regards geological position and relations, between the Thetford gravels and the flint-implement-bearing beds of Amiens, Abbeville, Fisherton, Icklingham, Hoxne, &c. He further noticed the close resemblance which these implements and some others discovered in England bear to those of the valley of the Somme; and concluded by expressing his dissent from Mr. Prestwich's conclusions, and stating his own views on their mode of accumulation, remarking that, in his opinion, these implements were manufactured prior to the severance of this island from the continent.

6. "On some evidences of the Antiquity of Man in Ecuador." By J. S. Wilson, Esq.

The western slope of the Cordilleras was stated by the author to be occupied with projected volcanic matter distributed in terraces, the most recent of which is but slightly above high-water mark; the second rises in some places 10 feet above the former, and is well seen in the lower part of the Esmeraldas river and in the valleys of its lower tributaries; above this rise four other terraces, respectively 8, 15, 12, and 6 feet above one another.

The second terrace contains in many places remains of articles of human art, broken pottery, earthen figures, and fragments of gold ornaments. This pottery stratum is traceable along a line of 80 miles of coast, and, by partial observations, is determined to occur under corresponding conditions for a distance of 200 miles more.

A section at Chancama was also described; it is 24 miles from the coast, 180 feet above the sea, and 50 feet above the Esmeraldas river, and exhibits undisturbed sea-distributed gravel and sands, 6 feet 6 inches in thickness, containing fragments of pottery.

7. "On the relations of the Tertiary Formations of the West Indies." By R. J. L. Guppy, Esq., F.G.S.

In this paper the author first briefly noticed the present state of our knowledge of the different formations occurring in the Caribbean

area, which he named respectively Eocene, Lower Miocene, and Upper Miocene, these names having reference to their relative position rather than to their positive age. It was stated that Eocene strata were as yet known to occur only in Jamaica; and the author then described the Lower Miocene deposits of Trinidad, Anguilla, and Antigua, and the Upper Miocene of San Domingo, Jamaica, Trinidad, and Cumana, giving sections illustrating the nature and position of the beds, and lists of the fossils found therein. Mr. Guppy then discussed the age of the Caribbean Miocene deposits of the different islands, giving the evidence on which the above-mentioned classification is founded, and a sketch of the deposits in other islands not included in it. In conclusion the author discussed the relation of the West-Indian Miocene deposits to the Tertiary strata of other regions, especially with regard to the migration of species and the Atlantis hypothesis; and he inferred that the Miocene of the West Indies must be included in the same great period of time as that of Europe, and may therefore be considered, in a geological sense, synchronous, that it is highly improbable that the West-Indian Miocene forms reached the localities where they occur as fossils by way of the Isthmus of Panama, or by an easterly route from Europe or from the Indian sea, and that it is probable that during the early and middle Tertiaries such a connexion existed between the shores of the Atlantic as admitted of the migration of organized beings from one side to the other, although the continents may not have been absolutely joined.

8. "On the discovery of new Gold-deposits in the district of Esmeraldas, Ecuador." By Lieut.-Col. Neale, Her Majesty's Chargé d'Affaires in Ecuador.

The author stated that unworked and hitherto unknown gold-deposits had been discovered in the district of Esmeraldas, Ecuador, and that the President of the Republic, who had received specimens of the gold of a very pure quality, purposed sending a scientific commission to report on the probable yield of the gold-district. Further, he recorded a recent influx of immigrants from California and Nevada to the gold-mines of Barbacoas in New Granada.

9. "On bones of fossil Chelonians from the Ossiferous Caves and Fissures of Malta." By A. Leith Adams, M.B., F.G.S.

The remains of more than one species of River-Tortoise, agreeing in their characters with the Helodians and Potamians, were stated to occur in the Maltese caves and fissures associated with exuviæ of the fossil elephant, *Hippopotamus Pentlandi*, *Myoxus Melitensis*, and birds (the last chiefly aquatic, including *Cygnus Falconeri*), a lizard, and one or more frogs. The author considered that the nature and arrangement of the deposits and the conditions of their fossil fauna clearly show that they had for the most part been conveyed into the above situations by the agency of large bodies of water, which at one time overflowed the greater portion of the eastern half of the island.

10. "On the discovery of remains of *Halitherium* in the Miocene beds of Malta." By A. Leith Adams, M.B., F.G.S.

The four upper beds of the Miocene formation of the Maltese group, more especially the Sand-bed and Nodule-bands of the calcareous sandstone, have yielded several forms of Cetaceans, teeth of *Zeuglodon*, one or more species of Dugong allied to recent forms, and *Balænæ*; to these the author has added a tooth, an ear-bone, and some caudal vertebræ of the *Halitherium*.

11. "On the affinities of *Chondrosteus*, Ag." By John Young, M.D., F.G.S.

The object of this communication was to show, from the characters of the skeleton, that *Chondrosteus* belongs not to the Chondrostean division of the Ganoids, as stated by Agassiz, but to the Holostean division, since it possesses a well-ossified basioccipital; and the lateral walls of the cranium are composed of bones answering to the cartilage bones of ordinary Teleosteans.

12. "On new Carboniferous genera of Crossopterygian Ganoids." By John Young, M.D., F.G.S.

In this paper the following new genera were described:—*Rhizodopsis*, *Strepsodus*, *Dendroptychius*, and *Rhomboptychius*, all of which were provisionally named some years ago by Prof. Huxley. Their generic distinctness has been fully established by specimens recently discovered. The relation of *Rhomboptychius* to *Megalichthys*, and the position of *Holoptychius* and *Rhizodus* in this subdivision of the Ganoids, are discussed in the latter part of the communication.

13. "On supposed burrows of Worms in the Laurentian Rocks of Canada." By Dr. Dawson, F.G.S.

The author communicated the discovery of perforations, resembling burrows of worms, in a calcareous quartzite, or impure limestone, of Laurentian age, from Madoc, in Upper Canada, but belonging to a somewhat higher horizon than the Eozoon-serpentines of Grenville.

XXX. *Intelligence and Miscellaneous Articles.*

OBSERVATION ON THE PASSAGE OF THE SPARK OF AN INDUCTION-COIL THROUGH FLAME. BY A. KUNDT.

If the current of sparks of an induction-coil be passed through the luminous flame of gas or of a candle, no alteration is seen in the flame, excepting that in the path of the sparks the flame is intensely luminous, and, under certain circumstances, this brightly luminous path of sparks is traversed by dark cross bands. When the polar wires are suitably introduced, it appears constant and steady. Yet if the flame is viewed in a slowly rotating mirror, or in one which is moved to and fro in the hand, this apparent constancy is found really not to exist; for, looked at in the mirror, the image does not seem constantly broadened, but the part above the spark appears alternating, like the flame of a chemical harmonicon when looked at in a ro-

tating mirror. From the upper point of the flame to the spark, the image in the mirror appears to have serrate incisions, and at the lower point of each dark incision there is a passing spark. During this transition of an individual spark, the flame, therefore, is always extinguished above. The part below the spark is constant and steady.

The reason of this extinction of the upper part of the flame by the spark is due to the fact that the spark causes a very rapid combustion of the gases on its path, and then by the mechanical pressure which the spark thereby exercises on all sides, the access of gas from below is prevented for a moment.

The extinction of the upper flame at each spark which passes is also seen by another mode of investigation, in the following manner:—

While the sparks pass through the flame, and in such a manner that the latter is apparently quite steady, it is viewed through a rotating disk in which there are several narrow slits. Viewed *at right angles* to the direction of the passing spark, the flame above the spark seems formed of bright and dark layers; viewed *in the direction* of the spark, layers in the proper sense are not seen, but dark circles rather continuously rising on the flame. It is best for the latter observation if one electrode is in the flame, the other remaining outside, and this latter is looked at from the side.

It is not necessary here to explain minutely in what manner this production of bright and dark lines by the cooperation of the alternating flame and momentary observation is brought about. The phenomenon depends essentially on the same principle as that on which an emerging jet of water, when looked at through a rotating disk, seems formed of individual drops. It is clear that the number and motion of the dark and bright layers of the flame alter with the number of slits of the rotating disk, and with the velocity of rotation.—Poggendorff's *Annalen*, May 1866.

ON THE DEPORTMENT OF SOLUTIONS OF GLAUBER'S SALT ON REDUCTION OF TEMPERATURE. BY DR. F. LINDIG OF SCHWERIN.

As the physical processes in the so-called supersaturation of Glauber's salt solutions have as yet found no sufficient explanation, either from physicists or chemists, it will not be uninteresting if I communicate a few observations on this subject.

If a solution of Glauber's salt, whether saturated or not, is allowed to cool slowly, it contracts with diminution of temperature, like any other body, as long as there is no crystallization. But as soon as the first crystals form in the clear solution, instead of contracting, it begins to expand, and continues to do so in proportion as the crystallization proceeds. Hence the density of the crystals forming is less than that of the solution from which they form*.

Surprising as is this deportment of a gradually crystallizing solution of Glauber's salt, that of a so-called supersaturated solution is

* With this the circumstance seems to disagree, that detached crystals did not swim in the liquid, but sank to the bottom.

still more surprising and remarkable. If such a one by careful treatment is cooled down to 0°, and then made to crystallize suddenly, the crystal cake formed, which constitutes a compact solid mass, exhibits an extraordinary increase in volume, and on further cooling, to about 10° C. below zero, contracts more and more. As in this condition of the original solution there can be no question of a separation of crystals as in the former case, it seems (like water below 4°) not to follow the law according to which bodies contract by diminution of temperature.

The experiments in question, which any one can easily execute, I made with a glass flask of about 60 cubic centims. capacity, into which in different experiments I poured solutions of Glauber's salt of various strengths, covered them with a layer of petroleum, and closed the flask with a perforated caoutchouc stopper. Through this stopper passed a glass tube of 30 centims. in length and 2·09 cubic centims. in capacity, provided with a paper scale and reaching down to the layer of oil. To vary the temperature, the flask could be placed in a beaker containing either a freezing-mixture or warm water. If the crystallization of the enclosed solution did not take place spontaneously at the proper moment, it was immediately produced by a small particle of crystal projected through the open tube. The sudden change of temperature was frequently so energetic that the flask was cracked if the glass tube was accidentally stopped and presented no outlet for the displaced covering layer. The whole apparatus could be used as a thermometer (only in an inverse manner), and indicated a change of temperature of not too brief duration in a tolerably delicate manner. If, for instance, the strongly cooled apparatus was warmed with the hand for a few moments, a depression of the covering layer was distinctly perceived.—Poggendorff's *Annalen*, May 1866.

ON THE FIGURE OF THE EARTH. BY CAPTAIN. A. R. CLARKE.

To the Editors of the Philosophical Magazine and Journal.

Ordnance Survey Office, Southampton,
August 17, 1866.

GENTLEMEN,

The question of the correctness of the simple and direct application of the method of least squares to the determination of the figure of the earth, which is controverted by Archdeacon Pratt, is an important one, but, being purely mathematical, should not be a matter of opinion. As, however, it cannot be of great interest to the majority of your readers, I shall only ask leave to remark here that, so far from having vindicated the legitimacy of his "correction" of the method of least squares, his "improvement upon his correction" is not only expressly hypothetical and arbitrary, but is a still further departure from simplicity and truth. The values which Archdeacon Pratt, in his work 'On the Figure of the Earth,' had obtained for the local attraction at the "reference-station" of the three great arcs

were very nearly true; but by his improved method he has been led to entirely different and quite erroneous values.

I am, Gentlemen,
Your obedient Servant,
A. R. CLARKE.

ON THE ROTATORY ACTION WHICH QUARTZ EXERCISES ON THE PLANE OF POLARIZATION OF THE LEAST-REFRANGIBLE RAYS OF THE SPECTRUM. BY M. P. DESAINS.

In a memoir inserted in the *Annales de Chimie et de Physique*, 3e sér. vol. xxx., De la Provostaye and I investigated the rotatory action which *active* substances exert on the calorific rays of the visible spectrum. I have extended these researches to the obscure part of the solar radiation, and beg leave of the Academy to communicate the results which I have obtained.

At first, working with rays which, in the spectrum I used, occupied a position beyond the extreme red almost corresponding to the yellow, I observed that the plane of polarization of these rays only experienced a rotation of 19° when they traversed at right angles a plate of quartz capable of imparting a rotation of 52° to the mean red of the same spectrum.

This first fact obtained, I worked with rays still less refrangible, in the position on the side of the extreme red corresponding to the blue on the other side; I found heat-rays the plane of polarization of which only underwent a rotation of 8 or 9 degrees under the action of the quartz plate previously defined. Under equal conditions, the rotation of these rays was thus about one-sixteenth that of the extreme violet of M. Biot. Their wave-length would thus be four times that of the violet, if it be true that up to these extreme limits we could, as a just approximation, assume that these rotations are inversely proportional to the squares of the wave-lengths.

To observe conveniently the very feeble rotation whose value I have given, it is not necessary to work with rays isolated in the dark part of the spectrum. If a solar beam is sent through a pretty thick layer of solution of iodine in bisulphide of carbon prepared by Professor Tyndall's method, rays only are left which quite resemble those on which the observations above described have been made.

On this new point I have made a considerable number of experiments, of which I will describe one series.

A well-polarized solar ray, which had passed through a layer of iodized bisulphide, was quite extinguished when my analyzer indicated 45 degrees. The interposition of the quartz brought about an action on the rheometer; but this action again disappeared when the analyzer was brought to the division 55, and all observations made in other azimuths agreed in proving that the rotation was indeed 10 degrees. Here are the observations:—

Position of the analyzer.	Deflection.
55	0
55	0·1
− 35	20·5
+ 10	10
+100	10·5

For positions of the analyzer equally distant from 55 the deflections are equal; and the sum of those obtained at +10 and +100 is equal to that obtained at −35, as it ought to be. The division of the scale employed in these measurements goes up to 180 on each side of zero. The quartz was always the same.

I shall finish this note by adducing some observations of a totally different nature, which appear to confirm my previous results. M. Dumoulin-Froment had the goodness to lend me a grating which he himself had constructed. On this delicate apparatus I let fall a solar pencil, transmitted through a narrow aperture, and concentrated by a lens; at a suitable distance I obtained, on a screen and with great distinctness, the phenomena of Fraunhofer. By placing the pile in the dark spaces which extend from one side to the other of the central pencil, I obtained no deflection. The needle, on the contrary, was sometimes deflected as far as 15 degrees by the action of the green, yellow, or red rays of the first spectrum. The limit of the extreme red of this spectrum touched the violet of the second. Receiving in addition the rays within this region, I obtained 10 degrees more of deviation; at a greater distance the effects rapidly decreased, and in the conditions of my experiments I only obtained a deflection of 2 to 3 when I received on the pile the orange and the yellow of the second spectrum, with the portions of red and of green the nearest these colours; but (and this is the point on which I dwell) by interposing in the path of the rays the trough full of iodized sulphuret, I extinguished all the effects produced by heat which are found in the visible part of the first spectrum, and as far as the violet of the second; while, when the pile was so placed as to receive the green, the yellow, and the orange of the second spectrum, the interposition of the sulphuret did not completely extinguish the calorific action. Such was then, in the *first* spectrum, the position of the obscure rays transmitted through the sulphuret. These latter results were obtained with a very delicate pile, constructed by M. Ruhmkorff according to the recent directions of M. Edm. Becquerel. —*Comptes Rendus*, June 11, 1866.

ON THE USE OF NITROGLYCERINE IN THE QUARRIES OF VOSGESIAN SANDSTONE NEAR SAVERNE. BY M. E. KOPP.

The fulminating properties of nitroglycerine, $C^6H^5(NO^4)^3O^6$, and the experiments made with this substance in various localities of Sweden, Germany, and Switzerland, have led MM. Schmitt and Dietsch, proprietors of the great quarries of sandstone in the valley of the Zorn (Lower Rhine), to try its use also in their workings.

The success has been so great, both as regards economy and facility and rapidity of working, as to lead temporarily, at any rate, to the disuse of gunpowder, so that for the last six weeks quarries are worked with nitroglycerine only.

From the commencement we thought it necessary to prepare this substance on the spot; the carriage, whether by ship or by rail, of such a substance, so explosive and of such frightful power, appeared inadmissible. The great misfortunes which have occurred at Aspenwall and San Francisco have shown that these fears were well founded, and that the carriage of nitroglycerine ought to be absolutely forbidden.

After studying in my laboratory, with the aid of M. Keller, the various modes of preparing nitroglycerine (mixtures of glycerine with concentrated sulphuric acid and nitrates of potash and soda, or with nitric acids of different degrees of concentration), we have adopted the following method of manufacture, which has been established in a wooden cabin, constructed in one of the quarries:—

1. *Preparation of Nitroglycerine.*—In a vessel of sandstone placed in cold water, fuming nitric acid of 49° or 50° Beaumé is mixed with double its weight of the most concentrated sulphuric acid. (These acids are prepared expressly at Dieuze, and sent to Saverne.) On the other hand, glycerine of commerce, but free from lime and lead, is evaporated in an iron pot until it marks from 30° to 31° Beaumé. This concentrated glycerine should be syrupy when quite cold.

The workman places then 3300 grammes of the mixed acids, well cooled, in a glass flask (a sandstone pot, or a porcelain or sandstone basin may also be used), dipped in a bath of cold water, and pours slowly, with constant stirring, 500 grammes of glycerine. The important point is to avoid a perceptible heating of the mixture, which would occasion a tumultuous oxidation of the glycerine with production of oxalic acid. Hence the vessel in which the change of glycerine into nitroglycerine is effected, should be constantly cooled on the outside by cold water.

The mixture having been completely effected, the whole is left for from five to ten minutes, then the mixture is thrown into cold water which has been previously agitated. The nitroglycerine is rapidly precipitated as a heavy oil, which is collected by decantation in a tall vessel; it is then washed once with a little water, which is decanted; then the nitroglycerine is placed in bottles, where it is ready for use.

In this condition the nitroglycerine is still a little acid and aqueous; but that is not inconvenient, for it is used a short time after its preparation, and these impurities by no means prevent its detonation.

2. *Properties of Nitroglycerine.*—Nitroglycerine constitutes a yellow or brownish oil, heavier than water (in which it is insoluble), soluble in alcohol, ether, &c.

Exposed to even a feeble degree of cold, provided it is prolonged, it crystallizes in elongated needles. A very violent shock is the best mode of exploding it. It is, moreover, managed easily, and without danger. Spread on the earth, it is only difficultly inflammable by a body in combustion, and only burns partially; a flask containing nitroglycerine can be smashed on the stones without the liquid detonating; it may be volatilized without decomposition by a regulated heat; but if the ebullition becomes brisk, explosion ensues.

A drop of nitroglycerine falling on a moderately hot plate volatilizes quietly; if the plate is red-hot the drop inflames immediately, and burns like a grain of powder without noise; but if the plate, without being red, is hot enough to make the nitroglycerine boil immediately, the drop decomposes suddenly with a violent explosion.

Nitroglycerine, especially when it is impure and acid, may decompose spontaneously at the expiration of a certain time, with disengagement of gas, and production of oxalic and glyceric acids.

It is probable that to some such cause are due the spontaneous explosions of nitroglycerine of which we read in the papers. The nitroglycerine being enclosed in well-stoppered bottles, the gaseous products of decomposition, not being able to escape, exert a very great pressure on the nitroglycerine; and under these circumstances the least shock and the slightest motion may bring about an explosion.

Nitroglycerine has a taste at once saccharine, piquant, and aromatic; it is a poisonous substance; in very small doses it provokes strong headaches. Its vapour produces similar effects; and this circumstance might be an objection to its use in the deep galleries of mines, where the vapour cannot escape as easily as in the open air.

3. *Mode of using Nitroglycerine.*—Suppose it is desired to detach a layer of rocks. At a distance of from 2·5 to 3 metres from the outside, a mine-hole is dug of about 5 to 6 centimetres diameter, and 2 to 3 metres depth.

After having cleaned this of dirt, water, and sand, 1500 to 2000 grammes of nitroglycerine are introduced by means of a funnel.

A small cylinder of wood, of cardboard, or sheet iron about 4 centims. in diameter, and 5 to 6 centims. in height, is then introduced filled with ordinary powder. This is fixed to a wick or ordinary mine fuse, which penetrates into it to a certain depth, to assure the inflammation of the powder. By means of the match on the fusee the cylinder is lowered; and by the feel the moment can easily be judged at which the cylinder reaches the surface of the glycerine. The match being then held firmly, fine sand is run into the hole until it is quite full. It is useless to compress or tamp the sand. The match is cut a few centimetres above the orifice, and set fire to. In eight or ten minutes, the burning of the wick having reached the cylinder, the powder inflames. A violent shock ensues, which instantaneously explodes the nitroglycerine. The explosion is so sudden that the sand has no time to be projected.

The mass of the rock is seen to rise, become displaced, and settle down quietly without any projection; a dull sound is heard.

It is only on reaching the places that an idea is formed of the great force developed. Formidable masses of rock are displaced and fissured in all directions, ready to be worked mechanically.

The principal advantage consists in the fact that the stone is but slightly bruised, and that there is little waste. With the charges mentioned, 40 to 80 cubic metres of very resisting rock can be detached.—*Comptes Rendus*, July 23, 1866.

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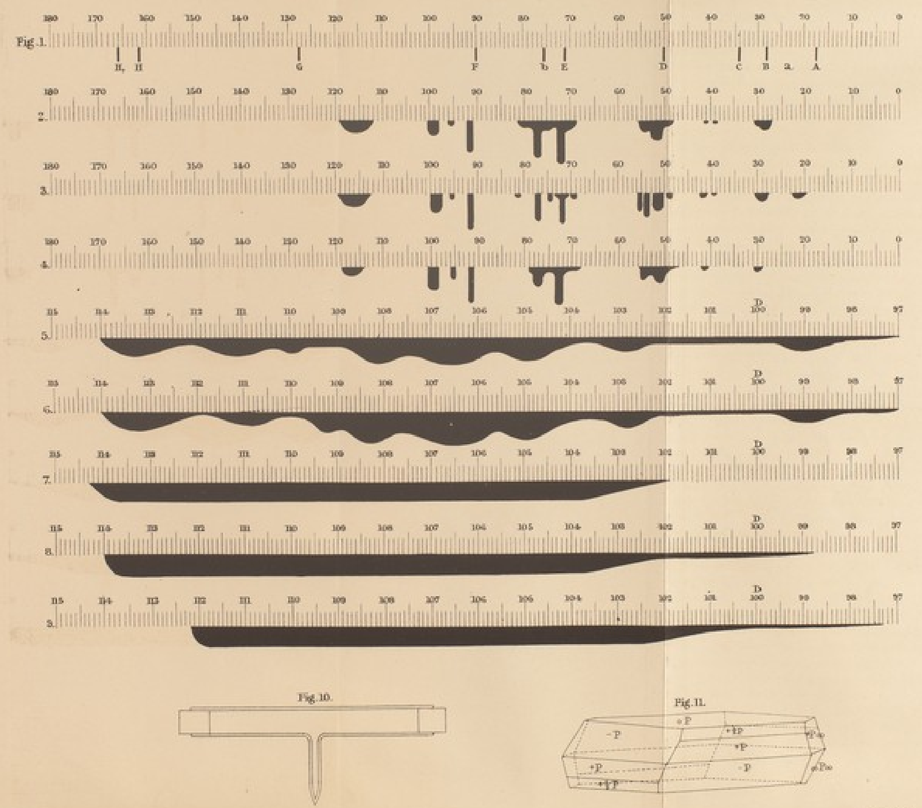

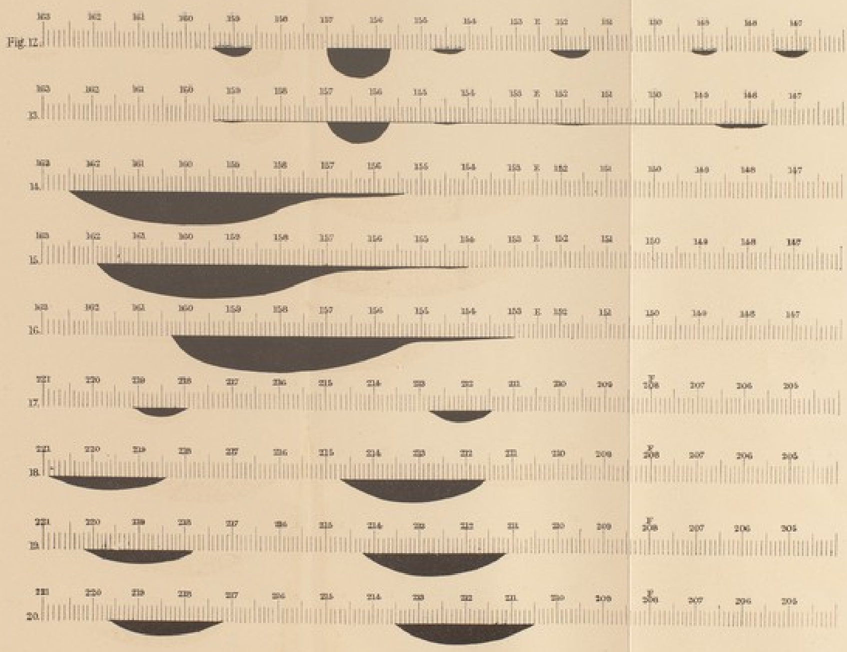

J. Basire, lith.

ESSAI

SUR LA RELATION QUI EXISTE A L'ÉTAT PHYSIOLOGIQUE

ENTRE

L'ACTIVITÉ CÉRÉBRALE

ET LA COMPOSITION DES URINES

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PAR

LE D[r] H. BYASSON

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(Médaille d'argent, 1863; 1[re] Médaille, 1865);
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LAURÉAT DE L'ÉCOLE SUPÉRIEURE DE PHARMACIE DE PARIS,
(Médaille d'argent, 1861; 1[re] Mention, 1862);
RÉCOMPENSE PUBLIQUE A L'OCCASION DE L'ÉPIDÉMIE CHOLÉRIQUE DE 1865,
MÉDAILLE DE BRONZE A L'OCCASION DE L'ÉPIDÉMIE CHOLÉRIQUE DE 1866
VICE-PRÉSIDENT DE LA SOCIÉTÉ D'ÉMULATION POUR LES SCIENCES PHARMACEUTIQUES,
MEMBRE DE LA SOCIÉTÉ CHIMIQUE DE PARIS.

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MADRID C. BAILLY-BAILLIÈRE, PLAZA DEL PRINCIPE ALFONSO, 8.

1868

INTRODUCTION

Parmi les sciences dont les progrès deviennent chaque jour plus marqués, grâce à la fois à l'application de la méthode expérimentale et à l'impulsion donnée par nos maîtres, nous sommes heureux de pouvoir citer la biologie et surtout l'une de ses branches, la physiologie. Placée sur l'échelon le plus élevé des sciences positives, étudiant les corps organisés et les lois de leur activité, possédant des moyens d'investigation à elle propres, mais s'aidant des ressources de la chimie et la physique, son champ est immense, et nulle carrière humaine, si longue et si bien remplie qu'elle soit, ne peut se flatter de le parcourir en entier.

De cette considération ressort la nécessité de scinder cette branche des sciences médicales, pour en mieux étudier les faits et ériger les lois, sans vouloir faire, de ces tronçons, en apparence épars, des sciences séparées. C'est en effet une tendance bien naturelle de notre esprit de vouloir accorder l'importance la plus large au sujet spécial de nos études et de nous laisser entraîner à prendre le secondaire pour le principal; heureux entraînement et qui, à défaut de l'attrait irrésistible qu'offre la science, suffirait pour comprendre les recherches les plus laborieuses entreprises à toutes les époques.

Frappés des découvertes modernes de la chimie et de leur application immédiate à l'étude de plusieurs phénomènes physiologiques, des savants ont voulu fonder des théories exclusives, et qui, par cela seul devaient tomber, en produisant une réaction nuisible au progrès; de là des divergences, qui sont loin d'être détruites, mais qui ont servi toutefois à montrer combien le concours des sciences physiques est indispensable. C'est grâce à cette solidarité nécessaire et enfin reconnue de toutes les sciences, que nous voyons disparaître chaque jour quelqu'un de ces agents mystérieux inventés par l'ignorance pour expliquer la vie : ignorance relative, s'entend, et dont nous serons taxés un jour sur bien des sujets.

Nous avons cru devoir présenter ces considérations bien générales et bien courtes, pour montrer au début de ce travail, dans quel esprit nous l'avons conçu, et écarter le reproche de vouloir attribuer aux phénomènes chimiques une prépondérance exclusive. Cette thèse ne sera, nous l'espérons, que le prélude de travaux que nous continuerons longtemps sur le même sujet, et pour lesquels nous profiterons des découvertes de chaque jour et des conseils de nos excellents maîtres.

Au point de vue physiologique, un être vivant est constitué par une aggrégation de matière, limitée dans l'espace et le temps, assujettie dans toute sa masse à un double travail d'assimilation et de désassimilation. La résultante de ce travail complexe est ce qu'on appelle la *vie*. Son essence nous échappera toujours; les esprits lancés dans les spéculations métaphysiques, qui courent à sa recherche, ne se doutent pas de leur impuissance, et leurs dissertations parfois fort belles, mais toujours stériles, fourniraient à la science, si elle en avait besoin; la preuve que notre pensée n'est apte qu'à saisir des

rapports. Dans tout être vivant, à mesure que l'organisation de la matière se complique, on voit apparaître des propriétés nouvelles immanentes à cet état.

L'anatomie nous apprend que toutes les parties de cet être sont réductibles en éléments parfaitement définis, isolables, vivant dans des liquides déterminés, et on peut dire, avec M. Robin, que l'accomplissement d'une fonction est la manifestation des diverses propriétés des éléments anatomiques. Pour manifester ses propriétés, l'élément anatomique doit être en voie de rénovation, et comme pour chacun des groupes ou systèmes auxquels correspond une fonction différente, la matière organisée varie dans sa composition élémentaire, à chacune des fonctions correspondront des produits spéciaux en qualité ou quantité, qui seront déversés à l'extérieur, modifiés ou non durant leur trajet. Lavoisier nous a comparés à une lampe qui brûle, et, malgré son peu de rigueur scientifique, cette comparaison restera comme une de celles qui frappent l'esprit, lui font entrevoir des aperçus nouveaux et l'invitent aux recherches. On pourrait ajouter, poursuivant la même idée, que les urines forment les cendres de cette combustion. Pour écarter toute discussion, nous prendrons ce dernier mot dans son sens le plus large et faute d'en trouver un meilleur : il exprimera non-seulement la formation de composés plus oxydés, et partant généralement plus simples et plus stables, mais aussi le double travail de synthèse et d'analyse dont l'organisme vivant est le siége.

Normalement les urines sont uniquement formées des matériaux de combustion. C'est le seul liquide qui mérite véritablement le nom d'humeur excrémentitielle;

dans une classification des humeurs, nous l'isolerions, tout en rapprochant l'exhalation pulmonaire d'abord, la sueur ensuite, et cela pour des considérations qu'il n'est pas dans notre cadre de développer. Les reins, en vertu de leur texture spéciale et par un mécanisme dont toutes les phases ne sont pas bien connues, tendent à maintenir le sang dans sa composition normale en lui enlevant les matériaux en excès qu'il a puisés dans les tissus, ou qui se forment directement dans sa masse. A l'altération de l'appareil séparateur comme à celle du sang correspondront des changements de composition dans les urines. Dans l'état physiologique, si une fonction vient à s'exagérer, si la vie des éléments anatomiques correspondants devient par suite plus active, on devra en trouver la preuve irrécusable dans l'examen comparé des urines. Ainsi nous possédons un moyen admirable et sûr de juger de la bonne harmonie des fonctions par l'examen des matériaux déversés à l'extérieur.

Cet aperçu, que nous avons cru nécessaire avant d'aborder le vif de la question, montre, en réfléchissant un peu, combien est vaste ce sujet d'études et aussi combien il est difficile. Des travaux immenses ont été publiés, tant en France qu'à l'étranger, sur les urines; leur énumération seule suffirait à fournir la matière d'un long chapitre. Nous n'en connaissons pas qui ait été entrepris en vue de résoudre directement l'importante question que nous nous sommes posée. Au début de ce travail, nous nous plaisons à citer les noms de Prout, Lecanu, Chevreul, Becquerel, Bouchardat, Quévenne, Liebig, Wœlher, Lehmann, Neubauer, Frerichs, Roberts, Bence-Jones, Beale et Golding Bird.

Nous diviserons cette thèse en deux parties : la première, dans laquelle après avoir nettement posé la question, nous développerons comment nous l'avons envisagée, en insistant sur les procédés analytiques employés. La deuxième partie comprendra les résultats des expériences et analyses, leur interprétation et la conclusion.

Ce travail a été préparé et médité depuis près de deux ans : après nous être familiarisé avec les procédés analytiques que nous décrirons soigneusement, nous avions entrepris d'étudier la variation des sulfates et des phosphates dans les urines; mais bientôt, sans qu'aucune idée théorique préconçue nous guidât, nous nous posâmes cette question de relation ; malgré sa complication et sa difficulté, sans prévoir quel serait le résultat de l'expérience, nous nous sommes mis à l'œuvre, et nous croyons que nos conclusions seront appuyées sur des faits expérimentaux à l'abri de sérieuses objections.

ESSAI

SUR

LA RELATION QUI EXISTE A L'ÉTAT PHYSIOLOGIQUE

ENTRE

L'ACTIVITÉ CÉRÉBRALE

ET LA

COMPOSITION DES URINES

PREMIÈRE PARTIE

La question, telle que nous l'avons posée en tête de ce travail, est énoncée, croyons-nous, trop clairement pour avoir besoin de longues explications. Est-il possible de démontrer expérimentalement que, lorsqu'un homme travaille du cerveau (et par travail du cerveau ou activité cérébrale, nous entendons plus spécialement ce qu'on est convenu d'appeler la pensée dans ses divers modes), il s'effectue dans cet appareil une dépense provenant de la combustion organique telle que nous l'avons définie, dépense représentée en partie par les produits de désassimilation déversés à l'extérieur par les urines? Des considérations d'un autre ordre ont amené à répondre affirmativement et, depuis deux ans environ, nous avons tous entendu retentir cette parole du grand physiologiste allemand Molleschott : « Sans le phosphore, point de pensée. » C'était, selon nous, exprimer d'une façon par trop imagée un fait d'une haute importance et qui n'avait pas besoin de s'affirmer et de se répandre sous une pareille forme. On pourrait dire avec autant de vérité : sans le carbone, point de pensée; sans l'azote, point de pensée, etc.

On sait déjà que, chez le même homme en repos ou se livrant à un exercice musculaire violent, la composition des urines est variable quant à la proportion relative des principaux éléments. Chez ce même individu, qui va maintenant exciter plus spécialement son cerveau, verrons-nous apparaître des changements dans ce liquide et, si la réponse est affirmative, quels sont-ils, et peut-on les distinguer des précédents? Nous reportant aux quelques considérations présentées plus haut, nous voyons que, dans le premier cas, on établit la relation qui lie l'activité musculaire au travail interstitiel effectué dans la fibre musculaire par la connaissance des produits formés et déversés à l'extérieur. Dans le second cas et en suivant la même marche, on établit cette même relation entre l'activité cérébrale et les produits de combustion interstitielle formés dans la cellule cérébrale. Nous n'avons nul besoin ici de savoir quels sont les corps qui se forment sur place, quels sont les changements qu'ils subissent jusqu'au moment où ils sont séparés du sang par le rein. Ces questions fort importantes, et dont la solution pourra certainement être donnée un jour, ne sauraient nous arrêter. L'important pour nous est d'arriver à établir que, lorsque la fonction cérébrale entre en activité, il y a une dépense

organique dont nous retrouvons les traces manifestes, dépense qui se rapporte incontestablement à l'accomplissement de la fonction.

Lorsqu'on médite ce sujet, on ne tarde pas à reconnaître qu'il est fort complexe. Mais tout problème, quel qu'il soit, se simplifie lorsqu'on arrive à transformer en constantes certaines variables. C'est ce que nous avons fait dans la mesure du possible.

Quelles sont les causes principales qui influent sur la composition des urines chez une personne bien portante? L'alimentation, l'absorption plus ou moins complète des matériaux pris à l'intérieur, l'accroissement physique, l'activité de la respiration, de la circulation, l'état de repos ou de mouvement, l'activité cérébrale, la température extérieure, la pression barométrique, sans parler du sexe, de l'âge et d'autres causes que nous n'avons pas besoin de faire intervenir. Supposons un homme arrivé à son développement physique complet, prenant chaque jour aux mêmes heures les mêmes aliments, admettons que ses matières fécales aient le même poids et la même composition ; que la température extérieure, la pression barométrique, l'état hygrométrique soient identiques, qu'il n'éprouve pas dans son poids de variation appréciable, imaginons-le pendant une série de jours distincts, tantôt à l'état de repos, tantôt à l'état d'activité musculaire, tantôt à l'état d'activité cérébrale, recueillons et analysons ses urines. Si, dans ces conditions, nous découvrons des variations de composition s'effectuant dans le même sens, se répétant avec régularité, ne serons-nous pas en droit d'en rapporter la cause aux différents états dans lesquels s'est trouvé l'organisme, et d'établir des rapports? Il faudrait, pour répondre négativement, ou bien admettre des causes occultes, ou bien supposer des changements profonds et alternatifs passant inaperçus, ne modifiant pas l'état normal et ne pouvant recevoir d'explication.

C'est dans des conditions aussi voisines que possible de celles que nous venons d'énumérer, que nous nous sommes placé. Evidemment, on ne peut s'imposer exclusivement ni l'état de repos, ni l'activité musculaire, ni l'activité cérébrale. Mais on peut d'une manière relative faire prédominer l'un des trois. Il nous arrive à tous de passer des jours calmes de corps et d'esprit, de faire un travail musculaire énergique durant lequel la pensée est en repos relatif; de passer, au contraire, d'autres journées durant lesquelles notre intelligence travaille énergiquement, l'activité musculaire étant presque nulle. Nous n'insisterons pas davantage sur ces considérations : nous aurons, d'ailleurs, occasion d'y revenir en analysant les résultats obtenus.

Nous ne consignerons dans cette thèse que les expériences faites en dernier lieu, et qui sont les plus complètes. Elles ont été entreprises le 1er février dernier, après avoir vérifié les procédés d'analyse suivis, et nous être familiarisé avec eux.

Nous nous sommes pris pour sujet d'expérience; car il nous paraissait difficile, dans une question aussi délicate, d'arriver sans cela à des resultats précis.

Il est inutile d'insister sur la nécessité de recueillir la totalité des urines des vingt-quatre heures : mais, contrairement à ce qu'on fait habituellement, les premières urines du matin étaient considérées comme appartenant à la journée commencée la veille ; c'est pendant le repos que procure un sommeil paisible, que

sont en grande partie éliminés les matériaux de désassimilation et que le sang reprend sa composition normale.

Avant de nous soumettre à une alimentation uniforme, il nous a paru intéressant de connaître quelles étaient les variations qui allaient se produire, sous l'influence du régime avant et après, et d'établir en même temps à une époque peu éloignée, la composition de nos urines. Nous avons adopté comme unique boisson l'eau ; comme aliment solide un pain ou gâteau, composé ainsi qu'il suit : farine de blé, 1 kilogramme ; œufs 6 ; beurre, 125 grammes ; sucre, 60 grammes ; eau, quantité suffisante. Une petite quantité de sel marin non pesée était ajoutée. Pour ne pas nous exposer à des redites, et abréger autant que possible, nous avons groupé plus loin en tableaux toutes les indications et tous les résultats. Nous avons pensé qu'il serait ainsi plus facile et plus commode de saisir l'ensemble et de comparer.

Nous croyons nécessaire d'exposer avec quelque détail les procédés analytiques suivis, et que nous avons modifiés ; on en comprend sans peine l'importance ; on trouverait peut-être dans leur plus ou mois grande sensibilité la cause de certaines divergences entre les résultats obtenus par divers auteurs.

Les substances que nous avons dosées directement sont l'urée, l'acide urique, l'acide phosphorique, l'acide sulfurique, le chlore, la chaux, la magnésie, la potasse.

Dosage de l'urée. — Les procédés généralement employés pour le dosage de l'urée sont les suivants :

Procédé de M. Lecanu, dosant cette substance à l'état de nitrate d'urée, et modifié quant au mode opératoire par M. Chalvet.

Procédé de Heints, fondé sur la propriété qu'a l'urée, de se décomposer, par la chaleur, en ammoniaque qu'on dose à l'état de chloro-platinate.

Procédé de Liebig fondé sur la propriété que possède l'urée, de se combiner avec le bioxyde de mercure.

Procédé de M. Leconte, fondé sur l'oxydation de l'urée en présence du chlorure de soude; la mise en liberté de l'azote et son dosage, procédé qui a subi quelques modifications peu importantes de la part de MM. Davy et Hamsfield.

Procédé de Millon, fondé sur la propriété que possède un mélange d'azotate et d'azotite de mercure, de transformer en acide carbonique tout le carbone de l'urée, procédé qui a subi de nombreuses modifications quant au dosage de l'acide carbonique.

Procédé qui consiste à évaluer la proportion d'urée en partant du poids spécifique de l'urine ; il donne des résultats beaucoup moins approchés que les précédents ; le coefficient adopté par les différents expérimentateurs n'est pas le même, et il faudrait supposer que les éléments autres que l'urée restent fixes.

Nous n'avons pas ici à comparer ces différentes méthodes d'analyse, à savoir quelle est la meilleure. Nous croyons que, lorsqu'un homme a fait sans précipitation de longs travaux sur un sujet, il faut, avant de se permettre de dire qu'il a fait des erreurs, supposer qu'on se trompe soi-même, qu'on ne se place pas dans toutes les conditions voulues et recommencer à bien des reprises ses expériences.

Les méthodes de dosage à l'état gazeux, présentent

des avantages incontestables ; elles demandent des précautions spéciales, en vertu même de leur sensibilité. En ce qui regarde l'urée, il n'est pas bien établi que les corps azotés que l'on rencontre mélangés avec elle, ne peuvent pas donner de la même manière de l'azote et de l'acide carbonique. Lorsqu'il est nécessaire de faire un grand nombre de dosages, les procédés volumétriques sont très-commodes, et leur précision dépend du soin que l'on a apporté à contrôler directement les résultats par les pesées, à préparer les liqueurs titrées, à vérifier ses instruments de mesure. Comme en toute chose, on acquiert par l'expérience une plus grande précision.

Nous avons adopté le procédé de Liebig; mais seulement après l'avoir modifié quant à la préparation de la liqueur mercurielle, et en partie quant au mode opératoire. Lorsqu'on fait dissoudre à chaud, du mercure dans de l'acide nitrique, et qu'on évapore en consistance sirupeuse, tout le métal n'est pas transformé en azotate de bioxyde. L'acidité de la liqueur est variable, et ces circonstances font que, même en vérifiant le titre, on peut avoir des résultats erronés. L'urée en effet ne se combine qu'avec le bioxyde de mercure ; le composé formé, presque insoluble dans une liqueur neutre, se dissout en proportions variables dès qu'elle est acidulée par l'acide nitrique ; en présence d'un excès de potasse cette dissolution peut donner un précipité jaune, alors que la réaction n'est pas terminée. Après nous être assuré de ces causes d'erreurs, nous avons cherché à les éviter.

On pèse exactement 36 grammes d'oxyde rouge de mercure; il est toujours facile de purifier soi-même le mercure et de le transformer en ce composé; on le fait dissoudre dans 50 grammes d'acide nitrique ordinaire, étendu de la moitié de son poids d'eau; on évapore doucement, jusqu'à apparition de vapeurs rutilantes, et on fait à 15° environ, avec cette liqueur et de l'eau distillée, le volume d'un litre. Si par le mélange avec l'eau, il se manifestait un léger trouble, quelques gouttes d'acide suffiraient à le faire disparaître. En opérant ainsi, on aura une dissolution dans laquelle tout le mercure sera à l'état de bioxyde, et aussi peu acide que possible. On prépare en outre, avec 20 grammes d'urée cristallisée et de l'eau distillée, 1 litre de liqueur d'épreuve.

L'urée peut former, avec l'oxyde de mercure, plusieurs combinaisons, dont trois au moins sont bien connues et ont été étudiées par MM. Werther, Neubauer et Kerner. Le composé blanc, amorphe, légèrement caséeux qui va se produire par le contact des deux solutions précédentes est constitué par un équivalent d'urée et quatre équivalents d'oxyde de mercure : $C^2H^4Az^2O^2$, 4HgO. Un gramme de ce corps renferme 0 gr. 878 d'oxyde de mercure et 0 gr. 122 d'urée; ces nombres sont à très-peu de chose près dans le rapport de 7 à 1, proportion favorable à un dosage exact de l'urée. En partant de cette formule, la liqueur mercurielle précédente est telle, qu'un centimètre cube précipitera 0 gr. 005 d'urée. Il fallait tout d'abord vérifier ce résultat.

Pour abréger, nous désignerons la solution d'azotate de mercure par liqueur A, la solution d'urée par liqueur B. Mettons dans un vase à précipité, placé sur une feuille de papier blanc, 50 cent. cubes de liqueur B. Si l'on vient à verser une certaine quantité de liqueur A, on

reconnaît qu'à mesure que le précipité blanc se forme, la solution devient acide par la mise en liberté de l'acide nitrique. Neutralisons-la de temps en temps par une dissolution de potasse, sans toutefois la rendre alcaline. Il arrivera un moment où la liqueur potassique, versée le long des parois de verre, de manière à se répandre à la surface, fera naître une coloration jaune. On s'arrête et on voit qu'on a employé 200 cent. cubes de liqueur A. Le précipité après lavage et dessiccation pèse 7 gr. 95; si, comme nous l'avons fait, on répète un très-grand nombre de fois cette expérience, on trouve des résultats très-concordants quant aux volumes des liqueurs et quant aux poids des précipités qui n'ont jamais varié qu'entre les limites suivantes: 7 gr. 915 et 8 grammes. Nous avons pesé 0 gr. 50 de ce composé et cherché par le procédé si exact de M. Personne la quantité de mercure. Nous avons détruit l'urée par l'eau régale avec excès d'acide chlorhydrique et par le chlorate de potasse; le mercure a été ainsi transformé en bichlorure, dissous dans l'eau à la faveur du chlorure de potassium. Cinq dosages effectués sur des précipités distincts nous ont donné comme moyenne 0 gr. 395 de mercure pour 0 gr. 50, le chiffre extrême étant 0 gr. 390 et 0 gr. 400. Or, si l'on adopte la formule $C^2H^4Az^2O^2$, 4HgO, le poids théorique du précipité obtenu avec les volumes indiqués de liqueur doit être 8 gr. 2, et 0 gr. 50 doivent renfermer, 0 gr. 486 de mercure.

Ces nombres sont fort rapprochés des précédents et il est hors de doute que la formule admise est exacte. On remarquera que les chiffres obtenus sont toujours trop faibles: en voici la raison. Lorsqu'on a mélangé les deux liqueurs et qu'on est arrivé à la limite de la saturation, comme il est nécessaire que la dissolution soit acide au moment où la potasse produit une légère coloration jaune, une certaine quantité du composé d'urée reste en dissolution et on le retrouve dans le liquide recueilli après filtration. Le précipité jaune dont la formation indique la fin de la réaction n'est pas uniquement formé d'oxyde de mercure hydraté mais de son mélange avec le corps $C^2H^4Az^2O^2$, 4Hgo. Les dissolutions concentrées et chaudes de potasse ou de carbonate de soude, agissant seules sur le composé mercuriel d'urée, il n'est pas nécessaire d'attendre que le liquide soit devenu limpide à la surface. On évite l'inconvénient et la perte de temps qu'il y a à soustraire avec une baguette une petite quantité de liqueur pour la faire réagir à part.

Le procédé analytique étant fondé sur des faits bien constatés, voici comment on doit opérer. On mesure avec une pipette 50 cent. cubes d'urine que l'on verse dans une fiole. On ajoute 25 cent. cub. d'eau de baryte, et on agite. Un précipité blanc très-complexe, principalement formé d'urate, de phosphate et de sulfate de baryte se produit. Après cinq minutes environ, on filtre; on remarque qu'il y a eu décoloration partielle; le liquide filtré est alcalin. Dans tous les cas cette proportion d'eau de baryte a été trouvée suffisante. Au moyen d'une pipette graduée, on mesure 10 ou 20 cent. cubes de l'urine ainsi préparée, que l'on verse dans un vase à précipité, posé sur une feuille de papier blanc. A l'aide d'une burette graduée, on verse d'une main la liqueur mercurielle, on agite de l'autre. De temps en temps, on ajoute de petites quantités d'une dissolution faite avec 25 grammes de potasse pour 1 litre. On arrive ainsi à voir apparaître le précipité jaune qui tranche sur la couleur blanche du précipité et du papier. Ce premier

dosage ne fait qu'indiquer approximativement vers quelle division se trouve la limite exacte; on en opère deux ou trois autres qui sont toujours rapprochés et dont on prend la moyenne. Il suffit, la lecture opérée sur la burette, d'un calcul très-simple, puisque chaque centimètre cube précipite 0 gr. 005 d'urée : on n'oubliera pas qu'on a opéré sur un liquide mélangé avec l'eau de baryte dans la proportion d'un tiers. Avec un peu d'habitude on arrive à exécuter dans une demi-heure un dosage d'urée très-exact.

Nous avons fait, sur les précipités obtenus avec des échantillons variés d'urine, les mêmes essais et analyses déjà décrits, avec des résultats identiques. Après avoir exécuté un dosage, ainsi qu'il vient d'être dit, si l'on ajoute à la même urine de l'urée dans la proportion de 0 gr. 50 pour 1 litre, en recommençant l'opération on retrouvera sûrement cette addition. Le procédé est applicable sans modification à l'urine qui renferme du glucose.

Détermination de l'acidité de l'urine et dosage de l'acide urique.

La détermination du degré d'acidité de l'urine, simple en apparence, demande toutefois qu'on s'entoure de certaines précautions. Nous faisons avec 11 gr. 91 cent. de potasse fondue et de l'eau distillée 1 litre de dissolution; chaque centimètre cube renfermera 1 centigr. de potasse anhydre. Dans un vase à précipité posé sur une feuille de papier blanc, on met 50 c. cubes d'urine et on ajoute quelques gouttes de teinture bleue de tournesol. On place à côté de soi dans un verre, un peu de la même urine, qu'on colore de la même manière. Au moyen d'une burette graduée, on verse la dissolution alcaline, en agitant constamment, jusqu'à ce qu'il y ait passage du rouge au bleu. Cette transition n'est pas toujours facile à saisir nettement, à cause de la couleur propre de l'urine. On a pour s'aider la comparaison avec la coloration primitive, l'apparition d'un léger nuage blanc quand la limite a été dépassée, trouble dû à la précipitation d'un peu de phosphate de chaux; enfin nous conseillons de se servir de papier de tournesol lilas, dont on laisse tomber quelques fragments dans le vase où l'on opère. En ne négligeant aucune de ces précautions, on arrive à connaître la quantité exacte de potasse nécessaire à la neutralisation d'un volume connu d'urine.

Pour doser l'acide urique, qui existe dans l'urine, n'importe pour le moment sous quel état, il faut opérer sur une quantité minimum de 200 cent. cub., qu'on mélange avec le 1/4 environ d'acide chlorhydrique. Ce liquide est abandonné pendant trois ou quatre jours dans une éprouvette bien propre, à verre poli, et qu'on peut enduire intérieurement d'une couche imperceptible de paraffine ou de cire blanche. On voit apparaître sur les parois et principalement au fond un dépôt brun-rougeâtre, qui est tout entier formé d'acide urique cristallisé, souillé par un peu de la matière colorante, modifiée elle-même par l'acide chlorhydrique : la solubilité de l'acide hippurique s'oppose à sa précipitation, et nous n'avons d'ailleurs jamais pu vérifier son existence dans le dépôt. Au bout du temps indiqué, la précipitation est complète; on décante avec soin, et comme les cristaux d'acide urique sont relativement volumineux, ils se déposent rapidement. On les transvase dans une petite capsule ou sur un verre de montre, et après dessiccation on pèse.

Deux causes d'erreur viennent fausser le chiffre obtenu : la première est due à la solubilité de l'acide urique qui, quoique faible, est cependant sensible : la seconde est due à ce qu'il est impossible de laver et de rasembler le dépôt sans en perdre. La première cause peut être appréciée assez exactement; la seconde dépend du plus ou moins de soin et de dextérité apportés à l'opération. Nous avons fait une solution d'urée, de sulfate, de phosphate et chlorure de sodium, dans la proportion moyenne indiquée plus loin pour la composition de notre urine. Nous l'avons mélangée avec le quart de son volume d'acide chlorhydrique et nous en avons fait digérer 225 centim. cubes avec 20 centigr. d'acide urique recueilli dans nos analyses : après quatre jours nous avons retrouvé 181 milligr. D'un autre côté, ayant lavé avec la même eau (environ 100 centim. cubes) les dépôts obtenus dans cinq essais successifs et ayant comparé au poids total obtenu celui de l'acide urique qui s'est déposé des eaux du lavage, nous avons trouvé que la perte ainsi évaluée approximativement était 1/17e. En chiffres ronds, nous évaluons l'erreur totale au dixième du poids de l'acide urique que l'on pèse. Les chiffres inscrits dans nos tableaux ont subi cette correction. Nous aurons dans la deuxième partie de notre thèse, en analysant les résultats, à discuter cette question si débattue de la cause de l'acidité des urines, et nous aurons occasion, en indiquant nos expériences, de nous prononcer sur ce sujet.

Dosage de l'acide phosphorique. — Nous avons adopté pour le dosage de l'acide phosphorique un procédé généralement peu suivi et cependant d'une grande précision ; il est dû à M. Leconte, qui l'a développé dans sa thèse pour le doctorat en médecine intitulée : « Sur l'emploi de l'azotate d'urane dans la recherche et le dosage de l'acide phosphorique et des phosphates ; Paris, 1853. » Les sels d'urane ont été depuis cette époque à plusieurs reprises et par différents chimistes, signalés comme réactifs des phosphates. Nous croyons que la première indication revient à l'auteur cité, et on trouvera dans son travail des expériences de comparaison qui ne permettent pas de douter de la sensibilité du réactif, expériences que nous avons contrôlées presque entièrement, et que nous ne pouvons reproduire ici.

Lorsqu'on mélange deux dissolutions, l'une d'azotate d'urane, l'autre d'un phosphate, neutres ou légèrement acidifiées par l'acide acétique, il apparaît presque instantanément un précipité jaunâtre, floconneux, dense, se rassemblant dans un temps relativement court, de manière à être surnagé par une liqueur limpide. Ce précipité est du phosphate d'urane qu'on peut facilement recueillir, détacher du filtre et peser. Seuls les arséniates donnent, dans les mêmes conditions, un précipité d'une insolubilité presque aussi parfaite. Le dosage peut être effectué en présence des sulfates, chlorures, nitrates, pourvu que l'acide phosphorique soit à l'état de phosphate de la forme $3\,MO, PhO^5$, état sous lequel il est toujours facile de le ramener par une des méthodes décrites dans tous les traités d'analyse. Les cyanoferrures alcalins sont, après les phosphates, le réactif le plus sensible des sels d'urane avec lesquels ils forment un cyanoferrure double d'une coloration brune-rougeâtre, réaction qui a

été utilisée par M. Leconte pour donner les règles d'un procédé volumétrique. Mais, après de nombreux essais, nous n'avons pu admettre les chiffres donnés, et on verra, par les quelques détails qui suivent, que nous n'avons pas conclu à la légère.

Ainsi que M. Leconte l'a fait, nous nous sommes servi, comme base de nos expériences, d'une dissolution de phosphate de soude ordinaire, ayant pour formule : $2NaO, HO, PO^5, 24\,HO$. Ce sel a été purifié par plusieurs cristallisations successives et obtenu en très-petits cristaux, que l'on dessèche très-facilement, sans les effleurir, en les comprimant entre plusieurs doubles de papier Berzélius. Nous avons déterminé l'eau de cristallisation des échantillons au moment de leur emploi. Pour 2 gr. de phosphate de soude cristallisé, nous avons obtenu les chiffres suivants : 1 gr. 249, 1 gr. 254, 1 gr. 253, le chiffre théorique étant 1 gr. 256. Suivant M. Leconte, 4 gr. 486 de ce sel renferment 1 gr. d'acide phosphorique supposé anhydre, de telle sorte qu'en faisant dissoudre cette quantité dans 1 litre d'eau distillée, chaque centimètre cube correspondrait à 0 gr. 001 d'acide phosphorique anhydre. En prenant les équivalents admis et vérifiés par tant de chimistes : $P=31$, $Na=23$, $O=8$, H, étant égal à 1 et la formule indiquée plus haut, on trouve que 5 gr. 042 de phosphate de soude renferment 0 gr. 001 de PO^5 par centimètre cube de dissolution faite avec de l'eau distillée, de manière à obtenir le volume d'un litre.

M. Leconte a admis comme formule du phosphate d'urane formé dans ces conditions : $3\,(U^2O^3), PO^5$. Il dit, en se fondant sur elle et sur celle de l'azotate d'urane parfaitement exacte : $U^2O^3, AzO^5, 6\,HO$, que 1 centimètre cube d'une solution faite avec 4 gr. 416 de ce dernier sel par litre correspond ou doit précipiter 0 gr. 0005 d'acide phosphorique anhydre. En prenant pour équivalent de l'uranium le nombre de 60 donné par M. Péligot et se basant sur la même formule, on trouve 5 gr. 324. Avant d'aller plus loin, nous dirons que l'azotate d'urane qu'on trouve en assez grande quantité dans le commerce, à cause de ses propriétés pyrogéniques et photogéniques, n'est pas pur et qu'il est de toute nécessité de le purifier soi-même. Pour cela, on le fait dissoudre dans l'éther; la dissolution est filtrée et abandonnée dans un endroit obscur à l'évaporation spontanée. On n'attend pas la fin de l'opération pour décanter une eau mère, laiteuse, jaunâtre. On traite de la même manière le sel qui s'est déposé, et finalement on le fait cristalliser, après l'avoir dissous dans l'eau distillée. La dissolution éthérée de ce sel exposé au soleil s'altère instantanément, altération que rendent sensibles des colorations successives rapides, aboutissant au brun noir.

Purifié ainsi, le sel correspond à la formule $U^2O^3, AzO^5, 6HO$; il perd son eau entre 130 et 140° et, comme contrôle pour savoir si la limite n'a pas été dépassée, on a la coloration verte que prend le corps fondu et surtout sa non complète solubilité lorsqu'on veut le faire redissoudre dans l'eau.

Ayant préparé deux solutions, la première que nous désignerons par *a*, avec phosphate de soude (5 gr. 042), la seconde, *b*, avec azotate d'urane (10 gr. 648), de manière à faire pour chacune le volume d'un litre, ayant, comme le prescrit M. Leconte, versé dans cette dernière quelques gouttes de dissolution de potasse et redissous

dans un peu d'acide acétique le précipité d'oxyde d'urane, il faudrait que des volumes égaux se neutralisent de manière que tout l'acide phosphorique et tout l'oxyde d'urane soient précipités. Or il n'en est pas ainsi.

En prenant 50 cent. cubes de chaque liqueur et suivant les quelques précautions indiquées plus loin, il faut, si le phosphate d'urane, formé dans ces conditions, correspond à la formule $3(U^2O^3), PO^5$, recueillir théoriquement 0 gr. 354. Comme moyenne de plus de dix pesées, dont les chiffres extrêmes sont 0 gr. 249 et 0 gr. 255, nous avons trouvé 0 gr. 252. Le ferrocyanure de potassium indique un excès de sel d'urane. La formule de M. Leconte ne peut donc être admise. On voit, au contraire, que le chiffre 0 gr. 252, trouvé directement, se rapporte à la formule $2(U^2O^3) HOPO^5$ ou $2(U^2O^3), PO^5$, après dessiccation et légère calcination. Théoriquement, en effet, le précipité doit peser 0 gr. 253, et il n'est guère possible de se trouver en plus parfaite concordance. La solution d'azotate d'urane, telle que 1 centimètre cube précipite 0 gr. 001 de PO^5, doit être fait avec 7 gr. 0985 de ce sel pour le volume de 1 litre à 15° centigrades.

Nous avons vérifié que les deux liqueurs se neutralisent d'une manière presque absolue à volumes égaux, et que le poids du précipité est d'accord avec notre formule. La solution titrée d'azotate d'urane se conserve parfaitement à la lumière; après plus d'une année, la même liqueur nous a présenté le même titre.

Cela posé, voici la manière de procéder : l'acide phosphorique contenu dans un poids donné d'une substance, est amené en solution neutre à l'état de phosphate alcalin, dans un volume connu, opération qui s'effectue par un des deux procédés principaux, savoir : par calcination ave un excès de carbonate de soude, ou par ébullition avec un acide minéral, tel que l'acide nitrique concentré, quand la substance y est soluble. Nous supposons qu'il n'y a pas mélange d'arséniate. Au moyen d'une pipette graduée, on mesure exactement 10 ou 20 c. cubes de la liqueur titrée d'azotate d'urane, et on les met dans un vase à précipité. A l'aide d'une burette graduée, on verse la dissolution renfermant l'acide phosphorique qu'il s'agit de doser. On a d'avance fait dissoudre dans eau 100 gr. 5 gr. de ferrocyanure de potassium. On en imprègne des bandelettes de papier à filtrer, qu'on fait sécher. Ce papier jaune mis en contact avec la liqueur d'azotate d'urane, prend une teinte brune-rouge ou simplement rose, si la quantité de sel est très-faible. En présence du phosphate d'urane seul il n'y a pas changement de coloration, même après plusieurs heures. En laissant tomber dans le vase où l'on fait l'essai, des fragments de 1 ou 2 millim. carrés, on en suit parfaitement la marche. On continue à verser avec soin la solution de phosphate, et il arrive un moment où le papier n'accuse plus de teinte. On est certain que la réaction est terminée, et on opère la lecture. Un premier essai indique la richesse approximative en acide phosphorique. On en répète deux ou trois autres, et on arrive ainsi à un dosage exact. Il ne faut pas oublier la précaution indispensable, de n'opérer que sur des liqueurs neutres, ou très-peu acidifiées par l'acide acétique. Tout en nous dispensant de rapporter toutes nos expériences, nous avons cru devoir entrer dans quelques détails; car ce procédé très-sensible est susceptible d'une grande généralité.

L'acide sulfurique, le chlore, la chaux, la magnésie,

la potasse, ont été dosés à l'état de baryte, de chlorure d'argent, d'oxalate de chaux pesé à l'état de carbonate, de phosphate ammonico-magnésien, de chloro-platinate, en nous conformant à la marche et aux prescriptions développées dans tous les ouvrages.

MESURE DE LA TEMPÉRATURE DE L'URINE AU MOMENT DE L'ÉMISSION.

Il nous a paru utile à notre sujet, et intéressant à la fois, d'apprécier la température de l'urine au moment de l'émission, et d'en suivre les variations. Pour le faire utilement, nous avons cru qu'il ne suffisait pas, après s'être muni d'un thermomètre bien vérifié, donnant le 10^e de degré, à échelle comprise entre 35 et 45° environ, de plonger son réservoir dans l'urine émise, après avoir chauffé vers 35° le vase dans lequel elle est recueillie. Dans ces conditions, on commet très-facilement une erreur qui peut aller jusqu'à 2 degrés, et le nombre trouvé est toujours trop faible. L'urine se refroidit d'abord, et surtout les premières portions, par son passage à travers le canal uréthral, qui, dans la plus grande partie de sa longueur, est à une température de 2 degrés environ au-dessous de la moyenne. Son arrivée dans l'air sous forme de jet, son contact avec des corps plus froids, l'évaporation, sont autant de causes d'erreur.

Nous avons imaginé, pour les éliminer autant que possible, une disposition commode et facile à réaliser soi-même. Le thermomètre est fixé, au moyen d'un bouchon en liége qui laisse à découvert le réservoir, au centre d'un entonnoir d'une capacité de 50 cent. cubes environ, en verre aussi mince que possible. On fait à la surface de ce bouchon quelques cannelures qui permettront à l'urine de s'écouler autour du réservoir du thermomètre engagé dans la douille sans en obstruer l'orifice. De la même manière, on fixe le petit entonnoir au centre d'un second d'une capacité d'environ 500 cent. cubes, qu'on engage dans l'orifice du flacon où sera recueillie l'urine. Les orifices représentés par la cannelure sont disposés de façon que l'arrivée de l'urine dans l'entonnoir central soit plus rapide que son écoulement; dès lors elle se déverse et vient former autour un espèce de bain-marie. Ainsi renouvellement du liquide autour du thermomètre au fur et à mesure de son émission, et échauffement des parties environnantes. Il est facile d'effectuer la lecture dans le court intervalle de temps compris entre la fin de la miction et l'écoulement non encore achevé de l'urine renfermée dans l'appareil. Nous croyons cette disposition commode, surtout pour celui qui fait des expériences sur lui-même ; elle permet d'obtenir des résultats plus exacts que les dispositions anciennes.

La hauteur du baromètre était observée le matin vers neuf heures, et très-souvent le soir. La température de la pièce où nous séjournions principalement a été notée. Soir et matin, comme après un exercice énergique ou un repos aussi absolu que possible, nous notions le nombre de pulsations, d'inspirations, la température de la bouche sous la langue et celle du pli de l'aine. Toutes ces données sont relatées dans les tableaux.

Marche de l'analyse. — Après avoir fait connaître les

procédés d'analyse suivis et nous être étendu sur ceux que nous avons modifiés, nous allons indiquer la marche suivie pour l'expérimentation, en ayant soin de n'omettre aucun détail essentiel.

Durant vingt et un jours, l'urine a été recueillie intégralement; nous avons noté l'heure de chaque émission et la température. Dès qu'elle était refroidie, jusque vers 15° centigrades, son volume était mesuré ; nous en prenions la densité au moyen d'un densimètre que nous avions fait construire, et dont les indications avaient été contrôlées par la méthode directe du flacon. Nous avons déjà dit pourquoi l'urine du matin était mélangée à celle de la veille, de sorte que dans nos expériences la journée commence vers huit heures pour se terminer le lendemain à la même heure. Après avoir obtenu la totalité des urines des vingt-quatre heures, nous mesurions de nouveau pour vérifier l'exactitude des mesures partielles ; nous prenions la densité du mélange.

Durant les deux premières heures nous procédions de la manière suivante à l'analyse : l'acidité était déterminée comme nous l'avons déjà exposé; nous opérions le plus souvent sur 50 c. cub., et nous faisions toujours deux essais. Disons en passant que nous n'avons pas pu vérifier toujours d'une manière bien nette cette première fermentation acide de l'urine généralement admise. Il nous est arrivé de faire, à cinq reprises, d'heure en heure, des déterminations d'acidité; durant les cinq premières heures, à part une seule fois où il y avait augmentation évidente, le résultat a été négatif; le chiffre de potasse restait invariable, puis il y avait diminution rapide. Une solution d'acide urique dans l'eau distillée s'altère dès le troisième jour; elle acquiert une odeur qui se rapproche beaucoup de l'odeur urineuse proprement dite la solubilité de ce corps étant très-faible, il ne nous a pas été possible de bien caractériser encore certains des produits de dédoublement. Dans l'urine, cette altération doit être plus rapide.

Nous passions ensuite à la détermination de l'urée en suivant les règles indiquées; trois quarts d'heure environ étaient suffisants pour répéter trois ou quatre fois l'essai; le chiffre obtenu était ainsi aussi approché que possible.

Dans une éprouvette à pied étaient mesurés 200 cent. cubes d'urine qu'on mélangeait avec 25 cent. cubes d'acide chlorhydrique : le quatrième jour l'acide urique déposé sur les parois et principalement au fond, était décanté, lavé et pesé. Nous répétons que ce dosage est une affaire de soin, que l'acide urique se rassemble et se lave très-facilement par décantation, que 2 centigrammes occupent un volume relativement considérable, et en apportant aux pesées les précautions voulues, on doit considérer ces chiffres, après la correction indiquée, comme très-voisins du chiffre vrai. La balance employée était sensible au milligramme, et le système des doubles pesées toujours suivi.

500 cent. cubes d'urine étaient mis à évaporer dans une capsule de porcelaine; l'évaporation était effectuée dans un espace limité, mais où l'air pouvait circuler, au moyen d'une petite lampe à alcool; l'habitude nous apprenait à en régler la flamme, de manière à éviter l'ébullition. Le liquide réduit en consistance sirupeuse était décanté dans une capsule de dimension moindre, où l'évaporation s'achevait. On doit obtenir, si l'on opère bien, un résidu d'un brun jaunâtre particulier, boursouflé, facile à dessécher; cette dessiccation était faite sans jamais dépasser 100°; on n'a pas ainsi à craindre d'altération. Deux ou trois pesées étaient effectuées et donnaient le poids total des matières solides de l'urine. Nous faisions toujours à part, une évaporation portant sur 50 cent. cubes et servant de contrôle. Si un commencement de carbonisation avait été produit, l'expérience était recommencée; le résidu était utilisé pour l'analyse des sels.

Ces diverses opérations étaient effectuées jour par jour; on comprend en effet que l'on ne peut les renvoyer de plus de quarante-huit heures, sous peine de s'exposer à des erreurs considérables, à cause de la prompte décomposition des diverses autres substances.

Nous n'avons même pas songé à entreprendre le dosage de la créatine, de la créatinine, de l'acide hippurique et de quelques autres composés, dont on a signalé la présence à l'état normal. Ces substances sont en quantité relativement trop faible, et les procédés de dosage trop peu sensibles.

Des expériences antérieures nous avaient appris que l'ammoniaque n'existe pas dans nos urines, même plusieurs heures après l'émission. Nous ne voudrions pas généraliser et conclure que l'ammoniaque n'est pas à l'état normal un principe constituant de l'urine. Nous penchons toutefois très-fort vers cette dernière opinion qui est celle de la presque totalité des chimistes allemands et de Lehmann entre autres. Les essais faits sur les urines de quatre ou cinq personnes nous ont donné des résultats négatifs. Nous nous servions pour cette recherche du procédé qui consiste à chauffer, dans un ballon de 1 demi-litre environ, de l'urine additionnée de

10 grammes de magnésie calcinée. Comme l'a surtout montré M. Pasteur, cette substance n'a pas d'action sur les matières azotées très altérables. On adapte au ballon un tube de Liebig renfermant de l'acide chlorhydrique faible; le bichlorure de platine n'a jamais donné le moindre précépité. Ce fait a, selon nous, une importance considérable, malgré la facilité avec laquelle certains sels ammoniacaux sont éliminés par les urines, après leur ingestion en faible quantité. La présence de l'ammoniaque bien constatée, en quantité bien sensible, serait pour le médecin un symptôme qui aurait sa gravité dans le cas de non-altération dans la vessie.

Le dosage des sels n'a été commencé que vers la fin de février; nous n'avions pas à craindre en effet de déperdition pour les substances minérales fixes, et dans l'état actuel des connaissances chimiques il est d'une impossibilité absolue d'isoler des urines les différents sels à l'état sous lequel ils y sont réellement renfermés. Nous ne pensons pas davantage, qu'après avoir dosé les acides et les bases, on fasse un travail vraiment scientifique en essayant de grouper ces corps et de les associer. Lorsqu'on a affaire à un mélange de plusieurs sels en dissolution, les données relatives à la répartition de leurs éléments, sont trop peu certaines pour permettre de l'effectuer de la même manière. Cette observation est surtout vraie pour un liquide aussi complexe que l'urine, et on ne sera pas étonné que nos tableaux ne portent pas inscrits les chiffres provenant de ces calculs. Quand je précipite et que je pèse l'acide phosphorique à l'état de phosphate d'urane, l'acide sulfurique à l'état de sulfate de baryte, etc., je suis certain de l'exactitude des chiffres déduits de mes pesées. Mais, lorsque j'écris, en partant de là, qu'il y a tant de phosphate de soude, tant de phosphate de chaux, tant de sulfate de potasse, tant de chlorure de sodium, j'avoue qu'il est impossible d'en connaître les proportions relatives, tout en étant certain de leur existence simultanée. On trouve dans presque tous les traités qui traitent des urines, un moyen d'évaluer séparément les phosphates alcalins et les phosphates alcalino-terreux en précipitant par l'ammoniaque : il suffit de signaler le procédé, pour montrer combien il est défectueux; et étant donné un mélange de phosphates, sulfates, chlorures, etc., de soude, potasse, chaux, magnésie, je ne connais pas de moyen de séparer ces sels tels qu'ils existent réellement. J'ai présenté ces considérations, parce qu'on fait bien souvent aux chimistes le reproche de ne pouvoir isoler en totalité les substances telles qu'elles existent dans les divers liquides de l'organisme. C'est vouloir exiger d'eux plus qu'ils ne peuvent tenir.

L'examen microscopique des urines, des dépôts spontanés ou provoqués, conduit, dit-on, qualitativement à ce résultat. Nous répondrons que les résultats qualitatifs, malgré leurs indications fort précieuses parfois, n'auront jamais l'importance des résultats quantitatifs, et puis on se fait souvent illusion. Il n'en faut pas accuser cet instrument admirable que nous devrions toujours consulter, mais ceux qui tirent des déductions erronées ou ne s'aperçoivent pas des changements qu'ils provoquent dans le cours de leur expérience.

Ces observations me dispenseront d'entrer dans les détails concernant les changements que la carbonisation fait éprouver aux matériaux de l'urine.

Les produits d'évaporation bien desséchés ont été carbonisés à la flamme simple de la lampe à alcool et l'action de la chaleur continuée jusqu'à ce qu'on ait obtenu un charbon poreux ne donnant plus lieu à un dégagement de vapeurs. Des expériences antérieures, confirmées d'ailleurs par les observations de tous ceux qui ont bien suivi la marche des phénomènes, nous ont appris que l'incinération complète, outre qu'elle est fort longue, donne lieu à une déperdition très-notable de phosphore et de soufre principalement.

Le résidu charbonneux, après avoir été pesé, était traité à plusieurs reprises et à l'ébullition par l'eau acidulée par l'acide nitrique. Après l'avoir épuisé, il était séché et pesé de nouveau : la différence de poids représente celui des substances minérales fixes contenues dans un volume connu d'urine et actuellement en dissolution. En incinérant une portion du charbon ainsi lavé, on peut s'assurer qu'il ne laisse que des quantités presque impondérables de cendres, constituées par un peu de silice.

On n'a plus dès lors qu'une analyse ordinaire à effectuer. Un volume déterminé de la dissolution était neutralisé pour le dosage de l'acide phosphorique au moyen de la liqueur d'azotate d'urane. Les autres substances étaient séparées et dosées à l'état de sulfate de baryte, d'oxalate de chaux, de chlorure d'argent, de phosphate ammoniaco-magnésien, de chloro-platinate de potasse, donnant l'acide sulfurique, la chaux, le chlore, la magnésie et la potasse.

Connaissant d'une part les substances solides de l'urine des vingt-quatre heures, d'un autre côté les matières minérales en totalité, la différence exprime le poids des substances organiques; si on retranche de

cette dernière quantité le poids de l'urée et de l'acide urique dosés directement, la différence exprimera celui des matières organiques non dosées, telles que l'acide hippurique, la créatine, la créatinine, etc. En retranchant du poids total des substances minérales la somme de celles qui ont été déterminées directement la différence exprimera celui des matières non dosées, telles que la soude, le fer, etc.

Ces dosages étaient effectués successivement et par séries, par conséquent dans les mêmes conditions : la même marche a été suivie pour les matières fécales, le pain servant d'aliment, l'eau de boisson. Commencés à la fin du mois de février, ils ont été terminés dans les premiers jours d'avril.

Du 1er février au 9 et du 23 au 25, c'est-à-dire pendant douze jours, l'alimentation était mixte, mais principalement animale. Les repas, au nombre de trois, le premier peu important à huit heures du matin, les deux autres à onze et six heures. Nous n'avons noté ni la quantité des substances prises à l'intérieur, ni l'occupation journalière : nous savions déjà qu'il n'est possible de rien conclure sans une alimentation uniforme, et si nous avons minutieusement et jour par jour déterminé la composition des urines pendant ces douze jours, c'est parce que nous voulions connaître l'influence du régime et établir des comparaisons utiles.

Du 14 février au 22, c'est-à-dire durant neuf jours, nous nous sommes soumis à une alimentation régulière. Nous faisions aux heures suivantes quatre repas par jour, mesurant l'eau et le pain.

Premier repas à huit heures et demie avec....	Pain, 125 gr. Eau, 250
Deuxième repas à onze heures et demie, avec..	Pain, 250 Eau, 500
Troisième repas à trois heures et demie, avec..	Pain, 125 Eau, 250
Quatrième repas à six heures et demie, avec...	Pain, 250 Eau, 500

Nous avons par vingt-quatre heures un total de 750 grammes de pain et de 1,500 cent. cubes d'eau. Avec ce régime, l'organisme ne peut être en souffrance. Aucun symptôme particulier ne s'est manifesté chez nous pendant que nous l'avons suivi. Notre poids n'a pas subi de variation, et il s'est maintenu à 53 kilog. Malheureusement l'habitude où nous sommes d'absorber des substances d'une sapidité très-différente fait que nous avions besoin parfois de tout notre courage pour arriver au bout de la ration. Nous aurions pu adopter un régime uniforme moins sévère; mais nous avons cru qu'il était préférable de nous mettre dans des conditions telles qu'il serait facile à tous ceux qui en auraient le désir sincère, de contrôler nos résultats.

Voici quel a été l'emploi des neuf journées; plus loin sur les tableaux nous donnons toutes les indications; mais nous devons ici quelques détails sur ces mots: repos, travail musculaire, travail cérébral : en les lisant on ne perdra pas de vue les courtes explications données au début de notre thèse. La durée du sommeil a été à très-peu près uniforme et de sept heures effectives, le coucher ayant lieu vers onze heures, le lever vers sept.

Les premières heures de chaque journée étaient consacrées aux dosages qui ne pouvaient subir de retard.

14 février. De midi à trois heures et de quatre à cinq, exercice musculaire en plein air, consistant à bêcher la terre dans notre jardin; le soir, de huit à dix, marche.

Le 15. De midi à deux heures, même exercice que la veille; le soir, de cinq à six et de huit à dix, marche. Journée de travail musculaire moins énergique que la veille.

Le 16. De midi à trois heures et de quatre à cinq, travail de tête consistant en étude sur la géométrie analytique; de huit à dix heures et demie, lecture attentive de physiologie.

Journée de travail cérébral.

Le 17. De dix heures du matin jusqu'au lendemain sept heures, repos au lit et dans l'obscurité presque complète. Les mouvements indispensables étaient seuls accomplis. Durant ce temps, nous avons été dans un repos relatif d'esprit assez complet à part la dernière partie de la nuit qui a été un peu agitée, agitation qui a eu certainement une légère influence sur le résultat. — Journée de repos.

Le 18. Très-peu de marche : pas de travail de tête spécial; la journée s'est écoulée fort paisiblement; le soir, conversation; sommeil tranquille; journée de repos.

Le 19. De midi à trois heures, travail de tête consistant en calcul algébrique; de quatre à cinq, rédaction de l'introduction de cette thèse : de huit à onze, audition au théâtre lyrique d'une pièce de musique; vers neuf heures et demie, miction abondante dont nous n'avons pas pu noter la température, et qui a été effectuée dans un flacon; — sommeil tranquille.

Le 20. De midi à deux heures, marche en plein air: de deux à trois et de quatre à cinq, ascension et descente alternative et régulière de 4,500 marches d'escalier représentant une hauteur de 600 mètres. Nous notons plus loin sur le tableau l'augmentation du nombre des pulsations d'inspiration et celle de la température du corps. Le soir, deux heures de marche; — sommeil tranquille.

Journée de travail musculaire.

Le 21. Légère courbature dans les membres inférieurs; peu de marche, peu d'occupation d'esprit;—journée de repos.

Le 22. De midi à trois heures, étude sur la chimie philosophique de M. Wurtz; de quatre à cinq, rédaction d'une partie de cette thèse, relative à l'analyse: le soir exaltation factice de l'imagination; — sommeil tranquille.

Par ces indications sommaires nous n'avons qu'un seul but; celui de faire connaître d'une façon relative l'état de repos, d'activité musculaire, ou d'activité cérébrale, qui a dominé dans les vingt-quatre heures. Il est hors de doute, que dans la veille, et très-souvent dans le sommeil, la pensée n'est jamais en repos absolu; mais la différence de travail qu'elle produit lorsqu'elle est surexcitée, et lorsqu'elle est dans ce repos relatif, est de même ordre que celle qui existe, incontestablement, entre le travail que produit un homme qui fait paisiblement, dans sa journée, quelques heures de marche, et celui qu'effectue le terrassier qui péniblement déploie ses forces.

Nous pensons que les détails dans lesquels nous sommes entré, en évitant autant que possible les longueurs, suffiront pour faire comprendre les tableaux qui suivent, et dans lesquels sont consignés les résultats d'observation et d'analyse.

Dans le seul but de ne pas trop détourner l'attention de l'objet principal de cette thèse, nous n'avons pas produit pour les douze jours de régime mixte le tableau relatif au nombre des mictions, à la quantité, à la température, à la densité correspondante. Ces indications sont d'ailleurs de même ordre que celles qui sont consignées dans le tableau n° 1. Quant à la variation de la température au moment de l'émission, la loi est la même que celle qui sera établie sur les neuf jours.

Nous avons, comme on le fait habituellement avec raison, rapporté au kilogramme les poids des diverses substances dosées.

Tableau n° 1, *portant indication, jour par jour, des diverses données relatives à la température, la pression barométrique, les heures d'émission, la quantité de matières fécales.*

JOURS DU MOIS, ETC.	HEURES d'émission	TEMPÉRATURE au moment de l'émission.	MOYENNE de la Température.	QUANTITÉS d'urine.	TOTAL des 24 heures.	DENSITÉ.	POIDS des matières fécales.
14 février.........	3 h. s.	38°		15[illegible] c.c.		1.025	
Pression : 0.7508..	8 h. s.	37°8	37.7	150	670 c. c.	1.025	0 gr.
Pulsa- { soir. 61 tions.. { matin. 54	7 h. m.	37°4		150		1.01[illegible]	
Jour de trav. musc.				365			
15 février.........	1 h. 1/2 s.	37°8		240 c.c.		1.011	
Pression : 0.7600.	8 h. s.	37°8	37.7	310	960 c. c.	1.014	150 gr.
Pulsa- { soir. 60 tions. { matin. 53	8 h. m.	37°5		410		1.009	
Jour de trav. musc.							
16 février.........	12 1/2 s.	37°8		260 c.c.		1.008	
Pression : 0.7744..	3 1/2 s.	27°5	37.77	140		1.0[illegible]9	200 gr.
Pulsa- { soir. 62 tions.. { matin. 50	[illegible] 1/2 s.	38°		310	1370 c. c.	1.008	
Jour de travail.....	10 1/0 s.	38°		200		10.013	
cérébral...	7 h. m.	37°5		400		1.008	
17 février.........	2 1/2 s.	38°		355 c.c.		1.009	
Pression : 0.7602..	5 h. s.	38°	37.8	325	1065 c. c.	1.009	0 gr.
Pulsa- { soir. 61 tions. { matin. 52	1 h. m.	37°6		325		1.012	
Jour de repos......	9 h. m.	37°6		100		1.017	
18 février.........	12 1/2 s.	37°8		340 c.c.		1.006	
Pression : 0.7623..	2 1/2 s.	38°2	37.9	260	1425 c. c.	1.006	130 gr.
Pulsa- { soir. 58 tions.. { matin. 49	5 h. s	38.1		315		1.011	
Jour de repos......	11 h. s.	38°		610		1.011	
—	8 h. m.	37°5		3[illegible]0		1.010	
19 février.........	1 1/2 s.	37°8		240 c.c.		1.009	
Pression : 0.7518..	5 h. s.	38°		225		1.0[illegible]	
Pulsa- { soir. 61 tions.. { matin. 52	7 h. s.	37°5	37.7	150	1410 c. c.	1.00[illegible]	120 gr.
Jour de travail.....	9 1/2 s.	[illegible]		570		1.004	
cérébral......	8 h. s.	37°5		215		1.0[illegible]9	
20 février.........	1 1/2 s.	37°8		205 c.c.		1.010	
Pression : 0.7653..	4 1/2 s.	38°2	37.8	100		1.012	125 gr.
Pulsa- { soir.. 60 tions. { matin. 5[illegible]	11 h. s.	38°		170	625 c. c.	1.021	
Jour de trav. musc. (1).	8 h. m.	37°2		150		1.023	
21 février.........	3 1/2 s.	37°9		150 c.c.		1.018	
Pression : 0.7622..	7 h. s.	37°6	37.77	110	980 c. c.	1.009	
Pulsa- { soir. 60 tions. { matin. 53	10 h. s.	38°2		530		1.006	0 gr.
Jour de repos......	8 h. m.	37°4		190		1.021	
22 février.........	1 h. s.	37°9		520 c.c.		1.007	
Pression : 0.7595..	3 h. s.	38°		150		1.00[illegible]	
Pulsa- { soir. 60 tions. { matin. 54	5 1/2 s.	38°	37.86	115	1180 c. c.	1.013	110 gr.
Jour de travail cé-	9 h. s.	38°		145		1.017	
rébral..........	8 h. m.	37°4		250		1.017	

(1) Après chaque heure d'ascension, le nombre des pulsations s'était élevé de 66 à 72, le nombre d'inspirations de 19 à 24.

NOTA. La température, prise sous la langue, a toujours été trouvée égale, à 37°7, ou 37°9. — La température au pli de l'aine était égale à 36°8. Après un travail musculaire, elle s'élevait jusqu'à 37°4.

Tableau n° 2, *indiquant pour les 12 jours de régime mixte et par vingt-quatre heures les quantités d'urine, la densité, l'acidité, l'urée, l'acide urique, etc.*

JOURS.	QUANTITÉ d'urine en centimètres cubes.	DENSITÉ.	ACIDITÉ en potasse anhydre.	URÉE.	ACIDE URIQUE.	SUBSTANCES SOLIDES.	SELS MINÉRAUX anhydres.	TOTAL des substances dosées.	DIFFÉRENCES représentant les substances organiques non dosées.
Février.	c. c.		gr.	gr.	gr.	gr.	gr.	gr.	gr.
1	1380	1017	0.790	33.33	0.455	52.995	12.585	46.370	6.615
2	1350	1018	0.310	35.64	0.193	56.310	12.893	48.726	7.584
3	1665	1017	0.620	37.46	0.421*	57.970	13.468	51.349	6.621
4	1435	1019	0.810	38.75	0.670	59.350	13.584	53.004	6.349
5	1730	1016	0.390	33.74	0.275	53.810	12.803	46.818	6.992
6	1435	1018	0.500	33.36	0.485	53.965	12.906	46.631	7.334
7	1510	1017	0.310	32.84	0.222	51.970	12.410	45.472	6.498
8	1145	1022	0.310	41.22	0.516	60.012	13.279	55.015	4.99
9	1135	1020	0.320	35.75	0.499	56.123	14.338	50.647	5.476
23	1310	1015	0.450	27.51	0.400	47.737	11.796	39.706	8.431
24	1355	1015	0.270	29.47	0.150	54.661	13.7505	42.7505	11.9105
25	1780	1013	0.350	34.60	0.200	54.962	13.092	47.892	7.070

Tableau n° 3, *indiquant pour les 12 jours de régime mixte et par vingt-quatre heures les quantités des substances minérales.*

JOURS.	ACIDE phosphorique anhydre.	ACIDE sulfurique anhydre.	CHLORE.	CHAUX.	MAGNÉSIE.	POTASSE.	TOTAL des substances dosées.	TOTAL des sels anhydres.	DIFFÉRENCE représentant la soude, fer, etc.
Févr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
1	1.9645	0.9663	4.3672	0.2543	0.1367	0.3662	8.2532	12.5850	4.3318
2	2.0345	0.9873	4.3456	0.3062	0.1325	0.3567	8.1628	12.8930	4.7302
3	2.1657	1.0347	4.9031	0.2987	0.1452	0.3897	8.9371	13.4680	4.5309
4	2.0347	0.9894	4.9360	0.2134	0.1831	0.3212	8.6778	13.5810	4.9032
5	1.8974	0.8975	4.8743	0.2231	1.1872	0.4031	8.4826	12.8030	4.3204
6	1.8635	0.9071	4.5431	0.2165	0.1734	0.4032	8.1048	12.8060	4.7012
7	1.8120	1.3327	4.0770	0.2416	0.1525	0.3350	8.0108	12.4100	4.4012
8	2.2034	1.0347	4.2123	0.2987	0.1674	0.3162	8.2927	13.2790	4.9863
9	2.2700	1.0401	4.8024	0.2737	0.1631	0.3650	8.9123	14.3980	5.4857
23	1.4779	1.0312	4.3387	0.2075	0.1327	0.3521	7.7601	11.7960	4.0359
24	1.7649	1.4715	5.4742	0.2168	0.1437	0.3640	9.4317	13.1305	3.6988
25	2.1634	0.9378	1.1372	0.3057	0.1294	0.3877	8.0609	13.0920	5.0311

12 23'5459 16'483 3'0322 2'8696 4'400 55'6567
52'4311 4'4001 10'8668 156'2335

Tableau n° 4 *indiquant rapportées au kilogramme les substances inscrites au Tableau n° 2.*

JOURS.	QUANTITÉ d'urine.	URÉE.	ACIDE urique.	SUBSTANCES solides.	SELS minéraux.	SUBSTANCES organiques non dosées.
Février.	gr.	gr.	gr.	gr.	gr.	gr.
1	26	0.6288	0.0086	0.9997	0.2375	0.1248
2	25	0.6724	0.0036	1.0625	0.2433	0.1431
3	31	0.7068	0.0079	1.0938	0.2541	0.1249
4	27	0.7311	0.0126	1.1198	0.2562	0.1198
5	33	0.6306	0.0052	1.0153	0.2416	0.1319
6	27	0.6294	0.0088	1.0182	0.2416	0.1384
7	28	0.6196	0.0042	0.0806	0.2342	0.1226
8	22	0.7777	0.0097	1.1323	0.2505	0.0943
9	21	0.6745	0.0094	1.0580	0.2717	0.1033
23	25	0.5191	0.0075	0.9011	0.2225	0.1594
24	26	0.5560	0.0028	1.0312	0.2477	0.2247
25	23	0.6528	0.0038	1.0370	0.2470	0.1334

Tableau n° 5, *indiquant rapportées au kilogramme les substances inscrites au Tableau n° 3.*

JOURS.	ACIDE phosphorique.	ACIDE sulfurique.	CHLORE.	CHAUX.	MAGNÉSIE.	POTASSE.	SOUDE. fer, etc.
Février.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
1	0.0371	0.0182	0.0862	0.0048	0.0030	0.0065	0.0911
2	0.0384	0.0186	0.0820	0.0056	0.0025	8.0067	0.0892
3	0.0409	0.0185	0.0925	0.0056	0.0027	0.0074	0.0855
4	0.0384	0.0187	0.9031	0.0040	0.0035	0.0061	0.0925
5	0.0358	0.0169	0.0920	0.0042	0.0035	0.0076	0.0815
6	0.0352	0.0171	0.0857	0.0040	0.0033	0.0076	0.0887
7	0.0342	0.0255	0.0159	0.0046	0.0029	0.0071	0.0830
8	8.0416	0.0195	0.0795	0.0056	0.0032	0.0071	0.0941
9	0.0328	0.0196	0 0906	0.0051	0.0031	0.0069	0.1035
23	0.0279	0.0195	0 0860	0.0039	0.0025	0.0066	0.0761
24	0.0332	0.0278	0.1032	0.0041	0.0027	0.0069	0.0698
25	0.0406	0.0177	0.0781	0.0358	0.0024	0.007	0.0949

Tableau n° 6, *indiquant pour les 9 jours de régime uniforme et par vingt quatre heures les quantités d'urine, la densité, etc.*

NOTA. — La lettre *r* veut dire repos ; *m*, travail musculaire ; *c*, travail cérébral.

JOURS.	QUANTITÉ D'URINES.	DENSITÉ.	ACIDITÉ en potasse anhydre.	URÉE.	ACIDE URIQUE.	SUBSTANCES SOLIDES.	SELS minéraux anhydres.	TOTAL des substances dosées.	DIFFÉRENCES représentant les substances organiques non dosées.
Février.			gr.	gr.	gr.	gr.	gr.	gr.	gr.
14 *m.*	670	1018	0.250	24.12	0.180	31.865	6.070	30.370	1.495
15 *m.*	960	1012	0.200	21.60	0.205	26.663	4.583	26.388	0.275
16 *c.*	1370	1009	0.100	22.60	0.105	29.113	5.770	28.475	0.638
17 *r.*	1065	1011	0.100	19.97	0.111	30.033	6.046	26.127	3.906
18 *r.*	1525	1008	0.050	19.37	0.047	27.731	5.187	24.604	3.127
19 *c.*	1410	1009	0.100	24.25	0.116	30.033	4.625	28.990	1.043
20 *m.*	625	1017	0.450	22.97	0.282	31.563	4.049	27.301	4.262
21 *r.*	980	1012	0.200	22.05	0.237	29.518	5.086	27.373	2.145
22 *c.*	1180	1012	0.150	24.78	0.187	38.716	6.997	31.964	6.752

Mean

3 days m } 22·896

3 day c } 23·876

3 days r } 20·463

9 | 201·70 / 22·412

Tableau n° 7, *indiquant pour les 9 jours de régime uniforme et par vingt-quatre heures les quantités des substances minérales.*

JOURS.	ACIDE PHOSPHORIQUE anhydre.	ACIDE SULFURIQUE anhydre.	CHLORE.	CHAUX	MAGNÉSIE.	POTASSE.	TOTAL des substances dosées.	TOTAL des sels anhydres.	DIFFÉRENCES représentant la soude, fer, etc.
Février.	gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
14 *m.*	1.7688	0.4879	1.2864	0.1149	0.1245	0.2947	4.0772	6.070	1.9928
15 *m.*	1.3924	0.3325	0.5376	0.1367	0.1097	0.2731	2.8020	4.583	1.7810
16 *c.*	2.3275	0.9767	0.4932	0.1413	0.1121	0.2421	4.2929	5.770	1.4771
17 *r.*	1.8105	0.5417	1.1076	0.1384	0.1075	0.2363	3.9422	6.046	2.1038
18 *r.*	1.1400	0.2937	1.1286	0.1135	0.1089	0.2477	3.0321	5.187	2.1546
19 *c.*	1.6431	0.9361	0.3384	0.1128	0.1125	0.2871	3.4000	4.625	1.2250
20 *m.*	1.2625	0.3231	0.8137	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Tableau n° 8, *indiquant rapportées au kilogramme les substances inscrites au tableau n° 6.*

JOURS.	QUANTITÉ d'urine.	URÉE.	ACIDE urique.	SUBSTANCES solides.	SELS minéraux	SUBSTANCES organiques non dosées.
Février.	gr.	gr.	gr.	gr.	gr.	gr.
14 m.	13	0.4551	0.0034	0.6012	0.1145	0.0282
15 m.	18	0.4075	0.0039	0.5031	0.0865	0.0050
16 c.	26	0.4264	0.0020	0.5493	0.1090	0.0120
17 r.	20	0.3768	0.0021	0.5667	0.1141	0.0737
18 r.	27	0.3655	0.0009	0.5232	0.0999	0.0590
19 c.	27	0.4575	0.0022	0.5667	0.0873	0.0197
20 m.	12	0.4334	0.0053	0.5955	0.0764	0.0804
21 r.	18	0.4160	0.0035	0.5569	0.0960	0.0405
22 c.	22	0.4675	0.0035	0.7305	0.1305	0.1274

Tableau n° 9, *indiquant rapportées au kilogramme les substances inscrites au tableau n° 7.*

JOURS.	ACIDE phosphorique.	ACIDE sulfurique	CHLORE.	CHAUX.	MAGNÉSIE	POTASSE.	SOUDE, fer, etc.
Février.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
14 m.	0.0334	0.0092	0.0243	0.0022	0.0023	0.0056	0.0338
15 m.	0.0263	0.0067	0.0101	0.0026	0.0021	0.0052	0.0336
16 c.	0.0439	0.0184	0.0093	0.0027	0.0021	0.0046	0.0278
17 r.	0.0342	0.0102	0.0209	0.0026	0.0020	0.0045	0.0397
18 r.	0.0215	0.0055	0.0213	0.0021	0.0021	0.0047	0.0406
19 c.	0.0304	0.0177	0.0064	0.0027	0.0021	0.0054	0.0231
20 m.	0.0238	0.0061	0.0154	0.0023	0.0022	0.0059	0.0206
21 r.	0.0297	0.0105	0.0271	0.0024	0.0021	0.0052	0.0189
22 c.	0.0376	0.0173	0.0080	0.0022	0.0023	0.0052	0.0576

Tableau n° 10, *indiquant la composition du pain, de l'eau de boisson, des matières fécales, les quantités totales des substances prises par le tube digestif, et celles rejetées par les urines et les fèces, les différences entre les deux, pour les 9 jours, par vingt-quatre heures.*

	QUANTITÉ TOTALE.	SUBSTANCES ORGANIQUES.	SELS ANHYDRES.	EAU.
	gr.	gr.	gr.	gr.
Eau de boisson	1500	0	0.2375	1500
Pain	750	577.4230	7.5770	165
Total des substances ingérées	2250	557.4230	7.9145	1665
Fèces (moyennes)	92.7777	56.8736	1.4577	34.4414
Urines (moyenne)	1074	25.2002	5.3792	1047.7998
Total des substances rejetées	1166.7777	82.0738	6.8369	1083.2442
Différences	1083.2223	495.1472	1.0776	581.7585

Suite au Tableau n° 10.

	ACIDE PHOSPHORIQUE.	ACIDE SULFURIQUE.	CHLORE.	CHAUX.	MAGNÉSIE.	POTASSE.	TOTAL des substances dosées.	DIFFÉRENCES donnant la soude, fer, etc.
	gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
Eau de boisson	0.0025	0.0300	0.0105	0.0825	0.0315	Traces	0.1570	0.1805
Pain	1.9875	0.7325	0.9735	0.6737	0.3721	0.3479	5.0072	2.4898
Total des substances ingérées	1.9900	0.7625	0.9840	0.7562	0.4036	0.3479	5.2442	2.6703
Fèces	0.1245	0.0632	0.0127	0.5121	0.2784	0.0752	1.0561	0.4016
Urines	1.6534	0.5082	0.8400	0.1252	0.1142	0.2716	3.6026	1.7766
Total des substances rejetées	1.7779	0.6514	0.8527	0.6373	0.3926	0.3468	4.6587	2.1782
Différences	0.2121	0.1111	0.1313	0.1189	0.0110	0.0011	0.5855	0.4921

Tableau n° 11, *indiquant les différentes moyennes relatives à la composition des urines.*

MOYENNE de la composition des urines pour les jours :	QUANTITÉ en centimètres cubes.	DENSITÉ.	ACIDITÉ exprimée en potasse anhydre.	URÉE.	ACIDE URIQUE.	SUBSTANCES solides anhydres.	SELS ANHYDRES.	SUBSTANCES organiques non dosées.
	gr.		gr.	gr.	gr.	gr.	gr.	gr
de régime mixte animal.	1432	1.017	0.4858	34.47	0.3722	54.9895	13.0201	7.1581
de régime uniforme sans viande..........	1074	1.012	0.1777	22.41	0.1632	30.5810	5.3714	2.6270
de repos............	1157	1.010	0.117	20.46	0.13[illegible]	29.0940	5.4397	3.066
d'activité cérébrale....	1320	1.010	0.117	23.88	0.136	32.6210	5.797	2.811
d'activité musculaire...	752	1.016	0.300	22.89	0.222	30.0193	5.111	2.011

Suite au **Tableau n° 11.**

MOYENNE de la composition des urines pour les jours :	ACIDE phosphorique anhydre.	ACIDE SULFURIQUE anhydre.	CHLORE.	CHAUX.	MAGNÉSIE.	POTASSE.	SUBSTANCES minérales non dosées, soude, fer, etc.
	gr.	gr.	gr.	gr.	gr.	gr.	gr.
de régime mixte animal.	1.9707	1.0540	4.6193	0.2544	0.1555	0.3700	4.6381
de régime uniforme sans viande..........	1.6534	0.5982	0.8400	0.1252	0.1142	0.2716	1.7766
de repos............	1.5080	0.4646	1.2239	0.1264	0.1099	0.2531	1.7535
d'activité cérébrale....	1.9777	0.9124	0.4169	0.1212	0.1153	0.2674	1.9534
d'activité musculaire..	1.4779	0.3878	0.8792	0.1251	0.1173	0.2943	1.62[illegible]7

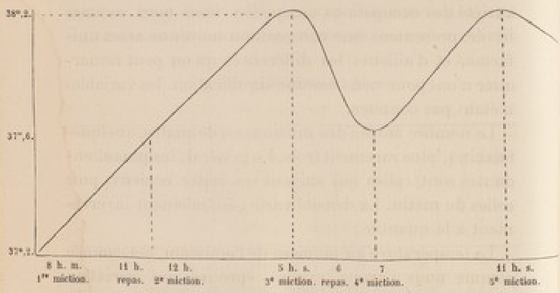
Tous les résultats analytiques principaux étant consignés dans les tableaux précédents, il nous suffira de donner aux chiffres leur signification vraie, pour en tirer des déductions rigoureuses. Nous ne pouvons songer à présenter complétement les conséquences nombreuses qui surgissent; les expériences physiologiques ayant pour sujet les phénomènes d'assimilation et de désassimilation sont d'une complexité telle, qu'il est difficile d'en rassembler à la fois toutes les données. Ce qui nous importe, c'est de ne pas perdre de vue le sujet de cette thèse, tout en recueillant, chemin faisant, les résultats accessoires principaux.

Pendant les douze jours d'alimentation mixte, mais principalement animale, sans détermination de la quantité d'aliments solides et liquides, les urines, malgré la variété des occupations auxquelles nous nous sommes livrés, présentent une composition moyenne assez uniforme, et d'ailleurs les différences qu'on peut remarquer n'ont pour nous aucune signification, les variables n'étant pas connues.

Le nombre moyen des mictions est de quatre, quelquefois cinq, plus rarement trois. En général, les plus abondantes sont celles qui suivent un repas copieux, puis celles du matin. La densité varie généralement inversement à la quantité.

La température au moment de l'emission, determinée comme nous l'avons exposé, éprouve une variation périodique de 1° par vingt-quatre heures. A l'urine du matin correspond le minimum égal à 37°,2, à celle qui est rendue après un temps aussi éloigné que possible des principaux repas, correspond toujours le maximum de température, qui atteint, surtout s'il y a exercice musculaire, 38°,2, mais qui, dans tous les cas, est de 38°; aux urines, émises peu après les repas et pendant la digestion, correspond une température moyenne qui d'ordinaire est égale à 37°,6 ou 37°,5.

En parcourant le tableau n° 1, relatif aux neuf jours de régime uniforme, on voit ces mêmes variations se manifester avec une régularité remarquable, qu'on pourrait représenter par une courbe qui, en supposant cinq mictions journalières et deux repas principaux, aurait un minimum, deux maximums et deux points moyens alternant avec ces derniers. Si on admet, ce qui nous paraît hors de doute, que l'urine (au moment de sa formation dans le rein) participe à ces variations, on aura une courbe continue que nous figurons ici :



Cette courbe n'a pour but que de bien faire saisir la loi et ne saurait d'une manière absolue être vraie. Nous n'entreprendrons pas de montrer que les changements de température sont en accord parfait avec l'exercice

simultané de telle ou telle fonction, et qu'on pouvait par conséquent les supposer; ce serait nous engager dans une question physiologique déjà traitée, et pour laquelle, d'ailleurs, nous n'avons ni des données assez nombreuses, ni la compétence.

Le changement dans l'alimention n'a eu aucune influence sur la température. Les différents traités donnent pour elle le chiffre de 35° à 37° centigrades ; nous avons déjà expliqué pourquoi ce chiffre devait être trop faible.

En parcourant les tableaux nos 2, 3, 4 et 5, on remarquera que la proportion d'urée que nous trouvons est supérieure à celle qui est généralement admise. Nous trouvons en effet une moyenne par kilog. de 0g,65, chiffre assez différent de 0g,42, admis pour les adultes. La quantité donnée par les auteurs allemands et anglais est au contraire un peu plus élevée. De nombreuses causes peuvent être invoquées pour expliquer cette dernière divergence et, entre toutes, l'alimentation étant mise à part, le climat, par ses nombreux éléments, est la principale. D'après une observation que nous avons recueillie au cours de M. Bouchardat, un habitant, vivant dans un pays chaud, sous une latitude très-faible, présente au bout de quelques mois de séjour à Paris une augmentation notable d'urée.

Nous ne pouvons insister sur tous les chiffres correspondant aux urines des douze jours de régime mixte, et nous arrivons aux tableaux nos 6 et 7, les plus importants pour notre sujet.

La première observation à faire est le changement immédiat, et en quelque sorte instantané, qui se produit dans la proportion des diverses substances lorsque le régime est modifié ; le plus frappant est celui qui est relatif à l'urée. Dès que l'alimentation, au lieu d'être mixte, mais principalement animale, devient presque végétale tout en étant fortement azotée, le chiffre de l'urée baisse immédiatement d'environ 10 grammes, c'est-à-dire de près du tiers pour les vingt-quatre heures. Ce fait, dont nous tirerons plus loin une conséquence importante, est d'accord avec ceux qui résultent d'expériences anciennes de Lehmann et de Frerichs, confirmées depuis par bon nombre de physiologistes.

Les variations principales observées pendant les neuf jours portent sur la quantité d'urine, la densité, l'acidité, la proportion d'urée, d'acide urique, d'acide phosphorique, d'acide sulfurique et de chlore.

Le minimum de la quantité d'urine correspond aux jours de travail musculaire : l'activité plus grande de l'exhalation pulmonaire, la production inévitable d'une plus grande quantité de sueur, sont les deux raisons principales de cette diminution. Je signalerai à ce propos un fait que chacun peut facilement vérifier : c'est celui de la résorption de l'eau dans la vessie, dans les circonstances suivantes : supposez que le besoin d'uriner se fasse sentir assez énergique, qu'on y résiste quelques instants et qu'on se livre immédiatement à un exercice musculaire un peu violent, le besoin disparaîtra, et cela pendant plusieurs heures, en admettant qu'on n'ingère pas de boisson. Ce fait, que d'autres expérimentateurs ont dû nécessairement observer, nous l'avons vu se produire à plusieurs reprises, et il ne peut s'expliquer que par l'absorption de l'eau s'effectuant par endosmose au travers des parois et rentrant dans la circulation par les veines. Cette réabsorption de l'eau

s'accompagne-t-elle de celle d'autres principes en dissolution ? Nous ne nous prononcerons pas, tout en penchant pour l'affirmative, l'endosmose de l'eau n'ayant jamais lieu sans la dialyse des sels et substances cristallisés qu'elle renferme.

La quantité la plus considérable d'urine éliminée dans les vingt-quatre heures correspond aux jours d'activité cérébrale. Depuis longtemps on a signalé l'abondance des urines dites nerveuses ; nous nous bornons à faire remarquer cette coïncidence.

Nous n'avons pas noté dans nos tableaux la coloration des urines ; la variation de cette donnée a son importance comme caractère physique facilement appréciable. A l'état physiologique, plus les urines sont abondantes, moins elles sont colorées, et réciproquement.

La densité varie avec la quantité, diminuant quand celle-ci augmente ; les urines du matin sont cependant les plus denses.

L'urine des vingt-quatre heures a toujours a été acide ; l'acidité variable est toujours proportionnelle à la quantité d'acide urique. Contrairement aux résultats des travaux de MM. Lecanu et Lehmann, on voit ce dernier corps éprouver une diminution notable sous l'influence du régime ; il suffit de parcourir le tableau n° 11 pour apprécier la différence considérable dans la proportion contenue comme moyenne dans les urines des douze jours d'alimentation mixte, et celle renfermée dans les urines des neuf jours suivants.

Si on calcule la quantité de potasse nécessaire à la neutralisation des différents poids d'acide urique obtenus, on trouve qu'elle est inférieure à celle que donne la détermination directe de l'acidité ; les différences entre les deux chiffres sont d'autant plus considérables que les urines renferment plus d'acide urique. L'explication de ce fait découle des observations suivantes : l'acide urique n'est pas le seul acide libre de l'urine; l'acide hippurique et l'acide carbonique concourent partiellement à l'acidité; la quantité de ce dernier est proportionnelle au travail musculaire, comme l'a montré M. Morin, et sa variation est dès lors de même sens que celle de l'acide urique.

Il est généralement admis que l'acidité de l'urine est due aux phosphates alcalins transformés en phosphates acides par l'acide urique qui, par l'effet de cette réaction, se trouverait exister à l'état d'urate de soude principalement. Nous ne saurions nous ranger à cette opinion.

D'abord, l'acide urique peut-il enlever la soude au phosphate de soude ordinaire, ayant pour formule : $2NaO,HO,PO^5,HO$. Il ne saurait être question du phosphate tribasique, $3NaO,PO^5,nHO$, dont quelques auteurs admettent la présence dans l'urine et le sang; ce sel est si facilement décomposable par l'acide carbonique, toujours à l'état libre dans ces deux liquides, que, jusqu'à preuve du contraire, nous serons d'un avis opposé. Pour résoudre la question que nous venons de poser, nous avons fait agir de l'acide urique sur du phosphate de soude dissous dans l'eau en proportion variable, savoir : 1 gr. pour 100, 5 gr. pour 100, 10 gr. pour 100. Nous avons préalablement déterminé le coefficient de solubilité de l'acide urique dans l'eau à 15°, et comme moyenne de quatre évaluations concordantes, nous avons trouvé égal à $\frac{1}{...}$; le chlorure de sodium ne change pas la solubilité; l'acide chlorhydrique la diminue; le phosphate de soude, dans les proportions ci-dessus

indiquées, l'augmente d'environ le double. Ce dernier corps, bien purifié par plusieurs cristallisations, donne des dissolutions très-légèrement alcalines au papier de tournesol. Lorsque, pendant deux à trois heures, on fait agir vers 60°, en agitant souvent, de l'acide urique en excès sur le phosphate de soude dissout, on remarque, après avoir filtré la liqueur refroidie, les réactions suivantes : la solution est devenue acide, et, au bout de vingt-quatre heures, un dépôt cristallin peu abondant se produit; examinés au microscope, les cristaux affectent la forme de prismes droits à base carrée, bien définis, lorsqu'on a opéré sur la solution à 1 p. 100 de phosphate de soude; avec les dissolutions à 5 et à 10 p. 100, les cristaux se groupent et apparaissent sous la forme décrite pour l'urate de soude.

Après quarante-huit heures nous avons séparé les cristaux par filtration, et nous avons fait évaporer doucement; si on pousse l'évaporation jusqu'à siccite et qu'on sépare de temps en temps les cristaux formés, et analogues aux premiers, on reconnaît que le résidu repris par de l'eau donne une solution à peine acide; si le phosphate de soude avait été transformé en phosphate acide, si de l'urate de soude s'était par suite formé, on devrait arriver à un résultat tout différent. Quels sont donc ces composés cristallins que l'on a séparés? Lavons-les à plusieurs reprises, desséchons-les; ils sont peu solubles dans l'eau froide, calcinons-les sur une capsule de platine. Un essai préalable nous a appris qu'ils renferment de l'acide urique; cette dernière opération nous montre qu'ils laissent un résidu fixe, alcalin, mais contenant du phosphate de soude. Le corps était cristallisé, parfaitement défini; nous sommes par suite obligé d'admettre une combinaison d'acide urique et de phosphate de soude; ce composé peu soluble donne d'ailleurs une solution acide, et sa solubilité est plus grande dans l'urine que dans l'eau distillée. Nous avons cherché à en déterminer la composition; mais nous avons été arrêté par la difficulté qu'il y a à séparer les composés, au moins au nombre de deux, qui paraissent se former dans les conditions où nous nous sommes placé. Malgré quelques indications que nous pourrions donner, nous préférons réserver entièrement cette question.

Nous avons entrepris ces recherches, sur lesquelles on nous excusera d'insister, à propos d'une question étudiée et débattue par plusieurs chimistes et entre autres par Berzélius, Vigla, Thénard, Becquerel, Prout, Quévenne, Donné, parce que nous avions reconnu que les dépôts d'urates qui se forment peu après l'émission des urines, dans celles qui en sont chargées, donnent des cendres, qui renferment beaucoup de phosphate de soude. Si, dans une urine normale, on retarde la décomposition en y ajoutant quelques goutes d'essence de pétrole, et laissant après agitation s'étaler à la surface une mince couche, l'acide urique se sépare en cristallisant lorsque sa quantité est supérieure à celle que la solubilité seule permet d'être dissoute. Plusieurs raisons concourent à ce que sa précipitation soit facilitée par l'acide chlorhydrique mélangé à l'urine : 1° cet acide diminue la solubilité propre de l'acide urique ; 2° il décompose les combinaisons de ce corps avec les phosphates alcalins ; 3° il retarde la fermentation ammoniacale de l'urine, se combine à l'ammoniaque qui se forme et qui, à l'état libre, agissant en présence de la chaux et

de la magnésie sur les phosphates alcalins, les transformerait en phosphates insolubles, et ferait passer l'acide urique à l'état d'urate de soude, et puis d'urate d'ammoniaque en partie, ce qui explique pourquoi ces différents corps se rencontrent dans les dépôts d'urines altérées. Pour toutes ces raisons, nous concluons que l'acide urique existe dans les urines, partie à l'état libre, partie copulé ou combiné aux phosphates alcalins.

En parcourant le tableau n° 6, on voit que l'état de repos ou d'activité cérébrale ne modifie pas la proportion d'acide urique, qui se trouve au contraire augmentée les jours d'activité musculaire. Ce dernier résultat semblerait en opposition avec ce fait bien avéré que chez un homme d'un genre de vie sédentaire, à alimentation principalement animale, dont les urines sont fortement chargées d'acide urique, l'exercice musculaire seul en plein air en fait notablement diminuer la proportion. Mais cette diminution n'est qu'apparente; car il est hors de doute d'après les expériences directes de MM. Wœhler et Frerichs que cet acide éprouve des oxydations dans le torrent circulatoire, oxydations dont les deux principaux produits sont l'urée et l'acide carbonique. On trouvera à ce sujet de belles considérations scientifiques dans l'*Annuaire thérapeutique* de M. Bouchardat, 1867, article *Gravelles*. Nul doute que tout l'acide urique produit par le travail musculaire n'apparaît pas dans les urines. Il faudrait aussi dans beaucoup de cas faire intervenir l'action si remarquable de plusieurs acides organiques, et, en particulier, ceux de la série aromatique. Dans l'état physiologique, nous concluons que l'exercice musculaire fait apparaître dans les urines une plus grande quantité d'acide urique.

La variation de l'urée en rapport avec les divers états de l'organisme est des plus remarquables et elle ne peut échapper en parcourant le tabl. n° 6. Nous avons déjà fait ressortir l'influence immédiate du régime, et nous voyons que vingt-quatre heures après avoir quitté l'alimentation mixte animale, comme vingt-quatre heures après l'avoir reprise, le chiffre de cette substance éprouve une variation considérable. On ne peut certainement manquer de se poser la question suivante : d'où vient cette différence et toute l'urée provient-elle dans ces conditions de la désassimilation des tissus, en rapport avec leur nutrition et leur fonction? Nous répondons négativement, et nous admettons la formation directe de l'urée dans le sang, surtout lorsque les substances albuminoïdes y arrivent en excès. Cette urée nous l'appelons *urée de calorification*. Serait-il possible d'expliquer cette diminution si subite de l'urée dans l'espace de vingt-quatre heures sans pouvoir noter de changement dans la température, la respiration, la circulation, sans l'apparition d'aucun phénomène spécial? Pourrait-on dire que, dans un si court espace de temps, la nutrition des tissus puisse être si variable, ou bien que les corps azotés sont assimilés et subissent les métamorphoses régressives descendantes. Nous savons qu'il déplaît souverainement aux anatomistes d'admettre que des combustions chimiques ou réactions de ce genre puissent s'opérer dans le sang. Nous ne considérons pas certainement ce liquide comme une simple dissolution de principes immédiats, et on doit l'envisager comme un tissu; l'anatomie et la chimie se doivent pour l'étudier un concours réciproque. Mais comment, dans un milieu si complexe, à une température si favorable, en mouve-

ment continuel, toujours en contact avec l'oxygène de l'air, recevant sans cesse, après la préparation spéciale que leur fait subir l'appareil digestif, des matériaux nouveaux, comment dans de pareilles conditions n'y aurait-il pas de combustions directes dans le sang? Et puis ce tissu, considéré en lui-même, ne doit-il pas éprouver les métamorphoses de nutrition? On connaît les expériences de Lehmann et de Frerichs sur la variation de l'urée sous l'influence de l'alimentation ; on sait que, dans le jeûne absolu ou accompagné seulement de boissons n'agissant pas comme aliment, l'urée continue à apparaître dans les urines ; on sait aussi que dans ces conditions, sa proportion diminue rapidement si on fait prendre à l'animal en expérience des aliments non azotés ; la température, qui s'était abaissée, conserve ensuite sa valeur normale. Ces derniers faits prouvent que la désassimilation des tissus peut être provoquée dans le but d'entretenir la chaleur animale, et au nombre de ces tissus le sang doit surtout ne pas être oublié. Ainsi nous concluons que, dans l'état normal et principalement lorsque l'alimentation est trop riche en substances animales azotées, l'urée se forme directement dans le sang, et pour la distinguer de celle qui provient des autres tissus, nous l'appelons *urée de calorification*.

En suivant attentivement la variation de l'urée pendant les neuf jours, on voit le minimum correspondre aux jours de repos; les maxima correspondent aux jours d'activité musculaire et d'activité cérébrale. Ainsi le corps est-il en repos relatif, l'activité cérébrale proprement dite est-elle seule surexcitée? augmentation d'urée dans les urines. Les conditions extérieures sont cependant les mêmes, comme on peut s'en convaincre par le tableau numéro 1. La différence maximum atteint près de 5 grammes, et nul doute qu'elle serait plus considérable, s'il était possible d'arriver à l'état de repos parfait. Ce qui doit encore frapper, c'est la répétition parfaitement concordante des résultats. Les expériences sont disposées de manière qu'un jour d'activité cérébrale succède, tantôt à un jour de repos, tantôt à un jour de travail musculaire. Comme nous l'avons déjà fait observer, il est probable d'après nos résultats que, dans l'espace de vingt-quatre heures, l'économie se débarrasse par les urines de la majeure partie des matériaux fixes de combustion formés; nous ne pensons pas, toutefois, qu'on puisse, d'une manière rigoureuse, fixer ce laps de temps; et après un exercice violent, quel qu'il soit, il est possible que l'urine recueillie dans le second jour en porte encore la trace. Nous dépasserions notre pensée en disant qu'on peut fixer des limites tranchées à des phénomènes aussi complexes; mais, d'un autre côté, l'expérience est là avec ses résulats, et elle est souveraine. La certitude que la production de la pensée s'accompagne d'une dépense organique, se traduisant principalement par une augmentation de l'urée, servira, nous n'en doutons pas, à expliquer bien des faits, et nous n'entreprendrons pas ici d'essayer même un aperçu.

L'urée, rejetée par les urines, chez une personne qui ingère une quantité suffisante d'aliments, au nombre desquels figurent ceux d'origine animale, a deux sources bien distinctes et d'importance bien inégale. La plus grande partie provient de la désassimilation des éléments anatomiques formant les tissus; elle

est d'autant plus grande, que leur activité, et par suite, leur nutrition et leur rénovation sont plus rapides. C'est l'urée en quelque sorte fondamentale, nécessaire pour qu'il y ait vie; quand sa formation descend au-dessous d'une certaine limite, tout mouvement s'éteint, et la mort en est la conséquence. L'importance de l'urée de calorification est beaucoup moindre et dans le cas d'une alimentation peu azotée et relativement riche en matières féculentes et matières grasses, sa proportion doit être bien faible. Cette observation est surtout vraie pour l'être qui, comme l'enfant, est en voie d'accroissement. L'urée de désassimilation provient principalement des appareils organiques dont les fonctions sont le plus en activité, et on peut en rapporter en grande partie la production :

1° A l'accomplissement de la respiration, de la digestion et de la circulation, considérées en elles-mêmes;

2° A l'accomplissement de l'activité musculaire volontaire;

3° A l'accomplissement de l'activité cérébrale.

Mais, nous le répétons, si cette distinction ne peut être faite d'une manière absolue, à cause de la connexité intime des différents systèmes n'agissant jamais isolément, elle est, cependant une conséquence de l'expérimentation.

Dans quelle proportion ferions-nous ces quatre parts principales du chiffre moyen de l'urée dans les cas de l'alimentation mixte animale? Nous tomberions trop dans le champ de l'hypothèse en l'essayant, et il n'y aurait aucune utilité à s'y lancer. Cependant nous croyons possible d'arriver par l'expérience à des approximations.

Nous n'insisterons pas sur la variation quelquefois considérable des substances organiques non dosées directement, et qui comprennent principalement la créatine, la créatinine, l'acide hippurique; nous ne pourrions tirer de cette étude aucune déduction rigoureuse, espérons qu'on arrivera à les doser d'une manière sûre et rapide et elles fourniront alors des données très-intéressantes.

Le changement de régime fait éprouver aux substances minérales en totalité une variation considérable et rendue bien manifeste par l'examen comparé des tableaux n^os^ 2 et 7. Il n'y a dans ce fait, que nous nous bornons à constater, rien qui doive surprendre; la différence porte en grande partie sur le chlorure de sodium, et le vin, relativement riche en sels, formait la boisson principale pour les douze jours d'alimentation mixte. Nous avons déjà donné les raisons pour lesquelles nous n'avons pas groupé les acides et les bases; ce groupement n'est pas soumis à des règles fixes, et la chimie est encore impuissante à doser directement les sels tels qu'ils existent dans l'urine.

Les variations observées les plus importantes, les seules que nous puissions déduire de nos analyses, sont relatives aux acides phosphorique et sulfurique, au chlore. On peut les résumer en disant : à l'activité cérébrale est liée l'apparition dans les urines d'une proportion relativement plus considérable des deux premiers corps ; à l'activité musculaire, celle du chlore. Quelques observations déduites de l'examen des urines de malades atteints de délire aigu et de delirium tremens, publiées par M. Bence Jones, concordent en partie avec nos résultats. Mais nous ferons remarquer avec M. Beale qu'on ne peut les considérer comme complètes, ayant été faites sur des échantillons partiels ; mais ces études ont une très-grande importance, et elles viendront, nous en avons l'assurance, compléter et fortifier nos conclusions.

On sera peut-être étonné de voir qu'à l'état de repos le chlorure de sodium est rejeté en plus grande quantité, que lorsque le corps est en activité musculaire. Nous ferons remarquer que, depuis les recherches de M. Favre, on sait que ce sel est de beaucoup le plus abondant de ceux que renferme la sueur, et comme cette sécrétion est activée nécessairement par l'exercice musculaire, il faudrait, pour conclure, pouvoir tenir compte de la quantité éliminée par cette voie. Les phosphates et les sulfates sont au contraire en proportion très-faible dans ce liquide, et les différences considérables qui correspondent à l'activité cérébrale et à l'activité musculaire ne peuvent lui être rapportées. On ne peut pas davantage les imputer aux matières fécales.

Si on consulte les tableaux n^os^ 1 et 10, on remarque que la quantité des fèces n'est pas constante, qu'il y en a absence durant trois jours (14, 17, 21); mais elles correspondent à un jour de travail musculaire et à deux jours de repos. Outre la constance des caractères physiques de ces excrétions, la composition moyenne que nous donnons a été trouvée pour les sels très-uniforme. On remarque leur faible proportion, comparée à celle qui est contenue dans l'urine; la chaux et la magnésie des aliments paraissent s'éliminer en grande partie par cette voie sans être absorbées ; le chlorure de sodium y est au contraire très-peu abondant.

En faisant, dans le tableau n° 10, une espèce de balance des substances introduites par le tube digestif et de celles déversées à l'extérieur, nous n'avons pour but que de tirer de nos expériences un résultat accessoire pour notre sujet, mais qui pourra avoir son utilité pour d'autres travaux. L'équivalence que nous établissons n'est pas d'ailleurs complète, et nous aurions dû, pour qu'elle fût importante à consulter, doser l'azote du pain et celui des matières fécales. Les différences inscrites représentent la perte opérée par l'exhalation pulmonaire, la sueur, les productions et desquamations épithéliales. On voit que, dans les circonstances où nous étions placés, la déperdition moyenne de l'eau en dehors de celle effectuée par les urines et les fèces est d'environ 581 grammes par vingt-quatre heures.

Les variations dans la proportion des trois éléments minéraux cités doit-elle être attribuée comme celle de l'urée et de l'acide urique au travail organique? Nous ne les croyons pas explicables sans cela; la quantité introduite journellement dans l'économie est uniforme; toutes les autres conditions sont sensiblement identiques; d'où viendrait qu'un jour j'élimine par les urines environ 2 grammes d'acide phosphorique, 1 gramme d'acide sulfurique, et qu'un autre jour ces nombres soient réduits de moitié?

Nous ne connaissons pas encore, malgré de fort belles recherches, la constitution des matières albuminoïdes. On sait qu'outre leurs quatre éléments fondamentaux, elles renferment du soufre et du phosphore, sans savoir sous quel état. En préparant par les divers procédés indiqués dans les ouvrages, et en particulier par celui de M. Wurtz, ce qu'on est convenu de regarder comme de l'albumine pure, on peut se convaincre que cette

substance, après sa destruction, laisse un résidu de sulfates et phosphates principalement. Ce serait nous engager dans un sujet trop distinct du nôtre que d'entrer dans des considérations sur les différences reconnues entre les tissus musculaires et nerveux proprement dits, et de montrer, en nous appuyant sur elles, comment nos résultats y trouveraient leur explication. Dans toute question, surtout dans celles qui en soulèvent de fort nombreuses, il faut savoir se borner.

Nous croyons pouvoir arrêter là cette étude et nous dispenser de faire ressortir de nos expériences quelques autres résultats peu importants. Nous espérons pouvoir en recommencer de nouvelles, qui seront certainement plus complètes lorsque celles-ci auront subi l'examen et les critiques de nos maîtres.

CONCLUSIONS.

Nous résumerons en peu de mots les principaux faits dont ce travail a été l'objet, et nous formulerons les conclusions principales auxquelles il nous a conduit.

Nous avons revu et donné une précision plus grande aux dosages :

1° De l'urée par l'azotate de bioxyde de mercure,

2° De l'acide urique,

3° Des phosphates, par l'azotate d'urane.

Nous avons imaginé un appareil simple pouvant servir à déterminer plus exactement la température de l'urine au moment de l'émission : nous avons été conduit à formuler une loi sur la variation de cette donnée, dans les vingt-quatre heures.

La durée des expériences nous a permis de montrer l'influence immédiate et considérable de l'alimentation sur la composition des urines.

L'acide urique existe dans les urines, partie à l'état de liberté, partie en combinaison avec les phosphates alcalins.

Les conclusions principales peuvent être formulées ainsi :

L'exercice de l'activité cérébrale proprement dite ou de la pensée s'accompagne de la production plus abondante et de l'apparition simultanée dans les urines d'urée, de phosphates et de sulfates alcalins.

L'exercice de l'activité musculaire s'accompagne de la production plus abondante et de l'apparition simultanée dans les urines, d'urée, d'acide urique et de chlorure de soduim.

Etant données séparément les urines d'un homme qui, pendant trois jours, aura suivi un alimentation uniforme et se sera trouvé dans des conditions extérieures sensiblement identiques, il sera possible, par l'analyse seule, de savoir à chacun desquels correspond, d'une manière relative, l'état ou de repos ou d'activité cérébrale, ou d'activité musculaire.

Nous serons heureux si nous avons pu apporter un grain de sable à l'édification de ce temple immense et grandiose qu'on appelle la science, d'où s'élève chaque jour plus puissante la voix qui combat l'ignorance, la misère et la maladie; c'est elle qu'a toujours fait entendre cette Faculté à laquelle je me ferai gloire d'appartenir.

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Lawes and Gilbert.

ON THE
SOURCES OF THE FAT OF THE ANIMAL BODY.

IN 1842, Baron Liebig* maintained that the fat of Herbivora must be derived in great part from the carbo-hydrates of their food, but considered that it might also be produced from nitrogenous compounds. MM. Dumas and Boussingault † at first called in question this view; but subsequently the experiments of Dumas and Milne-Edwards ‡ with bees, of Persoz § with geese, of Boussingault‖ with pigs and ducks, and of ourselves with pigs¶, were held to be quite confirmatory of Liebig's view, at any rate so far as the formation of fat in the animal body from carbo-hydrates was concerned.

In 1864, however, at the Bath Meeting of the British Association for the Advancement of Science, Dr. Hayden, of Dublin, read a paper before the Physiological Section, in which, basing his conclusions upon certain physiological considerations of a purely qualitative kind, he argued that fat was not producible in the body from sugar and allied substances, but that both eventually served for the production of carbonic acid and water; and sugar being the most readily oxidized, so saved the combustion, and favoured the storing of fat.

Again, in August 1865, at a Meeting of the Congress of Agricultural Chemists, held in Munich (at which one of the authors was present), Professor Voit**, from the results of experiments with dogs fed on flesh, maintained that fat must have been produced from the nitrogenous constituents of the food, and that these were probably the chief if not the only source of the fat, even of Herbivora. In favour of the probability of this view, Professor Voit refers to the formation of adipocere from nitrogenous substance; but he mainly relies upon the fact that, in experiments by Pettenkofer and himself in which large quantities of flesh were given to a dog, the whole of the nitrogen reappeared in the form of urea and in the fæces, whilst only a portion of the carbon was recovered in the urine, fæces, and the products of respiration and perspiration, from which it was concluded that some had been retained in the body, and had con-

* Organic Chemistry of Physiology and Pathology, p. 81 *et seq.*

† Balance of Organic Nature, 1844, p. 116 *et seq.*

‡ *Comptes Rendus de l'Académie des Sciences*, vol. xvii. p. 531.

§ *Ann. de Chim. et de Phys.* vol. xiv. p. 408 *et seq.*

‖ *Ann. de Chim. et de Phys.* vol. xiv. p. 419 *et seq.*; xviii. p. 444 *et seq.*

¶ "On the Composition of Foods in relation to Respiration and the Feeding of Animals," Report of the British Association for the Advancement of Science for 1852.

** *Versuchs-Stationen Organ.* vol. viii. No. 1, 1866, p. 23.

tributed to the formation of fat. That animals nevertheless do not become fat when fed upon very highly nitrogenous food, Voit considers sufficiently explained by the greater number of blood-corpuscles, the result of such diet, and the greatly increased activity of oxidation of nitrogenous substance under such conditions; whilst, on the other hand, the accumulation of fat when fat and carbo-hydrates are supplemented to a liberal nitrogenous diet he considers to be connected with the much less active oxidation of the nitrogenous substance and fatty matter that then takes place, rather than attributable to the direct production of fat from the carbo-hydrates.

In the discussion which followed the reading of Professor Voit's paper, Baron Liebig forcibly called in question Professor Voit's 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.

Since the Munich Meeting, Hermann von Liebig, son of Baron Liebig, has written a paper on the subject*, in which, admitting the probability that fat may be formed from nitrogenous substance, he nevertheless concludes that this is neither its only, nor even its chief source, in the ordinary feeding of Herbivora.

After referring to the leanness of the South Russian shepherds, who consume very large quantities of dried meat, and to the rotundity of the peasantry, especially the women, in districts where bread and fruits constitute the chief articles of food, H. von Liebig proceeds to illustrate the formation of fat from non-nitrogenous constituents of food by our domestic Herbivora, by the calculation of the results of numerous experiments made with cows in 1857, by Knop, Arendt, and Behr, in which the details as to food, live-weight, and quantity and composition of milk, were accurately recorded. According to the mode of calculation adopted, it appeared that, after deducting from the amount of nitrogenous substance taken in the food that estimated to be required by the system for other purposes, there was generally little or none remaining for the production of fat. In his calculations, however, H. von Liebig, besides taking into account the probable amount of nitrogenous substance stored up in increase with gain of weight, or set at liberty when there was loss of weight, as the case might be, deducted from the amount of nitrogenous substance given in the food, not only that required for the production of the caseine of the milk, but also

* *Versuchs-Stationen Organ.* vol. viii. No. 3, 1866.

the whole of that estimated to be required for the mere sustenance of the animal (according to its weight) independently of gain or loss, or milk produced.

It is obvious, however, as pointed out by Voit, and as afterwards admitted by H. von Liebig, that if nitrogenous substance may break up into urea and fat (with other products), the amount estimated to be required for the mere sustenance of the body should not be considered inadmissible for the formation of fat as one of its products, and therefore should not be deducted (with that appropriated for the production of increase and of the caseine of the milk) from the amount supplied in the food in estimating whether or not it provided sufficient for the formation of the fat known or calculated to be produced.

H. von Liebig states that he selected experiments with cows as the basis of his illustrations, considering that, when in a normal state, the change in the solid substance of the body of the animal was comparatively small, if not indeed immaterial, and that the fixed products of the food, beyond what might be required for the mere maintenance of the body, were accumulated and easily estimated in the milk collected; whilst he considered, on the other hand, that the point in question could not be settled by reference to results relating to fattening animals, without the aid of an apparatus for the determination of the products of respiration and perspiration. We believe, however, that with a proper selection of fattening animals it may be satisfactorily illustrated without the aid of any such apparatus; and it is the object of this paper briefly to discuss the question of the sources of the fat of the animal body by reference to the results of experiments with such animals.

As already intimated, the objections of Dr. Hayden to the supposition that fat is formed from the carbo-hydrates of the food, were based upon physiological considerations of a qualitative, but not at all of a quantitative kind. Voit's argument was, on the other hand, founded upon strictly quantitative results, obtained, however, under conditions as to choice of animal and of food, in which the formation of fat, if it took place at all, must of necessity be attributed to the nitrogenous constituents consumed. H. von Liebig also relied upon quantitative results as the basis of his illustrations; but those selected, when properly considered, afforded, to say the most, only negative evidence on the point.

The question arises—What description of animal is likely to yield the most direct and conclusive evidence as to the source of the fat stored up in its body? Obviously the one which is fed more especially with a view to the production of fat, which consumes in its most appropriate fattening food a relatively large proportion of carbo-hydrates, and which yields a large proportion of fat, both in relation to the weight of animal within a given time, and to the amount of food consumed. The following Table (I.), which summarizes the results of a great many direct experiments of our own *, will show that of the ox, the sheep, and the pig—the most important of the animals fed and slaughtered as human food—the last pre-eminently supplies the required conditions.

TABLE I.—Comparative fattening-qualities of different animals.

	Oxen.	Sheep.	Pigs.
Relation of parts in 100 live-weight.			
Average of	16	249	59
Stomach and contents	11·6	7·5	1·3
Intestines and contents	2·7	3·6	6·2
	14·3	11·1	7·5
Heart, aorta, lungs, windpipe, liver, gall-bladder and contents, pancreas, spleen, and blood.	7·0	7·3	6·6
Per 100 live-weight.			
Dry substance consumed in food per week	12·5	16·0	27·0
Increase yielded per week	1·13	1·76	6·43
Per 100 dry substance of food.			
Total dry substance in increase	6·2	8·0	17·6
Fat in increase	5·2	7·0	15·7
Total dry substance in urine and fæces	36·5	31·9	16·7
Average fat per cent.			
In lean condition	16·0	18·0	22·0
In fat condition	30·0	33·0	44·0
In increase whilst fattening	60·0	65·0	70·0

Looking first to the comparative structure of the animals, so far as it may be considered characteristic or indicative of the description of the food, it is seen that, of stomach and contents, the ruminant ox has a much larger proportion than the ruminant sheep, and the ruminant sheep in its turn much more than the non-ruminating pig. Consistently with these facts, we find that the ox consumes in its food a much larger proportion of

* For the data upon which most of the average results given in the Table are founded, see "Experimental Inquiry into the Composition of some of the Animals fed and slaughtered as Human Food," Phil. Trans. Part II. 1859. In the estimates given "per 100 live-weight" and "per 100 dry substance of food," it is assumed that the oxen and sheep are liberally fed on oil-cake, clover-chaff, and roots, and the pigs on barley-meal alone; with different foods the results will, of course, be different.

only slowly digestible, or indigestible, cellulose than the sheep, and the sheep again very much more than the pig. The usual food of oxen and sheep, consisting as it does in large proportion of unripened or imperfectly ripened vegetable matter, is, in fact, essentially crude, containing not only a considerable amount of defectively elaborated and probably unassimilable nitrogenous substance, but also a large proportion of comparatively indigestible non-nitrogenous matter. Accordingly complexity and great capacity of stomach, and slow progress of the food through the organ, are characteristics of the structure and digestive process of the animals.

Of intestines and contents, on the other hand, the ox has a less proportion than the sheep, and the sheep considerably less than the pig.

In fact, the relatively very small proportion of stomach and contents, and relatively very large proportion of intestines and contents in the pig are very striking. But when we consider that his most appropriate fattening food consists of ripened seeds and highly starchy roots, containing little indigestible woody fibre, and their non-nitrogenous constituents almost wholly in the form of starch, the primary change of which is known to take place almost throughout the length of the intestinal canal, the reason of the relatively small proportion of stomach, and large proportion of intestines, seems to be at once apparent.

Passing from a consideration of the receptacles and, so to speak, first laboratories of the food, we will only remark, in reference to the remaining results given in the upper portion of the Table, that, of what may be called the further elaborating organs of the body, and their fluids—the heart, liver, lungs, blood, &c. —the proportion, taken in the aggregate, is strikingly similar in the three descriptions of animal.

The second division of the Table shows that, notwithstanding its much larger proportion of stomach and contents, the ox consumes, for a given live-weight within a given time, only about three-fourths as much dry substance of food as the sheep, and less than half as much as the pig with its very small proportion of stomach and contents. The ox gives, too, in proportion to a given live-weight within a given time, much less increase than the sheep, and only from one-fifth to one-sixth as much as the pig.

Reckoned in proportion to a given amount of dry substance of food consumed, the ox gives less both of total dry substance in increase, and of fat in increase, than the sheep, and only about one-third as much of either as the pig, whilst the ox voids of dry substance in fæces and urine the largest proportion, the sheep somewhat less, and the pig little more than half as much as the sheep, and less than half as much as the ox.

Lastly, the proportion of fat, whether reckoned in relation to the total weight of the body, or to the weight of the increase whilst fattening, is greater in the sheep than in the ox, and greater still in the pig.

Whilst referring to the connexion between the weight and capacity of the stomach and the character of the food, it will not be without interest to call attention to the gradation in the proportion from the ox to the sheep, from the sheep to the pig, and from the pig to man. Below is given the approximate average proportion of stomach, by weight, in 100 live-weight of each.

Oxen.	Sheep.	Pigs.	Man.
3·19	2·44	0·88	0·38

Without assuming that relative weight represents with numerical exactitude relative capacity or size, we nevertheless cannot doubt that these figures have a very obvious significance. Thus, the ox consumes the largest proportion of difficultly digestible or indigestible woody-fibre, the sheep less, the pig scarcely any, but a much larger proportion of comparatively easily digestible starch, whilst man, within certain limits, the better he is fed the less does the non-nitrogenous portion of his food consist of starch, and the more of the much more highly concentrated alimentary substance fat, produced for him from much less concentrated vegetable food-materials by the animals which he feeds for his own consumption.

From the facts which have been briefly stated, it will be obvious that, of the most important animals which we feed for human food, the pig offers many advantages as a subject for the consideration of the source in the food of the fat which he yields. Thus, for a given live-weight he comprises a comparatively small proportion of alimentary organs and contents, and he consumes a large proportion of food, and yields a large proportion both of total increase and of fat, within a given time; his food is, as such, of a high character, yielding, compared with that of oxen or sheep, for a given weight of it much more total increase, much more fat, and much less excreted and necessarily effete matter; whilst his proportion of fat is the greatest, both in a given live-weight and in his increase whilst fattening. It results that changes in his live-weight are in a much less degree likely to be influenced by variation in the amount of the contents of the stomach and intestines, and are therefore much more direct indications of real increase of the substance of the body, and hence that there is much less probable range of error in calculating the amount and composition of the increase in live-weight in relation to the amount and composition of the food consumed.

In fact, from the very opposite characters of the ruminant in these respects, it is very much less appropriate for the purpose of estimating the sources in its food of the fat of its body. It is true that there is the advantage with the cow, that that important product of the food—the milk—is collected externally to the body, and hence its amount and composition can be easily determined; but the changes of weight of the animal itself, though comparatively small, are due to a greater variety of circumstances, and can, therefore, with less of certainty be properly interpreted than even in the case of either the ox or the sheep. Indeed, when experiments are conducted with cows or oxen, or even with sheep, for periods of a few weeks only, the variation in live-weight may in very great proportion be due to variation in the contents of the alimentary organs merely.

The selection and calculation of results brought to view in Table II. (p. 8) will show that, when experiments are conducted with pigs fed on good fattening food for periods of not less than eight or ten weeks, the amounts both of total increase and of fat stored up are so great in proportion both to the original weight of the animal and to the food consumed, that the data so obtained may be safely relied upon as a means of estimating, with sufficient accuracy for the purposes of the present discussion, from what constituent or constituents of the food the fat of the animals has been derived.

Experiment 1.—In this experiment two pigs of the same litter, of equal weight, and, as far as could be judged, of similar character, were selected. One was killed at once, and the amount of total dry or solid matter, nitrogenous substance, fat, and mineral matter, in its body, determined. The other was then fed for a period of ten weeks on a good mixed food, containing, however, a more than usually high proportion of nitrogenous substance. It was then weighed and killed, and its composition was determined as in the case of the other animal. The results so obtained supplied an important portion of the data requisite for the calculation of the composition of the increase in the other cases*. The food consisted of a mixture of bean-meal, lentil-meal, and bran, each one part, and barley-meal three parts, given *ad libitum*.

* For further details relating to this and the other experiments, we must refer to our former papers, as follow:—"On the Composition of Foods in relation to Respiration and the Feeding of Animals," Report of the British Association for the Advancement of Science for 1852. "Agricultural Chemistry: Pig Feeding," Journ. Roy. Ag. Soc. Eng. vol. xiv. part 2, 1853. "On the Equivalency of Starch and Sugar in Food," Report of the British Association for 1854. "Experimental Inquiry into the Composition of some of the Animals Fed and Slaughtered as Human Food," Phil. Trans. part 2, 1859.

TABLE II.—Relation of the total Fat in the Increase to the ready-formed fatty matter in the Food, and of the Carbon in the Fat produced within the body to that in the nitrogenous substance consumed, in experiments with Fattening Pigs.

Experiments		1.	2.	3.	4.	5.	6.	7.	8.	9.
Conditions, and actual results of experiment.										
Number of animals		1	3	3	3	3	3	3	3	3
Duration of experiment (weeks)		10	8	8	8	8	10	10	10	10
Non-nitrogenous substance to one nitrogenous substance in food		3·6	3·3	2·0	6·6	6·0	4·1	4·1	4·7	3·9
Original live-weight (lbs.)		103	429	440	431	448	286	285	281	292
Final live-weight (lbs.)		191	685	743	652	739	533	533	553	604
Increase in live-weight (lbs.)		88	256	303	221	291	247	248	272	312
Increase on 100 original weight		85·4	59·7	68·9	51·3	64·9	86·4	87·0	96·8	106·8
Calculated for 100 increase in live-weight.										
Fat	Stored up in increase	63·1	73·9	69·6	79·0	71·2	64·1	63·9	62·0	59·9
	Ready-formed in food	15·6	20·4	11·2	26·3	12·4	7·9	7·9	7·3	6·6
	Not ready-formed (produced)	47·5	53·5	58·4	52·7	58·8	56·2	56·0	54·7	53·3
Nitrogenous-substance.	Consumed in food	100·0	107·0	138·0	57·0	64·0	81·0	81·0	74·0	82·0
	Stored up in increase	7·8	6·1	6·7	5·3	6·5	7·5	7·6	8·0	8·2
	Not stored up (available for fat, &c.)	92·2	100·9	131·3	51·7	57·5	73·5	73·4	66·0	73·8
Carbon	In "produced" fat	36·6	41·2	45·0	40·6	45·3	43·3	43·1	42·1	41·0
	In "available" nitrogenous substance *minus* urea	44·0	48·1	62·6	24·7	27·4	35·1	35·0	31·5	35·2
	Difference	+7·4	+6·9	+17·6	15·9	17·9	8·2	8·1	10·6	5·8
Calculated for 100 carbon in estimated "produced" fat.										
Carbon	In "available" nitrogenous substance *minus* urea	120·2	116·7	139·1	60·8	60·5	81·1	81·2	74·8	85·9
	Not available from nitrogenous substance	...	...	...	39·2	39·5	18·9	18·8	25·2	14·1

Experiments 2 & 3.—In both these experiments the proportion of nitrogenous substance in the food was very large; the relation of non-nitrogenous to one of nitrogenous substance being in Exp. 2 little more than half, and in Exp. 3 little more than one-third as much as is usual in the recognized good fattening food of the animal. In Exp. 2 the food consisted of bran, bean and lentil-meal, and Indian-meal, each given separately, and *ad libitum*; and in Exp. 3 of an equal mixture of bean and lentil-meal only, given *ad libitum*.

Experiments 4 & 5.—In Exp. 4 the food consisted of Indian meal only, and in Exp. 5 of barley-meal only, in each case given *ad libitum*. Barley-meal is undoubtedly the most approved staple fattening food of the pig; and the result was, that in both these experiments the proportion of non-nitrogenous to nitrogenous substance in the food was very nearly, though rather higher than, the average in that which is recognized as the most appropriate fattening food of the animal.

Experiments 6, 7, 8, & 9.—The peculiarity of this series was, that the food contained less ready-formed fat than was the case in either of the other experiments, and that a large proportion of the non-nitrogenous substance supplied was in the form of either pure starch, pure sugar, or both. In Exps. 6, 7 & 8, a fixed quantity of lentil-meal and bran (averaging nearly $3\frac{1}{4}$ lbs. lentil-meal and about 9 ounces bran) was given per head per day, and, in addition, in Exp. 6 sugar, in Exp. 7 starch, and in Exp. 8 sugar and starch, each separately, *ad libitum*. In Exp. 9 lentil-meal, bran, sugar, and starch were each given separately, *ad libitum*.

The figures given in the Table show that the increase in weight was in no case less than 50, and in several nearly, and in one more than 100 per cent., upon the original weight of the animals, the amounts ranging from 51·3 to 68·9 per cent. when the experiment extended over eight, and from 85·4 to 106·8 per cent. when it extended over ten weeks.

The determined or estimated amount of fat stored up in the increase was also in all cases very large, amounting to 63 per cent. of the total increase in Exp. 1, in which it was experimentally determined, and calculated to be even more than this in several of the other cases. The tendency to error in the calculations would, however, be to give the proportion too low in Exps. 6, 7, 8 & 9, which were conducted over a period of ten weeks, and in which the proportion of increase upon the original weight was very high, and to give it too high in Exps. 2, 3, 4 & 5, conducted only over eight weeks, but more especially in Exps. 2 & 4, in which the proportion of increase upon the original weight was comparatively small. The range of the probable error of calculation here indicated is, however, not such as in any degree to throw doubt upon the validity of any conclusions which will be drawn from the indications of the figures as they stand.

It is seen that, of the determined or estimated total fat stored up in the increase, the proportion which could possibly have been derived from the ready-formed fat of the food, even supposing the whole of that supplied had been assimilated, was so small as to leave no doubt whatever that a very large proportion of the stored-up fat must have been produced from other constituents than the ready-formed fatty matter of the food. According to the figures given in the Table, the proportion of fat which must have been so produced, ranged from about two-thirds to about eight-ninths of the total amount stored up.

Assuming it, then, to be established beyond doubt that there was a very large formation of fat within the body from other constituents than the fatty matter of the food, the questions arise, whether this large amount of produced fat could possibly have been derived from the nitrogenous constituents of the food? or whether it must of necessity have had its source, in greater or less proportion, in the carbo-hydrates at the same time supplied? The results adduced afford conclusive evidence on this point also.

The figures show that, after deducting from the total amount of nitrogenous substance consumed for the production of 100 lbs. of increase in live-weight the small amount estimated to be stored up in the increase, there remains a very large proportion available, it may be, for the production of fat with other products.

If we next compare the amount of carbon in the estimated *produced* fat with the amount contained in the nitrogenous substance of the food not stored up as increase, *minus* that contained in the urea which would be one of the final products of the breaking up of this nitrogenous substance (or its equivalent given off), the result shows in some cases an excess, and in others a deficiency, of carbon possibly available from the nitrogenous constituents of the food, compared with that required for the formation of the fat estimated to be derived from other constituents than the ready-formed fat in the food.

Reckoned to the standard of 100 carbon in the estimated produced fat, it is seen, as shown in the two bottom lines of the Table, that in Exps. 1, 2 & 3, in which the proportion of non-nitrogenous to nitrogenous substance in the food was (especially in Exp. 3) considerably less than in such food as experience has shown to be the most appropriate in the fattening of the pig—that is to say, in which the nitrogenous substance was in considerable excess over the amount and proportion usually supplied—there was, according to the calculation, more than sufficient carbon possibly available from the nitrogenous substance of the food for the formation of the fat estimated to be produced.

In Exps. 4 & 5, however, in which the relation of the non-nitrogenous to the nitrogenous substance in the food was much more nearly that in the usual food of the well-fed fattening pig, it is reckoned that there was about 40 per cent. of the carbon of the produced fat which could not possibly have been supplied from the nitrogenous constituents of the food.

In the other experiments (Nos. 6, 7, 8 & 9), in which again the proportion of the non-nitrogenous to the nitrogenous constituents of the food was lower than usual (though not so much so as in Exps. 1, 2 & 3)—in which, in fact, the nitrogenous constituents were in excess—there was still a considerable proportion of the carbon of the produced fat which the nitrogenous constituents of the food could not possibly have supplied.

It is hardly necessary to point out that, according to the mode of illustration we have adopted, the figures show not only the utmost proportion of the carbon of the stored-up fat which could possibly have had its source in the nitrogenous substance of the food, but even notably more than could possibly have been so derived. Thus, to say nothing of other considerations, it has been assumed for simplicity of illustration, and granted for the sake of argument, that the whole of the ready-formed fatty matter of the food contributed to the fat stored up, that the whole of the nitrogenous substance of the food not stored up as increase would be perfectly digested and become available for the purposes of the system, and that in the breaking up of the nitrogenous substance for the formation of fat no other carbon-compounds than fat and urea would be produced. It is obvious, however, that these assumptions are in part improbable, and in part quite inadmissible, and that the tendency of each of them is to show too large a proportion of the produced fat to have been possibly derived from the nitrogenous constituents of the food.

The amount of fat necessarily derived from other sources than the nitrogenous constituents of the food must therefore be greater than our mode of estimate can indicate; and it is obvious, from the figures given in the Table, that the less the excess of nitrogenous substance in the food, the greater was the proportion of produced fat which must necessarily have had its source in the carbo-hydrates of the food, and that, at any rate in those cases in which the proportion of non-nitrogenous to nitrogenous constituents supplied was the more nearly that occurring in the admittedly most appropriate fattening food of the animal, the proportion of the fat which must necessarily have been derived from the carbo-hydrates was very large, even allowing all that was possible to have been produced from the nitrogenous substance of the food.

That, nevertheless, fat may be produced in the animal body at the expense of nitrogenous substance, in greater or less degree according to the character of the animal and of the food, not only chemical and physiological considerations, but direct experimental evidence would lead us to conclude. Indeed we have, in former papers already referred to, called attention to the fact that the results of our experiments with fattening animals, when carefully considered, afford evidence in favour of such a conclusion. To discuss the point satisfactorily on the present occasion, by the aid of figures, would, however, unduly extend the limits of our paper.

But, as indicating the bearing of the results referred to, it may be stated, in passing, that in numerous cases, otherwise comparable, but in which the amount and proportion of the nitrogenous constituents consumed varied very greatly, the results clearly showed that neither the amount of food consumed, nor the amount of increase in live-weight produced, bore any direct relation to the amount of nitrogenous substance supplied. On the other hand, both the amount of food consumed and the amount of increase produced bore a very close relation to the supply of digestible non-nitrogenous constituents, and even a closer relation still to the amount of total digestible dry organic substance (that is, nitrogenous and non-nitrogenous taken together); whilst, so far as could be judged from careful observation, the proportion of nitrogenous to non-nitrogenous substance (fat) in the increase did not vary in anything like a corresponding degree with the variation in the proportion of the nitrogenous and non-nitrogenous constituents in the food. The animals consuming excessive amounts of nitrogenous substance did, indeed, show a greater tendency to increase in frame and flesh; but they nevertheless became fat. It would appear that the excess of nitrogenous substance had acted vicariously in defect of a greater supply of the non-nitrogenous constituents, contributing material not only to meet the respiratory exigencies of the animal, but also for the production of fat.

The main conclusions in regard to the sources of the fat of the animal body to which the evidence adduced has led, may be briefly stated as follows:—

1. That certainly a large proportion of the fat of the Herbivora fattened for human food must be derived from other substances than fatty matter in the food.

2. That when fattening animals are fed upon their most appropriate food, much of their stored-up fat must be produced from the carbo-hydrates it supplies.

3. That nitrogenous substance may also serve as a source of fat, more especially when it is in excess and the supply of available non-nitrogenous constituents is relatively defective.

ON THE CONNEXION

BETWEEN THE

HEAT OF THE BODY

AND THE

EXCRETED AMOUNTS OF UREA, CHLORIDE OF SODIUM, AND URINARY WATER,

DURING

A FIT OF AGUE.

BY

SYDNEY RINGER, M.R.C.S.,

LATE PHYSICIAN'S ASSISTANT IN UNIVERSITY COLLEGE HOSPITAL.

COMMUNICATED BY

RICHARD QUAIN, F.R.S.

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1859.

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RICHARD QUAIN, F.R.S.

Received June 6th—Read June 28th, 1859.

THE observations on the temperature of the body during ague fits are now numerous and accordant,[2] and may be considered sufficient to indicate the general condition of the animal heat during the several stages of ague.

[1] The following observations were made at the suggestion of Dr. Parkes, from whom, from time to time, I have received assistance. Dr. Parkes has also supplied the entire literature in the paper, and has kindly examined it with care.

[2] Zimmermann; Bärensprung, Müller's 'Archiv,' 1852, p. 217; Michael, 'Archiv für Phys. Heilk.,' 1856, p. 39; Wunderlich, 'Archiv für Phys. Heilk.,' 1858, p. 12.

The increase in the amount of urea and of chloride of sodium during the cold and hot stages has been affirmed by Traube and Jochmann,[1] Moos,[2] Redenbacher,[3] and Hammond.[4] These observations have been criticised, however, and certainly some of the cases were not examined with any great minuteness. In no case yet reported either has any comparison been drawn between the rise in the temperature and the amount of the urea and chloride of sodium.[5] It therefore seemed extremely desirable, on the occasion of a patient with ague being admitted into University College Hospital, under the care of Dr. Parkes, to examine the subject, and to see, first of all, whether the increase of urea and of chloride of sodium really occur in the cold and hot stages of ague, as affirmed by Traube, Redenbacher, and Hammond; and, secondly, whether any close connexion could be traced between the amount of urea and the abnormal temperature. My position as one of the resident officers in the hospital at the time gave me the opportunity of carrying out the inquiry with all the minuteness necessary for accuracy.

The general results may be thus stated—that not only was the increase of urea and of chloride of sodium constant during the cold and hot stages of ague, but that their amount was in very close relation to the temperature. The first case recorded in this paper will, I believe, give little short of mathematical proof of the connexion between these two phenomena, viz., the increase of heat of body and of the excretion of urea and chloride of sodium.

The second case communicated is one in which the phenomena were less carefully observed, but it is valuable as affording another instance of the increase of urea and chloride of sodium during the fit.

[1] 'Deutsche Klinik,' No. 46, Nov., 1855.

[2] Henle's 'Zeitschrift für rat. Med.,' Band vii, p. 291.

[3] Henle's 'Zeitschrift,' Band ii (Dritte Reihe), p. 384.

[4] 'American Journal of Med. Science,' April, 1858.

[5] Some other observations have been made on the excretions of twenty-four hours, but these are of little value as febrile and non-febrile bones are put together.

In addition, a case of hectic fever occurring in phthisis has been narrated, for in this case the phenomena were found to be identical with those presented by malarial ague. The result was the same in both cases, although the causes were so different.

CASE 1. *Quotidian ague.*—The patient, a man, was first attacked by tertian in August last, whilst working at Maldon, in Essex. The ague continued on him for three weeks, by which time he was apparently cured; he remained working at the same place till the December following. At that time, moving up to London, he broke his leg, and was carried to Charing Cross Hospital. Upon being discharged from that institution, cured, he caught cold, and an attack of ague immediately followed, and has continued more or less since, being immediately brought on by exposure to cold. This long continuance produced the usual effects of prolonged ague; and in this state he was admitted into the hospital, the disease having changed a day or two previously from the tertian to the quotidian type.

He suffers from slight aortic obstructive disease. His pulse is continually about 120 per minute,[1] exceedingly irregular in both force and rhythm. His arteries are very tortuous and visible. He never suffered from rheumatism, and has no arcus senilis. Except the above heart-disease, he suffers from no organic lesion.

He is fifty-nine years of age, but looks much older. His weight is 144 pounds; his height five feet nine and three quarter inches. His vital capacity (Hutchinson's spirometer) is 140 cubic inches.

The examination was conducted in the following manner. The patient was put to bed, and his urine collected through the night. At 5 a.m. he had breakfast, consisting of two eggs, bread and butter, and tea, the latter being measured. At 6 a.m. he was made to pass his urine, and all passed at

[1] No tables are therefore given of the pulse.

the time was mixed with that passed during the night. The thermometer (a good one, by Negretti and Zambra, and divided into fifths of a Fahrenheit degree) was next placed in his axilla, and kept there during the remainder of the day, the temperature being noted down every quarter of an hour. He was also made to pass his urine hourly, or upon the commencement of another stage, before the completion of the hour, and the next hour was dated from that time; by this means we not only obtained the urine of every hour, but also of each stage, separately. The patient was allowed no food until the completion of the fit, when he had a good meat dinner and six ounces of wine. Lemonade he was permitted to drink *ad libitum*, but the quantity was always measured and noted, with the time at which it was taken. He was weighed immediately after his breakfast, and again at the termination of the fit, and during this time no motions were allowed to be passed. The patient was purposely kept without medicine. In determining the amount of urea and chloride of sodium, Liebig's volumetric method with nitrate of mercury was used.

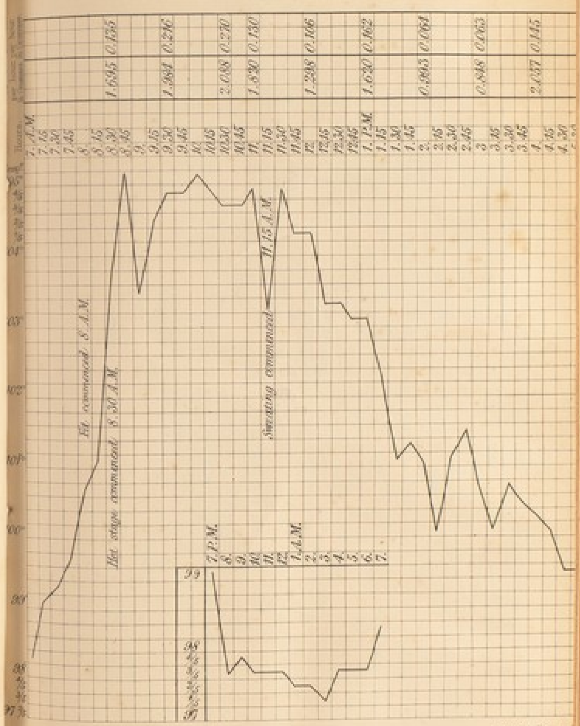
It was not possible to determine the other urinary constituents in this case. The chloride of sodium was not got rid of before testing for urea, but the usual correction was made.

The following charts show the variations in the temperature in fifths of a degree, as taken every quarter of an hour. Above the table are two columns, one showing the amount of urea, the other the amount of chloride of sodium, poured out. The quantity per hour is stated in each case, and put down in the column denoting the time at which it was passed. It indicates, of course, the quantity formed during the previous hour. The amounts of urea and chloride of sodium are given always in French grammes, and the water in cubic centimetres. The commencement of each stage, as judged of in the usual way, by the sensations of the patient, is also noted in the column proper to the time at which it began.

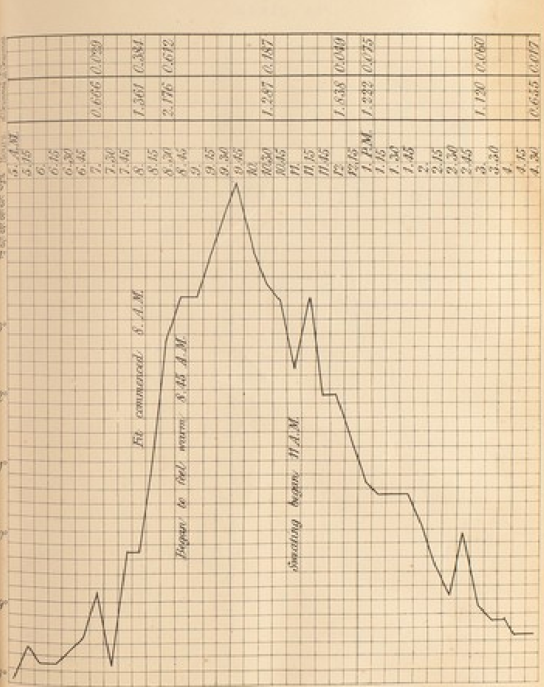
For the purpose of greater clearness and of giving fuller

No 1.

APRIL 6TH

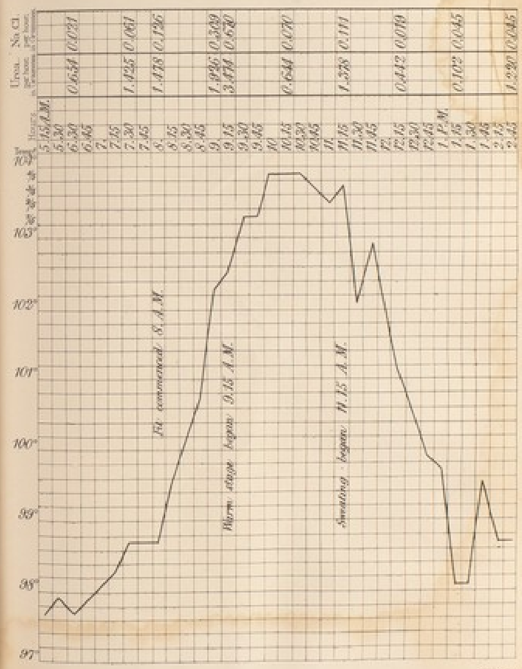


APRIL 7TH

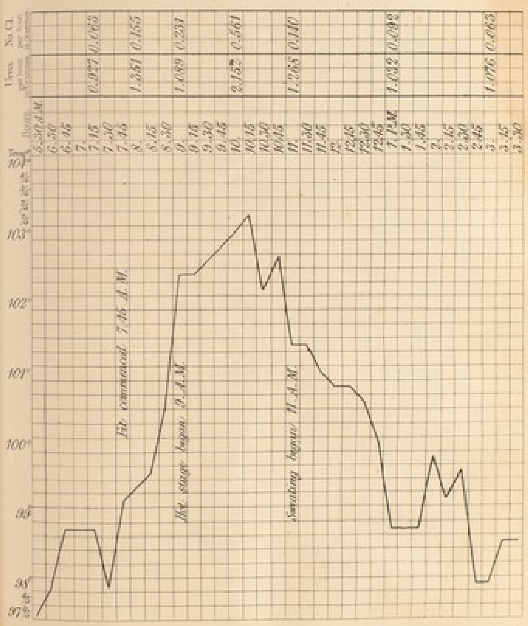


W. West lith.

APRIL 8TH

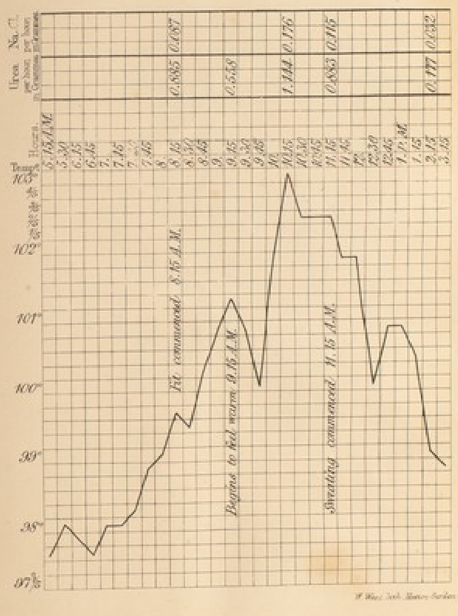

Urea.
Na Cl.
0.654 0.031
1.425 0.061
1.478 0.126
1.996 0.309
3.874 0.670
0.644 0.070
1.358 0.111
0.442 0.019
0.102 0.045
1.290 0.045
Hours
5.15 A.M.
1 P.M.
Temp.
104°
103°
102°
101°
100°
99°
98°
97°
Fit commenced 8. A.M.
Warm stage began 9.15 A.M.
Sweating began 11.15 A.M.

APRIL 9TH



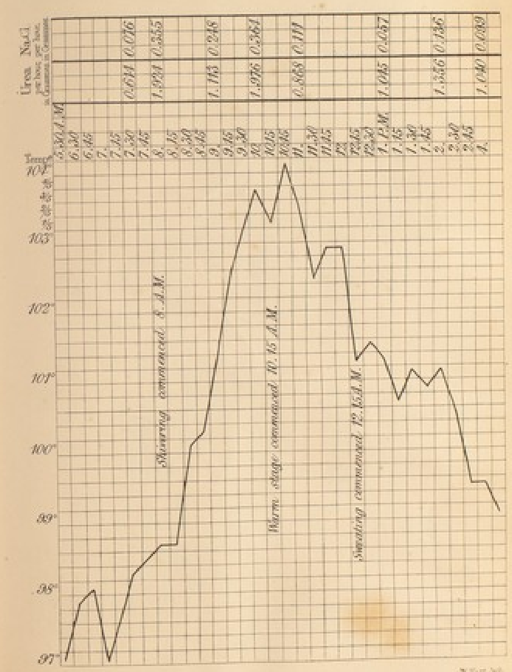
W. West lith.

APRIL 10TH



W. West lith. Hatton Garden

APRIL 11TH


Urea. NaCl. per hour, per hour, in Grammes in Grammes
0.614 0.076
1.321 0.355
1.115 0.248
1.976 0.361
0.868 0.111
1.045 0.257
1.356 0.136
1.040 0.089
5.30 A.M.
Temp.
104°
103°
102°
101°
100°
99°
98°
97°
Shivering commenced 8 A.M.
Warm stage commenced 10.15 A.M.
Sweating commenced 12.15 A.M.

details, tables are added, recapitulating the amount of the urea, chloride of sodium, and water, separately. In the first column of each table the stages are written down; in the next the hour at which the urine was passed; then follows the total amount of urea contained in the urine passed during the specified time; the next column shows the amount reduced to hours; and, lastly, the average amount of each stage per hour is given.

TABLE.

APRIL 6th.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Cold	7·45 to 8·30 a.m.	1·356	1·695	1·695
Hot	8·30 to 9·30 a.m.	1·984	1·984	
	9·30 to 10·30 a.m.	2·088	2·088	1·964
	10·30 to 11 a.m.	0·910	1·820	
Sweating	11 to 12 a.m.	1·289	1·289	
	12 to 1 p.m.	1·620	1·620	
	1 to 2 p.m.	0·993	0·993	1·363
	2 to 3 p.m.	0·848	0·848	
	3 to 4 p.m.	2·057	2·057	
		CHLORIDE OF SODIUM.		
Cold	7·45 to 8·30 a.m.	0·108	0·135	0·135
Hot	8·30 to 9·30 a.m.	0·216	0·216	
	9·30 to 10 30 a.m.	0·270	0·270	0·220
	10·30 to 11 a.m.	0·065	0·130	
Sweating	11 to 12 a.m.	0·106	0·106	
	12 to 1 p.m.	0·162	0·162	
	1 to 2 p.m.	0·064	0·064	0·108
	2 to 3 p.m.	0·063	0·063	
	3 to 4 p.m.	0·145	0·145	
		WATER.		
Cold	7 45 to 8·30 a.m.	75 c.c.		
	8·30 to 9·30 a.m.	80 c.c.		
Hot	9·30 to 10·30 a.m.	90 c.c.		
	10·30 to 11 a.m.	82 c.c.		
Sweating	11 to 12 a.m.	59 c.c.		
	12 to 1 p.m.	90 c.c.		
	1 to 2 p.m.	54 c.c.		
	2 to 3 p.m.	53 c.c.		
	3 to 4 p.m.	121 c.c.		

April 7th.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
During night...	3 p.m. to 5·30 a.m.	10·272	0·684	0·684
Period immedi-	5·30 to 7 a.m.	0·999	0·666	0·666
ately before				
shivering	7 to 8 a.m.	1·361	1·361	1·361
Cold	8 to 8·30 a.m.	1·088	2·176	2·176
Hot	8·30 to 10·30 a.m.	2·575	1·287	1·287
Sweating	11 to 12 a.m.	0·838	0·838	
	12 to 1 p.m.	1·222	1·222	0·935
	1 to 3 p.m.	2·240	1·120	
	3 to 5 p.m.	1·311	0·655	

CHLORIDE OF SODIUM.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
During night ...	3 p.m. to 5.30 a.m.	0·642	0·029	0·029
Period before	5·30 to 7 a.m.	0·440	0·29	0·29
shivering	7 to 8 a.m.	0·384	0·384	0·384
Cold	8 to 8·30 a.m.	0·306	0·612	0·612
Hot	8·30 to 10·30 a.m.	0·375	0·187	0·187
	11 to 12 a.m.	0·049	0·049	
	12 to 1 p.m.	0·075	0·075	0·064
	1 to 3 p.m.	0·120	0·060	
	3 to 5 p.m.	0·034	0·017	

QUANTITY OF URINE PASSED.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
	3 p.m. to 5·30 a.m.	36 c.c.		
Beforeshivering	5·30 to 7 a.m.	25 c.c.		
	7 to 8 a.m.	80 c.c.		
Cold	8 to 8·30 a.m.	136 c.c.		
Hot	8·30 to 10·30 a.m.	62 c.c.		
	11 to 12 a.m.	33 c.c.		
	12 to 1 p.m.	50 c.c.		
	1 to 3 p.m.	50 c.c.		
	3 to 5 p.m.	28 c.c.		

April 8th.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
Period immedi-	5 p.m. to 6·30 a.m.	8·832	0·654	0·654
ately before	6·30 to 7·30 a.m.	1·425	1·425	1·452
shivering	7·30 to 8 a.m.	0·738	1·478	
Cold	8 to 9 a.m.	1·926	1·926	2·700
	9 to 9·15 a.m.	0·868	3·474	
Hot	9·15 to 10·15 a.m.	0·644	0·644	1·011
	10·15 to 11·15 a.m.	1·378	1·378	
Sweating	11·15 to 12·15 p.m.	0·044	0·044	
	12·15 to 1·15 p.m.	0·102	0·102	0·498
	1·15 to 2·45 p.m.	1·830	1·220	

CHLORIDE OF SODIUM.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Night	5 p.m. to 6·30 a.m.	0·576	0·021	0·021
Before fit	6·30 to 7·30 a.m.	0·064	0·064	0·095
	7·30 to 8 a.m.	0·063	0·162	
Cold	8 to 9 a.m.	0·309	0·309	0·381
	9 to 9·15 a.m.	0·167	0·670	
Hot	9·15 to 10·15 a.m.	0·070	0·070	0·090
	10·15 to 11·15 a.m.	0·111	0·111	
	11·15 to 12·15 p.m.	0·019	0·019	
	12·15 to 1·15 p.m.	0·045	0·045	0·027
	1·15 to 2·45 p.m.	0·045	0·030	

QUANTITY OF URINE PASSED.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
Night	5 p.m. to 6·30 a.m.	42 c.c.		
Before fit	6·30 to 7·30 a.m.	54 c.c.		
	7·30 to 8 a.m.	60 c.c.		
Cold	8 to 9 a.m.	86 c.c.		
	9 to 9·15 a.m.	172 c.c.		
Hot	9·15 to 10·15 a.m.	26 c.c.		
	10·15 to 11·15 a.m.	52 c.c.		
	11·15 to 12·15 p.m.	13 c.c.		
	12·15 to 1·15 p.m.	38 c.c.		
	1·15 to 2·45 p.m.	50 c.c.		

April 9th.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
Immediately be-	6·15 to 7·15 a.m.	0·927	0·927	0·927
fore shivering..	7·15 to 8 a.m.	1·081	1·351	1·351
Cold	8 to 9 a.m.	1·089	1·089	1·089
Hot	9 to 10 a.m.	2·152	2·152	1·710
	10 to 11 a.m.	1·268	1·268	
Sweating	11 to 1 p.m.	2·164	1·032	1·129
	1 to 3 p.m.	2·353	1·076	

CHLORIDE OF SODIUM.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
Before fit	6·15 to 7·15 a.m.	0·063	0·063	0·063
	7·15 to 8 a.m.	0·124	0·155	0·155
Cold	8 to 9 a.m.	0·231	0·231	0·231
Hot	9 to 10 a.m.	0·561	0·561	0·351
	10 to 11 a.m.	0·140	0·140	
Sweating	11 to 1 p.m.	0·184	0·092	0·078
	1 to 3 p.m.	0·127	0·063	

QUANTITY OF URINE PASSED.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
Before fit	6·15 to 7·15 a.m.	35 c.c.		
	7·15 to 8 a.m.	61 c.c.		
Cold	8 to 9 a.m.	55 c.c.		
	9 to 10 a.m.	117 c.c.		
	10 to 11 a.m.	52 c.c.		
	11 to 1 p.m.	44 c.c.		
	1 to 3 p.m.	53 c.c.		

APRIL 10TH.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Before fit	6·30 to 8·15 a.m.	1·420	0·885	0·885
Cold	8·15 to 9·15 a.m.	0·558	0·558	0·558
Hot	9·15 to 10·15 a.m.	1·144	1·144	1·013
	10·15 to 11·15 a.m.	0·883	0·883	
Sweating	11·15 to 2·15 p.m.	0·532	0·177	0·177

CHLORIDE OF SODIUM.

Before fit	6·30 to 8·15 a.m.	0·140	0·087	0·087
	8·15 to 9·15 a.m.	Urine	lost.	
Hot	9·15 to 10·15 a.m.	0·176	0·176	0·145
	10·15 to 11·15 a.m.	0·115	0·115	
Sweating	11·15 to 2·15 p.m.	0·070	0·032	0·032

QUANTITY OF URINE PASSED.

Before fit	6·30 to 8·15 a.m.	50 c.c.		
Cold	8·15 to 9·15 a.m.	20 c.c.		
Hot	9·15 to 10·15 a.m.	44 c.c.		
	10·15 to 11·15 a.m.	32 c.c.		
	11·15 to 2·15 p.m.	22 c.c.		

APRIL 11TH.—UREA.

During night ...	2·15 p.m. to 6 a.m.	9·785	0·609	0·609
	6 to 7·30 a.m.	0·921	0·614	0·614
Immediately before fit ...	7·30 to 8 a.m.	0·962	1·924	1·924
Cold	8 to 9 a.m.	1·113	1·113	1·476
	9 to 10 a.m.	1·839	1·839	
Hot	10 to 11 a.m.	0·868	0·868	0·868
Sweating	11 to 1 p.m.	2·090	1·045	1·147
	1 to 2 p.m.	1·356	1·356	
	2 to 4 p.m.	2·090	1·040	

CHLORIDE OF SODIUM.

	2·15 p.m. to 6 a.m.	0·139	0·087	0·087
	6 to 7·30 a.m.	0·115	0·076	0·076
Before fit	7·30 to 8 a.m.	0·355	0·355	0·355
Cold	8 to 9 a.m.	0·248	0·248	0·306
	9 to 10 a.m.	0·364	0·364	
Hot	10 to 11 a.m.	0·111	0·111	0·111
Sweating	11 to 1 p.m.	0·114	0·057	0·097
	1 to 2 p.m.	0·136	0·136	
	2 to 4 p.m.	0·119	0·099	

QUANTITY OF URINE PASSED.

Stage.	Hour.	Total Quantity in this time.
Night	2·15 p.m. to 6 a.m.	...
	6 to 7·30 a.m.	24 c.c.
Before fit	7·30 to 8 a.m.	74 c.c.
Cold	8 to 9 a.m.	64 c.c.
	9 to 10 a.m.	76 c.c.
Hot	10 to 11 a.m.	31 c.c.
	11 to 1 p.m.	38 c.c.
	1 to 2 p.m.	57 c.c.
	2 to 4 p.m.	95 c.c.

CONCLUSIONS FROM THE PREVIOUS FACTS.

I. *Temperature.*

The temperature during the several fits, amid much general resemblance, presented numerous partial differences, all of which are, however, reducible to order.

(*a*) In every case, the temperature commenced to rise previous to the cold stage,[1] as experienced by the patient, that is to say, before any feeling of cold or illness of any kind. The time that it commenced, previous to the cold stage, varied from an hour and a half to three quarters of an hour, and no connexion between the time and the severity of the fit could be traced. The temperature commenced to rise—

April 6th. . $1\frac{1}{2}$ hour before any feeling of cold or illness.
" 7th. . 1 " "
" 8th. . $1\frac{1}{2}$ " "
" 9th. . $1\frac{1}{4}$ " "
" 10th. . $1\frac{1}{4}$ " "
" 11th. . $\frac{3}{4}$ " "

A close connexion between the severity of the fit and the character of the rise before the cold stage existed, the severity being in proportion to the continuousness of the rise, and also to the extent of each rise. As the fit became less severe, the temperature had a tendency either to oscillate, rise slowly, or remain stationary, and these tendencies

[1] The same fact is noted by Michael, op. cit., p. 43.

increased as the fit decreased in severity, and eventually all combined.

(*b*) During the cold stage the temperature rose throughout, the rise being greater than during any other period; but here also great variations existed, having their counterpart in variations in the severity of the fit.

Thus, in the cold stage, the temperature rises continuously when the fit is severe; then, as the severity lessens, there is a tendency for the rise at the termination of the cold stage to flag, and eventually to become stationary; then the range of each rise becomes less; and lastly, it oscillates. It also appears that the alteration first affects the commencement and termination of the cold stage, an oscillation in its middle indicating a greater diminution of the severity of the fit than at either end.

There was no connexion between the duration of the stage and the severity of the fit, as measured by the temperature. Thus, it lasted—

On the 6th	$\frac{3}{4}$	hour.
" 7th	$\frac{3}{4}$	"
" 8th	$1\frac{1}{4}$	"
" 9th	$\frac{3}{4}$	"
" 10th	1	"
" 11th	2	hours.

The fits were most severe on the 6th and 7th, and declined in severity till the 11th, when the fit was again more severe.

(*c*) During the hot stage the temperature continued to rise, and in its early part closely corresponded to the former periods.

From a careful examination of the charts, it appears that in the severer cases the temperature in the hot stage ran up at once to its acme, and had a tendency to remain permanent; the permanency not, however, being obtained at once nor retained throughout, the temperature oscillating both at the commencement and termination. In less severe cases it ran up slowly, and did not remain stationary, and when least severe, oscillated in its rise.

The temperature in those fits becoming less severe first lost its permanency, and next the temperature rose to a less extent

and again became permanent; then, when they became still less severe, it rose to the same extent, but again lost its permanency. Again, in those cases where the temperature remained permanent, the hot stage ended at the termination of the permanent period in an oscillation, these cases being the severest at their own temperature. In all other cases the temperature fell before the sweating stage commenced, ending in an oscillation, and the less severe the fit the greater was the portion of the hot stage occupied by the fall of the temperature. Also the fit was less severe, and the fall greater, when the temperature in falling every now and then stood still for some time, and was still less severe when it oscillated.

(*d*) The temperature continued to fall through the sweating stage, and was often a long time before it reached the point from which it started. The temperature fell most rapidly in those cases in which the oscillations occurred, and least rapidly where the fall was continuous. Towards the termination of the sweating stage a rise occurred to a slight extent (perhaps followed by oscillations) in four of the six fits. Thus, on April 6th, sweating commenced at 11·15, when the thermometer marked $103\frac{1}{5}°$; at 2·15 the temperature had fallen to 100°; it then rose to 101° at 2·30, to $101\frac{3}{5}°$ at 2·45; then fell to $100\frac{1}{5}°$ and 100° at 3 and 3·15; then rose to $100\frac{2}{5}°$ at 3·30, and then finally fell regularly to $97\frac{3}{5}°$, a point not reached till seven o'clock. The charts show, at a glance, these slight but perfectly definite rises at this late period of the sweating stage.

II. *Urea*.

As the type of the disease in this man was quotidian, there was no opportunity of comparing the amount of urea on a fever and on a fever-free *day*. But the amount of urea passed in the fever-free *hours* was decidedly much smaller than might have been anticipated. He excreted only 0·650 grm. on an average per hour in the apyrectic period, which would give in twenty-four hours 15·600 grms., had the excretion remained at the same amount during the whole day. Now, a man of the same weight, between twenty and forty years of age, on a good diet, as this man was, would have secreted 32 grms. in the twenty-four hours, if he passed the average amount. Our patient was, however, older (fifty-nine), and would, no doubt, form less urea than a man at a more vigorous period of life. But it can hardly be supposed that the amount would be reduced so low as 15½ grms. in the twenty-four hours by this difference of age. It may, therefore, be concluded that, in accordance with Redenbacher's observations, the excretion of urea in the fever-free period was below the healthy amount.

The observations made by Traube, Redenbacher, and Hammond on the increase of urea during the cold and hot stages are entirely confirmed by this case, but a more minute statement of the kind and amount of the increase can now be given.

It must, however, be premised, that the only obvious causes of the increase of the urea in this case during the fit are, either the food taken at breakfast at 5 a.m., the fluid drunk during the fit, or the fit itself. No other known causes existed which could have had the effect of increasing the urea. The following objections to the idea of the food being the cause may be urged. That the amount of food was not great, and that the increase in the urea was far larger than has yet been noted after even the heaviest meal. For example, on the 7th, the amount per hour rose from 0·684 grm. to 2·176, being an increase of more than 200 per cent.; on the 8th, from 0·654 to 3·474, being an increase of nearly 500 per cent.; on the 9th and 10th, when the fits were slighter, the increase was less marked, though still considerable; while on the 11th, when the fit was severe, it was again 200 per cent. Such an amount is greater than has yet been found after an ordinary amount of food.[1] The time, however, at which the urea increases after food accords with our case; for, augmenting even

[1] Dr. Parkes has noted in one person without fever a rise from 0·665 grm. in a fasting hour to 1·554 grm. in a food hour, but this was after a hearty dinner.

during the first hour after food,[1] it attains its maximum sometimes in the third hour;[2] sometimes, however, not till the seventh hour; usually, however, it reaches its maximum at the fourth hour.

Now, in this case food was taken between 5 and 6 a.m., and the maximum amount of urea secreted was during the cold stage, from 8 to 9·30 a.m., or in the fourth and fifth hours. But it will probably be conceded, after it has been shown how closely the amount of the urea was associated with the variation in the temperature, that its increase in the fourth and fifth hours after food was merely a coincidence, and was not owing to the very moderate breakfast, but to the fact of the highest temperature occurring at this time. On one day, moreover, he took no food, having no appetite, and on this day the usual increase occurred. Again, after the fits were stopped by the quinine, the food was given him as usual, and the urine being collected on the same day, in the same way, comparatively little rise took place in the urea. These two last points, I conceive, set the question quite at rest.

With respect to the amount of fluid drunk, this could have no effect on the urea, as very little fluid was taken till after the time when the urea had commenced to rise. The urea, moreover, reached its maximum often at the termination of the cold stage, whilst he seldom drank anything between his breakfast and the hot stage. I believe, then, that I am justified in concluding that the rise in the amount of urea was not owing either to food or liquid. It must, therefore, have been owing to the fit.

The urea begins to increase in amount *before* the cold stage, as judged of by the first feeling of shivering, in four of the five fits. Thus, it rose—

On the 7th, from 0·666 to 1·361 per hour.
" 8th, from 0·654 to 1·425 "
" 9th, from 0·927 to 1·351 "
" 11th, from 0·614 to 1·924 "

[1] Voit, quoted by Meissner, "Report on Phys. for 1857," in Henle's 'Zeitschrift,' p. 352.

[2] Becker, Henle's 'Zeitschrift,' 1855, p. 549.

The time it commenced to rise before the subjective fit varied. Thus—

On the 7th.... ¾ hour before.
,, 8th....1½ ,,
,, 9th....1¾ ,,
,, 11th.... ½ ,,

It often began to increase, indeed, even before the temperature began to rise. Thus, as the temperature on the 7th rose before the fit ¾ of an hour, on the 8th 1¼, on the 9th ¾, the urea on those days commenced to rise previous to any similar change in the temperature.

Great apparent irregularity existed in the rise of urea, but a close correspondence is observed between these variations and similar ones in the temperature.

The alteration in the above case of urea and the temperature did not always exactly agree in time, though this was the rule, but sometimes the alteration in the temperature did not occur till after the alteration in the urea; the reverse never happened.

The quantity of urea continued to increase, and reached its highest point either at the termination of the cold stage, as on the 8th and 11th, or at the commencement of the hot, as on the 6th, 9th, 10th, and 12th. It then began to fall in quantity, slowly at first. On the 6th and 10th the temperature and urea commenced to fall simultaneously. On the 7th, 8th, 9th, and 11th, the temperature continued to rise, notwithstanding the fall in the urea. Up to the period when the urea commenced to decrease, the temperature rose rapidly, each rise being extensive, but after that, at the point where the urea commenced to fall, the temperature either oscillated, or every rise remained stationary for a short time, the rise being always slow and by small additions.

The urea continued to fall slowly from the end of the cold, or from the commencement of the hot stage, till the sweating stage began; and the temperature corresponding in time to the latter part of this slow decrease, after reaching its height, fell also slowly and slightly.

From the following table it appears that no close correspondence existed between the fall in the temperature towards the close of the hot stage and the decrease in the amount of urea.

Date.	Fall in Urea.	Fall in Temperature in same time.	Temperature oscillated.	Tendency to be stationary.	Temperature fell continuously.	Fall of Urea reduced to quantity for 1° Fahr.
6th	0·268	⅕°		—		1·346
7th	0·830	1⅗°			—	0·533
8th	1·452	⅘°			—	...
9th	0·884	1°			—	0·884
10th	0·261	⅗°		—		0·435
11th	1·000	2⅘°	—			0·357

In the first column the date is given; in the second the fall in the amount of urea, corresponding in time to the fall in the temperature, from its highest point to the commencement of the sweating stage, is given; next, the number of degrees the temperature fell in the same time; then, in three columns, the character of the fall is given; and in the last column the fall in the amount of urea is reduced to the quantity corresponding to each degree. But though there is no intimate connexion, still the greater the fall in the temperature the greater is the decrease in the quantity of urea. At the same time the character of the fall varied; thus, sometimes the temperature showed a tendency to remain stationary, then the urea fell but little, as on the 6th and 10th. On other days the temperature fell gradually, and then the fall in urea rather increased. The greatest fall occurred on those days when the temperature oscillated greatly. Thus, the least decrease in urea corresponded to that temperature which has a tendency to remain stationary, more when it fell slowly but continuously, and much the most when it oscillated, as on the 11th; and it is possible to judge of the rapidity of the decrease of the urea by the character of the temperature.

The temperature has been shown to fall either at the close of the hot or the commencement of the sweating stage. A similar fall occurred in the urea, and the amount of the decrease of urea corresponded to the length of the oscillation, and the subsequent rise in the urea corresponded to the rise in the oscillation, though the whole oscillation always occurred in an hour previous to that in which the subsequent rise in the urea occurred.

Date.	Fall in Temperature.	Fall in Urea.	Rise in Temperature.	Rise in Urea.
6th	$1\frac{3}{5}$°	1·820 to 1·289	$1\frac{3}{5}$°	1·289 to 1·620
7th	1°	1·287 to 0·838	1°	0·838 to 1·222
8th	$1\frac{3}{5}$°	3·474 to 0·644	$\frac{4}{5}$°	0·644 to 1·378
11th	$1\frac{3}{5}$°	1·839 to 1·045	$\frac{3}{5}$°	1·045 to 1·356

A slight rise, however, took place at the same period, even when the temperature remained permanent, but then the increase was small in amount.

On the 9th, the temperature fell, but did not oscillate; the fall in the urea was not so great as when the temperature oscillated.

A relationship in the latter part of the sweating stage between the variation in the urea and temperature existed, corresponding to what is stated above. Thus, on the 6th, after the usual oscillation, the temperature had a tendency to fall, and then to remain stationary for some time, and again to fall and remain a second time stationary. During this time the urea fell in quantity slowly. The temperature then took a great fall, and oscillated greatly, and at the same time the fall in urea was very great.

On the 7th the temperature fell slowly, and often remained stationary; at the same time the urea fell slowly, and, like the temperature, did not reach the normal amount till late in the afternoon.

The same close correspondence exists between the other cases.

In order to test still further the relationship between the urea and the temperature, the following method was adopted. The total amounts of urea excreted during the fit, the quantity excreted during the rise and fall of temperature, during the period immediately before the fit, and during the cold and hot stages, respectively, have been added up and divided by the number of degrees the temperature rose or fell in the corresponding periods. By this means the amounts of the urea secreted during the whole and each of the divisions of the fit were reduced to a common standard, and could thus be compared more easily with one another.

Table[1] to show the amount of Urea corresponding to each degree of Fahrenheit of abnormal heat.

Date.	Entire fit. The amount of urea for each degree of abnormal heat.	Rise in the temperature taken, and the amount of urea to each degree.	Fall in the temperature and the urea to each degree.	The quantity of urea to each degree of the temperature before the fit.	The same in the cold stage.	The same in the hot stage.
6th	1·557	0·626	0·930	...	0·339	3·560
7th	1·413	0·615	0·798	0·972	0·391	1·609
8th	1·508	0·903	0·722	2·163	0·735	1·444
9th	1·460	0·675	0·710	0·566	0·246	2·793
0th	1·298	0·718	0·746	0·657	0·617	1·381
11th	1·781	0·712	1·280	0·601	0·590	0·868

[1] In this table each of the quantities of urea correspond to 1° of temperature. In the first column the date is given; in the second, the whole amount of urea passed during the entire fit (the fit being considered to

On comparing the different days, the closest correspondence existed when the whole fit was taken. The extreme in column two being taken, the difference only amounted to one fifth of a degree. The rise and fall also closely corresponded. The extreme again being taken, a difference amounting to one third of a degree was found.

As the duration of the fit might have varied greatly, and so the above mode of comparison of the different days one with the other have been invalidated, the following tables have been worked out.

The first shows the duration of the fit on each day.

6th......	$7\frac{3}{4}$	hours' duration.
7th......	8	"
8th......	$8\frac{1}{4}$	"
9th......	$7\frac{3}{4}$	"
10th......	$8\frac{1}{4}$	"
11th......	$7\frac{1}{2}$	"

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commence with the rise of the temperature and the increase of the urea and chloride of sodium). In the third column the rise (in the temperature) only is taken, and when the temperature remained stationary at its highest point for some time the time of the rise is calculated up to the middle of the stationary part. In the next column the fall, calculated in the same way, is given. Then the cold and hot stages are given separately.

The amount of urea varied in this patient; for the night urine varied on one day greatly from the quantity passed during a similar period on other nights. This day, the 9th (the urine of the night before not having been saved), the amount of urea corresponding to each degree was greatly in excess; but on assuming that the quantity passed during the first hour 0·900 grm.) was the quantity normal to him for that day, and so deducting the excess, a close correspondence to the other days resulted. Thus, when the whole amount of urea was calculated as it stood, to each degree 2·043 grms. were found to correspond; but when the additional 300 grms. were deducted, then the quantity per degree was 1·460; and throughout all the different comparisons it will be then found closely to agree. On the 10th, again, the normal excretion was below par (0·170 grm. per hour), which also prevented any comparisons from being made. When this was raised to 0·600 grm., then a much closer correspondence to the other days was found.

Another calculation must now be made. On three days the urine was collected through the previous night; taking that hourly amount, and assuming that to be the amount normal to the man through that day, if there had been no disturbing influences, and deducting that quantity from the quantity passed hourly through the fit, the increase only is obtained, and this divided by the number of degrees the temperature rose, the comparison is then found to be very close.

April 7th......	0·638	urea to each degree	of abnormal heat.
" 8th......	0·618	"	"
" 11th......	0·624	"	"

If this table be compared with the former one, the comparison will be found to be even much closer. Unfortunately, the amount during the night was not determined except on these three days, so that this mode of calculation is not applicable to all the fits.

On looking at the second column of the table given at page 377, where the total amounts are compared, it is found that on the 6th, 8th, and 11th, the quantity in excess is somewhat over other days. The temperature on the 6th and 8th remained permanent after reaching its highest point. Thus, on the 6th it remained at 105° for two hours and a half; on the 8th at $103\frac{3}{5}$° for one hour and a quarter; but when the amount passed during these permanent periods is deducted, then the urea on the 6th fell to 1·277 grm., and on the 8th to 1·366 grm., bringing each on a level with the 10th. On the 11th, the increase in urea occurred entirely in the fourth column, that is, during the fall of the temperature, the amount passed during the rise corresponding closely to the other days; and on examining the sweating stage on the 11th, there is found a great and sudden increase at its very termination, and this excessive quantity being replaced by the quantity passed on other days at the same period, the amount corresponding to each degree was 1·510, closely coinciding with the other days.

That the increase on the 6th and 8th was due to the above is rendered probable by the following statement: The whole quantity of urea passed on the 6th, during the stationary period of two hours and a half, was 1·957 grm. On the 8th, for one hour and a quarter, the quantity was 1·155. When compared to equal time, that is, the two hours and a half reduced to one hour and a quarter, then, on the 6th, the amount of urea was 0·978, corresponding closely to the quantity passed in the same time on the 8th. From this it would appear that the same amount of urea corresponds to 1°, when the temperature is permanent, whether it be at a high temperature or not, whilst the second table shows that *every degree in the hot stage corresponds to a much greater amount of urea than in the cold or any other stage, whilst the temperature is rising*; thus showing, that the higher the temperature, the more urea corresponds with each rise of a degree than formerly, and thus accounting in some measure for the greater quantity of urea to each degree, when the whole fit is compared, in the more severe fits.

The very close correspondence between the temperature and the urea seems to be thus placed beyond doubt; in fact, the one may be calculated from the other. An example or two will illustrate this.

Thus, on—

April 6th, the amount of urea excreted during the fit was 11·097 grammes; taking 1·503 gramme as the average quantity (all the degrees being taken) corresponding to each degree, this gives a rise of 7°, or a little over; the actual rise was from 98⅕ to 105, or 6⅘°.

April 7th, the total amount of urea during the fit was 10·635 grammes. The rise in the temperature here should be 7°; the actual rise was 6⅘°.

April 8th, the total amount of urea excreted was 9·353 grammes, which would imply a rise in the temperature of 6°; the actual rise was from 97⅗ to 103⅗, being exactly 6°.

III. *Chloride of Sodium.*

The chloride of sodium, to a very great extent, agreed with the urea in its relationship to the temperature. Traube's statement on this point is therefore confirmed. For the most part, the remarks above made on the urea hold good for the chloride of sodium; still some differences occurred.

On the 8th, the urea between 7·30 and 8 a.m. remained stationary, whilst the chloride of sodium rose continuously.

9th.—Between 8 and 9 a.m. the urea fell, the temperature corresponding, but the chloride of sodium rose to double its quantity.

Again, during the decline of the fit, they differed on the 8th and 9th; the urea at its termination rose somewhat, whilst the chloride of sodium continued to fall.

The following table corresponds to one given for the urea, and shows the intimate connexion between the severity of the fit and the amount of chloride.

Date.	Entire fit. The amount of chloride of sodium for each degree.	Rise in the temperature taken, and the amount of chloride to each degree.	Fall in the temperature taken, and the amount of chloride to each degree.	The quantity of chloride to each degree of the temperature during the cold stage.	The same during the hot stage.	The same during the period before the cold stage.
6th	0·195	0·085	0·086	0·027	0·293	...
7th	0·225	0·159	0·066	0·085	0·259	0·640
8th	0·171	0·107	0·078	0·131	0·162	0·134
9th	0·241	0·150	0·062	0·175	0·702	0·103
10th			Lost.			
11th	0·180	0·149	0·041	0·102	0·176	0·785

Here, as occurred in the urea, the patient on two days passed a different quantity normally from what he had on former days; so that on one day, the 8th, it was necessary to add 0·022 grm. to each hour to bring it up to the other days. On the 11th it was necessary to subtract 0·044 grm.

On the 6th the urine was not collected before the commencement of the cold stage, so that the amount for each degree is less than it would otherwise have been in the third and fourth columns.

When the extremes in the third column were compared, a difference corresponding to one third of a degree of temperature occurred. When the fourth column was compared, excluding the 6th, a difference of one third of a degree occurred. In the fifth column, a difference corresponding to less than one third occurred, excluding the 11th, on which day the chloride fell to its normal amount before the fit had ended.

In the cold stage an enormous difference was found.

In the hot the difference corresponded to half of a degree.

Beyond these above differences, all that has been said regarding the urea applies equally to the chloride of sodium.

IV. *Water of the Urine.*

In rising and falling in quantity, the water has a very close correspondence to the urea, though there is very little proportion between the different rises and falls in the two. On the 6th, 8th, and 9th, they corresponded in rising and falling at the same periods. On 11th and 7th the quantity of urine in the hot stage falls so considerably, that in the commencement of the sweating stage a slight rise occurs, this being followed, however, by a decided fall during the second hour.

The quantity of urine thus corresponding to the urea, must, like it, correspond somewhat to the variations in the temperature. That the quantity stands in close relation to the intensity of the fit is seen from the following table.

Date.[1]	Quantity of urine in the fit.	Number of degrees temperature rose.	Quantity of urine to each degree.
6th	608 c.c.	$5\frac{1}{5}$	117 c.c.
7th	411 c.c.	$5\frac{4}{5}$	71 c.c.
8th	449 c.c.	$6\frac{1}{4}$	72 c.c.
9th	382 c.c.	$5\frac{3}{8}$	71 c.c.
11th	435 c.c.	$5\frac{4}{5}$	75 c.c.

It has been shown that some correspondence exists between the oscillation of temperature at the commencement of the sweating stage and the fall and subsequent rise in the amount of urea. The following table shows the relationship between the same oscillation and the fall in the amount of urine passed.

On the 6th and 11th it has been said that the amount of urine secreted was so small during the hot stage that it rose somewhat at the commencement of the sweating, so that no comparison can be given on those days between the fall of the oscillation and the quantity of urine. In the next table, in the first column, as usual, the day of the month is given; in the second, the fall of the temperature in the oscillation; in the third, the fall in the amount of urine; in the fourth, the amount reduced to a degree; in the fifth column, the rise of the temperature in the oscillation is given; in the sixth, the rise in the amount of urine in the hour subsequent; and in the seventh, the quantity is reduced to that corresponding to a degree:

[1] In the first column the date is given. In the second, the total quantity of urine passed during the fit. In the third, the number of degrees the temperature rose during the fit. In the fourth, the number of cubic centimetres corresponding to each degree. Excluding the first day, a very close correspondence existed.

6th	Urine rose		...	1$\frac{3}{5}$°	0·030	0·020
7th	1°	0·030	0·030	1°	0·017	0·017
8th	1$\frac{3}{5}$°	0·039	0·025	$\frac{4}{5}$°	0·025	0·030
11th	Urine rose		...	$\frac{1}{5}$°	0·007	0·035

From this limited table, no great correspondence can be traced.

But though the urea, chloride of sodium, and water, thus constantly show a close correspondence to the temperature, the relative amount of rise was different in these three ingredients.

This indeed was shown well during the analysis, in which 10 c.c. of urine were taken; the amount of mercury solution required for the amount of urea varied greatly from hour to hour, showing no regularity of rise; whilst, on the other hand, the amount required in testing for the chloride gradually rose and then gradually sank, often again rising somewhat at the very close of the fit. Thus, to take the 7th of April:

UREA.

Hour.	Amount of mercury solution required for 100 pints of urine.	Same for NaCl.	Hourly amount of water passed.
7 a.m.	298 c.c.	48 c.c.	25 c.c.
8 a.m.	198 c.c.	144 c.c.	80 c.c.
8·30 a.m.	188 c.c.	135 c.c.	136 c.c.
10·30 a.m.	224 c.c.	90 c.c.	62 c.c.
12 noon.	272 c.c.	45 c.c.	33 c.c.
1 p.m.	268 c.c.	45 c.c.	50 c.c.
3 p.m.	248 c.c.	36 c.c.	50 c.c.
5 p.m.	258 c.c.	18 c.c.	28 c.c.

The urea varies in its proportion to the water; thus, during excessive diuresis, the per-centage amount falls, whilst, on the other hand, the per-centage amount of chloride even then increases, up to the time the greatest amount is poured out, then it as steadily falls, even when the water fluctuates

greatly. These remarks apply to the per-centage of chloride of sodium in the urine.

It appears that the amount of urea undergoes an increase definite in amount, independent of the water. The chloride of sodium also undergoes a definite increase, which, also, is independent of the amount of water; but the water being increased, the same per-centage of chloride is poured out as would have been the case if a smaller amount of water had been voided, the per-centage not being lowered by an excess of water, as is the case with urea.

The chloride has thus a tendency to rise and fall steadily, not observing the various alterations corresponding to temperature that the urea does, but the water, corresponding to the urea in this respect, causes variations of the same character in the total amount of chloride poured out.

The time of greatest per-centage excretion of chloride of sodium does not always correspond to the greatest hourly excretion.

Thus, on April 6th, the greatest amount of saturation was at 8 a.m., whilst the greatest hourly excretion was at 8·30, when the water was at its greatest amount.

7th.—The hourly excretion and the per-centage amount agreed in the time at which they occurred, and so also did the water.

8th.—The same occurred on this day.

9th and 10th.—The same occurred also on these days.

11th.—The per-centage amount of chloride was greatest at 9 a.m., whilst the hourly amount excreted was greatest at 10, the water being most abundant during the last period.

The urea, on the other hand, often decreased in per-centage during the fit, especially if the increase in the water was great, its highest per-centage amount corresponding to the lesser amount of water. Thus, on April 6th, the per-centage amount decreased through the entire fit. The decrease was, however, much more gradual at its commencement.

7th.—The greatest per-centage amount was at 7 a.m., the amount of water being 25 c.c. for the hour. The water then rose to 80 c.c., and the per-centage amount of urea fell; the amount of mercury solution required for 10 c.c. of urine fell from 29 c.c. to 19 c.c. The water then again rose to 136 c.c., and the solution of mercury required fell in amount to 18 c.c. The water next fell to 62 c.c., and the solution of mercury rose to 22·6 c.c. The water again fell to 33 c.c., and the amount of the solution required rose to 27·4 c.c. The water next rose to 50 c.c., and the solution required fell to 26·4 c.c. The water then remained at 50 c.c., but the solution fell to 24 c.c. Then the water fell to 28 c.c., and the quantity of the solution rose to 25 c.c.[1]

From these facts it is evident that both the urea and chloride increase, independent of the influence of the amount of water; that any increase in the latter does not modify the aggregate amount of urea, but that it does that of the chloride.

No connexion exists between the quantity of urine passed during the entire fit and the amount of water drunk.

This is seen in the following table:[2]

Date.	Water drunk.	Urine passed.
6th	2135 c.c.	648 c.c.
7th	1335 „	550 „
8th	1050 „	428 „
9th	500 „	499 „

The urine was passed much more equally as regards the different periods than the water drunk, for the entire quantity was mostly drunk during the hot stage only.

Thus, the urine, urea, and chloride of sodium, were independent, to a large extent, of the quantity drunk,

[1] Every c.c. of the mercury solution corresponds to 10 milligrammes of urea.

[2] As usual, the date is given in the first column; in the second, the quantity of water drunk during the entire fit, and in the third, the amount of urine passed during the entire fit, are given.

though some connexion appeared to exist, as, when the patient drank copiously, the next time the urine was collected it was found to be somewhat in excess; but this might be a mere coincidence.

Table.

April 12.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Before fit	6·15 to 7·15	1·868	1·868	1·630
	7·15 to 8·15	1·815	1·815	
	8·15 to 8·45	0·604	1·209	
Cold	8·45 to 9·45	0·530	0·530	2·310
	9·45 to 10	0·445	1·780	
Hot	10 to 11	2·287	2·287	2·219
	11 to 11·30	1·075	2·150	
Sweating	11·30 to 12·30	0·756	0·756	0·442
	12·30 to 3·30	0·384	0·128	
CHLORIDE OF SODIUM.				
Before fit	6·15 to 7·15	0·153	0·153	0·153
	7·15 to 8·15	0·225	0·225	0·225
	8·15 to 8·45	0·540	1·080	1·080
Cold	8·45 to 9·45	0·240	0·240	0·321
	9·45 to 10	0·100	0·403	
Hot	10 to 11	0·625	0·625	0·467
	11 to 11·30	0·154	0·309	
	11·30 to 12·30	0·090	0·090	0·173
	12·30 to 3·30	0·708	0·256	
QUANTITY OF URINE PASSED.				
Before fit	6 to 7·15	73 c.c.		
	7·15 to 8·15	75 c.c.		
	8·15 to 8·45	54 c.c.		
Cold	8·45 to 9·45	25 c.c.		
	9·45 to 10	84 c.c.		
Hot	10 to 11	125 c.c.		
	11 to 11·30	86 c.c.		
	11·30 to 12·30	32 c.c.		
	12·30 to 3·30	128 c.c.		

On the 12th, everything being conducted in the same manner, upon the temperature commencing to rise, the patient was given ℈j of quinine, which caused the temperature to fall again. It continued to fall for half an hour, and then began to rise; the cold stage came on an hour later than on the previous day. The fit, however, was afterwards as severe as usual. The effects on the urea corresponding to this fall cannot be ascertained, as, for some reason, the amount secreted was three times as great as on previous days, for the first two hours it was collected; it then fell somewhat, but continued high throughout the fit, the quantity to each degree being 0·980 grammes.

The temperature obeyed all the rules laid down previously—rising to the commencement of the cold stage; then remaining permanent for half an hour; then rising through the whole of the cold stage, the latter being shorter than the former; and just at the commencement of the hot stage, it again remained stationary for a quarter of an hour; then rose, but began to fall before the sweating stage commenced, and at that point oscillated, the oscillation being small; it then fell rapidly.

The urea corresponded also to these variations, after the manner described. Thus, it fell where the temperature remained stationary, but the fall was out of proportion to the alteration in the temperature, being very great. Possibly, this was due to the quinine. It then rose rapidly, reaching its highest point at the same time as the temperature. It fell slowly during the latter part of the hot stage, then fell greatly during the oscillation at the commencement of the sweating stage; but as the urea did not subsequently rise, the rise in the oscillation of the temperature was only one fifth; then the urea fell greatly.

The chloride of sodium differed somewhat from the urea at the commencement and termination. Thus, though an unusual quantity was passed in the first two hours, like the urea, still it did not fall when the temperature began to rise a second time, but continued rising throughout; just before the cold stage it reached its highest point, arriving at the very unusual quantity of 1·080 grm.

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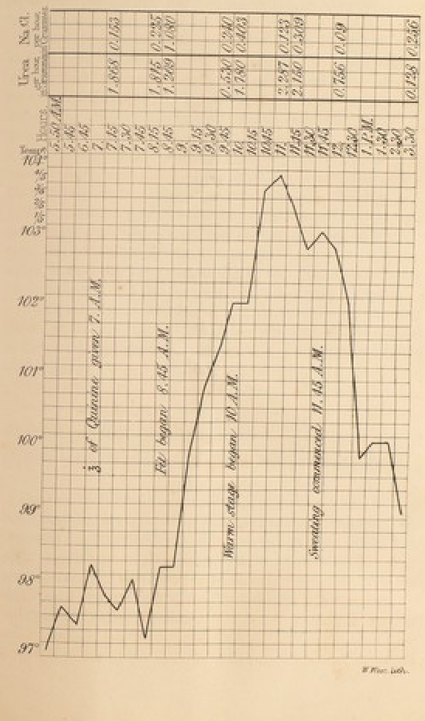
No. 7.

APRIL 12TH

Phenomena of a Fit modified by Quinine.



W. West, lith.

After this it corresponded in its variations to the urea, except in the second hour of the sweating stage, when, instead of continuing to fall, it rose considerably. The rise in the hour subsequent to the oscillation in the temperature was very extensive.

Considering the fit to have commenced on this day, at 7·15 a.m., the amount for each degree was 0·396, a larger quantity (as is also the case with the urea) than occurred on other days; but as the amount passed for some hours before the fit was unusually large, it is probable that the quantity normal for that day was in excess.

The water in its variations corresponded to the urea, except at its very termination (that is to say, the second hour of the sweating stage), when it rose considerably.

The amount for each degree was 105 c.c. It is rendered most highly probable that the reason for the amount for each degree being greater than usual in urea, salt, and water, is, that the normal quantity was in excess on this day, for it is found that the increase in each is proportionate.

The quantity he drank was not taken down on this day. Thus the only influence that can be ascribed to the quinine is the lowering of the temperature and the postponement of the fit.

Examination of the Urine on April 13th, ℈ij of Quinine having been taken the day before, the fit being entirely absent.

On the 13th no fit occurred. The patient took, without leave, another scruple of quinine the evening before. The temperature remained at 97° the entire day, with very slight variations, never amounting to more than $\frac{1}{5}$°. But, notwithstanding this equality of temperature, the urea and chloride of sodium underwent an increase. The urine was collected through the night.

UREA. CHLORIDE OF SODIUM.

Hour.	Quantity per hour.	Hour.	Quantity per hour.
	Grammes.		Grammes.
3·30 p.m. to 6·30 a.m.	0·610	3·30 p.m. to 6·30 a.m.	0·057
6·30 to 7·30 a.m.	1·340	6·30 to 7·30 a.m.	0·060
7·30 to 8·30 a.m.	1·122	7·30 to 8·30 a.m.	0·082
8·30 to 9·30 a.m.	1·150	8·30 to 9·30 a.m.	0·096
9·30 to 10·30 a.m.	1·412	9·30 to 10·30 a.m.	0·138
10·30 to 11·30 a.m.	0·803	10·30 to 11·30 a.m.	0·086
11·30 to 12·30 p.m.	0·999	11·30 to 12·30 p.m.	0·091
12·30 to 1·30 p.m.	0·606	12·30 to 1·50 p.m.	0·027

Thus, though the patient experienced no change as regards his sensations, and his temperature remained the same throughout the day, *a similar change to that occurring on the previous days was observed in the urea and chloride of sodium*. The fit, as judged by the urine, commenced at 6·30 a.m. The urea then fell somewhat during the next hour, rising again steadily up to 10·30 a.m., when it had reached its highest point. It then fell greatly, and this was followed by a slight rise, and then again fell to the amount normal to this man. The fall, immediately after the first rise, probably corresponded to the termination of the period prior to the cold or the commencement of the cold stage; and had the temperature risen, it would probably at this point have either oscillated or remained permanent for some time.

The next great fall, at 11·30 a.m., corresponded to the sweating stage, and would have been marked in the temperature by a great fall, and then, probably, the temperature would have remained stationary for half an hour, and not have risen, as the subsequent rise in the urea is small in amount. The urea after this small rise again falls to the normal amount.

The chloride also strictly followed the principles laid down, for, as has been shown, it has a tendency to rise continuously, an equal amount of urine being taken each hour, though, if the urine be much altered in

quantity, this salt also falls and rises. Thus, the chloride rises continuously, and is marked by no fall after the first hour, but, corresponding to the great fall of urea at 11·30, it also falls and again rises slightly in the next hour, and then again falls to the amount normal.

The urea and chloride increase *pari passu*, and each reaches its height at the same hour.

The amount of urine, again, corresponded to the urea and chloride of sodium; it fell slightly after the first hour, then rose, reaching its highest point at the same time as the urea; it then fell, but again rose somewhat, and then permanently fell.

3·30 p.m. to 6·30 a.m.	40 c.c.
6·30 a.m. to 7·30 "	50 "
7·30 " 8·30 "	46 "
8·30 " 9·30 "	46 "
9·30 " 10·30 "	66 "
10·30 " 11·30 "	41 "
11·30 " 12·30 p.m.	51 "
12·30 p.m. to 1·30 "	30 "

Taking the mean quantity of grammes corresponding to a single degree on other days, the increase of urea would correspond to a rise of $5\frac{1}{4}°$ in temperature. Thus, as far as the urea, &c., are concerned, the fit was as severe as on previous days.

The urine was again collected hourly during the 14th.

Here the urea and chloride of sodium were again in great excess during the whole day, and also during the night previous.

Hour.	Urea per hour.	Chloride per hour.	Water per hour.
1·30 p.m. to 6 a.m.	1·620	0·168	85 c.c.
6 to 7 a.m.	2·715	0·585	150 c.c.
7 to 8 a.m.	1·332	0·405	90 c.c.
8 to 9 a.m.	2·545	0·877	172 c.c.
9 to 10·30 a.m.	1·106	0·320	67 c.c.
10·30 to 11·30 a.m.	1·107	0·421	78 c.c.
11·30 a.m. to 1·30 p.m.	1·118	0·312	60 c.c.
1·30 to 3·30 p.m.	1·180	0·254	77 c.c.

On the second and fourth hours the urea rose to nearly double the quantity of other hours. After the second rise it remained stationary for two hours, and then rose. This corresponds to some extent with the urea the day before; but after the second rise the fall was not so great, and the subsequent rise was very small in amount.

The chloride more closely resembled the day before. Like the urea of the same day, it rose twice, but the last rise exceeded the first, and was followed by a considerable fall, and this again by a rise of some extent, and then it again fell. The second rise and fall corresponding to the commencement of the sweating stage, the first fall probably would have corresponded (had the temperature risen) to an oscillation or a permanency at the termination of the period prior to the cold stage.

The water closely corresponded to the chloride.

Detracting 1·000 gramme from each hour, that being the necessary quantity to reduce the urea in the night urine to the amount normal to this man, and again, as on the day before, calculating what height the temperature would have reached had it risen, it is found to be 3°.

This fit was therefore less severe than that on the previous day. It thus appears that *variations in the urea and chloride of sodium continue to occur at those periods when, if the fit had continued, the temperature would have risen.* The same fact has been noted by Redenbacher; and it would show that the cure of the fit by quinine, in this man, was followed by a much larger excretion of urea during what would have been the apyrectic hours than had been noted at the corresponding time when he was suffering from the disease.

Case 2.—*Tertian ague.*—The following case of ague occurred in a boy, æt. 19, strong, and in every respect healthy with exception of the attack of ague. The boy had not been out of London for twelve months before, but at that time came over from Dantzig, having been there for some months.

He was admitted into the hospital with the second fit, which was very slightly marked, the fit increasing subsequently in severity till he took four grains of quinine every two hours, which prevented its continuance. It was tertian in type.

Everything was conducted exactly after the manner of the former case. The urine, however, was collected (on one day only) by the stage, and not by the hour. His pulse was taken each quarter of an hour, with the temperature.

The two following charts of the temperature will be found to correspond closely with those given before, the correspondence holding mostly with the severer fits. In the first, the temperature rises slowly up to the cold stage; it then rises rapidly, running up continuously to the commencement of the hot stage, at which point it remains stationary for half an hour; it then runs up to its highest point, remains stationary for a short time, and sinks till the sweating stage commences, when, instead of oscillating, it remains stationary for half an hour, then falls and oscillates, but remains high even at 9 p.m., thus differing from the former and subsequent tables.

This further illustrates the slow rise of the stage previous to the cold one; then its rapid rise, the rise being here also greater than at any other period; also its tendency to be affected first at either termination as the fit becomes less severe. It shows that the temperature falls before the commencement of the sweating stage, and that at this point it either oscillates or remains stationary; and in the severe fit, that the temperature has a tendency to remain stationary at its highest point.

The next chart (that of April 4th) shows the temperature on the day the fit was severer, and consequently we find indications of this in the character and fall of the temperature. It shows at the commencement of the cold stage a tendency to be stationary, but after this it runs up continuously with no permanent period at the termination of the cold stage, though an approximation is seen in the rise at this point being less than the previous ones. It then remains permanent for some time, and then very slowly sinks, remaining permanent for half an hour just where the sweating commences; the temperature here also falling before the sweating stage commenced. Thus, the continuous rise, the permanency when at its height, and the slow and gradual fall, all indicate the severity of the fit, besides the great height to which the temperature reaches.

The urea, here only estimated for each stage, corresponds to the results before stated. Taking the average amount per hour in the sweating stage as the nearest approach to the quantity normal to the boy, it is found that there is an increase in the period previous to the fit; this continues increasing, is highest during the cold stage, then falls somewhat in the hot, and again falls still lower in the sweating.[1]

The same is exactly the case with the chloride of sodium and water.[2]

The patient drank—

At breakfast....	285 c.c.
Before cold stage	200 „
Cold stage...............	200 „
Hot stage...	700 „
Sweating stage	500 „

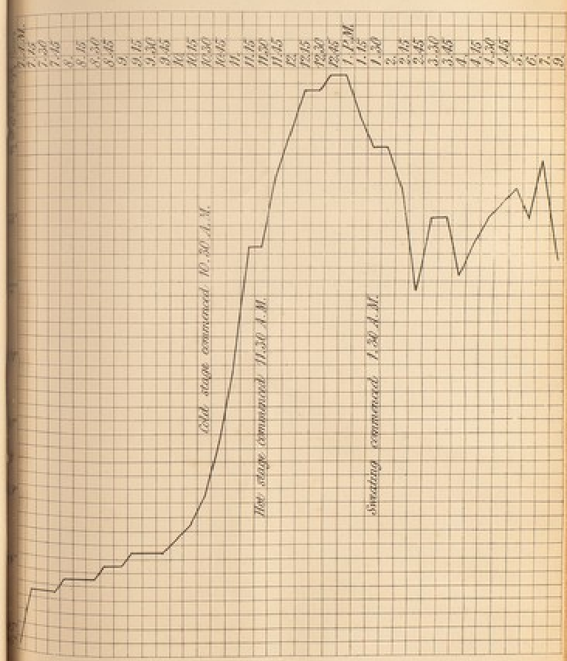
He passed the following quantity of urine:

Before fit (3 hours)........	162 c.c.	=	54 c.c. per hour.	
Cold stage (1 hour)........	100 „	=	100	„
Hot stage (2 hours)........	180 „	=	90	„
Sweating stage (4 hours) ..	150 „	=	37½	„

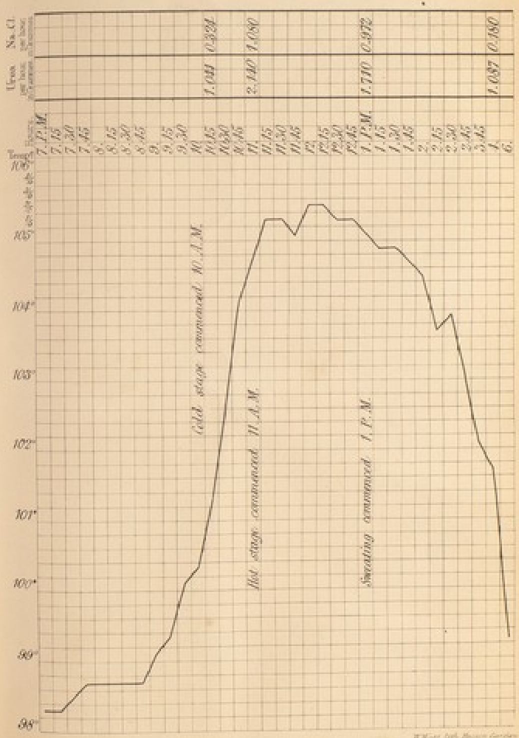
[1] The quantity of urea to each degree was 1·977 grm., showing a close correspondence to the amount for each degree in the former case. The correspondence would probably be closer still if the normal amount of this patient was reduced to that of the former.

[2] The amount of chloride of sodium corresponding to each degree was 0·502.

APRIL 2ND

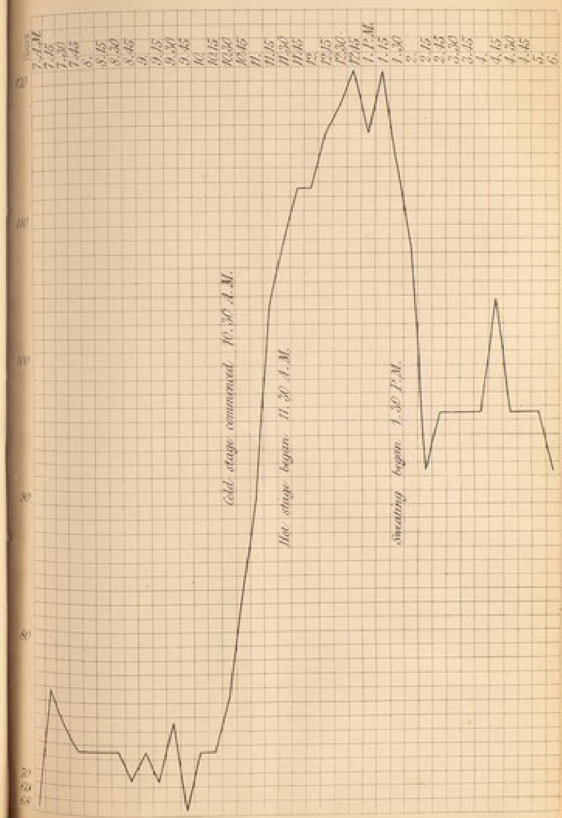


APRIL 4TH



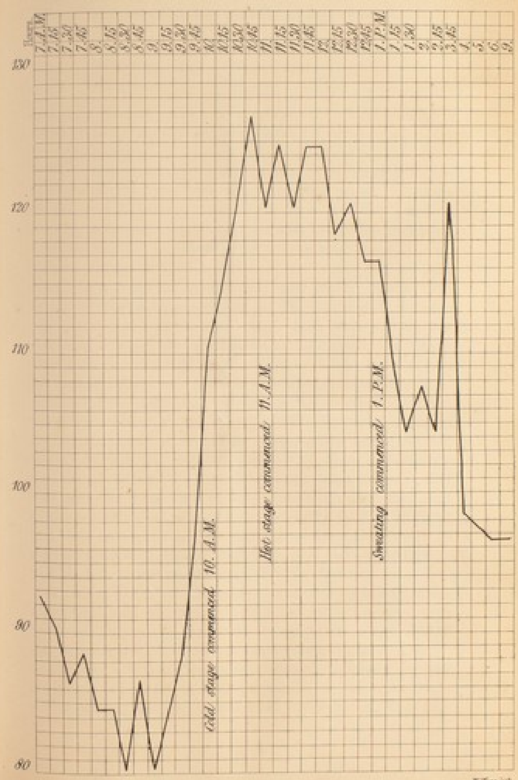
W. West lith. Hatton Garden.

Pulse.


Hours
Cold stage commenced 10.30 A.M.
Hot stage began 11.30 A.M.
Sweating began 1.30 P.M.
120
110
100
90
80
70

APRIL 4TH

Pulse.



Thus, during the period before the cold stage he drank 323 c.c. in excess over the quantity of urine passed.

Cold stage,	he drank	100	c.c.	more than he passed;
Hot stage,	,,	520	,,	,,
Sweating,	,,	350	,,	,,

making a total of 1293 c.c. in excess of the total amount of urine voided. In this respect, again, he corresponds to the previous patient.

The pulse was taken every quarter of an hour with the temperature in the two fits, and the result is shown in the two following charts. In the first the pulse at the very commencement falls somewhat, and then oscillates. Just before the commencement of the cold stage it commences rising, and then remains stationary for half an hour; then runs up enormously and rapidly during the whole of the cold stage, and at its termination remains again stationary for half an hour; then runs up slightly, oscillates when it has reached its highest point, and immediately on the sweating stage setting in it falls rapidly. In the second chart, at the very commencement, the temperature falls more than it did in the previous one; in the stage previous to the cold it rises more rapidly than in the previous one; runs up still more rapidly during the cold stage, reaches its highest point, and then immediately oscillating, before the sweating stage commences; after oscillating for some time, it falls, and at the commencement of the sweating stage it stands stationary for half an hour, and then falls greatly. During the entire rise there is no tendency for it to remain stationary, still less to oscillate.

This last fit was more severe than the former.

Thus, on both days the pulse commences by falling somewhat, then on both it rises before the fit commences, thus corresponding to the urine and its constituents and also to the temperature. At the commencement of the cold stage, in the less severe fit, the pulse remained stationary for half an hour, and again at the termination of the same stage, whilst nothing of this is seen in the severer one, thus corresponding to the temperature, &c. Again, on reaching the climax on both days the pulse oscillates, and the temperature also was unsteady at the same time, though to a much less extent. The pulse commences falling before the sweating stage, and then falls rapidly, more so than the temperature. The pulse, however, fell most rapidly in the less severe case.

Thus, a close correspondence exists between the pulse and the temperature. Like it, the character of the rise varies with the intensity of the fit; it rises before the cold stage commences; then, if the fit is not very severe, remains stationary; again rises through the entire cold stage, the rise being more considerable than during any other period; then falls before the sweating stage, and just at the commencement of this stage stands still for half an hour in the severer cases, and falls subsequently slower than in the less severe cases, in which also there is no stationary period.

CASE 3.—*A case of hectic fever.*—The following well-marked case of hectic is subjoined, on account of its close correspondence to the cases above given.

It occurred in a phthisical patient, æt. 45, under Dr. Walshe. It was particularly obstinate in resisting all measures for its removal, and was also peculiar in its occurring always during the day, and its very long continuance, as it commenced at 7 a.m., and continued to 5 p.m. All the stages were unusually well marked, consisting in severe rigors, accompanied with great pain about the loins and legs, the rigors coming on gradually, the sensation of cold being first observed about the feet; the hot and especially the sweating stages followed regularly. The observations were conducted in all respects after the manner of those already detailed.

Temperature.

Here, as in the other cases, the temperature began to rise before the rigors set in, ran up slowly through the

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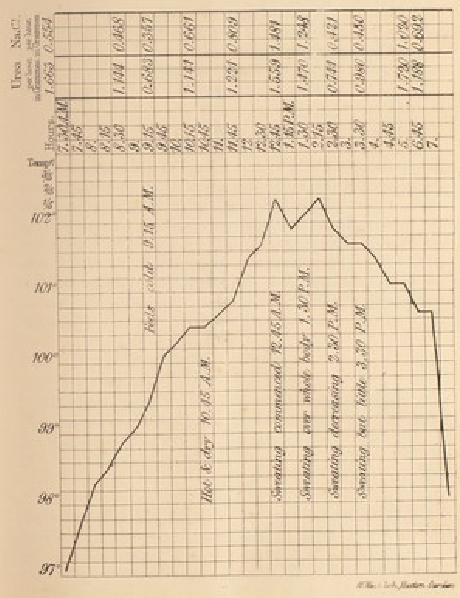
Case of Hectic.



whole of the cold stage, at its termination remained stationary for half an hour, then again ran up slowly, remaining stationary once, till it reached its highest point, when it oscillated, the rise in the oscillation occupying half an hour, then slowly and gradually fell, every now and then remaining stationary. A close connexion exists between this temperature and those given formerly; the rise, however, before the shivering set in was peculiarly great, and the cold stage very short, the rise in this stage not being so much in excess over the other stages as in the cases of ague.

Urea.

Unlike the cases of ague, the urea falls during the period preceding the rigors, and reaches its minimum immediately they set in; the quantity then rises rapidly, and becomes greatest during the hour immediately preceding the sweating stage. The quantity then falls rapidly through the sweating stage, reaching its minimum when the sweating is most severe; then rises again even higher than it was during the hot stage, and after that falls to its normal amount.

Chloride of sodium.

This corresponds to the urea in all respects, except that the rise at the termination of the fit does not equal the amount secreted during the hot stage.

Water

Again corresponds to both the above, except that its greatest quantity was poured out an hour later than the greatest amount of urea and chloride of sodium (showing the independence of these substances one of the other).

The patient was permitted to drink *ad libitum;* the quantity, however, was not measured.

The following table shows the variations in the amount of urinary water:

Hour.	Amount of Water.
7·30 a.m.	93 c.c.
8·30 ,,	52 ,,
9·15 ,,	35 ,,
10·15 ,,	58 ,,
11·15 ,,	71 ,,
12·30 p.m.	97 ,,
1·30 ,,	104 ,,
2·30 ,,	39 ,,
3·30 ,,	50 ,,
5 ,,	100 ,,
6·45 ,,	70 ,,

The pulse in this patient remained about 80 throughout the day.

General Conclusions.

The following are the conclusions deducible from the facts noted in the cases of ague.

Temperature.

1. This rises before the commencement of the subjective fit.
2. The time before the cold stage at which the rise commences varies.
3. It continues to rise during the entire cold stage.
4. The rise during this stage is greater than during any other.
5. It reaches its highest point during the hot stage, but falls again before the sweating stage, the fall being gradual.
6. The fall is more rapid during the sweating stage. The rapidity is in proportion to the slightness of the fit.
7. Definite variations occur in the rise and fall of the temperature indicative of alterations in the severity of the fit.
8. These variations first appear at the commencement and termination of the cold stage.
9. Their earliest indication is seen in a tendency for the later rises in the cold stage to become less extensive.

10. The temperature next becomes stationary for a variable time at either or both extremities of the cold stage.
11. Should the fit become still less severe, the temperature oscillates at these points.
12. An oscillation in the middle of the cold stage indicates a still further diminution in the severity of the fit.
13. Variations also occur during the hot stage.
14. In the severest fits there is a tendency for the temperature to remain stationary at its highest point.
15. If less severe, the temperature immediately falls on reaching its extreme height.
16. When the fits become still less severe the temperature next fails to reach to such a height as previously, but at this point again has a tendency to be stationary.
17. Should the severity of the fit be still less, it just touches the extreme point, and immediately falls.
18. Variations are also observed during the fall of the temperature.
19. Thus, at the junction of the hot and sweating stage, an oscillation, or a tendency to remain permanent, always occurs.
20. In continuing to fall, the temperature either falls gradually and continuously; this occurs in the severe fit—or,
21. Remains stationary every now and then; this indicates a somewhat less intensity of the fit—or,
22. It oscillates; this accompanies the most rapid fall, and occurs during the least severe fit.
23. After sinking to its extreme point, the temperature has a tendency to rise again, the rise being often considerable.

Urea, Chloride of Sodium, and Water.

24. The urea, chloride of sodium, and water, also begin to increase in quantity before the commencement of the cold stage.
25. They continue to rise rapidly, and become most

abundant either at the termination of the cold or the commencement of the hot stage.

26. These urinary constituents commence to fall in amount before the temperature reaches its highest point.

27. During the latter part of the hot stage they decrease in amount slowly.

28. They fall rapidly during the sweating stage, the rapidity of the fall being proportionate to the slightness of the fit.

29. These constituents exhibit variations corresponding to the variations in the temperature.

30. Thus, when the temperature remains stationary at either end of the cold stage, these urinary constituents also remain stationary, or fall somewhat. When the temperature oscillates, the fall in these constituents is proportionately greater.

31. A fall occurs in these constituents corresponding to the oscillation in the temperature at the termination of the hot stage, the fall in their amount corresponding to the depth of the oscillation.

32. A greater diminution in the amount of these constituents is observed when an oscillation occurs, than when the temperature decreases gradually, or tends to remain stationary.

33. The increase in the urea and water is definite, the same amount of increase corresponding to a single degree each day.

34. A greater increase in these constituents corresponds to a single degree at a high than at a low temperature.

35. No connexion existed between the various rises of the urea and chloride of sodium when equal periods were compared together.

36. The urea appeared to be independent entirely of the influence of the amount of water.

37. The chloride of sodium also underwent a definite increase, but this increased per-centage remained the same whatever the amount of water was, so that the total amount of chloride was greatly under the influence of the urinary water.

38. The chloride rose steadily and constantly, and did not observe the variations corresponding to temperature which were seen in the urea, but the water varying with the urea and influencing the chloride, as seen above, caused similar variations in the hourly amount of the chloride.

39. The quantity of water drunk in no way influenced the total amount excreted.

40. The increase in the above constituents often commenced before a corresponding rise in the temperature occurred.

41. The variations in the temperature above described often followed similar variations in the urea, but never preceded them.

42. Quinine given in a single, but large dose, when the temperature commenced to rise, lowered the temperature and postponed the fit for an hour, but had no other effect on that fit, though it prevented its recurrence next day, another scruple having been taken.

43. The pulse corresponded closely with the temperature.

Ueber die
Einwirkung des kohlensauren Natrons
auf den Gehalt des Harns
an Harnsäure und freier Säure.

Inaugural-Dissertation,
welche
unter Zustimmung der hochlöblichen medicinischen Facultät zu Marburg
zur
Erlangung der Doctorwürde
in der
Medicin, Chirurgie und Geburtshülfe
einreicht
und am 23. December 1868 öffentlich vertheidigen wird

Ludwig Severin
aus Arolsen.

MARBURG.
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Seinem lieben Freunde

Dr. U. KREUSLER

in Bonn.

Ueber

die Einwirkung des kohlensauren Natrons

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Da die Untersuchungen über den Einfluss des kohlensauren Natrons auf den Stoffwechsel im Allgemeinen, sowie die Harnsecretion im Besonderen trotz ihrer nicht unbedeutenden Zahl noch zu keinem in jeder Hinsicht befriedigenden Resultate geführt haben, indem ein Versuchsobject, wie der thierische Organismus, welcher so vielen und keineswegs immer zu eruirenden Einwirkungen ausgesetzt ist, dieselben äusserst schwierig macht und zu schwankenden, oft sich widersprechenden Ergebnissen führt, so hielt ich es nicht für unwerth durch Erwählung dieses Gegenstandes zur Inaugural-Dissertation einen kleinen Beitrag zu jenen Untersuchungen zu liefern. Wenngleich die Resultate einer einzelnen Untersuchung an sich wenig genügend sind, so werden sie sich doch denen Anderer anreihen, und da ich dieselbe unter gewissenhaftester Berücksichtigung aller Cautelen an mir selbst angestellt habe, glaube ich wenigstens zu einem ziemlich sichern Ergebnisse gelangt zu sein, weil man sich selber ja am besten controlliren kann.

Indem ich nun einer hohen medicinischen Facultät diese Arbeit vorzulegen wage, kann ich nicht umhin, Herrn Geh. Rath Beneke, der mir mit grösster Bereitwilligkeit sein Laboratorium zur Verfügung stellte, die Sache mit Interesse verfolgte und derselben auf alle Weise förderlich war, meinen angelegentlichsten Dank abzustatten.

Als man bei den ausserordentlichen Fortschritten der modernen Physiologie auch angefangen hatte hinsichtlich der Arzneimittel die reine Empirie zu verlassen, ihre chemisch-physiologische Wirkung zu ergründen, um damit das Gebäude der rationellen Therapie eventuell zu fördern und zu stützen, suchte zuerst Parkes die Veränderungen ausfindig zu machen, welche nach Aufnahme von Alkali im Harne sich manifestiren.*)

Parkes nahm während einer Reihe von 11 Versuchstagen zwischen $1\frac{1}{2}$ u. 2 Drachm. Liq. Kali caustici und prüfte den Harn, namentlich in Beziehung auf seine Quantität, seinen Gehalt an festen Bestandtheilen und an Schwefelsäure. Während das Mittel nach den Mahlzeiten genommen die Wirkung eines Antacidum äusserte, ohne jene Bestandtheile zu modificiren, vermehrte seine Aufnahme im nüchternen Zustande in kurzer Zeit die Harnsecretion, und die ganze Menge des eingenommenen Kali fand sich im Harne wieder; derselbe enthielt ausserdem relativ viel Schwefelsäure, eine grosse Menge Extractivstoffe neben wenig Harnstoff und in zwei Untersuchungen keine Harnsäure. Parkes schliest daraus auf eine vermehrte Oxydation und eine von der Norm abweichende Metamorphose der stickstoffhaltigen Substanzen im Organismus, nach Liebig's Entdeckungen, dass der Zusatz eines Alkali zu denselben ihre Oxydation befördert. Daher die vermehrte Ausscheidung von Schwefelsäure, welche vom Schwefel der Albuminate abstamme. Wie aber deren Oxydation sonst alterirt werde, konnte Parkes nicht entscheiden, wahrscheinlich, meint er, hinge von den aus ihr hervorgehenden Producten die vermehrte Urinsecretion ab; dieselbe könne nicht der Bildung von Kali sulphuricum zugeschrieben werden.

Die Wirkung des kohlensauren Natrons auf die Gallensecretion bei'm Hunde untersuchte Nasse **) und kam zu dem Resultate, dass jene dadurch ausserordentlich beschränkt werde, am auffallendsten in den ersten Tagen der Versuchszeit. Je weniger Nahrung, um so stärker wirkte der Zusatz von Natron vermindernd. Eine Alteration in quantitativer Hinsicht fand aber auch ausserdem noch in der Absonderung

*) The brit. et foreign. med.-chirurg. review. No. XXI. Jan. 1853.

**) Archiv des Vereins für gemeinsame Arbeiten zur Förderung der wissenschaftl. Heilkunde von Vogel, Nasse und Benecke, Bd. IV, pag. 598.

statt; denn während der Hund vor der Natronaufnahme stets während des Tages mehr Galle secernirte, als in der Nacht, trat in Folge derselben das Umgekehrte ein. Mehr noch als die Galle überhaupt verminderten sich deren feste Bestandtheile und zwar vor allen die Gallensäuren und der Farbstoff, viel weniger Schleim und Salze, welche mithin im Verhältniss zu den erstern stiegen. Es ist somit der Schluss gestattet, dass das kohlensaure Natron wenigstens die Seite des Stoffwechsels retardirt, deren Product die Galle ist.

Eine umfassende Arbeit über die Wirkung des Natron carbonicum auf den Stoffwechsel überhaupt lieferte Münch*), welcher an fünf Individuen verschiedenen Alters und Geschlechts exacte Versuche anstellte. Nachdem er eine Reihe von Tagen hindurch ihre Diät geregelt und somit den Stoffwechsel auf eine gewisse sich gleichbleibende Norm gebracht, gab er Anfangs 3 Grm. Natr. carb. im Verlaufe des Morgens, jedesmal 1 Grm. p. d. Zunächst resultirte eine Verminderung der Ausgaben. Im Einklange damit stand eine Vermehrung des Körpergewichts, eine relative bei den Personen deren Gewicht während der Versuchsperiode vor der Natronaufnahme täglich abgenommen hatte. Diesem entsprach später eine Zunahme der Ausgaben mit Verminderung des Körpergewichts. Vorzugsweise beruhten diese Schwankungen auf Vermehrung und Verminderung der Harnsecretion und diese betraf hauptsächlich wieder den Wassergehalt des Urins, während die Ausscheidung des Harnstoffs, des Kochsalzes, der Schwefelsäure durchaus keine beachtenswerthe Veränderungen erlitt. Die der Phosphorsäure dagegen musste sich steigern, da das eingeführte kohlensaure Natron als phosphorsaures den Körper wieder verlässt. Die Harnsäure und freie Säure waren verringert, letztere bei einer Person bis zum Alkalischwerden des Urins. Die Harnquantität überhaupt war im Anfange bei einigen Personen vermindert, später bei allen vermehrt, theils schon noch während der Aufnahme von Natron, theils erst nach Aussetzung desselben. Die durch das Natron bewirkte Vermehrung der Wasserausscheidung durch die Nieren, nimmt Münch an, beruht nicht allein auf einer Compensation der vorhergehenden Verminderung der Wasserabgaben, sondern die Gewebe erleiden auch einen wirklichen Verlust an Wasser.

*) Archiv des Vereins für gemeins. Arbeiten von Vogel, Nasse und Beneke, Bd. VI, pag. 369.

6 Grm. Natr. carbon. pro die hatten im Allgemeinen dieselben Erfolge als 3 Grm., doch traten sie rascher und intensiver auf. Bei allen Personen sank das Körpergewicht sofort, welches Sinken nach Aussetzung des Mittels theilweise noch fortdauerte. Die Vermehrung der Ausgaben erstreckte sich vorzugsweise wieder auf die Harnsecretion. Indem aber die Steigerung derselben mit einer Verminderung der Ausgaben durch Haut und Lungen zusammenfiel, glaubt Münch annehmen zu müssen, besonders da trotz der hohen Lufttemperatur die Perspiration sehr unbedeutend war, durch Natr. carbon. werde selbst bei verstärkter Diurese mehr Wasser im Körper zurückgehalten als unter gewöhnlichen Bedingungen. Die Harnsäure sank beträchlich und die freie Säure verschwand gänzlich um nach dem Aussetzen des Mittels zuweilen über das in den Normaltagen gewöhnliche Maass zu steigen.

Die Maximalquantitäten des Harns traten immer erst gegen Ende und nach Einstellung der Natroneinnahme auf, wie auch von den Harnmengen innerhalb 24 Stunden jedesmal diejenigen am grössten waren die am längsten nach der Natroneinnahme gelassen wurden, was ebenfalls für die Kraft des Natrons spricht, Wasser im Körper zurückzuhalten. Mit der Ursache verschwindet die Wirkung, das Wasser verlässt den Körper.

Bei 9 Grm. pro die traten einige Störungen ein, wie Diarrhoe bei einem Individum, während sonst auch von Seiten des Darmcanals die Ausgabeu herabgesetzt waren; doch sind die Resultate wesentlich im erhöhten Maasse dieselben, wie bei den vorhergehenden Dosen.

Indem also die Verminderung der Ausgaben in Folge des kohlensauren Natrons den Harn, die Perspiration und Defäcation, bei grössern Dosen vorzugsweise die letztern betraf; indem die in der Folge eintretenden und die Verminderung mehr als compensirenden Mehrausgaben nur auf einer Vermehrung der Harnsecretion und vorzugsweise auf einer vermehrten Wasserausscheidung von Seiten der Nieren beruhte, so glaubt Münch das Mittel als ein Diureticum bezeichnen zu können.

Was von Münch nicht besonders hervorgehoben wird, nämlich dass Natr. carbon. die freie Säure des Harns vermehrt beobachtete früher schon Beneke *) im deutschen Hospitale

*) Archiv für gemeins. Arbeiten von Vogel, Nasse und Beneke. Bd. I. pag. 445.

zu London an einem scrophulösen Individuum welches obiges Mittel als Medicament erhielt. Beneke neutralisirte 30 cc Harn durch Zusatz von Natron carbonicum von $^{1}/_{12}$ zu $^{1}/_{12}$ Gran und während nun vor der Einnahme des Mittels jene Quantität $^{20}/_{12}$ bis $^{21}/_{12}$ Gran erforderte, während der Urin bei dem Gebrauche desselben alkalisch wurde und durch verdünnte Schwefelsäure, welche genau obiger Natronlösung entsprach, neutralisirt werden musste, so stieg unmittelbar nach der Aussetzung des kohlensauren Natrons der Säuregrad auf eine Höhe wie sie selten beobachtet wird, der Art dass $^{40}/_{12}$ bis $^{80}/_{12}$, einmal $^{94}/_{12}$ Gran nöthig waren um 30 cc zu neutralisiren. Diese Erscheinung trat auch bei mir ein, wenngleich weniger evident, da ich nur kleine Dosen des Mittels nahm, so dass der Harn selten alkalisch wurde. Bei Dosen von 2 Grm. pro die trat sogar schon während des Natrongebrauchs eine Steigerung der freien Säure ein, wenn auch nicht sofort in den ersten Tagen.

Es scheinen überhaupt die Wirkungen dieses Mittels erst nach längerer Anwendung zur Beobachtung zu kommen, während z. B. die des Chlornatrium auf vermehrte Harnstoffausscheidung sofort in Erscheinung treten, wie Beneke in seinem Werke über die Soolthermen Nauheims constatirte. *) Indem dort von drei Versuchspersonen an Normaltagen 31,59 35,15 und 34,31 Grm. Harnstoff im Durchschnitt ausgeschieden wurden, stieg derselbe in Folge des Genusses von 600 cc deshalb verdünnten Curbrunnens, welche 73,016 Gran NaCl enthielten, sogleich auf 35,56 39,68 und 39,37 Grm. im Durchschnitt.

Wenn Seegen **) eine ähnliche Wirkung auch dem kohlensauren Natron zuschreibt, indem er behauptet dass durch dasselbe die Ausscheidung der stickstoffhaltigen Umsatzproducte durch die Nieren in Form von Harnstoff wesentlich gesteigert würde, so habe ich dies bei mir nicht bis zur Evidenz wahrnehmen können. Man sollte es allerdings erwarten, da die Vermehrung der freien Säure auf eine gesteigerte Oxydation überhaupt hindeutet.

*) cf. die tabellarische Zusammenstellung daselbst.

**) Ueber die Ausscheidung des Stickstoffs der im Körper zersetzten Albuminate; Sitzungsberichte der k. k. Akademie der Wissenschaften, Bd. LV.

In seinen chemischen Briefen stellte Liebig den Satz auf, dass durch die Gegenwart der Alkalien im Organismus die Oxydation vermittelt und gesteigert werde. Durch die Gegenwart des Alkali wird nach ihm erst die Wirkung des Sauerstoffs bedingt. Dass der Genuss von Früchten, oder wie von Wöhler nachgewiesen, dass die in jenen enthaltenen pflanzensauren Salze für sich genommen den Harn alkalisch machen, indem sie als kohlensaures Kali darin erscheinen, scheint demnach plausibel. Werden aber die Säuren rein, ohne an Basen gebunden zu sein, dem Organismus einverleibt, so verlassen sie denselben nach Liebig unverändert und erscheinen im Harn wieder. Dies ist jedoch nur beim Fleischfresser der Fall. Wenn die Pflanzen fressenden Thiere mit ihrem Futter stets eine grosse Menge freier Pflanzensäuren verzehren, ohne dass diese als solche in ihrem Urine auftreten, so erklärt dies Liebig aus dem Gehalt ihres Blutes an kohlensaurem Alkali: dieses wird zersetzt, die Pflanzensäuren verbinden sich mit den Alkalien zu neutralen Salzen, die Kohlensäure wird exspirirt. Das Blut der Fleischfresser enthält kein kohlensaures, sondern phosphorsaures Alkali und während nun die pflanzensauren Salze auf dieses ohne Einfluss bleiben, verbinden sich die Säuren mit einem Theile des an Phosphorsäure gebundenen Kali, wodurch Phosphorsäure frei wird und, da sie als nicht gasförmig den Organismus nicht so rasch verlassen kann, die Alkalescenz des Blutes herabsetzt. Mit dieser verschwindet aber auch die Fähigkeit des Blutes jene Säuren zu metamorphosiren; daher das Auftreten derselben im Harn.

Mit dieser Theorie stehn indessen die Ergebnisse im Widerspruch welche Piotrowsky und Magawly bei ähnlichen Untersuchungen erhielten. *) Sie fanden bei'm Einnehmen von freien Pflanzensäuren diese nur zum geringen Theile (Oxalsäure Weinsäure, mochte die Menge gross oder klein sein) und meist an Kalk gebunden, oder aber Nichts davon im Harn wieder (Citronensäure) und dasselbe Resultat stellte sich nach Aufnahme der sauren, neutralen und basischen Salze heraus. Demnach würden die Pflanzensäuren in das Blut übergeführt, allmählich oxydirt, wie Piotrowsky meint, und schliesslich

*) Piotrowsky, De quorundam acidorum organicorum in organismo humano mutationibus. Magawly, De ratione qua nonnulli sales organici in tractu intestinali mutantur. Dorpat. 1856.

zum grössten Theile zu Kohlensäure und Wasser übergeführt. Dass sich Benzoesäure in Hippursäure verwandelt ist bekannt. Die Verbindung jener Säuren mit Alkalien bedingte durchaus keine Veränderung ihres Verhaltens: es fanden sich nur geringe Mengen von Oxal- und Weinsäure, keine Spur von Citronensäure im Harn. Demnach würden die Alkalien Nichts zur leichtern Oxydation jener Säuren beitragen, vielmehr jene verhindern, da die Verwandschaft der Alkalien zu den stärkern organischen Säuren deren Verwandlung in die schwächere Kohlensäure beeinträchtigen müsse.

Als ich meine Untersuchungen begann war ich zunächst darauf bedacht meine Lebensweise womöglich bis zur Congruenz der einzelnen Tage zu regeln, denn nur so können die Wirkungen eines bis dahin nicht vorhandenen Einflusses rein hervortreten. So weit es in meiner Macht stand gelang mir auch jene Regelung ziemlich vollständig, während äussere und zufällige Einwirkungen, wie die der Witterung u. a. jedesmal besonders berücksichtigt werden müssen.

Mein Alter ist beiläufig bemerkt 25 Jahre, Körperlänge 172,5 Cm., Gewicht bei der ersten Wägung 59176 Grm., Puls am Morgen 68, Mittags 62, Abends 78 in der Minute. Temperatur Morgens 36,7, Abends 36,3.

Was also zunächst die Nahrung anbelangt, so wurden folgende Einnahmen während der ganzen Untersuchungszeit festgehalten:

I. $8^1/_2$ Uhr Morgens:
- 30 Grm. Kakao: 300 Grm. Wasser *)
- 20 „ Zucker
- 120 „ Weissbrod
- 15 „ Butter.

II. 1 Uhr Mittags:
- 250 Grm. Rindfleisch in Form eines Beefsteak.
- 30 „ Butter
- 10 „ Salz
- 250 „ Kartoffel
- 60 „ Weissbrod
- 20 „ Käse. $^1/_2$ Stunde darauf 200 Grm. Wasser, desgl. im Laufe des Nachmittags.

*) Kaffee wurde seiner stark diuretischen Wirkung wegen ganz gemieden;

III. 8 Uhr Abends:

5	Grm.	Thee: 450 Grm. Wasser
45	„	Zucker
180	„	Weissbrod
25	„	Butter
50	„	Käse.

Summe der festen Bestandtheile: 1095 Grm., des Wassers 1150 Grm.

Die festen Nahrungsmittel wurden auf einer Balkenwage gewogen, das Wasser in einem graduirten Glase bestimmt. — Die sonstige Lebensweise war folgende: bis $9^1/_2$ Uhr, später als mir die chemischen Arbeiten rascher von der Hand gingen, bis 11 Uhr wissenschaftliche Beschäftigung, darauf bis Mittag chemische Arbeiten; nach dem Mittagsessen Zeitungslectüre, chemische Arbeiten bis 4 Uhr, Spaziergang, Unterhaltung oder Lectüre bis 6, endlich bis 11 Uhr Abends wissenschaftliche Beschäftigung oder oft Correspondenz. Auch das Abwägen der Nahrungsmittel nahm zwischendurch etwas Zeit in Anspruch. — Die Körperwägungen fanden jedesmal um 11 Uhr 45 Minuten statt auf einer genauen Brückenwage von Pintus; der Körper war dabei unbekleidet. — Von den Ausgaben von Seiten des Darmcanals glaubte ich abstrahiren zu können, da die Defäcation sehr regelmäsig des Morgens gleich nach der ersten Urinentleerung eintrat. Was nun diese anbetrifft, so fanden deren in 24 Stunden 5 statt in regelmässigen Zwischenräumen und da ich nicht viele Fluida zu mir nahm, so war der Harn quantitativ gering und so concentrirt dass er sehr häufig, der Abend- und Nachturin bei kaltem Wetter fast regelmässig sedimentirte. Die Farbe variirte zwischen II.—III. und IV. der Vogel'schen Scala, so zwar dass der Morgens um $11^1/_2$ und Nachmittags um $3^1/_2$ Uhr gelassene Urin — III. der Abends um 7 Uhr — II.—III. um 11 Uhr — III.—IV. der Morgens um 7 Uhr gelassene der Farbe IV. entsprach. Wie sich bei der Harnsäurebestimmung zeigte, enthielt er wenig Farbstoff. Das specifische Gewicht bestimmte ich durch Aräometer und berechnete es auf die Durchschnitsquantität nach der von Beneke *) angegebenen Formel. Die sg. freie Säure wurde durch eine titrirte Lösung von Natron, von welcher 1

*) Archiv für gemeins. Arbeiten von Vogel, Nasse und Beneke, Bd. I. pag. 427.

cc 0,01 Grm. Oxalsäure entsprach, an jeder einzeln gelassenen Quantität bestimmt, so oft diese 100 cc überstieg, an 50 cc, so oft dieselbe nicht 100 cc erreichte, an 25 cc, und daraus auf die Gesammtmenge berechnet. *) Die Hälfte des gesammten 24 stündigen (filtrirten) Harns wurde jedesmal zur Harnstoff- und Harnsäurebestimmung reservirt, hinsichtlich deren ich mich ganz nach dem vortrefflichen Werke von Vogel und Neubauer richtete. Um die Harnsäure quantitativ zu bestimmen wurden 200 cc Harn mit 5 cc Salzsäure 48 Stunden lang einer kalten Temperatur ausgesetzt. Die ausgeschiedene Harnsäure wurde alsdann auf einem vorher bei 100° C. getrockneten und gewogenen Filtrum gesammelt und mit diesem unter den bekannten Canteln gewogen.

Für meine Pflicht halte ich es hier auf einen Fehler aufmerksam zu machen der bei der Harnsäurebestimmung täglich mit unterlief. Trotzdem nämlich dass man den Harn 48 Stunden mit Salzsäure in der Kälte stehn liess — die Lufttemperatur war bis auf wenige Tage gegen den Schluss der Untersuchungzeit hin durchgehends nahezu 0 Grad — wurde die Harnsäure doch nicht vollständig ausgefällt, es schied sich stets nachher im Filtrat noch eine relativ nicht unerhebliche Menge aus, die wohl auf 10 — 15 Milligramm pro die veranschlagt werden muss. Da dieser Fehler erst nach einigen Tagen bemerkt wurde, so konnte er nicht mehr ausgeglichen werden, da indess stets ein ganz gleichmässiges Verfahren beobachtet wurde, so konnte er vielleicht um so eher vernachlässigt werden, als es hier ja überhaupt weniger auf die absolute Menge der Harnbestandtheile ankam, als auf die Differenzen welche das kohlensaure Natron etwa nach sich zieht.

Was die Harnstoffbestimmung anbetrifft, so mischte ich den Harn mit einer gleichen Quantität Barytmischung **) und filtrirte. Natürlich musste ich dem Filtrat alsdann 20 cc entnehmen, um die Harnstoffmenge in 10 cc Harn zu bestimmen. ***)

Da die stärksten Natronquellen immerhin relativ wenig kohlensaures Natron enthalten †), so kam es mir hauptsächlich darauf an die Wirkungen kleiner Dosen zu prüfen und

*) cf. Neubauer und Vogel, Harnanalyse. §. 64.

**) 2 Th. „alpeters. 1 Th. caust Baryt.

***) cf. Neubauer und Vogel. pag. 144.

†) Vichy 37 Gran, Bilin 33 Gran auf 16 Unzen.

ich nahm daher Anfangs 2 Grm. (32 Gran) auf 2 Dosen, später 4 Grm. auf 4 Dosen vertheilt; wovon jedesmal die Hälfte nüchtern 1 Stunde vor dem Frühstück, die andere Hälfte zwischen 10 und 11 Uhr Vormittags genommen wurde. Um die Dosirung möglichst genau zu machen, mischte ich scharf abgewogene 50 Grm. mit 500 cc destilirten Wasser und entnahm dieser Mischung jedesmal 10 cc vermittels einer Pipette; diese wurden mit noch 90 cc Wasser getrunken; später als ich 4 Grm. nahm, mit nur noch 40 cc. Damit das Plus von Wasser nicht etwa einen beträchtlichen Einfluss ausübe, nahm ich 100 cc Wasser weniger zum Kakao, so dass im Verlaufe des Vormittags im Ganzen während der Natronanwendung nur 100 cc Wasser mehr getrunken wurden, was gewiss nicht schwer in die Wagschale fällt. Die Gesammtquantität des während der Natronaufnahme getrunkenen Wasser belief sich demnach auf 1250 cc.

Zunächst folgt also eine Reihe von Tagen in denen ich unter Einhaltung der oben angegebenen Lebensweise den Stoffwechsel auf eine feste Norm brachte und diese hinsichtlich der Harnsecretion bestimmte. In folgender Tabelle bedeutet jedesmal die erste der unter spec. Gewicht angeführten Zahlen das direct gefundene, die zweite das auf die Durchschnittsquantität berechnete specifische Gewicht des Harns.

I. Normaltage.

Datum. November.	Witterung im täglichen Mittel.	11½ Uhr Morgens.		3½ Uhr Nachm.		7 Uhr Abends.		11 Uhr Abends.		7 Uhr Morgens.		Gesammtmenge.	spec. Gewicht derselben.
		Quant. in cc.	spec. Gewicht.	Quant.	spec. Gewicht.	Quant.	spec. Gewicht.	Quant.	spec. Gewicht.	Quant.	spec. Gewicht.		
14	27″ 5,53 Bar. 2,4° C. N.W. bedeckt Mrgs. etw. Nebel. 95,5.	161	1024 1021	182	1025,5 1025	210	1027 1030,7	160	1026,5 1022,7	210	1031 1034	923	1027,5
15	27″ 7,15 Bar. -0,3 C. NO. tb. 83,6.	125	1026 1018	244	1021 1028	188	1028 1028,8	112	1029 1018	220	1031 1038	889	1027,5
16	27″ 5,64 Bar. -0,9°C. OSW. w. 93,0.	116	1027 1016	206	1024 1025	286	1025,5 1036	160	1029 1023,6	216	1028 1030	984	1027
17	27″ 6,42 Bar. 2,1 C. SW. tb.Nebel.97,2.	142	1025 1017,5	262	1021 1027	244	1026 1031	162	1029 1023	206	1030 1030,3	1016	1026,5
18	27″ 7,13 Bar. 3,2 C. SW. w.Neb. 87,1.	102	1026,5 1014	192	1023 1023	285	1025 1036	159	1029 1023,8	226	1028,5 1032,9	964	1027
19	27″ 7,44 Bar. 0,4 C. NO. cht. 75,3.	123	1024,5 1017	182	1023,5 1024,8	214	1030 1037	133	1029 1022	200	1029 1034	852	1027,5
20	27″ 7,69 Bar. - 2,9 C. NO. zht. 89,1.	106	1027 1017	162	1027 1026	202	1028 1033,5	163	1029,5 1028,5	195	1030 1034	848	1028

Quantität der freien Säure in Grm.

Datum.	11½ Uhr Vorm.	3½ Uhr Nachm.	7 Uhr Abends.	11 Uhr Abends.	7 Uhr Morgens	Gesammtmenge.
14. Nov.	0,177	0,400	0,168	0,272	0,819	1,836
15. „	0,205	0,439	0,283	0,336	0,871	2,134
16. „	0,266	0,391	0,088	0,320	0,842	1,907
17. „	0,198	0,272	0,099	0,362	1,015	1,946
18. „	0,199	0,284	0,123	0,359	0,723	1,688
19. „	0,184	0,211	0,258	0,324	0,720	1,697
20. „	0,199	0,220	0,109	0,309	0,760	1,597

Datum.	Harnquantität.	spec. Gewicht.	Dasselbe auf (933) den Durchschn. berechnet.	Harnstoff.	Harnsäure	Körpergewicht.
						Grm.
14. Nov.	923 cc.	1027,5		32,766	0,424	59176
15. „	899	1027,5		33,337	0,612	59080
16. „	984	1027	1028,5	36,904	0,334	59260
17. „	1016	1026,5	1028,8	37,084	0,320	59310
18. „	964	1027	1028	37,114	0,545	59170
19. „	852	1027,5	1026	36,636	0,527	59060
20. „	848	1028	1026,5	36,898	0,437	59071

Da die angenommene neue Lebensweise nicht wesentlich von meiner frühern abwich, nur hauptsächlich die frühere Ungebundenheit in der Nahrungsaufnahme in Bezug auf Zeit und Quantum auf ein strenges Maas gebracht wurde, so waren die in Folge dessen eintretenden Schwankungen einerseits nicht sehr erheblich, anderseits glichen sie sich bald aus. Für die Feststellung des Nochmalmaasses können jedoch nur die Tage von 16 — 20 Nov. in Betracht gezogen werden, schon deshalb, weil die Harnstoffmenge am 14. und 15. viel geringer war. Die täglichen Schwankungen der Harnmenge fielen in diesen Tagen zwischen 848 und 1016 cc; die durchschnittliche Tagesquantität betrug 933 cc. Da die Perspiration, soweit sie im Bereich meiner Beobachtung lag, sich immer gleich blieb, eine Transspiration wenigstens nicht statt fand, so haben jene

Schwankungen wol hauptsächlich in Schwankungen der insensibeln Perspiration, vielleicht in Folge der Witterung ihren Grund. In der That war an dem einzigen Tage an welchem die Harnquantität die Summe von 1000 cc überstieg ausserordentlich schönes Wetter und wenn bei Beneke's Untersuchungen *) sich herausstellte dass eine heitere Gemüthsstimmung die Urinsecertion steigert, eine deprimirte sie herabsetzt, so ist der aufheiternden Witterung jenes Tages vielleicht die vermehrte Harnmenge zuzuschreiben. — Was die Schwankungen der Harnquantität an den einzeln Tageszeiten betrifft, so waren die am Nachmittage gelassenen Mengen jedesmal am bedeutendsten, zeigten aber auch die grössten Differenzen, während z. B. die Nachturine eine auffallende Uebereinstimmung zeigten.

Hinsichtlich der Bedeutung des specifischen Gewichts verweise ich auf die oben angegebene Arbeit von Beneke. Die Bestimmung desselben ist nur unter steter Berücksichtigung der Harnmenge von Werth und während z. B. das direct gefundene spec. Gewicht einer 24stündigen Harnquantität zu dieser meist im nahezu umgekehrten Verhältnisse stand, änderte sich dies bei der Berechnung desselben auf die Durchschnittsquantität: mit der grössern Harnmenge wurde auch die grössere Menge fester Stoffe ausgeschieden.

Der Harnstoff stieg in den ersten Tagen in Folge der Mehraufnahme stickstoffhaltiger Substanzen, um alsdann sich ziemlich constant auf der Höhe von 36 — 37 Grm. pro die zu erhalten.

Die Harnsäure, in der sich bei der mikroskopischen Untersuchung auch einige Crystalle von oxalsaurem Kalk vorfanden, zeigte in den ersten Tagen bedeutende Schwankungen, für welche ich keine genügende Erklärung finden konnte; in den spätern Tagen betrug sie gemeinlich 0,35 — 0,5 Grm. für den Tag. Ein Gleiches betraf die freie Säure welche ebenfalls gerade an dem Tage an welchem die Menge der Harnsäure am bedeutendsten war ihren Höhepunkt erreichte, um später an den eigentlichen Normaltagen ungefähr zwischen 1,9 und 1,6 Grm. pro die zu schwanken. Wahrscheinlich manifestirte sich hauptsächlich in diesen beiden Beziehnungen der Einfluss der veränderten Lebensweise. — Dass der Säuregrad des

*) Archiv für gemeins. Arbeiten von Vogel, Nasse und Beneke, Bd. I. pag 419.

Nachturins stets am stärksten ist zeigte sich auch bei mir, nicht aber dass er nach den Malzeiten bis zum Alkalischwerden des Urins verschwinden soll; der Nachmittagsurin zeigte einen mittlern Säuregrad, der Abends 7 Uhr gelassene den geringsten.

Nachdem ich, jedoch unter Beibehaltung derselben Lebensweise einen Tag pausirt, folgen:

II. Sechs Tage mit 2 Grm. Natr. carbon. cryst.

pro die mit Ausschluss von 2 Tagen ohne dasselbe:

Datum Novbr.	Witterung im täglichen Mittel.	11½ Uhr Mittags. Quant.	11½ Uhr Mittags. spec. Gewicht.	3½ Uhr Nachm. Quant.	3½ Uhr Nachm. spec. Gewicht.	7 Uhr Abends. Quant.	7 Uhr Abends. spec. Gewicht.	11 Uhr Abends. Quant.	11 Uhr Abends. spec. Gewicht.	7 Uhr Morgens. Quant.	7 Uhr Morgens. spec. Gewicht.	Gesammtmenge.	spec. Gewicht derselben.
22	27'' 2,62 Bar. -0,1 C. S. wolk. 86,6.	114	1027 1015,5	250	1022 1028,5	228	1026 1028	199	1030 1030	205	1031,5 1032	1000 cc	1027
23	27'' 0,81 Bar. 3,1 C. S. trb. bed. 90,2	121	1028,5 1018,2	213	1025 1028	162	1029 1025	154	1031 1025	195	1030,5 1031	845 „	1028,5
24	27'' 3,48 Bar. 3,1 C. S. bedeckt. 94,5.	145	1026,5 1020	206	1025 1027	274	1027 1038	115	1033 1020	210	1030,5 1032	950 „	1027
25	27'' 3,51 Bar. 2,7 C. S. wlk. bed. 184,6.	118	1028 1017,5	200	1027 1029	264	1026 1037	175	1030 1028	180	1031 1030	937 „	1028
26	27'' 3,01 Bar. -0,3 C. S. wolk 88,2	109	1028,5 1015	205	1026 1026,7	287	1026 1037	186	1029 1027	215	1029,5 1031,5	1002 „	1027,5
27	27'' 3,35 Bar. -0,4. S. bed. 91,9.	102	1029 1017	181	1027,5 1029	212	1028 1033,5	154	1031 1028	194	1032 1036	843 „	1028,5
28	27'' 3,04 Bar. -1,3. N bed. Schnee 96,0.	88	1029,5 1015	170	1028 1028,5	227	1026 1033,5	130	1031 1027	220	1030 1038	835 „	1028
29	27'' 3,86 Bar. -1,0. NO. bed. 92,5.	110	1027 1016	170	1027 1025	278	1026 1039	150	1032 1026	211	1031 1036	908 „	1028

Quantität der freien Säure in Grm. bei 2 Grm. Natr. carb.

Datum.	11½ Uhr Vorm.	3½ Uhr Nachm.	7 Uhr Abends.	11 Uhr Abends.	7 Uhr Morgens.	Gesammtmenge.
22. Nov.	0,155	0,129	0,086	0,392	0,669	1,411
23. „	0,181	0,119	0,055	0,301	0,620	1,276
24. „	0,180	0,251	—	0,391	0,855	1,657
25. „	0,236	0,220	0,132	0,530	0,935	2,053
26. „	0,340	0,258	0,166	0,646	0,941	2,341
27. „	0,267	0,282	0,203	0,462	0,841	2,055
			Nach dem Natrongebrauch:			
28. „	0,219	0,332	0,081	0,429	0,976	2,037
29. „	0,156	0,176	0,215	0,480	0,950	1,977

Harnstoff, Harnsäure etc. bei 2 Grm. Natr. carb.

Datum.	Harnquantität in cc.	spec. Gewicht.	dsselbe auf 915 cc berechn	Harnstoff.	Harnsäure	Körpergewicht. Grm.
22. Nov.	1000	1027	1029.5	36,5	0,525	59348
23. „	845	1028,5	1025,5	36,067	0,397	59386
24. „	950	1027	1028	36,140	0,361	59525
25. „	937	1028	1028,5	36,074	0,430	59525
26. „	1002	1027,5	1030	36,072	0,505	59260
27. „	843	1028	1025	35,406	0,430	59270
			Nach dem Natrongebrauch:			
28. „	835	1028	1024,5	35,905	0,376	59240
29. „	908	1028	1027,5	37,682	0,635	59130

Obgleich während der Anwendung von Natron 100 Grm. Wasser täglich mehr eingenommen wurden, so zeigt die tägliche Harnquantität keine wesentliche Differenzen von den Normaltagen, die Durchschnittsquantität ist sogar noch um ein Weniges verringert. Dies würde demnach die von Münch auf Grund seiner Versuche aufgestellte Ansicht, nämlich dass das kohlensaure Natron im Stande sei, Wasser im Körper zurückzuhalten, bestätigen. Im Einklange damit steht die

Zunahme des Körpergewichts, welches auf eine Höhe stieg die es während der Normaltage nicht erreicht, hatte, um dann gegen das Ende der Versuchszeit und nach dem Aussetzen des Natrons wieder zu sinken. — Das specif. Gewicht weicht nicht besonders von dem frühern ab. Die Harnstoffmenge ist eher etwas verringert was mit der Seegen'schen Behauptung nicht übereinstimmt; erst nach dem Aussetzen des Mittels stieg sie wieder, während die sonstigen Differenzen sich nur auf Centigramme beliefen.

Die Menge der Harnsäure sank an den ersten Tagen der Natronaufnahme, um vom 4ten Tage an wieder zu steigen; doch erreichte sie die Höhe nicht auf der die während der Normaltage öfters gestanden, übertraf sie jedoch am zweiten Tage nachdem das Mittel ausgesetzt war. Auffallend war die dunkle Färbung derselben welche sofort nach der ersten Natroneinnahme eintrat und mit Einstellung derselben auch gleich wieder verschwand. In den Normaltagen war sie wenig gefärbt, von Ziegelmehlfarbe.

Die freie Säure sank anfangs nur unbedeutend, stieg vom dritten Tage an und übertraf selbst die Maximalquantität der Tage an welchen kein Natron genommen wurde. Da dieses auch zu seiner Neutralisation einer gewissen Menge Säure bedarf, so kann es nicht anders sein, als dass das Natron die Bildung derselben im Organismus steigerte. Die Quantität der Säure blieb auch noch nach Aussetzung des Natrons beträchtlich und sank erst wieder bei Anwendung von 4 Grm. desselben.

III. Sechs Tage mit 4 Grm. Natron carbon. pro die.

Datum.	Witterung im täglichen Mittel.	$11^1/_2$ Uhr Morgens.		$3^1/_2$ Uhr Nachm.		7 Uhr Abends.		11 Uhr Abends.		7 Uhr Morgens.		Gesammtmenge.	spec. Gewicht derselben.
		Quant. Gr.	spec. Gewicht.	Quant.	spec. Gewicht.	Quant.	spec. Gewicht.	Quant.	spec. Gewicht.	Quant.	spec. Gewicht.		
30Nov.	27" 4,97 Bar. -0,3C. O. bedeckt. 88,2.	110	1026 1016	179	1025 1025	247	1027 1037	171	1031 1030	191	1031 1032	898	1028
1 Dec.	27" 4,17 Bar. -0,6 C. SO.bed.96,7. Feucht.	120	1027,5 1017,6	213	1025 1028	235	1027 1033,7	178	1028,5 1027	188	1031 1031	934	1028
2	27" 3,91 Bar. -0,2 C. ?. bed. Nebel. 99,3.	150	1027,5 1021,5	205	1024 1026	221	1027,5 1032	186	1031 1030,5	185	1031 1030,5	947	1028
3	27" 3,43 Bar. 2,1 C. S. tb.Reg.Neb.98,6.	140	1026,5 1020	237	1023,5 1030	212	1028,6 1031,6	166	1030 1027	171	1030 1027,5	920	1028
4	27" 3,45 Bar. 4,8 C. S. tb. Neb. 99,4.	122	1027,5 1017,5	221	1024 1027,5	276	1026,5 1038	150	1031 1024,5	181	1030,5 1029	950	1028
5	27" 1,51 Bar. 8,4 C. SSW tb.Reg. 93,8.	132	1027 1022	230	1023 1029	201	1028,5 1031,7	135	1031 1022,7	205	1030 1034	903	1028

Quantität der freien Säure bei 4 Grm. Natr. carb.

Datum.	11½ Uhr Vorm.	3½ Uhr Nachm.	7 Uhr Abends.	11 Uhr Abends.	7 Uhr Morgens.	Gesammtmenge.
30. Nov.	0,187	0,150	—	0,471	0,775	1,583
1. Dec.	0,201	0,102	0,117	0,373	0,830	2,623
2. „	0,210	- 0,246	- 0,130	0,297	0,440	1,671
3. „	0,173	0,075	- 0,106	0,279	1,617	1,038
4. „	0,195	0,150	—	0,327	0,886	1,578
5. „	0,230	0,151	0,116	0,353	0,791	1,641

Datum.	Harnquantität	spec. Gewicht.	Dasselbe auf (933) den Durchschn. berechnet.	Harnstoff.	Harnsäure	Körpergewicht.
						Grm.
30. Nov.	898	1028	1026	35,920	0,512	59140
1. Dec.	934	1028	1028,3	37,360	0,406	59380
2. „	947	1028	1028,5	37,356	0,467	59510
3. „	926	1028	1028	37,040	0,565	59655
4. „	950	1028	1028,6	37,425	0,446	59815
5. „	903	1028	1027,3	37,023	0,483	59900

In unmittelbaren Anschluss hieran folgten:

IV. Fünf Tage ohne kohlensaures Natron.

Datum Decbr.	Witterung im täglichen Mittel.	11½ Uhr Mittags. Quant.	11½ Uhr Mittags. spec. Gewicht.	3½ Uhr Nachm. Quant.	3½ Uhr Nachm. spec. Gewicht.	7 Uhr Abends. Quant.	7 Uhr Abends. spec. Gewicht.	11 Uhr Abends. Quant.	11 Uhr Abends. spec. Gewicht.	7 Uhr Morgens. Quant.	7 Uhr Morgens. spec. Gewicht.	Gesammtmenge.	spec. Gewicht derselben.
6	27'' 1,8 Bar. 10,3 C. W. wlk. tb. Stm. 75,3.	154	1025 1020	234	1022 1026	290	1025 1036,5	125	1032 1020	183	1031 1028,5	986	1027
7	27'' 4,8 Bar. 8,2 C. W. w. zht. Reg.	145	1027 1022	290	1023 1028	214	1027 1032	155	1032 1026,5	183	1031 1031	917	1027,5
8	27'' 1,4 Bar. 6,8 C. W. trb. Regen.	108	1028 1017	175	1025,5 1025,5	212	1026,5 1032	185	1020 1030	190	1030 1031	870	1027
9	27'' 5,8 Bar. 6,2 C. W. tb. Reg. 0,8.	116	1027 1018	191	1024 1026,5	206	1026,5 1031,5	153	1030,5 1027	198	1030 1034,5	864	1028
10	27'' 9,4 Bar. -1,8 C. SW. bt. wolk. 0,4.	123	1026 1017	219	1024 1027,8	283	1025 1037	151	1031 1025	170	1031 1028	946	1027

Quantität der freien Säure nach dem Natrongebrauch:

Datum.	$11\frac{1}{2}$ Uhr Vorm.	$3\frac{1}{2}$ Uhr Nachm.	7 Uhr Abends.	11 Uhr Abends.	7 Uhr Morgens.	Gesammtmenge.
6. Dec.	0,217	0,098	—	0,405	0,904	1,624
7. „	0,353	0,330	0,080	0,502	0,849	2,114
8. „	0,259	0,215	0,165	0,406	0,824	1,969
9. „	0,201	0,230	0,173	0,397	0,863	2,864
10. „	0,172	0,284	0,073	0,492	0,765	2,786

Datum.	Harnquantität in cc.	spec. Gewicht.	dasselbe auf 917 cc berechn.	Harnstoff.	Harnsäure	Körpergewicht.
						Grm.
6. Dec.	986	1027	1029	37,468	0,586	59710
7. „	917	1027,5	1027,5	36,689	0,523	59670
8. „	870	1027,5	1026	35,670	0,439	59405
9. „	864	1028	1026	36,288	0,475	59440
10. „	848	1027	1028	36,294	0,544	59510

Während die tägliche durchschnittliche Harnmenge bei Anwendung von 4 Grm. kohlensauren Natron nur um einige cc. vermehrt war, an den einzelnen Tagen nur innerhalb 50 cc. nach auf- und abwärts schwankte, das specifische Gewicht der täglichen Gesammtquantität sich constant auf 1028 erhielt, trat von Tage zu Tage eine nicht unbedeutende Steigerung des Körpergewichts ein, um erst nach Einstellung der Natronaufnahme und zwar sofort wieder zu sinken. Wenn also in dieser Hinsicht schon die Resultate der ersten Versuchsreihe bei Aufnahme von 2 Grm. kohlensauren Natron die Untersuchungen von Münch bestätigen, so ist es hier in noch höherm Grade der Fall. Wodurch das Körpergewicht unter dem Gebrauche des Mittels steigt, ist fraglich. Ist es Folge von Wasserretention, so besitzt das Mittel die Kraft, Wasser im Körper zurückzuhalten und zwar, da die Harnsecretion, wenn auch nur um ein Geringes, gesteigert war, auf Kosten der Respiration und Perspiration. Es war nun zu erwarten dass nach der Natronaufnahme dieses Plus von Wasser in Gestalt vermehrter Harnausscheidung wieder zum Vorschein käme, doch war diese

Vermehrung unerheblich und zeigte sich auch nur am ersten Tage, an dem ausserdem, wenigstens des Morgens, das Wetter schön war, so dass man im Zweifel ist, ob der früheren Wasserretention allein diese Erscheinung zugeschrieben werden darf. Von Seiten der Defäcation bemerkte ich ebenfalls keine Veränderungen, welche entschieden hätten auffallen müssen, wenn sie die Ursache solcher Differenzen im Körpergewicht gewesen wären. Allerdings kann man, da nun einmal die Harnsecretion sich nicht wesentlich steigerte, bei einer Lufttemperatur von 8—10 Grad, welche an jenen Tagen war, an eine erhöhte Hautthätigkeit denken und annehmen, dass dadurch das Plus von Wasser ausgeschieden wurde, was zwar den Untersuchungen von Münch widersprechen würde. Eine Transspiration fand jedoch nicht statt. Thatsache bleibt dass der Körper nach dem Aussetzen des Natron bei ganz gleichen Einnahmen innerhalb 3 Tagen um nahezu 500 Grm. leichter wurde.

Der Harnstoff war mit Ausnahme des ersten Tages um ca. 1 Grm. täglich vermehrt und es würde dies Seegen's Behauptung allerdings bestätigen, wenn an den Normaltagen nicht ähnlich hohe Summen vorgekommen wären. Anderseits hat man auch die wenngleich geringe Steigerung der Harnmengen zu berücksichtigen, denn bei Vergleichung der Tabellen wird man finden, dass eine vermehrte Harnstoffmenge mit einer vermehrten Harnmenge coincidirt. Immerhin bleibt aber ein Einfluss des kohlensauren Natrons auf gesteigerte Harnstoffausfuhr wahrscheinlich und es würde anzunehmen sein, dass, da 2 Grm. des Mittels keine Vermehrung zur Folge hatten, eine solche erst bei höhern Dosen hervortritt.

Im Widerspruche mit den Resultaten welche Münch erzielte und welche bei mir 2 Grm. kohlensaures Natron, doch nur im geringen Grade, lieferten, war die Harnsäure in Folge von 4 Grm. gar nicht vermindert, durchschnittlich etwas erhöht, doch auch die während jener 6 Tage geringste Menge betrug immer noch 0,406 Grm., während sie am 4. Tage auf 0,565 Grm. stieg. Da ferner auch in den Normaltagen ihre Quantität zwischen 0,320 und 0,545 schwankte und im Mittel 0,477 betrug, wovon die durchschnittliche Menge jener Tage an denen 2 Grm. Natron genommen wurden (0,441) nur ganz unerheblich abweicht, so ist es sehr problematisch ob überhaupt jenes Mittel die Harnsäureproduction influenzirt.

Die Quantität der freien Säure sank, wie dies nicht anders zu erwarten; beträchtlicher erst am dritten Tage um von da ab wieder in die Höhe zu gehen. Eine bedeutendere Höhe als in den Normaltagen erreichte sie aber erst nach Einstellung des Natrongebrauchs, während dies bei 2 Grm. schon früher eintrat. Berücksichtigt man nun noch wieviel Säure das Natron ausserdem zu seiner Neutralisation in Anspruch nimmt, so ist der Schluss gestattet dass dasselbe die Säurebildung im Organismus steigert.

Ob bei dem Ausdruck „freier Säure" an eine wirklich freie Säure zu denken, ob Milch- oder Essigsäure sich frei im Harne vorfinden, oder ob die saure Reaction des Harns nicht vielmehr abhängig ist von saurem phosphorsauren, vielleicht auch von saurem harnsauren, hippursauren u. a. Salzen — darüber sind die Ansichten noch getheilt. Gesetzt das letztere sei der Fall, so müssen jene Salze, wenn man nicht etwa annehmen will, dass sie erst in den Nieren gebildet würden, im Blute vorgebildet sein um durch die Nieren abgeschieden und in den Harn übergeführt zu werden. Bekommt das Blut durh Einführung starker Alkalien einen Ueberschuss an Alkali, so nimmt dieses Säuren für sich in Anspruch, und die etwa vorhandenen sauren Salze werden zu neutralen Salzen reducirt, der Harn büsst in Folge dessen an sogenannter freier Säure ein, wird neutral oder durch Auftreten basischer Salze selbst alkalisch. Die Attractionsgesetze kommen dabei selbstverständlich im hohen Maasse zu Geltung.

Man kann nun berechnen wie viel das kohlensaure Natron in diesem Falle an Säure bedurfte um neutralisirt zu werden und demnach auch wie sehr die Säurebildung im Körper gesteigert war, da doch der Harn selten alkalisch wurde, bei längerm Gebrauche kleiner Dosen und unmittelbar nach dem Natrongebrauche selbst einen weit höhern Säuregrad zeigte als in den Normaltagen. Wenn 31 Theile Natron durch 60 Theile Essigsäure, 88 Theile Buttersäure, 45 Theile Oxalsäure, 90 Theile Milchsäure neutralisirt werden, in 2 Grm. kohlensaurem Natron aber 0.4336 Grm Natron enthalten sind, so nehmen diese von Essigsäure 0.8365, Buttersäure 1.2308, Oxalsäure 0.6294, Milchsäure 1.2588 Grm. zu ihrer Neutralisation in Anspruch, 4 Grm. kohlensaures Natron das Doppelte, so lange der Harn noch nicht alkalisch ist. Fanden sich in den Normaltagen durchschnittlich 1.767, bei 2 Grm. Natron carbon. 2.303 Grm. freier Säure im 24stündigen Harn, so musste die Bildnng derselben im Organismus, wenn man — in Oxalsäure ausgedrückt — noch diejenige addirt welche das

Natron zu seiner Neutralisation gebrauchte, auf 2.932 Grm. gestiegen sein. Bei 4 Grm. Natr. carb. betrug die tägliche Durchschnittsquantität 1.355, also nach Addition von 2·06294 Grm. Oxalsäure 2.5938 Grm. Hier trat erst nach Aussetzung des Mittels eine Steigerung der Durchschnittsquantität auf 1.871 ein.

Vergleicht man schliesslich die Wirkungen des Natron cabronic. mit denen des Kochsalzes, und des schwefelsauren Natrons, so ergeben sich interessante und therapeutisch wichtige Verschiedenheiten. Was zunächst die Harnstoffausscheidung betrifft, so hat das Kochsalz auf dieselbe eine ganz entschieden und ziemlich beträchtlich fördernde Einwirkung; das kohlensaure Natron dagegen scheint dieselbe zwar ebenfalls zu befördern, jedoch jedenfalls in weit geringerm Grade als das Kochsalz; das Schwefelsaure Natron endlich verhält sich nach Voits Untersuchungen *) in dieser Beziehung ganz indifferent. Vermehrt aber das schwefelsaure Natron und Kochsalz die Ausscheidungen von Seiten des Intestinaltractus und so zwar dass nur das schwefelsaure Natron längere Zeit in grössern Dosen genommen werden kann ohne die Schleimhaut zu afficiren und in ihrer Function zu stören, so kommt eine solche Wirkung dem kohlensauren Natron nicht zu. Dagegen scheint wieder dieses, so wie das Kochsalz die Säfteströmung im Organismus zu steigern. Dem kohlensauren Natron allein bleibt aber schlieslich eine Einwirkung auf die Bildnng organischer Säure im Organismus eigenthümlich. —

*) Zeitschrift für Biologie, Bd. I. pag. 195.

Thesen.

1. Es giebt nur drei Heilmittel, welche den Namen Specifica mit einigem Rechte verdienen, das sind die Chinarinde gegen Intermittens, Quecksilber gegen Lues und Eisen gegen Chlorose.
2. Bei Ellbogenresection ist der Längsschnitt andern Schnitten vorzuziehen.
3. Nur für die Frucht ist die Gesichtslage ungünstiger, als die Schädellage.

Ueber den

Stoffwechsel eines Diabetikers

verglichen

mit dem eines Gesunden.

Eine mit Genehmigung einer Hochverordneten Medicinischen Facultät der Kaiserlichen Universität zu **DORPAT** zur Erlangung des

Doctorgrades

verfasste und zur öffentlichen Vertheidigung bestimmte

Abhandlung

von

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Rigenser

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Dr. Friedrich Bidder,

ord. Prof. der Physiologie, etc.

in dankbarer Verehrung

gewidmet

vom Verfasser.

Vorwort.

Auf den nachfolgenden Blättern wird ein Theil der Untersuchungen veröffentlicht, welche mich seit der Beobachtung des hier beschriebenen Falles von Zuckerharnruhr in mehrfacher Beziehung beschäftigt haben. Nichtsdestoweniger bin ich zuletzt durch Umstände, die ich nicht vorhergesehen hatte, gezwungen worden, auf die Abfassung der vorliegenden Abhandlung weniger Zeit und Arbeit zu verwenden, als mir von der Schwierigkeit des Gegenstandes geboten schien. Schon deshalb habe ich allen Grund, um eine nachsichtige Beurtheilung dieses Erstlingsversuches auf literärischem Gebiete zu bitten.

Diese Gelegenheit giebt mir den gewünschten Anlass Herrn Prof. Weyrich, der mich zu dieser Untersuchung veranlasst und mit grosser Liberalität die dazu erforderlichen Mittel von Seiten der Klinik zu meiner Verfügung gestellt hat, für das durch Rath und That an den Tag gelegte Interesse an dieser Arbeit, öffentlich meinen Dank auszusprechen.

Nicht weniger fühle ich mich verpflichtet dem Herrn Prof. C. Schmidt, der mit Ausnahme des Mannes, dem diese Blätter gewidmet sind, die erste Stelle in der Dankbarkeit einnimmt, die ich meinen akademischen Lehrern zolle, und der, wie Allem, was mich seit unserer Bekanntschaft geistig beschäftigt hat, auch dieser wissenschaftlichen Frage seine Theilnahme zuwandte, herzlichst zu danken, wobei ich nicht besonders zu versichern brauche, dass die gemeinsam mit ihm verbrachten und von seiner so ausserordentlich anregend wirkenden Unterhaltung belebten Stunden zu den werthesten Erinnerungen gehören, die mich durch mein Leben begleiten werden.

Endlich muss ich dem Herrn Beckmann, Mag. pharm., der sich bei dem Umfange der analytischen Untersuchungen freundlichst erbot, die Bestimmung der anorganischen Harnbestandtheile zu übernehmen, für diesen schätzenswerthen Beitrag zu meiner Dissertation und für manchen die Technik der von mir ausgeführten Analysen betreffenden, guten Rath, an dieser Stelle meinen Dank wiederholen.

I. Einleitung.

Der Kranke, welcher in den folgenden Versuchen den Gegenstand unserer Beobachtung bildete, war ein 31jähriger Tischler, von esthnischer Abkunft, der zur Zeit der Aufnahme in die Dorpater Klinik (am 29. October 1862), die ich damals als Praktikant besuchte, ungefähr ein Jahr krank zu sein vorgab. Die Krankheit soll ihn plötzlich, und unter so auffallenden Erscheinungen befallen haben, dass er sich der Einzelheiten seiner damaligen Erlebnisse noch sehr gut zu erinnern vermochte.

Auf der Fahrt nach einem besuchten Markte begriffen, wurde er im Walde mit seinen Gefährten von der Nacht überrascht. Da es kalt war (am 28. September) machte man ein Feuer an, neben welchem sich der Patient auf der nackten Erde niederlegte; trotzdem dass er mit einem Pelze bekleidet war, fror ihn erheblich. Dabei wurde er, wie er sich deutlich erinnern will, plötzlich von einem gewaltigen, früher nie gekannten, Durste überfallen, der mit dem Vorrath an Wasser, der sich im Walde auftreiben liess, nur unvollkommen gelöscht werden konnte. Im nächsten Kruge, wo er Wasser fand, habe er daher beinahe einen vollen Eimer davon getrunken. Seit der Zeit schwand der Durst aber nie ganz, es stellte sich häufiges und reichliches Harnen ein und bei wachsendem Appetite sanken die Kräfte. Bald nach dem ersten Durst-Anfalle trübte sich auch das Sehvermögen; 8 Tage später konnte er einen Menschen auf 3 Schritte nur den Umrissen nach erkennen. Ungefähr 5—6 Wochen darauf soll nach einem Aderlass am Arme das frühere Sehvermögen zurückgekehrt sein. Zu Zeiten haben auch wassersüchtige Anschwellungen der untern Extremitäten bestanden.

In Bezug auf anamnestische Momente liess sich ferner ermitteln, dass Patient stets sein gutes Auskommen gehabt habe. Er ist verheirathet, hat in der Ehe drei Kinder gehabt, von denen zwei während einer Scharlachepidemie starben; das lebende ist gesund. Patient lebt seit einem Jahre in der Stadt (Dorpat), früher auf dem Lande; die Wohnung hier wie dort war weder feucht noch kalt. Deprimirende Gemüthsaffecte, eine Verletzung, ein Stoss, Erschütterung des Körpers werden bestimmt in Abrede gestellt.

Der Patient misst 162 Ctm., ist von gutem Knochenbau, schwachem panniculus adiposus, reducirter, schlaffer Muskulatur. Die Haut hat eine schmutzige, schwach gelbliche Färbung, ist sonst rein und fühlt sich allenthalben rauh und trocken an; namentlich an den untern Extremitäten. Das Haar ist schlicht, von gelbröthlicher Farbe, die Augen blau, der Blick frei, die Conjunctiva blass, die Pupillen von normaler Beweglichkeit und Weite. Die Wangen zeigen eine leichte Röthe. Die Schleimhaut der Lippen von bläulich-rother Farbe, anämisch. Die Zunge ist rein, die Schleimhaut der Mundhöhle blass. Mehrere Backenzähne zeigen sich stark cariös afficirt, dieselben sind nach der Aussage des Patienten erst vor einem halben Jahre erkrankt. Vorher hatte er nie Zahnschmerzen; jetzt leidet er daran häufig. Die Supra- und Infraclavikulargruben mässig ausgeprägt; die Clavikeln prominiren entsprechend der an der oberen Thoraxhälfte namentlich auffälligen Abmagerung. Die Bauchdecken sind gespannt, gegen Druck schmerzlos. Die

glans und das praeputium sind intact; die untern Extremitäten ebenfalls abgemagert, die Wade collabirt; über den Knöcheln zeigt sich eine ödematöse Schwellung.

Der modus respirandi ist gleichmässig. Der Thorex lang, breit, und wenig convex. Die rechte Lunge reicht in der Mammillarlinie bis zum obern Rande der 6., in der Axillarlinie bis zum untern Rande der 8., in der Cristo-Dorsallinie bis zum untern Rande der 11. Rippe hinab. Die linke Lunge schneidet die Mammillarlinie in der Höhe der 6. die Axillarlinie in der Höhe der 8. und die Cristo-Dorsallinie in der Höhe der 11. Rippe. Die vitale Capacität der Lungen beträgt 3083 C. C. (Mittel aus 8 Bestimmungen).

Der Perkussionsschall ist durchweg lufthaltig, sonor; nur an der Spitze der linken Lunge erscheint er ein wenig kürzer.

Die Auskultation weist überall Durchgängigkeit ber Luftwege nach; das Athmungsgeräusch ist schwach, die Luft dringt gleichmässig aber träge ein. Respirations-Frequenz 18 in der Minute. An der schon bezeichneten Stelle des verkürzten Perkussionsschalles erscheint die Inspiration langgezogen, die Exspiration mässig verstärkt.

Die Herzdämpfung, von der Insertionsstelle des 3. Rippenknorpels an den linken Sternalrand beginnend, geht in der Höhe des 4. in die Herzleere über, welche sich bis an den 6. Rippenknorpel hinab verfolgen lässt. Rechts wird sie durch den linken Sternalrand von dem Lungenton abgegrenzt. Der nur schwach fühlbare Herzstoss findet sich im 4. Intercostalraum, etwa 3''' nach innen von der Mammillarlinie. Die Herztöne sind schwach, rein, deutlich von einander abgegrenzt. Der Puls ist leer, leicht comprimirbar; Pulsfrequenz 58 in der Minute.

Die Leber überragt, von der 4. Rippe beginnend, den Rippenbogen um 2 Fingerbreit. Die Milz ist nicht anomal gelagert, und reicht vom obern Rande der 8. bis zum untern Rande der 10. Rippe. Die Grenzen ihrer Dämpfung lassen sich ungefähr mit der Handfläche bedecken.

Appetit, Durst und Urinsekretion sind übermässig gesteigert. Die Temperatur der Achselhöhle ist 36° C. Der Harn ist von hellgelber Farbe, erleidet beim Kochen eine leichte Trübnng, die auf Zusatz eines Tropfens Essigsäure sofort verschwindet und hat ein specif. Gewicht von 1040. Mit Aetzkali-Lösung gekocht wird er rothbraun gefärbt; kocht man ihn mit einer Lösung von CuO, SO_3 unter Zusatz von Aetzkali-Lösung so erhält er eine intensiv hellbraune Farbe.

Dieser nach dem bisher Mitgetheilten sich ganz unzweifelhaft als ein Fall von diabetes mellitus charakterisirende Kranke, sollte nun auf den Wunsch des Herrn Prof. Weyrich einer vergleichenden Beobachtung mit einem Gesunden (mir) [1], in Bezug auf den Stoffwechsel unterworfen werden — so weit dies an einem klinischen Objecte, dem Kranken, ausführbar und namentlich mit klinischen Hülfsmitteln durchführbar erschien — unter Bedingungen, welche für beide Vergleichsobjecte als identische gelten konnten.

Die Fragen, welche vorzugsweise durch diesen Versuch beantwortet werden sollten, waren folgende:

1) wie verhält sich der Stoffwechsel eines Diabetikers zu dem eines Gesunden der denselben Bedingungen unterworfen ist, in einem Zustande, welcher weder absolute Ruhe noch Bewegung genannt werden kann.

---

1) Ich war damals 23 Jahr alt, fühlte mich gesund. Knochenbau normal, Muskulatur mässig entwickelt, panniculus adiposus schwach, Körperhöhe 169,7 Ctm., vitale Lungencapacität 3800 CC. (Mittel aus 5 Bestimmungen). Die in Betreff des Alters, des Ernährungszustandes, der Körperlänge, des Körpergewichts (s. die Tabellen) zwischen mir und dem Kranken bestehenden Differenzen lassen sich daher übersehen.

Und wie gestaltet sich dieses Verhältniss

2) unter dem Gebrauche von doppelt-kohlensaurem Natron und

3) unter dem Gebrauche von Benzoësäure.

Zur Lösung dieser Fragen wurde folgendes Verfahren eingeschlagen.

1) Ich bezog ein geräumiges Zimmer der Klinik, das ich mit dem Kranken während eines Zeitraums von 40 Tagen (40 mal 24 Stunden) gemeinsam bewohnte und es mit wenigen Ausnahmen (wovon später) nur in Begleitung des Patienten verliess. Die Zeit des Schlafes (von 11 Uhr abends bis 7 Uhr morgens) und des Wachens (von 7 Uhr morgens bis 11 Uhr abends), der Bewegung (Umhergehen im Zimmer) und der Ruhe (Sitzen, Liegen auf dem Bette) war beiden Personen möglichst gleich zugemessen. Die von der Atmosphäre, der Ruhe und Bewegung etc. abhängigen Einflüsse auf den Stoffwechsel waren somit für beide Objecte der Vergleichung gleich gesetzt und der Kranke unter unausgesetzte, strenge Aufsicht gestellt.

2) Die Speisen, die für beide Personen stets von gleicher Qualität waren, wurden unmittelbar vor dem Genuss gewogen (über die Wage siehe weiter unten), die flüssigen (Fleischbrühe, Milch etc.) gemessen; ebenso die Getränke, in einer solchen Quantität, welche von einem Gesunden, ohne ihn besonders zu belästigen, aufgenommen werden konnte. Die Menge des Trinkwassers hing ganz von dem Wunsche des Kranken ab, was ihm ausdrücklich mitgetheilt worden war, mit der Bemerkung, dass ihm übermässiges Trinken nicht zuträglich sei. Er hatte das Wasser, welches in einer Flasche auf dem Tische neben meinem Bette stand nur von mir zu fordern; nie ist ihm seine Forderung abgeschlagen worden. Ich selbst trank dann gleichzeitig dieselbe Menge. Das Wasser

wurde stets in Gläsern verabreicht, die bei vollkommener Anfüllung genau 210 CC. fassten. Ebenso fand die Aufnahme der Speisen immer gleichzeitig und (mit wenigen Ausnahmen) in derselben Menge statt. Zur Abmessung der Fleischbrühe, welche in der klinischen Küche geschah, war dem Oekonomen der Klinik ein von uns selbst ausgemessenes und bis zur Marke genau 600 CC. fassendes Gefäss übergeben worden. Dass die Fleischbrühe stets in durchgeseihtem Zustande auf den Tisch kam (nur die allerersten Versuchstage machen davon eine Ausnahme) konnte ich selbst überwachen. Für die Milch war ein besonderes Gefäss bestimmt, welches im gefülltem Zustande genau 560 CC. enthielt. Zur Abmessung des später verabreichten Kaffees und Thees diente ein und dasselbe Gefäss; es fasste vollkommen angefüllt 330 CC. Das Brod habe ich nur in der ersten Zeit für jede Mahlzeit besonders abgewogen. Es war schon von vornherein wahrscheinlich, dass die auf das Krankenzimmer geschickten Brodmengen, die der Oekonom für einen festgesetzten Preis zu liefern hatte, keine grossen Schwankungen zeigen würden; von mir vorgenommene Wägungen bestätigten es. In der Folge wurden sie von mir nur in den damals gefundenen Durchschnittswerthen notirt. Ebenso geschah es mit der Butter, welche uns stets in derselben Menge und Form, letztere der Abdruck eines besondern Maasses, zugeschickt wurde.

Um die Summe der innerhalb 24 Stunden in den Körper eingeführten Einnahmen ziehen zu können, war es nöthig, die gemessenen, flüssigen Einnahmen auf die ihrem Volum entsprechenden Gewichtswerthe zu übertragen. Zu dem Zwecke habe ich zu wiederholten Malen das specif. Gewicht der Fleischbrühe und des Trinkwassers bestimmt, um mit Hülfe desselben und aus dem bekannten Volum das absolute Gewicht festzustellen. Proben dieser Flüssigkeiten wurden so lange im Zimmer auf-

bewahrt, bis sie sich ungefähr mit der Zimmertemperatur ins Gleichgewicht gesetzt hatten und dann wurde das specif. Gewicht mit Hülfe eines gläsernen, genau gearbeiteten Araeometers aufgesucht. Dasselbe war für eine Temperatur von 15° R. construirt und es liess sich daran das specif. Gewicht bis auf $^1/_{1000}$ genau ablesen. Um mich von der Genauigkeit dieses Instruments zu überzeugen, habe ich ein Paar vergleichende Bestimmungen mit einem von Geissler in Berlin gearbeiteten Piknometer vorgenommen, welches eine vollkommen Uebereinstimmung der auf diesen verschiedenen Wegen gewonnenen Resultate nachwiesen. Dasselbe Araeometer hat mir auch zur Bestimmung des specif. Gewichts des Harns gedient. Weil die Skala nur von 1,000 bis 1,050 reichte, so musste an denjenigen Tagen, an welchen das specif. Gewicht des diabetischen Harns 1,050 überstieg (was übrigens nur ausnahmsweise vorkam) die jene Ziffer übersteigenden Grade blos abgeschätzt werden. Mit diesem Instrumente erhielt ich für die Fleischbrühe im Mittel aus 4 Bestimmungen 1,014, womit eine von Prof. C. Schmidt ausgeführte Bestimmung vollkommen übereinstimmte. Das Trinkwasser hatte im Mittel aus 6 Bestimmungen ein specif. Gewicht von 1,002. Es ist indessen in den Tabellen = 1,000 angenommen worden, wobei der Fehler für den Gesunden und den Diabetiker identisch ist. Der Kaffee, in der Zusammensetzung wie er überhaupt auf der Klinik zubereitet und auch von uns genossen wurde zeigte nach einer von Prof. C. Schmidt angestellten Untersuchung bei 35° C. eiu specif. Gewicht von 1,005, der Thee bei derselben Temperatur 1002. Milch und Rahm habe ich wiederholt auf der mir zu Gebote stehenden genauen Wage gewogen; im Mittel wog die morgens und abends eingenommene Quantität von 560 CC. Milch 580 grm., womit das von Voit (Untersuchungen über den Einfluss des Kochsalzes u. s. w., München 1860 pag. 71) gefundene, mittlere specif. Gewicht guter Kuhmilch = 1,020 ziemlich gut übereinstimmt. Bei einem solchen specif. Gewicht hätten unsere 560 CC. 578,8 Grm. wiegen müssen. Die am Morgen zum Kaffee verbrauchte Quantität Rahm wog 56 grm., die Abends dem Thee zugeschüttete 30 grm.

Von der Ansicht ausgehend, dass Fehler, wenn sie nur auf beiden Seiten (für den Gesunden und für den Kranken) identisch ausfallen, bei dieser Methode der vergleichenden Beobachtung, die Richtigkeit von Schlüssen, welche vorzugsweise die Relation des kranken zum gesunden Individuum zur Grundlage haben, nicht wesentlich beeinträchtigen können, schien es mir ferner des Versuches werth, auch ohne direkte Elementaranalyse, die ganz unmöglich war, eine Zerlegung der Einnahmen nach bereits vorliegenden Analysen, welche die mittlere Zusammensetzung der Nahrungsmittel berücksichtigen, vorzunehmen. Es sollte auf diesem Wege versucht werden, einen Einblick in die Abweichungen zu gewinnen, welche der Kreislauf des Wassers, des Kohlenstoffs, Stickstoffs, Sauerstoffs, Wasserstoffs u. s. w. durch einen gesunden und einen diabetischen Organismus darbietet. Dieser Zerlegung in die Elemente lagen die Angaben zuverlässiger Forscher zu Grunde, welche ich nachfolgend tabellarisch zusammengestellt habe.

In 100 grammes.	Wasser.	Feste Substanz.	Org. trockne Substanz.	Mineralsalze.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff.	Schwefel.	
Milch	87,08	12,92	12,19	0,73	7,05	1,11	0,63	3,40		2)
Gebratenes Fleisch	54,1	45,9	44,05	1,85	25,56	3,51	5,68	8,94	0,36	3)
Roggenbrod	36,4	63,6	62,33	1,27	28,65	3,94	1,09	28,59	0,06	3)
Gebeutelt. Rog.-Brod	40,3	59,7	58,51	1,19	26,89	3,70	1,03	26,83	0,06	3)
Weissbrod	35,52	64,48	63,19	1,29	29,00	4,00	1,10	29,03	0,06	3)
Butter	4,81	95,19	94,99	0,20	75,04	10,83		9,12		4)
Fleischbrühe	97,34	2,66	1,23	1,43	0,62	0,09	0,22	0,30		5)
Kaffee	98,4	1,6	1,55	0,05	0,72	0,12	0,06	0,65		6)
Thee	99,94	0,06	0,058	0,002	0,027	0,005	0,002	0,024		7)

3) Die sensiblen Ausgaben (Harn, Excremente), wurden so sorgfältig als dies hiebei überhaupt möglich ist aufgesammelt; bei der Stuhlentleerung wurde darauf Rücksicht genommen, dass ein Verlust an Harn vermieden werde. Letzterer wurde am Tage zu bestimmter Zeit (stündlich, 2 stündlich, 3 stündlich) von beiden Personen gleichzeitig in Gläser, die eine passende Vorrichtung zum Ausgiessen besassen gelassen. Nach einiger Zeit (die der Abkühlung bestimmt war) wurde er in einem gläsernen, ebenfalls mit einem Ausgusse versehenen Standgefässe, an welchem 10 C. C. noch genau abzulesen und weniger abgeschätzt werden konnten, gemessen, das specif. Gewicht, die chemische Reaction, die Farbe bestimmt und in einem mit gutschliessendem Korke, der mit der entsprechenden Stundenzahl bezeichnet war, verschlossenen Reagenzgläschen zur quantitativen Untersuchung auf Zucker aufgehoben. Der übrige Harn wurde dann in besondere Flaschen mit eingeriebenen Glasstöpsel gegossen, welche stets rein gehalten wurden,

1) Vergl. Voit, Untersuchungen über den Einfluss des Kochsalzes, des Kaffees u. s. w. auf den Stoffwechsel. München 1860 pag. 71.

2) Der Wassergehalt ist das Mittel aus den 4 von J. Ranke (die Kohlenstoff- und Stickstoff-Ausscheidung des ruhenden Menschen Arch. f. Anatom. und Physiolog. 1862 pag. 371) mitgetheilten Bestimmungen. Ueber die Zusammensetzung des festen Substanz vergl. Bidder und Schmidt. Die Verdauungssäfte und das Stoffwechsel. Mitau und Leipzig 1852 pag. 301 und 302.

3) Nach Analysen von Prof. C. Schmidt.

4) Vergl. Barral, Sur la statique etc. pag. 147 und 148.

5) Der Wassergehalt, die trockne org. Substanz und der Gehalt an Mineralsalzen nach einer Analyse von Prof. C. Schmidt. Die untersuchte Fleischbrühe war in der klinischen Küche in der dort gewöhnlichen Weise zubereitet worden. In Bezug auf die Zusammensetzung der trocknen, org. Substanz vergl. Bidder und Schmidt, a. a. O. pag. 303.

6) Der Wassergehalt nach einer Analyse des Prof. C. Schmidt eines in der klinischen Küche zubereiteten Kaffeeextrakts. Die übrige Zusammensetzung nach Voit, a. a. O. pag. 80.

7) Der Wassergehalt nach einer Analyse von Prof. C. Schmidt eines in der klinischen Küche zubereiteten Theeextrakts. Die übrige Zusammensetzung nach Voit, a. a. O. pag. 80.

und von denen eine zur Aufbewahrung des von dem Gesunden gelassenen, die andere zur Aufnahme des diabetischen Harns diente. Der während der Nacht (von 11 Uhr abends bis 7 Uhr morgens) gelassene Harn unterlag am nächstfolgenden Morgen einer gleichen Behandlung. Waren 24 Stunden abgelaufen, so wurde der Harn in den beiden Flaschen gehörig umgeschüttelt, das specif. Gewicht, die Reaction, die Farbe bestimmt und die nöthigen Mengen zur quantitativen Bestimmung auf Harnstoff, Kochsalz, Schwefelsäure etc. in kleine Glasgefässe, die mit Etiketten versehen wurden, abgegossen; die Flaschen dann ausgeleert, um am folgenden Versuchstage in derselben Weise verwendet zu werden.

Die Excremente wurden meist sofort nachdem sie abgesetzt waren gewogen (hiebei kamen indessen Ausnahmen vor, so namentlich an dem Tage als der Kranke an Diarrhoe litt, oder wenn eine Stuhlentleerung des Nachts erfolgt war; die Wägung wurde dann erst am Abend, resp. am Morgen vorgenommen). Eine qualitative Untersuchung des Mag. Beckmann auf Zucker konnte keine Spur davon in den Excrementen des Diabetikers nachweisen, ganz entsprechend den Erfahrungen von Heller [1]), welcher bei Untersuchungen, die im Verlaufe eines ganzen Jahres häufig angestellt wurden, in den festen Excrementen nie Zucker fand, wol aber in Diarrhoe-Stühlen [2]). Die elementare Zusammensetzung der Excremente von Diabetikern ist, soviel mir bekannt, bisher noch nicht untersucht worden. Es liegt auch kein Grund vor, eine bedeutendere Abweichung von der in den Excrementen von Gesunden anzunehmen. Der Wassergehalt scheint (dem äussern Ansehen nach), wenn nicht gerade Diarrhoe besteht, herabge-

1) J. Flor. Heller, Ueber diab. mell. Hellers Archiv 1852 pag 403.

2) Bei unsrem Pat zeigten auch die flüssigen Excremente keinen Zucker.

setzt zu sein, was direkte Beobachtungen von Böcker nicht bestätigten [1]). Derselbe fand durch Untersuchungen, welche an 6 Tagen über den Wassergehalt diabetischer Fäces angestellt wurden, den mittleren Gehalt an Wasser in 100 Grm. = 68,62 Grm., während die von gesunden Personen gelieferten, gemischten Excremente nach J. Ranke [2]) 64,8 % Wasser enthalten (Mittel aus 4 Beobachtungen). Für die Berechnung der in die Zusammensetzung der Excremente eingegangenen Elemente habe ich daher sowol für den Diabetiker, als den Gesunden die Angaben von J. Ranke (a. a. O pag. 371 u. ff) über die Zusammensetzung gemischter, menschlicher Excremente benutzt. Der procentische H-Gehalt ist der Arbeit von Barral [3]) entnommen worden und stellt das Mittel einer 20-tägigen Beobachtung an mehreren Personen dar. Dem zu Folge liegt meiner Berechnung folgende procentische Zusammensetzung der Excremente zu Grunde:

In 100 grm. Faeces bei gemischter Nahrung sind enthalten 64,8% Wasser, 35,2% feste Substanz, 31,05 org. trockne Subst., 4,15 Mineralsalze, 16,54% C., 2,45% H., 2,13% N. und 9,93% O.

4) Zum Zwecke der Bestimmung des Körpergewichts-Verlustes durch insensible Ausgabe wurden zweimal täglich Wägungen beider Versuchspersonen angestellt und zwar morgens gleich nach dem Aufstehen und nachdem von beiden Personen die Blase entleert worden war und abends vor dem man zu Bette ging, wobei der Harn, wenn es nicht schon vor der Wägung geschehen war, unmittelbar darauf gelassen, und dann

1) Boecker, Untersuchungen über diab. mell. Deutsche Klinik 1853 pag. 374.

2) J. Ranke, Die Kohlenstoff- und Stickstoff-Ausscheidung des ruhenden Menschen. Arch. f. Phys. und Anatom 1862. pag. 311.

3) Barral, Sur la statique chimique du corps humain. Annales de Chimie et de Physique S. III T. 25. 1849 pag. 129.

von dem soeben ermittelten Körpergewichte in Abzug gebracht wurde. Die zu wägende Person war nur mit einem dünnen, eigens zu diesem Zwecke bestimmten Mantel, der sowol von dem Kranken als von dem Gesunden benutzt wurde, bekleidet. Die Wage, welche zu diesen Wägungen benutzt wurde, war eine gute Weber'sche Federwage, welche, wie ich mich sowol damals, als auch durch spätere Versuche überzeugt habe, bei geringerer Belastung schon durch 1 — 2 Grm. aus dem Gleichgewichtszustande gebracht wurde. Dieselbe Wage wurde auch bei den Wägungen der Excremente und der Nahrungsmittel in Anwendung gezogen.

5) Unter den einzelnen Bestandtheilen der Ausgaben wurden nur die des Harns einer nähern Untersuchung unterzogen und zwar wurde aus der in 24 Stunden gelassenen Harnmenge an jedem Tage bestimmt:

a) das Harnwasser, durch Subtraction der festen Bestandtheile von dem Gewichte der ganzen, während dieses Zeitraumes gelassenen Harnmenge. Letzteres wurde durch Multiplication des Volums mit dem specif. Gewicht gefunden; die festen Bestandtheile wurden aus dem specif. Gewicht berechnet: für den Diabetiker mit Hülfe einer von Professor C. Schmidt zu diesem Zwecke entworfenen Tabelle [1]) für den Gesunden nach der Haeserschen Formel [2]).

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1) Prof. Schmidt benutzte dazu die aus den Resultaten der ganzen Versuchszeit für den Diabetiker erhaltenen Mittelwerthe an Zucker, Harnstoff etc. in 1000 C. C. Harn. bei einem mittlern spec. Gew. von 1046. Der gesammte trockne Harnrückstand betrug dabei 112,80 grm, woraus sich für die Zu- oder Abnahme des specif. Gew. um 0,001 je 2,45 + oder — berechnet. Auf diesem Wege konnte man bei dem hohen specif. Gew. des diabet. Harns sicherere Resultate zu erhalten erwarten, als mit Hülfe der Häserschen oder Trappschen Formel (vergl. darüber Neubauer und Vogel pag. 291.)

2) Neubauer und Vogel, Anleitung zur qualitativen und quantitativen Analyse des Harns, Wiesbaden 1863 pag. 123 und 209.

b) der Harnstoff, nach Liebig (s. Neubauer und Vogel a. a. O. pag. 140).

c) das Kochsalz, ebenfalls nach der Methode von Liebig (s. v. Gorup-Besanez Lehrbuch der physiologischen Chemie, 1862, Braunschweig, pag. 521).

d) die Schwefelsäure, vermittelst einer titrirten Chlorbaryumlösung (s. Neubauer und Vogel a. a. O. pag. 155).

e) die Phosphorsäure auf gewichtsanalytischem Wege

α) an 3 CaO gebunden,

β) an 2 MgO gebunden, indem der Harn zuerst durch Ammoniak gefällt, und der erhaltene Niederschlag von Erdphosphaten nach der von Neubauer und Vogel a. a. O. pag. 175 und 176 beschriebenen Methode behandelt wurde.

γ) an Alkalien gebunden, durch Fällung der in dem Filtrate, welches bei Entfernung der Erdphosphate gewonnen war, noch vorhandenen PO_5 mittelst schwefelsaurer Magnesia und Ammoniak. Der dadurch erzeugte Niederschlag wurde getrocknet, geglüht, als 2 MgO, PO_5 gewogen und daraus die an Alkalien gebundene PO_5 berechnet.

Nur an 6 Tagen wnrde bestimmt:

f) Kreatinin, mittelst einer alkoholischen Chlorzinklösung (s. Neubauer und Vogel a. a. O. pag. 169).

An 5 Tagen (während des Gebrauchs von Benzoësäure):

g) Hippursäure, mittelst einer titrirten Lösung von neutralem Eisenchlorid (vergl. R. Wreden Bulletin der Petersburger Akademie XVII p. 500 und Journal für praktische Chemie LXXVII p. 446 (1859) daraus: in Liebig's u. Kopp's Jahresb. d. Chemie 1859 p. 700.)

Der von **Stoc vis**[1]) gegen diese Methode erhobene Einwand: es werde, um eine deutliche Reaktion auf Eisencyankaliumpapier herbeizuführen, bei so wenig concentrirten Flüssigkeiten, wie der Urin, in Folge der Verdünnung, eine vielgrössere Menge FeCl verbraucht, als nöthig ist, um die Hippursäure als hippursaures Eisenoxyd niederzuschlagen, hat in unserem Falle, wo es sich hauptsächlich um relativ richtige Werthe handelte, keine besondere Bedeutung.

Aus dem diabetischen Harn wurde ausserdem bestimmt:

h) der Zucker, für eine jede gesondert gesammelte Harnquantität, also in der ersten Zeit aus der stündlichen, später 2 stündlichen, resp. 3 stündlichen Menge des Tagharns (von 7 Uhr Morgens bis 11 Uhr Abends), sowie aus der Gesammtmenge des Nachtharns (von 11 Uhr Abends bis 7 Uhr Morgens). Die Bestimmung geschah mittelst eines in Berlin genau gearbeiteten Saccharimeters[2]) (Soleil-Ventzke) und wurde

1) **Stocvis**, Studien des physiologischen Instituts zu Amsterdam, herausgegeben von Heynsius. Leipzig und Heidelberg 1861 pag. 115.

2) Aus der gemischten, gut durchgeschüttelten, 24stündigen Harnquantität wurde noch ausserdem der Zucker nach der Methode von Fehling bestimmt, wozu stets frischbereitete und beim Erwärmen das Kupferoxydul nicht spontan, ausscheidende Lösung verwendet wurde. Die hiebei erlangten Resultate stimmten nur selten vollkommen mit dem durch den Saccharimeter gewonnenen überein, ebensowenig liess sich eine constante, positive oder negative Differenzgrösse feststellen, ganz analog den Versuchen von Neuschler (Arch. f. physiolog. Heilkunde 1858 pag. 401. Ueber optische Harnzuckerbestimmung.), den ältern Erfahrungen von Wicke und Listing (Zeitschrift f. rat. Med. N. F. VI pag. 311—325) und den neusten Untersuchungen von M. Tscherinoff (Wiener akadem. Sitzungsbericht mathem. — naturwissenschaftl. Klasse 2. Abtheilung LI, auch in einem Referat von Kühne im Centralblatt 1865 No. 47) „über die Bestimmung des Harnzuckers aus der Drehung der Polarisationsebene.“ Da es in unserem Falle viel weniger auf absolut als auf relativ richtige Werthe ankommt, so lasse ich die auf dem Wege der Titrirmethode gefundenen Resultate ganz unberücksichtigt und verschiebe die Mittheilung des zur Vergleichung beider Bestimmungsmethoden geeigneten Versuchsmaterials und dessen Besprechung auf eine andere Gelegenheit.

meistentheils kurz nachdem der Harn gelassen war ausgeführt. Zu dieser Zeit erschien der Harn vollkommen klar, nur wenig gefärbt und liess sich daher ohne weitere Vorbereitung in die Glasröhre des Instruments bringen. Musste die Untersuchung einige Zeit aufgeschoben werden, so war einmaliges und auch mehrmaliges Filtriren des Harns erforderlich, um die inzwischen eingetretene und die scharfe Beobachtung der Farben störende Trübung zu entfernen.

6) Neben diesen Aufgaben, welche zum Zwecke hatten, eine so genaue Kenntniss, als es bei solchen klinischen Untersuchungen überhaupt möglich ist, von den Einnahmen und Ausgaben der beiden Versuchspersonen zu erhalten, lag es mir noch ob, die Temperatur des Zimmers, des Körpers beider Vergleichsobjekte und deren Puls- und Respirationsfrequenz zu bestimmen. Dieses geschah ausser am Morgen vor dem Aufstehen und am Abend nach dem Niederlegen mehrere Mal täglich zu bestimmter Zeit. Die Thermometer, welche zu den Temperaturbestimmungen dienten, hatte ich zuvor auf den Thaupunkt verglichen. In der Achselhöhle blieben sie so lange liegen, als keine Veränderung in dem Stande der Quecksilbersäule bemerkt werden konnte.

Diesen Bemerkungen will ich noch hinzufügen, dass die Bestimmung der Schwefelsäure, der Phosphorsäure und des Kreatinins ausgenommen, welche der Herr Mag. **Beckmann** besorgte, alle analytischen Untersuchungen, Wägungen, Messungen u. s. w. von mir selbst ausgeführt wurden.

Da ich durch äussere Gründe bestimmt wurde, die Mittheilung des Tagebuchs der Parallelbeobachtungen in dem Umfange, wie sie anfangs beabsichtigt wurde, aufzugeben, so habe ich auf den folgenden Blättern vorläufig nur die Untersuchungsresultate, welche eine Uebersicht über eine 24 **stündige Stoffwechsel-**

periode beider Versuchspersonen geben, in tabellarischer Form zusammengestellt. Diese Tabellen enthalten zum Theil die zu 24 stündigen Werthen zusammengefassten Einzelresultate aus dem, den detaillirten Gang der Untersuchung wiedergebenden, Tagebuche, zum Theil solche Zahlen, welche von Hause aus für eine 24 stündige Stoffwechselperiode aufgesucht wurden (Harnstoff, Kochsalz u. s. w.).

Zur Erläuterung der Tabellen brauche ich nur wenig vorauszuschicken.

Die Querzeilen derselben enthalten die im Verlaufe eines jeden einzelnen Versuchstages erhaltenen Untersuchungsresultate in Zahlen ausgedrückt, deren Bedeutung aus den mit Ueberschriften versehenen Vertikalzeilen leicht ersehen wird. Von den Vertikalzeilen beziehen sich die mit G. bezeichneten auf den Gesunden, die mit D. überschriebenen auf den Diabetiker.

Alle sich auf den Gesunden und den Diabetiker beziehenden, in derselben Querzeile untergebrachten Untersuchungsresultate sind auf dieselbe Weise und gleichzeitig, oder richtiger unmittelbar nach einander gewonnen worden.

Auf die Tabellen [1]), folgen kurze Bemerkungen, welche das subjective Verhalten beider Versuchspersonen berücksichtigen.

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1) Das Körpergewicht wurde, wie bereits angegeben, durch zweimal am Tage vorgenommene Wägungen bestimmt. Das in die Tabellen aufgenommene mittlere Körpergewicht habe ich nicht einfach als arithmetisches Mittel aus der Morgen- und Abendwägung berechnet, sondern, da es mir darauf ankam, das Körpergewicht um 1 Uhr nachmittags (Anfang und Ende jedes einzelnen Versuchstages) möglichst genau kennen zu lernen, so habe ich zu seiner Berechnung folgendes Verfahren eingeschlagen. Nenne ich:

das Morgengewicht um 7 Uhr	(direkt bestimmt)	a
die Einnahme von 7—1 Uhr	(„ „)	b
(Harn + Excremente von 7—1 Uhr)	(„ „)	c
Perspiration von 7—1 Uhr		d*)
Mittleres Körpergewicht (um 1 Uhr)		x,

so berechnet sich letzteres nach der Formel: $(a + b) - (c + d) = x$.

Nachdem der Kranke, wie bereits angegeben, am 29. Oktbr. 1862 in die medicinische Abtheilung der Klinik aufgenommen war, und die beiden folgenden Tage in einem der Krankensäle mit andern Patienten zusammen verbracht hatte, bezogen wir am Abende des 31. Oktobers das mir zum Zwecke der Beobachtung angewiesene Zimmer. Dasselbe lag einen Stockwerk höher als die übrigen zur Aufnahme von Patienten bestimmten Krankensäle der medicinischen Klinik. Schon dadurch war eine Isolirung meines Patienten von den andern Kranken bewirkt. Unser Zimmer wurde ausser von dem Direktor, dem Assistenten und der diensthuenden, durchaus zuverlässigen Wärterin nur noch ab und zu in den Nachmittagsstunden (von 3—5 Uhr) von der Frau des Patienten besucht. Dieselbe musste auf einem mitten in das Zimmer gestellten Stuhle Platz nehmen, so dass bei meiner steten Gegenwart eine verbotene Zufuhr von Lebensmitteln ganz unmöglich gemacht war.

Obgleich wir schon vom Abende des 31. Oktobers eine gleiche Lebensweise eingehalten haben, ist die genaue Führung des Tagebuchs doch erst mit dem 1. Novbr. begonnen worden. Von dem zweiten Novbr. nahm auch die Bestimmung der Harnbestandtheile aus der 24 stündigen Menge ihren Anfang, mit welchem Tage, den ich als 1. Versuchstag bezeichne, auch die nachfolgend mitgetheilte, tabellarische Uebersicht beginnt.

An dem 22. und 23. Novbr. war ich verhindert, die Beobachtung persönlich fortzusetzen. Statt meiner überwachte mein Studiengenosse, der Dr. W. Weyrich, in ähnlicher Weise wie ich es selbst gethan hatte, den Kranken und besorgte auch die wichtigsten Bestimmungen. Die Vergleichung mit einem gesun-

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*) Die Perspirationsgrösse von 7—1 Uhr ist der mit 6 multiplizirte Quotient der Tagesperspiration (von 7 Uhr morgens bis 11 Uhr abends) durch 16. Am ersten Versuchstage (2. Novbr.) ist das Körpergewicht um 1 Uhr nachmittags durch direkte Wägung gefunden worden.

den Individuum fiel in dieser Zeit fort. Ich selbst war inzwischen bemüht eine, meiner bisherigen, möglichst nahekommende Lebensweise einzuhalten. Um 1 Uhr des 24. Novembers nahm ich die Beobachtung in der frühern Weise wieder auf und setzte sie bis zum Schlusse des Semesters fort.

Der Fragestellung entsprechend sind die nachfolgend beschriebenen 39 Versuchstage in 3 Gruppen su bringen:

1) I. Periode (15 tägig) vom 2. bis inclusive 16. Novbr. — kein arzeneilicher Gebrauch.

2) II. Periode (19 tägig) unter dem Gebrauche von doppelt-kohlensaurem Natron

a) täglich 7,464 grammes (2 Drachmen) vom 17. bis incl. 26. Novbr.

b) täglich 8,196 grammes (3 Drachmen) vom 27. Novbr. bis incl. 5. Decbr.

3) III. Periode unter dem Gebrauche von benzoësaurem Natron vom 6. bis incl. 10. Decbr. (am ersten Tage 2, an den übrigen 4 Tagen 3 Drachmen).

Von diesen drei Perioden soll mit Rücksicht auf bereits angedeutete äussere Gründe in dem Nachfolgenden nur die erste einer nähern Betrachtung unterzogen und demgemäss nur die erste der den Gang der Untersuchung bestimmenden Fragen eingehender beantwortet werden. Nichtsdestoweniger werden alle Anhaltspunkte für die Beurtheilung, ob und in welcher Weise die während der beiden letzten Perioden in Gebrauch gezogenen Arzeneien auf das für die erste festgestellte Verhältniss zwischen dem Stoffwechsel des Gesunden und des Diabetikers modificirend eingewirkt haben, gegeben werden, während die Verwerthung jener zu Schlüssen gleichzeitig mit der Mittheilung des noch restirenden Versuchsmaterials einer andern Gelegenheit vorbehalten wird. (Hier folgen die Tabellen.)

beobac…

beider … St…

№ des Versuchstages.	Excremente. G.	Excremente. D.	Harn. G.	Harn. D.	Harnstoff. G.	Harnstoff. D.	Kreatinin = Hippursäure = … G.	Kreatinin = Hippursäure = … D.
1	60	230	1971,3	3668,8	35,720	59,840		
2	80	160	2548,1	3015,7	57,768	61,404		
3	107	45	2609,6	5287	43,428	73,876		
4			2442,8	3121,8	53,391	50,745		
5	192	120	2086,2	2928,3	45,210	60,027		
6	133	62	2214,6	3275,6	55,808	72,105		
7		103	2006,9	3249,6	48,190	59,090		
8			1230	2950	48,640	72,192		
9	65	322	2268	3298,8	55,304	73,944		
10	175		2311,2	3496	64,922	65,562		
11	155	90	1834,2	3924,3	57,240	79,712		
12	60	758	2866	3935,3	62,150	77,220		
13	87	776	1630,1	3292,8	59,148	71,077		
14	247	465	1731,8	2412,3	56,100	80,325		
15	85	280	2011,5	3115,5	53,595	68,144		
S.	1446	3411	34776,2	51971,8	796,614	1025,3		
16	135	15	1997,7	2505,2	52,662	62,964		
17		238	2306,7	2452,6	45,955	45,396		
18	130	385	1824,6	2857	44,640	47,766		
19	185		2055,2	2591,8	44,421	51,480		
20	90		1389,4	2510,5	36,720	54,720		
21		110		2048,4		39,500		
22				3713,5		56,248		
23	205		756,6	3140,4	31,017	58,800		
24	123		901,3	3629,5	40,020	53,856		
25	45		1460,6	3483,8	40,608	67,367		
26		293	2182,2	2814,6	39,680	54,439		
27	168	232	1363,6	2433,9	39,383	52,192		
28	160		1642,6	2811,1	46,368	57,673		
29	125	175	1199	2607,8	40,950	54,000		
30	165	325	1494,4	2825,2	48,180	62,832		
31	190		967,7	2214,9	37,036	60,588		
32	155	360	1615,2	2490,2	42,390	47,223		
33	142		1862	2872,7	50,960	53,447		
34	120	205	1558,3	3108,4	48,800	57,909	k 0,943	1,1…
S.	2138	2338	26577,1	53111,5	729,790	1038,400	k 0,943	1,1…

II. Mittheilung der Parallelbeobachtungen.

A. Tabellarische Uebersicht über die EINNAHME beider Versuchspersonen in 24 Stunden.

№ des Versuchstages.	Wasser. G.	D.	Milch. G.	D.	Kaffee. G.	D.	Thee. G.	D.	Zucker. G.	D.	Rahm. G.	D.	Butter. G.	D.	Weissbrod. G.	D.	Roggenbrod. G.	D.	Gebeuteltes Roggenbrod. G.	D.	Fleischbrühe. G.	D.	Fleisch. G.	D.	Summe der Einnahmen. G.	D.
1	840		1160										30		350						694		200		3274	
2	846		1160										30		350						608		270		3264	
3	1266		1160										30		350						608		278		3692	
4	840		1160										30		400						608		326		3564	
5	1305	1515	1160										30		300				200		608		300		3903	4113
6	870	1080	1160										30		300				200		608		260		3428	3638
7	870	1080	1160										30		300				200		608		300		3468	3678
8	435		1160										30		300				200		608		320		3053	
9	885		1160										60		350				200		608		630		3893	
10	675	1065	1160										30		300				200		608		498		3471	3881
11	675		1160										60		300				200		608		551		3554	
12	1290		1160										60		300				200		608		456		4074	
13	240		1160										30		300				300		608		478		3116	
14	450	660	1160										30		300				200		608		215		2963	3173
15	855				332		331		10		94		30		300				200		608		530		3290	
S.	12342	13602	16240		332		331		10		94		540		4800				2300		9206		5612		51807	53067
16	966				332		331		50	20	94		30		250				200		608		521		3382	3362
17	1157				332		331		50	20	94		30		300				200		608		283		3385	3355
18	603				332		331		50	20	94		30		300				200		608		370		2918	2888
19	726				332		331		60	20	94		30		300				200		608		363		3044	3004
20	306				332		331		60	20	112		30		300				200		608		387		2666	2626
21		486				332		331		60		94		30		300				300		608		325		2866
22		276				332		331		60		94		30		300				400		608		313		2744
23	788				332		331		60	20	94		10	30	200	300			300		608		110	270	2853	3073
24	578				332		331		60	20	94		30		300				300		608		368		3001	2961
25	578				332		331		60	20	94		30		300			200	300	100	608		362		2995	2955
26	396				332		331		60	20	112		30		300		100		100		562		353		2676	2636
27	558				332		331		60	20	112		30		300		200				608		329		2860	2820
28	659				332		331		60	20	94		30		300		100		100		578		450		3034	2994
29	372				332		331	662	60	20	112		30		300		100		100		334		385		2456	2415
30	279				332		331		60	20	112		30		300		100		100		608		449		2701	2661
31	489				332		296		50	20	94		30		300		100		100		608		425		2824	2794
32	885				332		331		60	20	10[illegible]		60		325		100		100		608		465		3367	3327
33	210				332		331		60	20	9[illegible]		60		300		100		100		608		493		2688	2648
34	210				332		331		60	20	94		75		300		100		100		608		524		2834	2794
S.	9750	10512			5644	5976	5000	6585	960	470	1626	1838	595	675	4975	5675	1000	1200	2700	3200	9986	11202	6637	7435	49664	54923
35	308				332		331		60	20	94		60		300		100		100		608		562		2855	2815
36	372				332		331		60	20	112		60		300				100				461		2128	2088
37	792				332		331		60	20	94		45		250		300				1216		488		3908	3868
38	582				332		331		60	20	94		45		300		100		100		608		613		3165	3125
39	582				332		331		60	20	94		45		325				100				508		2377	2337
S.	2636				1660		1655		300	100	488		225		1475		500		400		2432		2632		14433	14233

№ des Versuchstages.	Wasser. G.	D.	Trockne Substanz. G.	D.	Mineral-Salze. G.	D.	Trockne organische Substanz. G.	D.	Kohlenstoff. G.	D.	Wasserstoff. G.	D.	Stickstoff. G.	D.	Sauerstoff. G.	D.	Schwefel. G.	D.
1	2759,63		514,37		26,66		487,71		261,21		37,76		24,05		163,75		0,93	
2	2719,79		544,21		26,73		517,48		278,57		40,15		27,84		169,75		1,18	
3	3144,12		547,88		26,87		521,01		280,62		40,43		28,29		170,46		1,21	
4	2761,85		602,15		28,41		573,74		307,28		44,11		31,57		189,26		1,42	
5	3257,86	3467,86	645,14		29,02		616,12		325,52		46,60		31,05		211,57		1,38	
6	2801,22	3011,22	626,78		28,28		598,50		315,29		45,20		28,78		207,99		1,34	
7	2822,86	3032,86	645,14		29,02		616,12		325,52		46,60		31,05		211,57		1,38	
8	2398,68		654,32		29,39		624,93		330,63		47,30		32,19		213,36		1,45	
9	3035,59		857,41		35,83		821,58		446,89		63,43		50,34		258,32		2,60	
10	2734,98	3144,98	736,02		32,69		703,34		376,13		53,55		42,30		229,27		2,09	
11	2765,09		788,91		33,72		755,19		412,19		58,66		45,31		236,75		2,28	
12	3328,70		745,30		31,96		713,34		387,90		55,33		39,91		228,26		1,94	
13	2329,46		786,54		33,50		753,04		397,91		56,55		42,19		254,31		2,08	
14	2356,87	2566,87	606,13		27,45		578,68		303,79		43,62		26,22		203,97		1,07	
15	2646,03		643,97		25,58		618,39		328,63		45,72		37,48		204,35		2,21	
S.	41862,73	43112,73	9944,27		445,10		9499,17		5078,18		725,01		518,57		3152,94		24,56	
16	2734,40		647,60	627,60	24,757		622,843	602,843	328,689	320,269	45,987	44,697	36,417		209,61	199,32	2,14	
17	2814,40		570,60	540,60	21,007		549,593	519,593	282,349	269,709	39,627	37,697	23,447		202,85	187,42	1,32	
18	2307,47		610,53	580,53	22,607		587,923	557,923	304,589	291,959	42,687	40,747	28,397		210,62	195,19	1,63	
19	2426,68		617,32	577,32	22,487		594,833	554,833	307,019	290,169	43,077	40,507	27,997		215,13	194,55	1,61	
20	2032,39		633,61	573,61	23,037		610,573	570,573	316,869	300,009	44,467	41,897	29,447		218,10	197,53	1,69	
21		2206,42		659,58		22,967		636,613		324,199		45,447		26,867		238,56		1,54
22		2030,23		713,77		23,937		689,833		348,719		48,027		27,217		264,32		1,55
23	2355,63	2475,67	477,37	594,33	17,637	21,957	459,733	572,373	225,259	293,269	31,737	40,947	13,557	23,747	188,48	213,08	0,70	1,33
24	2321,69		679,31	639,31	23,767		655,543	615,543	335,189	318,329	46,937	44,387	29,507		242,41	221,84	1,88	
25	2318,44		676,56	636,56	23,657		652,903	612,903	333,659	316,799	46,747	44,177	28,967		241,87	221,30	1,86	
26	2035,31		620,69	580,69	21,837		598,853	558,853	309,649	292,789	43,477	40,907	27,477		216,68	196,11	1,57	
27	2245,21		614,79	574,79	22,127		592,663	552,663	305,539	288,679	42,917	40,347	26,277		216,45	195,88	1,48	
28	2375,65		660,35	620,35	23,747		636,603	596,603	330,829	313,969	46,347	43,777	32,027		224,58	204,01	1,92	
29	1826,69	1830,80	629,31	584,20	19,167	19,004	610,143	565,196	316,419	297,258	44,387	41,444	28,787	28,594	218,86	196,21	1,69	
30	2033,03		665,97	625,97	24,267		641,703	601,703	334,469	317,609	46,887	44,317	33,027		225,40	204,83	1,92	
31	2184,34		639,66	609,66	23,706		615,954	585,954	320,401	307,761	44,855	42,925	31,576		217,292	201,862	1,83	
32	2652,23		714,77	674,77	24,887		689,903	649,903	366,049	349,189	51,357	48,787	34,187		236,33	215,76	1,98	
33	1978,55		709,45	669,45	25,027		684,423	644,423	364,529	347,669	51,127	48,557	35,437		231,25	210,68	2,08	
34	2096,04		737,96	697,76	25,627		712,333	672,333	383,719	366,859	53,817	51,247	37,197	37,197	235,41	214,84	2,19	
S.	38758,15	43121,95	10905,85	11801,05	380,328	440,389	10516,522	11360,661	5465,225	5955,212	766,457	830,838	504,428	568,509	3751,322	3973,292	29,09	32,81
35	2113,88		741,12	701,12	26,307		714,813	674,813	382,159	365,299	53,657	50,987	39,357		237,42	216,85	2,32	
36	1507,73		620,27	580,27	15,587		605,683	565,683	327,629	310,769	46,067	43,497	31,277		198,81	178,24	1,90	
37	3163,70		744,30	704,30	34,297		710,003	670,003	371,679	354,819	52,047	49,477	37,077		247,10	226,53	2,10	
38	2414,75		750,25	710,25	27,217		723,033	683,033	383,949	367,089	53,707	51,137	42,257		240,61	220,04	2,51	
39	1738,60		638,40	598,40	15,637		622,763	582,763	331,939	315,079	46,527	43,957	34,137		208,08	187,51	2,08	
S.	10938,7		3494,3	3294,3	118,045		3376,295	3176,295	1797,355	1793,055	251,905	230,055	184,105		1132,02	1029,17	10,91	

B. Tabellarische Uebersicht über die AUSGABEN beider Versuchspersonen in 24 Stunden.

№ des Versuchstages	Excremente G.	Excremente D.	Harn G.	Harn D.	Harnstoff G.	Harnstoff D.	Kreatinin = k. Hippursäure = h. G.	Kreatinin = k. Hippursäure = h. D.	Zucker D.	Kochsalz G.	Kochsalz D.
1	60	230	1971,3	3668,8	35,720	59,840			259,25	10,904	24,640
2	80	160	2548,1	3015,7	57,768	61,404			178,95	16,434	13,416
3	107	45	2609,6	5287	43,428	73,876			416,12	12,608	22,264
4			2442,8	3121,8	53,391	50,745			255,29	15,878	19,100
5	192	120	2086,2	2928,3	45,210	60,027			252,04	12,535	13,464
6	133	62	2214,6	3275,6	55,808	72,105			267,91	10,900	12,540
7		103	2006,9	3219,6	48,190	59,090			271,32	9,480	12,440
8			1230	2950	48,640	72,192			231,24	9,600	10,716
9	65	322	2268	3298,8	55,304	73,944			281,95	15,387	13,508
10	175		2311,2	3496	64,922	65,562			260,91	14,074	16,725
11	155	90	1834,2	3924,3	57,240	79,712			276,82	17,172	19,552
12	60	758	2866	3935,3	62,150	77,220			288,71	15,820	20,020
13	87	776	1630,1	3292,8	59,148	71,077			274,10	13,038	10,064
14	247	465	1731,8	2412,3	56,100	80,325			290,98	11,560	13,837
15	85	280	2011,5	3115,5	53,595	68,144			260,03	9,925	9,132
S.	1446	3411	34776,2	51971,8	796,614	1025,3			4065,6	195,110	231,419
16	135	15	1997,7	2505,2	52,662	62,964			203,13	12,183	8,963
17		238	2306,7	2452,6	45,955	45,396			218,66	12,285	7,020
18	130	385	1824,6	2857	44,640	47,766			183,95	14,040	6,913
19	185		2055,2	2591,8	44,421	51,480			202,94	10,050	8,415
20	90		1389,4	2510,5	36,720	54,720			147,72	11,424	11,040
21		110		2048,4		39,500			109,00		11,850
22				3713,5		56,248			258,61		16,376
23	205		756,6	3140,4	31,017	58,809			274,41	6,615	12,600
24	123		901,3	3629,5	40,020	53,856			205,84	8,004	9,842
25	45		1460,6	3483,8	40,608	67,367			258,32	11,562	18,676
26		293	2182,2	2814,6	39,680	54,439			238,35	12,000	14,553
27	168	232	1363,6	2433,9	39,383	52,192			187,86	10,947	9,320
28	160		1642,6	2811,1	46,368	57,673			181,43	12,719	11,589
29	125	175	1199	2607,8	40,950	54,000			160,90	10,764	14,000
30	165	325	1494,4	2825,2	48,180	62,832			179,81	11,096	15,708
31	190		967,7	2214,9	37,036	69,588			186,09	8,648	7,293
32	155	360	1615,2	2490,2	42,390	47,223			153,95	19,159	12,402
33	142		1862	2872,7	50,950	53,447			187,50	17,856	15,428
34	120	205	1558,3	3108,4	48,800	57,909	k 0,943	1,105	208,38	13,725	17,313
S.	2138	2338	26577,1	53111,5	729,790	1038,400	k 0,943	1,105	3746,85	202,787	227,301
35	140		1369,8	3305,9	44,946	61,056	k 1,120 h 1,135	1,140 1,526	188,99	8,626	15,264
36		185	1244,5	3697,9	48,158	67,450	k 0,873 h 0,702	0,962 1,420	254,01	10,043	17,750
37	215		1769,4	2913,5	50,689	53,664	k 0,865 h 2,352	1,020 1,342	196,71	18,338	13,416
38	130	777	1694,7	3014	52,960	58,378	k 0,941 h 1,523	1,100 2,312	190,57	15,557	17,340
39	118		1510,1	3702,3	47,040	69,161	k 1,130 h 1,470	1,070	203,72	9,408	22,816
S.	603	962	7488,5	16633,6	243,793	309,709	k 4,931 h 7,182	4,222 7,670	1034,00	61,972	86,586

№ des Versuchstages	Phosphorsäure gebunden an Alkalien G.	gebunden an Alkalien D.	gebunden an Kalk G.	gebunden an Kalk D.	gebunden an Magnesia G.	gebunden an Magnesia D.	Summe derselben G.	Summe derselben D.	Schwefelsäure G.	Schwefelsäure D.	Summe der sensiblen Ausgaben G.	Summe der sensiblen Ausgaben D.	Wasser G.	Wasser D.
1	1,936	3,641	0,363	0,460	0,257	0,387	2,556	4,488	2,106	2,907	2031,3	3898,8	1927,18	3446,84
2	2,814	2,580	0,329	0,280	0,354	0,238	3,497	3,098	2,938	2,941	2628,1	3175,7	2489,94	2841,38
3	2,792	4,301	0,512	0,116	0,359	0,086	3,663	4,503	3,102	5,313	2716,6	5332	2593,94	4758,16
4	2,463	3,561	0,479	0,468	0,329	0,322	3,271	4,351	2,694	3,284	2442,8	3121,8	2358,8	2784,8
5	2,055	2,469	0,411	0,437	0,281	0,303	2,748	3,209	2,774	3,086	2278,2	3048,3	2122,62	2676,06
6	1,202	2,822	0,416	0,486	0,303	0,332	1,921	3,640	3,270	3,511	2347,6	3337,6	2217,78	2969,78
7	2,370	3,483	0,363	0,479	0,255	0,332	2,988	4,294	2,923	3,421	2006,9	3352,6	1922,9	2965,34
8	1,920	3,254	0,198	0,358	0,140	0,259	2,257	3,971	2,556	3,102	1230	2950	1160	2632
9	2,566	4,274	0,395	0,437	0,274	0,299	3,235	5,010	2,765	3,972	2333	3620,8	2215,11	3106,45
10	2,724	3,613	0,416	0,525	0,293	0,361	3,433	4,499	3,541	4,181	2486,2	3496	2332,6	3119
11	2,988	4,136	0,272	0,579	0,191	0,406	3,551	4,715	3,339	4,970	1989,2	4014,3	1823,64	3567,62
12	3,447	4,183	0,517	0,551	0,364	0,382	4,328	5,116	5,085	4,504	2926	4693,3	2804,88	4040,48
13	1,972	3,648	0,249	0,494	0,178	0,340	2,488	4,482	3,498	3,774	1717,1	4068,8	1595,48	3456,65
14	1,972	3,713	0,294	0,516	0,211	0,358	2,477	4,587	2,788	4,118	1978,8	2877,3	1810,85	2332,62
15	2,442	3,278	0,300	0,378	0,218	0,268	2,960	3,924	1,985	2,861	2096,5	3395,5	1991,58	2953,94
S.	35,663	53,056	5,514	6,564	4,007	4,673	45,184	64,293	45,364	55,945	36222,2	55382,8	34272,2	48179,13
16	2,314	2,237	0,310	0,351	0,212	0,238	2,836	2,926	2,751	2,528	2132,7	2520,2	2001,18	2228,92
17	2,321	2,457	0,343	0,232	0,248	0,222	2,912	2,911	2,730	2,574	2306,7	2690,6	2220,7	2319,82
18	1,926	2,179	0,331	0,329	0,234	0,226	2,491	2,734	2,520	2,095	1954,6	3242	1822,84	2854,48
19	2,412	2,512	0,205	0,389	0,221	0,262	2,838	3,163	2,412	2,129	2240,2	2591,8	2094,08	2288,8
20	1,537	2,698	0,307	0,372	0,182	0,255	2,026	3,315	1,986	2,928	1479,4	2510,5	1369,72	2233,5
21		1,900		0,355		0,190		2,445		2,015		2158,4		1906,68
22		2,418		0,559		0,338		3,315		3,702		3713,5		3311,5
23	1,661	2,412	0,132	0,471	0,087	0,180	1,880	3,063	1,103	3,050	961,6	3140,4	831,44	2772,4
24	1,107	2,458	0,162	0,417	0,124	0,282	1,393	3,157	1,792	2,772	1024,3	3629,5	916	3312,5
25		2,915		0,480		0,337		3,732		4,002	1505,6	3483,8	1401,77	3107,8
26	1,632	2,505	0,285	0,461	0,200	0,512	2,117	3,478	1,920	1,695	2182,2	3107,6	2110,2	2680,46
27	1,360	2,039	0,251	0,394	0,183	0,277	1,894	2,710	1,869	2,610	1531,6	2665,9	1299,46	2321,24
28	1,644	2,453	0,285	0,426	0,209	0,323	2,138	3,202	2,576	3,773	1802,6	2811,1	1662,28	2514,1
29	1,207	2,188	0,190	0,362	0,135	0,272	1,532	2,822	1,872	2,930	1324	2782,8	1195	2457,2
30	1,480	2,329	0,261	0,454	0,190	0,314	1,931	3,097	2,336	3,899	1659,4	3150,2	1514,32	2732,8
31	1,084	1,800	0,180	0,362	0,132	0,252	1,396	2,414	1,551	2,644	1157,7	2214,9	1020,82	1960,9
32	1,685	1,920	0,265	0,372	0,195	0,277	2,145	2,569	2,198	2,923	1770,2	2850,2	1598,64	2466,48
33	1,878	2,262	0,233	0,452	0,209	0,314	2,380	3,028	2,929	3,223	2004	2872,7	1845	2576,7
34	1,620	2,525	0,262	0,479	0,184	0,346	2,066	3,350	2,181	3,731	1678,3	3313,4	1549,06	2926,24
S.	26,868	44,297	4,062	7,717	2,945	5,417	33,875	57,431	33,726	55,253	28715,1	55449,5	26552,5	48971,52
35	1,285	2,740	0,193	0,455	0,148	0,318	1,626	3,513	1,861	3,244	1409,8	3305,9	1278,52	2985,9
36	2,713	3,028	0,207	0,390	0,145	0,387	3,065	3,805	1,972	3,550	1244,5	3882,9	1154,5	3451,78
37	1,848	2,376	0,272	0,481	0,199	0,252	2,319	3,109	2,458	3,522	1984,4	2913,5	1805,72	2611,5
38	1,821	2,384	0,265	0,433	0,194	0,335	2,280	3,152	2,317	2,919	1824,7	3791	1679,94	3205,5
39	1,607	2,860	0,254	0,510	0,184	0,392	2,045	3,762	2,102	3,565	1628,1	3702,3	1484,56	3352,3
S.	9,274	13,388	1,191	2,269	0,870	1,684	11,335	17,341	10,710	16,800	8091,5	17595,6	7403,2	15607,0

№ des Versuchstages	Trockne Substanz G.	Trockne Substanz D.	Mineral-Salze G.	Mineral-Salze D.	Trockne organische Substanz G.	Trockne organische Substanz D.	Kohlenstoff G.	Kohlenstoff D.	Wasserstoff G.	Wasserstoff D.	Stickstoff G.	Stickstoff D.	Sauerstoff G.	Sauerstoff D.
1	104,12	451,96	49,77	61,346	54,350	390,615	17,068	153,710	3,828	26,874	17,959	32,845	15,493	177,0
2	138,16	334,32	56,552	44,286	81,608	290,034	24,785	110,324	4,772	19,901	28,684	32,086	23,367	127,72
3	122,66	573,84	46,010	69,873	76,650	503,967	26,384	188,668	5,486	33,718	22,560	35,458	22,220	240,12
4	84	337	30,946	30,965	53,391	306,035	10,678	112,260	3,524	20,359	24,934	23,698	14,255	149,7
5	155,58	372,24	50,749	22,916	104,831	349,324	40,800	132,674	7,604	28,793	25,292	30,590	31,135	162,2
6	129,82	367,82	32,714	8,556	97,106	359,264	33,159	131,845	6,944	24,138	28,896	34,993	28,107	168,2
7	84	387,26	35,910	24,869	48,090	362,391	9,638	137,584	3,181	24,503	22,505	29,789	12,866	170,5
8	70	318	21,360	14,568	48,640	303,432	9,728	106,928	3,210	20,186	22,715	33,713	12,987	142,6
9	117,89	514,35	42,405	58,476	75,485	455,874	21,812	180,828	5,242	31,569	27,211	41,290	21,220	202,0
10	158,6	377	39,334	50,528	119,266	326,472	41,929	117,473	8,579	21,717	34,047	30,617	34,711	156,6
11	165,56	446,68	60,194	62,205	105,366	384,475	37,085	141,538	7,575	25,916	30,032	39,142	30,674	177,8
12	121,12	652,82	40,340	51,532	80,780	601,288	22,354	256,297	5,572	42,918	30,262	52,205	22,552	249,8
13	121,62	612,15	35,460	27,025	86,160	585,125	26,220	252,205	6,034	41,973	29,475	49,723	24,431	241,2
14	167,95	544,68	35,157	28,994	132,793	515,686	32,074	209,366	9,753	36,083	31,461	47,416	39,505	222,8
15	104,92	441,56	24,934	26,436	79,986	415,124	24,778	163,950	5,618	28,697	26,840	37,787	22,750	186,0
S.	1950,00	7203,67	432,39	606,27	1517,62	6597,4	399,024	2395,492	87,994	422,277	402,826	551,441	356,275	2779,0
16	131,52	291,28	36,942	20,530	94,578	270,750	32,861	96,324	6,783	18,063	27,468	29,723	27,466	126,6
17	86	370,78	40,045	33,836	45,955	336,944	9,191	135,904	3,033	23,497	21,461	26,269	12,270	151,7
18	131,76	387,52	46,755	36,255	85,005	351,265	30,430	146,812	6,132	24,853	23,616	30,567	24,827	149,0
19	146,12	303	44,250	48,580	101,870	254,420	39,483	91,466	7,472	16,928	24,684	24,041	30,231	121,9
20	109,68	277	45,017	74,560	64,663	202,440	22,230	70,034	4,628	13,461	19,065	25,554	18,740	90,9
21		251,72		69,065		182,655		69,694		12,572		20,789		79,6
22		402		87,142		314,858		114,690		20,952		26,268		152,9
23	130,16	368	35,492	34,790	94,668	333,210	40,110	121,520	7,069	22,179	18,851	27,469	28,638	162,0
24	108,3	317	30,087	57,304	78,213	259,696	28,348	93,111	5,656	17,274	21,310	25,151	22,809	124,1
25	103,84	376	49,261	50,313	54,579	325,687	13,564	116,803	3,782	21,666	19,922	31,461	15,311	155,7
26	72	427,14	32,320	43,278	39,680	383,862	7,936	154,690	2,618	26,760	18,531	31,663	10,595	170,5
27	132,14	344,66	40,594	32,574	91,546	312,086	35,663	123,951	6,715	21,658	21,970	29,315	27,198	137,0
28	140,32	297	44,236	57,897	96,048	239,103	35,737	84,105	6,980	15,906	25,062	26,933	28,269	112,1
29	129	325,6	49,236	66,076	79,764	259,524	28,865	94,385	5,765	18,589	21,787	28,945	23,347	117,6
30	145,08	417,4	45,669	73,844	99,411	343,556	36,927	138,241	7,221	24,099	26,015	36,268	29,248	144,9
31	136,88	254	40,852	7,322	96,028	246,678	38,833	86,558	7,098	16,408	21,343	28,295	28,754	133,6
32	171,56	383,72	81,046	70,767	90,516	312,953	34,115	130,568	6,594	22,196	23,098	29,722	26,709	130,6
33	159	297	63,951	56,053	95,049	240,947	33,678	85,690	6,842	16,027	26,822	24,960	27,707	114,2
34	129,24	387,16	32,242	56,121	86,998	331,039	30,008	129,306	6,218	22,802	25,696	31,819	25,076	147,1
S.	2162,6	6477,98	431,87	556,636	1730,69	5921,344	499,579	2093,106	100,558	375,610	386,341	534,735	407,147	2946,1
35	131,28	320	40,598	68,328	90,682	251,672	33,310	88,731	6,524	16,707	24,481	28,632	26,367	117,31
36	90	431,12	40,195	46,291	49,805	384,829	10,457	147,015	3,272	26,062	22,897	35,962	13,179	175,79
37	178,68	302	57,863	48,585	120,817	253,415	47,593	90,707	8,804	16,790	28,850	26,149	35,570	119,79
38	144,76	585,5	48,939	92,020	95,821	493,480	33,383	218,225	6,912	35,766	27,946	44,351	27,580	286,18
39	143,54	350	57,527	75,028	86,013	274,972	30,179	96,402	6,124	18,262	24,917	32,762	24,793	127,46
S.	688,26	1988,62	129,960	193,300	558,3	1795,32	154,921	641,528	31,536	113,659	149,09	166,690	127,489	752,84

C. Tabellarische Uebersicht über die mittlere Temperatur u. s. w.

№ des Versuchstages.	Mittlere Temperatur: des Zimmers n. R.	Mittlere Temperatur: des Körpers n. C.		Mittlere Pulsfrequenz.		Mittlere Respirationsfrequenz.		Körpergewicht.		Spec. Gewicht des Harns.		Reaktion des Harns.	
	G. / D.	G.	D.	G.	D.	G.	D.	G.	D.	G.	D.	G.	D.
1	17	37,33	36,175	76	56	20	15,33	54658	49068	1019	1043	Sauer	Sauer
2	16,54	37,7	35,94	90	56,33	20	15,33	54756	48595	1019	1044	"	"
3	16,06	37	36,2	80	59,5	20	15	54804	48845	1014	1045	"	"
4	14,9	37,2	36,03	90,5	51,96	18	15	54414	47724	1015	1046	"	"
5	15,75	37,2	36,083	82	55	17	15,16	55426	48477	1018	1048	"	"
6	15,86	37,16	36,01	82,6	53,42	19	14,71	54563	47494	1016	1045	"	"
7	15,52	37,1	36,11	77	52,14	22	18	54503	47181	1018	1046	"	"
8		36,9	36,23	76	68	20	17	54676	47078	1025	1046	"	"
9	14,82	37,06	36,12	75,5	64	20,6	20,5	55705	47241	1018	1046	"	"
10	15,08	37,06	36,06	80	60	23,3	19,6	55393	46685	1018	1046	"	"
11	15,6	37,06	36,16	76	61,3	20	19,16	55293	46836	1024	1045	"	"
12	15,97	37,26	37,12	80	69,14	18,3	18,57	55717	46580	1015	1044	"	"
13	15,95	37,06	37,08	78,6	84	19,3	17,28	55617	45988	1025	1044	"	"
14	15,74	37,06	36,4	76,6	66,2	17,6	19,4	56302	45940	1020	1046	"	"
15	15,74	36,8	36,18	66	57	18	17,6	55867	45706	1016	1047	neutral	"
Mittel	15,75	37,13	36,26	79,12	69,32	19,54	17,17	55179,6	47362,53	1018,6	1045,4		
16	16,1	37,13	36,16	72,6	59,2	20	17,4	55695	45983	1018	1049	neutral	sauer
17	15,6	36,9	36,3	80	55,3	16,5	13,8	55832	46477	1016	1050	sauer	sauer
18	16,06	37	36,3	80	55	18	14,25	56124	47090	1020	1049	"	schw. sauer
19	15,3	37,15	36,2	82	51	21	15,7	55937	47571	1017	1050	alkalisch	alkal.
20	14,6	37	36,2	82	46	18,5	17	55561	48721	1024	1047	ueutral	neutral
21	16		36,7		60,26		15,6		48860		1044		"
22	16,05		36,65		59		15		50033		1046		"
23	15,7	38,45	36,75	87	56,5	19,2	17,5	54551	49471	1033	1050	schw. sauer	"
24	15,6	38,4	36,8	90	54,6	18,3	16,6	55280	49630	1031	1049	sauer	neutral
25	14,67	37,8	36,9	86,5	51	17,5	17,5	55731	49913	1026	1046	"	"
26	14,8	37,7	36,65	89,1	54	17,2	15,75	55853	50061	1019	1049	"	alkal.
27	14,5	37,4	36,4	80,6	49,3	20,3	15	55295	49514	1023	1046	"	neutral
28	15,37	37,35	36,35	83,0	49,5	19,7	17,2	55573	49647	1022	1045	"	"
29	14,17	37,2	36,7	78,6	53,3	18,6	16	55321	50115	1026	1043	"	"
30	14,17	37,25	37,25	83	63	16	17,25	55299	49846	1025	1044	neutral	"
31	14,8	37,13	36,9	69,3	60,5	18,3	15,5	55388	49416	1031	1049	sehr schw. sauer	sehr schw. sauer
32	15,05	37,05	36,57	82,5	58	18,25	16,25	55406	49976	1031	1044	schw. alkal.	neutral
33	15,3	37,05	36,95	87	62	18	16	56189	51723	1025	1044	sehr schw. sauer	"
34	15,55	37,05	36,8	80,5	58,5	17,75	16,25	55629	52231	1024	1043	sauer	sehr schw. sauer
Mittel	15,24	37,34	36,65	81,98	55,57	18,41	16,08	55686,12	49277,8	1024,17	1046,68		
35	15,2	37,15	36,65	80	58,25	16,25	16,25	55441	51607	1030	1041	sauer	neutral
36	15,2	36,93	36,66	77,3	58	17,66	15,66	55822	51063	1031	1042	"	schw. sauer
37	15,12	37,15	36,65	80	63	17,25	15,75	55477	49231	1025	1044	schw. sauer	"
38	15,12	37,15	36,55	85	56	17	14	56075	50941	1025	1044	sauer	"
39	15,7	37,15	36,9	85	57	16,75	15,75	56365	50792	1029	1040	"	sehr schw. sauer
								55471	49327				
Mittel	15,26	37,1	36,68	81,46	58,45	16,98	15,48	55836	50726,8	1028	1042,2		

D. Das subjective Verhalten
beider Versuchspersonen betreffende Bemerkungen.

I. Periode.

3. Versuchstag. G. Bald nach dem Niederlegen zur Nachtruhe unruhiger Schlaf; später ruhiges Einschlafen und um 7 Uhr Morgens Erwachen bei vollkommenem Wohlbefinden.

D. Patient behauptet weniger an Durst zu leiden. In der Nacht guter Schlaf.

6. Versuchstag. G. Spätes Einschlafen.

Während der Nachtruhe will D. gegen Morgen leichtes Frösteln gespürt haben. Auch am Tage klagt er über leichtes Kältegefühl und hält sich mit Vorliebe in der Nähe des Ofens auf.

7. Versuchstag. G. Spätes Einschlafen.

8. Versuchstäg. G. Am Abende allgemeine Abspannung. Guter Schlaf.

D. Klagen über Ermattung und Kälte in den Beinen.

10. Versuchstag. G. Am Abende nehmen der Gesunde und der Kranke ein halbstündliches, warmes Bad.

D. Das Gefühl des Durstes, behauptet Patient, ist fast ganz geschwunden; dagegen reiche die Kost nicht zur vollkommenen Stillung seines Hungers aus. Der Harn ist frei von Eiweiss.

12. Versuchstag. D. Die Klagen betreffen stets die Ermattung der Beine, namentlich in der Gegend der Kniee. Das Oedem an den Fussknöcheln unverändert. Die Mahlzeiten sollen nur für kurze Zeit das Gefühl der Sättigung schaffen. Im Uebrigen Wohlbefinden.

14. Versuchstag. Während der Nachtruhe, gegen Morgen, ist D. von Frösteln befallen worden; gleichzeitig empfand

er Schmerz in der Magengrube, welcher nach einer copiösen Stuhlentleerung um 10 Uhr Morgens schwand. Nach dem Frühstück um 10 Uhr 30 Minuten stellt sich dieser Schmerz aufs Neue ein. Der Patient legt sich aufs Bett. Bei der Besichtigung erscheint die Magengrube aufgetrieben und ist gegen Druck empfindlich. Um 1 Uhr legt sich der Patient, der inzwischen aufgestanden war, abermals aufs Bett, sieht sehr collabirt aus, stöhnt leise; lautes Gurren im Unterleibe. Es erfolgen mehrere flüssige Stühle..

15. Versuchstag. Am Nachmittage liegt Patient auf dem Bette. Die Haut fühlt sich trocken und heiss an. In der Nacht folgt guter Schlaf. Er verbringt den folgenden Vormittag grösstentheils auf dem Bette liegend, sieht sehr blass und collabirt aus. Im Laufe dieses Versuchstages erfolgen 5 flüssige Stühle.

II. Periode.

16. Versuchstag. D. In der Nacht guter Schlaf. Ab und zu Schmerzen in der Magengrube und dem Unterleibe. Es erfolgen mehrere breiige Stühle.

17. Versuchstag. D. Am Nachmittage ist relatives Wohlbefinden zurückgekehrt. Stuhlentleerung fest.

18. Versuchstag. D. Es wird nur über das Gefühl der Abspannung in den untern Gliedmaassen geklagt. In der letzten Nacht Zahnschmerzen.

19. Versuchstag. D. Wohlbefinden. Patient meint durch die häufigern Mahlzeiten (die er jetzt ohne Steigerung der Quantität in kürzeren Pausen erhält) von dem Hungergefühl befreit zu sein.

20. Versuchstag. G. Spaziergang. Warmes Bad. In der Nacht unruhiger Schlaf, beim Erwachen Wohlbefinden.

D. Patient fühlt sich besonders wohl „wie ein Gesunder.“ Das Oedem an den Fussknöcheln unverändert. Am Nachmittage um 4 Uhr 30 Minuten Spaziergang in mässigem Schritt bis 5 Uhr 10 Min. Patient behauptet sehr erfrischt zu sein, ist aber ermüdet, darauf bis 5 Uhr 30 Min. ein warmes Bad.

21. und 22. Versuchstag. D. Guter Schlaf. Unveränderter Zustand.

23. Versuchstag. G. Kopfschmerzen und Frösteln. Am folgenden Morgen sind die Kopfschmerzen geschwunden, aber ein Gefühl allgemeiner Unbehaglichkeit noch vorhanden. Appetit fehlt. Statt der Lösung von Natr. bicarbon. werden entsprechende Quantitäten von Wasser aufgenommen.

D. erklärt wiederholt, dass er garkeinen Durst spüre. Die früher sehr trocken erscheinenden Excremente gleichen seit dem Natrongebrauch mehr denen des Gesunden.

24. Versuchstag. G. Die Abendmahlzeit wird nur mit Widerwillen aufgenommen. An Stelle der Natron-Lösung Wasser.

D. Das Oedem in der Gegend der Fussknöchel hat inzwischen in dem Maasse gewonnen, dass es sich über den Fussrücken und nach aufwärts bis zum obern Dritttheile des Schienbeins hinzieht.

25. Versuchstag. G. Kein Natron.

D. Das Oedem erreicht bereits das Knie. Im subjektiven Befinden keine Veränderung.

26. Versuchstag. G. Das Gefühl der Unbehaglichkeit ist vollkommen geschwunden; Appetit vorhanden. Kein Natron.

D. Der rechte Unterschenkel erscheint stärker angeschwollen als der linke. In der Gegend des tuber calcanei hat sich eine ziemlich tiefe, schmerzhafte Schrunde gebildet, aus welcher sich eine serös-eitrige Flüssigkeit ausdrücken lässt. Schlaf und Allgemeinbefinden gut.

27. und **28.** Versuchstag. G. Statt Natron-Lösung Wasser.

29. Versuchstag. An Stelle der Natron-Lösung nimmt G. entsprechende Mengen von Wasser.

D. klagt, in der Nacht einen Schmerz in der Magengrube und über die Brust hin gehabt zu haben. Der Schmerz hat sich am Morgen gegeben; dagegen ist ein Gefühl allgemeiner Ermüdung zurückgeblieben: Pat. „fühlt seine Arme garnicht.“ Husten nicht vorhanden, Frösteln nicht gespürt worden. Die Unterschenkel, namentlich der rechte, noch bedeutender ödematös geschwellt, so dass der Fingerdruck tiefe und dauernde Dellen hinterlässt. Die Schrunde am Fuss ist unverändert und schmerzhaft. Bei Bewegungen stellt sich ein unbestimmtes Gefühl von Schmerz im Knie- und Fussgelenk ein.

30. Versuchstag. G. Natron-Gebrauch.

31. Versuchstag. Natron-Gebrauch bis zum **35.** Versuchstage.

D. Guter Schlaf. Pat. klagt über Schmerz in der Magengrube; diese ist gegen Druck empfindlich, gespannt. Wegen Steigerung des Schmerzes in der Nacht wird ein Senfteig auf die Magengrube gelegt, wonach Linderung eintritt.

33. Versuchstag. D. Das Oedem der Unterschenkel nimmt ab, erreicht nicht mehr das Kniegelenk. Die Schrunde am Fuss unverändert.

34. Versuchstag. D. Patient behauptet vollständig gesättigt zu werden.

III. Periode.

35. und **36.** Versuchstag. D. Relatives Wohlbefinden. Patient behauptet vollständig gesättigt zu werden.

37. Versuchstag. G. Die qualitative Untersuchung des Harns beider Versuchspersonen auf Hippursäure ergiebt in bei-

den Harnproben eine reichliche Menge von Hippursäure-Krystallen.

D. Das Oedem des rechten Unterschenkels hat wieder ein wenig zugenommen. Patient behauptet sich wohl zu fühlen; nur das Gefühl von Schwere und Steifigkeit in den Beinen sei noch vorhanden. Gesättigt werde er vollständig.

38. Versuchstag. Patient fühlt sich vollständig wohl, nur das Gefühl von Steifigkeit in den untern Fxtremitäten verliert sich nicht. Gesättigt wird er, wie er behauptet, vollständig. Er fordert auffallend wenig Getränk, obgleich, wie er wiederholt erklärt, er ganz seinem Durstgefühle folge. Das Oedem der untern Extremitäten erstreckt sich von dem Fussrücken bis unter das Knie hin. Rechterseits ist es bedeutender. Die Perkussion weist keine Abnormitäten, mit Ausnahme der in der Einleitung angeführten, nach. Die Auskultation ermittelt träges Eindringen der Luft, vollständige Durchgängigkeit der Luftwege, prolongirte Exspiration, welche an der ganzen, vordern, obern Brusthälfte deutlich wahrzunehmen ist. Die Herztöne sind rein und schwach hörbar, der Puls leer, leicht comprimirbar. Die Leber misst in der Breite circa 7 Ctm. und überragt den Rippenbogen nur unbedeutend. Die Bauchdecken sind gespannt, gegen Druck nicht empfindlich. Der Patient erscheint abgemagert, die Haut trocken, die Schleimhäute blass, die fauces leicht geröthet. Der Athem des Patienten hat einen eigenthümlichen Geruch, wie etwa nach Obst.

39. Versuchstag. D. Keine Veränderung.

III. Verwerthung der Parallelbeobachtungen.

1) Wie verhält sich der Stoffwechsel eines Diabetikers zu dem eines Gesunden, der denselben Bedingungen unterworfen ist, in einem Zustande, welcher weder absolute Ruhe noch Bewegung genannt werden kann?

Die Beantwortung dieser Frage wird zunächst auf Grundlage der an den ersten 15 Versuchstagen gewonnenen Resultate versucht werden müssen. Zu dem Zwecke summire ich die Einnahmen und Ausgabrn und stelle die dabei erhaltenen Summen in folgenden Tabellen übersichtlich zusammen:

I. Periode. 15 Tage.

A. Gesunder.

Anfangsgewicht = 55008 grm.

Endgewicht = 55697 grm.
Gewichtsdifferenz + 689

Gesammt- A. Einnahme B. Ausgabe a) Harn b) Excremente B'. Summe d. Ausgabe	Wasser.	Trockne Substanz.	Organische trockne Substanz.	Mineralsalze.	Kochsalz.	Phosphorsäure geb. an Alkal.	Phosphorsäure an 3 CaO	Phosphorsäure an 2 MgO	Phosphorsäure Summe der PO_5	SO_3 Aequiv. des S.	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff, s. geb. an S.
A. 51807	41862,73	9944,27	9499,17	445,10						65,100	24,56	5078,18	725,01	518,57	3152,94
B. a) 34776,2 {796,614 Harnst.	33335,2	1441	1068,6	372,4	195,110	35,663	5,514	4,007	45,184	45,364	18,14	159,324	52,574	372,026	212,690 s. 27,224
b) 1446	937	509	448,99	60,01						1,20	0,45	239,7	35,42	30,80	143,6
B'. 36222,2	34272,2	1950	1517,59	432,41	195,110	35,663	5,514	4,007	45,184	46,564	18,59	399,024	87,994	402,826	383,514

B. Diabetiker.

Anfangsgewicht = 49598 grm.

Endgewicht = 45984 grm.
Gewichtsdifferenz — 3614

	Wasser.	Trockne Substanz.	Organische trockne Substanz.	Mineralsalze.	Kochsalz.	geb. an Alkal.	an 3 CaO	an 2 MgO	Summe der PO_5	SO_3 Aequiv. des S.	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff, s. geb. an S.
A. + 53057	43112,73	9944,27	9499,17	445,10	—	—	—	—	—	65,100	24,56	5078,18	725,01	518,57	3152,94
B. a) 5197,18 {Ü 1025,3 / Zucker 4065,6}	45968,8	6003	5538,3	464,7	231,419	53,056	6,564	4,673	64,293	55,945	22,38	205,052 1626,24	67,667 271,04	478,801	273,743 33,565 s 2168,32
b) 3411	2210,328	1200,672	1059,172	141,5						3,0	1,20	564,2	83,57	72,64	338,7
B'. 55382	48179,128	7203,672	6597,472	606,2	231,419	53,056	6,564	4,673	64,293	58,945	23,58	2395,492	422,277	551,441	2814,328

Aus diesen Tabellen berechnet sich die Grösse der Einnahmen und Ausgaben beider Versuchspersonen für den mittleren Versuchstag der I. Periode in folgender Weise:

A. Gesunder.

Körpergewichtsdifferenz + 45,9.

	Wasser.	Trockne Substanz.	Organische trockne Substanz.	Mineralsalze.	Kochsalz.	geb. an Alkal.	an 3 CaO	an 2 MgO	Summe der PO_5	SO_3 Aequiv. des S.	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff, s. geb. an S.
A. 3453,8	2790,8	663	633,3	29,7						4,3	1,6	338,6	48,3	34,6	210,2
B. 2314,8	2284,8	130	101,2	28,8	13,01	2,378	0,367	0,267	3,012	3,021	1,2	26,6	5,9	26,9	25,6
Rest	506											312	42,4	7,7	184,6

B. Diabetiker.

Körpergewichtsdifferenz — 353,3.

	Wasser.	Trockne Substanz.	Organische trockne Substanz.	Mineralsalze.	Kochsalz.	geb. an Alkal.	an 3 CaO	an 2 MgO	Summe der PO_5	SO_3 Aequiv. des S.	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff, s. geb. an S.
A. 3537	2874	663	633,3	29,7						4,3	1,6	338,6	48,3	34,6	210,2
B. 3692,1	3211,9	480,2	439,8	40,4	15,43	3,537	0,437	0,312	4,286	3,9	1,57	159,7	28,2	36,76	187,6
Rest	—337,9											178,9	20,1	—2,19	22,6

A. Hiernach gestaltet sich die Stoffwechsel-Bilanz für den **Gesunden** während der ersten Versuchs-Periode in folgender Weise:

1) G. scheidet 7,7 grm. N weniger aus, als er in den Nahrungsmitteln erhalten hat[1]; er muss daher täglich Nhaltiges Körpergewebe angesetzt haben. 7,7 grm. N entsprechen 47,8 grm. trockener, diese 209,3 grm. frischer Muskelsubstanz[2]), deren Zusammensetzung mit derjenigen der übrigen Körper-Albuminate ziemlich übereinstimmt. Das Anfangsgewicht der Versuchsreihe beträgt 689 grm. weniger als das Endgewicht, woraus sich eine tägliche Gewichtszunahme von 45,9 grm. berechnet. Der Körper muss daher gleichzeitig mit dem Ansatze von Nhaltigem Gewebe einen Verlust an anderem Material z. B. Fett oder Wasser erlitten haben. Aus weiter unten zu erörternden Gründen ist der Verlust an Wasser am wahrscheinlichsten. Jene, dem Körper angebildeten, 47,8 grm. trockner Muskelsubstanz enthalten 25,3 grm. C, ferner 3,4 grm. H, 10,9 grm. O, 0,4 grm. S, welche neben den 7,7 grm. N dem Oxydationsprocesse entzogen wurden. Endlich

1) Ich bemerke hiezu, dass sowol hier als auch an andern Stellen dieser Arbeit, die sich auf den Kreislauf des N beziehen, meine Schlüsse den zuerst von Bidder und C. Schmidt (die Verdauungssäfte und der Stoffwechsel, Mitau und Leipzig 1852 pag 386) später von Voit (physiolog.-chem. Untersuchungen 1857) und von Bischoff und Voit (die Gesetze der Ernährung des Fleischfressers, Leipzig und Heidelberg 1860.) für den Fleischfresser vertheidigten und von J. Ranke (a. a. O.) auch für den Menschen sehr wahrscheinlich gemachten Satz, zur Voraussetzung haben, dass der N der Einnahmen (mag er nun zum Theil schon innerhalb der Blutbahn in den Atomcomplex des Harnstoffs übergegangen sein — Bidder und Schmidt — oder jedesmal zuvor einen integrirenden Bestandtheil des Körpergewebes gebildet haben — Bischoff und Voit) nur in dem Harn und in den Excrementen an die Aussenwelt zurückkehrt. Vernachlässigt ist die in Harnsäure, Hippursäure, Kreatinin enthaltene N-Menge, als verschwindende und noch weit in den Bereich der Fehlergrenzen fallende Grösse.

2) Dieser Berechnung sowie den folgenden liegen die Angaben von C. Schmidt (a. a. O. pag. 301, 302 und 303) zu Grunde.

erfordern 209,3 grm. frischer Muskelsubstanz 147 grm. Wasser, 2,52 grm. anorganischer Salze, darunter namentlich 0,825 grm. PO_5.

2) Von den überschüssigen 42,4 grm. H wurden 3,4 grm. zur Bildung Nhaltigen Körpergewebes verwendet. 5,2 grm. H gehen, wie ich gleich angeben werde, höchst wahrscheinlich in die Zusammensetzung neuangebildeten Fettgewebes ein. Der Rest von 31,8 grm. H bildet mit 254,4 grm. O 286 grm. Wasser. Dazu werden die noch überschüssigen 169,3 grm. O der Nahrungsmittel [182,9 — (10,9 zur Muskelgewebsbildung + 4,4 zur Fettgewebsbildung)] verwendet. Der Rest von 85,1 grm. O wird der Inspirations-Luft entnommen.

3) Der Ueberschuss von 0,4 grm. S geht gerade in die Zusammensetzung des neuangebildeten Nhaltigen Körpergewebes auf. Durch die Excremente konnte jedenfalls nur eine verschwindend geringe Menge (0,03[1])) von S ausgeführt werden.

4) Von der überschüssigen C-Menge = 312 grm. sind die in die Zusammensetzung der 47,4 grm. trockner Muskelsubstanz eingegangenen 25,3 grm. abzuziehen, der Rest von 286,7 grm. müsste zur CO_2-Bildung dienen. Diese täglich der Oxydation zu CO_2 anheimgefallene C-Menge liegt allerdings noch unter dem durch directe Untersuchungen von Andral und Gavarret[2]) und auf indirektem Wege von Barral[3]) gefundenen Werthen[4]). Eine mit dem Pettenkoferschen Ap-

1) Dieser Berechnung ist der mittlere S-Gehalt menschlicher Excremente (2,13°/₀ SO_3) nach den Untersuchungen von Porter und Fleitmann zu Grunde gelegt. Vergl. v. Gorup-Besanez, Lehrbuch der physiologischen Chemie. Braunschweig 1862 pag. 500.

2) v. Gorup-Besanez am a. O. pag. 700.

3) a. a. O. pag. 157.

4) Erstere geben die täglich zur CO_2-Bildung verwendete C-Quantität für junge Männer von 20—24 Jahren (mein Alter) auf 292,8 grm. an, letzterer fand

parate von J. Ranke bei **überschüssiger**, Creicher Nahrung angestellte Untersuchung ergab indessen als Maximum der Ausscheidung durch die Respiration 252,4 grm. C[1]). Nehme ich diese Zahl auch für mich als das Maximum der C-Ausscheidung durch die Respiration an, so wären noch 34,3 grm. von dem 286,7 grm. betragenden C-Reste in Abzug zu bringen, welche der Organismus zur Fettbildung verwendet haben müsste. Diese Annahme entspricht, mit dem nothwendig zu postulirenden Ansatze von Nhaltigem Gewebe zusammengehalten, ganz den Erfahrungen über Mästung überhaupt. Die 34,3 grm. C entsprechen 43,9 grm. wasserfreien und 53,1 grm. frischen Fettgewebes, welche zu ihrer Bildung ausserdem 5,15 grm. H, 4,4 grm. O, 0,069 grm. anorgan. Salze, (darunter 0,016 PO_5) und 7,3 grm. Wasser beanspruchen. Zur Oxydation der 252,4 grm. C sind 673,2 grm. O der Respirationsluft erforderlich, mittelst welcher 925,4 grm. CO_2 gebildet werden.

5) Das Perspirationswasser setzt sich aus folgenden Posten zusammen.

a. 506 grm. Wasser-Rest der Einnahme.
b. 286 grm. aus der Oxydation des überschüssigen H hervorgegangen.
c. 62,2 grm. zur Balancirung des Körpergewichts angenommene Wasserabgabe.

854,2 grm.

6) Die O-Einnahme durch die Respiration beträgt daher im Ganzen (673,2 zur CO_2-Bildung + 85,1 zur Wasserbildung) 758,3 grm.

sie bei sich selbst (29 Jahr alt) 335 grm. Nach **Scharling** beträgt die in 24 Stunden exspirirte C-Menge eines erwachsenen, gesunden Mannes 236,5 grm., nach **Valentin** und **Brunner** (vergl. pag. 47) 264,5 grm.

1) a. a. O. pag. 365 — Ranke war 24 Jahr alt und 70 Kilogramm schwer.

B. Die Stoffwechsel-Bilanz des Diabetikers berechnet sich folgendermaassen:

1) Die 2,19 überschüssig ausgeschiedenen grm. N entsprechen 13,59 grm. trockener Muskelsubstanz, welche täglich dem regressiven Stoffwechsel anheimfielen. Auf frische Muskelsubstanz berechnet, würde der tägliche Verlust an Nhaltigem Körpergewebe 59,54 grm. betragen haben. Der durch Wägung constatirte Körpergewichtsverlust berechnet sich aber auf 353,3 grm. für den Tag. Demnach muss noch ausserdem ein Verlust von 295,5 grm. durch den Zerfall von Fettgewebe oder durch die Abgabe von Wasser stattgefunden haben; letztere erscheint auch hier wahrscheinlicher. Der Zerfall von 13,59 grm. trockener Muskelsubstanz macht ausser dem N auch noch 7,2 grm. C zu weiterer Verwendung disponibel, ferner 0,95 grm. H, 0,14 grm. S, 3,1 grm. O und 45,95 grm. Wasser, neben anorganischen (0,72 grm.), namentlich phosphorsauren Salzen (0,235 grm. PO_5).

2) Die 21,1 grm. H (20,1 grm. Ueberschuss der Einnahme + 0,95 grm. aus dem Zerfall der Muskelsubstanz hervorgegangen) erfordern 168,8 grm. O, welche mit ihnen 189,9 grm. Wasser bilden. Dazu werden der Rest von 22,6 grm. + 3,1 grm. (aus dem zerfallenen Muskelgewebe) und 143,1 grm. atmosphärischen O's verwendet.

3) Für den Stoffwechsel wurden 1,7 grm. S (Einnahme + S-Aequivalent des zerfallenen Muskelgewebes) disponibel. In Form von SO_3 des Harns sind 1,5 grm. ausgeschieden worden. Der Ueberschuss von 0,2 grm. muss daher durch die Excremente ausgeführt sein[1]).

1) Auf Grundlage der früher benutzten Angabe über den procentischen S-Gehalt menschlicher Excremente darf allerdings nur die Ausfuhr von 0,1 grm. S durch den Darm erwartet werden.

4) Das Wasser für die Perspiration entstammt folgenden Quellen:

a. 295,8 grm. aus dem Körpergewichts-Verluste gerechnet.
b. 45,95 grm. durch den Zerfall von Muskelsubstanz.
c. 189,9 grm. durch Oxydation des disponiblen H.

531,7 grm.

Von dieser Summe ist aber noch die im Harn und den Excrementen als Ueberschuss über die Wassereinnahme erschienene Wassermenge von 337,9 grm. in Abzug zu bringen, so dass für die Perspiration durch Lungen und Haut noch 193,6 grm. übrig bleiben.

5) Die 186,1 grm. überschüssigen C's (Rest der Einnahme = 178,9 + 7,2 C-Aequivalent des zerfallenen Muskelgewebes) fordern zur CO_2-Bildung 496,3 grm. O, welche der atmosphärischen Luft entnommen werden und 682,4 grm. CO_2 bilden.

6) Die Einnahme an atmosphärischem O beläuft sich demnach in summa auf 639,4 grm.

Nach dem, was ich darüber in der Einleitung gesagt habe, brauche ich nicht besonders darauf aufmerksam zu machen, dass ich sowol in den bisherigen als auch in den folgenden Ausführungen mir stets dessen bewusst bin, dass ich vielfach Grössen, die nicht direkt ermittelt worden sind, zu meiner Rechnung verwende. Ich kann daher nichts dagegen einzuwenden haben, wenn man meine Zahlen in anderer Weise deutet, als es mir am wahrscheinlichsten vorkommt. Nichtsdestoweniger werden aus einem jeden andern Erklärungsversuche die charakteristischen Differenzen zwischen dem Stoffwechsel beider Versuchspersonen, und diese in übersichtlicher Weise zur Anschauung zu bringen ist die Aufgabe dieses Abschnitts (vergl. die Fragestellung), in fast unveränderter Form hervorgehen müssen.

Die nähere Betrachtung des elementaren Stoffkreislaufs beider Versuchspersonen, welche hier selbstverständlich nur die Anfangs- (Aufnahme) und Schluss-Glieder desselben (Ausscheidung durch Lungen und Haut, Nieren und Darm), mit Uebergehung der Zwischenglieder, (intermediärer Kreislauf) berücksichtigen kann, führt zu folgenden Schlüssen.

I. Diffusionskreislauf. Kreislauf des präformirten Wassers.

G. scheidet in Harn und Excrementen 506 grm. weniger Wasser aus, als er in Getränken und Speisen zu sich genommen hat. Die Wassereinnahme gleich 1 gesetzt, erhält man für das Wasser der sensiblen Ausgabe 0,8187 oder für das Harnwasser allein 0,7961, was nach den bisherigen Erfahrungen über den Einfluss gesteigerter Wasseraufnahme im physiologischen Zustande eine verhältnissmässig geringe Wasserausscheidung anzeigt, abgesehen davon, dass bekanntlich in dieser Beziehung selbst innerhalb physiologischer Grenzen bedeutende Schwankungen vorkommen[1]). Dagegen stimmt dieses Verhältniss sehr gut mit den von Reich[2]) erhaltenen Resultaten an einem Gesunden,

1) Vergl. darüber: Böcker, Untersuchungen über die Wirkung des Wassers. Nov. Act. acad. Leop.-Carol. vol. 24 1854. pag. 342 u. ff. Genth, Untersuchungen über den Einfluss des Wassertrinkens auf den Stoffwechsel. Wiesbaden 1856. Mosler, Untersuchungen über den Einfluss des innerlichen Gebrauchs verschiedener Quantitäten von gewöhnlichem Trinkwasser. Arch. f. wiss. Heilk., Bd. III, Göttingen 1858 pag. 398. Andersohn, Beiträge zur Kenntniss der nichtzuckerführenden Harnruhr. Inaugural-Dissert. Dorpat, 1862 pag. 23. Falck, Beiträge zur Kenntniss der Wirkungen des Wassers, Arch. f. phys. Heilkunde Bd. XII 1853 pag. 150. Kaupp, Beiträge zur Physiologie des Harns Arch. f. phys. Heilk. 1856 pag. 555. Donders, Physiologie des Menschen; übersetzt von Theile Bd. I Leipzig 1859 pag. 486. Beneke, Studien zur Urologie Arch. f. wiss. Heilk. 1858 Bd. I pag. 418 und 576. Ferber, Der Einfluss vorübergehender Wasserzufuhren auf Menge und Kochsalzgehalt des Urins. Arch. der Heilkunde 1860 pag. 244, u. A. m.

2) Reich, De diabete mellito quästiones, dissert. inaugural. Gryphiae 1859.

der sich zur Vergleichung mit zwei Diabetikern einer übermässigen Wasserzufuhr neben gesteigerter Nahrungsaufnahme unterwarf, überein. Auch dieser Gesunde schied verhältnissmässig wenig Wasser im Harne aus (Wasseraufnahme zu Harnwasser = 1 : 0,75) und nahm dabei an Körpergewicht zu, sowie ich, während u. A. Böcker, Andersohn, Genth an Körpergewicht einbüssten.

D. scheidet in Harn und Excrementen 337,9 grm. mehr Wasser aus, als er in der Einnahme präformirt erhalten hat. Die Wassereinnahme = 1 gesetzt, berechnet sich die Verhältnisszahl für das Wasser der sensiblen Ausgabe auf 1,117, für das Harnwasser auf 1,066. Es muss also nach J. Vogel eine Täuschung vorliegen, sei es nun durch einen Beobachtungsfehler, oder absichtlich von Seiten des Kranken. Denn „im Grossen uud Ganzen,“ sagt [1]) dieser Forscher, beträgt die Menge der genossenen Flüssigkeiten (Getränk und flüssige Speisen zusammen) immer mehr als die Quantität des entleerten Urins. Dazu soll man nicht bloss durch theoretische Betrachtungen geführt werden, sondern es soll auch durch neuere, genaue Untersuchungen (Nasse, Griesinger u. A.) bewiesen sein. Da nun, wie ich voraussetze, der mittlere Tag einer 15 tägigen Beobachtungsreihe in die Kategorie des „Grossen und Ganzen“ fällt, so scheint nichts anderes übrig zu bleiben, als in unsrem Falle eine Täuschung anzunehmen. Diese könnte entweder in einer nicht zu meiner Kenntniss gelangten Aufnahme von Wasser durch den Kranken geschehen sein, oder in einer unrichtigen Berechnung des Wassergehalts der Nahrung oder der Ausgaben. In Bezug auf den ersten Punkt wird zugestanden werden können, dass die zur Ueberwachung des Kranken getroffenen Vorsichts-

1) J. Vogel, Diabetes mellitus. Handbuch der spec. Pathol. und Therap., redig. v. Virchow Bd. VI Abtheilung 2 Erlangen 1856—65 pag. 480.

massregeln anbetreffend mich kein Vorwurf der Versäumniss treffen dürfte. Die Berechnung des Wassergehalts der Einnahmen anlangend müsste, im Falle er zu niedrig veranschlagt wäre, die feste Substanz zu hoch angegeben sein. Die Menge der Mineralsalze ist nun aber, wie es die einfache Betrachtung der Stoffwechselbilanz des Gesunden und die Erwägung, dass die zur Zubereitung der Speisen verwendete Kochsalzquantität garnicht berücksichtigt worden ist, lehren, ganz gewiss nicht zu hoch gegriffen. Auch die trockene org. Substanz möchte keinen irgendwie erheblichen Abzug erfahren dürfen, was aus dem numerischen Verhältniss der einzelnen Elemente und aus der Art, wie letztere für die Bilanz in Verwendung kommen, hervorgeht. Bei der Ausgabe sind die in Rechnung gebrachten Mineralsalze des Harns, deren Menge gegenüber die nach einer Angabe über den mittlern Salzgehalt der Excremente berechnete Salzquantität der letztern ganz verschwindet, direkt bestimmt [1]) (KO, SO_3 + Phosphors. Salze + $ClNa$); es dürfte daher, um den Wassergehalt der Ausgabe herabzudrücken nur die organische Substanz einen Zuwachs erfahren. Diese ist für den Harn aus dem specif. Gewicht desselben berechnet worden; direkt bestimmt sind daraus für den Diabetiker Harnstoff und Zucker. Zieht man die Summe derselben von der trocknen Substanz — Mineralsalzen ab, so ergiebt sich als Rest für Kreatinin, Hippursäure und Extraktivstoffe 23,2 grm. pr. Tag, was verglichen mit den Beobachtungen von Becquerel und Scherer (11,7 resp. 24 grm. Extraktivstoffe) keine zu niedrige Zahl zu sein scheint. Da der Ueberschuss der Wasserausgabe über die Wassereinnahme schon im Harne zu Tage tritt, so ist es überflüssig den Wassergehalt der Excremente besonders ins Auge zu fassen (vergl. darüber die Einleitung pag. 17).

1) Wenigstens die SO_3, welche ganz an KO gebunden berechnet ist; die an Alkal. gebundene PO_5 ist an $NaO 2 HO$ gebunden berechnet. 3 CaO- und 2 MgO, PO_5 sind direkt bestimmt.

Trotz alledem dürfte ich nicht anstehen eine Täuschung einzuräumen, wenn die in Rede stehende Frage in der That in dem Vogelschen Sinne theoretisch und experimentell bereits entschieden wäre. Sehen wir zu, ob dies der Fall ist. Vogel selbst sagt auf S. 495 seiner ausgezeichneten Arbeit über den Diabetes mellitus [1]), dass das concentrirte (durch Zucker) Blutserum der Diabetiker auf endosmotischem Wege mit grosser Begierde aus allen Parenchymflüssigkeiten, und den in den Magen und Darm eingeführten Getränken und flüssigen Speisen Wasser an sich ziehe. „Jemehr das Blut Wasser anzieht, desto grösser wird sein Volumen, die Blutgefässe werden überfüllt und damit entsteht ein gesteigerter Blutdruck innerhalb des Gefässsystems. Dieser wirkt aber vorzugsweise auf die Nieren, es entsteht Polyurie." Er hat somit den Process der vermehrten Harnabsonderung im Diab. mellit. in das gebührende Abhängigkeitsverhältniss zu dem physikalischen Vorgange einer gesteigerten Endosmose gesetzt. Dieses Abhängigkeitsverhältniss kann durch äussere Umstände, durch eine dem Wasseräquivalente der im Blutserum gelösten Zuckermenge nicht ganz entsprechende Wasserzufuhr von aussen, nicht beliebig abgeändert werden. Bewirkt wirklich das concentrirte Blutserum der Diabetiker einen vermehrten Eintritt von Wasser in das Gefässsystem und in Folge dessen gesteigerte Harnabsonderung, woran garnicht zu zweifeln ist, so muss letztere auch fortbestehen, so lange als die Zuckerbildung ihren Fortgang nimmt. Fehlt eine genügende Wasserzufuhr von aussen, so wird das Wasser der Parenchymflüssigkeiten, das Hydratwasser der zerfallenen Gewebe, und das durch die Oxydation des freiwerdenden H-Aequivalents derselben gebildete Wasser den fehlenden Rest des endosmotischen Wasseräquivalents decken müssen. Die gesteigerte

1) a. a. O.

Harnsekretion bleibt immer abhängig von der Anhäufung von Zucker im Blut; nur bei übermässiger Wasseraufnahme ist dies nicht mehr zu erkennnn. Soll ich dafür noch Beweise anführen, so sind sie in einer Beobachtung von Voit am Hunde über den Einfluss des Kochsalzes enthalten. Er [1]) fand die Harnwassermenge nach Darreichung bestimmter ClNa-Quantitäten constant vermehrt, und zwar proportional den letztern. Erhielt der Hund kein Wasser zum Getränk so wurde dieses Verhältniss nur unbedeutend verändert. Es trat dennoch eine mit der ClNa-Menge steigende Menge von Wasser im Harn auf; welches zuerst dem sonst durch die Respiration ausgeschiedenen entzogen wurde, später selbst dem Körper.

Aber auch Vogel [2]) giebt eine vorübergehende, überschüssige Ausscheidung von Harnwasser zu; die Harnbildung soll dann theilweise auf Kosten des in den Flüssigkeiten und festweichen Theilen des Körpers enthaltenen Wassers erfolgen. „Ein solches Verhältniss kann höchstens einige Tage, ohne gefährliche Zufälle, ja den Tod herbeizuführen, anhalten." Die Zahlen meiner Tabelle über den mittlern Versuchstag des Diabetikers, wenn sie auch sonst nichts beweisen sollten, liefern zum wenigsten ein Schema, wie ein Ueberschuss der Wasserausgabe über die Einnahme selbst längere Zeit bestehen kann, ohne dass die wichtigsten Funktionen des Organismus in irgend erheblicher Weise beeinträchtigt zu werden brauchen. Erst dann, wenn die oben von mir bezeichneten Ersatzquellen der Wasserzufuhr, sei es in Folge übermässiger Hektik absolut zu spärlich fliessen, oder nur relativ nicht ausreichen, um einen bedeutendern Ausfall an Getränk zu ersetzen (wie in dem

1) Voit, Untersuchungen über den Einfluss des Kochsalz u. s. w. auf den Stoffwechsel. München. 1866 pag. 55.

2) a. a. O. pag. 480.

bekannten Versuch von Griesinger[1]), absichtliche Wasserentziehung), stehen gefährliche Zufälle nothwendiger Weise in Aussicht.

„Durch neuere, genaue Untersuchungen (Fr. Nasse, Griesinger u. A.)“ soll indessen eine Entscheidung dieser Frage bereits herbeigeführt sein. Sehe ich von den unter Griesingers Leitung angestellten Untersuchungen von Ott[2]), und von Guenzler[3]) (letztere kenne ich nur aus dem Referate), welche trotzdem, dass ich an der Richtigkeit der erhaltenen Resultate gar nicht zweifele, diese Frage doch nicht in dem Sinne von Vogel entscheiden können, ab, so bleibt meines Wissens keine andere von Griesinger in diesem Interesse gemachte Beobachtung übrig, als der bereits oben erwähnte Versuch einer absichtlichen Wasserentziehung. Dieser Versuch beweist aber nicht nur nicht, dass im Grossen und Ganzen eine Mehrausgabe von Wasser nicht möglich ist, sondern dass an den beiden letzten Versuchstagen eine Mehrausgabe ganz gewiss stattgefunden hat, und dass, weil die Wasserentziehung eine sehr bedeutende war (der Kranke bekam weniger als die Hälfte seines gewöhnlichen Getränkequantums) die Ersatzquellen des Organismus für das entzogene Wasser den Rest des endosmotischen Wasseräquivalents des producirten Zuckers nicht decken konnten, und daher nothwendig allgemeines Unwohlsein eintreten musste.

Bei der Berufung auf Nasse hat Vogel offenbar dessen Arbeit „über Wasserbildung im Diabetes[4])“, im Auge. Dessen

1) Griesinger, Studien über Diabetes. Arch. f. phys. Heilk. 1858. Bd. XVIII pag. 72.

2) A. Ott, Beiträge zur Theraphie der Zuckerharnruhr, Inaugural-Dissertat. Tübingen 1857.

3) A. Günzler, Ueber diabetes Mellitus. Inauguralabhandlug Tübingen 1856 und Virchows Arch. Bd. XII pag 341.

4) Fr. Nasse, Archiv. f. physiol. Heilkunde. Stuttgart 1851. pag. 72.

sehr genau angestellte Untersuchungen beweisen aber in der That nur, dass bei Diabetikern bei einer reichlichen (die beiden Pat. erhielten nach Belieben Wasser, ungefähr 4—5400 grm. täglich) oder nur dem endosmotischen Wasseräquivalente der im Organismus gebildeten Zuckermenge entsprechenden Wasserzufuhr keine Mehrausgabe von Wasser im Harne vorkommt. Wer wird aber auch glauben, dass es Diabetiker gebe, denen eine Mehrausgabe von Wasser unter allen Umständen zur Gewohnheit geworden ist.

Ich habe nun ausser diesen noch die meisten anderen, einer Beachtung werthen, Arbeiten der Neuzeit über den Stoffwechsel im Diab. mell. einer nähern Betrachtung unterzogen[1]), ohne mich von der Richtigkeit des Vogel'schen Ausspruches überzeugen zu können. Aus allen diesen Fällen hebe ich hier nur die von Reich[2]) angestellte Untersuchung hervor, weil sie von Niemeyer[3]) ebenfalls als Gegenbeweis gegen die Möglichkeit eines Wasserüberschusses der Ausgaben über die Einnahmen verwendet zu werden scheint. Auch hier werden die Diabetiker geradezu mit Wasser überschwemmt. Hätten die Pat. das Unmögliche geleistet, und mehr Wasser ausgeschieden als sie erhielten, so träfe in diesem Falle den krankhaften Process der Zuckerbildung wahrlich keine Schuld. In diesem Sinne dürfte man einem zu prüfenden Arzeneimittel diuretische Eigenschaften absprechen, wenn nach einer, gleichzeitig mit dem Arzeneigebrauche erfolgten, überreichen Wasseraufnahme nicht mehr Wasser ausgeschieden wird, als die eingenommene Wassermenge betrug.

1) Eine diesen Gegenstand betreffende Arbeit von mir ist im Manuscripte beinahe vollendet.

2) a. a. O.

3) Niemeyer, Lehrbuch der speciellen Patholagie und Therapie. Berlin 1862. Bd II pag. 750.

Bisher haben wir in der gesteigerten endosmotischen Anziehung von Wasser in das Gefässsystem die Ursache kennen gelernt, welche im Diab. mell. von theoretischer Seite den Vorgang einer Mehrausgabe von Wasser gegenüber der Einnahme unter ungenügender Zufuhr von aussen (durch Getränk, Speisen) möglich erscheinen lässt, ja ihn geradezu fordert. Diese Ursache allein würde, meiner Ansicht nach, zu seiner Erklärung vollständig ausreichen[1]). Der Zucker, so stellte ich mir vor, ganz der Anschauung von J. Vogel folgend, bewirkt eine vermehrte Wasserabscheidung durch die Nieren mittelst Steigerung der endosmotischen Wasseranziehung in das Gefässsystem. Eine specifische, die Urinsekretion vermehrende, also im strengen Sinne diuretische Wirkung des Zuckers zu supponiren, hielt ich, mit Vogel, für überflüssig. Die Untersuchungen von Weikart[2]), haben mich aber eine Eigenschaft des Traubenzuckers (Harnzuckers) kennen gelehrt, die ich mir aus seinem Vermögen die Wasser-Endosmose in das Gefässsystem d. h. den Wassergehalt und den Seitendruck des Bluts zu steigern nicht allein zu erklären vermag. Nach denselben müsste der Zucker dem Blutserum, in welchem er gelöst ist, ganz abgesehen von der Vermehrung des Wassergehaltes desselben durch Endosmose, die Fähigkeit ertheilen rascher, d. h. für die Zeiteinheit in grösserer Menge, durch die Nieren zu filtriren, als unter sonst gleichen Bedingungen im zuckerfreien Zustande. Nehmen daher ein Gesunder und ein Diabetiker innerhalb 15 Tagen auch dieselbe Getränkemenge zu sich, so muss im Harn des letzteren mehr Wasser wieder-

1) Diese Ansicht wird von Prof. Weyrich nicht getheilt.

2) Weikart, Ueber die Wirkungsart der diuretica. Arch. der Heilkunde 1861. pag. 69. Derselbe, der Diabetes mellitus. Arch d. Heilk. 1861 pag. 173. Derselbe, Versuche über die Harnabsonderung. Arch. der Heilkunde 1862. pag. 119.

erscheinen als in dem des erstern. Wird nun die dadurch entstehende grössere Wasserarmuth des diabetischen Bluts sofort im Zustande des Entstehens durch die innern Wasser-Quellen des Organismus wieder ausgeglichen, so wird der Diabetiker einen Mehrbetrag in seiner Wasserausgabe gegenüber der Einnahme aufweisen müssen, während der Gesunde, der diese gesteigerte Abgabe von Harnwasser nicht zu leisten hat, einen mehr oder weniger erheblichen Rest zu andern Zwecken des Organismus verwendet.

Nach diesen Auseinandersetzungen muss ich es aussprechen, dass aus theoretischen Gründen und mit Rücksicht auf meine Untersuchung mir eine Mehrausgabe von Wasser in der Ausgabe über dasjenige der Einnahme, für alle die Fälle sehr wahrscheinlich vorkommt, wo durch einen gesteigerten Körperconsum (die Abmagerung der Diabetiker ist bekannt), eine durch ruhigen Zimmeraufenthalt auf dem Minimum erhaltene Perspiration, dem Organismus Gelegenheit gegeben wird, in sich selbst den Rest an Wasser zu erübrigen, welcher, in Folge zu geringer Wasserzufuhr von aussen, von dem endosmotischen Wasseräquivalente des Zuckers oder den nothwendigen Ausgaben durch Excremente und Perspiration gefordert wird. In diesem Falle kann es nicht zu dem lebhaften Durste kommen, welcher den Diabetiker zu einer allen Ansprüchen seines Organismus genügenden oder diese selbst übertreffenden Wasser-Aufnahme veranlasst. Mein Diabetiker, den ich oft genug gefragt habe, weil mir die verhältnissmässig geringe Wasserquantität, die er und ich aufnahmen, auffiel, während ich mich im Beginne des Versuchs auf eine bis zum Ueberdrusse gehende Wasserzufuhr gefasst gemacht hatte, — ob er nicht an Durst leide, hat es jedesmal verneint (vergl. die Bemerkungen). Das Hungergefühl blieb dagegen, nach seiner Aussage, während der ersten Versuchs-Periode unverändert fortbestehen.

Welche Stellung man aber auch zu dieser Frage einnehmen möge, man wird unbedenklich zugestehen können: dass der Diabetiker von dem eingenommenen Wasser einen grössern Antheil in seiner sensiblen Ausgabe wieder erscheinen lässt, als ein unter dieselben Verhältnisse versetzter Gesunder.

Ich habe bei dieser Frage länger verweilen müssen, weil ich sie für die Theorie dieser Krankheit, oder wenigstens für manche Erscheinungen des Stoffwechsels in derselben für sehr wichtig halte, und sie ferneren Forschungen angelegentlichst empfehlen möchte.

Es erübrigte noch, darnach zu fragen, ob auch in der Vertheilung des Wassers der Einnahme auf die verschiedenen Posten der Ausgabe (durch Nieren und Darm, Lungen und Haut) ein bemerkenswerther Unterschied zwischen dem Verhalten der gesunden und kranken Versuchsperson hervortritt. Aus der Bilanz-Rechnung ergiebt sich, dass ein Antheil des verausgabten Wassers inneren Quellen des Organismus entnommen werden muss. Da die Aufgabe dieses Abschnittes ist, die Verwendung des präformirten Wassers der Einnahme für Zwecke des Organismus zu betrachten, so würde dieselbe durch eine Berücksichtigung der Gesammtausgabe an Wasser füglich überschritten werden. Nichtsdestoweniger kann letztere der Uebersichtlichkeit zu Liebe an dieser Stelle einen Platz finden.

Die Gesammtausgabe von G. an Wasser (sensible + insensible) beträgt 3139 grm. Davon kommen auf den Harn 2222,3, auf die Excremente 62,5, auf die sensible Ausgabe 2284,8; auf die insensible durch Lungen und Haut 854,2. Das Verhältniss der sensiblen Ausgabe zur insensiblen gestaltet sich demnach wie 1 : 0,373, des Harnwassers zum Wasser der Excremente wie 1 : 0,028, des Harnwassers zur insensiblen Ausgabe an Wasser wie 1 : 0,384.

Die Gesammtausgabe von D. an Wasser beträgt 3396,6

grm. Davon kommen 3064,5 auf den Harn, 174,4 auf die Excremente, auf die sensible Ausgabe daher 3211,9; auf die insensible 184,7 grm. Es verhält sich daher die sensible Wasser-Ausgabe zur insensiblen wie 1 : 0,058, das Harnwasser zum Wasser der Excremente wie 1 : 0,048, das Harnwasser zur insensiblen Wasser-Ausgabe wie 1 : 0,060.

Aus diesen Zahlen geht unverkennbar hervor, dass ein bemerkenswerther Unterschied zwischen dem Stoffwechsel beider Versuchspersonen in Betreff der Wasservertheilung auf die einzelnen Posten der Ausgabe darin besteht: dass der Diabetiker viel mehr Wasser durch Nieren und Darm [1]), vorzugsweise durch erstere, bedeutend weniger durch Lungen und Haut [2]) ausscheidet als der Gesunde.

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1) Dass die Wasserausgabe durch den Darm bei dem Diabetiker gegenüber der des Gesunden ein wenig erhöht erscheint hat wahrscheinlich in der an den beiden letzten Versuchstagen bestehenden Diarrhoe ihre Ursache (vergl. die Bemerkungen.)

2) Vergleicht man die auf dem Wege der „Berechnung nach specificirten Posten", (C. Speck. Einige kritische Bemerkungen u. s. w. Arch. der Heilk. 1861. pag. 373) erhaltenen Zahlenwerthe für die insensible Perspiration beider Versuchspersonen mit den durch die Rechnung mit den Summen dieser Posten (Methode von Sanctorius) gewonnenen, so stimmen sie nicht mit einander überein. Für den Gesunden erhält man nach der letztern Methode als mittlere 24stündige Perspiration dieser Versuchsperiode 993,1 grm., für den Diabetiker nur 85,9 grm. Dieser Mangel an Uebereinstimmung ist nicht schwer zu erklären. Unsere Bilanz-Rechnung hat ergeben, dass ein nicht unbeträchtlicher Antheil der Gesammtausgaben an Wasser durch die Oxydation von H mittelst der Atmosphäre entnommenem O gebildet wurde. Diesen Einnahme-Posten lässt die Methode von Sanctorius unberücksichtigt. Ich kann aus diesem Grunde, wie ich beiläufig bemerken will, daher auch nicht Voit bestimmen, wenn er behauptet (Zeitschrift f. Biologie. München 1865 pag. 284.) es sei mit Hülfe dieser Methode möglich, das Gesammtgewicht der gasförmig ausgeschiedenen Stoffe ganz scharf festzustellen. Fast unter jeder Ernährungsweise wird mehr O eingeathmet als in der Exspirationsluft wieder erscheint. Diese O-Einnahme, welche nicht so unbedeutend erscheint, wenn man bedenkt, dass sie bei der Individualität und Nahrung von Valentin u. Brunner zur Zeit ihrer Untersuchung im Mittel für 24 Std. 104,3 grm. betrug (C. Brunner und Valentin. Ueber das Verhältniss der mit dem Athmen des Menschen ausgeschiedenen CO_2 zu dem durch jenen Process aufgenomm. O. Arch. f. phys. Heilk.

Ob dieser Unterschied in der That so gross ist, als ihn die hier vorliegenden Zahlen anzeigen, wage ich nicht zu entscheiden. Ich will nur darauf hinweisen, dass die Annahme eines Zerfalles von Fettgewebe in dem diabetischen Organismus, neben dem durch den Ueberschuss der Ausgabe angezeigten Consum von N haltigem Gewebe Perspirationswasser disponibel machen müsste, während der dabei freiwerdende C die etwas niedrige Zahl des zur CO_2 Bildung restirenden Cs der Einnahme heben würde [1]).

Der Grund dieser auffallenden Herabsetzung der Perspiration ist nach dem bisher Gesagten so einleuchtend, dass ich ihn nicht besonders zu erörtern brauche.

1843. pag. 415) dürfte nur dann vernachlässigt werden, wenn sie nur zur Wasserbildung verwendet worden wäre und gerade das mit ihrer Hülfe gebildete Wasser vollkommen in die Perspiration überginge. Sobald aber alles oder ein grösserer oder geringerer Theil dieses Wassers im Harn ausgeschieden wird, oder sobald der S, u. s. w. auf Kosten dieses zurückgehaltenen O oxydirt werden, muss nothwendiger Weise die Perspirationsgrösse zu niedrig ausfallen. Bei dem Diabetiker wird dieser Fehler um so beträchtlicher, als die Bildung von Zucker aus den Kolehydraten und Albuminaten ebenfalls eine bestimmte O-Menge beansprucht. Wenn wir nun in unsrem Falle annehmen, dass von den 143,1 grm. atmosphärischen O's, welche von dem Diabetiker zur Wasserbildung verwendet wurden (s. pag 36) nur 107,7 grm. nicht in das Perspirations-Wasser übergingen, so wäre die Differenz zwischen den Resultaten der beiden Bestimmungsmethoden ausgeglichen. Dazu kommt, dass ich in der Rechnung mit specificirten Posten nur die organischen Elemente nicht aber die Mineralsalze benutzen konnte, weil die zur Zubereitung der Speisen verwendete Salzmenge nicht einmal annähernd zu bestimmen war. Dieser Umstand ist auch bei dem Gesunden zu berücksichtigen, für den die Differenz beträglich geringer ausgefallen ist (39 grm.). Ausserdem würde hier die Annahme eines etwas grössern Fettansatzes, welcher garnichts im Wege steht (s. pag. 38), eine vollständige Ausgleichung herbeiführen.

1) Ueber die Perspirationsgrösse von Diabetikern s. Boecker a. a. O. Reich a. a. O. Mosler. Zur Theraphie des Diabetes. Arch. f. wiss. Heilk. 1858 Bd. III pag. 26. Unter älteren Arbeiten: Vogt. Einige Beobachtungen und Bemerkungen über die honigartige Harnruhr. Zeitschrift f. rat. Medic. Zürich 1844. Bd. I pag. 147.

2. Metamorphischer Kreislauf.

a) Kreislauf des Stickstoffs.

G. scheidet 7,7 grm. weniger N aus, als er in den Einnahmen erhalten. Er musste sich nach den Bedürfnissen des Diabetikers richten, welche eine starke, namentlich N reiche Nahrungs-Zufuhr nothwendig machten. Bischoff und Voit stellen auf Grundlage ihrer Beobachtungen am Hunde folgenden Satz auf [1]): „Erhält der Hund grössere Fleischmengen als zum vollständigen Ersatz nöthig sind, so setzt er den Ueberschuss an“; und J. Ranke [2]) musste aus seinen Selbstbeobachtungen schliessen: dass das Gleichgewicht in der N-Aufnahme und Ausgabe bei dem Menschen erst dann stattfindet, wenn nicht nur der N- sondern auch der C-Verbrauch des Organismus während der Versuchszeit vollkommen gedeckt ist. Wurde eine überschüssige N-Menge zugeführt, so trat ein N-Deficit in den Excreten ein (im concreten Falle 2,38 resp. 5,64 grm.) [3]); „Mit einer Mehrzufuhr von C nahm die N-Ausscheidung ab.“

Beide Momente, eine überreiche N- und C-Zufuhr, waren in dem Stoffwechsel des Gesunden wirksam. Ein anderes Resultat, als es die Tabelle aufweist, stand daher garnicht zu erwarten.

D. giebt 2,19 grm. N mehr aus, als er in der Nahrung eingenommen. Er erhält eine so reiche Kost, dass sie von dem Gesunden nur eben bewältigt werden kann; trotzdem klagte er unausgesetzt über ein lebhaftes Hungergefühl (vergl. die Bemerkungen), das nur unmittelbar nach der Mahlzeit auf kurze Zeit beschwichtigt werden konnte. Seinen Ansprüchen

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1) a. a. O. pag. 245.
2) a. a. O. pag. 330.
3) a. a. O. pag. 323 u. 324.

genügte daher die Nahrungszufuhr durchaus nicht. Bischoff und Voit fanden [1]), dass der Hund, sobald er nicht die zu seiner vollständigen Ernährung hinreichende Fleischmenge erhielt, immer von seinem eigenen Fleisch und Fett zusetzte; und Ranke [2]), für den Menschen, dass bei ungenügender Nahrung, mag es an N oder C fehlen, sich ein N-Ueberschuss in den Excreten gegenüber der Einnahme vorfinde.

Auch das für den Diabetiker erhaltene Resultat steht daher im Einklange mit den gegenwärtigen Anschauungen über Ernährung.

Aus welchem Grunde, lautet nun die Frage, war die für den Gesunden überreiche Nahrung für den Diabetiker unzureichend? Durch welchen Einfluss wurde die Zersetzung der dem Stoffwechsel anheimfallenden Albuminate für den letztern in so auffallender Weise gesteigert?

Bischoff und Voit haben uns drei Faktoren kennen gelehrt, welche bei der Zersetzung N haltiger Körperbestandtheile (Albuminate der Gewebe und des Bluts, C. Schmidt) betheiligt sind: das Organ, neuzutretenden Bildungsstoff (Plasma, Blastem) und den Sauerstoff der Atmosphäre [3]). „Die Umsetzung ist stets das Produkt der Einwirkung aller drei Faktoren auf einander und ist derselben direkt proportional [4]). Die Umsetzung muss daher steigen, wenn die Masse eines jeden einzelnen dieser drei Faktoren zunimmt, oder wenn diese Massen-Zunahme sich nur auf einen einzigen Faktor beschränkt, während die Masse der beiden andern gleich bleibt, oder die des einen oder des andern, oder beider zugleich selbst abnimmt.

1) a. a. O pag. 245
2) a. a. O. pag. 331.
3) a. a. O. pag. 7.
4) a. a. O. pag. 8.

Die Masse des Organs (Muskel, N haltiges Gewebe) ist nun bei G. ohne Zweifel grösser als bei D. Die Menge des in der Zeiteinheit aufgenommenen, atmosphärischen O ist für G. bedeutender als für D. [1]), das zur Ernährung der N haltigen Körperbestandtheile dienende Plasma wird in beiden Versuchspersonen vom Nahrungsschlauche her durch gleiche Fleischmengen gespeist [2]).

Diese Erwägungen lassen eher bei G. einen stärkeren Umsatz von Albuminaten erwarten als bei D.

„Aber auch wenn man kein Eiweiss einführt, sondern nur die Strömung der vorhandenen Parenchymflüssigkeit durch die Organe stärker macht“ schliesst Voit auf Grundlage seiner Untersuchungen über den Einfluss des Kochsalzes auf den Stoffwechsel [3]) „muss eine grössere Menge von Eiweiss verbrennen.“ Diess ist eine Thatsache die durch die zum grossen Theile bereits citirten (auf pag. 37) Arbeiten von Boecker, Bischoff [4]), Genth, Mosler, Kaupp u. A. für den Menschen unanzweifelbar festgestellt ist. Diese Forscher fanden mit grosser Uebereinstimmung, dass bei sonst gleichbleibenden Bedingungen eine mit der getrunkenen Wasser- besonders aber der ausgeschiedenen Harn-Menge proportionale Vermehrung der festen Harnbestandtheile, und namentlich derjenigen stattfindet, welche wir direkt auf eine Zersetzung N haltiger Stoffe im Organismus beziehen dürfen (Harnstoff, Schwe-

1) G. hat eine grössere, vitale Capacität der Lungen, eine stärkere Respirationsfrequenz. Die Bilanzrechnung weist ebenfalls eine grössere O-Einnahme durch die Respiration für G. nach (s. pag. 34).

2) Es sei denn, dass man mit Neuschler (Beitrag zur Kenntniss der einfachen und zuckerführenden Harnruhr. Arch. f. wiss. Heilk. Bd. VI 1863. pag. 37) eine Zuckerbildung aus den Albuminaten schon im Magen annimmt.

3) a. a. O. pag. 62.

4) Bischoff, Der Harnstoff als Maass des Stoffwechsels Giessen 1853 pag 143 und ff.

felsäure, weniger die Phosphorsäure, Genth.). Es bewirkt also Wassertrinken bloss durch eine Vermehrung der Saftströmung, mit andern Worten, durch eine Steigerung der endosmotischen Vorgänge innerhalb des Organismus einen grössern Stoffwechsel, in specie: eine gesteigerte Zersetzung der N haltigen Stoffe.

Damit können wir unsere Betrachtung an das in dem vorausgehenden Abschnitte Gesagte direkt wieder anknüpfen. Eine Steigerung der endosmotischen Vorgänge in dem Organismus eines Diabetikers ist garnicht zu bezweifeln. Vermehrt das Kochsalz, vermöge seiner physikalischen Eigenschaften die Oxydation des Eiweisses und dadurch die Harnstoffmenge[1]) so gilt dies in noch höherem Grade für den Zucker; denn er findet sich in dem Blute von Diabetikern in überreicher Menge und sein Wasseräquivalent ist höher als das des Kochsalzes.

Von den verschiedenen Möglichkeiten, welche Bischoff und Voit für eine Steigerung in der Zersetzung N haltiger Stoffe aufgestellt haben, könnte demnach bei unsrem Diabetiker folgende stattfinden. Während zwei Faktoren (Organ und Sauerstoff-Einnahme) dem Gesunden gegenüber selbst ungünstiger gestellt sind, ist der dritte Factor (das Plasma) unter weit günstigere Bedingungen versetzt. Der Effekt ist eine gesteigerte Zersetzung der Albuminate und die Folge davon, dass eine die Bedürfnisse des Gesunden mehr als befriedigende Nahrung dem Kranken nicht genügt, er von seinem eigenen Körper einen Antheil der N-Ausgabe bestreiten muss.

In dieser Weise könnte man sich den Vorgang der gesteigerten Albuminat-Zersetzung in dem diabetischen Organismus vorstellen, wenn man die Annahme machte, dass der Zucker demselben, gleich dem Versuchsthiere in den Untersuchungen

1) Voit, a. a. O. pag. 65.

von Voit über das Kochsalz, von aussen, neben der übrigen Nahrung zugeführt worden sei, oder wenn man voraussetzen wollte, der Zucker würde nur auf Kosten in der Nahrung zugeführter Kohlehydrate gebildet. Von Diabetikern, bei welchen der letzte Fall stattfinden soll, ist in der Literatur genügend die Rede und M. Traube[1]) und J. Seegen[2]) haben vorzugsweise darauf ihre bekannte Eintheilung des Diabetes mellitus gegründet. In allen solchen Fällen scheint eine gesteigerte Albuminat-Zersetzung in gesteigerter Strömung des Plasmas ihren einzigen Erklärungsgrund finden[3]) zu können.

Ganz sicher sind aber auch solche Fälle constatirt[4]) in welchen die Zuckerausscheidung im Harne durch eine jede aus rein animalischen Stoffen bestehende Mahlzeit gesteigert wurde und trotz längere Zeit fortgeführter, ausschliesslicher Fleischdiät fortbestand. Hier hat man keine andre Wahl als eine Zuckerbildung aus den C-, H- und O-Elementen der Albuminate[5]), anzunehmen. Findet sich nun bei einem Diabetiker, neben dem Zucker viel mehr N., in der Form von Harnstoff, im Harn, als bei einem unter dieselben Bedingungen versetzten Gesunden, so ist nichts natürlicher als die Annahme: es werde das bei dem Austritte der zur Zuckerbildung erforderlichen C-, H- und O-Aequivalente aus dem Atomcomplexe der Albuminate zurückbleibende N-Aequivalent den Ausscheidungsorganen in der Form von Harnstoff zugeführt.

1) M. Traube, Virchows Arch. Bd. IV 1851.

2) J. Seegen, Beobachtungen über diabetes mellitus. Wiener medicin. Wochenschrift 1863 No 14.

3) So leitet auch Rosenstein (Ein Fall von diabetes mell. Virchows Arch. Bd. XII, pag. 414.) die grosse Harnstoff- und Kochsalz-Ausscheidung seiner beiden Kranken von der bedeutenden Menge des getrunkenen Wassers ab.

4) Neuschler a. a. O. pag. 37 und Griesinger. Studien über diabetes a. a. O. pag. 53.

5) Der geringe Fettgehalt derselben kommt dabei garnicht in Betracht.

Von dieser Seite betrachtet gewinnt aber eine gesteigerte Zersetzung der Albuminate in einem diabetischen Organismus eine ganz besondere Bedeutung. Sie ist dann nicht mehr eine von den physikalischen Eigenschaften des durch den specifischen Krankheitsprocess gebildeten, abnormen Produkts (des Zuckers) abhängige Folgeerscheinung; sondern sie ist bei der Entstehung dieses Produkts, d. h. bei dem Krankheitsprocesse selbst direkt betheiligt. Der von dem Diabetiker unter gleichen Bedingungen mehr als von dem Gesunden ausgeschiedene Harnstoff ist dann ebensogut ein specifisches Produkt seiner Krankheit als der Zucker. Es erscheint dann aber auch im höchsten Grade unwahrscheinlich, dass von der gesammten Ausscheidungsgrösse des Diabetikers an Harnstoff ein Theil, und zwar derjenige, welcher mit der von dem gesunden Vergleichsobjekt gelieferten Harnstoffmenge übereinstimmt ganz anders entstanden sein sollte, als der Antheil, um welchen er den Gesunden übertrifft.

So wird man nothwendiger Weise zu der Annahme gedrängt, dass einerseits auch im physiologischen Zusta de die Harnstoffbildung aus den Albuminaten unter der Abspaltung der N losen Atomgruppe Zucker[1]) erfolge, wenn auch dieses letztere Spaltungsprodukt in den Ausscheidungen eines Gesunden garnichto der wenigstens nicht (Bruecke) in der Menge, in welcher es producirt wurde, nachgewiesen werden kann[2]), und dass

1) Diese Anschauung scheint mir auf physiologisch-chemischer Seite, namentlich nach den Untersuchungen von Meissner (Zur Kenntniss der Stoffmetamorphose im Muskel. Chem. Centralblatt. 1861 Nr. 61.) auf keine besonderen Schwierigkeiten zu stossen. Vergl. ausserdem: Boedecker und Fischer. Ueber künstl. Bildung des Zuckers aus dem Knorpel und über die Umsetzung des genoss Knorpels im menschl. Körper. Chem. Centralblatt 1861 pag. 204—206. Horsford. Liebigs Annalen Bd. 60. pag. 1—57. Küthe. Zur Funktion der Leber. Studien d. physiol. Instit. zu Amsterdam. 1861 pag. 48. Milkowski. Ueber Zuckerbildung aus Nhaltigen Substanzen im Diabet. mell. Wiener med. Wochenschrift. 1864. pag. 700.

2) Diese Ansicht finde ich von H. Huppert, wie ich au seiner, mir während

andererseits der Diabetes mellitus vorzugsweise in einer Steigerung dieses physiologischen Vorgangs begründet sei und wir von einem Diabetiker, den wir ceteris paribus mit einem Gesunden vergleichen ebenso sicher eine gesteigerte Zersetzung der Albuminate, mit andern Worten, eine gesteigerte Harnstoffausscheidung erwarten müssen als eine Ausscheidung von Zucker.

Diese theoretische Betrachtung hier weiter fortzuführen ist mir durch die engen dieser Arbeit gesetzten Grenzen nicht gestattet. Ich werde mich daher auf den Versuch beschränken müssen, im concreten Falle eine Entscheidung darüber herbeizuführen, welchem der beiden bisher erwogenen Gründe ein grösseres Gewicht bei der Entstehung der zwischen dem Gesunden und Kranken in Bezug auf den N-Kreislauf beobachteten Differenz einzuräumen ist.

Zu dem Zwecke werde ich folgendes Verfahren einschlagen.

Von der Harnzuckermenge des Diabetikers ist das Zuckeräquivalent der in der Nahrung verabreichten Kohlehydrate abzuziehen. Der dabei erhaltene Rest muss dann zu der von dem Diabetiker mehr (als von G.) gelieferten Harnstoffmenge in demselben Verhältnisse stehen, in welchem sich die Atomgruppen des Zuckers und des Harnstoffs von der des Eiweisses abspalten. Trifft dieses Verhältniss auch nur annähernd zu[1])

der Abfassung dieser Schrift zugegangenen Arbeit dieses Forschers (Ueber die Beziehung der Harnstoffausscheidung zur Körpertemperatur im Fieber. Arch der Heilkunde. 1866. Heft I pag. 51). ersehe, getheilt. Im Unterschiede von mir ist er zu ihr durch die Beobachtung eines bestimmten Verhältnisses, in welchem der Harnstoff und der Zucker im Harn mit ausschliesslicher Fleischkost ernährter oder hungernder Diabetiker auftreten (auf dieses Verhältniss werde ich später zurückkommen) gelangt. Der Erfüllung seines Versprechens mit Rücksicht auf diese Beobachtung in nächster Zeit eine neue Theorie des Diabetes zu veröffentlichen, sehe ich mit eben solcher Erwartung entgegen, als es mir Genugthuung verschaffen würde, wenn sich die hier niedergelegten Beobachtungen als Stütze derselben verwenden liessen.

1) Huppert a. a. O. spricht allerdings von einem bestimmten Verhältnisse

so wäre damit der Beweis eines Zerfalles der Albuminate in Harnstoff und Zucker, sowohl im gesunden als im diabetischen Zustande bis zur Evidenz geliefert.

Denn will ich mit Benutzung der, über den Einfluss einer vermehrten Saftströmung (deren unmittelbaren Ausdruck ich in der von dem Kranken mehr gelieferten Harnwassermenge zu erkennen glaube) gemachten Erfahrungen auf die Steigerung der Harnstoffausscheidung festzustellen suchen, ob die ganze zwischen beiden Versuchspersonen beobachtete Differenz in der N-Ausscheidung, diesem Einflusse zugeschrieben werden darf. Mindestens das dabei erhaltene Plus an Harnstoff müsste dann jedenfalls auf Rechnung des specifischen Krankheitsprocesses gesetzt werden.

Das zuerst angegebene Verfahren führt aber in diesem Falle nicht zum Ziele. Das Verhältniss, in welchem sich Harnstoff und Zucker mit Rücksicht auf die Formel des Eiweisses von letzterm abspalten müssten, ist 34,52: 115,22, mit andern Worten: je 100 grm. dem Stoffwechsel anheimfallender und sich an der Zuckerbildung betheiligender Albuminate liefern unter Abspaltung von 34,52 grm. Harnstoff[1]) (unter Aufnahme von 26,64 grm. Wasser + 24,10 Sauerstoff) 115,22 grm. Zucker d. i. 333,7 grm. Zucker entsprechen 100 grm. Harnstoff. Der tägliche Ueberschuss des Diabetikers über die von dem Gesunden gelieferte Harnstoffmenge beträgt 15,243 grm. Diese würden demnach 50,88 grm. Albuminat-Zucker entsprechen.

Die im Brod täglich eingeführte Zuckermenge beträgt 270,8 grm. (Stärkmehl-Aequivalent = 243,8 grm). Die täglich auf-

zwischen Harnstoff und Zucker in dem Harne von Diabetikern unter Umständen, bei welchen die Bildung von Zucker aus den Kohlehydraten der Nahrung von Hause aus wegfällt, macht aber noch keine Zahlenangabe, so dass ich vorläufig nicht weiss, ob er das von mir geforderte Verhältniss im Sinne hat.

1) Bidder und Schmidt a. a. O. pag. 303.

genommene Menge an Milchzucker (43,7 grm.) und an Rohrzucker (nur 0,7 grm.) ungerechnet, erhielt der Kranke daher im Brod beinahe ebensoviel Stärkmehl als die täglich gelieferte Harnzucker-Menge (271,4 grm.) voraussetzt.

Soll man daraus folgern, dass die von dem Harnstoff-Ueberschuss postulirte Menge von 50,88 grm. Albuminat-Zucker überhaupt nicht gebildet worden ist, oder dass ein Theil des in den Kohlehydraten der Nahrung zugeführten Zucker-Aequivalents an der Entstehung des im Harne gefundenen Zuckers unbetheiligt ist? Denn man darf nicht vergessen, dass nur in dem Falle, wo aller aus den Kohlehydraten gebildete Zucker und aller überschüssig producirte Albuminat-Zucker im Harne wiedererscheint, das geforderte Verhältniss zwischen überschüssigem Harnstoff und ausgeschiedenem Albuminat-Zucker, bei dem von mir angewendeten Verfahren, zu Tage treten kann. Ist es nicht der Fall, so muss das zwischen abgespaltenem Harnstoff (der ohne Verlust in den Harn übergeht) und producirtem Zucker bestehende Verhältniss verwischt werden. Dies bleibt auch in dem vorliegenden Falle als die wahrscheinlichste Annahme übrig. Denn einerseits giebt es mehrfache Wege auf denen sich der im Organismus gebildete Zucker der Bestimmung im Harn entziehen kann, oder sogar gewöhnlich entzieht[1]) und andererseits müsste bei der

1) Ausfuhr unverdauter Stärkmehlkörner durch die Excremente, in welchen ausserdem J. Heller (Ueber diabet. mellit. Hellers Archiv 1852 pag. 403.) auch bei vollkommener Abwesenheit von Zucker Fermentpilze nachwies, die er auf eine vorausgegangene Zersetzung von Zucker bezog; Milchsäure- und Buttersäure-Bildung innerhalb des Darms; Uebergang in andere Ex- oder Secrete: Speichel, Galle, Magensaft etc. Zurückhaltung des exosmotisch in die Parenchymflüssigkeiten übergegangenen Zuckers. Endlich ist selbst an die Zersetzung (Oxydation) eines Theils des vom Blute resorbirten Zuckers zu denken. Nach der eben ausgesprochenen Ansicht stelle ich mir allerdings das Consumptions-Vermögen des Diabetikers, ohne dass eine Herabsetzung desselben im Sinne Alvaro-Reynoso's angenommen zu werden braucht, durch die übermässige Zufuhr des bei der Albuminat-Zersetzung gebildeten Zuckers stark beeinträchtigt vor.

Zurückweisung einer Zuckerbildung aus den Albuminaten überhaupt auf eine Erklärung der beobachteten, übermässigen Albuminat-Zesetzung im Diabetes mellitus verzichtet werden.

Das andere von mir eingeschlagene Verfahren scheint es nämlich als eine Unmöglichkeit nachzuweisen, dass der ganze bei dem Diabetiker dem Gesunden gegenüber beobachtete Harnstoff-Ueberschuss von der vermehrten Saftströmung innerhalb seines Organismus abgeleitet werden könne. Aus den Beobachtungen von Boecker, Genth, Kaupp und Mosler[1]) ergiebt

Da jenes aber eine gewisse Breite hat, so braucht es hierdurch allein noch nicht vollkommen erschöpft, gleichsam gesättigt zu sein. Der Sättigungsgrad kann vielmehr erst durch eine gewisse Quantität von der Nahrung gelieferten Zuckers überschritten werden. In dieser Weise finden auch die von M. Traube (a. a. O.) als verschiedene Stadien u. J. Seegen (a. a. O.) als verschiedene Formen bezeichneten Species der Zuckerharnruhr ihre Erklärung. Im ersten Stadium hat die gesteigerte Albuminat-Zersetzung noch keinen sehr hohen Grad erreicht: im Hunger und bei absoluter Fleischkost verschwindet der Zucker vollständig aus dem Harn. Erst durch eine reichlichere, wie aus den Kohlehyraten gebildete, Zuckermenge welche sich dem Albuminat-Zucker hinzuaddirt, wird der Sättigungsgrad des Consumptions-Vermögens überschritten; es entsteht ein nachweisbarer Diabet. mellit. In mehr vorgeschrittenen Fällen ist die Albuminat-Zersetzung schon so bedeutend, dass allein oder beinahe allein der dabei gebildete Zucker nicht mehr oxydirt werden kann: auch im Hunger und bei absoluter Fleischkost verschwindet der Zucker im Harne nicht. Gehört nun mein Diabetiker in die Klasse von Kranken, bei welchen die gesteigerte Zersetzung der Albuminate das Vermögen, die dem Blute in der Zeiteinheit zugeführte Zuckermenge zu consumiren bloss beeinträchtigt, nicht aber aufgehoben ist, so wird ein Theil des in der Nahrung zugeführten Zuckers nothwendiger Weise zersetzt werden müssen. In derselben Weise erkläre ich mir die bereits (auf pag. 53) angeführte Beobachtung von Griesinger. Der Diabetiker schied bei der Aufnahme von annähernd 950 grm. wasserfreier Albuminate (absolute Fleischdiät) 542,32 grm. Zucker innerhalb vier Tagen aus. Erfolgt die Zuckerbildung in der von mir als wahrscheinlich bezeichneten Weise, so hätten sich aus diesen 950 grm. Albuminaten (unter Aufnahme von Wasser und Sauerstoff) circa 1094 grm. Zucker abspalten und von einer Oxydation oder anderartigen Verwendung innerhalb des Organismus abgesehen, im Harn erscheinen müssen. Statt dessen fand man im Harne nur 532,32 grm., also ungefähr nur die Hälfte der muthmaasslich producirten Menge. Auch Vogel (a. a. O. pag 495) hält die in das Blut von Diabetikern eintretende Zuckermenge „für jedenfalls viel grösser als die durch den Urin abgehende."

1) a. a. O.

sich für je 100 grm. Harnwasser, welche die genannten Forscher oder deren Versuchsobjekte mehr ausschieden im Mittel eine Zunahme von 0,3 grm. Harnstoff. Der tägliche Ueberschuss an Harnwasser des Diabetikers gegenüber dem Gesunden beläuft sich auf 842,3 grm., welche nach dem angegebenen Verhältnisse eine Zunahme von 2,5 grm. Harnstoff erwarten lassen. Berechnet man diese Zunahme nicht nach dem mittlern, sondern nach dem von Mosler[1]) erhaltenen höchsten Werthe von 0,6 grm., so erhielten wir für den Diabetiker immer nur 5 grm. Harnstoff, also weniger als ein Drittheil des thatsächlich beobachteten Harnstoffüberschusses (15,243 grm.)

Aus diesem Grunde und mit Rücksicht auf die schon gegen die Beweiskraft der ersten Methode ausgesprochenen Bedenken sehe ich mich veranlasst nur den kleinern Theil des von dem Diabetiker gelieferten Harnstoff-Ueberschusses (= gesteigerte Albuminat-Zersetzung) als das Resultat eines sekundären Vorganges (vermehrte Saftstörung), den grössern dagegen von der specifischen Krankheitsursache direkt abhängig aufzufassen[2]).

Sind nun mit den drei von Bischoff und Voit für den physiologischen Zustand aufgestellten Faktoren in der That alle Bedingungen für die Zersetzung der Albuminate erschöpft, so bleibt nichts übrig, als für den pathologischen des Diab. mellit. ausser der vermehrten Strömung des Plasmas, welche zur Erklärung meiner Beobachtung nicht ausreicht, eine qualitative

1) a. a. O. pag 458.

2) Wenn ich es unterlasse auf die Untersuchungen anderer Forscher über die Harnstoffausscheidung im diabet. mellit. an dieser Stelle einzugehen, so lasse ich mich dabei ebenfalls nur durch äussere Gründe bestimmen (vergl. übrigens die Anmerkung auf pag. 43.). Ich verweise hier bloss auf die Untersuchungen von Rosenstein (a. a. O.), von Reich (a. a. O), meines Wissens die einzigen, in welchen, ähnlich dem meinigen, der Versuch einer vergleichenden Beobachtung mit einem Gesunden gemacht worden ist; ferner auf Thierfelder und Uhle: Ueber die Harnstoffausscheidung im diabet. mellit. Archiv der physiol Heilkunde 1858 pag. 32.

Aenderung, sei sie physikalischer oder chemischer Natur, des Organs (Albuminat-Gewebes) oder des Plasmas anzunehmen. Unter diesen Möglichkeiten scheint es mir noch am wahrscheinlichsten, eine derartige Veränderung des Albuminat-Gewebes vorauszusetzen, dass es, wenn ich an dem von den genannten Verfassern (auf pag. 7) gebrauchten Bilde festhalte, der vereinten Einwirkung der beiden andern Factoren, der Druckwirkung von Seiten des Plasmas und dem Zuge von Seiten des atmosphärischen Sauerstoffs einen geringern Widerstand entgegenzusetzen vermag, als das Gewebe eines gesunden Menschen, wobei ich bereitwillig zugebe, dass sich diese Definition nicht über das Niveau einer Hypothese erhebt.

b. Kreislauf des Wasserstoffs und Sauerstoffs.

Nach dem Vorgange Barral's [1]) werden der Kreislauf des Wasserstoffs und Sauerstoffs gemeinsam besprochen.

G. behält nach Abzug der in die Zusammensetzung des Albuminat- und Fett-Gewebes übergehenden Wasserstoffmenge von dem Wasserstoff-Gehalte der Einnahmen noch 31,8 grm. übrig, von welchen 21,2 grm. mit dem Sauerstoff-Ueberschusse der Nahrung (oxygène de constitution, Barral, = 169,3 grm.) 190,5 grm. Wasser bilden (eau prédisposée) während der Rest von 10,6 grm. Wasserstoff mit Hülfe des Sauerstoffs der Inspirationsluft (85, 1 grm. O) zu 95,7 grm. Wasser oxydirt werden (eau de combustion pulmonaire).

Der nach Abzug der sensiblen Ausgabe für D. übrig bleibende Rest der Sauerstoff-Einnahme beläuft sich auf nur 20,1 grm., welche durch den Zerfall von Albuminat-Gewebe auf 21,1 grm. erhöht werden. Von diesen werden 3,2 grm. durch das von den Einnahmen und dem umgesetzten Gewebe dem Orga-

1) a. a. O. pag. 159.

nismus disponibel gewordene Sauerstoff-Quantum von 25,7 grm. (oxyg. de const.) zu 28,9 grm. Wasser umgewandelt (eau prédisposée), der Rest von 17,9 grm. Wasserstoff bildet mittelst 143,1 grm. Sauerstoff der Inspirationsluft 161 grm. Wasser (eau de combustion pulm.)

Vergleiche ich zunächst die für meine Person gefundenen Werthe mit den von Barral für sich selbst berechneten (a. a. O. Pag. 160) so sind die dabei hervortretenden Unterschiede gering und vorzugsweise auf die Verschiedenheit der von ihm und von mir, dem eine N reiche Kost vorgeschrieben war, aufgenommenen Nahrung zu beziehen. Im Mittel seiner beiden (im Sommer und Winter) angestellten Beobachtungen nahm er täglich 50 grm. Wasserstoff und 228,5 grm. Sauerstoff (ich 48,3 grm. Wasserstoff und 210,2 grm. Sauerstoff) auf und behielt nach Abzug der sensiblen Ausgabe zu anderweitiger Verwendung 45,9 grm. Wasserstoff und 213,7 grm. Sauerstoff übrig (ich 42,4 grm. Wasserstoff und 184,6 grm. Sauerstoff, ungerechnet die zum Fleisch- und Fett-Ansatz verwendeten Mengen).

Anders gestaltet sich das Verhältniss bei dem Kranken. Dass der Verlust an unzersetztem Zucker einen geringern Rest an Wasserstoff zur Wasserbildung für D. zur Folge haben muss, ist selbstverständlich. D. gewinnt aus der Oxydation des überschüssigen Wasserstoffs nur 189,9 grm. G. dagegen 286 grm. Wasser. Hervorgehoben zu werden verdient aber das Verhältniss, in welchem das mit Hülfe des im Organismus überschüssigen Sauerstoffs und das, mittelst dem der Respirationsluft entnommenen, gebildete Wasser bei beiden Versuchspersonen zu einander stehen. Bei G. beträgt die Menge des erstern (eau prédisp.) 66,6%, die des letztern (eau de combustion pulmon.) 33,4% der gesammten aus dem Wasserstoff gebildeten Wassermenge; bei D. resp. 15,2% (eau prédisp.) und 84,8%. Es beträgt ferner das mit Hülfe der Respiration gebildete Wasser bei G. 95,7, bei D. 161 grm. Es sind demnach sowol relativ als absolut die Ansprüche die D. bei dem Processe der Wasserbildung innerhalb des Organismus an die Atmosphäre macht ausserordentlich gesteigert. So auffallend diese Erscheinung auf den ersten Blick ist, so natürlich erklärt sie sich aus dem bedeutenden Sauerstoffverluste des Diabetikers durch den Harnzucker. Dieser gesteigerte Anspruch des Diabetikers an den Sauerstoff der Atmosphäre zum Zwecke der Wasserbildung ist ein Umstand, der namentlich bei Beurtheilung der nach der Methode von Sanctorius bestimmten Perspirationsgrösse Berücksichtigung verdient, wie ich es in der Anmerkung auf pag. 47 bereits näher auseinandergesetzt habe. Denn angenommen selbst dass die Einnahme an Sauerstoff zum Zwecke der Kohlensäure-Bildung und aus diesem Grunde auch die Gesammt-Einnahme an diesem Stoffe niedriger ausfällt als bei dem Gesunden, was die Betrachtung des Kohlenstoff-Kreislaufes in der That nachweisen wird, so darf sie bei diesem doch eher vernachlässigt werden als bei jenem weil sie dort in höherm Grade zu der Bildung solcher Produkte der Ausgabe beiträgt für welche wenigstens die Möglichkeit, in wägbarer Form bei der Bilanz in Rechnung zu kommen, vorhanden ist als hier.

c) Kreislauf des Kohlenstoffs.

Bei der Annahme einer Verwendung von 34,3 grm. C. zur Fettbildung (vergl. pag. 34.) beläuft sich die für G. als Exspirations-Rest nachbleibende Menge auf 252,4 grm. C, welche mit 673,2 grm. O der Respirationsluft 925,4 grm. CO_2 bilden.

D. behält zu diesem Zwecke bloss 186,1 grm. C übrig, sodass die, täglich mit Hülfe von 496,3 grm. O der Inspirationsluft gebildete CO_2 682 grm. beträgt.

Die von dem Gesunden täglich producirte CO_2-Menge ist

demnach viel bedeutender als bei dem Diabetiker unter denselben Ernährungs-Bedingungen, was als die unmittelbare Folge seines gesteigerten C-Verlustes durch die sensiblen Ausgaben (Zucker, Harnstoff) selbstverständlich ist. Dieses Verhältniss erklärt auch die allgemeine Steigerung derjenigen physiologischen Funktionen des Gesunden, welche in der Körpertemperatur, der Respirations- und Pulsfrequenz [1]) ihren Ausdruck finden, gegenüber dem Diabetiker. Die grössere Menge oxydationsbedürftiger Stoffe im Blute des Gesunden macht einen gesteigerten [2]) Wechselverkehr desselben mit der Atmosphäre nöthig (höhere Respirationsfrequenz, beschleunigte Blut-Cirkulation durch die Lungen-Capillaren – höhere Pulsfrequenz) und der Umstand, dass seine mittlere Respirationsfrequenz das Mittel der von Quetelet [3]) an Individuen von 20—25 Jahren beobachteten (18,7) und die Häufigkeit seines Pulses die gewöhnliche Frequenz desselben im 20. bis 24. Jahre (71 [4])) übertrifft, kann vielleicht auf Rechnung der, ihm durch den Vergleich mit dem Diabetiker vorgeschriebenen, überschüssigen Nahrung, und der davon abhängigen gesteigerten Oxydations-Vorgänge gesetzt werden. Dass seine Körpertemperatur keine der gesteigerten CO_2-Bildung entsprechende Erhöhung erfuhr, muss dem erheblichern Wärme-Verluste durch das, bei der häufigern Respiration, vermehrte Ein- und Ausströmen von Luft, durch die grössere sensible Ausgabe [5]) sowie einer gesteigerten Wasserverdunstung inner-

1) Dies wird durch folgende Durchschnitts-Zahlen der I. Periode veranschaulicht:

	Temperatur d. Achselhöhle	Respirationsfreq.	Pulsfreq.
G.	37,1° C.	19,5	79,1
D.	36,3	17,2	69,3

2) Zunächst im Hinblick auf den Diabetiker.

3) Funke, Lehrbuch der Physiologie. Leipzig 1863. Bd. I pag. 431.

4) Uhle und Wagner. Handbuch der allgemeinen Pathologie. Leipzig 1864. pag. 482.

5) Barral a. a. O. pag. 165.

halb der Respirationsorgane und durch vermehrten Haut-Turgor, zugeschrieben werden. Dieser Verlust war aber nicht so bedeutend, als dass die Körpertemperatur des Gesunden bis auf die des Kranken, dem von Seiten der Nahrung dasselbe „Heizmaterial" zu Gebote stand, abgekühlt werden konnte. Im Gegensatze hiezu trägt die verminderte Respirations- und Pulsfrequenz, die herabgesetzte Wasser-Verdunstung in den Respirationsorganen, die trockne, spröde Haut, mit einem Worte der verminderte Wärme-Verlust, dazu bei, dass die Körpertemperatur des Diabetikers doch noch auf der Höhe von 36,3° C. erhalten werden kann. Letzterer Umstand ist indessen hauptsächlich davon herzuleiten, dass die überreiche Nahrung den Diabetiker in den Stand setzt, trotz der gesteigerten sensiblen Ausgabe, einen C-Rest für die CO_2-Bildung zu erübrigen, welcher nur im Vergleiche mit der Einnahme und mit dem gesunden Vergleichsobjekte auffallend gering genannt werden kann. Vergleicht man nämlich die Ausscheidungs-Grösse von D. mit der von Ranke[1]) als Mittelzahl für die Haut- und Lungenausscheidung eines gesunden, ruhenden Menschenorganismus gefundenen Grösse von **211** grm. C, so beträgt die Differenz bloss **24,9** grm. und übertrifft die von Ranke im Hungerzustande ausgeschiedene C-Menge um **5,2** und **5,3** grm[2]).

1) a. a. O. pag. 366.

2) Dieses Resultat steht mit den beiden, so viel mir bekannt ist, einzigen Bestimmungen der CO_2-Exhalation von Diabetikern auf direktem Wege in ziemlich guter Uebereinstimmung. Es fand nämlich C. Schmidt (Charakteristik der epidemischen Cholera. Leipzig und Mitau 1850. pag 160) im Mittel aus 3 Versuchen als 24stündige Ausscheidungsgrösse bei einem männlichen Diabetiker (34 J. alt, 50, 56 Kilogr. schwer) 210,2 grm. C.; Böcker (a. a. O. pag. 382.) bei seinem Pat. (männliches Indiv., 30 J. alt, 47,2 Kilogr. schwer) 224,3 grm., im Mittel aus 41 Versuchen Denkt man nun daran, dass von beiden Forschern das Verfahren von Vierordt eingeschlagen werden musste, so wird es sehr wahrscheinlich, dass diese Werthe etwas zu hoch gegriffen sind, was schon aus dem Parallel-Versuche von Schmidt an sich selbst ($26\frac{1}{2}$ J. alt, Körpergewicht 63,12 Kilogr.) hervorgehen

Der bisher betrachtete Unterschied in der C-Ausscheidung: der grössere des Gesunden auf dem Wege der insensiblen Ausgabe und der grössere des Diabetikers durch sensible Ausgaben wird durch nachfolgende Zahlen veranschaulicht. Von **100** grm. der Ausscheidung dienenden Kohle scheidet der Gesunde **90,5** grm. durch Lungen und Haut, **9,5** grm. durch Nieren und Darm aus, der Diabetiker auf ersterem Wege **53,8** grm. auf letzterem **49,2** grm.

Es ist ferner von Interesse das Verhältniss der gesammten Grösse ausgeschiedenen Stickstoffs zu der Gesammt-Menge in derselben Zeit ausgeführter Kohle bei dem Gesunden und Diabetiker einander gegenüberzustellen. Für G. wird es ausgedrückt durch das Zahlen-Verhältniss **1 : 10** für D. durch **1 : 9**.

Aus dem, was über die Verwendung des H- und C-Ueberschusses der Einnahmen gesagt worden ist, ergiebt sich, dass die Gesammtmenge des zur Oxydation desselben erforderlichen Sauerstoffs der Inspirationsluft sich für G. auf **758,2** grm., für D. auf **639** grm. beläuft. Die von Vierordt[1]) für den gesunden, erwachsenen Mann berechnete tägliche Sauerstoffconsumtion beträgt **746** grm; für Valentin und Brunner[2]) ergab sich als Mittel **838,5** grm.

d) Kreislauf des Kochsalzes.

G. scheidet täglich **13,01**, D. **15,43** grm. ClNa aus. Bei gleicher Zufuhr liefert D. demnach ein Plus von **2,42** grm. CLNa im Harn[3]). Handelt es sich um den Grund dieser Er-

dürfte. Er bestimmte mit Hülfe desselben Apparats der bei dem Diabetiker in Anwendung kam, seine 24stündige Ausscheidungsgrösse unter gewöhnlichen diätetischen Verhältnissen auf 262,9 grm.

1) Funke a. a. O. pag. 446.

2) a. a. O. pag. 415.

3) Vergl. darüber Reich und Rosenstein a. a. O.

scheinung, so wird, da der Harn in dieser Untersuchung das alleinige Objekt abgab, zuerst die Frage zu erledigen sein, ob die Nieren als der einzige Weg für die Ausscheidung dieses Stoffes betrachtet werden dürfen. Neben den Nieren wären der Darm und die Haut zu berücksichtigen. Der Koth enthält aber, wenn durch das Salz nicht Diarrhöen[1]) entstehen, so gut wie kein Kochsalz; die Haut führt nur dann, wenn sie schwitzt, Salz in erheblicher Menge weg[2]).“ Der Gesunde hat nun allerdings während der ganzen Versuchszeit an keinen bemerkenswerthen Schweissen gelitten; indessen unterliegt es keinem Zweifel, dass seine Haut verhältnissmässig mehr Schweiss abgegeben hat, als die des Kranken. Die beobachtete Differenz ist aber zu bedeutend und findet sich zu constant an den einzelnen Versuchstagen wieder, als dass sie in der Ausscheidung durch die Haut ihren alleinigen Erklärungsgrund finden könnte.

Während es nun auf der einen Seite feststeht, dass Kochsalz in letzter Instanz stets aus der (für beide Vergleichsobjekte gleichen) Nahrung stammt und in einer irgendwie in Betracht kommenden Menge nicht im Körper selbst gebildet werden kann[3]), würde andererseits die Annahme einer von der Lebensweise und Nahrung theilweise unabhängigen und durch die (individuelle) Körperconstitution des Diabetikers bestimmten, höhern Ausscheidungsgrösse[4]) an ClNa erst dann geboten sein, wenn kein anderer Weg für die Erklärung dieser

1) An diesen litt nur der Diabetiker während der beiden letzten Versuchstage. Die Mehrausgabe an ClNa vertheilt sich aber ziemlich gleichmässig auf die einzelnen Tage (vergl. die Tabelle der Ausgaben).

2) Voit, Untersuch. über den Einfluss des Kochsalzes u. s. w. pag. 37.

3) Kaupp, Untersuchungen über die Abhängigkeit des Kochsalzgehaltes des Urins von der Kochsalzmenge der Nahrung. Archiv. f. phyls. Heilk. 1855 pag 386.

4) Hegar, Ueber die Ausscheidung der Chlorverbindungen durch den Harn. Giessen 1852. Als Referat im Archiv. f. wiss. Heilk. Bd. I pag. 666.

Erscheinung offen stände. Ein solcher bietet sich zunächst in der vermehrten Harnausscheidung dar.

In Bezug auf den Einfluss vermehrter Harnmenge auf die Ausscheidungsgrösse des Kochsalzes haben die Untersuchungen von Kaupp (a. a. O. pag. 417) ergeben, dass eine Zunahme des Harnvolums um 100 C. C. eine Steigerung des ClNa um 0,193 grm. bewirkt[1]); dieselbe kann von garkeinem andern Umstande als von der grössern Harnwasser-Menge abzuleiten sein, was schon aus der Beobachtung von Weikart[2]), dass von einer 2% haltigen ClNa-Lösung 52,6 C. C., von einer 6% haltigen ceteris paribus nur 17,5 C. C. durch eine thierische Membran hindurchfiltriren, hervorgeht.

Der Diabetiker liefert 842,3 grm. Harnwasser mehr als der Gesunde, welchen eine Zunahme von 1,6 grm. ClNa entsprechen würde. Legen wir dieser Berechnung das aus den Untersuchungen mehrerer Forscher berechnete Mittel von 0,224 grm. ClNa für je 100 grm. Zuwachs an Harnwasser zu Grunde, so würden 842,3 grm. Harnwasser den ClNa-Gehalt des Harns um 1,88 grm. erhöhen.

Es ist ferner auf den grossen Einfluss vorausgegangener Kochsalzzufuhr auf die Mengen des während der Versuchszeit ausgeschiedenen Kochsalzes aufmerksam zu machen. Diese Thatsache, welche nach den Untersuchungen von Barral[3]), Kaupp[4]) und Voit garkeinem Zweifel unterliegt, wird von letzterem[5]) in der Weise gedeutet, dass die Aufnahmsfähigkeit

1) Nach Mosler a. a. O. berechnet sich für eine Zunahme des Harnwassers um 100 grm. eine Vermehrung des ClN-Gehaltes um 0,393; nach Böcker. a. a. O. pag. 342 um 0,209, nach Genth a. a. O. pag. 69 um 0,101. Im Mittel (die Kauppsche Beobachtung mitgerechnet) entspricht demnach einer Zunahme von 100 grm. Harnwasser eine Steigerung der Kochsalzausscheidung um 0,224 grm.

2) Arch. der Heilkunde. 1862. pag. 134

3) a. a O. pag. 342.

4) a. a. O. pag. 399.

5) Voit, a. a. O. pag. 38.

des Blutes und der Organflüssigkeit für Kochsalz zu verschiedenen Zeiten eine verschiedene sei, und sich wesentlich darnach richte, ob letztere durch vorausgegangene, reichliche Salzzufuhr mit diesem Stoffe überladen seien oder daran Mangel litten. Nimmt man nun an, was ohne weiteres zulässig erscheint, dass der Kranke in der dem Versuche vorausgegangenen Zeit in seiner Nahrung reichlichere Kochsalz-Mengen zu sich nahm, als der Gesunde, so darf man garnichts anderes erwarten, als dass der Kranke einen Ueberschuss an diesem Stoffe dem Gesunden gegenüber im Harne ausscheidet.

Erscheint demnach die Ausscheidungsgrösse des ClNa's im Harn in gewissem Sinne von der Menge der im Blute und der Organflüssigkeit befindlichen Kochsalz-Menge abhängig, indem sich das Blut gewissermaassen bestrebt, innerhalb bestimmter Grenzen eine gewisse Concentration einzuhalten, so wird mit grosser Wahrscheinlichkeit vorauszusetzen sein, dass auf die Zurückhaltung des Kochsalzes, oder auf dessen ungehindertes Hindurch-Passiren durch den Organismus, die im Blutserum und den Parenchymflüssigkeiten gelöste Menge an festen Bestandtheilen überhaupt nicht ohne Einfluss sein kann [1]. Nach dieser Anschauung müsste ein an festen Bestandtheilen (z. B. Zucker) reicheres Blut eine geringere Aufnahmsfähigkeit (Zurückhaltung im Organismus) haben, als daran ärmeres. Die Consequenzen für den in Rede stehenden Gegenstand ergeben sich daraus von selbst.

f) Kreislauf des Schwefels.

Während die Ausscheidung des ClNa's insofern geringeres Interesse beansprucht, als dieser Stoff kein unmittelbares Pro-

1) Vergl. darüber C. Schmidt, Characteristik der epidemischen Cholera. Mitau und Leipzig. 1850. pag. 13—16, 27, 52 und ff.

dukt im Körper erfolgter Zersetzungen ist, verdient der Schwefel als ein vorzugsweise aus der Umsetzung dem Stoffwechsel anheimfallender Albuminate hervorgegangenes Spaltungsprodukt grössere Beachtung. Dieser engen Beziehung zu dem Umsatze der Albuminante wegen würde der bisher geführte Beweis einer gesteigerten Albuminat-Zersetzung in dem diabetischen Organismus unvollständig bleiben, so lange nicht der Nachweis einer gesteigerten Schwefelsäure-Ausscheidung in dem Harne des Diabetikers geliefert würde. Dieser liegt indessen nach den erhaltenen Resultaten zu Tage. Sowohl an dem mittleren als an dem einzelnen Versuchstage ist ein Parallelismus [1]) zwischen der Harnstoff- und Schwefelsäureausscheidung nicht zu verkennen. Der grössern Harnstoffmenge des Diabetikers entspricht ein Plus an Schwefelsäure.

Am mittlern Versuchstage liefert D. 0,37 grm. mehr S als G.

Diese Zahl darf aber nicht als der directe Ausdruck der von D. mehr zersetzten Albuminat-Menge angesehen werden, sondern, um diesen zu erhalten, muss die gefundene Differenz noch um die bei der Bildung des für G. angenommenen Ansatzes an N haltigem Körpergewebe betheiligte S-Menge vergrössert werden. In dieser Weise beträgt die auf den Mehr-Umsatz zu beziehende Differenz 0,77 grm. S.

In Bezug auf die Erklärung der von dem Diabetiker unter denselben Ernährungsbedingungen mehr als von dem Gesunden gelieferte Schwefelmenge brauche ich nur auf die Betrachtung des Stickstoff-Kreislaufes zu verweisen.

1) Ich mache darauf aufmerksam dass dieser bereits durch die Arbeiten von Genth (a. a. O.) Mosler (c. a. O.) Clare (experimenta de excretione acidi sulfurici per urinam. Diss. inaug. Dorpat. 1854) u. A. nachgewiesene Parallelismus in diesem Falle im gesunden und kranken Zustande von zwei unabhängig von einander arbeitenden Personen (Mg. Beckmann und mir) constatirt worden ist.

g) Kreislauf der Phosphorsäure.

Da die Phosphate „als stete Begleiter der Albuminate und nach Maassgabe der Betheiligung letzterer am Stoffwechsel in den intermediären und Endausscheidungen erscheinen“ [1]), so verdient die Ausscheidungsgrösse derselben aus dem diabetischen Organismus gleiche Berücksichtignng mit der des Schwefels. An dieses Interesse knüpft sich ein anderes, welches vorzugsweise die detaillirte Bestimmung der PO_5 (geb. an Alkal., an $3\,CaO$ und $2\,MgO$) in dieser Untersuchung veranlasste.

Seitdem nämlich Neubauer[2]) in dem Harn eines sechsjährigen diabetischen Kindes im Mittel einer 9tägigen Beobachtung als 24stündige Menge an phosphorsaurem Kalk ($3\,CaO, PO_5$) 0,711 grm. und an $2\,MgO$, PO_5 0,388 grm. gefunden hatte: also eine auffallende Steigerung der Ausscheidung an Erdphosphaten, sprach v. Maak[3]) die Ansicht aus, dass aller Wahrscheinlichkeit nach nicht allein eine vermehrte Ausscheidung der Erdphosphate, sondern auch des KO's und NaO's den Diabet. mellit. in höchst bedeutungsvoller Weise complicire. Eine Stütze dieser Ansicht sah er in der vortheilhaften Wirkung des Natron bicarbonicum in dieser Krankheit. Auch J. Vogel[4]) berichtet von einem Falle, in welchem er eine vorübergehende übermässige Ausfuhr von Kalk beobachtete, und emphiehlt diesen Gegenstand zur Beachtung. Ebenso fand Boecker[5]) die Erdphosphate eines erwachsenen Diabetikers beinahe um das Dreifache der normalen Menge vermehrt (4,239 grm.) und

1) Bidder und Schmidt a. a. O. pag. 400.

2) Neubauer, Ueber die Erdphosphate des Harns. Iournal für practische Chemie Bd. 67. Heft 2. pag. 65.

3) v. Maak, Zur Therapie des Diabet. mell. Arch. f. physiol. Heilkunde Bd. V pag. 129.

4) a. a. O. pag. 498.

5) a. a. O. pag. 383.

zwar auf Kosten des phosphorsauren Natrons (3,407 grm.) und Beneke[1]) sowohl den phosphorsauren Kalk (1,11 grm.) als auch, und zwar in hohem Grade, die phosphorsaure Magnesia (1,72).

Auf Grundlage des von mir in dieser Beziehung mitgetheilten Versuchsmaterials lassen sich die hier aufgeworfenen Fragen in folgender Weise beantworten.

1) Die Gesammt-Ausscheidung an PO_5 des Diabetikers ist entsprechend der gesteigerten Albuminat-Zersetzung grösser als die des Gesunden. G. scheidet 3,012, D. 4,286 grm. PO_5 aus. Berücksichtigt man nun, dass G. 0,825 grm. PO_5 zum Ansatze von Albuminat-Gewebe verwenden musste, für D. 0,235 grm. durch den Zerfall von Albuminat-Gewebe zur Ausscheidung disponibel wurden: so müsste die beobachtete Ausscheidungsgrösse des Gesunden + der zum Körperansatz verwendeten Menge = der thatsächlich ausgeschiedenen Phosphorsäure-Menge des Diabetikers — der aus dem Zerfall von Körpergewebe hervorgegangenen sein. Führt man diese Rechnung aus, so erhält man für D. 4,051, für G. 3,837, also sehr nahe übereinstimmende Zahlenwerthe, welcher Umstand von dieser Seite her die Richtigkeit der auf die N-Ausscheidung gegründeten Annahme von Gewebs-Ansatz und Zerfall für die beiden Versuchspersonen bestätigen dürfte, ebenso wie dadurch der enge Zusammenhang der beobachteten Steigerung in der Phosphorsäure-Ausscheidung und die behauptete, gesteigerte Zersetzung von Albuminaten im diabetes mellitus evident wird[2]).

1) Beneke, Zur Physiologie und Pathologie des phosphorsauren und oxalsauren Kalks. Göttingen 1850 pag. 19.

2) Dies wäre um so bedeutungsvoller, wenn die Beobachtung von Sik (Versuche über die Abhängigkeit des Phosphorsäure-Gehalts des Urins von der Phosphorsäure-Zufuhr Arch. f. phys. Heilkunde 1857 pag. 490) dass alle über das gewöhnlich gelieferte Maass dem Organismus zugeführte Phosphorsäure vollständig

2) Die Steigerung in der Phosphorsäure - Ausscheidung des Diabetikers betrifft nicht einen einzelnen der drei in den Tabellen aufgeführten Posten [1]) und gestaltet sich nicht derartig, dass der eine auf Kosten des andern wächst [2]), sondern bezieht sich auf alle drei Posten, vorzugsweise aber auf den an Alkalien gebundenen Antheil. Das Verhältniss der an Erden gebundenen PO_5 zu einander zeigt sich nur unbedeutend zu Gunsten der an 3 CaO gebundenen Menge verändert. Dies wird durch folgende Zahlen veranschaulicht.

D. scheidet täglich an Alkalien gebundene Phosphorsäure 1,159 an 3 CaO geb. 0,070, an 2 MgO geb. 0,045, im Ganzen 1,274 grm. PO_5 mehr als G. aus.

Die tägliche Ausscheidungsgrösse an PO_5 von G. beläuft sich auf 3,012 grm. Diese = 100 gesetzt, beträgt die an Alkalien geb. PO_5 = 78,95 % (nach Sik 75,95 %), die an Erden gebundene 21,05 % (25,05 % Sik). D. liefert im Mittel 4,286 grm. PO_5, von welcher Menge 82,52 % auf die an Alkalien gebundene und nur 17,48 % auf die an Erden gebundene PO_5 kommen.

Nimmt man ferner die an Erden gebundene PO_5 = 100 an, so ergiebt sich für G. an 3 CaO geb. 57,88 % an 2 MgO geb. 42,12 %, für D. resp. 58,35 und 41,45 %.

im Harn wiedererscheint, durch weitere Versuche bestätigt würde. Es dürfte dann weder an eine Zurückhaltung im Organismus, noch an einen Verlust auf anderem Wege gedacht werden. Bei gewöhnlicher Lebensweise fand v. Haxthausen indessen im Mittel aus 17 Bestimmungen einen täglichen Verlust von 0,666 grm. PO_5 durch die faeces; also circa $^1/_4 - ^1/_5$ der durch den Harn ausgeschiedenen Menge. Vergl. Neubauer und Vogel a. a. O. pag. 331.

1) Wie in den Versuchen von Sik (a. a. O. pag. 494), welcher eine vermehrte PO_5-Menge des Harns bei vermehrter PO_5-Zufuhr nur durch Vermehrung der an Alkalien gebundenen PO_5 nachwies.

2) Wie in dem erwähnten Falle von Neubauer (Zunahme der an 3 CaO geb. auf Kosten der an 2 MgO geb. PO_5) oder bei Boecker (Zunahme der Erdphosphate auf Kosten des phosphorsauren Natrons).

3) Aus dem bisher Angeführten ergiebt sich ohne weiteres, dass der Diabetiker unter denselben Ernährungs - Bedingungen sowohl eine grössere Menge an Phosphorsäure gebundener Alkalien (NaO, KO? [1]) als auch phosphorsauren Kalks und phosphorsaurer Magnesia ausscheidet, und dass diese Steigerung vorzugsweise die Alkalien [2]), in geringerm Grade den Kalk und am geringsten die Magnesia betrifft.

Zur Erläuterung des Gesagten dienen folgende Zahlen.

Am mittlern Versuchstage treten aus dem gesunden Organismus: 4,019 grm NaO, 2 NO, PO_5, 0,801 3 CaO, PO_5 und 0,417 grm. 2 MgO, PO_5, in summa 5,237 grm. phosphorsaurer Salze. Aus dem diabetischen Organismus: 5,978 grm. NaO_2NO, PO_5, 0,954 grm. 3 CaO, PO_5 und 0,599 grm. 2 MgO, PO_5 d. i. 7,432 grm. phosphorsaurer Salze. Der Diabetiker scheidet demnach täglich 1,959 grm. NaO2UO, PO_5, 0,153 grm. 3 CaO, PO_5, 0,083 grm. 2 MgO, PO_5, im Ganzen 2,195 phosphorsaurer Salze mehr als der Gesunde aus. Auf 100 Theile ausgeschiedener phosphorsaurer Salze kommen für G. auf NaO_3UO, PO_5 76,75 %, auf die Erdphosphate 23,25 %,

1) Vergl. darüber v. Gorup-Besanez a. a. O. pag. 100.

2) Auch die in der Verbindung mit SO_3 ausgeführten Basen zeigen bei dem Diabetiker eine bemerkenswerthe Vermehrung. Nimmt man sämmtliche SO_3 an KO geb. an, so scheidet G. am mittleren Versuchstage 6,578, D. 8,119 grm. KO,SO_3 aus. Sollte nun, wie es sehr wahrscheinlich ist, in der That nicht alle SO_3 mit KO, sondern zum Theil mit NaO und alkalischen Erden, geeint sein, so dürfte die von mir in Anwendung gezogene Berechnung doch nicht die Gesammtmenge des mit dem Harn ausgeführten Kalis zu hoch veranschlagt haben. Direkte Untersuchungen von Böcker über den KO-Gehalt des Harns (Beiträge zur Heilkunde pag. 33) ergeben nämlich, dass wenn auch alles KO an SO_3 berechnet wurde, doch noch ein Unterschuss nachblieb, welchen er der Verbindung mit Cl zuwiess; ebenso bei Genth (a. a. O.). Dem entsprechend liegen auch die für die Paarung mit SO_3 erforderlichen Werthe an KO (für G. 3,557 und für D. 4,390) noch unter den von Böcker (4,446 grm. KO) und Genth (5,517 grm.) direkt gefundenen Zahlen. Erinnert man sich nun daran, dass auch die ClNa-Ausfuhr des Diabetikers grösser war als die des Gesunden, so ergiebt sich im Diab. mellit. eine allgemeine Steigerung in der Ausscheidung der Mineralsalze überhaupt.

für D. resp. 80,44%/3 und 19,56%. Auf 100 Theile ausgeschiedener Erdphosphate kommen für G. 65,76% auf $3CaO, PO_5$, 34,24% auf $2MgO, PO_5$ [1]), für D. resp. 65,61% und 34,39%.

Die Erklärung der gesteigerten Ausscheidung von Phosphaten im Diabetes mellitus anbetreffend muss ich ebenfalls auf den über den Kreislauf des Stickstoffs handelnden Abschnitt zurückverweisen.

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2) Wie verhält sich der Stoffwechsel des Diabetikers verglichen mit dem des Gesunden unter dem Gebrauche von täglich 7,81 grm. doppelt-kohlensauren Natrons?

Die Anhaltspunkte für die Beantwortung dieser Frage findet man in folgenden Tabellen.

1) Ich mache darauf aufmerksam, dass dieses Resultat im Widerspruche steht mit der Beobachtung von Neubauer, welcher auf 100 Thl. phosphorsaurer Erden bei Gesunden nur 33 Thl. phosphors. Kalks und 67 Thl. phosphors. Magnesia erhielt, dagegen in guter Uebereinstimmung mit den Erfahrungen von Kletzinsky, der 67,3% phosphorsauren Kalks und 32,7% phosphorsauren Magnesia bestimmte. Vergl. Neubauer und Vogel a. a. O. pag. 335.

II. Periode.

A. Gesunder (17 Tage).

Anfangsgewicht 55697 grm.

Endgewicht = 55441 grm.
Gewichtsdifferenz — 256.

	Gesammt-a. Einnahme, b. Ausgabe, c. s. d. Ausgabe	Wasser.	Trockne Substanz.	Mineralsalze.	Organische trockne Substanz.	Kochsalz.	Phosphorsäure geb an Alkal	Phosphorsäure an 3 CaO	Phosphorsäure an 2 MgO	Phosphorsäure Summe der PO_5	SO_3 Aequiv. des S	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff, s geb. an S.
a. Einnahme +	49664	38758,15	10905,85	389,328	10516,522						72,72	29,09	5465,225	766,457	504,428	3751,322
b. Harn 729,790 U	26577,1	25167,1	1410,0	343,06	1066,9	202,787	28,543	4,318	3,128	35,989	35,833		145,955	48,161	340,817	194,857
																21,42 s
Excremente	2138,0	1385,4	752,6	88,81	663,79								353,624	52,397	45,524	212,29
s. Summe d. Ausgab.	28715,1	26552,5	2162,6	431,87	1730,69	202,787	28,543	4,318	3,128	35,989	35,833		499,579	100,558	386,341	428,567

B. Diabetiker (19 Tage).

Anfangsgewicht 45984 grammes.

Endgewicht 51607 grammes
Gewichtsdifferenz 5623 +

	Gesammt-a. Einnahme, b. Ausgabe, c. s. d. Ausgabe	Wasser.	Trockne Substanz.	Mineralsalze.	Organische trockne Substanz.	Kochsalz.	Phosphorsäure geb an Alkal	Phosphorsäure an 3 CaO	Phosphorsäure an 2 MgO	Phosphorsäure Summe der PO_5	SO_3 Aequiv. des S	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff, s geb. an S.
a. Einnahme +	54923	43121,95	11801,05	440,389	11360,661						82,0	32,81	5955,212	830,838	568,509	3973,292
b. Harn {1038,400 U / 3746,85Zck.	53111,5	47456,5	5655	459,6	5195,4	227,301	44,297	7,717	5,417	57,431	55,253		207,681	68,529	484,936	277,254
													1498,72	249,80		1998,33
																33,151 s
Excremente	2338	1515,02	822,98	97,036	725,944								386,705	57,281	49,799	232,163
s. Summe d. Ausgab.	55449,5	48971,52	6477,98	556,636	5921,344	227,301	44,297	7,717	5,417	57,431	55,253		2093,106	375,610	534,735	2540,898

Daraus ergiebt sich für den mittleren Versuchstag der II. Periode nachfolgende tabellarische Uebersicht:

A. Gesunder.

Gewichtsdifferenz — 15 grm.

	Gesammt	Wasser.	Trockne Substanz.	Mineralsalze.	Organische trockne Substanz.	Kochsalz.	geb an Alkal	an 3 CaO	an 2 MgO	Summe der PO_5	SO_3 Aequiv. des S	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff
a. Einnahme	2921,4	2279,9	641,5	22,9	618,6						4,3	1,7	321,5	45,1	29,7	220,7
b. Ausgabe	1689,1	1561,9	127,2	25,4	101,8	11,92	1,679	0,254	0,184	2,117	2,1	0,8	29,4	5,9	22,7	25,2
Rest		718									2,2	0,9	292,1	39,2	7,0	195,5

B. Diabetiker.

	Gesammt	Wasser.	Trockne Substanz.	Mineralsalze.	Organische trockne Substanz.	Kochsalz.	geb an Alkal	an 3 CaO	an 2 MgO	Summe der PO_5	SO_3 Aequiv. des S	Schwefel.	Kohle.	Wasserstoff.	Stickstoff.	Sauerstoff
a. Einnahme	2890,7	2269,6	621,1	23,2	597,9						4,3	1,7	313,4	43,7	29,9	209,1
b. Ausgabe	2918,4	2577,4	340,9	29,3	311,6	11,96	2,331	0,406	0,285	3,022	2,9	1,2	110,2	19,8	28,1	133,7
Rest		—307,8									1,4	0,5	203,2	23,9	1,8	75,4

A. Darnach berechnet sich die Stoffwechsel-Bilanz des Gesunden in folgender Weise.

1) G. scheidet in Harn und Excrementen 7,0 grm. N weniger aus, als er in der Einnahme erhalten; 7 grm. N entsprechen 43,5 grm. trockner, diese 190,3 grm. frischer Muskelsubstanz, welche dem Körper neu-angebildet sein müssen. Die Wägungen haben einen täglichen Körper-Gewichtsverlust von 15 grm. ergeben. Es hat also der Körger eine bestimmte Menge von Fett oder Wasser abgegeben. Die 190,3 grm. neuangebildeten Muskelgewebes erfordern zu ihrer Bildung 23,03 grm. C, 3,1 grm. H. 9,9 grm. O, 0,4 grm. S, und 133,7 grm. Wasser, welche neben den 7,0 grm. N dem regressiven Stoffwechsel entzogen wurden.

2) Der Ueberschuss von 34,5 grm. H bildet mit 276 grm. O (183,4 aus dem Ueberschusse der Nahrung, 92,6 grm. der Inspirationsluft entnommen) 310,5 grm. Wasser.

3) Der Ueberschuss von 0,5 grm. S muss zum Theil durch Abschuppung der Epidermis und Verlust an Haaren aus dem Organismus ausgeführt sein.

4) Von den 269,1 grm. überschüssigen C werden 16,7 grm. zur Bildung von 21,4 grm. wasserfreien d. i. 25,8 grm. frischen Fettgewebes (vergl. pag. 34) verwendet, welche 2,5 grm. H, 2,2 grm. O und 3,6 grm. Wasser beanspruchen. Der Rest von 252,4 grm. C dient zur CO_2bildung. Dazu werden 673 grm. O der Inspirationsluft entnommen und 925,4 grm. CO_2 gebildet.

5) Die gesammte Menge des Perspirations-Wassers bilden:
   a) 718 grm. Wasser-Ueberschuss der Einnahme.
   b) 276 grm. aus der Oxydation des überschüssigen Wasserstoffs.
   c) 93,8 grm. zur Balançirung des Körpergewichts angenommene Wasserabgabe.

Summa: 1087,8 grm.

6) Die Gesammt-Einnahme an O der Inspirationsluft beträgt 765 grm.

B. Bei dem Diabetiker gestaltet sich die Bilanz in folgender Weise.

1) D. scheidet 1,8 grm. weniger N aus, als er in der Nahrung erhalten; diese entsprechen 11,2 grm. trockner und 48,9 grm. frischer Muskelsubstanz, in deren Zusammensetzung ausserdem 5,9 grm. C, 0,8 grm. H, 2,6 grm. O, 0,1 grm. S und 34,4 grm. Wasser übergehen.

2) Die als Ueberschuss über die Ausgabe durch Harn und Excrementen restirenden 23,1 grm. H bilden mit 184,8 grm. O (72,8 grm. aus dem Reste der Einnahme, 112 grm. aus der Inspirationsluft) 207,9 grm. Wasser.

3) Der Rest von 0,4 grm. S muss der Ausfuhr durch die Excremente, durch Epidermis-Abschuppung und Haar-Verlust zufallen.

4) 203,2 grm. C werden durch 541,8 grm. O der Inspirationsluft zu 745 grm. CO_2 oxydirt.

5) Die Gesammt-Einnahme an O der Inspirationsluft beläuft sich auf 653,8 grm.

6) Der Perspiration könnte nur das durch die Oxydation des Wasserstoffs gebildete Wasser dienen, von welchem jedoch zuvor der Wasser-Ueberschuss der sensiblen Ausgabe über das Wasser der Einnahme in Abzug zu bringen wäre. Dieser (307,8 grm.) wird aber durch jenen nicht einmal vollkommen gedeckt. Der Rest von 92,7 grm., sowie der durch die Körpergewichts-Zunahme geforderte Ansatz von 281,5 grm., also wenigstens 374,2 grm. Wasser müssten, wenn von einer Täuschung von Seiten des Pat. oder einem Beobachtungsfehler abgesehen wird, von dem diabetischen Organismus täglich aus der atmosphärischen Luft in Dampfform durch die Haut resor-

birt sein. Für eine in solchem Maasse[1]) stattfindende Resorption vermag ich mich nicht zu entscheiden, obgleich ich sowol durch die Fruchtlosigkeit meiner Bemühungen eine Vorstellung darüber zu gewinnen, in welcher Weise eine verbotene Zufuhr von Getränk habe vorkommen können und welcher Posten meiner Rechnungen den Beobachtungsfehler verbergen sollte, sowie durch die Ueberzeugung, dass eine Aufnahme von Wasser durch die Haut zur Zeit auch für den physiologischen Zustand noch nicht als unmöglich nachgewiesen[2]) ist, dazu gedrängt werde.

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3) Wie verhält sich der Stoffwechsel des Diabetikers zu dem des Gesunden unter dem Gebrauche von täglich 8,05 grm. benzoë-sauren Natrons?

Mit Benutzung der an den fünf, für die Beantwortung dieser Frage bestimmten, Versuchstagen enthaltenen Resultate lassen sich folgende Tabellen zusammenstellen.

1) Der von Home (klinische Vorträge pag. 348) beobachtete Diabetiker soll allerdings während zwei Stunden des Vormittags um 105 grm. durch Einsaugung an Gewicht gewonnen haben, was auf 24 Stunden berechnet, bei gleichbleibender Resorptionsthätigkeit der Haut, 1260 grm. betragen würde. Vergl. C. Weber, Ueber den Diabetes mellibus. Innugural-Dissertation. Würzburg. 1854, pag. 15.

2) Vergl. darüber: O. Funke, Lehrbuch der Physiologie. Bd. I. Leipzig 1863. pag. 594 und 595. Falck, Archiv für physiolog. Heilkunde. Bd. XI. 1852. pag. 768 und 770. C. G. Lehmann, Handwörterbuch der Physiologie, Artikel „Harn“. pag. 22. L. Lehmann, Ueber die Wirkung 12°—7,7° R. warmer Sitzbäder. Archiv für wissenschaftliche Heilkunde. Bd. I. 1853. pag. 540. Valentin. Moleschotts Untersuchungen zur Naturlehre des Menschen und Thieres. 1857. pag. 223 und ff. Fr. Nasse. Die Wasserbildung im Diabetes. Archiv für physiologische Heilkunde. Stuttgart. 1851. pag. 72.

III. Periode. 15 Tage.

A. Gesunder.

Anfangsgewicht 55411 grm. — Endgewicht 55471 grm. Gewichtsdifferenz + 60.

	Gesammt- a. Einnahme, b. Ausgabe, c. s. d. Ausgabe	Wasser	Trockne Substanz	Mineralsalze	Organische trockne Substanz	Kochsalz	Phosphorsäure geb. an Alkal.	Phosphorsäure an 3 CaO	Phosphorsäure an 2 MgO	Phosphorsäure Summe der PO_5	Schwefelsäure Aequiv. d. S.	Schwefel	Kohlenstoff	Wasserstoff	Stickstoff	Sauerstoff, s. geb. an S.
a. Einnahme	14433	10938,7	3494,3	118,045	3376,295						27,27	10,91	1797,355	251,905	184,105	1132,02
+																6,426 s
b. Harn 243,793 U	7488,5	7012,5	476	104,960	371,07	61,972	9,274	1,191	0,870	11,335	10,71	4,28	48,758	16,088	133,844	65,093
4,931 kr.													2,096	0,306	1,834	0,695
7,182 h.													4,331	0,366	0,560	1.925
Excremente	603	390,7	212,26	25,03	187,23								99,736	14,776	12,842	59.776
c. Summe d. Ausgab.	8091,5	7403,2	688.26	129,960	558,30	61,972	9,274	1,191	0,870	11,335	10,71	4.28	154,921	31,536	149,090	133,915

B. Diabetiker.

Anfangsgewicht 51607 grm. — Endgewicht 49327 grm. Gewichtsdifferenz — 1280.

	Gesammt- a. Einnahme, b. Ausgabe, c. s. d. Ausgabe	Wasser	Trockne Substanz	Mineralsalze	Organische trockne Substanz	Kochsalz	Phosphorsäure geb. an Alkal.	Phosphorsäure an 3 CaO	Phosphorsäure an 2 MgO	Phosphorsäure Summe der PO_5	Schwefelsäure Aequiv. d. S.	Schwefel	Kohlenstoff	Wasserstoff	Stickstoff	Sauerstoff, s. geb. an S.
a. Einnahme	14233	10938,7	3294,3	118,045	3176,295						27,27	10,91	1793,055	239,055	184,105	1029,17
b. Harn + 309,709 U													61,943	20,437	144,637	82,692
5,285 kr.	16633,6	14983,6	1650	153,380	1496,620	86,586	13,388	2,269	1,684	17,341	16,800	6,72	2,245	0,335	1,965	0,740
7,670 h.													4,625	0,391	0,598	2,056
1034,0 Zuck.													413,60	68,92		551,48
Excremente	962	623,4	338,6	39,92	298,698								159,115	23,576	20,49	95,526
c. Summe d. Ausgab.	17595,6	15607,0	1988,6	193,300	1795,313	86,586	13,388	2,269	1,684	17,341	16,800	6,72	641,528	113,659	167,690	732,494

Darnach berechnet sich die Grösse der Einnahmen und Ausgaben beider Versuchspersonen für den mittleren Versuchstag der III. Periode in folgender Weise:

A. Gesunder.

Gewichtsdifferenz + 12.

	Gesammt	Wasser	Trockne Substanz	Mineralsalze	Organische trockne Substanz	Kochsalz	geb. an Alkal.	an 3 CaO	an 2 MgO	Summe der PO_5	Schwefelsäure	Schwefel	Kohlenstoff	Wasserstoff	Stickstoff	Sauerstoff
Einnahme	2886,6	2187,7	698,9	23,6	675,3						5.5	2,2	359,5	50,4	36,8	226,4
Ausgabe	1618,3	1480,6	137,6	26,0	111,7	12,4	1,855	0,238	0,174	2,3	2,1	0,9	31,0	6,3	29,8	25,5
Rest		707,1										1,3	328,5	44,1	7,0	200,9

B. Diabetiker.

Gewichtsdifferenz — 256.

	Gesammt	Wasser	Trockne Substanz	Mineralsalze	Organische trockne Substanz	Kochsalz	geb. an Alkal.	an 3 CaO	an 2 MgO	Summe der PO_5	Schwefelsäure	Schwefel	Kohlenstoff	Wasserstoff	Stickstoff	Sauerstoff
Einnahme	2846,6	2187,7	658,9	23,6	635,8						5,5	2,2	342,6	47,8	36,8	205,8
Ausgabe	3519,1	3121,4	397,7	38,9	359,1	17,5	2,678	0,454	0,337	3,2	3,4	1,3	128,3	22,7	33,5	146,5
Rest		—933,7										0,9	214,3	25.1	3,3	59,3

A. Demnach gestaltet sich die Stoffwechsel-Bilanz des Gesunden in folgender Weise.

1) Die 7,0 grm. N, welche weniger ausgeschieden wurden, als die N-Zufuhr betrug, entsprechen 43,5 grm. trockner, diese 190,3 grm. frischer Muskelsubstanz. Die Wägungen haben einen täglichen Gewinn des Körpergewichts von 12 grm. ergeben. Dem zu Folge hat der Körper eine bestimmte Menge von Fett oder Wasser abgegeben. Die 190,3 grm. neu-angebildeten Muskelgewebes erfordern zu ihrer Bildung 23,03 grm. C, 3,1 grm. H, 9,9 grm. O, 0,4 grm. S und 133,7 grm. Wasser, welche neben den 7,0 grm. N dem regressiven Stoffwechsel entzogen wurden.

2) Der Rest von 29,6 grm. H bildet mit 236,8 grm. O (180,9 grm. dem Exspirations-Rest, 55,9 grm. der Inspirationsluft entnommen) 266,4 grm. Wasser.

3) Von dem C-Rest von 328,5 grm. werden 76,1 grm. (vergl. pag. 34) zur Bildung von 97,4 grm. wasserfreien d. i. 117,7 grm. frischen Fettgewebes verwendet, welche 11,4 grm. H, 10,1 grm. O und 16,3 grm. Wasser beanspruchen. Der Rest von 252 grm. C dient zur CO_2-Bildung. Dazu werden 673 grm. der Inspirationsluft entnommen und 952,4 grm. CO_2 gebildet.

4) Der Rest von 0,9 grm. S muss der Ausfuhr durch die Excremente, durch Epidermis-Abschuppung und Verlust von Haaren zugewiesen werden.

5) Die Gesammt-Summe des Perspirations-Wassers setzt sich aus folgenden Posten zusammen:

a) 707 grm. Wasser-Ueberschuss der Einnahme.
b) 266,4 grm. durch Oxydation von Wasserstoff.
c) 146 grm. zur Balançirung des Körpergewichts angenommene Wasser-Abgabe.

Summe 1119,4.

6) Die Gesammt-Einnahme an O der Atmosphäre beträgt 728,9 grm.

B. Die Bilanz des Diabetikers berechnet sich folgendermaassen:

1) 3,3 grm. nicht in die Exerete übergegangenen Stickstoffs entsprechen 20,5 grm. trockner und 89,7 grm. frischer, neu-angebildeter Muskelsubstanz. Diese beanspruchen zu ihrer Bildung 10,9 grm. C, 1,4 grm. H, 4,7 grm. O, 0,2 grm. S und 63 grm. Wasser.

2) Der H-Rest von 23,7 grm. bildet mit 189,6 grm. O (54,6 grm. dem Exspirations-Rest, 135 grm. der Inspirationsluft entnommen) 213,3 grm. Wasser.

3) 214,3 grm. C erfordern zu ihrer Oxydation zu CO_2 571,4 grm. O, die tägliche CO_2-Exspiration beträgt demnach 758,7 grm. CO_2.

4) Die Gesammt-Einnahme an O der Inspirationsluft beläuft sich auf 706,4 grm.

5) Der Rest von 0,7 grm. S fällt der Ausfuhr durch den Darm etc. zu.

6) Der Perspiration könnte nur die aus der Oxydation von H hervorgegangene Wasser-Menge (213,3 grm.) + der durch den täglichen Körpergewichts-Verlust geforderten Wasserabgabe dienen, nach Abzug der in der sensiblen Ausgabe als Ueberschuss über die Einnahme erschienenen Quantität. Da aber letztere erstere noch erheblich (um 464,4 grm.) übertrifft, so fällt die weitere Erklärung mit der auf pag. 78 gegebenen zusammen.

Thesen.

1. Für die Behandlung der Nachgeburts-Periode ziehe ich die sogenannte aktive der passiven Methode vor.
2. Es giebt keine zuckerbildende Funktion der Leber.
3. Es ist wünschenswerth, dass die forensisch-medicinische Praxis den Vorgängen während (in) der Geburt grössere Aufmerksamkeit zuwende.
4. Der Diabetes mellitus beruht auf einer gesteigerten Zersetzung der Albuminate des Körpers.
5. Für eine sichere Erkennung von Neigungen und Beugungen der Gebärmutter ist in den meisten Fällen die Sonde entbehrlich.
6. Gegen die Möglichkeit einer selbst längere Zeit anhaltenden Mehrausgabe von Wasser durch die sensiblen Ausgaben über das Wasser der Speisen und des Getränks im Diabetes mellitus spricht weder Theorie noch Erfahrung.

1.

Welche wissenschaftlichen Erfahrungen lassen sich bei dem Rekrutirungs-Geschäft gewinnen

und

in welcher Weise würde sich dieses Geschäft am zweckmässigsten organisiren lassen, um jene Erfahrungen in zuverlässiger Weise zu gewinnen?

Vom

Stabsarzt Dr. **Horn** zu Berlin.

Unter Rekrutirungs-Geschäft versteht man die Revue der männlichen Bevölkerung eines Staates behufs Erklärung ihrer Militärbrauchbarkeit, gleichgültig, ob Werbesystem, Conscription oder allgemeine Wehrpflicht dieselben zusammenberuft.

Das Werbesystem, der Contract des Staates mit dem Einzelnen, seine Körperkraft für die Zwecke des Krieges zu verwenden,

die Conscription, die Forderung an jeden Staatsangehörigen, dem Staate mit den Waffen zu dienen, jedoch mit dem Zugeständnisse der Stellvertretung und dem Loskaufsrechte verbunden,

die allgemeine Wehrpflicht, die Ausbildung der gesammten männlichen Bevölkerung im Waffendienste ohne Ausnahme, ob es den Reichen oder den Armen betrifft, — führen dem bei jenem Geschäfte thätigen Arzte gewisse Altersclassen der Bevölkerung eines Landes zu und gestatten ihm, den competenten Richter über Tauglichkeit zu jenem Dienste, sich ein Urtheil über die physische Kraft derselben zu bilden, im Gegensatze zu ihr aber auch die physische Schwäche, das Heer der Krankheiten kennen zu lernen.

Jene drei Systeme der Ergänzung der stehenden Heere gestatten aber nicht, die Resultate dieser Beobachtungen statistisch unbedingt neben einander zu stellen, um die physischen Kräfte der Völker zu vergleichen, denn dazu gehören gleiche Bedingungen, gleiche Anforderungen und gleiche Maasse.

I. Das Werbesystem, mit der Bildung der stehenden Heere eingeführt, existirt für jetzt in Europa nur noch in Portugal und in England. Je nach dem Preise, welchen die Regierung aussetzt, je nach den Anlockungen, mit welchen Krieg oder Politik die Menge herbeizieht, oder auch je nach der Noth, welche den Armen zwingt, seine Kräfte zu verkaufen, wird der Markt der Werbebureaus, wenn ich mich so ausdrücken darf, mehr oder weniger belebt sein; der Werbe-Officier, unter dem Beistand des sachkundigen Arztes, wählt die besten Körper. Alle Aufzeichnungen des Arztes über die vorgekommenen Krankheitsformen werden nur darthun, dass dieser oder jener Körperfehler unter einer bestimmten Anzahl von Menschen mehr oder weniger häufig, durch Vergleichung vieler solcher Musterungen vielleicht auch ziemlich constant vorkommt, bei dieser oder jener Volksclasse sogar vorwiegt; ein Rückschluss auf die physische Kraft der Bevölkerung darf nur mit grossen Einschränkungen gemacht werden und hat nur geringen relativen Werth. Noch viel weniger ist es aber gestattet, daraus die endemische oder epidemische Verbreitung gewisser Krankheitsformen ableiten zu wollen.

Der Bedarf des Heeres an neuem Ersatze wird je nach Zeit und Verhältnissen die Anforderungen an die Militair-

brauchbarkeit steigern oder verringern. Wie bedeutend dieser Unterschied sein kann, geht aus den Berichten des *Provost-Marshal-General* der Vereinigten Staaten von Nord-Amerika hervor (*Annual-Report* 1864. November).

Im Jahre 1852 wurden 16,064 Mann behufs Anwerbung untersucht; von diesen wurden 13,338 für unbrauchbar erklärt, also 83 pCt., während in den Jahren 1863 und 1864 von 316,445 nur 95,868, also 28,5 pCt., unbrauchbar waren.

Der grosse Unterschied beider Zahlen kann nur erklärt werden durch die verschiedenen Anforderungen, welche an die Brauchbarkeit der Einzelnen gemacht wurden.

In Nordamerika wurden im Frieden die Angeworbenen auf den *Recruiting-rendez-vous* einzelner Districte, in dem sogenannten *Recruiting-depôt*, vor Absendung zum Regiment und bei Ankunft zu demselben, mithin vier bis fünf Mal, ärztlich untersucht; stellte sich bei der letzten Untersuchung nun noch heraus, dass der Rekrut nicht brauchbar sei und der Körperfehler schon vom ersten Arzte hätte entdeckt werden können, so wurde dieser zur Zahlung der Kosten verurtheilt; somit fand gewiss eine sorgsame Auswahl der Rekruten statt.

Bei dem *Regimental-recruiting-service* hatte der dienstthuende Surgeon den Dienst (Revised U. S. Army Regulations. 1863. §. 929.); eine eigentliche Ersatz-Instruction gab es für die reguläre Armee nicht.

Die Missstände in der Voluntär-Armee während der Jahre 1861 und 1862 führten zur Conscription. Zwar sollten auf Befehl des Kriegsministers vom August 1861 alle Truppen kurz vor oder nach ihrer Einstellung untersucht werden, allein die Berichte der *Sanitary-Commission* zeigen, dass nur 42 pCt. vor, 9 pCt. nach der Einstellung untersucht sind. *Hammond* selbst erzählt (*Military-Hygiene*), dass die Aerzte nur die Reihen der Truppen hinuntergingen und wo sie selbst ausmustern wollten, auf Hindernisse bei den Officieren stiessen. Die Voluntär-Regimenter wurden nämlich meist von ehrgeizigen Capitalisten angeworben, die dann Befehlshaber und deren Freunde Officiere wurden. Die Folgen zeigten sich bald; theils musste der grössere Theil als untüchtig entlassen werden (aus der Potomac-Armee allein 2000 Ende 1861, im Ganzen während der beiden Jahre 1861 und 1862 gegen 200,000), theils desertirte eine enorme Anzahl (laut officiellen Berichten des *Provost-Marshal-General* wurden 60,760 in 17 Monaten als Deserteure gefangen [Annual Report pag. 8]), um sich nochmals anwerben zu lassen oder um den Strapazen des Krieges zu entgehen.

Am 3. März 1863 wurde durch die *Act for enrolling and calling out the national forces* die Conscription verfügt für alle von 20—45 Jahr alte Männer der U. St., die bürgerliche Rechte ausgeübt hatten, mit wenigen Ausnahmen, wie Staatsbeamte, einziger Sohn einer Wittwe etc, aber mit Loskaufsrecht (300 Dollars für jede Aushebung) und Substitutionsrecht. Jedem Staate wurde aufgegeben, durch die sogenannte „*draft*" eine gewisse Anzahl brauchbarer Leute zu stellen; kam dieses nicht zu Stande, so wurde vom Präsidenten innerhalb 50 Tagen eine zweite *Compulsing draft* ohne Loskaufsrecht, aber mit Substitution, ausgeschrieben.

Die körperliche Untersuchung geschah vor dem *board of enrollment* von einem Militärarzte, jedoch nur derjenigen, welche sich krank meldeten. Auf dem *general rendez-vous* wurden die Ausgehobenen wiederum untersucht und besonders die untauglichen Substituten ausgemustert.

Im Jahre 1863 war eine Aushebung von 200,000 Mann

angeordnet, allein es waren nur 90,000 Mann, im Jahre 1864 von 83,000 nur 45,000 zu erlangen.

Ich habe diese Verhältnisse hier besonders angeführt, um später bei Benutzung der von dem *Provost-Marhal-General* zusammengestellten Tafeln über Unbrauchbarkeit der Untersuchten nicht wiederholt Erklärungen geben zu müssen.

II. Die Conscription wurde zuerst in Frankreich eingeführt.

Das Rekrutirungsgesetz vom 19. Fructidor des Jahres VI (1798) erklärte jeden Franzosen für wehrpflichtig und schuldig, das Vaterland zu vertheidigen, und legte jedem aus irgend einem Grunde davon Befreiten eine Steuer auf; das Gesetz vom 17. Ventose des Jahres VIII (1800) forderte von Allen, welche zu schwach zum Dienste oder um ihre Studien fortzusetzen befreit waren, einen Ersatzmann; *Napoleon I.* führte das Loosungs-System ein, erhob aber von den Unbrauchbaren eine Steuer, welche erst im Jahre 1818 aufhörte. (Eine solche Steuer besteht jetzt noch in der Schweiz, *Engel* will dieselbe auch für Preussen eingeführt wissen [Zeitschrift des statistischen Bureaus. 1864. Nr. 3. pag. 83]).

Das Gesetz „*sur le recrutement de l'armée*" vom 21. März 1852 regelte die Verhältnisse, brachte nähere Bestimmungen über *Engagement volontaire* (*Titre III., Art. 31*), über *rengagement* (in *Art. 19, Titre II.*) und die Bedingungen für den *remplaçant*. Bis zum Jahre 1850 musste Jeder seinen *remplaçant* selbst stellen und war verantwortlich für ihn; *Napoleon III.* änderte dies, nicht zu Gunsten der Conscription, sondern um sich ein ergebenes, altgedientes Heer zu schaffen. Die Regierung übernahm es, die Stellvertreter zu engagiren; jeder Dienstpflichtige kann sich gegen Zahlung einer Summe von 2400 Francs an die Dotationscasse loskaufen. Die Folge davon war, dass die Zahl der einzustellenden Rekruten in jedem Jahre ab, die Zahl der gedienten Leute dagegen zunahm.

Das französische Rekrutirungsgesetz schreibt nur eine einmalige Untersuchung der militairpflichtigen Mannschaften vor, obschon viele, besonders Untermässige und Schwächliche, nach Verlauf von 1—2 Jahren bei einer zweiten und dritten Untersuchung vollkommen tüchtig erscheinen würden.

Ueber die Brauchbarkeit der Conscribirten hat ein *Officier de santé militaire* vor einer Commission, dem *Conseil de revision*, sein Urtheil abzugeben; als Richtschnur dient dazu *l'instruction pour servir de guide aux officiers de santé, dans l'appréciation des infirmités ou maladies qui rendent impropre au service militaire* (14. November 1845). In derselben sind die körperlichen Fehler, welche die *infirmité* bedingen, aufgezählt und mit Anmerkungen versehen. Dieselben sind vollständig nach Körpergegenden geordnet, wie sie im gewöhnlichen Gange der Untersuchung auf einander folgen: Kopf, Hals, Brust etc.

Eine Aufzählung dieser die Unbrauchbarkeit bedingenden Krankheiten und Gebrechen ist auch in der Tafel *D* der *Comptes rendus annuels sur le recrutement des armées* enthalten; diese muss bei den Zusammenstellungen der Militairärzte des *Conseil de révision* benutzt werden, die wiederum zur statistischen Bearbeitung der eben genannten Berichte dienen. Leider habe ich Letztere nicht erlangen können, höchstens Auszüge derselben in den *„Memoires de medic. milit."* gefunden.

III. Die **allgemeine Wehrpflicht** in Preussen besteht gesetzlich erst seit dem 3. September 1814.

Im 18. Jahrhundert war das Land in Districte getheilt, in welchen die Regimenter aushoben und anwarben; das Reglement für die Infanterie vom 11. März 1726, Th. II., Tit. 7., enthielt über die Untersuchung der in Dienst tretenden Soldaten die erste Verordnung, welche lautete: „Die Obristen und Capitaine müssen alle Kerls, bevor sie selbige annehmen und schweren lassen, wohl visitiren ob die Kerls gut und zu Kriegsdiensten capables sind".

Ebenso wurden zu Folge der unter *Friedrich II.* erschienenen Reglements die Beurtheilung und Wahl der Rekruten den Officieren überlassen. Erst in dem Reglement für die Infanterie von 1788, Th. II., Tit. V., Art. 13., findet sich die Bestimmung, dass die Regiments- und Bataillons-Feldscheere die angeworbenen Leute wohl und genau visitiren sollten, ob sie zum Felddienste tüchtig seien und keine Fehler und Gebrechen hätten. Für die Aushebung im Inlande wurde 1792 im Canton-Reglement die allgemeine Wehrpflicht zwar zur Pflicht gemacht und als Princip vorangestellt, durch die vielen Ausnahmen, welche sich auf ganze Landes-Districte, Städte und Stände bezog, jedoch illusorisch, so dass nur die armen und ungebildeten Handwerksgesellen und Tagelöhner die Pflicht zu dienen hatten.

(*Richter*, Geschichte des Medicinal-Wesens der Preussischen Armee. 1860. pag. 27.)

Während des Jahres 1808 wurde das sogenannte Krümper-System eingeführt, Cantonisten in Stelle beurlaubter Soldaten eingezogen, militairisch ausgebildet und wieder entlassen, um anderen Platz zu machen; hierdurch wurde eine zahlreiche Kriegsreserve gebildet, welche bei der Erhebung des Volkes im Jahre 1813 von wesentlichem Vortheil war.

Schon im Beginne des Krieges im Jahre 1813 hatte *Görcke* eine Instruction über Brauchbarkeit zum Feld- und Garnison-Dienste erlassen; nach dem Kriege erfolgten eine grössere Anzahl von Instructionen, Regulativen, Organisationsplänen, Bestimmungen etc., welche je nach Bedürfniss bald diese, bald jene Seite des Ersatzgeschäftes zu verbessern suchten. Zu den ärztlichen Instructionen gehören besonders die vom 16. August 1817 und vom 14. Juli 1831, denen eine Masse von einzelnen Bestimmungen und Erklärungen folgte, durch welche frühere aufgehoben, verändert oder commentirt wurden. Alle diese Veränderungen, besonders die der militairischen Verwaltung, machten eine neue Militair-Ersatz-Instruction nothwendig. Dieselbe erschien mit einer Instruction für Militair-Aerzte zur Untersuchung und Beurtheilung der Dienstbrauchbarkeit unter dem 9. Dezember 1858 und trat mit dem 1. Januar 1860 in Kraft.

Diese Instructionen hier vollständig zu erläutern, erlaubt der Raum nicht; Dr. *Engel* hat (in Nr. 3 der Zeitschrift des statistischen Büreaus, Jahrgang 1864) eine recht klare Uebersicht der Grundzüge des Militair-Ersatz-Geschäftes behufs Erklärung der von ihm gelieferten Tabellen gegeben; eine für Ausländer gewiss sehr schätzenswerthe Arbeit, da hierdurch das Verständniss der etwas weitläufigen und darum nicht ganz klaren Instruction wesentlich erleichtert wird.

Für militairische Zwecke, besonders für die Berechnung des jährlich einzustellenden Ersatzes sind die von den Regierungen gelieferten Listen ganz ausgezeichnet; die Führung der Stammrollen und alphabetischen Listen ist eine so vorzügliche, dass jeder einzelne Militairpflichtige auf etwaige Recherchen der Behörden herausgefunden werden kann, allein für Zwecke der Statistik wären, wie wir weiter unten noch nachweisen werden, wohl noch einige Rubriken einzuschalten.

Die sogenannte Arztliste, d. h. eine für jeden Tag der Untersuchung nach einzelnen Ortschaften und in diesen wieder nach einzelnen Jahrgängen aufgestellte, nach dem Alphabet geordnete Liste dient dem Arzte zur Notirung seines Gutachtens; letzteres wird ebenfalls in die alphabetische Liste aufgenommen.

Sein Urtheil „Brauchbar oder Unbrauchbar zur Einstellung“ ist für die Commission nicht bindend, die Entscheidung, ob der Vorgestellte die erforderliche Körperkraft besitzt, kommt dem Militair-Vorsitzenden zu. Mithin ist der Arzt nur Sachverständiger, er hat nur die Fehler anzugeben, welche die Unbrauchbarkeit bedingen, höchstens ist es ihm erlaubt, sein Urtheil zu Protocoll zu geben.

Das Verkennen dieses Standpunktes von Seiten des Arztes, sowie die Ueberhebung des Militair-Vorsitzenden, der dem sachgemässen Rathe des ärztlichen Beirathes nicht folgt, ruft sehr oft Klagen von beiden Seiten hervor.

In der Instruction für Militair-Aerzte sind in den §§. 15 und 21 die verschiedenen Kategorien von Unbrauchbarkeit in welche die Gemusterten eingetheilt werden sollen, sowie die Fehler und Krankheiten angegeben, welche erstere bedingen. Hiernach wird in die Listen eingetragen:

1) vollkommen dienstfähig, kurzweg brauchbar;
2) nicht vollkommen dienstfähig (bei der dritten Vorstellung: Ersatz-Reserve);
3) zeitig dienstunbrauchbar (zu schwach, ein Jahr zurück);
4) dauernd dienstunbrauchbar.

Die vierte Abtheilung, „dauernd dienstunbrauchbar“, zerfällt insofern in zwei Abtheilungen, als:

a) die augenfällig Unbrauchbaren, wie Lahme, Blinde, Krüppel etc, von der Kreis-Ersatz-Commission direct ausgemustert werden und nicht wieder zur Vorstellung gelangen, und
b) die dauernd Dienstunbrauchbaren, von der ersteren Commission ausgewählt, aber erst von der

zweiten der Departements-Ersatz-Commission bestätigt werden. Stimmt diese nicht der ersten bei, so erscheint der Militairpflichtige noch mehrmals zur Untersuchung.

In Nr. 3 werden alle diejenigen zusammengefasst, die Fehler haben, welche während der drei Vorstellungsjahre sich noch bessern können, oder diejenigen, welche noch zu schwach sind, im dritten Jahre aber oft genommen werden.

Zu Nr. 2 gehören endlich diejenigen, welche zwar mit Fehlern behaftet sind, die sie nicht vollkommen dienstfähig erscheinen lassen, im Falle des Krieges aber keine Rücksicht verdienen, oder diejenigen, welche im dritten Jahre zu schwach sind; dies ist gewöhnlich bei weitem der grössere Theil der Untersuchten.

Durch Aufstellung dieser vier Abstufungen will man

1) soviel wie möglich zum Waffendienst heranziehen;
2) auch diejenigen, deren körperliche Entwickelung erst nach dem 21. bis 23. Jahre vollendet ist, und
3) für den Fall des Krieges die Massen der bis zum 24. Jahre noch nicht vollkommen Brauchbaren oder mit geringen Fehlern Behafteten aufsparen.

Dadurch nun, dass die Militairpflichtigen nicht, wie in Frankreich oder in anderen Ländern mit Conscriptionssystem nur einmal, sondern mehrmals vor den Commissionen erscheinen, geschieht allerdings die sorgsamste Auswahl, die Schwierigkeit, richtige Aufzeichnungen der vorgefundenen Körperfehler zu erhalten, steigert sich jedoch bedeutend, ja richtige Resultate auch nur über die Zahlen der Brauchbaren und Unbrauchbaren zu gewinnen, ist nur möglich, wenn sie von jedem Jahrgang einzeln ausgezogen werden.

Bei den Vorschlägen zur Erreichung richtiger Resultate komme ich darauf zurück.

Was die Fehler betrifft, welche jene drei Abstufungen der Unbrauchbarkeit bedingen, so sind dieselben nicht immer nach den wirklich zu Grunde liegenden Krankheiten benannt, sondern meist nur als Symptome bezeichnet, die dem Laien, d. h. hier also den Mitgliedern der Commission, theilweise verständlich sind. Meist sind dieselben nach den Körpergegenden geordnet, dem Gange der Untersuchung gemäss, die sich jeder Arzt nach gewissen Grundsätzen der Zweckmässigkeit angewöhnt. Neben dem Auffinden dieser Fehler wird jedoch vom Arzte auch ein Gutachten über die Körperstärke des Militairpflichtigen verlangt, welche seine Brauchbarkeit bedingt.

Nachdem wir so die Grundlagen der drei verschiedenen Rekrutirungssysteme betrachtet haben, kommen wir zur näheren Erörterung der Frage: **Welche wissenschaftliche Erfahrungen lassen sich bei diesen Geschäften gewinnen?**

Zu wissenschaftlichen Erfahrungen gelangt man durch Beobachtung, Sammlung der gemachten Beobachtungen und Vergleichung der gefundenen Resultate.

Die Hauptforderung des Rekrutirungs-Geschäftes an den untersuchenden Arzt, die Entscheidung über **Brauchbarkeit** und **Unbrauchbarkeit** des Militairpflichtigen oder Angeworbenen zwingen den Arzt, gewisse Beobachtungen an jedem einzelnen Individuum zu machen; versteht er es, diese einzelnen Beobachtungen zweckmässig zu sammeln und die Resultate derselben zu vergleichen, so kann er dadurch zu wissenschaftlichen Erfahrungen gelangen.

I. Die Brauchbarkeit zum Militairdienst.

Die Brauchbarkeit zum Militairdienst ist von der durch die Entwickelung des Körpers erlangten **Körperkraft** der

Untersuchten nach Ausschluss derjenigen, welche durch Krankheit und Gebrechen untauglich sind, abhängig. Dieselbe zu beurtheilen, genügt der sogenannte praktische Blick nicht, weil er zu grossen Irrthümern unterworfen ist; man ist daher von jeher bemüht gewesen, für die Bedingungen der Mililairdienstbrauchbarkeit, also für die zum Dienste nothwendige Körperkraft, einen sicheren Maassstab zu gewinnen.

Da das Alter der Militairpflichtigen gewöhnlich durch das Gesetz bestimmt ist, so ist zur Beurtheilung des Kräftigkeitsgrades sonst gesunder Menschen benutzt worden: 1) die Körpergrösse, 2) der Umfang der Brust, 3) die Schwere des Körpers und 4) die Kraftäusserung seiner Muskulatur.

Diese vier Momente lassen sich nämlich durch Zahlen ausdrücken und werden bei zahlreichen Beobachtungen relative Mittelwerthe ergeben, die dem Arzte nicht allein sein mühsames Geschäft erleichtern, sondern auch gestatten, die physische Kraft jedes Einzelnen durch Zahlen festzustellen bei richtiger statistischer Verwendung dieser Zahlen aber die physische Kraft der gesammten männlichen Bevölkerung eines Staates zu ermitteln.

Es sei nun hier gestattet, diese einzelnen Momente, welche zur Abschätzung des Kräftigkeitsgrades verwendet werden, näher zu betrachten, und die wissenschaftlichen Resultate, welche dieselben bis jetzt gegeben haben, kurz zu erwähnen.

1. Körpergrösse.

Die Körpergrösse oder vielmehr Körperlänge ist früher fast nur einziger Maassstab der Militairbrauchbarkeit gewesen, natürlich nur bei sonst gesunden Individuen. Unter dem im Anfange des vorigen Jahrhunderts gebräuchlichen Canton-

system wurden die jungen Männer jährlich gemessen, und wenn sie eine bestimmte, ziemlich ansehnliche Höhe erreicht hatten, eingestellt. (*Wendroth*, Anleitung zur Untersuchung Militairpflichtiger. I. pag. 44.)

Boudin gibt in einem Aufsatze (*sur l'accroissement de la taille et de l'aptitude militaire en France* in dem *Rec. mém. de méd. chir. et de pharm. mil. 3 Ser. X. Mars 1863. pag. 4*) an, dass unter den wesentlichsten Bedingungen der Tüchtigkeit zum Militairdienst bei allen alten und neuern Völkern ein Minimum der Körpergrösse festgestellt ist.

Bei den alten Römern war das niedrigste Maass 5¼′ (unter Kaiser *Hadrian I.* 1,638 Meter). *Nero* verlangte 6′ für die Aufnahme in die *Phalanx Alexandri*. Eine Verordnung von *Louis XIV.* (1701) setzte das Minimum der Körpergrösse auf 5 Fuss fest (1,624 Meter), von 1789—1793 blieb das Minimum bei 1,598 stehen, 1804 setzte man es auf 1,544 herab, um 1808 wiederum auf 1,570 zu steigen; endlich bestimmte das noch jetzt gültige Gesetz vom 21. März 1831 das Grössen-Minimum 1,560 Meter.

Bei den Engländern gilt ein Minimum von 5′ 4″, gleich 1,659 Meter, in Nord-Amerika 5′ 6″. In Preussen galt im vorigen Jahrhundert auch ein grösseres Maass; seit der Einführung der allgemeinen Wehrpflicht war ein Minimum von 5′ 2″ (rheinisch) nothwendig, bis zum Jahre 1860; mit dem Eintritt der Reorganisation der Armee, d. h. einer bedeutenden Vermehrung des jährlichen Ersatzes, ist man auf 5′ 1″ 3‴, ja bei sehr kräftig gebauten Leuten auf 5′ heruntergegangen. Hieraus ersieht man, sowohl aus dem Wechsel des Minimum-Maasses in Frankreich, als auch in der Herabsetzung desselben in Preussen, dass je nach dem grösseren Bedarf an Ersatz das Minimum geschwankt hat, d. h. dass man also dasselbe herabgesetzt hat, um nur hinreichenden Ersatz zu erhalten, wobei allerdings nicht zu vergessen ist, dass die Neuzeit eine auffallende Erleichterung durch bessere, zweckmässigere und besonders leichtere Bewaffnung herbeigeführt hat, welche eine geringere Körperkraft beanspruchen.

Kann man nun aus der bei der Musterung erhaltenen Summe der Leute, welche das Minimum nicht erreicht haben, in ihrer körperlichen Entwickelung also zurückgeblieben sind, wenn man dieselbe mit der Zahl der Untersuchten vergleicht, sich irgend welche Rückschlüsse auf die physische Kraft der männlichen Bevölkerung erlauben? Gewiss, wenn nur anerkannt wird, dass die Körpergrösse einen sicheren Maassstab dafür abgibt. Auf die Zu- und Abnahme derselben während verschiedener Jahre jedoch darf dies nur stattfinden, wenn die Maasse und die übrigen Bedingungen gleiche waren oder die Zahlen demgemäss reducirt werden.

Diese ist z. B. von *Boudin* in seiner Arbeit *de l'accroissement de la taille en France (Rec. de médic. mil. pag. 4, 3 Ser. X. 1863)* geschehen, in welcher er nachzuweisen sucht, dass die Körpergrösse in Frankreich zugenommen haben muss, weil die Zahl der *exemptés par defaut de taille* abgenommen hat, und zwar von 9 pCt. bis auf 60 pCt. innerhalb 30 Jahren. Die Zahlen sind aus den *Comptes rendus sur le recrutement* entnommen und stimmen mit den von *Sistach* gelieferten (*Rec. de mém. de méd. mil. 1861. tom. VI. pag. 353*) ziemlich überein. Die Richtigkeit derselben kann nicht in Zweifel gestellt werden, wenn nicht während dieser 30 Jahre andere Bestimmungen auf die Zahlen jedes Jahres Einfluss ausgeübt haben.

Zu der auf pag. 42 a. o. angegebenen Vergleichung der Länder nach der in ihnen vorgefundenen Anzahl der wegen Mindermaass Untauglichen ist *Boudin* jedoch nicht berechtigt. Da nämlich das Wachsthum der Körperlänge mit dem 20. Jahre, wie aus den Untersuchungen von *Quetelet* (Ueber

den Menschen und die Entwickelung seiner Fähigkeiten, übersetzt von *Riecke*. Stuttgart 1838. pag. 354) und von *Liharzik* (das Gesetz des menschlichen Wachsthums etc. Wien 1858), hervorgeht, noch nicht vollendet ist, die jährliche Messung der Militairpflichtigen dies auch sehr auffallend bestätigt, so wird nach den preussischen Instructionen ein Zwanzigjähriger noch nicht für unbrauchbar zum Militairdienst erklärt, sondern nur wegen Mindermaass zurückgestellt. Erst nach drei Jahren ergiebt sich also hier die Zahl der wegen Mindermaass Untauglichen. Dieses muss wohl in Betracht gezogen werden, wenn man eine Vergleichung der Untersuchten zu den wegen Untermaass Unbrauchbaren verschiedener Nationen aufstellen will; in den Listen von Preussen z. B. sind in der Rubrik „wegen Untermaass unbrauchbar oder zurückgestellt" drei Jahrgänge zusammengefasst; kommt hierzu noch, dass das Mindermaass so bedeutend verschieden ist, wie das französische und preussische, so müssen sich vollständig falsche Resultate ergeben. Beide Fehlerquellen hat *Boudin* (pag. 42) in der oben erwähnten Arbeit nicht berücksichtigt, wenn er eine Curve bildet aus den Zahlen der wegen Mindermaass Zurückgestellten. Hiernach wären in Frankreich auf 1000 *examinés* 58,7 *pour défaut de taille* zurückgestellt, in Belgien 134, in Oesterreich 140,2, in Sachsen 211, in Preussen 237,4. Wir wollen hier nur den grössten Unterschied zwischen Frankreich und Preussen erörtern.

In Frankreich ist ein Jahrgang mit einem Minimum von 1,56 Meter berechnet, in Preussen drei Jahrgänge mit einem Minimum von 5′ 2″ (die Zahlen sind aus *Dieterici*'s statistischen Tabellen entlehnt, daher noch 5′ 2″ Minimum). 5′ 2″ sind gleich 1,623 Meter. Die Differenz zwischen beiden Körperhöhen beträgt daher 0,063 Meter oder 2 Zoll und 4 Linien.

In der französischen Armee dienten nach einer Uebersicht von *Boudin* vom Jahre 1861 *(Rec. de mém. de méd. mil. pag. 50)* 124,000 Mann, welche noch nicht das Minimum von dem preussischen Maasse erreicht hatten, der deutlichste Beweis, dass eine Abschätzung der Militairtüchtigkeit beider Völker nach so ungleichen Maassverhältnissen nicht stattfinden darf.

In seinen *études sur le recrutement des armées* (pag. 31) stellt *Boudin* nach *M. Marshall (Military Miscellany, London 1846)* die Maasse der in der englischen und französischen Armee dienenden Soldaten, auf 1000 berechnet, zusammen, diesmal jedoch das französische Maass auf englische Zoll reducirt, und findet, dass von 1000 Franzosen 513 noch nicht das Mindermaass der Engländer erreichen.

Die Zahlen der wegen Mindermaass Untauglichen, welche sich in Frankreich nach den *Comptes rendus sur le recrutement* ziemlich sicher für jeden Jahrgang berechnen lassen, sind öfters von den Schriftstellern benutzt worden, theils um die Militairbrauchbarkeit in den einzelnen Departements zu berechnen, theils um die Ursachen des mangelhaften Wuchses zu eruiren.

Eine der besten Arbeiten hierüber ist die von *Sistach, médecin-major (Etudes statistiques sur les infirmités et le défaut de taille considerés comme causes d'exemption du service militaire* in den *Rec. des mém. de méd. mil. Paris 1861. pag. 353 et suiv.)*, der mit Hülfe der *Comptes rendus* folgende Verhältnisse findet:

Von 1,959,302 innerhalb der Jahre 1850—1858 untersuchten jungen Leuten waren 124,806 Unbrauchbare wegen Mindermaass, also durchschnittlich 62,8 *pro mille*, mit einer Abstufung von 56 ‰ im Jahre 1854 und 68,8 ‰ im Jahre 1856. Dagegen war die mangelnde Grösse als Befreiungsgrund vom Dienste sehr ungleich auf die 86 Departements

vertheilt, sie schwankten zwischen 21 und 160 auf 1000 Untersuchte; *Sistach* hat nach diesen Zahlen die Departements in vier Gruppen getheilt und die verschiedenen Abstufungen derselben auf einer Karte durch verschiedene Schattirungen graphisch dargestellt.

Die Vergleichung seiner Karte mit der nach denselben Grundsätzen für die Altersclassen von 1831 bis 1849 von *Broca (traité de géogr. et de statistique méd. 1857. tom. 2. pag. 238)* aufgestellten Reihenfolge der Departements zeigt mit seltenen Ausnahmen eine auffallende Uebereinstimmung.

Welche Ursachen hat die geographische Vertheilung der Dienstuntauglichkeit wegen mangelnder Grösse?

Nach *Villermé* „wird *(Annales d'hygiène publique 1829. tom. I. pag. 354)* der Wuchs der Menschen um so höher und um so rascher vollendet, je reicher — *ceteris paribus* — das Land, je allgemeiner der Wohlstand ist, je besser die Wohnungen, die Kleidung und besonders die Nahrungsmittel, und je geringer die Strapazen und Entbehrungen in der Kindheit und Jugend sind". Mit anderen Worten: Noth und Armuth, d. h. ihre Folgezustände, erzeugen kleine Leute und verzögern die Epoche der vollendeten Körperentwickelung. Auf hohen Bergen mit rauhem Klima tritt diese Epoche später als in der Tiefebene ein, auch ist auf ihnen der Wuchs kleiner. Indess für Frankreich muss im Allgemeinen die Verspätung der Entwickelung und der Kleinheit des Wuchses mehr der Armuth als dem directen Einflusse eines rauhen Klimas zugeschrieben werden. Mit einem Worte, nicht nur die menschliche Gesundheit, sondern mehr noch die Gestalt hängt zum Theil ab vom Grade der Civilisation, der öffentlichen Wohlfahrt oder Noth, und sehr oft würden es die Regierungen in ihrer Gewalt haben, indem sie ihre ganze Macht für das Gemeinwohl verwenden, nach Belieben den Wuchs ihrer Unterthanen im Allgemeinen zu erhöhen".

Sistach sowohl, wie *Broca* läugnen den Einfluss der Armuth auf das Grössenverhältniss der Menschen nicht, allein sie finden für Frankreich in dem Reichthum oder der Unfruchtbarkeit des Bodens nicht ausreichende Erklärung für die daselbst beobachteten Differenzen des Wuchses. Die sprüchwörtliche Fruchtbarkeit der Touraine und der Limagne d'Auvergne contrastirt sehr mit ihrer Classificationsnummer (81 und 84) nach der Zahl der Untauglichen wegen mangelnder Grösse.

„Alle hygienischen Ursachen, alle örtlichen Einflüsse," sagt *Broca (Mém. de la société d'anthrop. de Paris. t. I. 1860. pag. 52)*, „vermögen die Verschiedenheiten des Wuchses nicht zu erklären, während die Erforschung der beiden grossen celtischen Volksstämme, der Kimris und der Celten, ihrer Vertheilung und Vermischung auf die befriedigendste Weise die allgemeinen Resultate erklärt.

Hiernach hat *Broca* Frankreich in drei ungleiche Zonen getheilt, von denen die südöstliche oder celtische 50, die nordöstliche oder kimrische 21, die zwischen ihnen liegende die kimro-celtische 13 Departements enthält.

Sistach stimmt nach den gefundenen Resultaten mit *Broca* überein. Beide begründen daher ihre Ansicht über die Ungleichheit des Wuchses, bedingt durch Racenverschiedenheit, ziemlich glücklich, besonders wenn man noch in Erwägung zieht, dass, wenn jene Grössenunterschiede durch die Summe der hygienischen Verhältnisse, wie *Villermé* angiebt, bedingt würden, auch ein gewisser Zusammenhang zwischen der Untauglichkeit zum Militairdienst wegen Gebrechen und wegen mangelnder Grösse bestehen müsste. Allein es lassen sich keine Beziehungen der Coincidenz oder

des Antagonismus zwischen diesen beiden Kategorien der Unbrauchbarkeit auffinden.

Dieses sind ungefähr die Resultate der Untersuchungen in Frankreich, gegründet auf die Berechnungen der *Comptes rendus sur le recrutement.*

Aehnliche Untersuchungen könnte man auch in Preussen anstellen, wenn man die Rubrik Nr. 18 der Uebersichten über die Ersatz-Geschäfte der Kreise und Regierungsbezirke benutzte, in der es heisst: „zur Ersatz-Reserve übergetreten wegen Untermaass nach dreimaliger Concurrenz"; es könnten durch Vergleichung der drei Jahrgänge, wenn dieselben gemessen, notirt und berechnet würden, auch Durchschnittszahlen des Wachsthums während dieser drei Jahre gewonnen werden, Anhaltspunkte sowohl für die Wissenschaft zur Ergründung der Gesetze des Wachsthums, als auch für den Staat (wenn man der Ansicht *Villermé*'s folgt) zur Regelung seiner Gesetze, sei es, dass man, hierauf gestützt, die öffentliche Hygiene einzelner Bezirke zu verbessern sucht, sei es, dass man Behufs militairischer Zwecke die Zeit der Aushebung selbst später eintreten lässt, weil man erkannt hat, dass die Entwickelung des Körpers erst später als im zwanzigsten Lebensjahre erfolgt. Allerdings muss man bei solchen Erhebungen verlangen, dass durchgehend gleiche Bedingungen in Anwendung kommen, dass z. B. die beiden Kategorien der Unbrauchbarkeit nicht verwechselt werden, dass die wegen Mindermaass Unbrauchbaren nicht beliebig auch zu den wegen Krankheit oder Gebrechen Ausgemusterten gerechnet werden.

2. Der Brustumfang.

Das Messen des Brustumfanges zur Beurtheilung, ob Jemand zu schwach oder stark genug zum Militairdienst ist, vorausgesetzt, dass keine bedeutendere Krankheit oder Gebrechen ihn schon eigentlich von dieser Beurtheilung von vornherein ausschliessen, mit einem Worte — bei einem sonst gesunden Menschen, wird erst seit 12 bis 15 Jahren ausgeübt.

Sehr bald hatte man in der Militairhygiene nämlich erkannt, dass Rekruten mit schwacher Brust nie zu einer vollkommenen Entwickelung der sonst durch den Dienst erstarkenden Muskelkraft gelangten, sehr oft dagegen in der Ausbildung derselben zurückblieben, durch Anstrengung im Dienst, veränderte Nahrung, Kasernenleben etc. brustkrank wurden und bald entlassen werden mussten. Der hohe Procentsatz der an *Phthisis pulmonum* Verstorbenen, die Masse der Invaliditätsatteste der Lungenkranken forderten die Militairärzte dringend auf, gerade bei der Tauglichkeitserklärung besonders auf die vollendete Entwickelung des *Thorax* zu achten.

In der medicinischen Wissenschaft besitzen wir den Spirometer, um die Grösse der Lungencapacität zu messen; bei der Musterung der Militairpflichtigen lässt sich dieses Instrument nicht verwerthen, weil bei Benutzung desselben der Wille des Prüflings eine wesentliche Rolle spielt. Man war daher genöthigt, von der Form, Weite und Dehnbarkeit des Brustkorbes zu schliessen auf die Weite und Dehnbarkeit, auf die Capacität der Lungen. (*Loeffler*, Militairärztliche Zeitung.)

Percussion und Auscultation helfen unser Urtheil über Gesundheit und Krankheit der Brustorgane stützen, die Mensuration ist ganz geeignet, unsere Aussprüche über die Leistungsfähigkeit gesunder Lungen objectiver zu gestalten. Das Bandmaass hilft uns daher nicht in den Fällen, wo es sich um Extreme der Schwächlichkeit und Kräftigkeit handelt, sondern eben da, wo Zweifel in den mittleren Kräftigkeitsgraden obwaltet.

Die durchgehende Anwendung desselben hat sehr bald über die Nützlichkeit entschieden. Zuerst wurde es in Preussen vom Stabsarzt Dr. *Hildesheim* im Jahre 1854 angewendet, nach seinem Berichte dann im 3. Armee-Corps empfohlen und hier sowohl bei den Ersatz-Geschäften, als auch bei Untersuchungen auf Invalidität mit gutem Erfolge gehandhabt, nicht nur die Aerzte wurden in der Beurtheilung sicherer, sondern auch die Militairvorgesetzten stellten dem Urtheil des Arztes seltener ein Veto entgegen; durch die Brustmessung selbst ist ein praktisch brauchbarer Maassstab gefunden, das Maass selbst jedoch, welches als Norm dienen soll, ist keineswegs so festgestellt, dass ihm durch die Aufnahme in die Militair-Ersatz-Instruction gewissermaassen, wie bei der Körpergrösse, gesetzliche Bedeutung verliehen wäre.

Schon im Jahre 1860 erschienen in der Militairärztlichen Zeitung mehrere Arbeiten von Militairärzten über Brustmessung, von denen sich besonders die von *Loeffler* durch klare objective Auffassung des neuen Hülfsmittels der Untersuchung auszeichnete; ihnen folgten einige kleinere Abhandlungen, die sich hauptsächlich mit der Technik der Messung beschäftigten und das Minimum des oberen Brustumfanges festzusetzen versuchten, welches ein Rekrut haben müsse, um ihn für hinreichend stark zum Waffendienst erklären zu können. Der obere Thoraxumfang wurde durch ein Bandmaass, in Zolle getheilt, in der Höhe der Brustwarzen bei neben dem Kopfe senkrecht emporgehobenen Armen, um durch das Abstehen der Schulterblätter nicht ungleiche Messungen zu erhalten, auf der Höhe der Exund Inspiration gemessen und das in der Athempause erhaltene Maass als das eigentlich bestimmende angenommen. Als Minimum galt den meisten Militairärzten ein Brustumfang von 33 Zoll.

Die sanguinischen Hoffnungen, welche sich damals an diese Messungen knüpften, wurden durch Zweifel an der Gleichmässigkeit der Messung zerstört; so wurde besonders behauptet, dieselbe könne keine bestimmenden Werthe für Lungencapacität abgeben, weil der Umfang des Brustkorbes auch abhängig sei von dem *Panniculus adiposus*, von stärker entwickelter Musculatur, jedenfalls aber wesentlich verändert werde durch ungleiche Entwickelung etc. Die wissenschaftlichen Einwürfe, dass der Werth der Lungencapacität durch ein solches Maass nicht bestimmt werden könne, dass zu einer Bestimmung des Rauminhaltes des *Thorax* wenigstens 7 Punkte nothwendig seien, deren Entfernung gemessen werden müsse, dass bei vorgeschrittener *Phthisis pulmonum* dennoch ein bedeutender *Thorax*-Umfang bestehen könne und bestehe, gingen weit über den eigentlichen Zweck derselben, „einen Maassstab bei sonst gesunden Leuten für die hinreichende Entwickelung der Brustorgane zu haben", hinaus; denn zur Diagnose der Brustkrankheiten reichen solche Messungen allerdings nicht aus.

Die statistischen Arbeiten über durchgehende Brustmessungen der Rekruten sind im Ganzen sehr selten, doch weiss ich, dass recht werthvolles Material in den Büreaus der General-Aerzte, Berichte der bei der Kreis-Ersatz-Commission fungirenden Aerzte, vorhanden ist, welches manchen nicht allein technischen, sondern auch wissenschaftlichen Aufschluss über die Brustmaasse verschiedener Jahrgänge liefern könnte. Leider sind dieselben nicht nach gleichen Principien aufgestellt und selten in denselben Kreisen fortgeführt; die Schwierigkeiten sind bei der Anfertigung solcher statistischer Zusammenstellungen ziemlich bedeutend da der untersuchende Arzt alle Notizen selbst anfertigen muss, weil ein Schreiber selten und dann nur für die sogenannte Arztliste, gestellt wird.

Sollen solche Tabellen Werth zur Bestimmung des Minimum-Maasses haben, so müssen die Brauchbaren von den Unbrauchbaren gesondert werden, sollen sie die Zunahme des Brustumfanges als gleichmässige Entwickelung des ganzen Körpers beweisen, so müssen sie auch mit der während der drei Jahrgänge zunehmenden Körpergrösse verglichen werden. Dieses ist möglich, wenn man neben der Rubrik Körperhöhe in den namentlichen Listen auch eine für Brustumfang hätte, der also auch dreimal gemessen würde. Hieraus liessen sich mit Sicherheit die Gesetze des Wachsthums, gewiss aber das Verhältniss der Körperlänge zum Brustumfange ermitteln.

Das Verhältniss der Körperlänge zum Brustumfange Behufs Entscheidung der Dienstuntauglichkeit hat Dr. *Bernstein* (Prag. Med. Wochenschrift. 9. 1864) erörtert. Er fand, dass unter 67 tauglichen Rekruten:

Anzahl	Körperhöhe	Brustumfang
9 Mann	60—61"	31—33"
8 „	61—62"	31—33"
15 „	62—63"	32—34"
18 „	63—64"	32—34"
14 „	64—65"	33—35"
3 „	65—66½"	33—35½"

hatten und gründet darauf folgende Schlüsse:

1) der Brustumfang nimmt mit der Körperhöhe zu, aber nur da, wo eine harmonische und proportionirte Körperentwickelung stattgefunden;
2) den grössten Brustumfang im Verhältniss zur Körperhöhe bietet nur der sogenannte Mittelschlag von 62—65", erfahrungsgemäss der ausdauerndste für die Kriegsstrapazen;
3) übersteigt die Körperhöhe das Mittelmaass, geht sie über 65", dann folgt der Brustumfang nicht mehr

in derselben Proportion, er bleibt häufig zurück, und wir gelangen auf das Feld der Aufgeschossenen, Engbrüstigen und des tuberculösen Habitus;

4) bei allen Tauglichen überragt der Brustumfang um 1—2″, auch 3″ die Hälfte der Körperhöhe; da wo dies nicht der Fall, erscheinen die Leute als schwach.

„Kriegsdiensttauglich ist derjenige, der vollkommen gesund, mit keinem körperlichen Gebrechen behaftet ist und dessen Brustumfang wenigstens um 1″ mehr beträgt, als die Hälfte der Körperhöhe."

Aehnliche Versuche und Berechnungen sind nach *Elliot (On the Military Statistics of the U. A. International Stat. Congr. at Berlin. pag. 19)* in Amerika bei der Potomac-Armee gemacht, haben jedoch keine so bestimmten Resultate ergeben.

Als Mittel erhält er bei 1516 Untersuchten 34,99 Zoll Brustumfang.

Hammond (Military Hygiene pag. 30) will die Entfernung beider Brustwarzen mit einem graduirten Lineale messen und das Resultat mit 4 multipliciren. Er kommt zu dem Schlusse, dass kein Rekrut einstellungsfähig ist, bei dem die obere Circumferenz des *Thorax* geringer ist als die halbe Körperhöhe *(conf. Bernstein)*, indem erstere bei jedem Zoll Höhe mehr um ½″ zunehme.

Wenn die Brustmessungen auch bis jetzt keine absolut sicheren Resultate für die Beurtheilung der Körperkraft gegeben haben, so ersieht man doch aus den eben angeführten Behauptungen des Dr. *Bernstein*, dass, wenn der Brustumfang mit der Körperhöhe verglichen wird, sich doch ganz gut verwerthbare Anhaltspunkte aus solchen Beobachtungen ergeben; es würde sich jedenfalls belohnen, wenn weitere Forschungen auf diesem Gebiete die erwähnten Beobachtungen ausser Zweifel stellten.

3. Das Körpergewicht.

Dr. *J. C. Mayer* (Bayer. ärztl. Intell.-Bl. 24. 25. 1862) weist in seiner Abhandlung darauf hin, dass man dem Körpergewichte der Conscribirten bisher noch nicht die Aufmerksamkeit geschenkt hat wie der Körpergrösse, und „doch gestattet uns eine genaue Untersuchung beider Factoren einer Bevölkerung einen richtigen Schluss auf deren physische Beschaffenheit".

Die Fragen, welche Dr. *Mayer* aus den nach amtlichen Quellen bearbeiteten Tabellen zu erörtern sucht, beziehen sich hauptsächlich auf das Verhältniss der Körperhöhe und des Körpergewichtes zu Vergleichen der Bewohner der Städte und des platten Landes der verschiedenen Stände benutzt. Schlüsse, welche er hieraus zu ziehen sucht, sind z. B.:

1. die Bodenformation, die Art der Arbeit und der Grad der Wohlhabenheit sind diejenigen drei Factoren, welche auf das Wachsthum in der Länge und Breite den grössten Einfluss ausüben, und unter ihnen steht die Wohlhabenheit oben an. Die wohlhabendsten Districte haben auch im Verhältniss zur Körperhöhe die schwersten Conscribirten;
2. auch bei dem menschlichen Körpergewicht herrscht ein nur von der Wissenschaft festzustellender Typus.

   Der Mensch der mittleren Körpergrösse hat auch ein mittleres ziemlich constantes Gewicht.

   Die Grenzen, zwischen denen das Körpergewicht schwankt, sind weiter, als die zwischen der Körpergrösse etc.

Aehnliche Wägungen der Angeworbenen sind in Nord-Amerika gemacht. *Hammond (Military Hygiene)* stellt nach denselben folgende Forderung auf:

„Ein Mann von zwanzig Jahren darf nicht weniger als

125 Pfund (113⅓ Zollpfund) haben, für jeden Zoll über 5′ 5″ (5′ 3″ rheinisch) müsse sein Gewicht um 5 Pfund zunehmen, da sonst eine constitutionelle Krankheit vorliege oder doch ein depotenzirender Einfluss längere Zeit den Körper beeinflusst habe".

Elliot (*Mil. Stat. pag. 21*) bringt, indem er die oben erwähnte Tabelle aufstellt, ebenfalls Wägungen, besonders der Soldaten der Potomac-Armee, doch lassen sich aus denselben noch keine bestimmten Resultate ziehen.

In Preussen sind solche Untersuchungen bei dem Rekrutirungs-Geschäfte noch nicht vorgenommen; soviel ich darüber erfahren konnte, sind wiederholte Wägungen der Rekruten bei Beurtheilung des Einflusses des Dienstes, besonders des Turnens, auf den Körper des Soldaten benutzt.

Dass das Körpergewicht besonders im Vergleich zur Körperhöhe einen wichtigen Anhaltspunkt für die Beurtheilung der gleichmässigen Entwickelung des Körpers, zur Bestimmung der Militairbrauchbarkeit und zu Rückschlüssen auf die physische Kraft der Bevölkerung geben kann, ist nach dem oben Gesagten wohl unzweifelhaft, eine gleichmässige Durchführung der Wägung bei dem Rekrutirungs-Geschäfte selbst ist jedoch so bald noch nicht zu hoffen, da dieselbe so wenig Interesse, speciell für die Militairbehörden selbst, hat, der Zeitaufwand aber ein ziemlich bedeutender ist.

4. Bestimmung der Muskelkraft.

Die Muskelkraft endlich direct zu messen, durch den Dynamometer z. B., ist nur anwendbar bei der Anwerbung der Soldaten; bei der Conscription, bei dem Ersatzgeschäfte sind solche Messmittel nicht brauchbar, wie alle Proben, bei welchen auf die Leistungswilligkeit der Untersuchten gerechnet werden muss. Versucht sind dieselben in England, doch habe ich aus der Literatur nicht ersehen können, ob dieselben wirklich zu verwerthbaren Resultaten geführt haben (*Elliot, pag. 23*). Dem Gesichtssinne der Aerzte kommt hier wesentlich auch noch der Tastsinn zu Hülfe, und selbst da, wo nur eine mittelmässige Entwickelung der Muskelkraft durch diese beiden Sinne ermittelt wird, lehrt uns die Erfahrung, dass dieselbe sich gerade durch die Uebungen, besonders durch das in den Militairdienst aufgenommene Turnen, rasch entwickelt.

Wenn wir bei der Erörterung der vier Momente, mit deren Hülfe eben der Arzt die ausreichende Körperkraft des Militairpflichtigen zu beurtheilen sucht, uns eingestehen müssen, dass bis jetzt noch keine absoluten Werthe für dieselben gefunden sind, und dass die bis jetzt erlangten Resultate auch mit Vorsicht aufgenommen werden müssen, so ist doch nicht zu bezweifeln, dass durch genaue Beobachtungen und nur durch, nach richtigen Principien geleitete, statistische Arbeiten endlich Resultate gewonnen werden können, die der Statistik, wie der Wissenschaft reichen Gewinn bringen.

II. Die Tauglichkeitsziffer.

Die Literatur der medicinischen Statistik hat die Zahlen, welche sich bei dem Rekrutirungs-Geschäfte durch die Sonderung der Brauchbaren von den Unbrauchbaren und durch Procentberechnung derselben aus der Zahl der Untersuchten ergeben, als wichtiges Material aufgefasst.

Die Statistik liess es sich besonders immer sehr angelegen sein, die Verhältnisszahlen der zum Militairdienst Tauglichen zu eruiren, weil ein relativ hohes Verhältniss Militairtüchtiger im Allgemeinen überall als eines der sichersten Zeichen der Gesundheit und Wohlfahrt der Bevölkerung gelten kann, fast so gut als der Kraft und Tüchtigkeit jener junger Männer selbst, eine etwaige Zu- oder Abnahme der Militairtüchtigkeit aber auch Licht über die Frage der zu- oder abnehmenden Morbilität der Bevölkerung verbreiten kann. Sehen wir uns nun einmal in der Literatur um, wie die Frage von der Militairtüchtigkeit oder Brauchbarkeit von der Statistik behandelt worden ist.

Die Tauglichkeitsziffer erhält man aus der Vergleichung der Zahl der Unbrauchbaren oder vielmehr der Brauchbaren mit der Zahl der Untersuchten; gewöhnlich wird diese auf 1000 Mann berechnet, um die Zahlen verschiedener Kreise, Provinzen, Länder und Staaten vergleichen zu können.

In der nebenstehenden Tabelle habe ich aus verschiedenen daselbst bezeichneten Büchern die Resultate der Berechnungen zusammengestellt.

Ein flüchtiger Blick auf die Zahlen der Untauglichen und Tauglichen genügt, um die Differenzen zwischen den einzelnen Staaten als ziemlich bedeutende hervortreten zu lassen; dieselben sind zu gross, um nicht sogleich mit Misstrauen aufgefasst zu werden; sie hängen nicht allein von der Ungleichheit der Anforderungen an die Tauglichkeit zum Militairdienst in den verschiedenen Ländern ab, sondern sie sind wesentlich Fehler der Berechnung.

Geht man die einzelnen Positionen durch, so zeigt sich zuerst, dass in Frankreich die Zahlen ziemlich constante geblieben sind; die Art und Weise, wie daselbst die Resultate gewonnen werden können, ist auch die einfachste; hier ist nur ein Jahrgang der Rekruten zu berücksichtigen, die Entscheidung über Untauglichkeit muss sogleich geschehen, da der Conscribirte nur einmal vor der Commission erscheint; die *Comptes rendus sur le recrutement* lieferten die richtigsten Berechnungen der Tauglichen, doch ist für jetzt dieselbe wiederum zweifelhaft, da der Loskauf auch vor dem Eintritt in das zwanzigste Jahr gestattet ist, mithin von dem

		Zahl der Unter-suchten	Zahl der ganz oder nur zeitig Untauglichen und minder Tauglichen wegen: Untermaass	Zahl der ganz oder nur zeitig Untauglichen und minder Tauglichen wegen: Körper-schwäche und Krankheit	Summa aller Untaug-lichen	Zur Aushebung waren tauglich	Von 1000 Untersuchten waren un-tauglich	Von 1000 Untersuchten waren tauglich
1. Frankreich	1837—1845	1,591,193	116,435	498,289	614,724	976,469	386	614
2. Frankreich	1831—1843	2,097,876	—	680,560	680,560	1,417,316	324	676
3. Frankreich	1850—1858	1,959,302	124,806	514,588	639,394	1,309,908	326	674
4. Preussen	1831—1854	3,248,561	1,029,591	1,296,841	2,326,432	922,129	716	284
	3 Jahre							
5. Preussen	1860—1862	1,196,864	56,848	931,680	988,528	208,336	826	174
6. Oesterreich	1857 u. 1858	1,984,780	278,305	718,409	996,714	988,066	502	498
7. Bayern	1852—1857	238,550	9,913	60,422	70,335	168,215	295	705
8. Württemberg	1834—1857	240,510	24,600	98,863	123,463	117,407	513	487
9. Sachsen	1845—1854	117,023	24,805	61,909	86,714	30,309	741	259
10. Dänemark	1852—1856	56,512	8,509	18,457	26,966	29,546	477	523
11. Nord-Amerika (*draft* 1863)	1863—1864	316,445	—	—	95,868	220,577	285	715
12. Nord-Amerika (Werbung im Frieden)	1852	16,064	—	—	13,338	2,726	830	170
13. England	1842—1852	171,276	—	—	57,381	113,895	335	665

Entnommen aus:

1. *Wappaeus*, allgem. Bevölkerungsstatist. t. II. S. 138—141.
2. *Annual report of the provost General. Nov. 1864. pag. 21.*
3. *Sistach, Recueil des memoires médicin-mil. Paris 1861. tom. 6. pag. 353.*
4. *Dieterici*, Mittheil. aus dem statist. Bureau zu Berlin. 1864. pag. 325.
5. *Engel*, Zeitschrift des statist. Bureau zu Berlin. 1864. pag. 72.
8. *P. Sick*, Württemberg. Jahrbuch 1857, Stuttgart. H. I. S. 131.
9. *Engel*, die physische Beschaffenheit der Militairbevölkerung in Sachsen. Zeitschrift des statist. Bureau in Sachsen. 1856. Nr. 405.

6., 7. und 10. *Oesterlen*, Handbuch der med. Statistik. pag. 945.

11., 12. u. ad 13. *Annual Rep. of the Provost-Marshal-General. Table 14. Tab. I. p. 30.*

Conseil de révision der Losgekaufte wahrscheinlich auch als gesund angenommen wird.

Die Anforderungen der französischen Ersatz-Instruction sind in Bezug auf die Feststellung der Unbrauchbarkeit durch Krankheit oder Gebrechen scharf genug, ob sie aber bei dem bestehenden Loskaufsrecht bei der Bestimmung der Körperstärke der Brauchbaren nicht zu geringe Ansprüche machen, ist aus den bis jetzt gelieferten Uebersichten nicht zu ersehen. Durch die Morbilitäts- und Mortalitätsstatistik der Armee diese Frage zu entscheiden, ist nicht möglich, da ein grösserer Theil der für tauglich Erklärten gar nicht zum Dienst kommt, sondern sich loskauft. Und wenn dieses auch nicht wäre, ein Abgang von 7 pCt. durch Tod und Invalidität kann durch Klima, epidemische Krankheiten etc. bedingt sein, nicht durch schlechte Auswahl.

Was nun die Zahlenreihen von Preussen betrifft, so ist die erste von *Dieterici* (Statistische Uebersicht des Aushebungsgeschäfts im Preussischen Staate, Mittheilung des statistischen Bureau, Bd. VIII., pag. 325—364) berechnet, aber vollständig falsch; hier ist die Verhältnisszahl der Unbrauchbaren zu den Untersuchten nicht richtig, weil die Zahl der letzteren nicht die der Gestellungspflichtigen eines Jahrganges ist, sondern auch die Zurückgestellten und disponibel Gebliebenen aus den früheren Jahrgängen enthält, also bedeutend zu gross ist, zu den Untauglichen aber auch die aus den Rubriken „zeitig unbrauchbar“ und „garnisondienstfähig“ hinzugenommen sind.

Die zweite Reihe habe ich aus den von Dr. *Engel* berechneten Tabellen des Ersatzaushebungsgeschäftes entnommen (Zeitschrift des statistischen Bureau 1864, März, S. 73). Dieselben sind nach den Uebersichten der Regierungsbezirke (Schema 27 zum §. 101 der Ersatz-Instruction) angefertigt; die bedeutende darauf verwendete Mühe ist im Ganzen

schlecht belohnt, weil die Resultate vollständig falsche sind. Schon dass die Zahl der Brauchbaren mit der der Gestellungspflichtigen, von denen allein über 119,000, ja selbst 168,994 in einem Jahre unermittelt geblieben sind, verglichen ist, muss falsche Procente geben, wie viel mehr noch, wenn die Restanten aus den früheren Jahrgängen hinzukommen, die doch vorzugsweise die Unbrauchbaren, noch nicht Ausgemusterten enthalten.

So erhält *Engel* von 1000 Gestellungspflichtigen nur 116, ja selbst 105 Auszuhebende; ich habe diesen Fehler durch Vergleichung der Unbrauchbaren mit den Gemusterten zu verbessern gesucht und so die Zahl 174 erhalten. Allein auch diese ist aus den oben angeführten Gründen falsch. Schon im Juli-Heft derselben Zeitschrift erklärt *Engel* die von ihm gelieferten Zahlen für falsch, und zwar wesentlich durch Fehler in dem von ihm benutzten Material.

In der Rubrik Nr. 5 der Ersatzübersichten sind sämmtliche in den alphabetischen Listen enthaltenen Zwanzigjährigen aufgenommen; dadurch wird aber eine grosse Anzahl derselben zwei-, auch dreimal geführt, da sie im Geburtsort, im Domicil und Arbeitsort in die Listen eingetragen werden; wie oft dieses bei den anderen Jahrgängen geschehen, ist nicht ersichtlich. Hierdurch geschieht es, dass allein im Jahre 1861 52,276 Zwanzigjährige zu viel angegeben sind.

Dazu kommt noch, dass die Rubrik 9 „in andere Kreise gezogen und dort zur Aushebung gekommen“ sämmtliche Altersklassen betrifft, eine Correctur durch dieselbe also auch nicht möglich wird. Auch die Rubrik 12 „als einjährige Freiwillige anerkannt“, gibt keine brauchbare Zahl, da zwischen Anerkennung und Einstellung noch immer die körperliche Untersuchung liegt; im Jahre 1863 waren dieses 15,000, aber nur 1577 traten ein.

Dr. *Engel* will nun, wenn sich's um Charakterisirung der Tüchtigkeit der männlichen Bevölkerung Preussens handelt, die Summe der wirklich Ausgehobenen, der Freiwilligen und der Disponiblen (Rubrik 11, 23 und 24) vergleichen mit der Zahl der Zwanzigjährigen, welche aus der Zählung der Civil- und Militairbevölkerung hervorgeht. Das Resultat seiner Berechnungen ist nun bedeutend besser als vorher; von 129,291 Zwanzigjährigen sind 68,547 oder 53 pCt. brauchbar zum Waffendienst.

Aus diesem kurzen Referate ergiebt sich, dass selbst unter der Hand jenes gewandten Statistikers die bis jetzt gelieferten Uebersichten des Ersatzgeschäftes nicht allein ausreichen, um irgendwie richtige Resultate über die Militairtüchtigkeit des preussischen Volkes aus ihnen allein zu erhalten; jedenfalls ist hieraus ersichtlich, wie wenig Werth die durch alle statistischen Werke verbreiteten Zahlen haben, und dennoch werden sie oft ohne jede Kritik gläubig aufgenommen.

Aehnlich wird es sich mit den über Sachsens Bevölkerung gelieferten Zahlen verhalten, doch kann ich darüber kein bestimmtes Urtheil fällen, weil ich die Einrichtung der Listen nicht genau kenne.

Die bedeutende Differenz in den Brauchbarkeitszahlen von Nord-Amerika ist, wie schon oben erörtert, bedingt durch den Unterschied, den die Werbung und die Conscription *(draft)* mit sich bringt. Bei den Werbungen kommt noch der Umstand hinzu, dass das Alter der Angeworbenen nie übereinstimmt, sondern oft zwischen 17 bis 35 schwankt.

Eine Vergleichung der bis jetzt erhaltenen Zahlen, um auf relative Tüchtigkeit und physische Gesundheit der Bevölkerung zu schliessen, darf nach dem über die einzelnen

Positionen Gesagten daher nicht stattfinden, so oft sie auch beliebt ist, und zwar nicht nur von der Tagespresse, die sich in glänzenden Antithesen und Ausrufungen über die Abnahme der physischen Kräfte eines Volkes gefällt, sondern auch von medicinischen Schriftstellern, die einen Anspruch auf Wissenschaftlichkeit erheben. So benutzt *Boudin* jene in Tabelle I. aufgeführten Zahlen, indem er ihnen noch einige über Sardinien und Belgien hinzufügt, um zu beweisen, dass Frankreich gegen die anderen Länder bedeutend besser in der Militairtüchtigkeit seiner Bevölkerung situirt sei; aus diesen Zahlen stellt er eine Curve zusammen, welche vollkommen falsch ist, da, wie oben bewiesen, jene Zahlen aus falscher Berechnung hervorgegangen sind (*Recueil de Mémoires de Médecine militaire.* 1863. *III. Ser. tom.* X. *pag.* 42). In den Journalen Frankreichs waren, besonders vom Auslande her übernommen, einige Bemerkungen über die Abnahme der *aptitude militaire en France* gemacht worden, veranlasst durch die Herabsetzung der zum Militairdienst erforderlichen Grösse: *Boudin* weist nun in seinem Aufsatze nach, dass die Körpergrösse sogar zugenommen hat in der Bevölkerung Frankreichs, und dass die *aptitude militaire en France* die grösste in Europa sei; die Wissenschaft hatte durch Zahlen gesprochen, Frankreich konnte wieder ruhig sein!

Die Unbrauchbarkeit der Militairpflichtigen wegen Krankheit und Gebrechen.

Bei der Entscheidung über die Dienstunbrauchbarkeit der Militairpflichtigen muss der untersuchende Arzt den Körperfehler angeben, welcher die Unbrauchbarkeit eben bedingt; als Anhaltspunkt dienen ihm dabei die in den Instructionen aufgeführten Körperfehler.

Die Diagnose der Krankheit oder des Gebrechens selbst übt und schärft den Blick des Arztes, weitere wissenschaftliche Beobachtungen und Erfahrungen ergeben sich erst bei sorgfältiger Zusammenstellung der gefundenen Körperfehler; diese wird hauptsächlich die Häufigkeit der Krankheiten in gewissen Altersklassen der Bevölkerung unter gewissen Ständen, die grössere und geringere Verbreitung über gewisse Landestheile direct durch Zahlen angeben, und dem Arzte wiederum Gelegenheit bieten, Erfahrungen zu sammeln, inwieweit Luft und Bodenbeschaffenheit, Cultur und Sittenverhältnisse, Lebensweise und Berufsthätigkeit auf die relative Häufigkeit gewisser Fehler, Gebrechen und Krankheitsanlagen bestimmenden Einfluss ausüben.

Beobachtungen nach diesen Richtungen, welche gesammelt, gesichtet und mit einander verglichen werden, würden ein sehr schätzbares Material für die medicinische Statistik liefern, aus welcher der Staat wiederum Anhaltspunkte für die öffentliche Gesundheitspflege, die verschiedenen Zweige der Medicin Material für die Aetiologie gewisser Krankheitsformen und für deren epidemische und endemische Verbreitung gewinnen könnten.

Die relative Häufigkeit der Krankheiten und Gebrechen, welche die Unbrauchbarkeit der Militairpflichtigen bedingen, zu eruiren, ist von den Aerzten verschiedener Nationen öfters versucht, meist jedoch nur da gelungen, wo bestimmte Vorschriften für die Berichte der Rekrutirungs-Geschäfte gegeben sind, die besonders diesen Theil der ärztlichen Thätigkeit in's Auge fassen.

Aus den *Comptes rendus sur le recrutement* hat *Sistach* in der oben erwähnten Arbeit (p. 355) eine Statistik der Krankheiten und Gebrechen geliefert, welche die *Exemptions pour infirmité* bedingen. Die Tabelle I. zeigt, dass von

1,959,302 untersuchten Heerespflichtigen aus den Altersklassen 1850—1858 incl. 514,588 wegen Gebrechen für dienstuntauglich erklärt werden mussten.

Vergleicht man die Maxima und Minima der Dienstbefreiungen mit den Jahreszahlen, so ist leicht ersichtlich, dass die Minima derselben zusammenfallen mit den Kriegen in der Krimm und in Italien, dass also nur der Bedarf des Heeres die strengere Auswahl veranlasst hat.

In einer zweiten Tabelle hat *Sistach* eine Uebersicht der Krankheiten und Gebrechen, welche die Unbrauchbarkeit der Conscribirten nach Tabelle *D* der *Comptes rendus* bedingen, und zwar die Zahlen derselben für jedes Jahr gegeben; in der dritten Tabelle sind, um eine genaue Kenntniss von der relativen Häufigkeit der Krankheiten zu geben, die einzelnen Proportionalzahlen, die Summe der dienstuntauglichen gleich 1000 gesetzt, berechnet.

Sodann folgen Tabellen über die Häufigkeit der Gebrechen nach den Organsystemen, über die Zahl der in jedem Departement untersuchten Mannschaften, der unter ihnen ermittelten Dienstuntauglichen, der Verhältnisszahlen der letzteren pro mille Untersuchter, sodann eine Tabelle, in welcher die 86 Departements nach der Proportion der Dienstunbrauchbaren classificirt sind, und ausserdem eine Karte, auf welcher die Departements nach ihrer Reihenfolge in der eben erwähnten Tabelle in 5 besondere Gruppen getheilt, durch 5 verschiedene Schattirungen äusserst übersichtlich dargestellt sind.

Aus allen diesen Tabellen ersieht man, dass die Brauchbarkeit zum Militairdienst eine äusserst verschiedene in den einzelnen Departements ist.

Auf dieses Verhältniss begründete *Boudin* (*Traité de géogr. et stat. méd. t.* 2. *p.* 241. 1857.) seine Einwendungen gegen die früher übliche Vertheilungsweise des Contingents, weil dieselbe Nachtheile für die Bevölkerung und den Staat habe, „für die Bewölkerung in der Gegenwart durch die Entziehung einer zu bedeutenden Proportion, oft sogar aller jungen kräftigen Leute, für die zukünftigen Generationen durch die traurigen Folgen zahlreicher Ehen unter Schwächlingen, endlich für den Staat selbst durch die zunehmende Schwächung der Bevölkerung und durch den Ausfall für die Armee."

Aehnliche statistische Tabellen, wie die von *Sistach*, sind von *Boudin*, *Broca*, *Lachèze*, *d'Angers etc.* in Frankreich aufgestellt, meist nach den *Comptes rendus annuels sur le recrutement*.

Ziemlich nach denselben Principien sind die statistischen Uebersichten der Krankheiten und Gebrechen, welche bei den *draft's* von 1863 und 1864 in Nord-Amerika die Zurückstellung der Untersuchten erforderten. Die Tabellen zeigen zuerst die Zahlen der Zurückgestellten der einzelnen Staaten, dann die Summe derselben, vertheilt auf die 41 oder 36 Paragraph-Nummern der Körperfehler, welche durch die Ersatz-Instruction (§. 85. *Revised Regulations for the government of the Bureau of the Provost Marshal General* 1864) für die *draft* aufgestellt waren, und die Berechnungen der Proportionalzahlen auf 1000 Untersuchte. Als Beispiel dieser mit grossem Fleisse ausgearbeiteten Tafeln stelle ich die aus den Tafeln 6 und 8 des *Annual report of the Provost Marshal General Nov.* 15. 1864. erhaltenen Resultate zusammen. Aus der Durchsicht der 41 und 36 Krankheitsnamen ersieht man schon, dass die Vorschriften der Instruction ziemlich scharf gefasst waren, besonders, weil sehr viel Simulation vorkam; manche zusammengehörige Krankheiten sind getrennt, dann wiederum die verschiedenartigsten Leiden zusammengefasst, so dass die dadurch bedingten statistischen Arbeiten, sei es zur Vergleichung mit anderen

		Annual report of the Provost Marshal General *under the draft* 1863. I.	*the second draft* 1864. II.		
§.	Liste der physischen und geistigen Gebrechen und Krankheiten. (Tabelle No. 6. u. No. 8.)	Untersucht: 255,188. Unbrauchbar	Untersucht: 61,257. Unbrauchbar	Verhältniss auf 1000 Untersuchte von No. I.	Verhältniss auf 1000 Untersuchte von No. II.
1	Offenbar geistige Schwäche	990	181	3,88	2,95
2	Irrsein	410	108	1,60	1,76
3	Epilepsie	2140	388	8,39	6,33
4	Paralyse, Chorea, Atrophie eines Gliedes	1044	262	4,09	4,28
5	Acute und organ. Krankheiten des Herzens, Lunge, Nieren, Gehirn, Rückenmark, Milz, Leber etc.	11573	1512	45,35	24,68
6	Ausgesprochene Abzehrung (*Tuberculosis*)	3829	830	15,00	13,55
7	Krebs, Aneurysma grosser Gefässe	72	28	0,28	0,46
8	Ausgedehnte inveterirte Hautkrankheit	491	106	1,92	1,73
9	Entschiedene Schwäche der Constitution, besonders Brust	9628	1696	37,73	27,69
10	Scropheln, constitut. Syphilis	1317	265	5,16	4,33
11	Habituelle Trunksucht und Onanie	121	in No. 9 enthalten	0,47	—
12	Chronischer Rheumatismus	1210	279	4,74	4,55
13	Kopfschmerz, Neuralgie, Lumbago, Affect. der Gelenke, Knochen, Muskeln	288	fehlt	1,13	—
14	Verletzung und Krankheiten der Kopfhaut	253	—	0,99	—
15	Völlige Blindheit, Cataract rechts	1920	445	4,74	7,26
16	Partieller Verlust der Sehkraft auf beiden Augen, Krankh. der Augenlider	3176	410	12,45	6,69
17	Verlust der Nase und Deformität derselben	131	27	0,51	0,44
18	Ausgesprochene Taubheit, chron. purul. Otorrhoe	1820	411	7,13	6,38
19	Unheilbare Krankheiten u. Deformitäten der Kiefer, *Anchylosis*	190	60	0,74	0,98
20	Stummheit. Stimmlosigkeit	45	11	0,18	0,187
21	Verlust, Hypertrophie und Atrophie der Zunge	7	5	0,03	0,08
22	Stottern	448	98	1,76	1,60
23	Verlust der Zähne	5230	1544	20,49	25,20
24	Geschwulst, Verletzung des Halses, Fisteln, *Cap. obstip.*	263	29	1,03	0,47
25	Difformitäten der Brust, Rückgrats, Rippen und Brustbeins	1675	327	6,56	5,50

§.	Liste der physischen und geistigen Gsbrechen und Krankheiten. (Tabelle No. 6. u. No. 8.)	I. Unter-sucht: 255,188. Un-brauch-bar	II. Unter-sucht: 61,257. Un-brauch-bar	Verhält-niss auf 1000 Unter-suchte von No. I.	Verhält-niss auf 1000 Unter-suchte von No. II.
26	Abnorme Fettsucht	237	—	0,93	—
27	Hernien	7894	2115	30,93	34,52
28	Kothfistel, *Strictura recti*, *Prolapsus ani*	446	152	1,75	2,48
29	Innere Haemorrhoiden	917	203	3,59	3,32
30	Ziemlich vollständiger Verlust des Penis, *Epi-* u. *Hypospad.*	31	11	0,12	0,17
31	Organ. perman. Harnstrict., Harnfisteln	379	45	1,49	0,73
32	Incontinenz, Blasenstein etc.	83	15	0,33	0,24
33	Verlust oder völlige Atrophie der Hoden, Retention	424	—	1,66	—
34	Bösartige Sarcocele, hochgradige Hydrocele	1218	158	4,77	2,58
35	Verlust einer Extremität	488	79	1,91	1,29
36	Wunden, Narbencontract. Geschwülste	1443	495	5,65	8,41
37	Brüche, irreponible Verrenkungen, Anchylosen etc.	9873	1756	38,55	28,67
38	Verlust des rechten Daumens oder Zeigefingers, Defecte der Hand	2025	295	7,94	4,82
39	Klumpfuss, Verlust der grossen Zehe, permanente Fusskrankheiten	2987	505	11,71	8,24
40	Varicose Venen	1947	504	7,63	8,23
41	Chronische Geschwüre, Narben der Unterextremitäten	1022	379	4,00	6,62
	Verschiedenes	486	—	1,90	—
	Summa	80134	15744	314,02	257,02

Staaten, sei es zur Benutzung, um auf den Krankheitscharakter der Bevölkerung zu schliessen, an Werth bedeutend verlieren. In denselben *Annual reports* sind auch statistische Tabellen von Frankreich, England, Belgien etc. zur Vergleichung wiedergegeben; der Raum dieser Arbeit gestattet mir nicht, noch mehrere solcher Tabellen beizubringen, erwähnen will ich nur die ziemlich gleichen Procentzahlen der wegen körperlicher Unbrauchbarkeit Zurückgestellten verschiedener Staaten, die in *Table* 19. *pag.* 24 des erwähnten Berichtes sich finden:

Nord-Amerika .	1863:	314	auf 1000 Untersuchte,
Frankreich . . .	1831—1843:	324	- - -
Gross-Britanien	1832—1851:	317	- - -
Belgien	1851—1855:	320	- - -

In Deutschland ist mit Ausnahme der Arbeit von Dr. *Engel* (die physische Beschaffenheit der milititairpflichtigen Bevölkerung Sachsens, Zeitschrift des statistischen Bureau des Königlich Sächsischen Ministerium des Innern, 1856) das Ergebniss der Ersatz-Geschäfte in Bezug auf Häufigkeit der vorgefundenen Krankheiten wissenschaftlich noch nicht verwerthet worden.

In Preussen, dessen Ersatz-Aushebungs-Listen durch Notirung der die Unbrauchbarkeit bedingenden Körperfehler ein bedeutendes Material für solche Untersuchungen enthalten, werden nur in einzelnen Provinzen von den aushebenden Aerzten Berichte über die von ihnen angestellten Beobachtungen gefordert.

Allein diese Berichte sind nach verschiedenen Gesichtspunkten und ungleichen Procentberechnungen angefertigt, so dass sie im Ganzen nicht, wenigstens nicht zu statistischen Arbeiten, verwendet werden können. Die einzige hierher gehörige Arbeit, welche veröffentlicht ist, betrifft die Notizen des Dr. *Rosenthal* „über das Ergebniss des Kreis-Ersatz-Geschäftes in Thüringen" (Militairärztliche Zeitung, 1862, Nr. 15, pag. 169). In derselben sind einzelne Beobachtungen über diejenigen Fehler und Krankheitsanlagen, die theils wegen ihrer Häufigkeit, theils wegen Einflusses auf die Dienstbrauchbarkeit am meisten beobachtenswerth erschienen, wiedergegeben.

Werfen wir noch einen Rückblick auf die von uns gegebenen Tabellen über die relative Häufigkeit der vorgefundenen Krankheiten, so müssen wir uns eingestehen, dass der Classification der Krankheiten und Gebrechen kein absoluter Werth beigemessen werden kann. Einmal hat die während des Ersatz-Geschäftes improvisirte Diagnose für gewisse Krankheiten keine vollkommene Genauigkeit zu beanspruchen; die Gruppirung der Krankeiten und Gebrechen, wie sie nach den Instructionen der Länder, wie Frankreich und Nord-Amerika, eingeführt ist, bietet nicht immer so bestimmte Eintheilungen dar, dass einzelne Fälle von verschiedenen Beobachtern nicht auch verschiedenen Gruppen zugezählt werden können. Liegt es nicht auf der Hand, dass z. B. unter den Dienstuntauglichen wegen *„faiblesse de constitiution et autres maladies"* etc. sich viele Individuen befinden werden, welche bei einer genauen Diagnose vielmehr der Gruppe der Phthisis pulmonum, der Scrofulosis, der Rachitis hätten zugezählt werden müssen. Dies hat *Sistach* bei seiner Arbeit sehr wohl erkannt; deshalb sagt er im Resumé I. „die seit 1851 eingeführte Classification der durch die verschiedenen Arten von Gebrechen bedingten Dienstuntauglichkeit bedarf wesentlicher Abänderungen gemäss der Fortschritte der Wissenschaft und einer rationelleren Würdigung der Natur gewisser Krankheiten".

Die Häufigkeit der die Dienstunbrauchbarkeit bedingenden Krankheiten und Gebrechen gewinnt für die medicinische Statistik und Geographie erst Interesse, wenn ihre Procentzahlen in Verbindung gebracht werden mit den einzelnen Districten der Rekrutirungs-Geschäfte und den Berufs-Classen der Gemusterten.

Fragen, welche durch die Combination der gefundenen Zahlen erledigt werden können, sind z. B.:

Wie vertheilen sich die einzelnen Krankheiten auf die Berufsclassen der Gemusterten, und bei welchen ist diese, bei welchen jene Reihe von Mängeln und Gebrechen die häufigste? Hat die medicinische Statistik durch die Beantwortung dieser Fragen erst sichere und vergleichbare Data gefunden, dann wird es ihr leicht werden, auch den Ursachen der Krankheiten näher zu kommen. Ist der Beruf, die Art der Arbeit, das Material derselben, der Aufenthaltsort der Arbeiter Schuld an der Entstehung der Krankheiten, entwickeln sich diese bei gewissen Berufs-Classen leichter, weil sie einen schwächlichen, in der Entwickelung zurückgebliebenen Körper eines Individuum antreffen, oder wählen die Schwächlinge jene Arbeiten, jenen Beruf lieber, weil dieselben keinen so grossen Kraftaufwand erfordern? Dieses sind Fragen, welchen durch die Rekrutirungs-Statistik wenigstens für die Jahre der Entwickelung der arbeitenden Classen der Bevölkerung gewisse Anhaltspunkte gewährt werden können, und wenn sie auf die ländliche oder industrielle Bevölkerung, je nachdem dieselbe in einem Bezirke vorwiegend den Ersatz der Militairdienstpflichtigen liefern muss, zurückgeführt werden, auch über den günstigen Einfluss jener oder den ungünstigen dieser Arbeitsverwendung urtheilen lassen.

Ueber die letzte, eben erwähnte Frage, den Einfluss der Beschäftigung, Nahrung, Wohnung, Kleidung auf Gesundheit und Körperkraft der Landbewohner gegenüber den Stadtbewohnern ist schon öfters und besonders ungünstig für letztere abgeurtheilt worden. So hat Dr. *Helwig* in einer, zu ihrer Zeit viel Aufsehen verursachenden Schrift („Ueber die Annahme der Kriegstüchtigkeit der ausgehobenen Mannschaften, namentlich in der Mark Brandenburg", Berlin, 1860) die Landbevölkerung besonders günstig in der physischen Qualität zum Militairdienst dargestellt.

„Wo die Bevölkerung ihren Erwerb (p. 41) in der Landwirthschaft und in den mit derselben zusammenhängenden Beschäftigungen findet, ist Gesundheit und Kraft; wo das industrielle Element vorherrscht, ist überall eine Abnahme der Gesundheit und Kraft zu spüren.“

Die statistischen Tabellen, die Dr. *Helwig* zu solchen allgemein ausgesprochenen Behauptungen benutzt, sind aus amtlichen Quellen, wie er schreibt, entnommen; allein dass in diesen auch unrichtige Zahlen vorkommen oder vielmehr aus falscher Berechnung hervorgegangen sein können, scheint als unwahrscheinlich vorausgesetzt zu sein; als Beispiel will ich nur erwähnen, dass in Tafel III. S. 20 der oben angegebenen Schrift sub Rubr. 4. die Zahl der 20jährigen zur Aushebung gelangenden Dienstpflichtigen für 1858 in Brandenburg auf 25,413 angegeben ist; allein nach den Berechnungen des Dr. *Engel*, aus den Volkszählungen entnommen, haben in jenem Jahre nur 18,592 20jährige in der Provinz Brandenburg existirt (Zeitschrift des statistischen Bureau, 1864. No. 7. S. 174).

In den Tabellen des Dr. *Helwig* können 7000, die nur durch wiederholte Zählung in den Listen entstanden sind, eine bedeutend geringere Procentzahl für das Gesammtresultat liefern; auch müssen die dreijährig- und einjährig-freiwillig Eingetretenen die herabgesetzte Procentzahl der Tauglichen jedenfalls bedeutend verbessern.

Dr. *Engel* hatte früher in seinen Berichten über die Aushebungen in Sachsen auch die vorwiegend günstigere Diensttauglichkeit der Landbevölkerung hervorgehoben, und dennoch sind die Resultate nach den jetzigen Berechnungen ganz anders (Zeitschrift des statistischen Bureau, No. 7. 1864. S. 180); so widerlegt er z. B. „das vielverbreitete und manchen Orts gepflegte Vorurtheil, dass die Ackerbau-Bevölkerung des Staats mehr und kräftigere Soldaten liefere als die gewerbliche“ dadurch, dass er nachweist, dass eine Quadratmeile des Düsseldorfer Regierungsbezirkes genau 5mal mehr Einstellungsfähige als die des Cösliner Regierungsbezirkes

liefert; jener ist 5mal so stark bevölkert als dieser und, obwohl er fast nur industrielle Bevölkerung hat, konnte er von der ausschliesslich Ackerbau treibenden Bevölkerung Cöslins in der Diensttauglichkeit nicht übertroffen werden.

Die Häufigkeitszahlen gewisser Krankheiten, Gebrechen oder Missbildungen auf die Rekrutirungslisten begründet, können, wenn sie nach den einzelnen Kreisen und Districten geordnet werden, einen wesentlichen Beitrag zur wissenschaftlichen Forschung der medicinischen Geographie liefern; einzelne besonders häufig vorkommende Gebrechen, die in grossen Procentzahlen die Untauglichkeit zum Militairdienst begründen, andererseits aber auch die Aufmerksamkeit der Staats-Behörden erregt haben, sind schon Gegenstand wissenschaftlicher Forschung gewesen.

Hirsch (Handbuch der historisch-geographischen Pathologie, Erlangen 1859) benutzt die aus den Rekrutirungslisten erhaltenen Resultate über die Häufigkeit und geographische Verbreitung einzelner Krankheiten, theils um letztere zu schildern, theils um der Aetiologie verschiedener Krankheiten näher zu kommen.

Frankreich ist es wiederum, welches in seinen Conscriptionslisten das beste Material zu solchen Forschungen besitzt. So sind daselbst ziemlich umfangreiche Untersuchungen über die Verbreitung des Cretinismus und des endemischen Kropfes gemacht worden; amtliche Zählungen sind, soviel ich erfahren konnte, erst in neuester Zeit daselbst angestellt; fast alle in der französischen Litteratur vorhandenen Abhandlungen über diesen Gegenstand beruhen auf Ausbeutung der Conscriptionslisten. In Betreff des Cretinismus sind die erhaltenen Resultate sehr zweifelhaft, weil sehr oft diese Krankheit mit sporadischem Idiotismus und Blödsinn in eine Rubrik gebracht worden ist.

Die gefundenen Zahlen der einzelnen Jahrgänge gewisser

Departements haben den Schriftstellern gedient, um die Zu- oder Abnahme des endemischen Kropfes zu erörtern; eine der besten hierher gehörigen Arbeiten ist die von Dr. *Bories*, *Médecin aide-major de I. classe*, „*Du recrutement au point de vue du goître et du crétinisme dans le département des Hautes-Alpes*“ (*Réc. de mém. de méd. mil.* 1853. *II. Ser. p.* 275) aus den Conscriptionslisten direct zusammengestellt. Die Zunahme der beiden Krankheiten geht aus der Uebersicht, welche ich hier nur im gedrängten Auszuge mittheilen kann, hervor. Im *Départ. des Hautes-Alpes* nimmt die Anzahl der mit Kropf und Cretinismus behafteten Individuen der Art zu, dass dadurch die Zahl der einstellungsfähigen Rekruten fortwährend abnimmt, während die Zurückstellung wegen anderer Fehler sich so ziemlich auf derselben Höhe erhält. *Canton de l'Argentiere* ergiebt folgende Zahlen für einzelne Jahre berechnet:

	1820.	1830.	1840.	1850.
Goître et crétinisme . . .	3	9	40	42
Affections diverses	8	18	16	21
Exemptions légales . . .	4	5	4	1
Propre au service . . .	13	14	9	5
Canton de Guillestre:				
G. et crét.	14	13	42	40
Aff. div.	15	29	29	23
Ex. lég.	11	7	7	10
Propre au service . . .	16	23	19	18

Aus einer anderen Tabelle des Dr. *Bories* kann man die beträchtliche Procentzahl der wegen jener Krankheiten Zurückgestellten ersehen:

Periodes	*Inscriti*	*Goitreux*	*Affections diverses*	*Propres au service*	*Exemptions légales*
1820—29 *incl.*	1475	244	477	300	143
1830—39 „	1491	512	522	290	162
1840—50 „	1727	728	484	177	177
Summa . .	4693	1484	1483	924	482

Aus diesen Zahlen ersieht man sogleich, dass während der 30 Jahre die Anzahl jener Krankheiten in rapider Weise zugenommen hat; zu bedauern ist dabei, dass beide Krankheiten von *Bories* nicht getrennt sind; den Grund dafür giebt er p. 302 an; indem er sagt: „*le crétinisme pour moi, à ses divers dégrès et est tellement la manifestation extrême de l'affection goîtreuse (lymphatisme gutturo-crétineux) que je n'ai pas cru devoir les séparer.*“

Die Erforschung der Anzahl der mit Kropf behafteten Individuen, ihrer Procentzahl von der Bevölkerung ist schon mehrmals Gegenstand eingehender Untersuchungen besonders auch in Süddeutschland — in Würtemberg und Baden — gewesen, und zwar hat man, wo keine amtliche Zählungen angestellt wurden, jedesmal auf die Conscriptionslisten zurückgegriffen, obwohl sie doch eben nur einen Theil der Bevölkerung repräsentiren können.

Eine sorgsame Zusammenstellung der Zahlen der mit Kropf und Cretinismus behafteten Militairpflichtigen nach den verschiedenen Aushebungsbezirken würde nicht allein über die geographische Verbreitung dieser beiden Krankheiten, über die Zu- und Abnahme derselben, über das Auftreten in Gegenden, die früher verschont waren, und das Erlöschen in davon heimgesuchten, wichtige Anhaltspunkte geben, sondern auch eben, weil man bei beiden Krankheiten eine Einwirkung des Bodens, der Luft, des Trinkwassers wohl mit

Recht voraussetzt, auch für die Aetiologie und den Zusammenhang derselben erhebliche Aufschlüsse bieten.

Aehnliche Resultate liessen sich auch für andere endemisch verbreitete und epidemisch auftretende Krankheiten durch statistische Benutzung des durch die Rekrutirungslisten gebotenen Materials erzielen, ob mit demselben Erfolge, will ich nicht behaupten; denn der wissenschaftliche Werth solcher Ergebnisse hängt wesentlich auch von der Genauigkeit der Diagnose ab, und diese ist gerade in dem Tumulte der Aushebungs-Geschäfte nicht immer sicher zu stellen.

Schon bei der Erörterung der relativen Häufigkeit der vom Arzte gefundenen Krankheiten und Gebrechen haben wir auf diesen schwachen Punkt aller statistischen Berichte über die Rekrutirungs-Geschäfte hingedeutet, und darin finden auch die Gegner aller statistischen Arbeiten immer die Punkte für ihre Angriffe und Widerlegungen; wesentlich in Zweifel gestellt werden jedoch die Ergebnisse der vergleichenden Statistik auf diesem Gebiete der Medicin, so lange nicht ein bestimmtes Schema für die vorgefundenen Krankheiten und Gebrechen existirt, und von den verschiedenen Staaten und Nationen als gültig angenommen ist.

Schon früher haben wir auseinandergesetzt, warum die Classificationen der Krankheiten und Gebrechen, welche die Unbrauchbarkeit der Militairpflichtigen bedingen, besonders die nach den Instructionen der *Comptes rendus* in Frankreich und der *Annual reports* in Nord-Amerika gebräuchlichen Paragraphen keinen vollkommenen wissenschaftlichen Werth haben. Eine solche allen Ansprüchen der Medicin genügende Classification der Krankheiten und Gebrechen ist nach den neueren Principien einer wissenschaftlichen Nosologie zuerst vom Prof. Dr. *Virchow* bei Gelegenheit des internationalen statistischen Congresses

im Jahre 1863 zu Berlin gegeben worden. In der IV. Section dieses Congresses hatte *Virchow* als Berichterstatter der Vorbereitungs-Commission einen Entwurf für eine Rekrutirungs-Statistik geliefert, leider aber aus bekannten und unbekannten Umständen, wie Dr. *Engel* (Zeitschrift des statistischen Bureau, 1864, S. 43) sagt, an den Sitzungen der Commission selbst nicht theilgenommen. Letztere prüfte die einzelnen Positionen der Vorlage, änderte im Ganzen wenig daran, erhob die Vorschläge zu Beschlüssen und empfahl die vorgeschlagenen Tabellenformulare den Staats-Regierungen zur Berücksichtigung. Ob dieses wirklich geschehen ist, kann ich aus den Mittheilungen des Dr. *Engel* in der erwähnten Zeitschrift nicht ersehen. Zu einer technischen Ausarbeitung der Tabellen ist die Commission nicht gelangt, und hierin mag auch wohl der Hauptgrund liegen, dass jener bedeutende und allen Anforderungen der Wissenschaft entsprechende Entwurf der Rekrutirungs-Statistik keine Berücksichtigung gefunden hat. Dr. *Engel* glaubt denselben in der Unausführbarkeit der Anforderungen jenes Entwurfes zu finden, indem er erklärt, dass die rein statistische Bearbeitung des gewonnenen Materials zu viel Kräfte in Anspruch nimmt, weil die vorgeschlagenen Formulare zu weitläufig sind und die Concentration der einzelnen Tabellen, um zu Resultaten für den ganzen Preussischen Staat zu gelangen, die Arbeitskraft von 30 in der Statistik gut geübter Arbeiter auf ein Jahr in Anspruch nehmen würde, um die geforderten Resultate für einen Jahrgang zu gewinnen.

„Dieses Verlangen ist zu gross, und es bleibt daher leicht unerfüllt (S. 48, No. 2. 1864. der Zeitschrift für das statistische Bureau). Ja die Nichtbeachtung der Congresswünsche lässt sich Seitens derjenigen, an welche sie gerichtet sind, sogar mit ihrer Unausführbarkeit entschuldigen.“

Dieser Begründung des berühmten Statistikers vollkommen zu folgen, ist nur möglich, wenn man hinreichende Kenntniss der eigentlichen Technik in der Statistik besitzt; die Forderungen des internationalen statistischen Congresses sind jedoch vom wissenschaftlichen Standpunkte aus so anerkannte und wichtige, dass die Erfüllung derselben auch auf andere Weise versucht werden muss.

Vorschläge zur Erreichung wissenschaftlicher Erfahrungen aus dem Rekrutirungs-Geschäfte.

Der Entwurf des internationalen statistischen Congresses, dessen Motivirung besonders durch die in Frankreich und England schon gewonnenen Resultate gestützt wird, geht hauptsächlich auf die Erhebung der Ergebnisse bei bestehender allgemeiner Wehrpflicht, d. h. also bei der Musterung verschiedener Altersklassen hinaus. Ergebnisse, wie sie die Staaten mit Conscriptionssystem bieten, sollten sich in Preussen einfach aus den Zahlen der als unbrauchbar befundenen Mannschaften in der Altersklasse von 20 Jahren im Vergleich zur Gesammtzahl der Gemusterten dieser Altersklasse ergeben; wir haben aber in Preussen vier verschiedene Kategorieen der Unbrauchbarkeit und wenigstens drei Jahrgänge zur Untersuchung; nur die strenge Scheidung derselben in den Listen kann nach wenigstens drei Jahren zu einem irgendwie den übrigen Staaten gegenüber gleichmässigen Resultate führen. Hierzu kommt, dass bei der Untersuchung der Militairpflichtigen im ersten Jahrgange die Ausmusterung nicht mit besonderer Aufmerksamkeit betrieben wird, weil dieselben noch zweimal vor der Commission erscheinen, dass viele als zu klein, zu schwach, zeitig unbrauchbar zurückgestellt werden, ohne Angabe des Grundes, dass oft Unbrauchbare von der Kreis-Ersatz-Commission nicht angegeben werden, um der entscheidenden Departements-Ersatz-Commission nicht eine zu grosse Anzahl Auszumusternder vorzustellen, so dass die Geschäfte derselben kaum abzuwickeln sind. Letzteres scheint mir ein falsches Princip zu sein; wenn man der Kreis-Ersatz-Commission, gestützt auf das Urtheil des untersuchenden Arztes, auch die gesetzliche Ausmusterung der Unbrauchbaren wegen Krankheit und Gebrechen, welche bis jetzt nicht als augenfällig unbrauchbar beurtheilt worden, zugestehen würde, so würden sich die Zahlen der Auszumusternden für die späteren Jahrgänge bedeutend vermindern, der Geschäftsbetrieb ein wesentlich einfacherer werden; die Superrevision der Departements-Ersatz-Commission würde dann nur für zweifelhafte Fälle anzuwenden sein. Jedenfalls müsste auch die Musterung der Untermässigen stattfinden, weil unter ihnen sich gerade sehr viele befinden, die wegen Krankheit und Gebrechen unbrauchbar sind, dieselben aber unnützer Weise 3mal durch die Listen wandern, um schliesslich, wenn sie auch das Maass erlangt haben, doch als unbrauchbar ausgemustert zu werden. Unbedingt nothwendig ist die Musterung der bis jetzt als „augenfällig unbrauchbar" Bezeichneten, der Lahmen, Blinden, Krüppel etc., sie würden jedenfalls ein bedeutendes Contingent stellen zur Beurtheilung der Lasten, welche einer Gemeinde, dem Kreise, dem Staate aus der Ernährung dieses Theiles der Bevölkerung erwachsen, andererseits aber auch den medicinischen Wissenschaften Aufschluss über gewisse Krankheiten und Gebrechen verschaffen.

Aus den eben aufgeführten Gründen würde sich eine wesentliche Vereinfachung des ganzen Kreis-Ersatz-Geschäftes ergeben, jedoch nur dann, wenn man dem untersuchenden Arzte mehr Zutrauen in der Beurtheilung der Militairpflichtigen schenkte; dass ein solches sicherlich gerechtfertigt wäre, ergiebt sich schon jetzt bei den Departements-Ersatz-Geschäften, bei denen selten das Urtheil des ersten Arztes umgestossen wird.

Eine wirklich gesetzlich entscheidende Stimme wird dem Arzte bei dem Rekrutirungs-Geschäfte so bald noch nicht zu Theil werden, obwohl die Forderung derselben berechtigt ist; eine Aenderung des ganzen Organismus des Ersatz-Geschäftes, um die Ergebnisse desselben zu Zwecken der Wissenschaft zu benutzen, ist sehr schwierig zu erreichen, dazu müssten erst dringende Bedürfnisse von Seiten der militairischen Administration die Aenderung gebieterisch fordern.

Versuchen wir es daher, mit den mindesten Anforderungen an die Behörde durch die Thätigkeit der Aerzte bei dem Kreis-Ersatz-Geschäfte die Forderungen, welche der internationale statistische Congress an eine zweckentsprechende Rekrutirungs-Statistik, welche der sichere Weg zu wissenschaftlichen Erfahrungen ist, gestellt hat, zu realisiren. Diese Erhebungen sind, wenn auch nicht in der ganzen Ausdehnung, wie sie daselbst gewünscht werden, dennoch auf folgende Weise zu erlangen:

1) Der Arzt der Kreis-Ersatz-Commission erhält ausschliesslich zu seiner Benutzung eine namentliche Liste aller der Leute, welche er an jedem Tage zu untersuchen hat.

2) Für jeden Jahrgang der Gemusterten ist eine besondere Liste angefertigt und zwar nach folgendem Schema:

1	2	3	4	5	6	7	8	9	10	11				12
										Angabe der Krankheit oder des Gebrechens				
Laufende No. Jahrgang 1867.	Zuname und Vorname.	Geburtsort, Kreis.	Domicil-ort, Kreis.	Arbeitsort, Kreis.	Datum der Geburt, Monat, Tag, 1847.	Gewerbe oder Stand.	Körpergrösse in Zollen.	Körpergewicht in Zollpfunden.	Brustumfang in Zollen.	dauernd resp. augenfällig unbrauchbar, ausgemustert wegen	nicht vollkommen brauchbar, zurückgestellt wegen	zeitig unbrauchbar, zurückgestellt wegen	brauchbar, behaftet mit	Entscheidung der Kreis-Ersatz-Commission.

Die ersten 7 Rubriken dieser Listen sind von der Civil-Commission ebenso, wie die schon üblichen alphabetischen Listen, auszufüllen, 8 und 9 von dem Officier der Commission festzustellen; erst die 10. und 11. Rubrik wird nach Angabe des Arztes von einem dazu gestellten Schreiber ausgefüllt, ebenso die Rubrik 12, indem vorausgesetzt wird, dass die Untersuchung der Militairpflichtigen in Gegenwart der Commission geschieht. Als Schreiber des Arztes sind am besten Lazareth-Gehülfen zu commandiren, da dieselben sowohl die gebräuchlichen Krankheitsnamen meist richtig zu schreiben verstehen, meist auch, durch Anfertigung von Rapporten geübt, zu den später anzugebenden Auszählungen dieser Listen zu verwenden sind.

Die Messung des Brustumfanges muss vom Arzte nach allgemein aufgestellten Regeln, damit dieselbe für die Statistik brauchbare Resultate liefert, während der Athempause in der Horizontalebene der Brustwarzen bei neben dem Kopfe emporgestreckten Armen ausgeführt werden.

Die Angabe der Krankheit oder des Gebrechens, welche die verschiedenen Kategorien der Unbrauchbarkeit bedingen, muss wissenschaftlich so genau wie möglich stattfinden, damit dieselbe einerseits zur Ausstellung der Unbrauchbarkeits-Atteste nach den Paragraphen der Ersatz-Instruction, andererseits aber auch zur Classification der Krankheiten und Gebrechen dienen kann.

3) Aus diesen Listen der verschiedenen Jahrgänge müssen nun vom Arzte und seinen Gehülfen für jeden Tag Zusammenstellungen nach dem Schema der beifolgenden vier Tabellen gemacht werden. Die Formulare hierzu werden für gewisse Kreise oder Loosungsbezirke, wenn erstere zu gross sind, geliefert; die zweckmässige Technik der Auszählung der Listen wird sich sehr bald ergeben; entweder schaltet man in die Tabellen Bogen ein, welche die Benennungen der verschiedenen Rubriken, die sogenannten Köpfe nicht enthalten, sondern nur die liniirten Rubriken, trägt in diese die Resultate jedes Tages ein und rechnet die Zahlen mehrerer Tage, an welchen für die Loosungsbezirke oder Kreise die Untersuchung der Rekruten stattgefunden hat, zusammen, oder man legt die Spalten oder Rubriken eines Conceptes so gross an, dass die Zahlen von mehreren Tagen bis zur Summirung derselben eingetragen werden können. Da ungefähr 200 bis 300 Mann täglich untersucht werden, so wird die Arbeitszeit für diese Zusammenstellungen ungefähr vier Stunden betragen, von denen zwei auf den Arzt für die Ausfüllung der Tabellen III. und IV., zwei auf seinen Gehülfen für die Tabellen I. und II. fallen.

Die Tabelle I. enthält über ihrem Kopfe einige Rubriken, wie Einwohnerzahl, Männliche Bevölkerung etc., welche aus amtlichen Listen der Civil-Behörden ausgefüllt werden müssen; die Spalten 4, 5, 11 und 12 können beim Kreis-Ersatz-Geschäft selbst nicht berücksichtigt werden, sondern müssen entweder durch Berichte der Truppen-Aerzte, denen ja die Untersuchung der eingetretenen Freiwilligen und der zu entlassenden Rekruten obliegt, oder aus den Listen jedes General-Commandos nachträglich ausgefüllt werden. Die Ergebnisse der Musterung für jeden Jahrgang allein wird man jedoch immer erst nach drei Jahren erhalten, wenn man dieselben während der drei Concurrenzjahre durch diese drei Tabellen verfolgt. Erst dann gelangt man zu der eigentlichen Tauglichkeitsziffer, sowie auch zu der Zahl der Unbrauchbaren wegen Untermaass oder wegen Krankheit und Gebrechen; die drei Altersklassen, 17-, 18-, 19Jährige, sind hier besonders noch aufgeführt, um zu zeigen, dass eine hinreichende Entwickelung des Körpers auch schon vor dem 20. Lebensjahre den Eintritt zum Militairdienst gestattet. Die Spalte 11 ist sehr leicht zu erhalten, da für jeden entlassenen Rekruten wiederum ein besonderes Attest ausgestellt werden muss.

Die Procentzahlen der Militairtüchtigen von der Bevölkerung sind aus diesen Tabellen dann leicht durch Berechnung zu finden.

Die Tabelle II. giebt die Ergebnisse der Musterung nach Körpergrösse und Brustumfang und das Verhältniss derselben zu einander, sowohl bei den Brauchbaren als auch bei den Unbrauchbaren, während der drei Concurrenzjahre an; eine ähnliche Tabelle würde man aufstellen müssen, wenn man das Körpergewicht beim Rekrutirungs-Geschäft erlangen könnte, statt Brustumfang würde Körpergewicht von 90—100, von 100—105, von 105—110 Pfund gesetzt werden müssen.

Die Vergleichung solcher Tabellen während der drei Concurrenzjahre für denselben Kreis oder Aushebungsbezirk würde die oben angedeuteten Resultate ergeben.

Die Tabelle III. zeigt an ihrem Kopfe die Bezeichnungen der aufgefunden Krankheiten und Gebrechen, nach der Classification des Professor Dr. *Virchow* (Zeitschrift des statistischen Bureau, 1864. No. 2. S. 35), jedoch nicht nur für die als unbrauchbar Befundenen und Zurückgestellten, sondern auch für Brauchbare, deren Einstellung durch temporäre Krankheiten oder geringe Abweichungen vom Normalzustande nicht gehindert wird; für jeden Jahrgang sind daher drei Zeilen angewendet. Empfehlen würde es sich hier, nur die Summe aus den für unbrauchbar Befundenen zu berechnen, da die Zurückgestellten gewöhnlich durch mehrere Jahrgänge sich schleppen und bei einer Berechnung derselben für einen Jahrgang viel zu hohe Summen entstehen.

Im dritten Concurrenzjahre würde diese Summe der Spalte 8 in Tabelle I. entsprechen, während aus jener Tafel

die in Spalte 11 enthaltene Anzahl der entlassenen Rekruten hier nach Krankheiten und Gebrechen eingetragen werden könnte. Schliesslich müsste aus drei vorhergehenden Berichten in dieses Schema für den ersten Jahrgang der drei vergangenen Concurrenzjahre die Summe der Unbrauchbaren nach Angabe der Krankheiten und Gebrechen eingetragen werden. Die beiden letzten Zeilen können wiederum nicht beim Kreis-Ersatz-Geschäft selbst ausgefüllt werden, dieses müsste bei der vorgesetzten militair-ärztlichen Behörde geschehen, in deren Hände ja sämmtliche Tabellen und Listen gelangen müssen.

Die IV. Tabelle endlich enthält die Ergebnisse der Musterung nach Krankheiten und Gebrechen, welche die Unbrauchbarkeit zum Militairdienst bedingen, und zwar auf die verschiedenen Berufs-Klassen und -Arten vertheilt. Für letztere ist ebenfalls das Schema des internationalen Congresses angenommen; eine weitere Ausführung der letzteren Tabelle nach Altersklassen etc. ist nicht nothwendig, weil eben nur die Verhältnisszahlen der Krankheit und Gebrechen der einzelnen Berufsklassen gewonnen werden sollen.

In dem Entwurf der Rekrutirungs-Statistik sind noch zwei Tabellen, die Maasse der Gemusterten nach ihren verschiedenen Berufsarten und (wie Tab. I.) das Ergebniss der Musterung nach Berufsklassen aufgeführt; höchstens könnte man letztere noch aufführen; erstere dagegen würde kaum ein so grosser Arbeit entsprechendes Resultat liefern.

4*a*) Diese von den Militair-Aerzten gelieferten Tabellen müssen von denselben mit sämmtlichen Original-Listen an ihre nächste ärztliche Behörde, in Preussen also an den General-Arzt des Armee-Corps, kurz nach Beendigung des Kreis-Ersatz-Geschäftes eingereicht werden.

b) Derselbe hat aus den Listen des General-Commandos oder aus Berichten seiner ihm untergebenen Truppen-Aerzte die oben angeführte Vervollständigung, eventuell Berichtigung der Tabellen zu veranlassen und die Procentberechnung der Brauchbaren, die relative Häufigkeitszahlen der Krankheiten und Gebrechen etc. aufzustellen.

c) Diese für den Bereich einer Provinz ausgeführten Zusammenstellungen werden mit den Tabellen der einzelnen Kreise, womöglich auch mit den Original-Listen einer Central-Behörde (der statistischen Abtheilung des Militair-Medicinal-Wesens) übergeben, um so zu einer statistischen Bearbeitung der Ergebnisse des Rekrutirungs-Geschäftes für den ganzen Staat zu gelangen.

Die Resultate dieser Arbeiten müssten den mit der Rekrutirung betrauten Aerzten, die Ergebnisse in den einzelnen Kreisen wiederum den im folgenden Jahre daselbst beschäftigten Aerzten als Anhalt zur neuen Arbeit mitgetheilt werden.

5) Die Zahlen der Tabellen werden an und für sich schon deutliche Berichte der Musterungen sein; besondere meist über allgemeine Fragen, z. B. über den Kulturzustand und die Intelligenz der Bevölkerung sich verbreitende Berichte von den aushebenden Aerzten zu verlangen, empfiehlt sich nicht. Wenn man eine Reihe solcher Berichte durchsieht, auf welche oft sehr viel Mühe und Fleiss verwandt ist, so finden sich bald immer dieselben stereotypen Redensarten, oder die Urtheile gehen nach der individuellen Anschauung der Berichterstatter so weit auseinander, dass wenig Material für eine gleichmässige Beurtheilung eines grösseren Theiles des Landes zu erwarten ist. Will man mit Vortheil gute Berichte erzielen, so müssen ganz bestimmte, für einzelne Gegenden oder Kreise passende Fragen gestellt werden, die sich an die grösseren, relativen Häufigkeitszahlen der Krankheiten und Gebrechen aus früheren Jahren anschliessen z. B. über die Verbreitung und Ursachen gewisser Krankheiten bei den Fabrik-Arbeitern dieses oder jenes Kreises, über die Verbreitung und Zu- und Abnahme contagiöser Krankheiten, z. B. der Augen, über die Verbreitung des Kropfes nach den Bodenverhältnissen etc.

Berichte über das Rekrutirungs-Geschäft ohne vorhergegangene genaue statistische Erhebungen anzufertigen, muss man für verfehlt halten, weil sie aller sicheren Grundlagen entbehren und oft zu unbegründeten Urtheilen über Land und Bewohner führen. Die in vorstehender Arbeit aufgeführten, wissenschaftlichen Erfahrungen, so gering sie auch bis jetzt noch sein mögen, fordern dringend zur Bearbeitung des durch die Musterung der Bevölkerung gebotenen Materials auf.

Ergebniss der Musterung für das Jahr 18 . . im Kreise *N. N.*

Tab. I. zu Seite 237

		Einwohnerzahl	Männliche Bevölkerung	Männliche Bevölkerung von 20—25 Jahren	Ländliche Bevölkerung	Städtische Bevölkerung
Provinz . . .						
Regierungsbezirk						
Kreis						

1	2	3	4	5	6	7	8	9	10	11	12
Altersklassen	Zahl der Gemusterten im Jahre 1867	Zahl der Brauchbaren			Zahl der Unbrauchbaren wegen Krankheit und Gebrechen	Zur Ersatz-Reserve nach 3maliger Concurrenz		Ein Jahr zurückgestellt		Zahl der wegen Unbrauchbarkeit entlassenen Rekruten	Zahl der eingestellten Brauchbaren *incl.* Freiwilligen
		beim Kreis-Ersatz-Geschäft brauchbar befunden	der dreijährig Freiwilligen	der einjährig Freiwilligen		wegen Untermaass	nicht vollkommen brauchbar, zeitig unbrauchbar	zeitig unbrauchbar und nicht vollkommen brauchbar	wegen Untermaass		
1850											
1849											
1848											
3 Concurrenzjahre: 1847 (20jährige)											
1846											
1845											
1844											
1843 u. älter											
Summa . .											

Ergebniss der Musterung nach Körpergrösse und Brustumfang.

Kreis *N. N.* Regierungsbezirk *N. N.* Zahl der Gemusterten. (Tab. I. Spalte 2.)

Tab. II. zu Seite 238

Altersklasse	Bei einer Körpergrösse von	Zahl der Brauchbaren. Bei einem Brustumfange von					Zahl der Unbrauchbaren wegen Krankheiten und Gebrechen. Bei einem Brustumfange von						Zahl der als zeitig oder nicht vollkommen brauchbar Zurückgestellten. Bei einem Brustumfange von					
		30-32"	32-33"	33-34"	34-35"	35" u. mehr	unter 30"	von 30-31"	31-32"	32-33"	33-34"	34" u. mehr	unter 30"	von 30-31"	31-32"	32-33"	33-34"	34" u. mehr
20jährige	unter 5'																	
	5 — 5' 1"																	
	5' 1" — 5' 2"																	
	5' 2" — 5' 6"																	
	5' 6" — 6'																	
	über 6'																	
	Summa . .																	
21jährige	unter 5'																	
	5' — 5' 1"																	
	5' 1" — 5' 2"																	
	5' 2" — 5' 6"																	
	5' 6" — 6'																	
	über 6'																	
	Summa . .																	
22jährige	unter 5'																	
	5' — 5' 1"																	
	5' 1" — 5' 2"																	
	5' 2" — 5' 6"																	
	5' 6" — 6'																	
	über 6'																	
	Summa . .																	

Ergebniss der Musterung nach Krankheiten und Gebrechen und nach den Altersklassen der Gemusterten.

Kreis *N. N.* Regierungsbezirk *N. N.* Zahl der Gemusterten (Tab. I. Spalte 2).

Tab. III. zu Seite 238

		I. Infectionskrankheiten.				II. Intoxicationen.		III. Pflanzliche u. thierische Parasiten.			IV. Erworbene Mängel u. Entstellungen.							V. Angeborene Missbild. u. Deformitäten.			VI.	VII. Störungen der Entwickelung und Ernährung.				VIII. Störungen der Circulation.			IX. Functionelle Störungen.							X. Organische Krankheiten der einzelnen Theile.									
		Syphilis	Intermittens	Rheuma	Struma	Alkoholismus	Ergotismus	Tinea	Herpes	Scabies etc.	Calvities	Defect. nar. lab. etc.	Caput. obstipum	Pectus angustum	Contract. et Ankylos.	Pes planus	Genu valgum etc.	Hermaphroditismus	Labium lepor.	Defect. digitorum	Disposit. herniosa	Cachexia pauperum	Rachitis	Scrophulosis	Tumores etc.	Hydrops anasarca	Haemorrhoides	Varicocele etc.	Muto-surditas	Cretinismus	Epilepsie	Spasmi	Myopie	Strabismus	Diplopia etc.	Psoriasis	Combustio, Congelatio	Caries, Necros. oss.	Vitia cordis	Varices	Amblyopia	Phthisis pulm.	Asthma	Peritonitis chronica	Nephritis etc.
		1	2	3	4	5	6	8	9	10	11	12	13	14	16	18	19	31	32	33	40	46	47	48	49	60	62	63	71	72	73	74	75	76	77	101	102	103	104	106	107	109	110	112	113
20-jährige	Brauchbar																																												
	Unbrauchbar																																												
	Zurückgestellt																																												
21-jährige	Brauchbar																																												
	Unbrauchbar																																												
	Zurückgestellt																																												
22-jährige	Brauchbar																																												
	Unbrauchbar																																												
	Zurückgestellt																																												
	Summa und zwar der als unbrauchbar Ausgemusterten																																												
	Zahl der im ersten halben Jahre zurückgeschickten Rekruten (Tab. I. Spalte 11)																																												
	Summa der Unbrauchbaren für den Jahrgang 18.. aus den drei vorhergehenden Berichten zusammengestellt																																												

Ergebniss der Musterung nach Krankheiten und Gebrechen und nach den Berufsklassen der Gemusterten*).

Kreis *N. N.* Regierungsbezirk *N. N.* Zahl der Gemusterten (Tab. Spalte).

Tab. IV. zu Seite 239

		I. Infections-krank-heiten.				II. Intoxicationen		III. Pflanzliche u. thierische Parasiten			IV. Erworbene Mängel und Entstellungen.							V. Angeborene Missbild. u. Deformitäten.			VI. Hernien.	
		Syphilis 1	Intermittens 2	Rheuma 3	Struma 4	Alcoholismus 5	Ergotismus 6	Tinea 8	Herpes 9	Scabies etc. 10	Calvities 11	Defect. nar. lab. etc. 12	Caput obstipum 13	Pectus angustum 14	Contract. et Ankylos. 16	Pes planus 18	Genu valgum etc. 19	Hermaphroditismus 31	Labium lepor. 32	Defect. digitorum 33	Disposit. herniosa 40	[illegible]
1	Ackerbau u. Viehzucht																					
2	Industrie im engeren Sinne																					
3	Handel																					
4	Verkehr																					
5	Persönliche Dienstleistungen																					
6	Gesundheitspflege																					
7	Erziehung u. Unterricht																					
8	Kunst und Wissenschaften etc.																					
9	Gottesdienst																					
10	Landesfürstliche Haus- pp. Verwaltung																					
11	Justiz																					
12	Armee u. Kriegsflotte																					
13	Personen ohne Beruf																					

		VII. Störungen der Entwickelung und Ernährung.					VIII. Störungen der Circulation.			IX. Functionelle Störungen.							X. Organische Krankheiten der einzelnen Theile.									
		Debilitas universalis 45	Cachexia pauperum 46	Rachitis 47	Scrophulosis 48	Tumores 49	Hydrops anasarca 60	Haemorrhoides 62	Varicocele etc. 63	Muto-surditas 71	Cretinismus 72	Epilepsie 73	Spasmi 74	Myopie 75	Strabismus 76	Diplopia etc. 77	Psoriasis 101	Combustio, Congelatio 102	Caries, Necros. oss. 103	Vitia cordis 104	Varices 106	Amblyopie 107	Phthisis pulm. 109	Asthma 110	Peritonitis chronica 112	Nephritis etc. 113
1	Ackerbau u. Viehzucht																									
2	Industrie im engeren Sinne																									
3	Handel																									
4	Verkehr																									
5	Persönliche Dienstleistungen																									
6	Gesundheitspflege																									
7	Erziehung u. Unterricht																									
8	Kunst und Wissenschaften etc.																									
9	Gottesdienst																									
10	Landesfürstliche Haus- pp. Verwaltung																									
11	Justiz																									
12	Armee u. Kriegsflotte																									
13	Personen ohne Beruf																									

*) Der Kopf der Tabelle III. u. IV. findet sich mehr ausgeführt in der statist. Zeitschrift von Engel.

finished 22nd March
1887

BAROMETRIC TABLES,

WITH DIRECTIONS FOR THEIR USE IN CALCULATING HEIGHTS, ACCORDING TO BAILY'S FORMULA.

EDINBURGH, *February* 1869.

THE following Tables were copied for my own use from various works which I had occasion to consult about fifteen years ago. Having found them exceedingly convenient in practice, and having reason to believe that they are as accurate as any Barometric Tables that have appeared, I have had a few copies printed for the use of friends who may be placed in situations in which the determination of Heights is of importance or interest. The formula is Baily's; the merit of expanding the Tables belongs to Mr. Howlett; they were first published (I believe) in the expanded form in "Jackson's Military Surveying."

In order to apply the Tables to Observations, a Table of seven-place Logarithms is required. The calculation by means of Logarithms is more expeditious, more accurate, and fully easier than the computations by the empirical arithmetical formulæ to be met with in some books. The symbols used in Baily's Formula and in these Tables will be found explained on page 6. I have appended an original Example, to illustrate the mode of work.

W. R.

Difference between Temperatures on Attached Thermometers, or difference between Temperatures of the Barometric Columns at the two stations, = D.

TABLE I.—TO FIND B.

D.	(T − T′) B.	(T′ − T)
Fahrt.	When the temperature is highest *below*.	When the temperature is highest *above*.
0°	0·0000000	0·0000000
1	·0000434	9·9999566
2	·0000869	·9999131
3	·0001303	·9998697
4	·0001737	·9998262
5	·0002171	·9997828
6	·0002605	·9997393
7	·0003039	·9996959
8	·0003473	·9996524
9	·0003907	·9996090
10	·0004341	·9995655
11	·0004775	·9995220
12	·0005208	·9994785
13	·0005642	·9994350
14	·0006076	·9993916
15	·0006510	·9993481
16	·0006943	·9993046
17	·0007377	·9992611
18	·0007810	·9992176
19	·0008244	·9991741
20	·0008677	·9991305
21	·0009111	·9990870
22	·0009544	·9990435
23	·0009977	·9990000
24	·0010411	·9989564
25	·0010844	·9989129
26	·0011277	·9988694
27	·0011710	·9988258
28	·0012143	·9987823
29	·0012576	·9987387
30	·0013009	·9986952
31	·0013442	·9986516

Temperatures of Air, or $t + t'$, $= S$.

TABLE II.—TO FIND A.

S. Fahrt.	A.	S. Fahrt.	A.
40°	4·7689067	75°	4·7859208
41	·7694021	76	·7863973
42	·7698971	77	·7868733
43	·7703911	78	·7873487
44	·7708851	79	·7878236
45	·7713785	80	·7882979
46	·7718711	81	·7887719
47	·7723633	82	·7892451
48	·7728548	83	·7897180
49	·7733457	84	·7901903
50	·7738363	85	·7906621
51	·7743261	86	·7911335
52	·7748153	87	·7916042
53	·7753042	88	·7920745
54	·7757925	89	·7925441
55	·7762802	90	·7930135
56	·7767674	91	·7934822
57	·7772540	92	·7939504
58	·7777400	93	·7944182
59	·7782256	94	·7948854
60	·7787105	95	·7953521
61	·7791949	96	·7958184
62	·7796788	97	·7962841
63	·7801622	98	·7967493
64	·7806450	99	·7972141
65	·7811272	100	·7976784
66	·7816090	101	·7981421
67	·7820902	102	·7986054
68	·7825709	103	·7990681
69	·7830511	104	·7995303
70	·7835306	105	·7999921
71	·7840098	106	·8004533
72	·7844883	107	·8009142
73	·7849664	108	·8013744
74	·7854438	109	·8018343

Temperatures of Air, or $t + t'$, $= S$.

TABLE II.—TO FIND A.

S. Fahrt.	A.	S. Fahrt.	A.
110°	4·8022936	145°	4·8180714
111	·8027525	146	·8185140
112	·8032109	147	·8189559
113	·8036687	148	·8193979
114	·8041261	149	·8198387
115	·8045830	150	·8202794
116	·8050395	151	·8207196
117	·8054953	152	·8211594
118	·8059509	153	·8215988
119	·8064058	154	·8220377
120	·8068604	155	·8224761
121	·8073144	156	·8229141
122	·8077680	157	·8233517
123	·8082211	158	·8237888
124	·8086737	159	·8242256
125	·8091258	160	·8246618
126	·8095776	161	·8250976
127	·8100287	162	·8255331
128	·8104795	163	·8259680
129	·8109298	164	·8264024
130	·8113796	165	·8268365
131	·8118290	166	·8272701
132	·8122778	167	·8277034
133	·8127263	168	·8281362
134	·8131742	169	·8285685
135	·8136216	170	·8290005
136	·8140688	171	·8294319
137	·8145153	172	·8298629
138	·8149614	173	·8302937
139	·8154070	174	·8307238
140	·8158523	175	·8311536
141	·8162970	176	·8315830
142	·8167413	177	·8320119
143	·8171852	178	·8324404
144	·8176285	179	·8328686

Coefficient for Latitude.

TABLE III.—TO FIND C.

L.	C.	L.	C.
0°	0·0011689	49°	9·9998372
3	·0011624	50	·9997967
6	·0011433	51	·9997566
9	·0011117	52	·9997167
12	·0010679	53	·9996772
15	·0010124	54	·9996381
18	·0009459	55	·9995995
21	·0008689	56	·9995613
24	·0007825	57	·9995237
27	·0006874	58	·9994866
30	·0005848	59	·9994502
33	·0004758	60	·9994144
36	·0003615	63	·9993115
39	·0002433	66	·9992161
42	·0001223	69	·9991293
45	·0000000	75	·9989852
48	9·9998775	81	·9988854

EXPLANATION OF BAILY'S FORMULA.

When β = Barometric pressure, in English inches, below.
β' = Do. do. do., above.
T = Temperature by attached Fahrt. Thermometer, below.
T′ = Do. do. do., above.
S = Sum of $t + t'$, or of Air Temperatures, below and above.
D = T − T′, or T′ − T, as the case may be.
L = ϕ = Latitude of Station where observation is made.

In Tab. II. $A = \log.\{60345·51 \times [1 + ·00111111\,(t + t' - 64°)]\}$.
In Tab. I. $B = \log.\{1 + ·0001\,(T - T')\}$.
In Tab. III. $C = \log.\{1 + ·002695 \cos 2\phi\}$.
Make $R = \log.\beta - (B + \log.\beta')$.

Then, by Baily's Formula, A + C + log. of R = X, and the number corresponding to this logarithm = HEIGHT.

(The Height is thus obtained in English feet.)

EXAMPLE.

Calculation, by means of these Tables, of the Height of St. Paraskevi Spring above the level of the Sea at Renkioi, Asia Minor, in latitude 40° N. Observations taken with a Gay-Lussac Barometer on 7th October 1855. Lower Station 10 feet above Sea; Upper Station 7 feet above Spring; hence, Additive correction = + 3 feet.

$$\left.\begin{array}{l}\beta = 30·197,\ \ T = 77°,\ \ t = 76°\\ \beta' = 29·494,\ \ T' = 74°,\ \ t' = 74°\end{array}\right\}\text{Observations.}$$

Then,

By Tables B =	·0001303	deduced from Table I.
Log. of β' =	1·4697337	from ordinary Log. Table.
	1·4698640	by addition.
Log. of β =	1·4799638	from ordinary Log. Table.
	·0100998	by subtraction.

	Work by Logarithms.
Log. of R, or of ·0100998	$= \bar{2}·0043128$
By Table II.,	A = 4·8202794
By Table III.,	C = ·0002030

	Feet.	
Height between Stations	= 668·029	= 2·8247952
Additive correction 10 − 7	= + 3·	
Height above Sea-level	= 671·029 feet, or say, 671 *feet.*	

N.B.—When, as in the Example above given, the Barometric Observations are taken with a Gay-Lussac Barometer, no correction for "Capacity" or "Capillarity" is required. When observations are taken with another form of instrument, β and β' must have these corrections applied before their logarithms are sought. No allowance is made by Baily's Formula for "Dew-point;" and, except in rare instances, none is needed.

EXAMPLE FOR PRACTICE.

The late Principal Forbes made the following observations on the Col du Géant, in lat. 45° 45′:—

Millimètres.	Centigrade.	Fahrenheit.
$\beta' = 507{\cdot}98$,	$T' = +\ 0{\cdot}6$,	$t' = 29{\cdot}8$.

Simultaneous observations in Geneva gave :—

Millimètres.	Centigrade.	Centigrade.
$\beta = 729{\cdot}85$,	$T = 0{\cdot}0$,	$t = 17{\cdot}2$.

Required : Height of the Col above Geneva? *Ans.* 9803 feet.

The above is an excellent example for practice. When the requisite reductions to English measures are made, the height found by these Tables is 9803 feet—the very number found by Forbes.

The reductions to English measures are as follows :—$\beta' = 20{\cdot}000$, $\beta = 28{\cdot}735$ inches. $T' - T = 1^\circ{\cdot}08$ Fahrt. $t + t' = 92^\circ{\cdot}76$ Fahrt.

APPENDIX.

CORRECTION FOR MOISTURE.

When observations with Daniell's Hygrometer or with Wet-Bulb thermometers have been taken at the two stations, the Mean Humidity may be estimated by Glaisher's Hygrometric Tables. Perfect Saturation being $=1$, the Mean Humidity must always be a decimal fraction, which may be represented by a.

Table IV. furnishes the simplest means with which I am acquainted for calculating the correction dependent on a. The Table was originally given by Warnstorff in Schumacher's Hülfstafeln 1845; but it consisted of only six terms, and was applicable to the Old French Measures only. I have greatly expanded the Table, and have adapted it to the English standards. Further interpolations for intermediate values of X and of $\frac{t+t'}{2}$ may be made, readily enough, by inspection.

In using the Table, it must be entered with the particular values of X found by Baily's method, and of $\frac{t+t'}{2}$, or of half the sum of the air-temperatures in degrees of Fahrenheit's scale. The factor obtained from the Table may be termed ψ.

Then, $X + a\,\psi =$ Corrected Height in English feet.

Table IV.—To find ψ.

Height, in Feet, or X, found by Baily's Method.	Half Sum of Air-Temperatures, or $\frac{t+t'}{2}$ in Fahrt. Scale.		
	32°	50°	68°
3000	7·90	15·05	27·84
4000	10·88	20·49	37·82
5000	13·95	26·17	47·87
6000	17·21	31·97	58·36
7000	20·48	37·90	69·11
8000	23·87	44·12	80·13
9000	27·33	50·55	92·16
10000	30·91	57·32	104·45
11000	34·56	64·23	117·14
12000	38·50	71·24	130·39
13000	42·56	78·94	144·00
14000	46·78	86·94	157·92
15000	51·20	95·01	172·25
16000	55·74	103·36	186·50
17000	61·24	112·07	201·50
18000	67·29	120·87	217·00
19000	73·60	129·76	233·40
20000	80·00	138·94	251·30

EXAMPLE OF CORRECTION FOR MOISTURE.

On the summit of Monte Rosa, in lat. 46 N, Schlagintweit made the following observations :—$\beta' = 17{\cdot}244$ inches, reduced to 32° Fahrt.
21·8 $t' = 23°{\cdot}36$. Wet-Bulb $' = 22°{\cdot}64$. Hence, Dewpoint $' =$ [illegible], and Relative Humidity = ·[illegible] = ·936

Simultaneous observations at Milan gave $\beta = 29{\cdot}5407$ inches, reduced to 32° Fahrt. $t = 76°{\cdot}82$. Wet Bulb 67°·82. Hence, Dewpoint =
63·13 [illegible], and Relative Humidity = ·[illegible] = ·628

Work by Baily's Method.

Logarithm of $\beta' = 1{\cdot}2366380$
$\beta = 1{\cdot}4704108$
$\cdot 2337728 = \text{R}.$

Logarithm of $\text{R} = \bar{1}{\cdot}3687940$
$\text{A} = 4{\cdot}7977619$
$\text{C} = 9{\cdot}9999700$

Feet.
$\text{X} = 14673{\cdot}282$ $\qquad 4{\cdot}1665259$

CORRECTION FOR MOISTURE.

Mean Humidity $= a =$ ·[illegible] ·782. Factor in Table IV. $= \psi = 92{\cdot}7494$.
72·53 Hence $a\,\psi =$ [illegible], and X being 14673·282, Corrected Height above Milan = [illegible] English feet. = 14745·812

In Metres, the above is = [illegible] = 4494·4

Add Height of Station in Milan above sea } = 154·2 as given by Schlagintweit.

Height of Monte Rosa above the sea } [illegible] 4648·6 Metres = [illegible] 15251·6 Feet.

Schlagintweit's calculation, by Gauss's Tables, gives 4649·3 Metres = 15253·9 Feet, as the Corrected Height of Monte Rosa.

Note. On calculating the Dewpoints by Apjohn's Formula and Regnault's Table of Elastic Force, the above corrections are necessary. W.R.

MILITARY GYMNASIA.

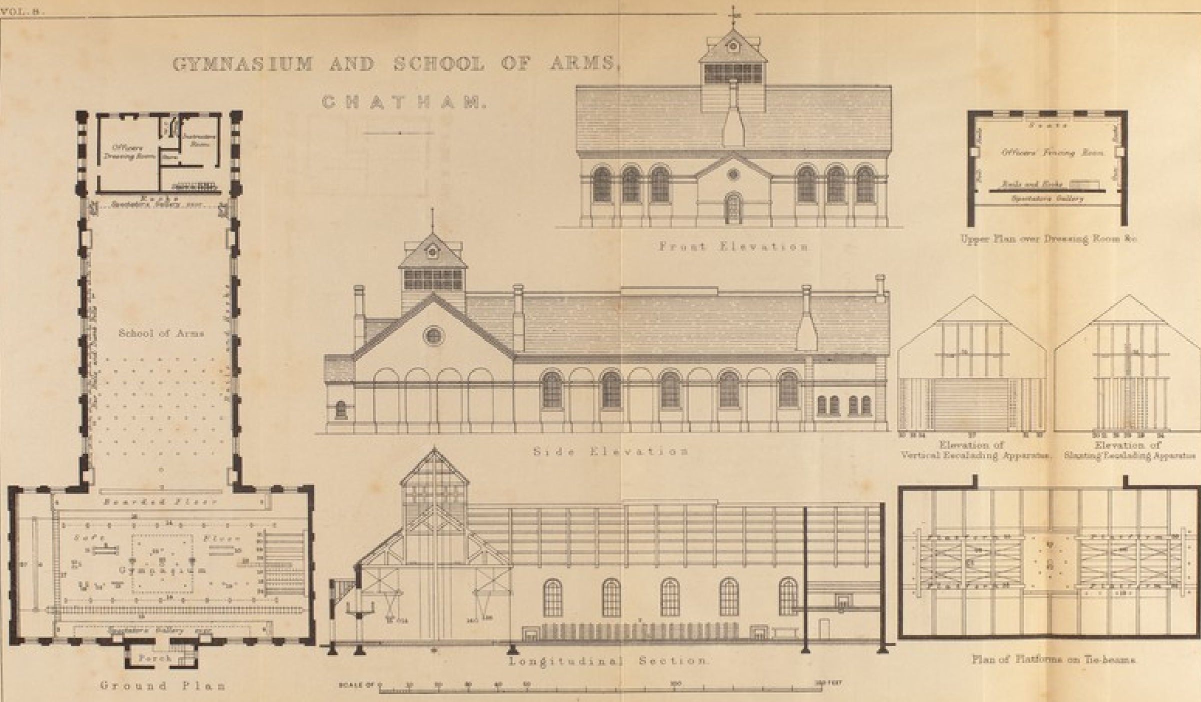
BY ARCHIBALD MACLAREN, ESQ.,
THE GYMNASIUM, OXFORD.

From the Journal *of the* Royal United Service Institution, *vol.* viii.

LONDON.

1864.

GYMNASIUM AND SCHOOL OF ARMS, CHATHAM.



Nº of Section	Title of Section	Nº on Plan	APPARATUS.
SECTION 1	Moveable Apparatus	1	*Dumb Bells*
		2	*Bar Bells*
SECTION 2	Exercises of Progression	3	*Leaping Barriers*
		4	*Leaping Rope*
		5	*Vertical Ladder and moveable Platform for Leaping depth*
		6	*Horizontal Moveable Beam*
		7	*Horizontal Fixed Beam*
		8	*Vaulting Bar*
		9	*Vaulting Horses*
SECTION 3	Elementary Exercises.	10	*Fixed Parallel Bars*
		11	*Moveable Parallel Bars*
		12	*Trapezium*
		13	*Pair of Rings*
		14	*Rows of Rings*
		15	*Elastic Ladder*
		16	*Horizontal Bar*
		17	*Bridge Ladder*
		18	*Ladder Planks*
SECTION 4	Free Climbing	19	*Vertical Poles*
		20	*Slanting Pole*
		21	*Turning Pole*
		22	*Mast*
		23	*Pair of Vertical Poles*
		24	*Pair of Slanting Poles*
		25	*Vertical Ropes*
		26	*Rosaries*
SECTION 5	Escalading	27	*Prepared Wall (Holes, Blocks, Grooves)*
		28	*Planks* (Against a Wall)
		29	*Inclined Ladder* (Against a Wall)
		30	*Rope Ladder* (Against a Wall)
		31	*Vertical Poles, fixed* (Against a Wall)
		32	*Vertical Pole, suspended* (Against a Wall)
		33	*Vertical Rope* (Against a Wall)
		34	*Knotted Rope* (Against a Wall)
		35	*Series of Upper and Lower Platforms with Vertical Poles connecting them.*
		36	*Wire Guard Net under Tie-beams*

J. S. [illegible]

MILITARY GYMNASIA.

By Archibald Maclaren, Esq., The Gymnasium, Oxford.

From the Journal *of the* Royal United Service Institution, *vol.* viii.

When I had the honour, some time ago, of reading a paper on this subject to the members of this Institution, I endeavoured to show the value of a gymnastic training to the soldier.* I desire to-day to do myself the honour of describing the material means which I have found it necessary to employ, and in many instances to devise, for the purpose of carrying out my system of training, which means are now being provided by the military authorities with a completeness which leaves nothing to be desired.

With the first conception of the leading features of the system, I perceived that the construction and fitting up of proper gymnasia would be a *sine quâ non*, indeed must be viewed as an integral part of the system itself; and as in this respect it differs from the military systems of other countries,† which are entirely carried on in the open air, I will, with your permission, mention a few of the advantages arising from the organization of such schools.

The first of these is, the value which regular and consecutive instruction possesses over irregular practice.

In the cultivation of the bodily powers it is quite necessary that the instruction should be progressive—that to-day's lesson should, as it were, be taken up to-morrow and carried a little farther on, and the next day and the next a little farther still, and so on to the end of the course. When thus administered each lesson is in accord with that which preceded it, and with that which is to follow it—each aiding each—

* Journal of the Royal United Service Institution. Vol. vi.
† National Military Systems of Bodily Exercise. By Archibald Maclaren.

each improving that which has gone before—each preparing the way for that which is to come; but this, of course, can only be done where provision is made for regular and consecutive instruction. Now regularity and consecutiveness, it will at once be seen, are quite incompatible with open air practice in any country whatever; and in a climate like ours are simply impossible. There are few days in the year that are really fit or suitable for such practice, or on which men would willingly encounter its discomforts; and a system of bodily training which is dependent upon the favour of the weather is in reality no system at all. My anticipations in this respect have been conclusively proved. In connection with the first gymnasium erected, that at Aldershot, which is comparatively small and inadequate to the requirements of so large a station, it was considered possible to extend the accommodation by the erection of certain machines in the open air, on a convenient spot in front of the building. For this purpose I prepared a plan of a gymnastic ground, embracing some of the most attractive and interesting articles of apparatus, on which exercises of competition and emulation might be performed. This was four years ago; but to this day a learner's hand has not been laid upon them; it is either too hot or too cold—too sunny—too windy—too wet or too something; but the fact stands out that, at a station where the gymnasium could give employment to but a small per centage of the troops, nothing whatever could be done with these open-air erections.

I have alluded to the discomforts of open-air practice; these may seem trifling, but when examined are more serious than appears at first sight.

To execute any gymnastic exercise, or any exercise indeed of any kind in which strength is to be exerted, or is to be derived from its performance, there must be complete freedom of clothing. This simply means that the soldier must be stripped to his shirt and trousers, with his neck open, his head bare, and his sleeves tucked up to his elbows. Now it is of the essence of gymnastic exercises, after the rudimentary lessons, that the efforts will for the most part be brief and energetic, with some necessary standing about,—waiting for and watching the efforts of others. And this with men so employed, and so exposed, and so constituted, is, save on rare occasions, neither sanitary, nor even safe in the open air.

I need scarcely, I am sure, state, that I, of all men, am least disposed to enervate or *coddle*; the whole work of my life has been, and is, and probably will be, to make men not only healthier but hardier; to teach them how to retain the strength they have, as well as how to add to its amount. But if experience has taught me anything, it has taught me this, that more evil may be done by rash and sudden exposure—by what is generally known as the *hardening system*, than by all the coddling in the world. It is not by exposure that men are either strengthened or rendered hardy; they must be strong and hardy, before they are fit to be exposed; they must be seasoned first, and exposed afterwards. If we cannot season a piece of timber by sudden, or extreme, or unregulated exposure, we must not think we

can do so with a living man, or a living anything; and from what I have myself seen, and from what I have learned from those who have had the most ample means of judging, soldiers are as sensible of discomfort, and as liable to injury from undue exposure, as any other class of men.

Another important reason why proper gymnasia are essential is, that they may be fitted up with apparatus of a character and description which could not be attempted out of doors. In elaborating my system of bodily training, I have found it necessary to invent many new machines to yield the special form of exercise which I desired in order to produce certain results in the learner; and almost all those which may be called of an educational character, and have for exclusive object to cultivate the resources of the body, require the roof and walls for support, and the soft floor of the building for safety; and those of an entirely opposite description, which have for special and direct object the teaching of the soldier to surmount obstacles, such as the scaling of walls, traversing beams and platforms, with and without implements and arms, can be erected, and the exercises practised on them, with much greater facility here than in the open air; for every portion of the interior face of the walls, and every part of the internal roof may be utilised for purposes of this kind, turned to immediate account, and made to serve as *bonâ fide* apparatus.

I say nothing of the facilities which a school presents over the open ground for giving and receiving instruction, and of preserving order and propriety among the learners, without having recourse to strict military discipline. For it is quite essential to safety, as well as to advancement, that the strictest order and propriety should be preserved in the gymnasium, at the same time that it is most desirable that formal military discipline should be relaxed during the lesson; and there is no fact more undoubted than this, that amongst gymnastic apparatus, the disposition of the learner to be inattentive and careless, and the difficulty of the instructor in communicating instruction and maintaining order, is uniformly beyond comparison greater in the open air than in the gymnastic school. Neither do I notice the safety arising from the assured condition of the apparatus in the gymnasium, exposed as it is to no atmospheric influence, always dry and always clean. Indeed while the out-of-door apparatus must necessarily be getting worse and worse, the indoor apparatus, if properly constructed, and its materials suitably selected, should be virtually indestructible. Nor do I dwell upon the advantages to the individual soldier, as well as to the service, which the building presents, of utilising bad weather—the very time when the soldier's frame most requires activity to sustain its power—the very time when his professional duties are necessarily suspended: and the equal, if not even greater advantage which it gives of utilising the long winter evenings, thus devoting to a source of health and strength, a leisure which is too liable to be spent in idleness and unworthy indulgence.

I have thus noted a few, and only a few, of the advantages which the gymnasium possesses over the gymnastic ground, but each one of these is most important in itself, and is pregnant with many others.

I would now inquire, are there any advantages on the opposite side, any advantages which the ground has over the building? And, if so, are they of such importance as in any way to counterbalance those which I have just enumerated? These are questions which I have fully considered, and I am prepared to answer, None—not one;—the open-air practice has not one real advantage; it has not even any apparent one which may not be shown to have a reverse influence and bearing.

Its apparent advantages are,—1st, ample space; 2nd, abundance of light; 3rd, pure air.

With the phrase "out of doors" and "open air," we are led to associate ample space, but this signification is often quite illusory when applied to the present subject; for it is found that the very places where gymnastic exercises are most wanted, where they would prove the greatest boon, are precisely those where ground is scarcely obtainable at any price or for any purpose, namely, in garrison towns and in barracks situated in closely populated districts. In such cases the advantage, of course, is all in favour of the building—in favour of the method which knows how to economise every square foot of ground, and to make it serviceable all day and every day, morning, noon, and night, wet or dry.

A small nook in a barrack yard, 80 feet by 40 feet, will be sufficient for a gymnasium, which will give abundant accommodation throughout the year to a garrison of a thousand men. They require no more ground than the apparatus covers; and they could use no more were it planted in the middle of the widest common in England.

The question of light, when examined, is solved at once; and I have no hesitation in saying that it is in favour of the building; abundant light for any purpose can be admitted into any building; and in the gymnasium it can be so admitted and so distributed, as to meet precisely the special wants of the special exercises.

The question of pure air is less easily disposed of, for there is nothing more essential to health and to health-giving exercise than pure air; while there is, on the other hand, nothing more liable to deterioration, for every breath we breathe acts injuriously upon it, —subtracts from it some portion of the good which it possesses, and imparts to it that which is pernicious; and it must be admitted that this process of deterioration is only sustained where the air is confined around the breather in a building, and is entirely avoided when he stands in the great air ocean out of doors.

Moreover it must never be forgotten that a gymnasium is a veritable temple to health in the highest sense of the word; and pure air, which is desirable everywhere, is imperatively necessary here, absolutely essential during exercise, not only for the perfect aëration of the blood, but as the natural stimulant to physical exertion.

And again, just in the same ratio with the requirement for the purity of the air in a gymnasium, is its liability to deterioration, first by the doubly increased respiration of the inmates—each breath being larger in volume, and each following each, in quicker succession during exercise than when the body is wholly or comparatively at rest; and

secondly, in a great degree also, by the exudations from the skin, which is stimulated to its utmost activity by the powerful and sustained muscular exertion; and it must be remembered also, that while these exudations are increased to their utmost extent by the energy and freedom of the partly denuded body, its escape into the surrounding atmosphere is also by the same means facilitated. Now while these are all incalculable advantages to the individual, and the very source and secret of the health and strength which he derives from exercise, they all tend directly and powerfully to deteriorate the air.

Another point still. Admitting that the gymnasium is occupied to the estimated extent of its working capacity, at the end of an hour (the usual time allotted to an ordinary lesson), the air in the building, were there not an unceasing interchange taking place between it and the external air, would be so deteriorated as to be rendered less suitable to the use, and less pleasant to the sense of the next batch of men, and less and less so with every succeeding one; for it is contemplated that every gymnasium shall be in full operation for many consecutive hours in each day.

Here, then, we encounter these important facts, not only that pure air is essential to health, and to the pleasurable sustentation of active bodily exertion; but that this exertion itself is a powerful agent in its deterioration, and that this deterioration is only felt where the same air has to be inspired and re-inspired, as in a building.

Now as this necessarily applies to all buildings, though not to all in an equal degree, we may be sure there is a way by which this evil can be avoided, for He who planned our existence did so with the full comprehension of our wants,—saw that while we were so constituted as to require the shelter and protection of dwellings, in which to live, and learn, and toil, that these very buildings expose to deterioration the substance on which we depend for momentary existence. The difficulty that seemed insurmountable is at once overcome by the action of the law regulating the constitution of the air itself. On the slightest change in the constituents of the air, such as that caused by respiration, or by the elevation of its temperature, it is impelled to instant motion—forced to shift and change its place, that place being immediately occupied by the surrounding air, so that motion and change of position is induced, proportionate in force and in extent to the primary displacement. This law is in unceasing and unerring operation over the whole surface of the globe, regulating equally the mighty currents caused by the sun's heat on the belt of the tropics, and the slight undulations in an ordinary room, caused by the breath of a solitary inmate. This law is the key to all our rational systems of ventilation. It teaches us to construct our dwellings in such a manner that the air, which is in unceasing motion—a mighty current ever flowing, though changing its direction with proverbial inconstancy—may pass through them in its course, cleansing them of every impurity. It teaches us so to regulate the admission of this current, that at any time, and at any season, it shall be in accordance always with the wants and the wishes of the inmates.

The special mode of ventilation for a gymnasium I conceive to be—the building must be so constructed that the whole body of air within it may in a few minutes be changed for an equal body of fresh air, for this will be required at frequent intervals. It must be so constructed that the deteriorated air may ascend and pass at once out of the building, and a fresh supply at the same time enter; and these apertures for ingress and egress must be so placed, that the fresh air may be admitted at such distance from the inmates that it shall not strike them in compact cold currents, or draughts, as they are called, but be uniformly diffused; for this must be maintained throughout the working day. It must be so constructed, in fine, that it may be as snug as an ordinary room in winter, and as free and airy as the open heath in summer. And this should be done by what may be called *natural* means; that is to say, by simply bringing the internal and external air face to face, as it were, passing through no intermediate channel, forced together by no artificial means.

Thus much for the purity of the air; but air has other qualities besides purity, for we live in it as well as breathe it—it comes in contact with our skin as well as with our lungs; temperature, therefore, is very important. The air may be quite pure when the thermometer stands at 20 degrees below freezing point, as we have experienced this winter, or rises to 80 in the shade, as we felt last summer; but neither of these conditions are favourable to exercise, and least of all to gymnastic exercise.

Now, in a properly constructed gymnasium, the temperature of the air may be rendered pleasantly cool in summer, and sufficiently warm in winter to let men freely strip for active exercise. We do not want a heated or rarefied air to breathe during active exercise; men have the materials of heat within themselves, and want but exercise to ignite the fuel and sustain the fire; but we *do* want the temperature of the air so raised as to remove actual discomfort in stripping to work, and to dry and keep dry the apparatus, which must come in constant contact with the naked hand, and this can only be done in the properly organised gymnasium.*

Having thus, as I trust, shown the advantages of a properly constructed gymnasium, I will now notice a few of the principles which guided me in preparing a design for a building suitable to the system.

The first of these was, that the gymnasium should be of that form and manner of construction, as to admit of the erection of the desired system of apparatus, and to present the greatest facilities for instruction and supervision. The second, that it should be constructed to meet, in the best manner, the sanitary requirements which I have just noticed.

The third, that in all important respects, the same design should be capable of reduction or extension, so that its working capacity might be in proportion to the garrison where it was erected. In this last respect it was not necessary to compute the absolute working space

* For this purpose there is nothing so good as the open fire-grate, distributed in suitable places in the different divisions of the building.—A. M.

required for every squad of learners, but to ascertain the smallest dimensions of a building which would contain with adequate freedom a fair selection of the essential apparatus in the different sections of the system, and from this minimum size to ascend by carefully regulated gradations to a maximum size; the minimum to be adequate to the requirements of a station with half a battalion of men; the maximum to that of our larger stations, such as Chatham. I mention Chatham, because at that station I can perceive, not one only, but all the different classes of men, and each of these in large numbers, to whom the gymnasium must be the greatest possible boon. First, the recruit, in large numbers, this being the depôt of several regiments of different arms of the service; next the trained soldier in considerable numbers, this being a station where several regiments are quartered; and, in consequence, many young officers, for whom it is most desirable to provide suitable and attractive means of healthy and manly exercise.

Indeed, all the features of all other gymnasia should be combined in a gymnasium at a station of this extent and character, and the one recently erected there, and of which a model is before you on the table (see Plate XVIII), in my opinion does so. Nay, all departures from the plan of this gymnasium, should be but modifications of it, to meet some local or exceptional want.

The sanitary requirements of lighting and ventilation have been provided for on the most abundant scale, and as the official regulations on this point have been well considered and clearly defined, there is no room for failure with the most ordinary care on the part of those to whom this special duty is entrusted.

In preparing a system which aimed at providing means for the bodily training of the recruit and for the trained soldier, for the officer of matured frame, and for the yet growing cadet—a system which should yield not only adequate, but suitable and attractive exercise to all of these—it was necessary that it should be very extensive and wide-embracing; that it should be markedly progressive, and above all, that the early courses should be of such a character as to accomplish the setting up, the putting into proper position, and the correction of the evils in gait, action, and attitude of the recruit; that his whole body, and his whole body equally, should receive employment, and that the employment should be of the kind which would at the same time cultivate the contractility of the muscles, and the mobility of the joints; and also promote the expansion of the chest, and give that stimulus to the organs of respiration and circulation which is best calculated to increase their functional vigour.

For these important objects, and for the reasons explained in the book of the system,* I have discarded all exercises of mere movement and position, as affording inadequate action to the muscles, while giving extreme effort to the joints, as being equally severe or inadequate to weak and to strong, as admitting of no progressive advance-

* "A Military System of Gymnastic Exercises, for the use of Instructors." By Archibald Maclaren, Adjutant-General's Office. February, 1862.

ment, and as being inattractive and uninteresting, irksome, indeed, to all; and for my introductory course I have adopted a series of exercises with what I have called *moveable apparatus*, that is, with dumb-bells, single and double-handed. This lesson is so organised, that almost any number of learners may take part in it at a time, all acting by the same word of command.

The whole exercises of the system thus resolve themselves into two distinct kinds: first, those with moveable apparatus, and used for the first or introductory course, in which the learner lifts or wields the article of apparatus, he himself standing firm; and, secondly, those on the fixed apparatus, comprising the bulk of the system, where the learner himself moves or turns, the apparatus or machine being fixed or firm.

This distinction is very important, not only as affecting the character of the exercise to be performed, but the whole material means used in performing it, and actually necessitates a distinct division of the building itself into two parts, each part being in certain respects, in appearance and in fact, the very antithesis of the other. In the first division, that for the moveable apparatus, the floor is retained perfectly free and firm; the apparatus, when not in use, being ranged in racks along the walls. In the second division, the floor is made of soft and elastic material, with every spot of its surface mapped out and studded with apparatus permanently fixed.

For the first division there was needed simply a large room, but as I had long been impressed with the fact that at the depôts, to which our recruits are sent for the exclusive purpose of drill, there was no place where this drill could be carried on in inclement weather, and keeping in view always that the gymnasium should be the place for the instruction and practice of all professional bodily exercises, I so prepared this portion of my design, that at our depôts it might also serve, when desired, for the setting up and position drill of the recruit, and also as a school of arms, where fencing, sword exercise, and bayonet exercise might be taught and practised. One of these exercises which I have just named, fencing, is eminently suitable to officers, and yet it is one in which the officers of our army, generally speaking, are greatly deficient. I am convinced that they require but fair opportunities of instruction and practice to follow it earnestly and successfully, and I have therefore made special provision for the practice of this exercise, by a large fencing school, carefully planned and fitted up for this purpose, with suitable dressing-room accommodation.

I have already stated that in elaborating my system of bodily training, I have found it necessary to invent many new machines to yield the special form of exercise which I desired, in order to produce certain results in the learner; for instance, I have found that few men are equally developed on both sides of the body, as a natural result of the greater employment given to the right side during the period of growth, and this applies not only to the arm, but to the whole side, from shoulder to hip, and not unfrequently including also the lower limb, when the development of the right leg and foot preponderates over the left. Now I consider it of the greatest importance to health, as

it undoubtedly is to serviceability, that this lost balance of power should be restored, this equilibrium of development re-adjusted; and to accomplish this I have found it necessary to invent a number of machines, for the sake of the form of exercise which I desired to be performed on them. Among the first of these I would mention the elastic ladder and row of rings, machines designed expressly to give employment to both sides of the body equally, and especially to the chest and upper limbs, by necessitating that both sides of the body shall perform the same duty, requiring the exertion of the same degree of effort, and that neither side shall be able to aid the other; each side must do its own share of work, and can do its own share only; and therefore if the weaker side be doing as much as the stronger, it will virtually be doing more (being weaker), and the amount of difference in exertion will be of course in relation to the amount of difference in development or power. And therefore, the unerring result of the natural law of development being, *cæteris paribus*, in relation to activity, the weaker side will ultimately recover its lost position and its fitness for fair companionship with its fellow.

I have mentioned the nature and object of these two machines, for the two-fold purpose—1st, of showing that it was what I had discovered to be the actual wants of learners or pupils which guided me in preparing my exercises, and in inventing apparatus which would yield the form of exercise desired; and, 2nd, of showing that it was the form of that apparatus and the nature of the exercises to be performed on them which determined the form and construction of the building itself.

For instance, an important section of apparatus is that which teaches the soldier to clear objects by running, vaulting, and leaping; therefore for these considerable length is required. A second section of an elementary character, as the horizontal bar and the two machines which I have just described,—machines all capable of being worked by large numbers of men at the same time, and by the same word of command, also requires length; therefore the oblong shape, which admits of the apparatus of these important sections being arranged side by side, has been chosen for the gymnasium. A third section, consisting of all climbing apparatus, whether mast, rope, or pole, requires *height;* but as these are all vertically placed, they may be closely grouped, so that a small portion only of the building needs to be very lofty. These three sections of the apparatus in a very clear manner determine the most suitable and serviceable form of building, namely, an oblong, of a breadth about half the required length, and with one portion of it lofty. The position of this lofty portion naturally falls to the centre; for the two end walls are utilised for escalading, the one bearing every form of vertical apparatus; the other every form of slanting apparatus, connected by narrow platforms running along the tiebeams of the roof, in order to accustom men to traverse narrow objects at a considerable distance from the ground. I have utilised the elevated portion farther by making it serve in lighting and ventilating the gymnasium in the place where light is most wanted, and ventilation, of one kind, may be most effectively obtained.

The same form I found also to be the most suitable for the other division of the building, for the distribution of squads in the introductory course of gymnastics, for their arrangement during drill, and for classes of fencing and sword exercise. And as it was desirable for purposes of supervision and instruction that both divisions should form one building, and that every portion of it should be overlooked from every other portion, these two divisions have been placed rectangular to each other.

I have only now to notice the galleries for spectators. It is most desirable to encourage visitors to the gymnasium; it is wonderful, sometimes, how the presence of visitors serves to stimulate the learners to energetic action, and at the same time to assist in preserving the proper decorum of the lesson; but it is equally desirable that they should not mix among or in any way interfere with the learners. The galleries for spectators in both divisions are so arranged that they overlook the whole gymnasium without encroaching upon the working space.

I have already, on several occasions, laid before the public some of the results of this system of bodily training; first, when divested of its military bearing, in connection with the youths of Oxford, with whom the practice is necessarily voluntary, and liable to every form of interruption; and yet such is the want of systematised exercise with growing lads—such are the innate and latent resources of the human body, waiting but the opportunity of development, and such is the power of this system to draw forth these resources, that I have been able to establish beyond controversy the fact, that every youth who comes up to the University, whatever may have been his previous health and habits, has a large arrear of power still undeveloped, and of which a considerable instalment may be almost immediately obtained.

In a paper which I had the honour to read here two years ago, I was able to show that in a detachment of twelve non-commissioned officers, some of them of many years' service, the results of the system were as satisfactory as upon the young Oxford undergraduate. The men composing the detachment had been irregularly selected, the youngest being 19, the eldest 28, the average age 24; and after a period of eight months' training, the increase in the measurements of the men were—

	Weight.	Chest.	Fore arm.	Upper arm.
	Pounds.	Inches.	Inches.	Inches.
The smallest gain ..	5	1	½	1
The largest gain ..	16	5	1¼	1¾
The average gain ..	10	2⅝	¾	1⅜

With a second detachment, in which the men were of more uniform powers, the youngest being 22, the eldest 26, and the average 24¼, the increase in the measurements, after a similar period of instruction, were*—

	Weight.	Chest.	Fore arm.	Upper arm.
	Pounds.	Inches.	Inches.	Inches.
The smallest gain ..	1¼	2	¼	½
The largest gain ..	7	4¼	1¼	2
The average gain ..	2¼	3	½	1¼

Within the last two years, the system has been so far extended into the army, that seven large gymnasia have been organized at important stations,† and are now in full operation, giving an ample bodily training to several thousand men; and I am enabled to state, that the returns of measurements on this wide scale confirm the results of the experiment made on small detachments.

It was wisely determined from the first adoption of the system by the Authorities, that the cadets at Woolwich and Sandhurst should have an early opportunity of sharing in its advantages, for whatever may be the value of a powerful and active frame to the soldier, it could be no less to the officer; and the advance in bodily power made by the cadets has fully equalled my anticipations.

I am indebted to Major Hammersley, the officer to whom the direction of the gymnastic training in the army has been entrusted, for a tabular statement of the measurements of a class of cadets at the Royal Military Academy at Woolwich, the youngest 16, the eldest 19, the average age being 17¾. In this class, in a course extending over four months, the increase was‡—

	Weight.	Chest.	Fore arm.	Upper arm.
	Pounds.	Inches.	Inches.	Inches.
The smallest gain ..	1	½	½	¾
The largest gain ..	8	5¼	½	1½
The average gain ..	1¾	2½	1/16	1

* Vide Appendix.

† The stations where gymnasia are already erected are, Aldershot, Woolwich, Warley, Parkhurst, St. John's Wood London, Chatham, Royal Military Academy, Woolwich, and Royal Military College, Sandhurst.—A. M.

‡ Vide Appendix.

Again, in another form still I have tested the results of the system, not by brief courses or periods of voluntary attendance, but by a year's steady practice from birthday to birthday, with two articled pupils, the younger being 16, the elder 20. In the year's work the increase was—

	Weight.	Chest.	Fore arm.	Upper arm.
	Pounds.	Inches.	Inches.	Inches.
With the younger ..	21	5	2	2
With the elder	8½	6	1¼	1½

I need not make any remark upon these figures. I can make none better than every gentleman who hears me now can make for himself. They simply record a complete transformation of the learner's frame; so complete, as to defy all comparison between it and that he possessed previous to the practice. And it must be remembered that this is not an acquisition to be retained only while following some exceptional course of life. It is what he can never wholly lose in this world again, because it is himself that is altered, it is himself that is changed—altered and changed only by being brought so much nearer to the power which he was designed to possess, and to the place which he was designed to occupy from his birth; a place from which he can never wholly fall, a power of which he can never be wholly dispossessed. Not even by illness can it be taken away; for, as his state was his legitimate state, and his power was a natural power, from illness he will rise again towards the height from which he fell.

In conclusion I would desire to draw your attention to this fact —a fact which all the tests of the system, and all the observations of my life go to establish, namely, that the advantages to be derived from physical training are all in favour of the *youth* of the learner. Get the man while he is young, get him, if possible, while he is growing; every year and every month of his growing time is worth double and quadruple the number of months or years after growth is completed; not only for giving shape and carriage, and activity, and muscular strength, but for giving that stamina of endurance to the whole frame, and that healthy and energetic action to the organs which, so to speak, carry on the whole work of life within the frame, which alone will make the man a man indeed, and enable him really to perform the duties of a soldier.

APPENDIX.

Tabular Statement of Measurements of the Second Detachment of Non-commissioned Officers selected to be qualified as Military Gymnastic Instructors,

FROM OCTOBER 27TH, 1862, TO JULY 12TH, 1863.

No.	NAME.	MEASUREMENTS, &c.								INCREASE.			
		Age.	Height.		Weight.		Chest.	Fore arm.	Upper arm.	Weight.	Chest.	Fore arm.	Upper arm.
		Years.	Ft.	In.	Stone	lbs.	Inches.	Inches.	Inches.	lbs.	Inches.	Inches.	Inches.
1	Corporal Mellor, 22nd Regiment ..	22	5	10½	9	13	34	10	10¾				
					10	6	38¼	11¼	12¾	7	4¼	1¼	2
2	,, Pike, 22nd Regiment	23	5	9½	12	0	36	10¾	12½	Less			
					11	11	39¼	12	14	3	3¼	1¼	1½
3	Sergeant Wilcox, Coldstream Guards ..	24	5	10½	11	2¼	36¾	10¾	12				
					11	4	39½	11½	13½	1¾	3¼	1½	1½
4	,, Griffiths, Royal Artillery ..	25	5	9¼	10	6	36½	11¼	12½	Less			
					10	4	38½	11½	12¾	2	2	¼	¼
5	,, Moss, Grenadier Guards ..	25	5	9½	9	12	34¼	10	11½				
					10	1	37¾	10¾	12½	3	3½	¾	1½
6	,, Adams, Scots Fusiliers	25	5	9¼	11	2¾	38	10¾	12				
					11	4	40½	11½	13	1¼	2½	½	1
7	,, Richards, Royal Artillery ..	25	5	11½	12	4	36½	11¼	12½	Less			
					11	9	39	12	13½	9	2½	¾	1
8	Corporal Pentecost, Royal Horse Artillery	25	5	9½	11	13	38	11¼	13				
					12	5	41½	11¾	14	6	3½	½	1
9	,, Pullin, Royal Artillery	25	5	8½	11	0	35	10¾	12¾	Less			
					10	11	38½	11¾	13½	3	3½	½	¾
10	Sergeant Elliott, Grenadier Guards ..	28	5	10¼	12	3	38¼	10½	12				
					12	7	40½	12	14	4	2¼	1½	2

Return of Course of Gymnastic Training at the Royal Military Academy, Woolwich,

FROM FEBRUARY 10TH, 1863, TO JUNE 22ND, 1863.

No.	MEASUREMENTS, &c.						INCREASE.				
	Age.	Height.	Weight.	Chest.	Fore arm.	Upper arm.	Height	Weight.	Chest.	Fore arm.	Upper arm.
	Yrs.	Ft. In.	St. lbs.	Inches.	Inches.	Inches.	Inches.	lbs.	Inches.	Inches.	Inches.
1	18	5 1⅜	7 8	29½	9½	8½					
		5 2⅜	7 8	30	9½	9½	1	....	½	....	⅝
2	19	5 8½	9 5½	28	11	10⅜					
		5 8⅞	9 11	31½	11	11⅝	⅜	5½	3½	....	1¼
3	17	5 5⅝	9 1	26½		8½					
		5 6¼	9 1	29½	10⅝	10	⅝	....	3	....	1½
4	18	5 8½	10 0	33	10⅝	10⅜					
		5 8½	10 3	35	10⅝	11½	⅜	3	2	....	1⅛
5	18	6 0½	10 13	32	10½	9⅜					
		6 1⅜	11 2	34	10½	10⅞	⅞	3	2	....	1½
6	17	5 3½	8 1	31	10¼	9½					
		5 4½	8 7	33	10¼	11	1	6	2	....	1½
7	18	5 5⅜	7 13	26	9½	7½					
		5 5⅞	8 2	29	9½	9⅜	½	3	3	⅜	1⅞
8	16	5 6⅞	8 3	28½	9	8½					
		5 7⅜	8 4	31	9½	9½	½	1	2½	½	1
9	17	5 8⅝	11 3	31	11⅝	10⅞					
		5 9½	11 3	33	11⅝	11⅛	⅞	....	2	....	¼
10	18	5 11½	11 8	30	10⅝	10½					
		5 11½	11 8	33	10⅝	11	....	....	3	½	½
11	19	5 7⅝	10 2	33	10½	10⅜					
		5 8⅜	10 2	34½	10½	10⅞	⅞	....	1½	....	½
12	18	5 10⅜	10 11	32	10½	10					
		5 11⅞	10 11	33½	10½	11	1½	....	1½	....	1
13	19	5 7½	11 13	33	11½	12					
		5 9⅜	11 13	35½	11½	12½	1⅞	....	2½	....	½
14	17	5 6⅝	9 13	29	10½	8⅝					
		5 7½	10 3	32	10½	9½	⅞	4	3	....	1½
15	19	5 10½	10 1	27½	10¼	9⅞					
		5 11⅞	10 9	32⅝	10¼	10⅞	1⅜	8	5⅛	....	1⅛
16	18	5 3⅜	8 13	29	10¼	10½					
		5 3⅞	8 13	32	10¼	10⅞	½	....	3	....	⅜
17	18	5 8⅝	11 8	33	11½	12⅛					
		5 9½	11 8	34½	11½	12⅛	⅞	....	1½	....	....
18	17	5 6½	9 8	27		8⅝					
		5 7½	9 8	30½	10⅝	10	1	....	3½	....	1⅜
19	16	5 6⅛	8 10	27⅝	9½	7½					
		5 6½	9 1	30½	9½	9½	⅜	5	2⅞	⅜	2
20	18	5 7	9 1	28⅝	10	9½					
		5 7½	9 1	31	10	10½	½	....	2⅜	....	1
21	18	6 1½	11 12	34⅝		11⅞					
		6 2	11 12	35⅞	11	12¼	½	....	1¼	....	⅝

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ON THE

COMPARATIVE ANTHROPOLOGY

OF

ENGLAND AND WALES.

BY

D. MACKINTOSH, F.G.S.,
ETC., ETC.

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PREFACE.

For upwards of twenty years, the author has been principally occupied in lecturing and making geological observations. He has visited and re-visited almost every part of England, and a considerable part of Wales. The same line of observation and generalisation pursued in geology, soon led him to notice, and inquire into the causes of, the peculiarities presented by the predominating inhabitants of different towns and districts. The paper he read on this subject before the Ethnological Society in 1861, when Dr. Hunt was secretary, and J. Crawfurd, Esq., chairman, was very favourably noticed in many of the public journals; and in the *Illustrated London News*, April 20, of the same year, an abstract appeared, with a number of engravings. A part of the *following* memoir was read before the British Association at Birmingham, in 1865. The whole appeared in the *Anthropological Review* of January 1866, and is now separately published, with very few alterations.

The author will feel happy in being corrected by critics on the subject of coupling ancient historical names with existing types or races; but he hopes that those who may question his facts, will be able to state that they have had equally favourable opportunities for observation. If he has unintentionally exaggerated in some instances, he is conscious that in others he has rather underestimated the extent of the peculiarities by which types are distinguished.

The map is a revision and modification of Dr. Kombst's Ethnographic Map, in Johnstone's *Physical Atlas*. The portraits, for obvious reasons, are notquite so satisfactory as might have been wished; and the reader is requested to rely on them only in connection with the description in the text. Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, are sufficiently correct. In 6, the eyes are too distinct and prominent, and the lower part of the face too full. Figs. 12, 13, 14, do not show a sufficiently projecting mouth. Figs. 15 and 16 are somewhat flattered, but they are not regarded as specimens of distinct types. The author attaches little importance to 17 and 18. In 19 and 20, the faces ought to have been broader, and still more continuously curvilinear. Fig. 21 is accurate, but in 22 the nose ought to have been higher and finer at the tip. The face in 23 is a little too smooth. Fig. 21, 25, and 27, are tolerable; in 26 the face ought to have been longer, and the cheek-bones more projecting. In 28 the eyebrows are too semicircular, and the nose rather too prominent.

D. M.

Chichester, Feb. 1866.

P.S. Since the appearance of the article in the *Anthropological Review*, I have seen an eminent Welsh scholar and antiquarian,* who is willing to admit the correctness of my classification of the inhabitants of the principality; but he wishes that a less general term than *British* could be applied to my second or short-headed and comparatively flat-faced Welsh type, which he believes must have come into this country *after* the Gael or Gwyddel. As I have made a few allusions to Ireland, I may here express my belief that Gaels are not more numerous in the sister island than in Britain, and that the middle and higher classes of Ireland are in general descended from races occupying a high rank in the ethnographical scale.

* J. Davies, Esq., Hereford.

COMPARATIVE ANTHROPOLOGY OF ENGLAND AND WALES.*

By D. MACKINTOSH, F.G.S.

> "Let some Fellow also do for England what M. Paul Broca has done so well for France, and write us a Memoir on the Ethnology of England."—DR. JAMES HUNT, *Anniversary Address before the Anthropological Society of London*, delivered January, 1864.

IN 1861 I read a paper before the Ethnological Society of London, entiled "Results of Ethnological Observations made during the last ten years in England and Wales." Up to that time ethnology had generally been treated as a branch of philology, archæology, or history. It could not be said to have had an independent foundation, or to have acquired the rank of a distinct department of science. Many, perhaps the majority, of those calling themselves ethnologists did not believe in ethnology according to the most approved and authoritative meaning attached to that word, namely the science of blood, or races of mankind resulting from genealogical descent. The attempt to classify races in Europe, and especially in England, was then generally looked upon as presumptuous, or, at least, as not likely to lead to a satisfactory result. In the discussion which followed the reading of the above paper, one of the Fellows considered the attempt as dangerous, by which I suppose he could only mean dangerous to preconceived theories. Several of the speakers favoured the views of the author, but the majority seemed to agree in thinking that the races described in the paper as occurring in England and Wales were not due to lineal descent from tribes of early inhabitants, but either arose by accident

* We propose to publish, from time to time, a series of personal observations on the Comparative Anthropology of the British Islands.—EDITOR.

or according to a law by which human beings become adapted to circumstances or occupations. It was likewise alleged that to substantiate the doctrine of genealogical derivation would require the discovery of counterpart races in those districts of Europe from which England was colonised.

As there would still appear to be a great indisposition to believe that distinct, hereditary, and long-persistent races or types can be traced in different districts of England, it may be necessary, before proceeding to a statement of facts, to make a few general observations.

Alleged Disappearance of Types by Crossing.—It is not to be wondered at that those who have had few opportunities of making particular and repeated* observations in different parts of England, should doubt the possibility of *types*† of mankind being perpetuated, more especially as we are continually reminded by the newspaper press of migrations taking place from one town or province to another. Previously to travelling, or as long as we are contented with being library anthropologists, we are likely to be left in ignorance of the extent to which the masses of the English population still cling to their native districts. Internal migration in England is generally limited to the middle or more affluent classes. The great bulk of the people very seldom shift their localities, except in manufacturing districts, and even then it could be shown that at least three-fourths of the inhabitants of a manufacturing town, such as Sheffield, have either been born in the town or have come from the neighbourhood. Railways in many respects have favoured migration, but it could be shown that in quite as many cases they have rendered a change of residence unnecessary. But the fact that different dialects still linger in different parts of England is a sufficient proof that the interblending of races has not proceeded to an extent capable of destroying typical distinctions, or rendering the classification of the inhabitants impossible. The uneducated natives of one anthropological area‡ are still nearly unintelligible to those of another area. In one area at least nineteen-twentieths of the people still say *we* for *us*, *her* for *she*, *I* for *me*, and *vice versa*. They likewise pronounce s as if written z, t as d, etc.§ This area includes a part of

* If *repeated* observations are necessary in geology to insure an arrival at truth, they are still more so in anthropology—a science in which the phenomena are much less strongly marked, and the boundary lines less distinctly defined.

† I shall principally use the word *types* in this article, because in an infant science, like anthropology, more systematic names are premature.

‡ A district, without reference to county divisions.

§ These modes of speech are used not by one race, but by several races, who must have come from the Low Countries, at a period or periods unrecorded in history. National and British school education, I have found, has

Dorsetshire and Wiltshire, nearly the whole of Somersetshire (Zomerzetzhire) and a part of Devon. In a churchyard between Salisbury and Wilton, I have seen the following epitaph :—

> "How strangely fond of life poor mortals be ;
> How few who see our beds would change with *we*," etc.

The traditional characteristic epitaph of the above area would appear to be—

> "Her no more shall come to *we*,
> But us must go to *she*."

The remark of a working man of Dorchester, in reference to a scolding wife, shows that these peculiar modes of speech are not incompatible with sound philosophy—"It pleases *she*, and it don't hurt *I*."

Proofs of Typical Perpetuation furnished by Surnames.—Besides dialects, surnames show that the people of many parts of England have escaped interblending. In one area we find prevailing surnames ; in other areas these surnames are almost entirely absent. There are large districts in the south-west of England where one might travel for days without meeting with a Smith, while in the east of England there are equally large districts in which Smith is the most common name. A long article, elaborated from Directories, might be written on the local limitation of surnames. Christian names are more uniformly distributed, though I think it will be found on inquiry that in the north-east or Scandinavian part of England there is a very much less tendency to use Scripture names than in the south, where in some places it amounts to little short of a propensity. Some years ago (and it may be so still) the name of the Librarian of the Ryde Literary and Scientific Institution was Nebuchadnezzar Belshazzar Pentecost !

Presumptions in favour of Genealogical Derivation.—That the difference in type or race which, during many years, I have had opportunities of tracing in various parts of England, is not the result of accident, or of a merely *teleological law*, but exists through *hereditary descent*, is rendered highly probable, in the absence of more satisfactory evidence, by the fact that distinct dialects are often, if not generally, spoken by races having distinct physical and mental peculiarities—that these races inhabit areas colonised from certain parts of Europe—and that these dialects (except where reasons to the contrary can be assigned) are in accordance with the historical account of their derivation. A whole article, or rather volume, might be written on this

done very little to obliterate peculiarities of dialect among the working classes, partly owing to the time at school being too brief to admit of a permanent impression being produced ; but likewise owing to the *high-pressure* system generating a dislike to education among children, who, on leaving school, gladly forget what they have been taught.

subject, and much has been written. Suffice it at present to remind the reader that in Cumberland, Westmoreland, and several neighbouring districts, many traces of *Norse* may be found,* and many family names are Norwegian. In Lincolnshire, many words in the dialect, and many family names are not only of Danish derivation, but in numerous cases the latter have continued unaltered in the spelling since the time of the Scandinavian invasions.† Now if the names of persons in use among ancient *colonising* tribes are still to be found in the *colonised* districts, is it not probable that the physical and mental peculiarities of these tribes have likewise persisted? or rather, is the anthropologist not justified in taking this for granted until the contrary can be shown.

How Types are to be determined.—Admitting the force of the foregoing remarks, and allowing that types may be classified in various districts, the important question still remains, what names are we to employ? If only one uniform type existed in a given locality, the task would be easy. But when in most districts (not all) we find two or more distinguishable types, how are we to tell which is Danish, which Saxon, etc.? It is here that the anthropologist may readily lay himself open to a charge of presumption, unless he proceeds with extreme caution. There would, however, appear to be several ways of arriving at approximately satisfactory conclusions on this subject.

First, we may compare the existing mental and (as far as possible) physical peculiarities of a given type with the historically-recorded character of either the original type, or colonising type, of the locality.

Second, we may collect traditions concerning the complexion, stature, etc., of certain types.

Third, we may visit regions, or rely on the accounts of those who have visited regions, either in the British Isles or on the continent, where we have reason to believe a given type prevails uniformly, or is very decidedly predominant.

With regard to the first, it is desirable that the anthropologist should render himself well acquainted with the character of the ancient Saxon, Dane, etc., as illustrated in such books as Bulwer's *Harold, the last of the Saxon Kings*, Mallet's *Northern Antiquities*, etc.

* Since the publication of Worsaae's very valuable contribution to anthropology, *Traces of the Danes and Norwegians in England*, etc., it has become more and more customary to refer words commonly regarded as Saxon to Norse, or Danish. Capt. Fergusson, President of the Carlisle Mechanics' Institution, has lately published an important work on the dialects of Cumberland.

† Of this I was assured some years ago by the very eminent, though not professed, anthropologist, Sir E. B. Lytton, several of whose novels might justly be styled *studies in anthropology*.

Traditions are not always to be trusted, but a traveller is often struck with the extent to which the inhabitants of various parts of England agree in assigning characteristics to ancient colonising or native tribes, such as ruddiness and tall stature to the Danes, blue eyes and lymphatic temperament to the Saxon, dark complexion and excitable temper to the ancient Britons, etc.

Much caution ought to be exercised in selecting regions likely to contain an all-prevalent or preponderating type. It is true one could scarcely err in visiting certain parts of Norway, the Orkney Islands, and some parts of the Hebrides (where Norsemen have kept aloof from the Gaels), in order to make out a type to which the name Norse might be applied—in going to some parts of Denmark (not West or South Jutland) in quest of the Danish type.* For Saxons one might explore the country between the Elbe and the Weser, steering clear of Friesland—for Angles, the district called Anglen in Schleswig, where Dr. Clarke, the traveller, could fancy himself in England. For the Jutian type, the anthropologist might visit the west of Jutland, from Schleswig to the Lime Ffiord—for Frisians, the region commonly called Friesland would probably answer his purpose better than Strandfrisia; for linguistic† considerations render it certain that England was largely colonised from the country to the east of the Zuyder Zee. One might expect to find pure Britons in Wales, and Gaels in the West Highlands of Scotland, though in both these countries the people are far from being homogeneous.

That the lineal descendants of ancient tribes may still be recognised in various parts of England, is not so much doubted by people in general, as by those whose minds are prepossessed by certain theories concerning the origin of admitted typical differences among mankind. The science of comparative anthropology, or that department of it—comparative ethnography, to which this article is mainly confined—is at present in a state somewhat resembling geology in the days of Dr. Hutton and Professor Playfair. These truly great philosophers wisely abjured all *speculations concerning the origin of things.* But when Dr. Hutton used these or similar words, he did not mean to exclude the

* At the British Association meeting at Birmingham in 1865, I was not surprised to see in Professor Steenstrup, the eminent Danish antiquary, a *fac-simile* of a physiognomy very common in the east and north of England.

† For all questions connected with what may be called glossological ethnography, Dr. Latham's works are the best that can be consulted. That eminent author does not seem to place much faith in ethnology as the science of blood; though I ought to acknowledge my obligations to him, many years ago, for leading me to believe that the prominent-mouthed type, so prevalent in the south-west of England, is only a less exaggerated form of the Irish Gael.

origin of *derivative* phenomena, but only what may be appropriately called the *first* origin of things; and although the question of the first origin of man lies more within the province of geology than anthropology, the changes or causes which have given rise to typical distinctions among men may be advantageously considered, before proceeding to a detailed statement of these distinctions as observed in England and Wales.*

Causes of Typical Distinctions.—Mr. Darwin has rendered great service to natural history by showing that a slight variation from an ancestor is capable of continued hereditary transmission. He has, however, I think, generalised beyond foundation in regarding all the modifications to which the organic world has been subjected as slight, or in supposing that species have arisen by almost insensible gradations. In the inorganic world—in the provinces of water and fire, we find gradual mutation alternating with *crises* of action, or a series of ordinary changes followed by a sudden paroxysm. The aqueous and igneous agents which modify the crust of the earth are more or less intermittent. Comparative repose in fluviatile, oceanic, and volcanic action, is succeeded by floods, storms, eruptions, and explosions; and there can be no reason for supposing, apart from palæontological evidences to the contrary, that all the variations from ancestral organic types have been minute, or for denying that "strides in the otherwise continuous chain of succession"† may not have frequently occurred. These minute variations and strides are equally to be regarded as *creations* unless we "deify second causes;" and I can see no reason why the creational act which gives rise to a perceptible *family* variation, may not, at intervals, introduce a *specific* or *generic* variation. A general survey of the higher results of scientific investigation would appear to favour the doctrine that in the economy of the universe there are subsidiary laws dependent on a more comprehensive plan; and the sudden introduction of new species is just what one might expect to mark the ingress or egress of one of these laws.‡

* On the first appearance of his *Principles of Geology*, Sir Charles (then Mr.) Lyell was accused by some reviewers of putting the cart before the horse—of discussing the respective merits of an unimpaired and uniform series of changes, and a succession of catastrophes diminishing in intensity, before proceeding to a statement of facts showing the adequacy of existing causes to account for ancient geological phenomena. But the order adopted by Lyell was the best calculated to prepare the mind of the reader not only to appreciate, but to take an interest in, the mass of circumstantial evidences, or *veræ causæ*, contained in that justly celebrated work.

† See Lyell's *Antiquity of Man*.

‡ I think all anthropologists must admit that no positive evidences in favour of there having been a series of consecutive connecting links between the anthropoid apes and man have yet been discovered. The theory of his anthropoid derivation, then, must rest on the assumption that these links have disappeared, or remain to be discovered—an assumption inadmissible in *inductive* science. From the latest discussions on the Neanderthal skull, it would appear to be allied to *Gaelic*.

But one part of Darwin's theory certainly accounts for anthropological phenomena not otherwise easily explained. In the *Fortnightly Review* (III, 276), Professor Huxley has applied this theory to the origin of typical distinctions among men. Variations occur in a family—one variation dies out, another is preserved. It becomes isolated. By hereditary transmission its peculiarities become hardened into the "enduring character of persistent modification." According to this view, it is not necessary that a type should amount to a *specific* distinction to enable it to be hereditarily transmissible. A variation is possessed of this power, and would seem to be subjected to a law preventing a return to the original. When it has become hardened into a "persistent modification," it may endure for many, if not for thousands of years, as is evident from geology. We have only then to suppose that the types under consideration in this article were originally family variations in certain parts of Europe—that they gradually acquired a persistent character—that they have continued *until now*, and will continue until the law* which limits the period of their perpetuation shall replace them by new variations, destined in their turn to become invested with enduring characteristics.

Among men there would appear to be types which have become sufficiently hardened to resist amalgamation, and even in England many phenomena would seem to indicate that hybridity is followed by extinction or reversion to the original. In some parts, where interblending has occurred to a great extent, we still find distinct types identifiable with those which may be classified in remote and comparatively unmixed districts; and very frequently two or more types may be seen in the same family. In many cases, typical amalgamation does not apparently take place at all, but the children of two parents of distinct types follow or "favour" the one or the other parent, or occasionally some ancestor more or less remote.

We have no reason to suppose that the comparatively brief period, geologically speaking, with which the anthropologist has to deal, is sufficiently long to reveal any processes by which new types are intro-

* I think Mr. Darwin errs in supposing, or allowing his readers to suppose, that variations capable of originating persistent modifications are accidental. We cannot conceive of their giving rise to phenomena which admit of being systematically classified without believing them to form part of a fixed system. See some able remarks on this subject, in the *Anthropological Review*, vol. iii, p. 130.

duced, so that we are justified in classifying the types which come under our notice as if they were unalterably fixed.

During the last fifteen years, I have had occasion to reside successively, and often repeatedly, in most towns of any importance in England and Wales; and I have devoted particular attention to the characteristics of the inhabitants of the surrounding districts. The people of some localities I have not been able to classify at all. In other localities, I have not felt justified in applying historical names to the typical peculiarities of the inhabitants. A description of those types, with their lateral gradations, which I have been able to make out, will form the remaining part of this memoir.

Types in North Wales.—I begin with the Welsh, not because they are really more easily classified, but because the reader will probably be more ready to believe that types may be met with in the Principality than in England.

On arriving in North Wales in 1861, I was not much surprised to find the inhabitants differing from one another, as I had previously observed a similar absence of homogeneity in South Wales. About the same time, Dr. Barnard Davis, and Dr. Beddoe, passed along the north coast on their way to Ireland, and I believe were surprised at the diversity of countenances presented by the Welsh. After a series of systematic observations, continued for several months, I succeeded in reducing the differences to the four following types:—

First, the prevailing type in North Wales, with its lateral gradations, I had an excellent opportunity of observing during a great Calvinistic Methodist gathering at Mold, Flintshire. On that occasion, at least nine-tenths of the adult men and women presented the following characteristics:—stature various, but often tall—neck more or less long—loose gait—dark brown (often very dark) and coarse hair—eyes sunken and ill-defined, with a peculiarly close expression—dark eyelashes and eye-brows—eye-basins more or less wrinkled. The face was long or rather long, narrow or rather narrow, and broadest under the eyes. There was a *sudden sinking in under the cheek-bones*, with denuded cheeks. The chin was rather narrow and generally retreating, though sometimes prominent. The nose was narrow, long or rather long, much raised either in the middle or at the point, and occasionally approaching the Jewish form (see fig. 5). The forehead was rather narrow but not retreating—the skin wrinkled, and either dark or of a dull reddish-brown hue—the skull rather narrow and rather elongated. (See figures 1, 2, 3, 4.)

Second Type in North Wales.—To the west of Mold, comparatively flat faces begin to make their appearance, and increase in number until in Carnarvonshire they are very common. In this type, as in the last,

the face is broadest under the eyes, with *a sudden sinking in under the cheek-bones*. The *nose* is sometimes highest in the middle, but more frequently *projecting at the point*. The eyes are sunk and often half closed. The mouth is well formed, with the chin more or less prominent. The forehead in general is broad, high, and capacious. The stature is short or middle-sized, with broad chest and shoulders—the complexion dark, with brown or dark brown hair—the skull broad and approximately square. (See figures 7, 8, 9.)

This type may be traced in considerable numbers along the western part of Wales as far as Pembrokeshire. It is likewise not unfrequent in Central Wales as far east as Montgomery, and it is very common in the West Midland Counties of England. In many parts of South Wales it predominates.

Third Type in North Wales.—Rather full and massive face—decidedly dark and often curly hair—dark whiskers, eye-brows, eye-lashes, and eyes—tall or rather tall and massive frame—skull approximately round. This type, which may be found in small numbers in both North and South Wales, is generally confined to the more prosperous inhabitants. It is not very dissimilar to a type which in Ireland has been called Milesian. It is not uncommon in Monmouthshire, and may possibly be of *Silurian* derivation. (See figures 10, 11.)

Fourth Type in North Wales.—This type presents a greater or less approximation to what I would call the Gaelic type (see sequel). In some places it is strictly Gaelic; in others it graduates into the first or prominent-nosed Welsh type, or into the comparatively flat-faced Welsh type. About Bangor it often presents a resemblance to the Jewish profile. On the occasion of a Criminal Court meeting at Beaumaris, in Anglesea, I observed this type presenting the extreme profile represented in fig. 15. The Gaelic type, however, it ought to be stated, is not very prevalent in Anglesea, or indeed in any part of North Wales.*

Mental Characteristics of the Welsh.—The following characteristics apply only to the first, second, and partly to the third of the Welsh types above described—(the Gaelic peculiarities will be found in the sequel):—Quick in perception—more critical than comprehensive—decidedly adapted to analytical research, and especially to philological and biblical criticism (the foregoing characteristics apply more particularly to the second Welsh type)—*extreme tendency to trace back ancestry*—great genealogists, and by race comparative anthropologists—

* At Beaumaris I met with an excellent specimen of the highest development of the second Welsh type, in the person of John Williams, Esq., solicitor, who is not only an accomplished general scholar, but an eminent theoretical musician, antiquarian, and comparative anthropologist.

poetical as regards the expression of deep feeling, but deficient in buoyancy of imagination—free from serious crimes, and very peaceable, with the exception of a tendency to cherish petty animosities which seldom break out into open hostilities—extreme tendency to religious excitement—economical, saving, and industrious to a fault—temperate, with a strong susceptibility to temptation when brought in contact with, or treated by, the English. The North Welsh, as a people, are decidedly superior to the *mass* of the English population; but the gentry of North Wales are in general behind in mental cultivation.

Among the more serious failings of the Welsh must be reckoned extreme parsimony, which, however, only degenerates into cheating when directed to the Saxon robbers of their ancestors.* The failing most commonly believed to be characteristic of the Welsh is a want of strict regard to truth. This failing, which is by no means so general in Wales as is often represented, I should be inclined to attribute to two causes—first, the existence of contradictory faculties in a Welshman's mind (this remark is most applicable to the second Welsh type). Thus, strong love of approbation may co-exist with equally strong covetousness, so as to lead a Welshman to promise what he either cannot bring himself to perform, or what lies beyond his power. Second, the nature of the Welsh language, which is not well adapted to express minute distinctions between truth and falsehood, and which by its constant use may encourage a tendency to ambiguity. How, it may be asked, can we harmonise a want of precision in the language with the eminence in philological and biblical criticism to which many Welsh scholars have attained? I think it does not follow that the original language of all the Welsh types was what is now called *Cumraeg*. The difference in dialects in various parts of the principality suggests the possibility of the present written or standard Welsh having been super-imposed on the original languages of at least some of the types. I have been informed that the names of many hamlets and farmsteads in North Wales are not *Cumraeg*, but have apparently been derived from a pre-

* The tendency among the inhabitants of some parts of Wales to cheat Englishmen, has been very greatly exaggerated. It is well known that at the inns of North Wales the charges are generally very much lower than in England; and, in the interior of South Wales, I have met with instances of disinterestedness, accompanying a sense of honour, which might be looked for in vain in most parts of England. With regard to Welsh inns, many favourable specimens may be found, not only as regards comfort, order, and systematic arrangement, but likewise as regards the intelligence and high character of the proprietors, throughout all parts of the Principality.

viously existing language. If this be a fact, it deserves to be particularly investigated.

Moral Condition of North Wales.—In most (not all) parts of North Wales, the moral condition of the working classes stands higher than in England. Infanticide is almost entirely unknown, and marriage as a rule is the consummation of what otherwise might be regarded as a reprehensible freedom of intercourse among men and women. The Welsh are too frugal and parsimonious to be guilty of those vices connected with extravagance, which are the very worst failing of the inhabitants of the larger towns of England. Though in certain respects excitable, they care little for those comic and sensational entertainments which, in England, form the keenest enjoyments of the mass of the population. There is likewise but little taste for those field sports which in England are more or less associated with gaiety. The Welsh are in general strangers to luxurious living, and many large villages might be mentioned with only one or two public houses, and these indifferently supported. The social order observable in some villages and towns can scarcely be exaggerated. Behind my apartments in Denbigh there was a row of cottages inhabited by men, women, and children, but so quiet* were the inmates, that after 9 p.m. I do not recollect having heard a single sound proceeding from these cottages during three weeks, excepting a hymn-tune on a Sunday. The village of Glan Ogwen, misnamed Bethesda, near the Penryn slate quarries, would, in England, be considered a model village, as regards order, quietude, temperance, and early hours. Reading, music, and religious meetings monopolise the leisure of the inhabitants. Their appreciation of the compositions of Handel, and other great musicians, is remarkable; and they perform the most difficult oratorios with a precision of time and intonation unknown in any part of England, except the West Riding, Lancashire, Worcester, Gloucester, and Hereford.

Music in North Wales.—The musical ear of the Welsh is extremely accurate. I was once present in a village church belonging to the late Dean of Bangor,† when the choir sung an anthem composed by their

* A traveller who expects to find in a Welshman the brother of an Irishman, is often surprised at the taciturnity characterising the former. In some parts of Wales, I have noticed this taciturnity prevailing to a very great extent, especially among the women. With them, even to smile is a very rare occurrence.

† It would be difficult to single out a dignitary of the Church of England, at any period of its history, who so completely devoted himself to the social, intellectual, and moral improvement of the people, as the late Dean of Bangor. His humility and activity were alike unbounded; and to the deepest reverence for things sacred he united the most brilliant conversational talent. He once assured me that the Welsh language is not nearly so un-

leader, and repeated an unaccompanied hymn-tune five or six times without the slightest lowering of pitch. The works of Handel, Haydn, Beethoven, and Mozart, are republished with Welsh words at Ruthin, and several other towns, and their circulation is almost incredible. At book and music shops of a rank where in England negro melodies would form the staple compositions, Handel is the great favourite; and such tunes as *Pop goes the Weasel* would not be tolerated. The native airs are in general very elegant and melodious. Some of them, composed long before Handel, are in the Handelian style; others are remarkably similar to some of Corelli's compositions. The less classical Welsh airs in 3-8 time, such as Jenny Jones, are well-known. Those in 2-4 time are often characterised by a sudden stop in the middle or at the close of a measure, and a repetition of pathetic slides or slurs. The Welsh are so musical that most of the Calvinistic Methodist preachers intone instead of merely delivering their sermons.

Religion in North Wales.—The Welsh, especially the North Welsh, are very religious, and the statistics of the country demonstrate that religion has done much to improve their moral condition. For every one who attends a place of worship in the more Scandinavian districts of England there are at least eight in North Wales. The religion is chiefly Calvinistic Methodism, which affords scope for the exercise of excited feeling and emotion. The Welsh are naturally a dramatic people,* and with them religious services are often converted into solemn dramatic entertainments. While at Llangollen I heard of a celebrated Welsh divine† (blind in one eye) opening a chapel on a wild hill-side not far from Bala lake. The subject was the progressive development of the Christian scheme from Adam to the final judgment. The prophets were made *dramatis personæ*, and the preacher represented them rising from the dead, appearing on the stage of time at the last day, and vindicating the correctness of their predictions concerning the Messiah.

Remarks on South Wales.—The first-mentioned, or long and high-

musical as is commonly supposed, and that he had no difficulty in getting Welsh children to pronounce such words as *lions* and *tigers* with great elegance; but that, in Nottinghamshire, he never succeeded in getting young persons to pronounce these words otherwise than as *loyons* and *toygers*.

* I cannot resist the belief that Shakespeare, if not a Welshman, was more allied to the Cymrian type, or one of its lateral variations, than any other type yet classified. In his native district, at least half of the inhabitants differ very little from the Gaelic-British and Cymrian-Welsh. To call Shakespeare a Saxon, would be to show a total ignorance of the science of races; though I should not like to be too confident in asserting that he was not a Dane.

† See Fig. 10.

featured physiognomy of North Wales (which, for convenience, I shall call CYMRIAN) becomes flatter and shorter as we proceed southwards through Central Wales, until in most parts of South Wales the comparatively flat-faced or second type (which I shall call BRITISH) is found to preponderate. This style of physiognomy is generally accompanied by very broad shoulders. The late eminent antiquarian, Archdeacon Williams, once informed me that about the time of the French Revolution 1,000 Cardiganshire volunteers were found on a certain occasion to take up as much room as 1,200 Midland County men (Angles and Danes!) In Glamorganshire and other parts of South Wales, I observed that, in addition to the above type, a large proportion of the inhabitants (chiefly the working classes) presented a greater or less approximation to what I have called the Gaelic physiognomy with the under part of the face projecting forwards.* (See figures 12, 13, 14.) This accords with the opinion of a very intelligent prize historian (Mr. Stevens, chemist, Merthyr Tydvil) that the first traceable inhabitants of Wales were Gaelic Britons, and that the Cymri from Strathclwyd† on entering Wales drove the pre-occupants to the South. The native music of South Wales is likewise to a great extent Gaelic, or similar to what we find in the more Gaelic districts of Scotland and Ireland—that is, in 6-8 time, and in the minor mode, with an ascending as well as descending flat sixth and seventh.

The mental characteristics of the South Welsh include these already stated in connection with the inhabitants of the North; but in most parts of the South the people differ from the North Welsh, and their dialects likewise differ. This may arise from the amount of Gaelic and British blood in the South, and from the extent to which the coast has probably been colonised from the south-west of Europe. Generally speaking, the South Welsh, though often very taciturn, are more excitable than in the North—more given to extremes—less orderly—and more divided among themselves. The Glamorganshire men have an antipathy to the Cardiganshire men, and other tribes are mutually at variance. In Caermarthenshire the people are very intellectually disposed. The chief ambition among young men in that county is to become speakers or preachers, and the congregational pulpits of England are largely supplied from Caermarthenshire and the neighbourhood. In the peninsulas, such as Gower, the descendants of Teutonic, chiefly

* About Merthyr Tydvil, a profile about midway between Gaelic and British seemed the most prevalent. See Fig. 6. One very occasionally meets with Fig. 16 in South Wales.

† A district lying between the rivers Clyde in Scotland, and the Mersey in England. Mr. Stevens has proved that some of the best Welsh poems were composed in Strathclwyd.

Flemish, colonists, may be found. It has been remarked that they make very much better sailors than the Welsh. The history of Pembrokeshire, or "Little England beyond Wales," is very well known.* I have been assured that the boundary line between the Flemings and Welsh is still sharply defined.

Along the borders of North and South Wales the people are more naturally intellectual than in any other part of England; Hertfordshire, Essex, Cambridgeshire, and Hampshire, perhaps excepted. In a long district running between Taunton and Oswestry—extending as far west as Hay, and as far east as Bath and Bewdley, science, especially geology, receives at least ten times more attention than it does in any other equally-sized area. This conclusion I have arrived at from personal observation, and it is corroborated by the comparative number of Fellows living in this district whose names may be found in the list of the Geological Society. It is difficult to explain this fact without supposing it to be connected with the Welsh derivation of many of the inhabitants, who may be regarded as Anglicised Welsh. It cannot arise from superior elementary education, for that is defective throughout the greater part of the district. Neither can mining pursuits be the cause, for the working miners are not the most intelligent part of the population. In the adjacent parts of Wales where English is spoken, we likewise find a greater taste for solid knowledge than in the heart of England. The little and poverty-stricken town of Montgomery, with its immediate neighbourhood, contains more than a dozen thoroughly informed and deep-thinking geologists; whereas a traveller might visit a dozen towns of the same size in Leicestershire, Lincolnshire, or East Yorkshire, without meeting with a single geologist. Ludlow, on the Welsh borders, possesses the best local geological museum in England.

Types in the West and South-West of England.—A considerable proportion of the inhabitants of the West Midland and South-western counties are scarcely distinguishable from three of the types found in Wales, namely the British, Gaelic, and Cymrian. In Shropshire, and

* The following history of settlers in Gower and Pembrokeshire is the most satisfactory I have been able to obtain:—In 1099, Henry Beaumont, Earl of Warwick, planted a colony of Somersetshiremen in Gower. About the year 1106, a tremendous storm carried away embankments and sand hills, allowing the sea to overflow a great tract in Flanders. A numerous body of the inhabitants sought refuge in England. They were first admitted into the northern counties; but, disagreeing with the English, they were removed to the district of Roos in Pembrokeshire. They are said to have afterwards disappeared. In the time of King Henry, a second body of Flemings came into England, and the king, wishing to oppose the power of Gryffydd ab Rhys in South Wales, sent them into Pembrokeshire.

ramifying to the east and south-east, the Cymrian* type may be found in great numbers, though not predominating (see *Anglian*). It seems probable that among the earliest inhabitants of the West and South-West of England, Britons, Gaels, and Cymri greatly preponderated. The Britons, either identical or mixed with Prehistoric Finns, may have been the first inhabitants. The Gaels may have come next, and then the Cymri. An Anglian element (from the east) and a Norse (from the north-west†) must, at a later period, have been superimposed on the previous compound population. In many parts of the south-west, and, at intervals, along the south coast, the prevailing type among the working classes is decidedly Gaelic. It may have come from Gaul, and the terms Gael and Gaul may be ethnologically synonymous. But it is certain that it not only prevails in the parts above-named, but in a more exaggerated, or in some places more mitigated form, in the Highlands of Scotland and in the greater part of Ireland. As already mentioned, it exists in South Wales, but North Devon and Dorset may be regarded as its head quarters in South Britain.

Gaelic Physical Characteristics.—*A bulging forwards of the lower part of the face*, most extreme in the upper jaw; chin more or less retreating (in Ireland the chin is often absent); forehead retreating; large mouth and thick lips; *great distance between the nose and mouth;* nose short, frequently concave, and turned up, with yawning nostrils;‡ cheek-bones more or less prominent; eyes generally sunk, and eyebrows projecting; *skull* narrow and very much *elongated backwards; ears standing off* to a very striking extent; very acute in hearing; slender or rather slender

* In Lancashire, and probably farther to the north, many words are of Welsh derivation. Besides Cymrian, the people of Lancashire would appear to be to a great extent Anglian (?) and Scandinavian.

† Worsaae's *Danes and Norwegians in England*, etc.

‡ In a large school at Tiverton, Devonshire, at least nine-tenths of the boys presented the most exaggerated Gaelic physiognomy, with gaping nostrils. It is a remarkable fact, that not one out of a thousand of the inhabitants of the North of England (apart from the Irish in towns) presents any approximation to the Gaelic type. The North of England nose is almost invariably thin, high, and sharp, with small nostrils. Archbishop Whateley, in his *Notes on Noses* (Bentley), is quite right in regarding this as an anti-cogitative nose, for the North is more characterised by activity than contemplation, and the people generally show a great indisposition to settle down to quiet meditation. The archbishop, in the above work, tells us, on the authority of the *Edinburgh Review*, that "there are certain districts in Leitrim, Sligo, and Mayo (as pointed out by an intelligent writer in the *Dublin University Magazine*), chiefly inhabited by descendants of the native Irish, driven by the British from Armagh and the south of Down about two centuries ago. . . . These people are especially remarkable for open projecting mouths, with prominent teeth (*i. e.* prognathous-jawed—the negro type), their advancing cheek-bones, and depressed noses, etc."

and elegantly formed body; stature short or middle-sized, though in some districts tall; hair brown or dark brown, and generally straight. There would appear to be two sub-varieties of this type, the one above described, and another with fair complexion, and red or light brown hair.

Gaelic Mental Characteristics.—*Quick in perception*, but deficient in depth of reasoning power; headstrong and excitable; tendency to oppose; *strong in love and hate;* at one time lively, soon after sad; vivid in imagination; extremely social, with a *propensity for crowding together;* forward and self-confident; deficient in application to deep study, but possessed of *great concentration in monotonous or purely mechanical occupations,* such as hop-picking, reaping, weaving, etc.; want of prudence and foresight; *antipathy to seafaring pursuits,* in which respect they contrast very strongly with Norsemen and Frisians; veneration for authority.

In Exeter and the neighbourhood, the Gaelic type (with both fair and dark complexions) is very prevalent; and with the exception of a type approximating to the Saxon, the population may be said to consist of Gaels, and a well-marked race with very dark hair, high forehead, Roman nose, thin lips, and prominent chins.*

In several parts of England to the south of the Thames, a type may be found predominating to which I shall apply the term Saxon. Its characteristics accord with local traditions concerning the ancient Saxon, and it is similar to a type still prevailing in many parts of Germany, to which no name but Saxon can well be applied. The localities in England where it most intrudes itself on the traveller's attention are very nearly those where Saxons landed according to history, or to which Saxons may conveniently have migrated. These localities may be stated as follows:—the Isle of Selsea and the neighbourhood of Chichester,† the district extending between East Grinstead and Hastings, chiefly in Sussex, but including the neighbouring part of Kent, the valley of the Hampshire Avon as far as Salisbury and the neighbourhood, the West of Berkshire,‡ especially the White Horse

* This race is likewise to be found in Cornwall. But the Cornish chiefly consist, first, of Gaels with dark or brown hair; second, a race with a rather short angular face, somewhat like the second Welsh type; third, a race more or less hatchet-faced; and fourth, a race with a very Spanish-looking physiognomy.

† At the national school of Bersted, near Bognor, I observed that nearly all the girls presented the most decided Saxon physiognomy. In many parts of England there are large schools in which not a single Saxon face can be found.

‡ The Saxon hock-tide sports are still kept up in Hungerford and the neighbourhood.

MAP TO ILLUSTRATE

THE

COMPARATIVE ANTHROPOLOGY

OF

ENGLAND AND WALES.

BY

D. MACKINTOSH, F.G.S.

The areas are not coloured, because the boundary lines cannot be precisely defined.

Valley and vicinity. But Saxons may likewise be found in considerable numbers, though not always predominating, in the interior of the Isle of Thanet, the south of Dorsetshire, the east of Devonshire, the greater part of Somersetshire, and likewise in the East Midland Counties.

Saxon Physical Characteristics.—Features excessively regular; face round, broad, and short or rather short; mouth well formed, and neither raised nor sunk; chin neither prominent nor retreating; nose straight, and neither long nor short; under part of the face a short ellipse; *low cheek bones; eyes rather prominent,* blue or bluish-grey, and very well defined; *eyebrows semicircular, horizontally,* and not obliquely placed; forehead semicircular, and skull of a shape midway between a parallelogram and a round, flat above the ears, and small in the occipital region; *flattened ears; hair light brown;* chest and shoulders of moderate breadth; *tendency to rotundity and obesity,** especially in the epigastric region; *short and round limbs, hands and fingers,* general smoothness and roundness; total absence of all angles and sudden projections or depressions. See fig. 19 (a Chichester Saxon), figures 20, 21.

Saxon Mental Characteristics.—Extreme moderation; absence of extraordinary talents, and equal absence of extraordinary defects, mind equally balanced; character consistent, simple, truthful, straightforward and honest; persevering in pursuits admitting of variety, but unadapted to purely mechanical or monotonous occupations; predilection for agriculture; determined, but not self-willed; self-reliant yet humble; peaceable, orderly, unexcitable, unambitious, and free from extravagance; not brilliant in imagination, but sound in judgment; great general benevolence accompanying little particular attachment; tendency to forget ancestors, to care little about relatives, and to have limited intercourse with neighbours.

The term *Anglo-Saxon* has little or no meaning in the present state of English anthropology, unless it be strictly limited to a combination of the Saxon and Anglian types. But some of the mental peculiarities commonly assigned to the supposed Anglo-Saxon, are quite as applicable to the Dane as to the Saxon; and in all political orations in which the word *indomitable* is used it ought to be coupled with Dano-Saxon instead of Anglo-Saxon.

Is there an Anglian Type in England?—Some suppose that the Anglian colonists of East Anglia, Mercia, Deira, and Bernicia,† were

* Numbers of very rotundiform and massive Saxons may be seen in the markets of most of the towns of Sussex, West Berkshire, etc. In Northampton market, a very Saxon-looking race, but taller and darker in complexion than the strictly typical Saxon, may be seen predominating.

† According to the best historians, in 527 and afterwards, Angles arrived

mere handfuls in comparison with other settlers from the Continent. Bede, on the contrary, asserts that the Anglian province in Jutland was laid waste by the extent of the emigration. I have not been able to trace a very well defined type to which the term Anglian can be exclusively applied, but a race not very dissimilar to Saxon, though in some points peculiar, and which looks like a lateral variation of the Saxon type in the direction of both Dane and Norwegian, may be found in great numbers, especially among the women in the following districts:—Suffolk, and parts of Norfolk, Cambridgeshire, Hertfordshire, Bedfordshire, Northamptonshire, Buckinghamshire, parts of Oxfordshire, Warwickshire, South Staffordshire, Shropshire, the east of Derbyshire, the west of Leicestershire and Nottinghamshire, and a zone running north through the West Riding of Yorkshire into Durham.

Anglian (?) Characteristics.—The characteristics which may be provisionally termed Anglian are the same as Saxon, with the following exceptions:—face rather longer and narrower than the Saxon; cheekbones slightly projecting; chin varying from rather prominent to rather retreating, and more or less approaching angularity; nose narrower and more elegantly chiselled than the Saxon, and the nostrils more compressed; frame much more slender than the Saxon, with narrow shoulders, long neck, and erect figure; hair of a more golden or yellowish hue than the Saxon; complexion exceedingly fair, with more or less of a pinkish hue; in mental character more active, determined, and ambitious, than the Saxon; deficient in the more disinterested tendencies of human nature, and dull in those faculties which elevate man above the necessary affairs of life, but pre-eminently adapted to make the most of the world. Figures 17, 18, 22, are from Anglian districts.

Frisians (?) and Jutes.—In the east-midland, eastern, and south-eastern counties of England, we frequently meet with a physiognomy

in Norfolk and Suffolk (East Anglia). In 547 a more numerous body arrived, under Ida, in the district between the Tyne and the Forth (Bernicia), and afterwards spread farther to the south. In 560, Angles arrived under Ella, and settled in the country between the Tyne and the Humber (Deira). In 585, Angles under Crida arrived in the midland districts of England (Mercia). It is stated in one or more Directories of Shropshire and Staffordshire (I cannot ascertain on what authority), that the English settlers were divided into families or tribes, with the following names:—The Harling, Horning, Hanning, Willing, Elling, Whitting, Totting, Patting, Holling, Essing, Hunting, Copping, Eding, Rolling, Darling, Wigging, Bucking, Winning, Stalling, Tibbing, Packing, etc. How far this may be correct, I am not prepared to say; but it is certain that numerous names of places, apparently referable to the above or similar tribes, may be found in the midland counties, particularly in Shropshire. I think it probable that *ton* (as in Whittington) is more especially, though not exclusively, an *Anglic* termination.

which is neither Saxon nor Danish, and which is similar to a prevailing type in many parts of Friesland. The face is narrow, and the features prominent, but the profile is not so convex as in the type next to be described. The complexion is fair, and the hair light brown. The skull is narrow, high at the spot called firmness by phrenologists, and low in veneration. (See fig. 24.) The mental character is chiefly remarkable for extreme self-complacency, and independence of authority. In Kent, this type graduates into a much more strikingly-marked type, to which I shall provisionally apply the term JUTIAN, as it is found in Kent and the eastern part of the Isle of Wight—localities which, according to Bede, were colonised by Jutes. On walking from Ryde to Brading, in the Isle of Wight, one evening, I met numbers of men returning from work, and in almost every instance they presented the under-mentioned peculiarities. I found the same type predominating in the neighbourhood of Brading, and likewise in West Kent, especially about Tunbridge.*

Jutian Characteristics.—Very convex profile, so that if one leg of a pair of compasses were to be fixed in the ear, the other would describe not only the contour of the face, but of the skull (see Fig. 25); cheek bones slightly projecting; nose sinuous, and rather long; dull complexion, and brown hair; grey or bluish-grey eyes; narrow head, and face more or less narrow; long neck, narrow shoulders and chest; frame broadest at the trochanters; springing gait; often tall, especially in the Isle of Wight; extremely adapted to the practical affairs of life; tendency (still greater than in the Saxon) to manifest indifference to ancestors, relatives, and neighbours.

In North Kent, the Jutian graduates into the Danish type. Concerning the latter, I have no remaining doubt, as it decidedly preponderates in those parts of England where Danes must have settled in the greatest numbers. It is to be met with more or less in all the midland counties; in Lancashire, Westmoreland, Cumberland, Northumberland, and Durham; but chiefly in the North and East Ridings of Yorkshire, Norfolk, Suffolk, and, above all, in Lincolnshire.† In

* Mr. Roach Smith has found that the sepulchral remains of Kent and the Isle of Wight are similar, and that they are different from Saxon strictly so-called. In Kent, I have heard of old songs and traditions which imply that the inhabitants did not formerly regard themselves as Saxons.

† The termination *by*, in names of places, has been pointed out by Dr. Latham and others as exclusively Danish. It is well known that an immense number of names in Lincolnshire have this termination; but many, perhaps, are not aware that in the north-east of Leicestershire it is quite as common. The following is a list of names terminating in *by* in Melton Mowbray union:—Ab-Kettleby, Asfordby, Ashby-Folville, Barsby, Brentingby, Wyfordby, Brooksby, Dalby, Freeby, Frisby, Gaddesby, Gradby,

the latter county, I have been at some pains to collect the characteristics of the inhabitants, and before proceeding to a detailed statement, I must remark that a frequently-observed variation from the predominating profile consists of a sunk mouth and prominent chin (instead of a rather prominent mouth and rather retreating chin). I have often thought that this variation in certain parts of the physiognomy in the same race (the other physical peculiarities being the same) may be part of a law calculated to secure sufficient individual differences in families, without the typical limits being transgressed.

Danish Physical Characteristics.—Long face and rather coarse features; *high cheek bones, with a sudden sinking in above on each side of the forehead; high and long nose;* rather prominent mouth, and rather receding chin (see preceding section); skull narrow, elongated, and increasing in width backwards; large occipital region; high in what phrenologists call *self-esteem, firmness*, and veneration; long neck, and low, rather narrow shoulders; stature various, but in general tall; *swinging gait;* hair either yellowish flaxen, yellow, *red*, auburn, chestnut, or brown with a reddish tinge; whiskers generally red; grey or bluish-grey eyes; *ruddy complexion.* (See figs. 27, 28.) Fig. 26 is a mitigated form of Danish face common in all Danish districts.

Danish Mental Characteristics.—Sanguine, active and energetic, with a tendency to be always doing something, which often leads into scrapes; determined, courageous, and *ambitious; proud*, vain, and ostentatiously benevolent; high sense of honour; warm in love or hate; obliging and hospitable; tendency to extravagance in eating and *drinking; very social and convivial;* talent for practical science, but deficient in depth of thought, or adaptation to philosophical studies; *good speakers* but *bad listeners;* tendency to apply inventions to pecuniary advancement; capacity for pushing *on external or material civilisation.* A well educated Dane is an ornament to society. An ignorant Dane stands very low in the anthropological scale.

Norse Districts of England.—Names of places and persons, dialects and history, would lead us to expect a Norse element in the population of Cumberland, Westmoreland, parts of Lancashire, and the northern parts of the West Riding of Yorkshire. Indications of the same element are not perhaps wanting in other parts of England.* I have searched, especially in Cumberland, for a type or types to which the term Norse may be applied. But in addition to Cymrians and Danes, I have not met with any extensively prevailing type except the following. Face rather flat, chin angular and rather prominent, mouth well formed and frequently depressed. Nose high, but not so long as in the Dane; cheek bones often a little projecting, eyes grey, forehead square, and head a short parallelogram;* neck rather short, and shoulders rather broad; *stature generally tall;* complexion among the men ruddy, and hair either brown or sandy; *whiskers generally sandy;* complexion among the women fair, with a lily or *pinkish* hue; good mental abilities, and, with sufficient inducement to cultivation, capable of attaining a high intellectual rank, but very deficient in precocity; practical, orderly, and cleanly; *obliging* to an unparalleled extent, though not free from suspicion; *honest* to an extreme perhaps unknown among any other race in England. The proof of this honesty may be found in doors not being locked during night—in the absence of imposition at inns and lodging-houses—in disdaining to take advantage of strangers—in making no charge for small services—and in refusing any return for favours bestowed. The latter peculiarities may likewise be regarded as resulting from that sense of honour and independence of mind by which the Norsemen in all ages have been characterised.†

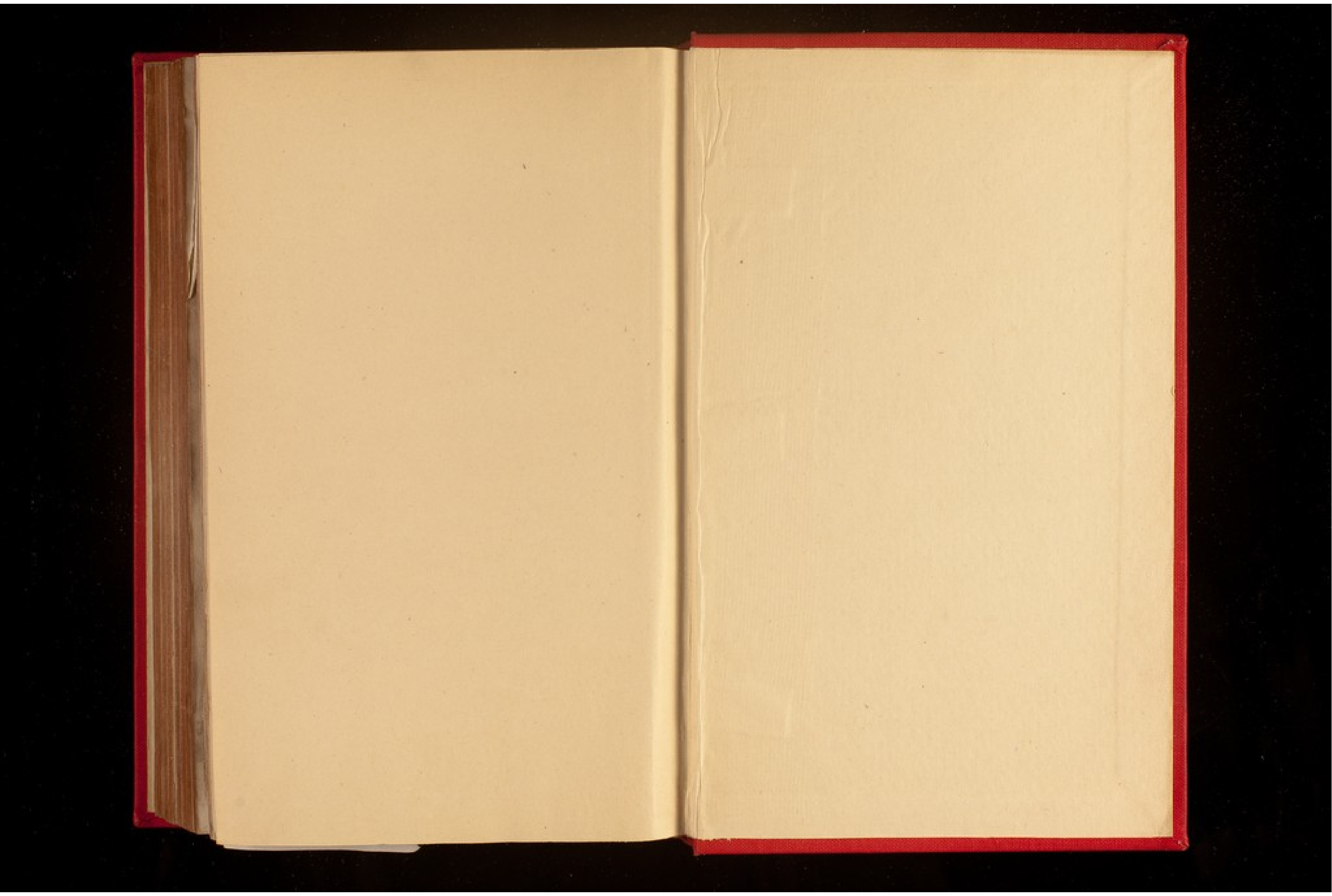
In the foregoing survey of the comparative anthropology of England and Wales, I have left anatomical details out of consideration, because I have found it necessary to confine attention to a particular line of observation in order to retain sufficiently distinct impressions, and because I have no doubt Dr. Barnard Davis, who has taken up the anatomical department, will soon be able to connect it with the evidence furnished by physiognomy and mental characteristics. The colour of skin, hair, and eyes, is likewise a subject on which I have touched very briefly, as that may be more profitably left in the hands of Dr. Beddoe. As we may learn from the history of geology, it will not be until after the results of distinct lines of investigation have been grouped and generalised, that we can succeed in establishing fundamental principles on which the superstructure of comparative anthropology can be safely erected.

Harby, Hoby, Kirby-Bellers, Rotherby, Saltby, Saxby, Saxelby, Somerby, Stonesby, Sysonby, Wartnaby, Welby.

* The Staffordshire *clog*, several specimens of which have been found (see *The Reliquary*, by Llewellyn Jewitt, Esq., F.S.A., Derby), consists of a piece of wood, with marks on the edges, and Runic symbols. It is generally attributed to Norwegians.

* I have refrained from giving any decidedly illustrative portrait of the Norse type, as I have not been able to meet with any furnishing a satisfactory average representation. Fig. 23 is not uncommon in the Scandinavian districts of the north of England.

† Worsaae is correct in his assertion, that the inhabitants of Cumberland are extremely addicted to litigation.



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