I.

INVESTIGATIONS INTO THE CONDITIONS GOVERNING THE TEMPERATURE OF THE BODY

BY

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It is well-known that the heat of the body is derived from the combustion of the nutritive materials absorbed; this combustion takes place mainly in the skeletal muscles and in the muscles and glands of the digestive tract.

The heat produced by metabolic changes is chiefly absorbed by the blood, an increased quantity of which flows through the heat-producing organ, and with the circulating blood the heat is conveyed to the other organs of the body.

The temperature measured in a particular part of the body will therefore depend on the quantity of blood passing through the organ concerned during a given time, and thus not only on the intensity of the metabolism but also on the action of the heart, on the vascularity of the tissues concerned, on the state of innervation of the vessels, and further on the conditions for giving off heat to the surroundings and on other purely physical conditions under which the thermometer is placed.

It is hardly necessary to point out how much the result of a thermometer reading may vary according as one or other of the factors named above exercises a determinative influence in the part concerned. It should be evident that it is only when these conditions have been cleared up for each single one of the usually employed local temperatures, that we have a proper basis for the comparison of the temperatures measured, and, on the whole, for judging of the value of a clinical reading of temperature.

Perusal of the literature dealing with this subject does not, however, convey the impression that the above considerations have been kept in mind in the investigations. In the text-books, for example, we find standard values for the various local temperatures, but not the limits of variation; in monographs we find purely numerical comparisons between the temperatures of the mouth and rectum; and as a rule the author is satisfied to show a constant or inconstant relation. One is warned against taking the temperature of the mouth immediately after a meal; it is an open question whether the temperature of the mouth should be taken in a heated room, or whether it may be taken anywhere. An English investigator finds the mouth temperature "much more unreliable on the sea than on land". Continually no "why"? The temperature of the axilla has silently disappeared from clinical medicine. No physician wanting to know the temperature of his patient now-a-days thinks of measuring it in the axilla. Why? It is said that this reading takes too long, that the axilla temperature like that of the mouth is "less reliable"; but no further evidence is given.

I cannot help thinking that the confusion lies in the word "body temperature", this mysterious conception, defined by no one, and which no one can define, since it lacks all real basis as soon as we leave the vague generality, namely, any temperature measured in or on the animal organism contrasted with the temperature in air or water.

We need only refer to any physiological text-book to learn that the temperature is not the same in the various organs of the body. Thus, if we measure it in the rectum, we do not obtain, as one author maintains, "an exact expression for the body-temperature", but, provided that the reading is accurately taken, merely an exact expression for the rectal temperature. If we now enquire into the factors which determine the rectal temperature, we may learn what the reading means and for what purposes it may eventually be used.

In the following pages I shall endeavour to answer some one or other "why" in thermometry, on the basis of a series of temperature measurements made in arctic regions over a period of two years.

The local temperature which is of special interest in clinical practice is the rectal temperature, but the temperature of the mouth is still employed by not a few physicians. The axillary temperature is probably no longer used, but as we have the data from its former application at any rate, and as the temperature of the skin is of general interest, compared with the other local temperatures named above, I have tried as far as possible to include the skin temperature in my investigations.

The rectum is the most usual and, as will appear from the following, the most reliable place for the clinical reading of temperature. There is hardly any reason for entering into details with regard to the measuring instrument itself, the thermometer. I have employed mercury maximum thermometers, the scale of which read to 0·1° C.; where two decimals occur in the tables below, the second decimal has been estimated. As my main object was to determine variations of temperature, a slight zero correction is of no importance, so long as the same thermometer is employed; if we use several thermometers indiscrimately in the same series of measurements, we must, of course, know their mutual relation.

For a sensitive thermometer $2^1/_2$—3 minutes will be sufficient for the mercury to adjust itself, even if the thermometer has been somewhat cooled down before use.

These conditions will not offer difficulties in obtaining comparable results; but on coming later to fix the place where the readings are to be made, it is less easy to procure the necessary uniformity. If we wish to compare a series of rectal temperatures, they must be measured as far as possible in the same place, and this is best obtained, I think, if the receiver of the thermometer is just conveyed through the anal canal into the ampulla recti, i. e. the thermometer is introduced about 4 centimeters, measured from the anal opening.

To measure the temperature in the anal canal is hardly correct, as contractions in the sphincter region may well raise the temperature at this spot. In this connection it is worth observing that Erlandsen[1], who has measured the temperature especially in "nervous" patients, in several cases found higher temperatures in the sphincter region than a little higher up.

But if the thermometer is introduced too high up, the uncertainty is also increased. In a short criticism of the casual, often very negligent way in which measurements of temperature are made and estimated, Thulstrup[2] states, that the temperature is higher further up in the rectum than in the ampulla; "highest and lowest position generally gave a difference of 4 to 9 decimals"; he has even seen a difference of 1·1°. My own observations, which are not many however, are not in Thulstrup's favour, but this may possibly be because Thulstrup's measurements were made on fever patients in bed — this is not directly stated but seems to have been the case from the context — whilst my measurements are self observations.

In the measurements noted below, the temperature was first taken in the ampulla and immediately afterwards 10 to 15 centimeters higher up in the rectum. It was not always possible to have the thermometer equally high up, the point being detained by folds of the mucous membrane. The highest point reached was at a distance of about 15 cm. from the anus; on trying to get farther

[1] Hospitalstidende No. 48, 1908.
[2] Hospitalstidende No. 6, 1905.

up, disagreeable pains were experienced, resembling pains produced by pressure on nerves.

Ampulla recti	10—15 cm. higher up in rectum	
37·20° C.	36·95° C.	
37·35° -	36·90° -	after defaecation & then sitting still in cold air for about 1½ hours.
36·85° -	36·80° -	after sitting still, felt chilly.
36·75° -	36·72° -	
37·10° -	37·07° -	
37·40° -	37·40° -	after a little exercise in open air.
36·50° -	36·50° -	after sleep.
37·00° -	37·00° -	after meal.
37·50° -	37·60° -	— —
37·25° -	37·30° -	after easy work.
36·90° -	37·00° -	
37·10° -	37·15° -	
36·95° -	37·04° -	

From these readings, which are not arranged chronologically, but in such a way as to give the easiest view, it appears, firstly, that all the differences are small, not nearly so great as those of THULSTRUP, but secondly that they have not the same sign, and this is of vital importance for the question, where to read the temperature; since, clearly, we cannot know in what direction the difference will tend if we change from the place once taken.

Nor is the rectal temperature the same under all conditions in the various positions of the body; it may especially be higher in an upright than in a lying position.

Where nothing to the contrary is said, the retal temperature in the following has been measured in the ampulla recti in a lying position.

Next to the rectum the m o u t h is the most frequently employed for the reading of temperature. The thermometer is here placed in the sulcus alveolo-lingualis.

For this use a thermometer of the usual shape is less convenient; on the one hand, such will disturb the tongue in its natural position and thus prevent the receiver of the thermometer from obtaining the closest possible contact with the tissues, and on the other it will interfere with the upper lip and render the closing of the mouth difficult. Thermometers of a special shape have, indeed, been constructed for the mouth readings. The thermometers have been made small, the receiver has been given the form of a two-pronged fork, the whole thermometer has been shaped like a bayonet, and finally these different forms have been combined. One may, of course, grow accustomed to the use of a thermometer, even if the shape is unpractical, and even with the best thermometers the readings in the mouth require a certain amount of training, before reliable results can be obtained; nevertheless, I do not hesitate to maintain that the mouth thermometer ought to be shaped like a bayonet, as it is only with such a thermometer that the tongue remains in its natural position and that the lips can meet spontaneously and keep the mouth closed without effort; consequently, that the muscles of the mouth and face can remain quiet during the reading.

Further, the temperature ought always to be measured in the same place, which is most easily attained by carrying the receiver of the thermometer as far back as possible into the sulcus alveololingualis; it will therefore be practical to have the thermometer of such dimensions that the distance from the end to the first bend corresponds with the distance from the posterior limit of the sulcus to the row of teeth (about 4·5 centimeters). Since the temperature of the mouth, as will be shown later, is to a high degree dependent upon the external temperature, it is reasonable to assume that the temperature is not always the same in the front, comparatively thin-walled part of the mouth and in the hinder, more sheltered part. I cannot show this by figures, because a small thermometer taken for this purpose was lost all too early; but what I have found tends in the direction mentioned. Finally, the receiver of the thermometer ought to be single not forked; for the temperature need not be the same in the two sides of the mouth, and a mean temperature is of no particular interest, as the mouth readings are not adapted to clinical use.

As regards the time occupied by a reading, it seems as if the thermometer is somewhat longer in adjusting itself in the mouth than in the rectum, which is partly due to the anatomical and physical conditions; for my measurements also, no doubt, to the thermometer itself. As a rule 5 minutes will prove sufficient; but to make sure of the result, especially if the thermometer has been much cooled down before use, one ought to let it remain in for about 10 minutes.

On measuring one's own temperature, as I have done, we may control the amount of rise by means of a mirror. On taking a series of mouth readings, beginning with a very cold thermometer, it has proved, that we run the risk of obtaining too low a result in the first reading and too high in the next, which is presumably due to a vasomotor reaction against the cold irritant. Finally, we must also note, that, with a strong rise of temperature in a room or on entering a heated room from the cold, the temperature of the mouth may rise still higher. This is perhaps the reason why a German phthisiologist warns against leaving the thermometer longer than 10 minutes.

As the temperature of the mouth is not the same in the different positions of the body, or perhaps more correctly, as it varies with changes in position, the position during the reading ought to be noted.

In the following, when nothing else is said, the temperature has been measured in a quiet sitting position in the right side of the mouth.

While the place where the rectal or mouth temperature is measured or ought to be measured, can be fixed with fairly great precision, so that any deviation from this place must be limited, occuring in certain known, fixed directions, and thus on the whole of quite minor importance in the reading, the case is very different with the temperature of the skin. The skin is not a definite position in the same sense as the sulcus alveolo-lingualis; on the skin we have different localities which vary as regards temperature, so that the readings may differ considerably. Parts of the skin are practically always in immediate contact with the air, others are, as a rule, covered with clothes and so on; but it is common to all of them that the receiver of the thermometer only comes into contact with cutaneous tissues, the firm epidermis of which can never surround it completely, as also that the medium lying between is air, whilst the skin will always be in immediate or indirect contact with the atmosphere.

In the axilla and similar places we may be able temporarily to form a closed space inaccessible to the outer air, and in such places a thermometer of the usual shape may be employed for the reading of the temperature[1], but if the temperature is to be measured at a place where the receiver of the thermometer can only be brought into contact with a practically level part of the skin, a thermometer of a special shape must be employed, so that the receiver and the part of the skin concerned are isolated from the atmosphere during the reading. Usually the mercury bulb is flattened and bent spirally at right angles to the tube of the thermo-

[1] Special thermometers have been construeted for reading the axilla temperature, but they are of no special interest, as the axilla is not used in clinical practice.

meter and scale, and at a suitable distance it is surrounded by some insulating material, glass or ebonite. In giving the receiver of the thermometer such a shape we obtain the greatest possible surface of contact with the skin.

The skin thermometer takes a considerably longer time to adjust itself than the rectal thermometer. That used by me for readings on the face always seemed to take about 5 minutes, of which I have often convinced myself by control by means of a mirror; at other places it may be much longer, up to 25 minutes. If certainty cannot be reached by reading in a mirror or by direct control, the thermometer ought to remain in its place for a longer period; but as the temperature of the skin regulates itself to the outer temperature even more easily than that of the mouth, at any rate on the uncovered parts of the skin, this must be remembered in the measurements. To rub the receiver round and round on the skin, in order to make the thermometer become more quickly adjusted, is not advisable; OEHLER[1] has followed this course but one is certainly too sanguine in believing that such manipulations can be made "without the circulation in the skin being affected".

Metabolism and temperature.

Although the relation between metabolism and rectal temperature may be said to have been elucidated by the researches especially of Swedish physiologists, TIGERSTEDT and his collaborators[2], I have made some respiration experiments at the different hours of the day and calculated the amount of carbonic acid given off. It appeared, namely, that my curve of variation for the rectal temperature differed in several regards from that generally stated to be the normal. The respiration experiments were made at intervals of 2 hours from 7 a. m. to 9 p. m. During the experiment I lived quietly and regularly, so that the curves correspond to day-curves for days without bodily work. It became evident to me, as to others, that if we draw a curve for the metabolic changes as manifested by the amount of carbonic acid given off, and a curve for the rectal temperature, the course followed by both these curves is mainly the same; full agreement is not to be expected. The rectal temperature is a local one and consequently subject to influences which have no direct connection with the metabolic changes and

[1] Deutsches Archiv f. klin. Medizin, Vol. 80, 1904, p. 245.

[2] TIGERSTEDT and SONDÉN: Skand. Archiv f. Physiologie 6, 1895. TIGERSTEDT, SONDÉN, LANDERGREN and JOHANSSON: Skand. Archiv f. Physiologie 7, 1897. JOHANSSON: ibid.

the latter, on the other hand, are not completely expressed by the amount of carbonic acid set free.

Curve 1 contains the average of the determinations of 4 series, of which one series is noted separately as curve 2. The differences between these two curves are exceedingly slight owing to the regular mode of life and the uniformity of the food.

Two of the four series are from November-December 1907, the

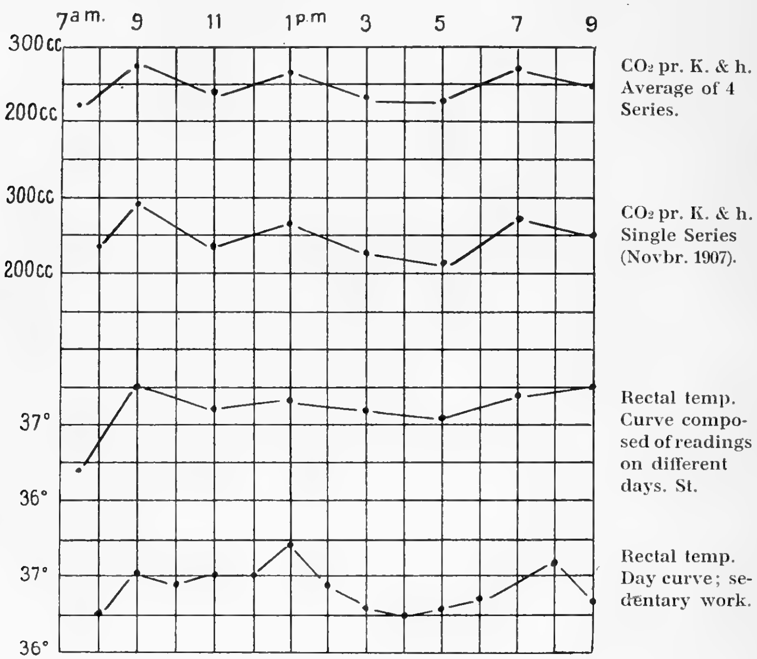

Fig. 1 a.

two others from May 1908. As I could only make one, at most two experiments on the same day, the curves are compounded of experiments on different days, by which means the firmness of the type is further strengthened. The 3 maxima of the curve occur immediately after the principal meals of the day.

Curve 3 shows the corresponding changes in the rectal temperature. The curve is based on measurements made at the same time as the respiration experiments and, though to a less degree, it displays fluctuations up and down corresponding to those of the CO_2-curve. In the last series of experiments I was imprudent enough to take the temperature in an upright position (in the curve given only the morning temperature was taken while lying down), which often gives too high values owing to stasis; this is certainly the case for the last reading, which was taken after standing still in the mess-room. Curve 4 (from measurements in another individual) is better adapted for comparisons; it is a typical curve of variation for a day without bodily work; the temperature was here read in recumbent position. The difference between this and the other curves with regard to the height and situation of the maxima is due to differences in the meal-times and character of the meals. In curve 4 the last meal falls an hour later than in the other curves, and while the meal of 6 p. m. is the main meal in the latter, the main meal in curve 4 was between 12 and 1.

The greater disagreement between TIGERSTEDT's curves for CO_2 and those for temperature is certainly due to the fact, that he employed JÜRGENSEN's "normal curve" for comparison. This curve is an artificial production and therefore hardly fit for concrete instances.

It would be of interest to compare a series of morning measurements with the amount of CO_2 set free "on an empty stomach". In this respect I can only show the results from 5 successive days; for my morning experiments I have in most cases only taken the mouth temperature, which for reasons not noticed till later is not suited to the comparison mentioned here.

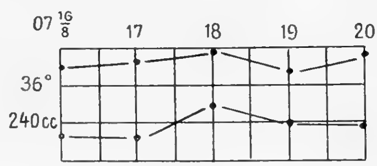

Fig. 1b.

The two curves do not differ very much, but as both are almost straight and the observations were few in number, their value is naturally rather limited.

The agreement between the amount of carbonic acid set free and the temperature only holds good for the rectal temperature. For the mouth temperature no connection can be discerned, even in long series of measurements; for this the greatest care must certainly be taken that the outer temperature is always nearly the same. And for my part I could not possibly fulfil this condition.

Owing to the distinct parallelism between the CO_2-curve and that for the rectal temperature, we may expect that the factors which influence the metabolic changes will also find expression in the latter.

Meal-time and temperature.

As the work of digestion extends over a long period and proceeds comparatively evenly, it will only to a slight degree tell upon the form of the temperature curve. It is in fact only during the first part of the process, the meal-time, that we can notice any effect. We have seen from the curves already given, that there are fluctuations corresponding to the meal-times. And on measuring the temperature before and after meals, the latter will, as a rule, be the higher; except, so far as the rectal temperature is concerned, however, the meals taken immediately after strong bodily exertion, on which more will be said below.

In 30 measurements taken twice, the temperature being taken immediately before and after meals under ordinary conditions, I have found an average difference of $0{\cdot}375 \pm 0{\cdot}03$, $\mu = 0{\cdot}26 = 69\,\%$ of average. The meal-time being a variable factor, an average will have a very limited value, which is also shown in the large standard deviation. The differences are for the rest evenly distributed, 17 minus and 13 plus, further

$$\frac{\mu}{2} > 12 = 40\ \%$$
$$\frac{2\mu}{2} > 21 = 70\ \text{-}$$
$$\frac{3\mu}{2} > 26 = 86{\cdot}7\ \text{-}$$
$$\frac{4\mu}{2} > 29 = 96{\cdot}7\ \text{-}$$
$$\frac{5\mu}{2} > 30 = 100\ \text{-}$$

The fact that bodily exertion is in certain cases able to conceal the actual change of temperature during the meal indicates, however, that even under ordinary conditions this factor ought not to be left quite out of account. Examination of the separate observations shows, indeed, that the rise of temperature is greatest after a meal following soon after sleep, least in the cases where one has been active before the meal. Herein lies certainly the main reason for the great deviation.

When leading a sedentary life, the rise of temperature after meals will, as a rule, appear distinctly in the curve of variation, but the temperature will soon begin to fall again and after 1—2 hours return to its former level.

The mouth temperature also rises at meal-time, and this rise is, at any rate in the main, independent of the simultaneous rise in the rectal temperature; this appears from its showing greater fluctuations than the latter, and from the fact that it also rises or may do so, in cases when the rectal temperature falls.

	Rectal temp.	Temp. of the mouth		
4/4 07	37·52° C.	36·45° C.	before	meal after exercise in open air and staying 1/2 an hour indoors.
	37·40° -	36·70° -	after	
6/4 07	37·70° -	36·60° -	—	
	37·40° -	36·95° -	—	

It ought perhaps to be remarked that the rise of the mouth temperature is in both these cases lower than the average rise, which as stated below is about 0·5°.

The result of 32 measurements taken twice displays a mean deviation of 0·52° ± 0·06, μ = 0·34 = 65 % of mean. Thus, very great variations occur, and consideration of the separate observations readily shows the origin of the great diversity. All the very great differences occur on going into a meal in a heated room after being in the open air or in a cold room; conversely, the differences are small when the meal is taken in a cold room after coming from a heated room. In my cases, all of which refer to meals within doors, I have never found a fall of temperature after the meal; the greatest deviation measured was 1·5°, the least 0. The 32 deviations are for the rest distributed among 13 plus and 18 minus differences, the difference in one case being 0; of these

$$\frac{\mu}{2} > 14 = 43{\cdot}8\ \%$$
$$\frac{2\mu}{2} > 27 = 84{\cdot}3\ \text{-}$$
$$\frac{3\mu}{2} > 28 = 87{\cdot}5\ \text{-}$$
$$\frac{4\mu}{2} > 29 = 90{\cdot}6\ \text{-}$$
$$\frac{5\mu}{2} > 31 = 96{\cdot}8\ \text{-}$$
$$\frac{6\mu}{2} > 32 = 100{\cdot}0\ \text{-}$$

This "advantageous" distribution is due to the few large variants which give a disproportionately high value for μ.

Omitting the 5 largest deviations the average is almost unchanged (0·47°); but μ decreases almost to the half. In a small group of 7 cases, where the temperature was measured together with other physiological functions before and after meal in the middle of the day, and where I stayed in a comparatively warm room during both series of measurements, the rise of the temperature was in average 0·49° with a standard deviation of 0·14°. As the disturbing influence of the air temperature is here reduced to the least possible (total elimination was out of the question), I venture to conclude from these measurements, in connection with what has been shown above, that the rise of the mouth temperature owing to the meal must be taken as about 0·5° C. under the particular conditions of life mentioned.

The nature of the meal is of considerable importance for the mouth temperature. The rise of temperature is highest after warm food, especially if seasoned a great deal; in the latter case I have seen the mouth temperature reach a higher value than the rectal temperature[1]; but I have never seen any great rise of temperature after partaking of the very hard ship's hread, which makes exceedingly great demands on the mastigating organs, and I therefore consider mastigation, and on the whole the working of the muscles during the meal, as a subordinate factor in the changes of temperature dealt with here.

Thus, a general rise of temperature is occasioned by the work of digestion in the more restricted sense, in connection with the increased action of the heart during the meal; but apart from this a local rise of temperature occurs in the mouth, and this is undoubtedly due to a fluxion in the mucous membrane owing to thermic and chemical irritants. Reasons have been given above why the rise in the mouth temperature cannot be a simple consequence of the general rise of temperature; the mutual independence of the two temperature movements is also displayed in their being disturbed from quite different sources.

The reduction of the heightened temperature of the mouth goes on evenly in the course of a few hours when staying in a temperate room; on passing to cold air the fall may take place in a few minutes.

I have also found the t e m p e r a t u r e o f t h e s k i n (taken on the forehead) to increase during a meal; but as the variations of the latter, on the external temperature changing, are much more violent than those of the mouth temperature, and as it is also influenced by brain work, mental engrossement, which cannot be excluded when taking meals in the company of others, I cannot venture upon making even an approximate estimate of the increase. In some few measurements I have found deviations between 0 and 3°, especially owing to changes in the temperature of the air. An example

[1] Ostenfeld (Meddelelser fra Vejlefjord Sanat. IV. 1904) in 18 cases frequently found the temperature of the mouth to be higher than the rectal temperature. Unfortunately he has not studied this interesting problem very closely. (Stomatitis?)

like the following probably gives the approximate relation between the changes in the local temperatures dealt with here.

	Temperature in	Rectum	Mouth	Forehead
11/8 06.	11,55 a. m. before meal	36·86° C.	36·25° C.	32·50° C.
—	12,40 p. m. after meal	37·09° -	36·68° -	33·35° -

Work and temperature.

The temperature changes occasioned by the meals are, however, but small compared with those due to the work of the muscles. This is the factor which more than any other determines the variations of the rectal temperature. The more sedentary one's mode of life is, the straighter is the course of the temperature curve, and on the other hand every bodily exertion will appear the more distinctly in the curve and change its regular shape; the more the bodily work, the higher the whole curve will be — other conditions being the same.

It lies in the nature of the case that no average value can be given for the rise of temperature occasioned by the work of the muscles; nor can the maximum and minimum values be found, the lowest limit especially being undeterminable. The uppermost limit will be individually very variable according to the muscular development and training of the person corcerned. Some examples show best the extent of these variations

	Time	Rectal temperature	
	8 a. m.	36·53°	After sleep.
	9 a. m.	37·32°	— breakfast and a little exercise.
The following day	8 a. m.	36·40°	— sleep.
	9·30 a. m.	38·09°	— breakfast and cutting ice for 20 minutes.
	9·40 a. m.	37·00°	— breakfast and rest.
	11·50 a. m.	37·89°	— walking 1½ hour in ÷ 27° C.
	9·30 a. m.	36·70°	— breakfast in tent in + 5° C.
	10·30 a. m.	38·15°	— ascending a mountain in ÷ 12° C.
	12·15 p. m.	38·45°	— shovelling snow for two hours.
	4·55 p. m.	38·75° (st.)	— shovelling snow in ÷ 7° C.
	3 p. m.	38·51°	— cutting ice for about ½ an hour.

The temperature is repeatedly higher than what is generally considered the "normal". As the temperature varies, however, with the intensity of the metabolism, there is hardly anything remarkable about these figures. And if by fever is meant a pathological not a physiological condition, it is quite a mistake to call such temperature febrile[1]. In this connection it may be mentioned that HILL & FLACK[2] have measured temperatures of up to 105° F. in English sportsmen after fatiguing exercise.

The rise of the temperature is naturally not unlimited. The limit occurs on the appearance of fatigue. The rise is therefore often greater after a short, intense exertion than after protracted toil; on beginning very energetically so that fatigue appears before the work is finished, the temperature will sink in spite of all. The stronger the muscular development is, therefore, and the longer one is able to keep on working energetically, the higher the temperature will rise, the conditions otherwise being the same.

The temperature heightened by muscular work and especially that raised comparatively highly and suddenly by forced work, falls again very sharply and suddenly, especially if the transition to rest is abrupt; I have seen the temperature fall 1° during 10 minutes; but, as a rule, the reduction takes somewhat longer.

Time	Rectal temperature		
11·05 a. m.	38·3° (st.)	After	work outside.
11·38 a. m.	37·5° -	—	staying indoors.
11·25 a. m.	38·45° -	—	work outside.
1·05 p. m.	36·91° -	—	staying in temperate room; lunch.
1·35 p. m.	37·80°	—	lunch and 1/4 hour's fencing.
3·40 p. m.	36·75°	—	sedentary work indoors.

This fall of temperature, not to be stopped by anything else but renewed, vigorous exertion of the muscles, must be assumed to be due to a deficient regulation of heat. It is only gradually that one becomes adjusted to a maximum output of heat, with consequently the sharp rise in temperature; but when this adjustment is once reached, it lasts even after the production of heat

[1] In "Sechste Versammlung der Tuberkuloseärzte Deutschlands" (Zeitschrift f. ärztliche Fortbildung. VI. Jahrg. No. 14, pag. 458—59), Karl Meyer maintains on the basis of 33,000 mouth measurements and 8,000 rectal measurements that every temperature above 37·3° is abnormal.

[2] Proceedings of the Physiological Society, July 20, 1907.

has become normal, perhaps subnormal; then we have the fall of temperature.

The relation of temperature to work has however two phases, and in this case we obtain as good information from the negative as from the positive. On sitting still the temperature is lower than during exercise, in a quiet, reclining position still lower and during sleep lowest[1]. Sleep itself does not tell upon the temperature; the fall of temperature is exclusively due to the fact that the organs which produce heat have reduced their activity to a minimum. This holds good especially for the work of the muscles, but of course it ought to be taken into consideration that the other main factor in the production of heat, the digestive function, is generally resting just within the same period. The fall of temperature during sleep is exceedingly variable; it is greatest when the sleep comes after an active period, least when some hours have been passed in quiet sedentary work previously. The morning temperature (temperature on waking up) is however fairly constant under ordinary conditions for the same individual; 72 morning measurements at different seasons (all self observations) gave as average $36{\cdot}46° \pm 0{\cdot}0013$, $\mu = 0{\cdot}14$ or $0{\cdot}38\,\%$ of average.

In 57 measurements taken twice, before and after sleep, in 4 different individuals, I have 47 times found negative deviations, 8 times positive and twice no difference, the average deviation being —0·46°. Of these 32 with average —0·45° fall in the "dark period", 25 with average —0·49° in the light period of the year. On dividing the cases into two groups according as the sleep took place between 8 p. m. and 8 a. m. or between 8 a. m. and 8 p. m., for sleep in the night (30 cases) there is an average deviation of —0·47° and for sleep in the day (27 cases) of —0·45°. For the summer, at night (14 cases) the average was —0·46°, in the day (11 cases) —0·52°; for the winter, at night (16 cases) the average was —0·49°, in the day (16 cases) —0·41°; 14 cases of 1—3 hours sleep gave an average deviation of —0·54°.

There is a tendency in these numbers which agrees with other observations on sleep and strengthens the view, that the fall of temperature is due to the fact that muscular activity during sleep is at a minimum. It is well-known, that the first hours of sleep are the soundest and quietest and the sleeper wakes with greater difficulty than when the sleep has lasted for a longer period. The quite short sleep of 1—3 hours shows, indeed, a greater fall of temperature than all measurements taken on the whole, i. e. the fall

[1] Cf. Johansson's researches in Skand. Archiv f. Ph. 7, 1897.

of temperature takes place rather soon, and then the temperature remains invariable, eventually rising a little again. This is confirmed by a few measurements.

On waking after 5 hours	On waking after 10 hours
36·39° C.	36·69° C.

Experience goes to show that during the "dark period" the sleep is worse than during the summer. In a single individual during the winter period, when his sleep was particularly bad, I have made 17 measurements taken twice, of which 6 with negative, 9 with positive values and 2 with no difference.

At a period during the winter, which will be discussed more closely below, we slept in the day-time and worked during the night. In some the transition rendered the sleep troubled during the first "nights"; there was consequently less difference when sleeping in the day than in the night during the winter period. Conversely, the difference was greatest when sleeping in the day during the summer months, because as a rule the sleep was quite short then.

With regard to the mouth temperature, its relation to the work of the muscles is quite inconstant. As a rule it does not rise during work. On working indoors it is almost constant, on working in the open air it always falls; but in a single case I have found it so much raised, while staying indoors soon after working in the open air, that the rise must without doubt be ascribed to the work. On the other hand, I have seen that even energetic exercise indoors was not able to prevent the mouth temperature, raised by the meal, from falling.

Time	Mouth temp.	
12 noon	36·15° C.	
1 p. m.	36·60° -	After lunch.
1·55 p. m.	36·38° -	— fencing.

Nor are the lowest values, as for the rectal temperature, obtained on waking — the morning temperature varies somewhat strongly according to outer conditions — but on being in the cold air. Thus, the mouth temperature does not follow the changes of the rectal temperature during sleep.

Time	Mouth temp.	Rectal temp.	
10·40 p. m.	36·05° C.	36·80° C.	After being in the open air.
12 night	35·50° -		—
7·15 a. m.	36·40° -	36·50° -	— sleep.

For the temperature of the skin pretty much the same holds good as for the mouth temperature; as a rule, I have not been able to notice any rise occasioned by muscular work.

Time	Temp. of the skin (forehead)		
2·30 p. m.	27·5° C.	On upper deck of ship,	windy.
4·45 p. m.	28·3° -	—	almost calm.

The last temperature was measured after some hours' energetic exercise. The rectal temperature was not measured; but by analogy with previous days it may be estimated at about 37° before work, and at about 38·4° after. Making allowances for the wind being less, it may without doubt be concluded that the temperature on the forehead had not risen during the work.

There is however one special form of work, brain work, which is able to influence appreciably the temperature on the forehead (on the hairy part of the head I have not been able to measure the temperature). With light reading the temperature rises a few tenths, with more strained work somewhat higher; fixed numbers cannot be given.

Time	Rectal temp.	Mouth temp.	Skin temp. (forehead)	
10·45 p. m.	37·2° C.	36·4° C.	34·6° C.	In temperate room.
11·45 p. m.	36·75° -	36·18° -	34·2° -	- after 1 hour's reading.

In the above case the rectal temperature falls, even rather considerably, owing to rest after some exercise; the mouth temperature also falls, though less, probably owing to the temperature in the room decreasing; but the temperature of the skin, otherwise so variable, does not take part in the decrease; owing to the work of the brain it remains almost unchanged[1].

During sleep the temperature of the forehead does not undergo any definite change. If sleeping with a blanket over the head, the temperature on waking will be found to be high in comparison with the rectal temperature and the mouth temperature, and then it will fall rather considerably while dressing in a cold room; while the other temperatures recorded will fall quite inconsiderably or remain unchanged.

As the heat of the body is not, or at any rate only to a slight extent, produced at the places where the temperature is measured, the heat must be imparted to the latter indirectly through the circulation. The more energetic the action of the heart, the sooner

[1] Cf. the further measurements on page 35.

and more equally must increased heat of the blood become perceptible in the various organs. As a measure of the action of the heart, the frequency of the pulse may in sound individuals be employed, and this, other things being the same, gives a relative expression for the quantity of blood sent peripherally in a given time.

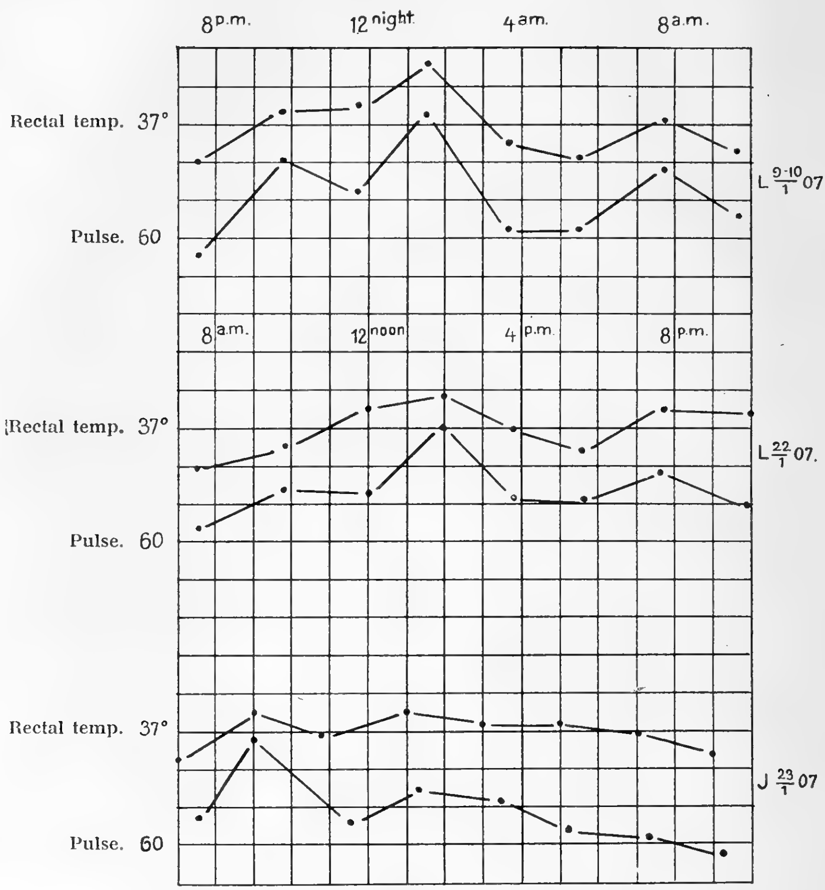

Fig. 2a.

On the accompanying plan curves for the rectal temperature and frequency of the pulse are drawn on the scale $0{\cdot}1° = 2$ pulsations $= 1$ mm. A well-marked parallelism is seen to be present between the two functions, more distinct than we can always expect it to be[1]. The frequency of the pulse is namely subject to nervous influences in no connection with the heat-producing processes. And it is only under a completely regular mode of life and when unrelated factors are as far as possible removed, that the agreement

[1] Cf. TIGERSTEDT and others; Skand. Archiv f. Physiologie 7, 1897, Table II.

indicated appears distinctly; but the practically complete parallelism between the two curves indicates that the action of the heart is one of the factors which determine the rectal temperature.

The curve of the pulse is the best marked of the two curves, giving on an average the greatest fluctuations, especially for the maxima and minima, which is quite reasonable, as the rectal temperature is the secondary function[1]. In the two upper double curves, both derived from self observations, on days with some bodily exercise — the one from the "inverted" day — inversions occur in the forenoon, derived from the comparatively greatly increased frequency of the pulse after breakfast. In the third curve, derived from another individual on a day with sedentary work, the rise after the last meal is absent, which is no doubt due to the fact,

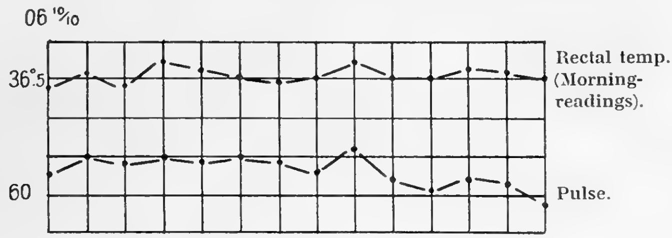

Fig. 2 b.

that the temperature was not measured immediately after this meal and that the person concerned ate unusually little on this occasion.

The agreement between morning temperature and frequency of the pulse day by day is more difficult to prove, but no doubt exists. The difficulty is due to the pulse. In the morning on waking, there is as a rule a more or less distinct desire to micturate and this accelerates the frequency of the pulse, often rather considerably; after micturition the frequency of the pulse decreases and after a short time rises again.

Frequency of the pulse before and after micturition

61	52	
68	50	
70	66	
	61—64	
67	55—66	After dressing.

The curve shows the relation between temperature and pulse in daily, morning measurements; as the relation dealt with above

[1] Investigations by Bock (Arch. f. exper. Pathologie u. Pharmacologie. Suppl. 1908 Pag. 83). Make it probable that the relation between the frequency of the pulse and the temperature is rather more complicated.

between the pulse frequency and micturition was not known to me when these measurements were made, no information concerning micturition occurs.

As a rule a day-curve for the mouth temperature or a series of morning measurements do not present any similarity in appearance with the corresponding pulse curves. As the temperature of the mouth, in a person staying quietly in the ordinary temperature indoors, shows, however, a tendency to vary together with the rectal temperature, parallel curves can also in such cases be found for the pulse and mouth temperature, as appears from the Fig. 2c.

The fluctuations are here much stronger than in the morning curves, which follow an almost straight line, and the agreement therefore appears more distinctly. It must however be remarked,

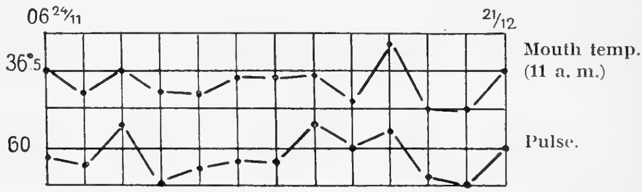

Fig. 2 c.

that the temperature in the room varied at the same time along a corresponding curve, a circumstance not without importance in considering the unusual harmony here between the two functions examined.

It is well-known, that the state of contraction of the peripheral arteries varies with changes in position; this has been shown by Oliver[1] for the radial and temporal arteries by direct gauging of the calibre of the arteries, arteriometry. According to Oliver the calibre is usually reduced in a recumbent position compared with the readings in the active positions at about the same time. On using the arteriometer of Oliver I have come to the same result.

Bouchard and others[2] have now confirmed that the rectal temperature in recumbency is "some tenths" lower than in an upright and sitting position; Benedict[3] has also found a higher temperature in the upright than in the sitting position. I have been unable however to confirm this.

[1] George Oliver: Pulse-Gauging. London 1895.
[2] Cit. Oliver, l. c. p. 24.
[3] The American Journal of Physiology. Vol. XI, No. II, 1904.

On measuring the temperature in the rectum after sedentary work alternately in a upright (st) and a lying (r) position, we find results which vary but little from one another and without any fixed tendency.

st	r
36·95° C.	37·00° C.
36·98° -	36·99° -
37·07° -	36·93° -
37·01° -	37·01° -

The temperature was first measured in the st and then in the r position at intervals between the individual measurements sufficiently long for the necessary cleaning and reading of the thermometer and the noting of the results.

If, however, we measure the temperature after standing still or moving slowly about, the result differs somewhat.

st	r
36·96° C.	36·90° C.
36·92° -	36·72° -
36·66° -	36·69° -

Much the same is the case when the temperature has been raised owing to work.

		st	r
22/5 07	11·05 a. m.	38·30° C.	
	11·18 a. m.	37·99° -	37·85° C.
	11·28 a. m.	37·80° -	37·65° -
	11·38 a. m.	37·50° -	37·50° -

or

1/2 08	4·23 p. m.	38·00° -	37·79° -
		37·79° -	37·65° -
		37·58° -	37·50° -
	5 p. m.	37·42° -	37·40° -

It is common to these 3 series that the second reading in the st position only varies a little and in a variable direction from the first measurement in the r position; in other words, there is a tendency towards lower temperatures in the r than in the s position; but this soon vanishes on the measurements being continued. We then find distinct falls in both positions, and then the temperature becomes the same for the st and r positions, as in the example first given.

The fluctuation in temperature, which differs completely from the changes of temperature in the mouth and on the forehead with varying positions of the body, as shown below, seems to me mainly suggestive of stasis. The mucous membrane of the rectum is very vascular and the venous side of the circulation especially presents peculiar characteristics; it is well-known that the very complex network of large and much anastomosing veins, which forms the Plexus hæmorrhoidalis, leads very readily to stasis; at the same time the conditions for measuring the temperature are exceedingly favourable, as the thermometer is completely and exactly surrounded by the very vascular tissues and the local loss of heat is slight. Thus, the anatomical conditions for stasis are always present, as also the physiological whenever one changes to a resting position after energetic exercise, in less degree in any standing and certain sitting positions. On trying to produce stasis consciously, the temperature will be found to be a few tenths lower in a reclining than in a standing position. Lastly, some of the above temperatures measured at different heights in the rectum, may probably be explained on these suppositions.

Without venturing to express any definite opinion, I consider it probable that the peculiar curve obtained by continued measurements of the rectal temperature in the st and r positions, is due to stasis in the Plexus hæmorrhoidalis, that the static factor conseals the eventual changes in temperature owing to the contraction of the arteries, and that the observers who have found a lower temperature in the r than in the st position, have only taken a single measurement without doing justice to the static factor.

The mouth temperature also varies with changes of position; in the following example it was measured alternately in the sitting (s) and recumbent (r) position. The variations of the mouth temperature in this series are typical.

	Forehead		Mouth	
	s	r	r	s
29/12 07		33·10° C.	36·13° C.	
	33·24° C.			36·24° C.
		33·59° -	36·15° -	
	33·83° -			36·30° -
		34·23° -	36·17° -	
	34·30° -			36·27° -
		34·32° -	36·17° -	

If the temperature falls or rises during the course of the measurements, the type becomes less conspicuous but is easily recognised on a closer examination of the figures.

		Mouth temperature s	r	
24/5 07	4·55 p.m.	37·30° C.		After hard work in open air (temperature taken in room).
	5·30 p.m.	37·35° -	36·90° C.	After meal.
		36·94° -	36·70° -	
		36·70° -		

The temperature, probably heightened in the first instance by the work with subsequent stay in temperate room, rises but little owing to the meal and then falls rather quickly; but the fall is not even, there is a pause or even a slight rise on the transition from the r to the s position, but on the opposite transition the fall is evident; in other words, there is a tendency to higher temperatures in the s than in the r position. This appears more distinctly from other series.

	Forehead		Mouth
13/10 06	34·70° C.	r	36·89° C.
	34·95° -	s	36·90° -
	35·35° -	st	37·00° -
	35·25° -	s	36·98° -
	35·05° -	r	36·80° -
	34·36° -	s	36·80° -
	33·97° -	s	36·77° -
	34·56° -	r	36·66° -
	34·50° -	r	36·60° -
	33·80° -	s	36·65° -

During the first 3 measurements the temperature in the room was rising, then falling, and during the last 5 almost constant. Each measurement took 5 minutes, simultaneously on the forehead (glabella) and in the mouth. In the temperature of the mouth we see the same tendency as above but more distinctly, especially in the falling temperature of the last 7 measurements; appreciable fall from s to r, a slight rise or at any rate no fall on transition from r to s.

The case is reversed with the temperature of the skin. When the temperature in the room falls most strongly, there is a slight fall from s to r (4th and 5th readings), then under the uniform temperature of the air a distinct rise from s to r and a distinct fall from r to s. The fluctuations are here much greater than for the mouth temperature.

The temperature on the forehead is, as already remarked, subject to mental influences, and, as appears from the following series, this may tell disturbingly on the measurements. During the last two measurements my attention was greatly distracted by conversation outside my door.

	Forehead r	Forehead s
28/12 07:		33·25° C.
	33·78° C.	
		34·10° -
	34·29° -	
		34·19° -
	34·70° -	
		35·00° -

While the temperature on the forehead always shows the same tendency, the mouth temperature may sometimes remain unchanged or assume another type when the readings are taken soon after meals.

In the first of the groups noted, taken before meals, the course followed by the mouth temperature is as usual, even if the tendency is not so well-marked; in the next group the mouth temperature is constant in spite of changes in position.

		Forehead		Mouth	
1/2 08	11·39 a. m.	34·20° C.	r	36·34° C.	Before meal, hungry; meal in progress in neighbouring room.
		33·87° -	s	36·36° -	
		34·05° -	r	36·36° -	
		33·50° -	s	36·46° -	
	12·45 p. m.	33·51° -	s	36·35° -	After meal; room colder.
		34·10° -	r	36·36° -	
		34·02° -	s	36·35° -	
		34·25° -	r	36·35° -	

In spite of the fall of temperature in the room, there is a slight rise in the mouth temperature, probably because "my mouth watered" on hearing others feeding.

In the next series the mouth temperature is undoubtedly higher in the r than in the s position.

		Forehead s	Forehead r	Mouth s	Mouth r	
25/5 07	8·05 a. m.	29·85° C.		35·60° C.		After dressing.
	8·45 a. m.	29·85° -		36·30° -		— breakfast.
			30·70° C.		36·40° C.	
		30·20° -		36·35° -		
			30·65° -		36·30° -	
		29·85° -		36·20° -		
			30·10° -		36·30° -	
		29·70° -		36·20° -		

The same is again seen in the last of the groups below, but the temperature on the forehead always reacts in the same way towards changes in position.

$^{27}/_{5}$ 07:	Forehead s	Forehead r	Mouth s	Mouth r	
7·55 a. m.		30·80° C.		35·90° C.	Soon after waking.
8·10 a. m.		30·10° -		35·83° -	After dressing.
	30·35° C.		36·10° C.		
		30·40° -		36·17° -	
	30·05° -		36·20° -		
9·40 a. m.	32·30° -		36·40° -		After breakfast, room warmer.
		32·80° -		36·50° -	
	32·00° -		36·35° -		

In the examples hitherto given no regard has been paid to the temperature in the st position; but according to the available measurements this seems to be like s. The question cannot, however, be answered decisively, as I specially noted in this position the irregularity produced by the "unevenly distributed" outer temperature. When the room was at all heated, and this was somewhat necessary with continued readings of the temperature, the temperature varied very appreciably at the ceiling and on the floor. Under favourable conditions, on swinging the thermometer a little to and fro, I found for the st position + 13·5°, for s + 11·5° and for r + 10·5°; but sometimes I have seen my water-jug, which stood on the floor beside the lighted petroleum-oven, freeze to the bottom, while there was at the same time more than + 20° at a man's height. The berth was, however, placed at such a height above the floor that the s and r positions differed but little as a rule; but there was always a perceptibly higher temperature in the st position.

$^{2}/_{9}$ 06:	Forehead		Mouth	
3·15 a. m.	34·6° C.	s	36·55° C.	Temp. higher in st than in s position.
	34·7° -	st	36·45° -	
	34·8° -	s	36·50° -	
	35·0° -	st	36·50° -	
	34·8° -	s	36·45° -	
	34·8° -	st	36·45° -	
$^{5}/_{9}$ 06:				
2·40 a. m.	32·3° -	st	35·9° -	Temp. higher in st than in s position.
	32·5° -	s	35·8° -	
	32·6° -	st	35·8° -	
	32·5° -	s	35·8° -	
	32·6° -	st	35·8° -	
	32·45° -	s	35·75° -	

While the temperature on the whole first rises and then falls, there is for the temperature on the forehead a slight tendency to rise in the st position. In the slightly falling mouth temperature there is however no change to be observed owing to the change in position.

In his work cited above, Oliver records that the calibre of the arteries decreases after meals in the active positions and the type of contraction changes, the greatest calibre being found in the lying position. With regard to the first observation, I am at variance with Oliver, as I have found as a rule the greatest calibre after meals in artery-gaugings in the s position; but on this point Oliver is also at variance with himself, as he maintains as a principle that the diameter of the artery follows "the temperature of the body". And it was noted above, that the temperature, whether measured in the rectum or in the mouth rises at meal-times. With regard to Oliver's second assertion, that the calibre is greatest in recumbency after the meal, I have now and then been able to confirm its correctness; my measurements are not many, however, so that I cannot tell how often this is the case. But just as I have occasionally found increased calibre of artery in r after a meal (and at the same time apparently a changed static reaction[1] in the number of leucocytes), I have also, as stated above, now and then found a higher mouth temperature after a meal in the r than in the s position. Whether these cases hold together, I do not know, as the measurements, at any rate under the conditions given, could not be made at the same time; but a certain probability seems to me to speak in favour of this taking place. It will be shown later on, however, that the mouth temperature may in certain cases follow the temperature of the skin on the forehead, in falling a little on the latter rising under conditions where a rise was to be expected. It is therefore not impossible that the slight fall in the mouth temperature on transition to r position is due to the temperature of the skin rising at the same time. Against this however we have a series of measurements from $^{29}/_{12}$ 07, where the mouth temperature falls distinctly in the r position, while the temperature on the forehead is but very little changed. Lastly, it ought to be observed, that the air temperature is always a little lower in the r than in the s position. This hardly influences the results, however, as the mouth temperature will not be able, during such a short time as is occupied by a measurement, to react to a difference of temperature

[1] See Hasselbalch & Heyerdahl: Det Kgl. danske Videnskabernes Selskabs Forhandlinger. 1907, No. 5.

of one degree. It may be assumed therefore that the variations in the mouth temperature on changes in position are a direct consequence of the latter.

The change of temperature on the forehead cannot be mistaken; it rises when the r position is occupied, to fall on changing to an active position.

As for the variations of temperature due to stasis or changes of position they play on the whole a secondary part, while on the contrary the temperature of the air is a factor of paramount importance, especially in regard to the temperature of the mouth and skin; its effects are undoubtedly apparent also in the rectal temperature, but here much less conspicuous.

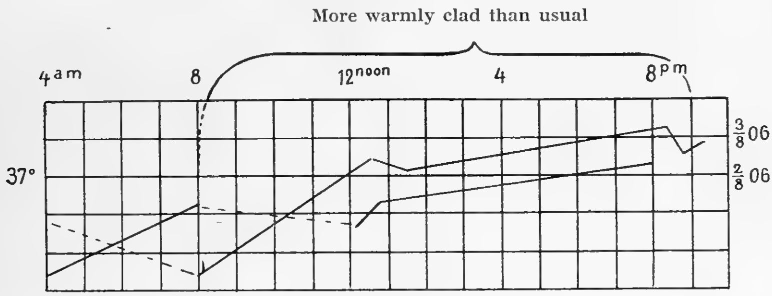

Fig. 3.

As we are practically always in active movement when in the open air in severe, cold weather, a comparison of day-curves for summer and winter will not show the influence of the temperature of the air. The curve will come to be a little higher for the winter, if anything; when out, one is in movement, the rooms are as far as possible heated and one is more warmly clad. But the very fact, that the demands mentioned must be met in order to keep well, proves that the outer temperature influences the heat of the body on the whole.

The two curves above are derived from measurements on the same individual on 2 successive days. In the morning of the second day he put on much thicker and warmer clothing than he was accustomed to wear. His rectal temperature proved higher both on this and the following days than on the foregoing, his mode of life being otherwise quite the same.

On the ship we could defend ourselves against the low temperature of the air, but this was not the case on journeys; even if the temperature was a little higher in the tent than without and even if the sleeping-bag contributes to the further equalization of the temperature, the latter varies nevertheless appreciably summer and winter. As the temperature on waking, as shown above, is fairly constant, we have a practical means of comparing temperatures taken at this time on journeys under varying external temperatures.

From a journey in April 1907 at an air temperature between 12° and 31° below zero, I have 10 measurements taken immediately after sleep with an average of 36·26°, μ = 0·16; from a journey in May-June in the same year 15 measurements with an average of 36·52°, μ = 0·24, when the air temperature was about 0°. All the measurements were made on the same individual, who employed the same sleeping-bag, while wearing very much the same clothing and living on the same food. On the journey homeward the person concerned was stoker and slept in the comparatively greatly heated rooms near the machine; for 8 measurements directly after sleep I found an average of 37·0°, μ = 0·125. There is hardly any doubt that the variations are due to the outer temperature.

The decrease in the temperature heightened by the work of the muscles is more conspicuous when staying in the open air than indoors.

Time	Rectal temp.		
11·25 a. m.	38·45° C.	(st)	After work outside.
1·05 p. m.	36·91° -	(st)	— staying indoors (but soon after defæcation outside.
1·10 p. m.	37·02° -	(r)	— —
12·15 p. m.	38·45° -		— work outside.
1·55 p. m.	37·33° -		— stay indoors.
10·30 a. m.	38·15° -		— hill-climbing (tp. — 12·2°).
12·30 p. m.	36·20° -		— measuring work (tp. — 10·5°).
9·30 a. m.	37·80° -		— hill-climbing (tp. — 6·2°).
12·30 p. m.	37·00° -		— measuring work and lunch (tp. — 5·2°).
4·15 p. m.	36·05° -		— — — (tp. — 6·0°).

Although the fall of temperature is usually the more conspicuous the higher the temperature has been, such low values as in the examples last given are not found when staying indoors, and this in spite of the measuring work claiming always some, even if slight exercise, and although one must often "clap hands" and the like to be able to work with the telescope screws.

It will be instructive to consider the temperature of the mouth in relation to the rectal temperature. Such a comparison is shown on the Plan below. The continuous line refers to the rectal temper-

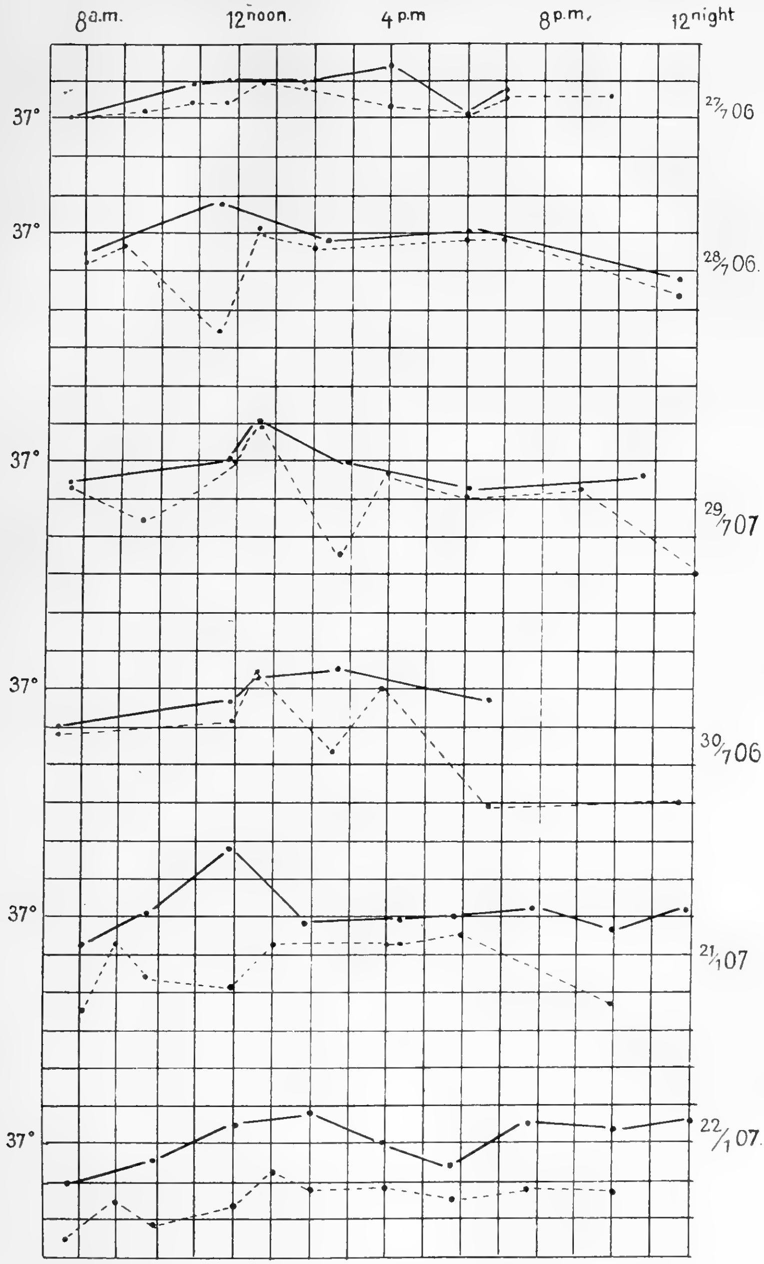

Fig. 4.

ature, the broken line to the mouth temperature. For the rectal temperature especially the measurements in the summer curves are rather incomplete, made at too great intervals; nevertheless they give a somewhat reliable picture of the changes of temperature on the days concerned. It will be seen, firstly, that the curves for the two temperatures are not parallel, also that the summer and winter curves vary distinctly; and on closer examination this difference appears to be: In summer in the morning measurements the two temperatures are almost equally high, about 36·5°; in winter the rectal temperature at the same time of day is nearly the same as in summer; but the mouth temperature is considerably lower, about 35·7°. The distance between two curves of the same set when approximately parallel is also generally greater in winter than in summer. The reason is that the summer measurements were made while sailing, at a time when all rooms were rather well-heated from the machine of the ship, but in winter the heating of the room by means of a petroleum-oven was, as a rule, insufficient, and the temperature in the cabin especially in the morning was generally below zero.

The greatest differences between two simultaneous temperatures, due to staying in the open air, are nearly the same summer and winter, reaching about 2°, but they are produced in different ways. In summer they are practically always due to fall in the mouth temperature; there is even several times a fall of the rectal temperature at the same time; in winter the fall in the temperature of the mouth is less well-marked, but at the same time there is a rise in the rectal temperature.

This comes from the fact, that while sailing we would sit on the deck for hours with the head uncovered at a temperature of about 2° C., but in winter one was constantly in movement, and except for the central part of the face the head was covered by a helmet-shaped camel's hair cap and wind-tight hood; consequently, the cold air could not act directly, as in summer, on the skin of the chin and neck. All measurements were made indoors, a fact more likely to contribute to equalize than to emphasize the difference between curves of one set.

Diagram V shows directly the dependence of the temperature of the mouth on the air temperature. The two upper curves are derived from measurements made every second day, about 10·45 a. m., 2 hours after breakfast in a heated room. In the whole series there is but one real disagreement, where the mouth temperature is lower than was to be expected from the air temperature. I have not noted down what I had been doing just before the measurement; but a departure in this direction would be readily explicable, if, shortly before the reading, I had been in a cold room or in the open air.

The lower pair of curves are morning measurements made daily about 6·15 a.m. The lower "air curve" is seen to force down the temperature of the mouth taken on the whole, and the fluctuations here are much smaller than in the uppermost curve. In this curve also the mouth temperature is at one place a little lower than was to be expected, but apart from this there is complete agreement.

This direct dependence of the mouth temperature on the temperature of the surrounding air has been shown earlier by AGNES BLUHM[1]; but her researches do not seem to have roused any special interest for the question. They are cited by OSTENFELD[2], who only confirms however that the relation between the rectal temperature

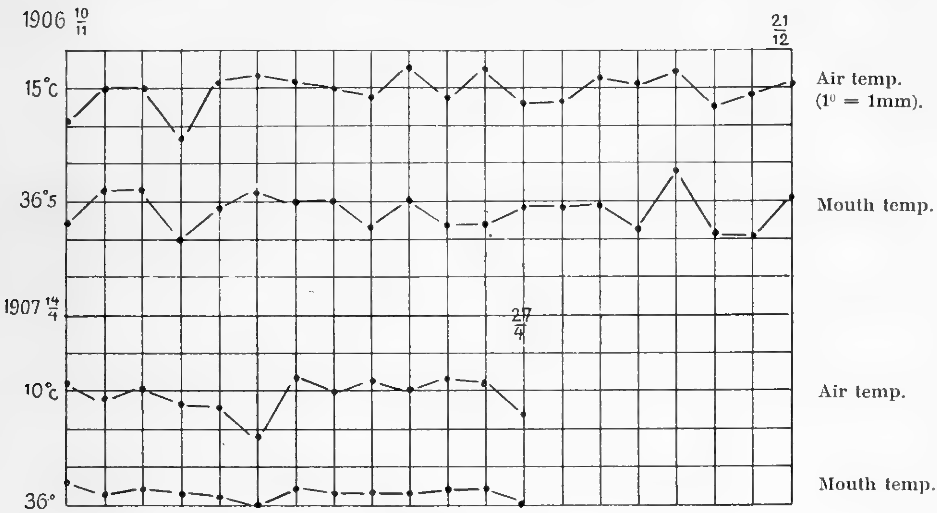

Fig. 5.

and mouth temperature is variable, which research, if the observations of AGNES BLUHM are correct (and this OSTENFELD does not deny), seems rather superfluous.

The loss of heat from the mouth takes place directly through the soft parts, not by cooling down from the respired air; this appears from AGNES BLUHM's and my own winter measurements, as covering of the face and neck prevents the fall of temperature. A lower temperature is, for example, found in the sulcus alveolo-buccalis than in the sulcus alveolo-lingualis and the thermometer takes a longer time to become adjusted in the former whilst, on the other hand, the temperature is sooner adjusted here than in the sulcus alveolo-lingualis.

[1] Zeitsch. f. Tuberkulose u. Heilstättenwesen, Bd. 2, 1901.

[2] Meddelelser fra Vejlefjord, IV, 1904.

		Sulc. alv. ling.	Sulv. alv. buccal.
19/5 08	7·55 p. m.	36·95° C.	36·48° C.
		36·90° -	36·40° -
		36·80° -	36·17° -
		36·78° -	36·15° -

All measurements were taken in the s position in the right part of the mouth after staying in a warm room.

Like the mouth temperature, the temperature of the skin is very much dependent on the outer temperature. This has been shown by OEHLER[1] whose work will be dealt with later.

The condition for this appearing distinctly however, on measuring indoors and reading the temperature on the head, is that one's brain-work should be uniform during the measuring period, this, as above-stated, being of no slight influence at any rate on the temperature of the forehead. If the temperature round the right and left side of the head is not the same, the direct dependence of the skin temperature on that of the air can be proved without paying any regard to the work of the brain.

	Time	Temp. in room	Temp. on forehead
16/5 07	10·30 a. m.	2·5° C.	30·9° C.
	11·35 a. m.	5°—10° -	32·4° -
	1·15 p. m.	18·5°—19° -	34·0° -
	2·15 p. m.	21° -	35·7° -
	3·15 p. m.	17° -	34·4° -

All readings were taken in an s position, the measurements each 10 minutes. During the whole period I was engaged in writing, except during lunch from 12 to about 12·30. Where two numbers are given for the temperature in the room, the latter has risen so much during the measurement as is represented by the difference.

	Cheek (s)		Mouth (s)	
	r	l	r	l
20/5 07 8·10—9·15 p. m.	35·80° C.	33·50° C.	36·65° C.	36·70° C.
	35·50° -	34·50° -	36·60° -	36·52° -
	35·50° -	34·90° -	36·48° -	36·50° -
			36·48° -	36·50° -

During these measurements the right cheek was at a distance of 1·5 meters from the stove, wherefore the radiating heat was at

[1] Deutsches Archiv f. klin. Medd.; Bd. 80, 1904, p. 245.

the beginning felt appreciably; the left cheek was at a distance of hardly 0·5 meter from the outer wall. The temperature in the room, read from a thermometer near the head of the person experimented on, fell from 21° to 18° C. during the readings. The temperature was measured at the same time on the right cheek and the left side of the mouth.

In the following measurements the right cheek was at a distance of 0·4 meter from a lighted petroleum-oven with a cold draught of air on the left side of the face.

		Temple (s)		Mouth (s)	
		r	l	r	l
1/2 08	10·40 a. m.	33·82° C.	32·83° C.	36·35° C.	36·43° C.
		34·00° -	33·50° -	36·48° -	36·50° -

In both these cases the skin thermometer was sheltered against the radiating heat by the hand. But besides covering the thermometer the hand sheltered part of the face against radiation and on the left side the skin from the cold to some extent; this is probably the reason why the temperature on the two sides, distinctly different at the beginning, gradually becomes somewhat equalized. It appears for the rest from the measurements, that the skin temperature reacts much more strongly to the difference in the air temperature than the mouth temperature does. The latter displays an unmistakable though slight tendency towards varying in an opposite direction to the skin temperature.

On staying in the open air it is more difficult to prove the direct dependence of the skin temperature on the air temperature, as other meteorological factors, wind and moisture, also influence the results.

		Rectal temp.	Mouth temp.	Forehead temp.	Air temp.	
2/9 06	12 midnight	36·8° C.	35·9° C.	27·8° C.	1·1° C.	
	4 a. m.	36·9° -	36·0° -	25·3° -	0·5° -	wind

In spite of the slight fall in the air temperature, there is a considerable decrease in the temperature of the forehead, which must undoubtedly be attributed to the wind.

2/9 06:	Retal temp.	Mouth temp.	Forehead temp.	Air temp.	
10·45 p. m.	37·20° C.	34·40° C.	34·6° C.		In room.
11·45 p. m.	36·75° -	36·18° -	34·2° -		- — 1 hour's reading.
12·15 a. m.		35·80° -	24·6° -	2·8° C.	Strong wind.
3/9 06:					
4 a. m.	36·55° -	35·90° -	28·5° -	3·4° -	Nearly calm.

The first two measurements have been dealt with before. The third measurement was made in continuation of the second, but not until I had been standing for a quarter of an hour on the upper deck of the ship in the strong wind. Though the temperature was not very low, the cold was felt appreciably. Both the mouth and skin temperatures fall, the latter even more than usually, partly owing to the wind, perhaps also as reaction after the relatively high value obtained by the second measurement.

In the last reading, 4 hours later, the rectal temperature has fallen a little owing to continued sedentariness; the mouth temperature displays a slight rise corresponding to the slight rise in the temperature of the air, but the skin temperature has risen considerably, much more than was to be expected from the difference in the air temperature; for the wind had subsided.

When staying in a heated room the temperature on the forehead and other parts of the skin does not differ very much.

Temp. in room	Temp. on forehead	Temp. on wrist
5° C.	30·05° C.	
8·5° -		31·9° C.

On the temperature, however, becoming so low that the cold is felt inconveniently, the difference may be considerable. After sitting still about 1½ hours at about 0° C. I found

on left	thenar	19·2° C.
—	wrist	19·6° -
	forehead	28·1° -
	mouth	36·45° -

I felt pains in the hands and fingers owing to the cold.

That the skin temperature is mainly dependent on certain atmospheric conditions and especially on the air temperature and does not generally rise and fall with the rectal temperature is also proved by direct observation on journeys in arctic regions.

On taking a walk at a low temperature, about 30° C. below zero, although the rectal temperature rises, the uncovered skin, especially the parts round the nose and mouth, are exposed to frost-bites. In wind the danger is increased. I have seen wide-spread frost-bites of the 1st and 2nd degree on the head, shoulders, arms and back at 32° below zero and strong wind, although the individual was in energetic movement. This mutual independence of rectal temperature and temperature of the skin was very distinctly displayed on a sledge journey at 36° below zero. We were two men on the sledge, sitting on it and running behind by turns. Sitting on the sledge one felt cold by and by and stiff in the joints; according to experience the rectal temperature sinks under such conditions after previous exercise; but it was only now and then that the dismal white spots appeared on the face. But on running behind the sledge, which was very tiring in travelling dress and produced a considerable hyperpnoe and after a short time strong perspiration in spite of the low temperature, frost-bites appeared almost immediately on the nostrils, probably because a considerably greater quantity of the dry, cold air was passing through the nose while running, the rate of progression being continually the same.

Physiologically the temperature taken in the axilla undoubtedly belongs to skin temperature. The measurements which have been made to show that the axillary temperature is constant in relation to the rectal temperature cannot be used, no regard whatever having been paid to the possibility, that the axillary temperature might be dependent on the surrounding temperature.

Thus, Erlandsen[1] has recently found, by means of a long series of measurements, that the axillary temperature is on an average 0·5° lower than the rectal temperature, the difference varying indeed individually but being invariable in the same person. The fluctuations are, however, very great; the author himself states that the single differences may be 0 or even change sign, so that the constancy is not at all apparent. Add to this that Erlandsen in a second series of measurements, where he has undressed his patients and placed them under blankets, comes to higher and less variable axilla temperatures; just as was to be expected when dealing with a local temperature which varies with the air temperature.

In his researches on the skin temperature Oehler[2] has not taken the axillary temperature into account; but in his opinion there is a connection between the axilla and skin temperature; this appears indeed from his measurements. The numbers below are taken from Oehler's tables.

Temp. of air	Axilla temp.	Rectal temp.				
25—24° C.	37·1° C.	37·2° C.	5	readings in	5	individ.
24—23° -	36·9° -	36·9° -	8	— -	7	—
23—22° -	36·8° -	37·2° -	31	— -	12	—
22—21° -	36·7° -	37·2° -	18	— -	7	—
21—20° -	36·4° -	37·0° -	12	— -	5	—
20—19° -	37·0° -		3	— -	2	—

[1] Hospitalstidende No. 48, 1908.
[2] l. c.

I have no means of controlling OEHLER's results; but the tendency in the series seems indisputable: falling axillary temperature with falling air temperature. An exception is shown by the last link of the series, which seems however to have been treated with less care than the others; it shows the fewest measurements, and the rectal temperature is not given. The latter appears, for the rest, quite independent of the air temperature.

In another table OEHLER, besides the axillary temperature, records the skin temperature taken in 5 different places; as above, I omit the other skin temperatures and for comparison only quote the temperature on the breast.

Temp. of air	Axillary temp.	Temp. on breast
17—18° C.	36·7° C.	34·1° C.
18—19° -	36·8° -	34·4° -
19—20° -	36·8° -	34·3° -
20—21° -	36·7° -	34·3° -
21—22° -	36·8° -	35·1° -
22—23° -	36·8° -	35·3° -
23—24° -	37·0° -	35·5° -
24—25° -	37·2° -	35·5° -
26—27° -	37·0° -	35·9° -

Single measurements show naturally the mutual dependence of the 3 series less distinctly, but on contrasting the first half of the series with the second, the result is conspicuous. It is worth noticing that the irregularities for the axillary temperature are not much greater than those occurring in the second series.

Lastly, KRAFFT[1] has found differences of 0·5°—1·0° in the right and left axilla of the same person, a difference which cannot be easily explained otherwise than by admitting the influence of the outer temperature.

Judging from all the data, I believe myself justified in maintaining that:

The rectal temperature is directly dependent on the heat-producing processes of the body, varying with their intensity concurrently with the action of the heart. The outer temperature may exceptionally have some effect and stasis may occasionally work disturbingly on the result of the measurements; but taken on the whole the influence of local factors on the rectal

[1] Sechste Versammlung der Tuberkuloseärzte Deutschlands. l. c.

temperature is of secondary importance. With a certain right the latter may therefore be called the "temperature of the body". The rectal temperature is given to be "normally" 37·5° C. Apart from the fact that this number is most probably too high on the average, it would certainly be more useful to record that under ordinary conditions the rectal temperature is between 36° and 38°, as a rule about 37° C. My own observations give the limits to be 35·66° and 38·75°; I cannot indicate an average.

The temperature of the mouth rises and falls with the temperature of the surrounding air; only when the latter is to a certain extent constant, is it inclined to follow the changes of the rectal temperature. The mouth temperature rises owing to meals independent of the rectal temperature, and only in special cases does it rise with bodily exercise. Other factors are of slight importance. The temperature of the mouth is given as 37·2° C.; but from the data noted above, an average must be considered as quite illusory, being mainly dependent on climate and season of the year. In my measurements the temperature of the mouth varied between 37·45° and 34·58° C.

The skin temperature is in still higher degree than the temperature of the mouth influenced by the air temperature and other atmospheric conditions. This can be shown directly for the uncovered skin, not so easily where the skin is covered by clothing, because the temperature of the air between the skin and clothing cannot be accurately enough determined, and because this temperature, as it changes much less than the air temperature in general, causes much weaker fluctuations in the skin temperature in the parts dealt with here. As the functions of the skin — even if facilitated by the clothing — are the same whether the skin is covered or uncovered, nothing entitles us to assume a fundamental difference in the skin temperature in the two cases. The temperature on the forehead is appreciably influenced by work of the brain and rises apparently during meals. Transition from a sitting to a lying position causes a conspicuous rise of temperature on the forehead.

It is naturally quite impossible to give an average for the skin temperature, and the maximum and minimum values give here but little information, the lower limit being specially deceptive; it means only that the observer has not been able or willing to stand the inconvenience involved in taking measurements in low temperatures of the air. For the temperature of the forehead my results vary between 35·7° and 24·6° C.

Daily variation of temperature.

The problem occupying foremost place in thermometry within recent times is the question whether the variation of temperature during 24 hours follow a fixed, inherited curve invariable in each individual; whether the curve of variation, as FINSEN[1] puts it, "belongs to the mysterious land of periodicity", or is a simple consequence of other physiological functions, the range of which we may consciously control and limit. Most authors have made the latter view dependent on its being possible to find a "reversed" curve of temperature on changing one's mode of life, by working at night and sleeping during the day. For several reasons these experiments, at any rate when made on human beings, must necessarily fail of success, and the problem is still regarded as unsolved, though many researches go to show that the variations of temperature can be explained from known presuppositions.

The classical work in this field is JÜRGENSEN's on the temperature of the body[2]. I shall not enter upon the general considerations and theories advanced by JÜRGENSEN, which seem to me too artificial, but restrict myself to his curves of variation. His "normal curve" has by all later authors been adopted as the standard for the daily change of temperature, and it cannot be denied that his material was large and that his curves agree well with one another, but nevertheless I think that his mode of procedure is open to objection and his results deceptive. Firstly, he concludes from the regularity displayed by his curves that the same regularity would be found again under other conditions, even if disturbed by various circumstances. He does not take into account the possibility that the same persons, when living under quite different conditions, might display as regular variations, but with curves of quite a different shape and on another level. Secondly, he considers individuals passing day and night in bed as normal individuals. He attains what he is seeking for, regularity in the variations of temperature; but he does not contribute towards the explanation of the phenomena.

In Sweden important papers on the variations of temperature have been published.

By means of a long series of experiments on metabolic changes, TIGERSTEDT and SONDÉN[3] have determined the amount of carbonic acid given off during the 24 hours; they have compared the variations in the production of carbonic acid with JÜRGENSEN's curve of

[1] Hospitalstidende 1894, No. 50, p. 1247.
[2] Die Körperwärme des gesunden Menschen. Leipzig 1873.
[3] Skandinav. Archiv f. Physiologie 6, 1895.

temperature and have found a pretty good agreement as a rule, as good as could be expected, when employing a fixed gauge for the comparison. They conclude that the daily variation in temperature in the resting human being follows principally and probably entirely the variations in the extent of the metabolic changes.

In a later paper by the same authors in conjunction with Johansson and Landergreen[1], dealing with the metabolic changes during fasting, the results are given of some temperature measurements made by the individual experimented on during the physiological experiments. Here the amount of carbonic acid and the temperature display a much more definite agreement, but there are also curves of temperature which do not follow Jürgensen's scheme. In the latter there is a maximum about 10 a. m. after some hour's bodily exercise and then a fall on resting, lasting to about 4 p. m. At 6 p. m. there is on ordinary days a slight rise of temperature after dinner; in the curve for the fasting days there is also a rise at this time of the day, but less marked. It is evidently due to the person experimented on having now and then taken a short walk at the time when he was accustomed to eat. In the single curves, where nothing is said about exercise, no rise occurs at 6 p. m.

We have here undoubtedly a type as fixed as the Jürgensen type, but of quite a different form, being derived from an individual whose mode of life differed from that followed by Jürgensen's individuals.

Johansson[2], finally, has shown that one may consciously influence temperature variations according as one observed a more or less complete rest.

Various attempts have been made to produce a "reversed" day and night curve.

Benedict and Snell[3] have measured the temperature in a man who had lived "reversely" for 10 days and nights, but they did not find any distinct tendency to a reversed curve.

Benedict[4] later tried the same method but without result.

In an individual with "reversed" mode of life, the temperature was measured on the 10th and 12th day; there was no change in the night curve but a fall in the day curve in the forenoon, corresponding with the sleep. In another case experiments were made on a professional night-watch; the result was an irregular but not a "reversed" curve. Benedict is himself aware that a complete

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[1] ibid. 7, 1897.
[2] ibid. - —
[3] Arch. f. d. gesammte Physiologie, 90, 1902.
[4] The American Journal of Physiology, Vol. XI, 1904.

reversal of day and night is impossible; but in his opinion he has come as near to it as can be done. He is hardly right here. His night-watch slept unusually long and at an unusual time, while eating no regular meal from 5·15 p.m. to 7·15 a.m. It might have been possible, however, to find a more reasonable arrangement, though the results would hardly have been changed. The difficulty is the same everywhere. One cannot disconnect the individual from society; so long as the latter follows a fixed rotation, the single individual will, consciously or unconsciously, tend towards the same mode of life. And, as Tigerstedt points out, it will always be necessary, at any rate during a "reversed" day and night, to work by lamp-light, sleep in the day-time, work in the stillness of night, sleep through noise and so forth.

Galbraith and Simpson[1] have investigated the variations of temperature in monkeys. Six monkeys were kept in a large room which could be made dark in the day-time. The temperature of the animals was measured every second hour in the axilla. The temperature in the room was always pretty high, 19°—23° C.; 5 experimental series were made.

During the 1st period, light day and dark night, a regular curve was obtained with higher temperature in the day, lower in the night.

During the 2nd period, darkness during the day, the curve becomes reversed already after 24 hours and by and by pretty regular, though not so regular as during the 1st period, according to the authors, because noise in the adjacent laboratory disturbed the animals. The average of the curve is a little lower than during the first period.

In the 3rd period, darkness from 3 a.m. till 3 p.m., light during the remaining 12 hours, the curve follows the new division of day and night, but is less regular.

In the 4th period, darkness day and night, and in the 5th period, light, electric light or day-light, day and night, the curves become irregular, in the first case with the average a little lower.

Thus, it is evident that the reversed curve can be produced. The whole monkey society is "reversed". The world of the monkeys is in the cage, and in the cage light and darkness and meal-times can be arranged at pleasure.

Galbraith and Simpson[2] have also examined the curve of temperature in night and day birds. They found the curve "reversed" in night birds (owls), the maximum occurring in the night

[1] Proceedings of the Physiological Society, July 1903. The Journal of Physiology, XXX, 1903.

[2] l. c. and The Journal of Physiology, Vol. XXXIII, 1905—06.

or towards the morning, the minimum in the day-time. They also found the average for night birds to be a little lower than that for day birds. Only very few of the owls have a well-marked period, and the time for the maximum varies greatly, following the time when the bird is hunting. These researches go to show, therefore, the very same as the researches on the monkeys; in both cases the result is, that the form of the curve of temperature is determined by the mode of life of the animals.

Osborne[1] has recently had the good idea of examining whether the temperature curve was changed during a journey from Melbourne to London. The difference of longitude between the two places being about 10 hours, the temperature curve, if following steadily the Melbourne time, must become reversed in London. Osborne found that his maximum temperature, which occurred at 6 p.m. in Melbourne, followed the local time; but as the transition was gradual he did not venture to base anything on his results.

It follows from the statements above that a day and night curve of typical form for the mouth temperature is out of the question. Hørmann[2] has indeed measured the temperature of the mouth and found curves very similar in appearance to curves for the rectal temperature; but his measurements were made under special conditions. Hørmann first says that it is well-known that the temperature in the mouth does not differ much from "die des Kerns"; in himself the difference was 0·25° C. (in the morning, in bed: rectum 36·35°, mouth 36·1°). Such a determination is naturally not satisfactory. Hørmann has quite overlooked all difficulties in the mouth measurements; he has unconsciously concealed the influence of the outer temperature on the daily variations, and has included this influence in the relation between the curves for day and night. Just as for the rectal temperature he thus found higher temperatures in the day, lower in the night; but very different causes have produced this uniformity and he has not given these causes due consideration.

Oehler[3] has shown for the skin temperature that the variations run parallel with the changes which take place at the same time in the rectal temperature, but they are in still better agreement with the curves for the air temperature. This dependency becomes specially evident whenever a sudden fall in the temperature of the room is caused by ventilation; a sharp fall in all the curves for

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[1] Proceedings of the Physiol. Society. Jan. 25. 1908. The Journal of Physiol, Vol. XXXVI, 6, 1908.

[2] Zeitschrift f. Biologie, Bd. 36, 1898.

[3] l. c.

the skin temperature corresponds with this fall. As most people sleep in a cold room and with uncovered head, the temperature of the head will be lower in the night than in the day; otherwise the skin temperature will depend upon purely outer conditions.

As stated several times above, the rectal temperature is directly dependent on the variations in the metabolism of the body and only to a small extent on external or purely local conditions. If this is correct, it should be possible to construct the main features of the curve of day and night variations for an individual whose work and mode of life is known. And then, just as a clergyman, a navvy and a sailor have very different kinds of work, unequally distributed over the hours of day and night, it follows that a "normal" curve of the variations of the rectal temperature is without any connection with reality.

The accompanying diagram (p. 45) shows day-curves derived from measurements on 3 different individuals under different conditions.

The first three are variants of the curve with 3 maxima, which is the usual curve on days with sedentary work, as shown on fig. 1.

In the first the temperature in the forenoon is heightened owing to bodily exercise, the subsequent rise after a meal between 12 and 1 is therefore but slight; owing to bodily exercise and a small extra meal there is in the evening a rise instead of the usual fall.

In the second case the rise after the last meal is absent, or is not seen at any rate in the curve. The temperature was not measured immediately before and after the meal. The evening temperature is raised for the same reason as in the first case.

In the 3rd curve there is an unusual rise between 1 and 3 p. m.; it is due to a walk on land.

The last two curves, which follow a course quite different from the previous, are derived from days passed in hill-climbing and sedentary work. Both begin with a rise of temperature corresponding with the ascent to the place of work; there is then a great fall while standing still at a low temperature and in a slight wind; and finally a rather steep rise, not reaching so high however as the rise in the forenoon, due to the descent from the hill.

It appears from these two curves that a day passed in an unusual way gives an unusual curve, but the form of this curve is also seen not to be accidental, there being an unmistakable agreement between the two curves.

The sailor, whose time is divided into a peculiar 48 hours' routine, displays an exceedingly irregular curve of temperature. In his case it is dependent on wind and weather when the hard and when the easy work must be done; and more than most other people he is exposed to abrupt changes.

The last three curves on fig. 7 illustrate these conditions.

The broken lines of the curves indicate sleep. It is seen that in curve 5 the decidedly highest temperature falls in the "dog-watch",

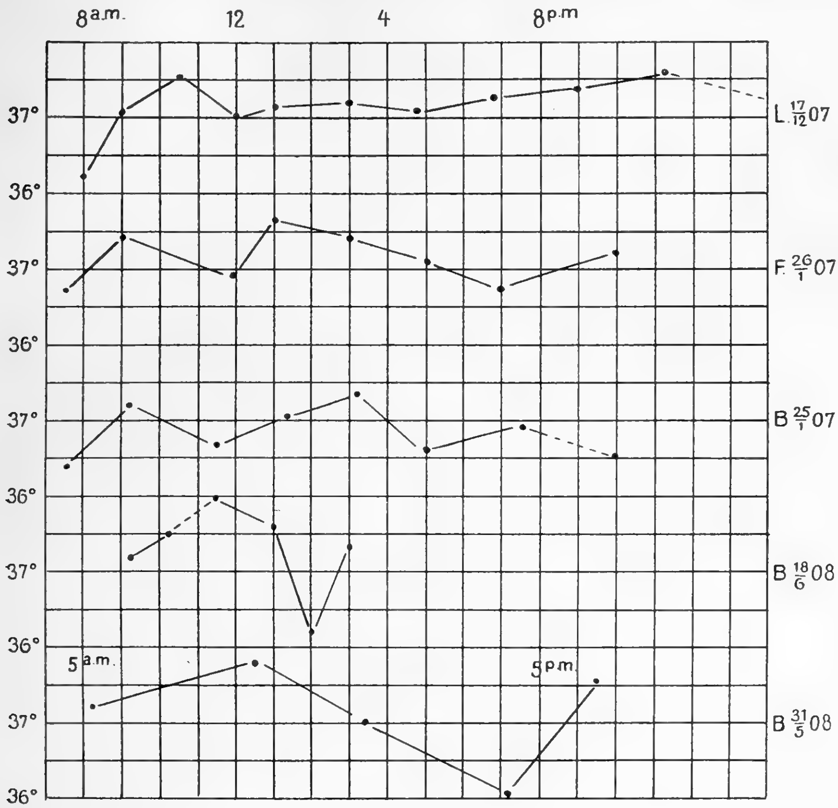


Fig. 6a.

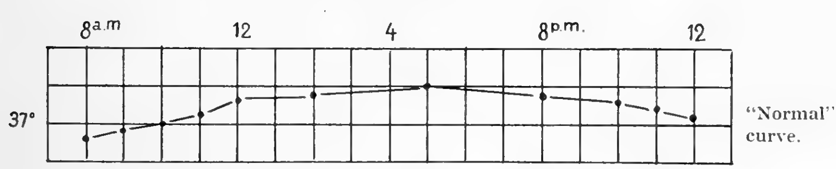


Fig. 6b.

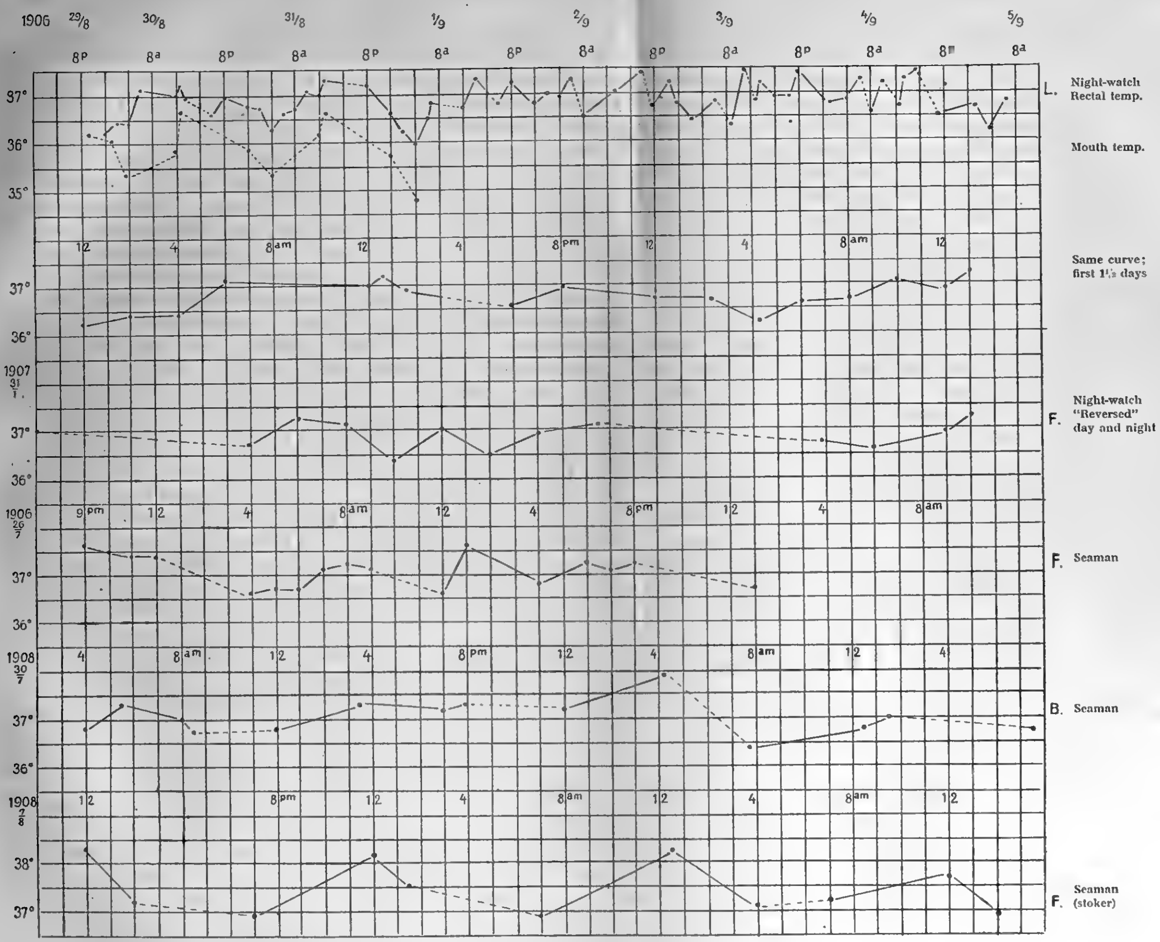
at 4 in the morning. It is further observed, that in curve 6, the stoker curve, owing to the much more regular work and the high air temperature, the rises are most uniform and the average level highest.

On fig. 7 curve 1 is a compound curve derived from self observations during a week when I was on the night-watch. Taken on the whole the curve exhibits rather a chaotic appearance; the periods for sleep are unequally long and unequally distributed; the average of the curve is fairly low. It is seen, however, that the lowest values occur in the night, which appears even more distinctly from curve 2, a portion of the former and drawn on the usual scale. This resembles by not so little the night-watch curves of BENEDICT, where as in my case the lowest temperatures occurred in the night; for both cases it holds good that the individual concerned partook of his meals in the day-time together with other people, and for this reason alone a fall of temperature must be expected towards night; in the day-time the temperature is also inclined to fall between the meals. Like most other night-watches I slept less than under ordinary conditions, not to be quite out of touch with other people, so that I often felt sleepy for periods, especially in the night when I passed most of the time with sedentary work. It was only towards the morning that I found occasion for some bodily exercise, lighting the fire in the stove etc.; a rise in the temperature is therefore found from 4 to 6 in the morning. But there is lastly one fact, probably unnoticed hitherto or at any rate not deservedly estimated in this connection, viz. the psychical factor of the night-time.

I shall not venture here on any discussion of the psychology of the night, but only call attention to the fact, that the night-time undoubtedly has a peculiar effect upon one's general state of mind owing to the stillness, solitariness and various other circumstances, and this changed, psychical condition reacts on all one undertakes. Every strong or sudden noise is disagreeable, and involuntarily one tries to avoid all such things; the movements become gliding and noiseless, resembling those of other night-animals, and are much less definite than in the day-time. A round in the night will therefore fail to raise the temperature like a corresponding walk in the day-time. And one is never sitting so still as in the night; the leaves in one's book are turned almost without touching them.

The view that this factor is of essential importance for the form of the curve of temperature in a night-watch is strengthened by some measurements made on a "night-watch in the day". At a period during the winter of 1907, on which more details will be given below, we reversed day and night, so that we were up in the night and slept during the day. The night-watch concerned continued his mode of life as regards sleep and waking, though we others "reversed". He took his sleep all at one time, as displayed by the curve (curve 3 on fig. 7), about $8^1/_2$ hours, so that he had no reason to be sleepy in the "night"; nevertheless, his curve is very

Fig. 7


1906
29/8
30/8
31/8
1/9
2/9
3/9
4/9
5/9
L. Night-watch Rectal temp.
Mouth temp.
Same curve; first 1½ days
1907 31/1
F. Night-watch "Reversed" day and night
1906 26/7
F. Seaman
1908 30/7
B. Seaman
1908 7/8
F. Seaman (stoker)

similar in appearance to mine recorded above and to the curves of other night-watches. The level of the curve is lower than usual for him (see curve 4 on fig. 7 and curve 2 on fig. 6), and there is a well-marked fall in the "night". The rise at 3 a.m. is due to a walk of observation on land; owing to the construction of the curve this rise becomes much more extensive than is justly due to it; but the low temperature before and after sufficiently clearly mark the fall of temperature.

Thus, there are valid reasons why a "reversed" curve of temperature has not hitherto been obtained; certain demands must necessarily be met first, demands not consistent with social life in civilised countries.

These demands may however be met on a high-arctic expedition during the polar night, which in a case like this is to be preferred to the "perpetual" day in summer. The sun occasions a continual unrest; the working hours are unlimited and a little food emancipates one from the regular meals. It will therefore be very difficult to make all adopt the same mode of life, and even if only one breaks away from the order the consequence easily becomes more or less irregularity all along the line. Apart from this the difference in the intensity of light is much more appreciable in summer than in winter, just as the variations in the day and night temperature are much greater. During the polar night the case is different. All lamps are put out at certain fixed hours, appointed by the leader of the expedition; then it is night; during the remaining hours the lamps are lit and we have day. As practically all work is done indoors, the dining hours are kept by every one. And in the open air the light by day and at night does not vary appreciably and the changes in the temperature are comparatively small.

The experiment was made on the "Danmark Expedition", whilst in the harbour at 76° 46′ N. L. in January 1907, and by general consent and obligingness, both from the leader of the expedition and the other members, the transition was made very quickly and easily. Bed-time was delayed once 4 hours and then 8 hours by inserting an extra meal. Then the "reversal" was accomplished. The lamp was lit and put out at the same time as before; one met at the table at the same time as usual. Only on clear days one might miss at "noon" a little brightness in the southern sky, otherwise the change was betrayed by no external sign. It was a fact of which we were conscious; but in the great majority even this knowledge was not able to produce the feeling of anything unusual. Time rolled along on its even, ordinary way. More than half of the 28 members of the expedition felt, indeed, just as usual as soon as the transition had been accomplished; after 5—6 days only a few were still a little indisposed to work, sleeping less well in the "night" and becoming sleepy at various times of the "day". The function altered with the greatest difficulty was defæcation; for a few it took about a week before this occurred at the usual time, for one (rarely quite free however from indisposition) it took even till the end of the experiment. As the end of December was not well-adapted to the experiment, owing to the irregular mode of living characteristic of Christmas, which cannot be dispensed with even in those latitudes,

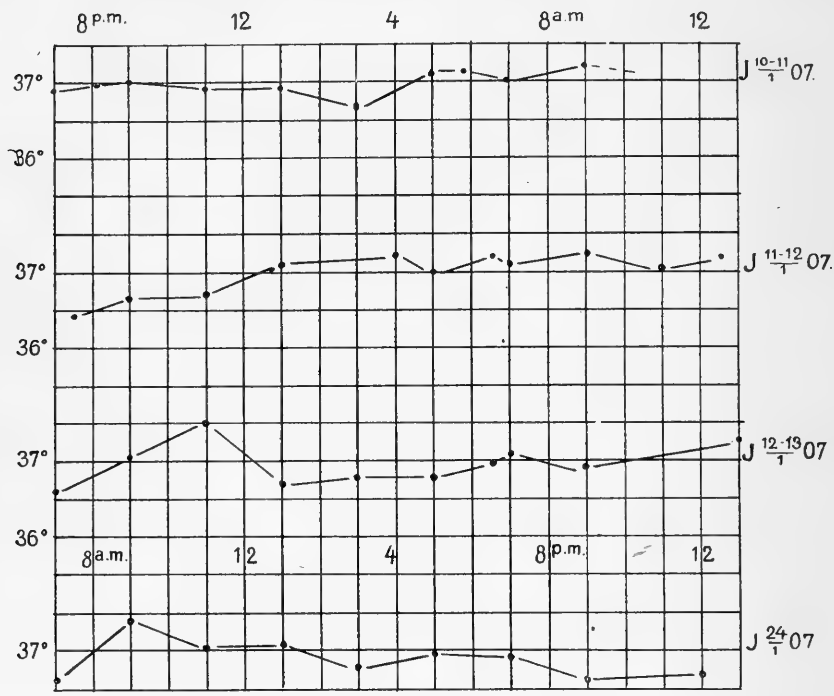


Fig. 8.

I restricted the experiment as much as possible, taking the necessary days for control afterwards.

Thanks to the easy transitions, the time proved sufficient. On the 2nd of January the change began, on the 4th we were on the new system; on the 9th I began the measurements of temperature for my own part. As late as the 11th one of the individuals examined had an irregular curve; but on the 12th his curve also had adopted the usual form. On the 15th the return began, and on the 18th we were again under normal conditions. As the second return was attended by the same phenomena as the first, appearing especially in the same persons who had presented difficulties on the previous period of transition, I think we may conclude that the adaptation to the changed conditions was complete.

The curves above (p. 48) give the results of measurements on a individual who had difficulty in becoming accustomed to the “reversed” days; though, on the 6th day after the transition had been accomplished, he begins with high morning temperature (curve 1) after a restless and partly sleepless night. The curve displays no rise at breakfast; the temperature remains almost unchanged; a slight fall in the afternoon is followed by a slight rise in the evening. On the following day we find normal morning temperature after better

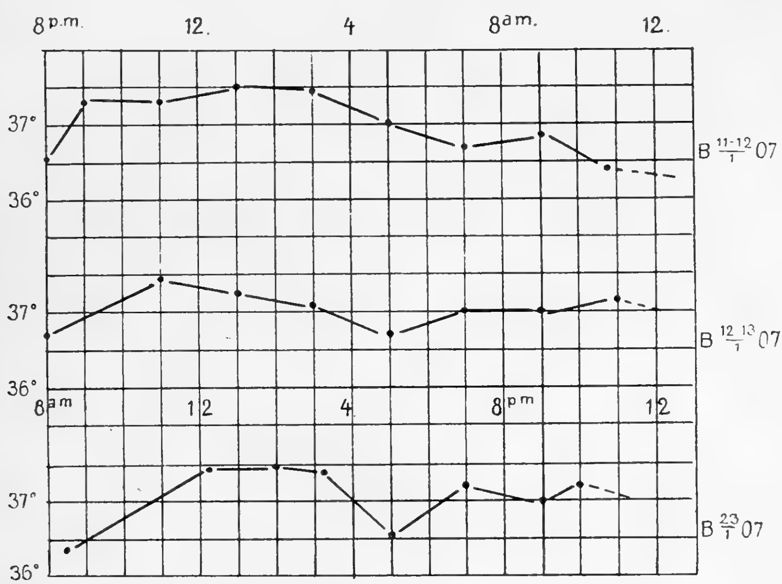


Fig. 9.

sleep; there is a tendency to rise at breakfast and the temperature further rises at noon, then continues fairly high with somewhat irregular course. Next day the curve has nearly the ordinary form, as appears from comparison with the control curve given as curve 4. The rise at 9—11 p. m. is due to a walk; the reaction after the latter prevents the rise after the smaller meal at 12, which is also not very well-marked in the control curve. In both of the latter curves there is a slight rise in the evening after an extra meal.

On fig. 9 3 curves are given for another person, the first two from reversed, the last from ordinary day and night. As a rule the temperature in this individual rose rather abruptly in the morning, which does not appear from the last curves, because one time of measurement was passed by. The high afternoon temperature in the last curve is due to bodily exercise; it is indeed followed by a specially well-marked fall. The rise late in the evening is in both cases derived from an extra meal. — In the first curve the rise at 7 p. m. is for some unknown reason absent.

The last 4 curves (fig. 10) are derived from self observations. In the second the rise after the last meal is absent, which was very rarely the case with me; it is simply due, no doubt, to the fact that

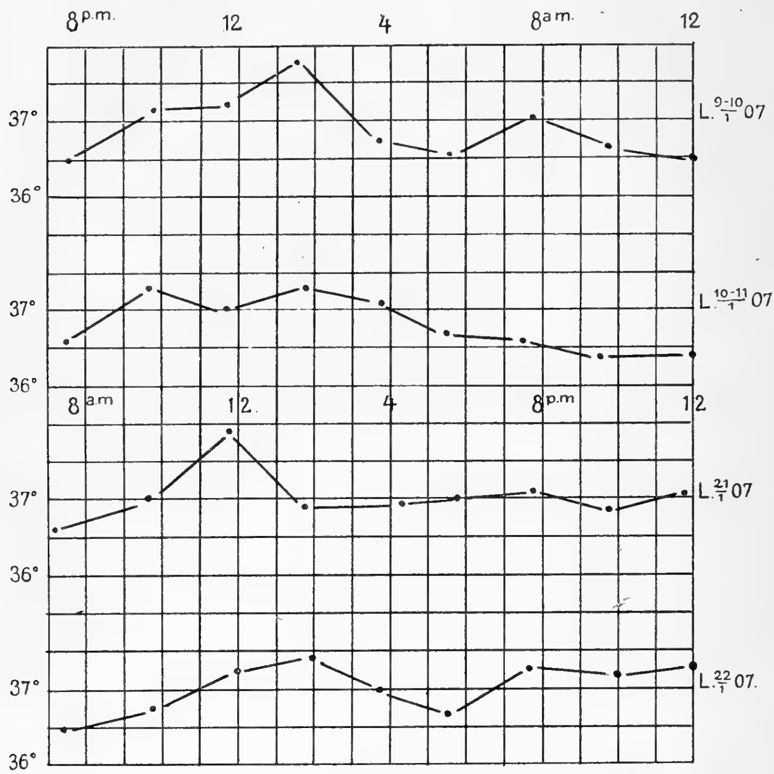


Fig. 10.

the measurement was made more than one hour after the meal and that I had been sitting still in a cold room during the interval. The rise at 1—2 in curves 1 and 4 is due to lunch and an hour of fencing; the great rise at 12 in curve 3 is the result of active bodily exercise in the open air, and the same holds good for the somewhat smaller rise at 12 in curve 4. The first two evenings I was sitting in my room counting blood corpuscles, but on the last two I was in the heated mess-room among companions and had an extra meal in the evening; the difference appears distinctly in the curves; in the first two there is an even fall in the evening, in the last but a slight fall after the meal and later in the evening a slight rise just as in the curves above and for the same reason.

In all cases measurements for the 7 night hours are absent; but nevertheless the curves seem to be sufficiently expressive. The curves are not the same, because one day does not easily become an exact copy of another, when the attention is not exclusively concentrated on this condition, but the curves 3 and 4 on fig. 8, 2 and 3 on fig. 9 and 1, 2 and 4 on fig. 10 show that the departures are not great under ordinary conditions.

In all cases the fundamental type is evident, and the causes of the departures present are obvious. All of them tend to show that the curve of temperature variations is determined by work and mode of living, that the astronomical division of day and night is without importance in this regard, and that an inherited form is consequently out of the question, a mysterious periodicity even more so.

On fig. 6b the Jürgensen "normal" curve is drawn for comparison. The latter and the day-curves shown here are not very similar in appearance. Jürgensen's experimental persons stayed in Europe and always in bed. But whether the disagreements are explained in this or in that way, it must undoubtedly be clear that a "normal" curve of variation for the rectal temperature is and must be artificial and consequently without physiological interest.

18—1—1910.

Tabular Summary of the Respiration Experi-

No.	Date	Time	Barometer	Temperature	Duration of experiment	No. of respirations	Frequency of respiration	Litres expired	Litres pr. h. 0°·760' dry	Vol. of an Expir. at noted Tp.	Vol. of an Expir. at 37°
83	29/11 07	9a 00	762	12·55	30	298	9·93	331	626	1111	1266
95	11/12 —	9a 00	757	13·95	30	264	8·80	306	570	1159	1311
117	22/5 08	9a 00	752·5	6·2	30	246	8·20	270	517	1098	1286
121	25/5 —	9a 18	761	8·9	30	261·5	8·72	279	535	1069	1238
74	20/11 07	11a 27	740	13·45	30	273	9·10	319	583	1168	1331
92	8/12 —	11a 33	756	18·0	30	258·5	8·62	268	490	1039	1155
125	27/5 08	11a 25	754	14·05	30	253·5	8·45	253	470	1000	1136
126	28/5 —	11a 20	757	14·5	30	245·5	8·18	273	508	1114	1259
25	14/12 06	11a 25	758	17·45	30	292	9·73	276	507	945	1054
26	17/12 —	11a 15	748	12·7	30	296	9·87	256	475	865	986
27	19/12 —	11a 05	756	14·3	30·5	252	8·25	258	473	1024	1161
28	21/12 —	11a 05	750·5	15·3	30	250	8·33	254	467	1016	1145
84	30/11 07	1p 05	747·5	11·3	30	317	10·57	337	628	1063	1220
91	7/12 —	1p 15	760	16·5	30	287	9·57	318	589	1108	1241
112	18/5 08	1p 10	750·5	9·6	30	285	9·50	274	517	962	1112
110	16/5 —	12p 55	756	11·2	29	264	9·10	264	515	1000	1150
86	2/12 07	3p 10	753·5	13·75	30	300	10·00	303	563	1010	1146
90	6/12 —	3p 25	762·5	15·4	30	267	8·90	274	512	1026	1155
111	17/5 08	2p 55	754·5	10·6	30	257·5	8·58	253	478	985	1133
113	19/5 —		755·5	11·0	30	254·5	8·48	256	483	1008	1157
89	5/12 07	5p 33	761·5	15·2	30	281·5	9·38	266	496	947	1067
93	9/12 —	5p 05	761·5	16·0	30	285	9·50	252	469	884	992
114	20/5 08	4p 40	760	14·05	30	244	8·13	264	494	1082	1225
116	21/5 —	4p 35	756	15·3	30	240·5	8·02	257	476	1071	1206
85	1/12 07	7p 05	750	14·8	30	358·5	11·95	364	670	1017	1149
87	3/12 —		756	14·5	30	313	10·43	323	600	1032	1167
122	25/5 08	6p 30	762	14·1	30	241·5	8·05	286	537	1187	1344
124	26/5 —		759	10·6	30	254	8·47	275	522	1083	1245
88	4/12 07	9p 25	759	13·8	30	300·5	10·02	301	563	1003	1137
94	10/12 —	8p 52	758·5	16·2	30	306	10·20	300	555	980	1099
118	22/5 08	8p 35	754	11·0	30	237	7·90	255	480	1076	1236
120	24/5 —	8p 30	760	15·0	30	233	7·77	277	516	1189	1341

ments made at different times of the day

% CO^2 in Expir.	% CO^2 in Inspir.	% CO^2 in Alveolar air	CO^2 cc pr. K. & h.	Weight in Kilos.	Pulse	CO^2, average for time of day	Rectal temp., average for time of day	Notes
3·11	0·05	4·06	274	(70)	82	—	—	
3·43	0·03	4·42	287	(67·5)	104—86	—	—	Experiment uncertain.
3·45	0·03	4·50	258	(68·5)	96—82	—	—	
3·28	—	4·32	251	69·5	92—83	267·5	37·1*	* Temperature readings very imperfect.
3·36	0·04	4·31	281	(69)	76	—	—	Awoke too late. Experiment made too near breakfast time.
3·28	0·04	4·42	234	(68)	78—76	—	—	
3·34	0·03	4·52	224	69·5	78—79	—	—	
3·27	—	4·28	237	(69·5)	76—82	—	—	Standing and walking a little before experiment.
3·31	0·04	4·61	249	(66·5)	73—76	—	—	Experiment interrupted and begun over again, as inspiration-tube became stopped up.
3·23	—	4·63	229	(66)	62	—	—	
3·38	0·03	4·55	240	(66)	60—60	—	—	
3·38	—	4·56	237	(66)	70—71	235·7*	37·2	*Experiment No. 74 omitted.
3·14	0·04	4·15	278	(70)	81	—	—	
3·06	—	4·02	260	(68·5)	74—78	—	—	
3·21	0·03	4·39	241	(68)	80—80	259·7	37·3	
x	—	—	—	(68)	65—74	—	—	Owing to an erroneous reading of the Chronometer, experiment ended too soon. Air analysis unsuccessful.
2·80	0·05	3·77	221	(70)	68—76	—	—	
3·07	0·04	4·14	227	(68·5)	69—78	224*	37·2	* Experiment No. 113 omitted.
x	0·03	—	—	(68)	71—76	—	—	Air analysis unsuccessful.
3·43	—	4·62	242	(68)	72—72	—	—	Walking for 2 hours before experiment.
3·23	0·04	4·48	229	(69)	79	—	—	
3·08	—	4·41	210	(68)	76—80	—	—	
3·27	0·03	4·32	236	68	71—74	—	—	
3·35	—	4·45	231	(68·5)	72—79	226·5	37·2	
3·08	0·04	4·15	291	70	96	—	—	
3·15	—	4·23	269	(69·5)	93—87	—	—	
3·30	0·03	4·24	253	(69·5)	69—75	—	—	
3·47	—	4·56	258	(69·5)	66—77	267·8	37·4	
3·19	0·04	4·32	257	(69)	76—85	—	—	
3·04	—	4·16	247	67·5	76—76	—	—	
3·35	0·03	4·41	231	(69)	75—74	245·0	37·5	
x	—	—	—	(69·5)	72—75	—	—	No air sample. Tap erroneously opened.

II.

SOME INVESTIGATIONS ON THE FLUCTUATIONS IN THE NUMBER OF WHITE BLOOD CORPUSCLES IN THE CAPILLARIES AND THE CAUSES OF THESE FLUCTUATIONS

BY

J. LINDHARD

1910

It was my intention on the "Danmark Expedition" to carry out a systematic research on the blood and the changes it undergoes in the arctic climate, but I was not successful; my investigations proved to be fragmentary.

Since the question of the variations in the number of white blood corpuscles and of the causes of these variations is, however, of great importance and as but few investigations on the number of leucocytes have been made at the same time as measurements of other physiological functions, which are of interest in this connection, there seem to be good reasons for publishing the observations recorded in the following pages.

Before discussing my own investigations, however, I may make a few remarks on the method of counting the leucocytes and its accuracy.

Various endeavours have been made in recent years to determine the accuracy with which the leucocytes can be counted; various methods have been chosen and different results have been arrived at. I shall not discuss here the considerable literature which has grown up round this subject, but merely mention a few of the more recent papers.

Bruhn-Fåhræus[1], who used the acetic acid method, counted the numbers in a series of preparations (5—8) from the same pipette in order to determine the error in counting. From 35 determinations he found the standard deviation to be $<$ 10 % of the average in 29 cases, in none was it over 12·1 %; but in only 1 case was it $<$ 3 %.

The author is aware, that in using this method he disregards some sources of error, absence of exactness in drawing up the blood and mixing fluid etc.; but he considers — and most probably with right — that the errors possibly arising in this way are of subordinate importance. He sees clearly, that an exact determination of the error requires the blood-mixture to be constantly the same and therefore believes that an artificial blood-mixture must be procured outside the body, as it is impossible to know whether the blood

[1] Nord. med. Archiv. 1897, No. 15, p. 19—20.

during circulation has not changed in regard to the number of leucocytes it contains, during the period necessary for taking a series of samples. The author does not attempt however to obtain such an artificial blood-mixture, as he considers the task bound with great practical difficulties.

In an extensive work KJER-PETERSEN[1] has specially dealt with the methods of counting the leucocytes and the determination of the error; he also uses the acetic acid method. He takes only 1 preparation from each pipette, after having filled 10 pipettes immediately after one another from the same incision, in a person whose blood he found to be relatively constant in regard to the leucocytes by means of a series of enumerations from day to day. From two series of 10 countings in the case of such a "constant" individual he finds a variation-coefficient of respectively 2·7 and 4·2; a colleague, who undertook a series of 10 enumerations from the blood of the same person at the request of the author, found the coefficient 3·6.

It is evident that this method used by KJER-PETERSEN will probably give too high values, since in addition to the errors he has also to deal with possible variations.

KJER-PETERSEN evidently knew the method of counting used by BRUHN-FÅHRÆUS but does not seem to have known his work and has not endeavoured either to form a blood-mixture outside the body; in fact, he commits the great mistake of including in his determination of the error a number of countings from the blood of other individuals, reasoning somewhat as follows: The accuracy with which we count is of more importance than that with which we *can* count. The reasoning is correct; but the presupposition underlying the possibility of learning anything at all about the matter is, that the blood-mixture examined must be constant, and the blood used by KJER-PETERSEN is not that. His determination of the error is therefore more or less accidental.

HASSELBALCH and HEYERDAHL[2] steer clear of the difficulties by determining an individual "standard-coefficient", which for their special purpose is undoubtedly an excellent method. But the variation-coefficient certainly varies from day to day, and the method is therefore scarcely of any use as a normal procedure.

As, however, it will be of greatest interest to know, how great an exactness can be expected from the method of counting used, as also how to reduce the unavoidable errors to the least possible, the

[1] Om Tælling af hvide Blodlegemer etc. Aarhus 1905.

[2] Om nogle fysiske Aarsager til Variationer i Mængden af hvide Blodlegemer. Vidensk. Selskabs Oversigter. 1907, No. 5.

endeavour must be directed towards obtaining a constant blood-mixture.

I may now briefly describe my experiments to ascertain the accuracy of the method of counting.

My preparations were also made according to the acetic acid method; in counting a Breuer's cell was used with Leitz Obj. 3, Oc. I. On multiplying the number obtained by 200/9 we get the number of leucocytes in a cubic mm[1].

It will be surprising to most that Bruhn-Fåhræus, who avoids a number of sources of error, has obtained a much greater error than Kjer-Petersen, though the latter determines the total error in variable blood. The difference is so considerable, that it cannot be considered as arising from personal peculiarities in the two investigators. The method is not so extremely complicated and the comparatively inexperienced assistant of Kjer-Petersen has thus obtained just as good a result as himself, in counting the corpuscles in the "constant" man.

To come to a clearer understanding of this condition, as also to ascertain which of the manipulations necessary in the counting give the greatest uncertainty in the result, I began by counting 2 drops from the same pipette (I had only two measuring cells at my disposal). I obtained considerably different results, which I ascribed to my relative lack of experience in counting; but it gradually became more and more evident, that whilst I obtained uniform results on counting the same number of drop from two different pipettes, two drops from the same pipette gave different results in spite of previous theorisings.

The following results are characteristic.

Pip. No.	Drop No. 6	7	8	9
3	306		248	
4	310		258	
1	493		399	
3	480		417	
1		387		323
3		379		280

[1] A circumstance which caused great trouble in all investigations of the blood, especially in winter, was the extraordinary rapidity with which the blood coagulated. It was almost impossible sometimes to get a pipette filled up to the usual mark without the blood rising up into the holder in one clump, which could not be shaken down. (With regard to this condition, see for the rest Bojanus: Exper. Beiträge z. Physiologie u. Pathologi d. Blut d. Säugethiere Dorpat 1881. S. 18).

Such figures indicate a certain system in the deviations. In 10 countings of the 6th and 7th drops from the same pipette I have found the highest number for the 6th drop in 9 cases, in 15 countings of the 7th and 9th drops the highest number in the 7th drop 13 times, once the same number for both drops and once the highest number for the 9th drop. Owing to my small material, 2 measuring cells and 4 pipettes, I could not obtain a complete series of drops from one pipette, nor a large number of simultaneous blood samples; but by counting two drops from the same pipette I have come to the result, that the number of leucocytes increases in the first 4 drops, then decreases a little in the 5th and 6th drops, a little more in the following 2—3 drops and then remains relatively constant; but I have never found such a good agreement in drops from the same pipette as in the same number of drop from different pipettes. It would therefore seem as if we must take into account a special "curve" for the pipette; and this would explain the otherwise strikingly large deviations obtained by BRUHN-FÅHRÆUS.

In consequence of this observation I have always used a definite drop, the 6th, in comparing the enumerations; whenever anything happened to this drop on mounting the cell I disregarded the pipette as a rule. It appears, namely, that if the pipette is laid aside for a while the leucocytes sink back from the capillary tube into the receiver, so that the first drop or drops taken out afterwards contain too few leucocytes. Where another drop than the 6th is used, this has been distinctly stated.

If we now wish to determine the amount of accuracy which the method with the above-mentioned reservation can offer, we must, as maintained by BRUHN-FÅHRÆUS, make use of a constant blood-mixture. I have made some experiments in this direction by letting the blood drip from the finger into a small quantity of a saturated solution of $MgSO_4$ and a little glycerine; from this mixture I filled the pipettes up to the mark I and then diluted it in the usual manner with acetic acid. I was successful also in obtaining a series of very constant results; but the mixture had several shortcomings nevertheless. Thus, I always obtained lower values after the mixture had stood for some time (24 hours in a cold room), and the last drops also gave too low values. The two following series may be cited.

I		102	II	221	231
	100	102		222	223
	106	104		222	223
	107	106		183	181

In the first series the pipettes were filled in two lots, one preparation being spoilt in the mounting; there was plenty of liquid in the mixing glass. In the second series the pipettes were also filled in lots, the liquid being mixed by blowing air through it. After I had filled the first two pipettes of the second lot, very little liquid was left and I was therefore obliged to let the scum settle and then mix with care. The result is shown in the greatly varying values for the 7th and 8th pipettes.

The question has not been solved, but it seems clear from the above that the ordinary method of counting suffers from a great difficulty which has not hitherto received attention. How far this source of error can be corrected must be determined by continued investigations. On the other hand, it seems certain that if we always take a definite drop, we can show by this method even small variations in the number of leucocytes; working with a constant blood-mixture the standard deviation barely exceeds 3 % of the average. The absolute number of leucocytes in a cubic mm. remains in fact unknown for the time being; but we can determine the variations and it is the variations which are of the greatest interest.

We know, that the number of leucocytes in a given quantity of blood varies from day to day or in the course of the day and even in samples taken immediately after one another; but the causes of these variations are still far from clear. So much seems certain, however, that the conditions of circulation, action of the heart, blood-pressure, pulse etc., are of considerable importance as the principal links in the chain of causes.

It is maintained by HASSELBALCH and HEYERDAHL[1] that mental impressions influence the number of leucocytes. They hold, that this "psychiske Leucocytose" is probably due to a simultaneous change in the action of the heart and not to vasomotor changes; it has been proved, namely by experiment, which they themselves have made, that even a very strong dilatation of the dermal capillaries (light erythema) is without influence on the number of leucocytes. In nervous individuals H. & H. have found very high numbers on examination of the first samples of blood taken and then a very distinct decrease in the number of leucocytes on the nervous condition passing away.

ELLERMANN and ERLANDSEN[2] who took up the question later, though they do not seem to have been aware of H. & H.'s work, also remark that the first drop of blood very often — especially in

[1] l. c. pp. 254—55.

[2] Psychiske Forhold som Aarsag til Svingninger i Leucocyttallet. Hospitalstidende No. 13, 1909, p. 406.

children and nervous persons — contains more leucocytes per unit than the succeeding drops. They also believe that this is due to the fear of the person experimented on for the prick of the needle.

I have several times observed this condition in myself, though I had no feeling of anxiety; I am therefore inclined to think that the true cause lies in vasomotor changes. After incision with the "Sneppert" (hidden needle) we may often notice a distinctly local ischæmia, frequently lasting some time, which is followed by a very copious flow of blood, and it seems to me, therefore, that a sudden vasomotor change might well have some effect on the number of the leucocytes, even if distinct paresis of the capillaries is of no importance in this regard.

I have also observed, however, suddenly occurring but very quickly disappearing changes in the blood-pressure (and pulse) owing to mental impressions and have been able to follow in part the corresponding changes in the number of leucocytes.

Diast. blood-pressure	Number of leucocytes
145	
175	
	7067
	5578

The first sample of blood was taken two minutes after the measurement of the blood-pressure, the second about 5 minutes later. I noticed suddenly during the investigation that the pulse of the medium had become hard and tense (on hearing conversation outside the room) and measured the blood-pressure over again in order to determine the increase; during the taking of the last sample the pulse was again as at the beginning. As will be seen from Fig. 3 (L. $^{18}/_{2}$ 07, p. 71) the two lowest values are the "normal" for the date mentioned.

It is undoubtedly the case, lastly, that in the morning after a wakeful night, at a time when the vasomotor system is in a state of "unstable equilibrium", we find unusually great variations in the number of leucocytes; as a rule also the numbers are higher.

It is reasonable and natural from this standpoint to find, as shown by Kjer-Petersen, that the number is as a rule more variable in women than in men.

In addition to these — we might well say "spontaneous" — variations we also find others, caused by various physiological changes, which in various ways alter the conditions of circulation. The "statiske Leucocytreactioner", variations in the number of leucocytes owing to changes in position, shown by Hasselbalch and

Heyerdahl[1], are very striking. I have also taken note of this condition by counting the leucocytes first in the sitting (s) and shortly after in the recumbent position (r). At the same time I measured the blood-pressure and the calibre of the arteries[2].

The frequency of the pulse was likewise taken in both positions. The measurements were made first in the sitting and then in the recumbent position in the order given.

	Position	Pulse	Calibre of artery	Diastolic Blood-pressure	Number of Leucocytes	
L.	s	74	2·6	170	4400[3]	blood from ear
	r	60	2·1	135	4933[4]	
	s	80	3·0	187·5	4489	—
	r	69	2·3	152·5	5133	
	s	87	2·8	130		
	s	87	2·1	112·5	4333	—
	r	71	2·0	107·5	4889	
	s	75	2·7	177·5	3889	
	r	54	2·4	127·5		—
	r		2·0	110	4400	
	s	70	2·7	170	5378	—
	r	56	2·1	120	5578	
	s	84	2·8	177·5	4022	—
	r	60	2.5	135	4778	
W.	s	70	3·2	210	6556	blood from
	r	62	2·9	182·5	7689	finger
	s	62	3·4	217·5	6156	—
	r	55	3·0	195	6978	
B.	s	59	1·6	142·5	5289	—
	r	55	1·5	107·5	6267	

[1] l. c.

[2] These were measured with Oliver's hæmadynamometer and arteriometer (the instruments are described by Oliver in "Pulse Gauging". London 1895). The first instrument after some experience gives the "diastolic blood-pressure" with an exactness of ca. $\pm$ 5 mm.; the systolic is less certain, as this is only given by the index standing still or remaining unchanged at a minimum. The pulse-pressure can therefore also be given only by estimate. The arteriometer, which is a much easier instrument to work with, has given me in a long series of experiments a coefficient of variability of 2—4 %.

[3] 7th drop. [4] 10th drop.

	Position	Pulse	Calibre of artery	Diastolic Blood-pressure	Number of Leucocytes	
	s	60	2·4	155	6089	blood from
	r	56	1·8	127·5	6533	finger
	s	72	2·2	150	4244	—
	r	68	1·8	100	6578	
	s	76	2·0	142·5	5489	—
	r	71	1·8	125	6667	
F.	s	60	2·4		6489	blood from ear
	r	56	2·1		7378	
	s	70	2·1		7156	—
	r	60	1·6		8711	
	s	60	1·9		8533	blood from
	r	56	1·6		8844	finger

Altogether 30 measurements were made on 4 different individuals. The highest number of leucocytes was in all cases found in the r position and the change must therefore be regarded as certain, even though the differences are much less than in the experiments of Hasselbalch and Heyerdahl. The reason for the small difference is most probably, that the blood was not taken directly after the change in position, as a period of 10—20 minutes must be reckoned to the first three determinations.

As I have shown in a previous paper, the mouth temperature falls on taking the r position, whilst the skin temperature on the head rises. The transition from the s to the r position has therefore the following results.

Pulse frequency	decreases
Calibre of artery	decreases
Diastolic blood-pressure	decresesa
(Pulse-pressure	increases)
Temp. in mouth	falls
Temp. on forehead	rises
Number of leucocytes	increases

The above measurements were all made on "fasting" individuals, i. e. shortly before meals. If the investigation were made after meal-times, we should find certain changes in the above scheme, as both the calibre of the arteries and the mouth temperature are often quite the reverse and the "leucocyte reaction" is also generally altered. But my observations are too few to determine whether there is any connection between these phenomena or not.

Another every-day occurrence which is of great importance for most of the above-mentioned functions is the meal-time. In order to test its influence on variations of the leucocytes, I have counted a number of samples of blood taken directly before and after meals, as soon after as the other preceding measurements permitted. These results do not touch therefore the question of the leucocytosis of digestion. The measurements are all from self observations.

Time	Rectal temp.	Mouth temp.	Pulse rate	Calibre of artery	Diastolic blood-pressure	Leucocytes
before	37·10° C.	36·2° C.	62	1·9	150	7267
after	37·25° -	36·7° -	80	2·2	127·5	9089
before	37·25° -	36·25° -	62	1·5	137·5	7222
after	37·30° -	36·70° -	78	2·0	127·5	8400
before	37·10° -	36·39° -	62	2·0	177·5	5756
after	37·35° -	36·95° -	79	2·1	125	6956
before	37·10° -	36·15° -	61	1·7	152·5	5600
after	37·40° -	36·85° -	74	2·1	115	6956
before	37·05° -	36·18° -	57	1·8	135	5867
after	37·33° -	36·80° -	74	2·0	117·5	6867
before	37·52° -	36·45° -	61		147·5	6400
after	37·40° -	36·70° -	80		142·5	8000
before	37·13° -	36·48° -	67		125	6444
after	37·40° -	37·00° -	77		117·5	5822
before	37·70° -	36·60° -	84	1·8	117·5	7044
after	37·40° -	36·95° -	87	2·0	167·5	8156

It appears from this series that the number of leucocytes increases during the meal-time, except on the second-last day, when the number of leucocytes after the meal, for some reason unknown to me, falls quite outside the remainder of the series. It appears further that the number of leucocytes is independent of the rectal temperature, in as much as it increases also on the days, when the temperature owing to previous bodily exercise falls.

Examination of dried preparations before and after meal-time likewise showed a distinct increase during the meal-time[1].

[1] Dried preparations were stained according to Leishman's method and were investigated in cedar-oil without cover-glass with Leitz immers. $^1/_{12}$, Oc. I. In

The total number of leucocytes was:

Before	After the meal	
133	272	
84	199	
180	277	
179	305	
262	248	immediately after a lesson in fencing,
142	329	
(52)	(105)	only 200 areas counted; preparation not good.

The greater increase during the meal may possibly be due to the circumstance that the samples of blood were drawn in closer proximity to this than in the first-given enumerations. The percentage proportions of the different leucocytes were:

	Before	After the meal
polynuclear	73·78 (73·88)	77·42 (77·54)
large mononucl.	13·67	7·79
small mononucl.	12·45	14·54
eosinophile	(0·1)	0·25 (0·12)

Taken separately the 12 preparations gave the following numbers, the different kinds of leucocytes being noted in the same order as above.

	Before	After meal-time		Before	After meal-time
08 6/1	105	195	9/1	125	253
	16	25		30	20
	12	52		24	32
	0	0		0	0
7/1	60	142	10/1	223	203
	13	16		23	12
	11	37		15	33
	0	2 (+2?)		(1?)	0
8/1	115	219	11/1	95	250
	33	21		19	33
	32	37		28	46
	0	0		0	0

each preparation 400 areas were counted and the preparation was moved as far as possible in such a way that the areas examined were in the same position in the different preparations.

The result of the differentiation is, therefore, that the polynuclear cells increase by 4 % and that the reduction in the number of the mononucleated only affects the large cells. And this result is not arrived at from one single preparation dominating the whole, but is found generally in all the preparations.

If we now draw up a scheme similar to that shown above for the changes in position, in order to exhibit the influence of the meal-time, it will appear as follows.

Pulse frequency	increases
Calibre of artery	increases
Diastolic blood-pressure	decreases
(Pulse-pressure	increases)
Temp. in mouth	rises
Temp. on forehead	rises
Number of leucocytes	increases

Comparing this with the previously given scheme it is readily seen, that the frequency of the pulse and the calibre of the arteries cannot have any determinative influence on the fluctuations in the number of leucocytes. This has already been pointed out by Hasselbalch and Heyerdahl so far as the frequency of the pulse is concerned. According to the same authors, the heightened temperature of the skin cannot come into consideration either, as blood from the skin made strongly hyperæmic by a chemical light-bath did not show an increased number of leucocytes. In the bath experiments of H. & H. the blood pressure increased with the number of leucocytes, whereas the reverse was the case in both my series of experiments. Of the hitherto examined functions there remains therefore the pulse-pressure as possible cause of the variations. My data on the pulse-pressure are as mentioned not exact, but the changes are nevertheless so great that we may consider them as real, for example, in the investigations before and after meals as also in the majority of the observations on the changes produced by change in position. The presumption finds confirmation in the last cases from other and more exact investigations[1].

It seems to me reasonable to conclude from the data that the rapidly occurring and quickly disappearing variations in the number of leucocytes, as the result of changes in position, meals, bodily movements of short duration or varying outer temperature, arise

[1] Heyerdahl: Om Sammenhæng mellem Antal af hvide Blodlegemer og Variationer i Pulstryk.

simultaneously with and probably as the result of increased pulse-pressure.

It follows from this, that the number of leucocytes must vary in the course of the day, and that the number on different days can only be compared when the samples are taken at the same time of day, after that the day has been passed in the same manner. The procedure of KJER-PETERSEN is therefore the correct one, namely, to take the blood samples in the morning before food has been taken, if the variations are to be studied from day to day, as it is

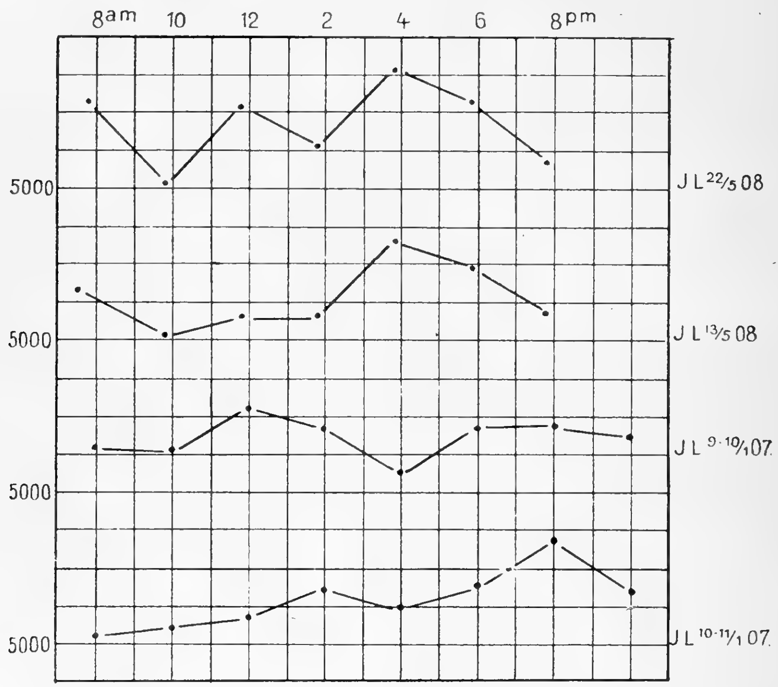


Fig. 1.

easiest at this time to obtain uniform material. Whether we should likewise obtain the lowest values at this time, as KJER-PETERSEN maintains, is however another question. In 5 day-curves derived from self observations I have only in 1 case found the lowest value in the morning (the difference between this and the next is here very small), in the 4 other cases the minimum falls in the second series, 2 hours later; this is also the case in 2 day-curves from another person. In a third individual the conditions were more irregular. In his case the lowest value was found in the morning in one instance, in another in the second series, whilst in two instances the minimum occurred later in the day.

The question is without interest in so far as KJER-PETERSEN maintains that the lowest value is the "normal"; this is quite a barren contention. On the other hand, it is not excluded that the result of these morning measurements may be dependent on special conditions, perhaps on the succeeding morning meal.

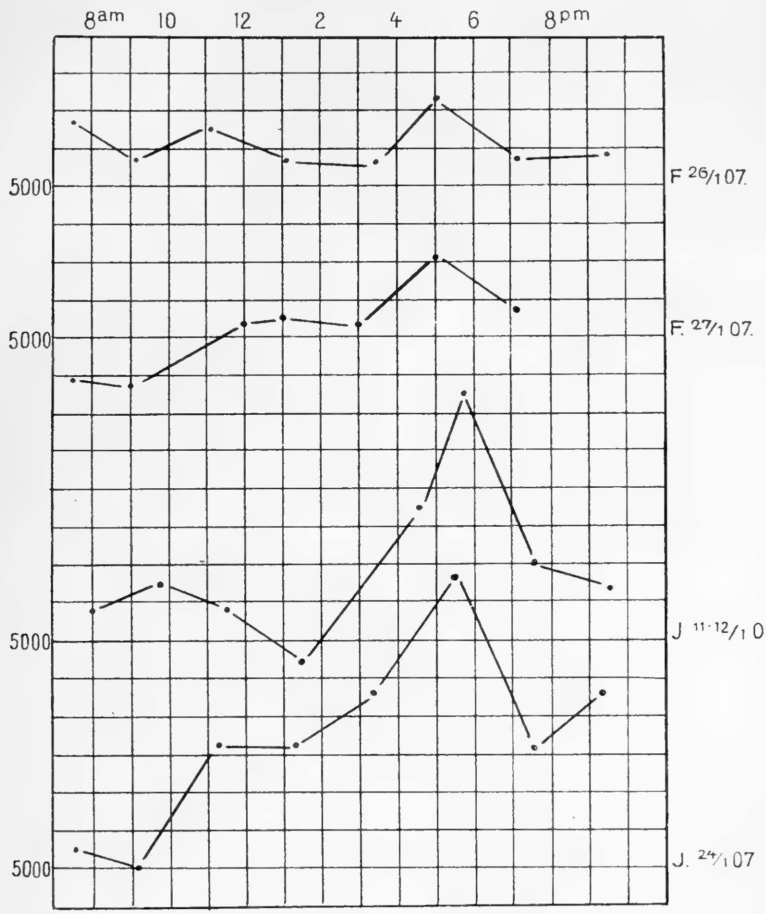


Fig. 2.

If we consider the 8 day-curves given above, it is not difficult to find in them a common type in spite of all differences. This type is most distinct in the first two curves, which come from two successive days, passed as far as possible in a uniform manner.

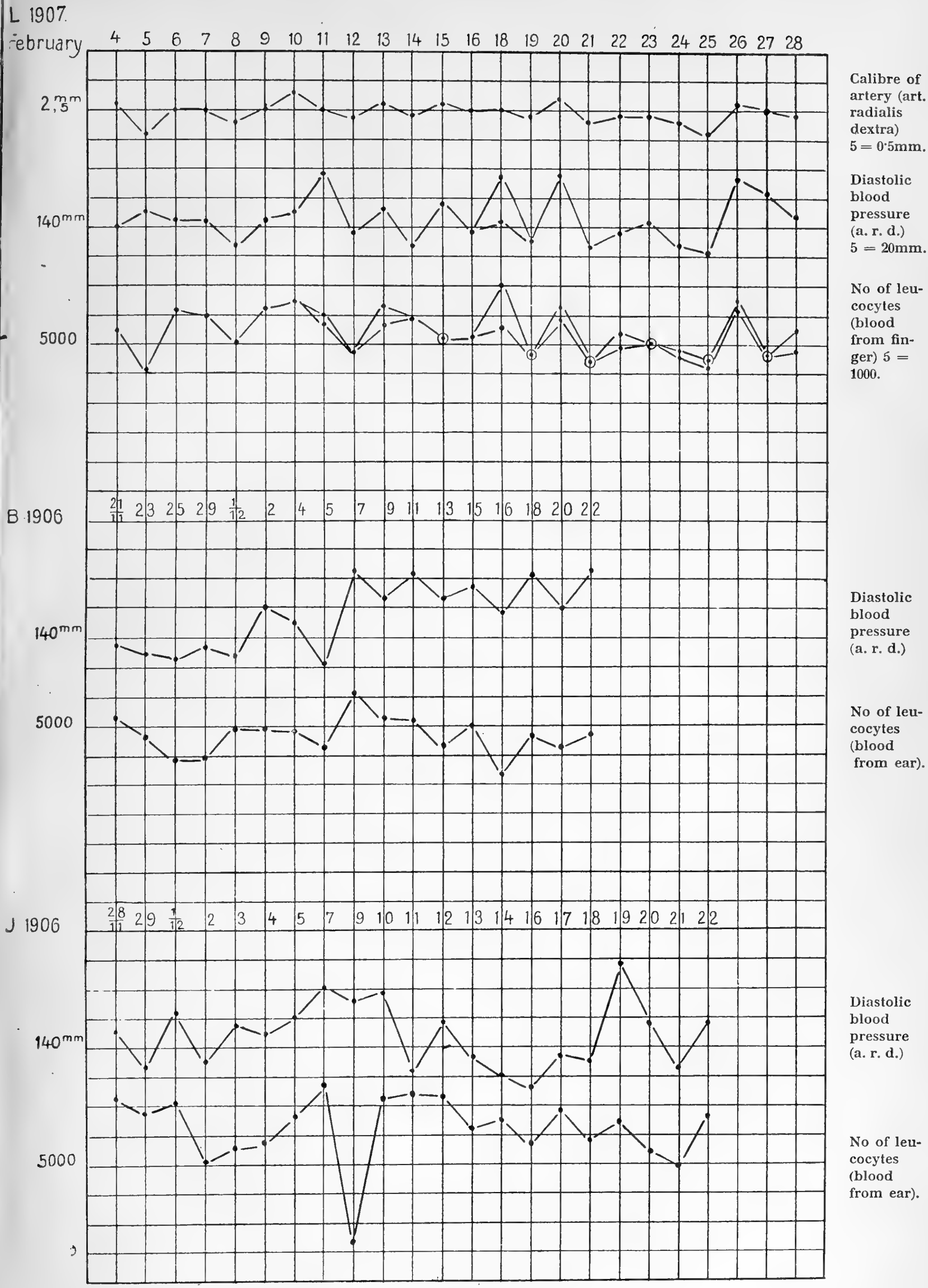
On both days I was merely occupied with sedentary work, taking no walks and meeting punctually to meals. The curves are all formed in the same manner; there are three maxima and these occur, in contrast to what is the case with the corresponding temperature curves — which also have three maxima — *before* the three principal meal-times. Taken in conjunction with what has been shown above with regard to the influence of the meal-time on the number of leucocytes, this does not mean that we find the maximum of the number of leucocytes before the meal-times; but it means that the number of leucocytes increases *towards* the meal-time, culminates at its end and then falls again, as a rule quite suddenly. Isolated observations would indicate that this rise before the meal-time does not occur if one is not hungry.

On the days represented by the first two curves, the 3 principal meal-times occurred at 8 a.m., 12 and 5 p.m.; the last is the usual "dinner". In the case of the other curves dinner occurred at 6 p.m.

The same form of curve is easily found again in curve 3 on remembering that the last meal-time was later. If we imagine the curve shortened by cutting out the part from 2—4 p.m., the resemblance becomes striking. The curves 5, 6 and 8 are likewise regular curves with 3 maxima, the form of which, when regard is paid to the different times for the samples and the distance of these from the meal-times as also to individual differences, supports the view that it is not the digestion but the meal-time which is the cause of the increase of the leucocytes in the capillary blood. Curve 7 differs mainly by the high value at 10 a.m.; this observation was taken immediately after defæcation under for the rest normal conditions. More important differences from the accepted norm is only shown by curve 4, the deviations in which I am unable to explain from the few notes taken. On the Expedition the first of the day's meals was unusually solid and was taken immediately after dressing; perhaps it was the getting up for this meal, which has as a rule given me higher morning values than were obtained 2 hours later.

If we consider the variations in the number of leucocytes from day to day, any endeavour to connect these with the variations in the pulse-pressure will be unsuccessful; nor have I been able to find the connection with the temperature, which Kjer-Petersen believes he has noticed.

As the following plan shows, however, there is an obvious parallelism between the variations in the diastolic blood-pressure and calibre of the arteries on the one hand and the number of leucocytes on the other. The three curves given are from three different persons. The first, L., was examined daily, while fasting, for about 4 weeks; after dressing he had a walk of some hundred yards to the laboratory. Daily measurements were taken of the pulse, calibre of the arteries, blood-pressure and number of the leucocytes. Enumeration of the different kinds of leucocytes in stained preparations


L 1907.
February
4 5 6 7 8 9 10 11 12 13 14 15 16 18 19 20 21 22 23 24 25 26 27 28
2,5 mm
140mm
5000
Calibre of artery (art. radialis dextra) 5 = 0·5mm.
Diastolic blood pressure (a. r. d.) 5 = 20mm.
No of leucocytes (blood from finger) 5 = 1000.
B 1906
21/11 23 25 29 1/12 2 4 5 7 9 11 13 15 16 18 20 22
140mm
5000
Diastolic blood pressure (a. r. d.)
No of leucocytes (blood from ear).
J 1906
28/11 29 1/12 2 3 4 5 7 9 10 11 12 13 14 16 17 18 19 20 21 22
140mm
5000
Diastolic blood pressure (a. r. d.)
No of leucocytes (blood from ear).

XLIV. Fig. 3. 6

was made 14 times; the erythrocytes were counted 16 times. Disagreement between the measurements for the blood-pressure and the calibre of the arteries was only found twice, $^{5}/_{11}$ and $^{11}/_{11}$; in both cases the blood-pressure was higher and in both cases the number of leucocytes follows the calibre of the arteries. In connection with what has been shown before this presumably means, that we are in these cases dealing with such a rapidly appearing and rapidly passing rise in the blood-pressure that it was over before the sample of blood was drawn; compared with the examination on $^{18}/_{11}$, described previously, we may perhaps think of psychical causes. For $^{15}/_{11}$ only is the disagreement more serious, as the number of leucocytes differs from both the other measurements. On $^{28}/_{11}$ the number of leucocytes is so very different that comparison with the other measurements is of no account.

As I have not been able to trace any connection between the pulse rate and the number of leucocytes, the pulse curve has been omitted.

The next curve is the result of measurements of the blood-pressure and the number of leucocytes in the individual B, the measurements being taken almost every other day at 11·30 a. m., about 3 hours after the first main meal. The last curve, J, comes from daily measurements immediately after dressing. Here also the curve only refers to the two functions named.

The above-mentioned parallelism, though recognizable, is not so distinct in these curves as in these first described, the reasons probably being several. In the first place the two individuals examined are in nervous regards not so stable as L.; in the second place the samples of blood in their cases were drawn from the lobe of the ear, in the first individual from a finger, whilst the blood-pressure in all three cases was measured on the art. radialis. Lastly, it has to be mentioned that the second curve does not come from morning measurements; the irregularities may perhaps in part be ascribed to this circumstance. On the other hand this curve serves to show, that if the mode of life is regular we are not absolutely restricted to morning measurements to obtain results which can be compared.

As the blood-pressure is in general undoubtedly higher in winter than in summer we should expect to find a corresponding difference in the number of leucocytes; this presumption also receives confirmation from my material so far as it goes. Enumeration of the different kinds of leucocytes in stained preparations indicates likewise a change in the relative numbers of the different forms.

The results of the examination of 43 preparations may be given here.

	J. L.				L.	
	IX—X/06		II/07	VII/07	XI/06	II/07
Polynuclear leuc.	73·9 %	(74·1)	60·1 %	62·1 %	81·8 %	70·2 %
Large mononuc. —	6·4 -		13·8 -	10·9 -	5·2 -	9·7 -
Small mononuc. —	19·3 -		26·0 -	26·9 -	12·4 -	20·1 -
Eosinophile —	0·4 -	(0·2)	0·1 -	0·1 -	0·5 -	0·0 -
No. of preparations	16		2	7	4	14

All the samples were drawn in the morning when fasting. In each preparation 400 areas were counted, the preparations being all examined as far as possible in the same manner. The average number of leucocytes in each preparation was 197; the total number of leucocytes counted was thus ca. 8500.

If we only consider the two main groups, mononuclear and polynuclear cells, we readily see that a relative increase of the first occurs during the dark period, an increase which in both cases amounts to ca. 12 % notwithstanding individual differences. Bruhn-Fåhræus[1] is of the opinion from Jolly's investigations, that the error in the determination of the relative number of 2 forms of leucocytes does not exceed 4 %, when at least 300 cells are counted. Even with the admitted limitations of my material I think we may therefore conclude, that the increase noted in the number of the mononuclear cells in winter-time is real. The following rounded percentages will show the comparison more clearly.

		Autumn	Spring
J. L.	Polynuclear cells	74	60
	Mononuclear —	26	40
L.	Polynuclear —	82	70
	Mononuclear —	18	30

Nevertheless the available data are as yet too few and too much subject to chance to permit of forming an approximately reliable picture of the nature and extent of the variations described here.

Some few observations on the numerical proportions of the erythrocytes may be added here.

For the work of counting I used, as already mentioned, Breuer's measuring cells with 105 times magnification. In each case I counted 2—300 blood corpuscles. The quotient is 25,000. All the samples of blood were drawn in the morning while fasting.

[1] l. c. p. 22.

J. L.	X/06	6·6 millions per mm.^{3}	(5 countings)
	II/07	6·3 — — —	(2 —)
	VII/07	5·9 — — —	(11 —)
L.	XI/06	6·3 — — —	(8 —)
	II/07	6·4 — — —	(16 —)

The average of 42 countings from two different individuals was therefore 6·3 millions of erythrocytes per cubic mm.

Whether this increased number of the red corpuscles is real or only apparent, I am unable to say, and the reason for it is just as little known to me. It may however be remarked that Ekelöf[1] has also found an increase to about 6 millions of the red blood corpuscles. In mountainous regions also a very considerable increase in the number of red blood corpuscles has been noticed, which in any case is partly real[2]. Zuntz considers the rarification of the air, perhaps also the lack of oxygen, as the cause of this increase. As there is however a very obvious agreement, so far as the respiratory phenomena are concerned, between high mountains and the arctic regions, and as there can hardly be any other reason for this than that the changes are in both cases due to the light, it is indeed not wholly excluded that the light is also of importance in the increase of the red corpuscles. In any case this is a point of view which deserves increased attention in future investigations.

With regard to the variations in the number of red blood corpuscles from day to day, these follow the same laws, so far as I can judge, as the leucocytes.

The quantity of hæmoglobin was determined according to Tallquist's scale in 16 individuals at the end of September 1906 and again in the same persons on the reappearance of the sun in February 1907.

There proved to be an average decrease of ca. 9 % (5—20). The decrease was distinctly shown in all the individuals examined. In the following winter I also noted a decrease in my own case of about 10 %; examination about 2 months later after a journey showed the hæmoglobin to be again at 100 %.

It would seem, in spite of the rough method of investigation, that the quantity of hæmoglobin decreases during the dark period in arctic regions.

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[1] Die Gesundheits u. Krankheitspflege während d. schwedischen Südpolarexpedition Okt. 1901—Jan. 1904.

[2] Zuntz and others: Höhenklima u. Bergwanderungen. Berlin 1906. Chape VI.

4—2—1910.

III.

CONTRIBUTION

TO THE

PHYSIOLOGY OF RESPIRATION UNDER THE ARCTIC CLIMATE

BY

J. LINDHARD

(WITH ONE PLATE)

1910

INTRODUCTION.

The conditions of life in the high-arctic regions present such great differences from those ordinarily met with in a temperate climate, that there is pressing need for an investigation into the biological and physiological phenomena displayed by beings under such conditions.

At the present time we have but very few and fragmentary physiological reports from these regions, especially as regards the physiology of human beings, and this though human races have lived and still live on the most northerly coasts and though numerous geographical expeditions have remained some years in the polar lands. This is due chiefly to two reasons, both of which can readily be traced in the present work.

In the first place, there is the difficulty of finding the right kind of people for arctic expeditions. It is only very few who possess sufficient scientific training to be able to undertake investigations alone at an age when a man can detach himself from his place in the community; still fewer are also so trained in physical regards that they can, without danger or damage to their health and working capacity, bear the exertions and discomforts occasionally met with during such expeditions. On the geographical expeditions, as a rule, attention has mainly and not without reason been paid to the physical conditions, whilst the scientific qualifications have been of subordinate importance. In the second place, the very unfavourable conditions for work on the expeditions have to be taken into account. The space for work allotted to each person is very strictly limited, and the dense crowding together of people who work at widely different things, makes it difficult to procure the necessary tranquillity for work. Nor is it possible, further, to introduce directly the ordinary methods used in laboratories. Bad light, difficult temperature conditions, lack of space etc. make it necessary to adapt oneself to these outer conditions, and this takes the courage out of any one not an expert master of his subject, or who, perhaps, simply from lack of better has been "appointed" as expert.

As these difficulties cannot be altogether eliminated from future expeditions, we can scarcely expect any very great results from the arctic field of work, until permanent stations are founded, where it will be possible for men with scientific training to live and work in these latitudes.

An endeavour in this direction has for the rest been made with the Danish Biological Station at Disco.

For excellent assistance during the preparations for the Expedition, as also later in drawing up this report, I am indebted to Dr. K. A. Hasselbalch, Director of the Physiological Laboratory of the Finsen Institute, and Dr. Aug. Krogh, Lecturer in Physiology at the Copenhagen University, who have always shown the greatest interest in my work and helped me in many different ways.

As it seemed little probable beforehand that my duties as doctor to the Expedition would take up my time to any great extent, I considered it from the beginning as my main task to collect observations, and as far as possible to undertake investigations tending to elucidate the influence of the arctic climate on the organism.

I was therefore well-provided with instruments for the measurement of temperature, for arteriometry and the measurement of blood-pressure as also for counting the blood corpuscles; lastly, I took with me the necessary apparatus for making respiration experiments, in the event that after learning the conditions I might find some reason for using it. The apparatus for the analyses could in any case be utilised for the determination of the carbonic acid tension in the atmospheric air and in the surface-water, as well as for its true function, the analysis of the expired air.

During the first autumn and winter I already began a series of preparatory experiments in order to make myself thoroughly acquainted with the instruments and methods; and after the first winter I had resolved that, if the conditions permitted it, I should endeavour to obtain a series of respiration experiments, so distributed over the year, that the true summer and winter periods as also the intermediate periods would be represented.

Before entering into the details of the respiration experiments, I may briefly explain the reasons which led me so to distribute them, that if an annual periodicity existed in the respirational phenomena it would show itself; at the same time I may state my general impression of the influence of the seasons on the health.

In August 1906 the Expedition succeeded in taking the ship up to 76° 46′ N. L., where it was laid up in winter quarters and remained until the return voyage in July 1908. In these regions the climate is distinctly arctic, i. e. there is a well-marked "dark period" and a just as well-marked "light period"; a period when the sun did not appear above the horizon at "Danmark Harbour" from November 1st to the middle of February, and a period when it did not sink under the horizon at our quarters from the end of April till the end of August. This is the great division-line in the arctic year. The seasons vary and the conditions are complicated by the temperature-movement, the annual period of which does not fall in line with that of the light; but so far as the health of human beings is concerned, this is of very little importance.

I very soon found that the "dark period" had in several ways a very distinct influence on the general condition and working capacity, although on the "Danmark Expedition", in contrast to most other expeditions to polar regions, the darkness did not prevent us going out to a considerable extent; short journeys on the sledge were undertaken, and hunting, as also numerous walks for the sake of fresh air and exercise; indoors also, sports were cultivated as far the limited space permitted.

The appetite in general was good; only one lost in weight during the winter. On an average the bodily weight was greater in the winter than in the summer half-year.

For many sleep was difficult, broken and accompanied by dreams; one often lay for hours without being able to fall asleep. Heaviness followed in the morning, as if after a night on the watch; endeavours during the day to make up for the sleep lost resulted simply in a worse night afterwards. Whereas, under ordinary conditions, a fall of the rectal temperature occurs during sleep, one found here not rarely a rise in the winter, in agreement with the above conditions; in one case, where the sleep was much broken, this was indeed the rule. The contrast from the deep, heavy sleep during the "off-watches" at sea was very striking[1].

Both the desire and capacity for work were reduced, especially the latter and especially for brain-work. As mentioned, one wakened up heavy and indisposed, with the feeling which is not unknown as "wooden"; one of the most energetic workers of the Expedition used this expression in a conversation with me, after I had written it down as characteristic of my own condition. The wish was present

[1] A contributory cause of this broken sleep was in various cases a frequently occurring and irresistible desire to micturate, probably a direct result of the cold; nothing abnormal could be detected in the urine in any of the cases examined.

to begin work seriously; the pipe was lit and the pen filled, but one got no further. The pen became dry and was again dipped in the ink; the pipe was smoked out, refilled and started again; always in the belief that some thing really was to be done or being done. It was only when the bell rang for dinner some hours later, with nothing done, that the inevitable conclusion was accepted, that one was unable to work. The "mental" work that suited us best was playing at cards; the object here to some extent corresponded with our powers.

Home-sickness in the true sense was not felt; but with some there was a tendency to have "dark forebodings" with regard to their homes. This was however only a form taken by the mental depression and discomfort. Feelings of depression were experienced by almost every one, but only in a single case did these deepen to slight melancholia. One felt oneself under the influence of the weather; depressed when the weather was bad, lighter when it again cleared up; and some investigations indicate that the meteorological factors really had an appreciable importance on the state of health.

One felt irritable and not disposed to be friendly, and the same qualities magnified were read into others. Conversation at table had a tendency to become fragmentary or even to assume a pointed form.

It may be admitted that certain outer conditions, which are not necessarily connected with the polar night and which were also not altogether unknown during the light period, may have had something to do with the state mentioned. The lack of cleanliness, for example; thus, the common feeding and living room was likewise used as the working room, as also for various other purposes, often not quite æsthetic; manners at table were as a rule "conspicuous by their absence" and the service was extremely primitive, at times unappetising. There can scarcely be any doubt, that when all forms are dispensed with in the common living room and on meeting, it will be difficult for any one, whose experience and training have not taught him complete self-control, always to show a presentable side to the surroundings.

There can be no doubt, however, that the polar night, the cold and the dark, especially the dark, were indeed the true cause. The same discomforts were all present in the summer, though in less degree, and yet one's whole existence both outside and inside was quite different during the light period. We did not feel so overcome and crushed by the natural forces as in the dark period; even with an open eye for all that was grand and imposing in nature one felt an impulse to exertion and to play our part. There was a desire to be active and we *could* work. We went unwillingly to bed, preferring to sleep just as the occasion offered; the sleep was short yet refreshing. One had the feeling of never resting and never tiring, which contrasted strongly with ths psychical helplessness of the dark period. We were disposed to be friendly towards one another, very willing to listen and likewise to talk.

With regard to the transition-periods, the spring was characterized at one and the same time by strong light and severe cold. We thus had the opportunity of convincing ourselves of the fact, that the temperature variations have but small influence on the health in comparison with that exerted by changes in the intensity of the light.

As my experience during the first arctic winter had thus led me to the conviction, that this, apart from a number of collateral causes, had a deep-going influence on my state of health, I came to the reasonable conclusion, that it would also have some effect on the respiration, whatever the summer might show. The more so, as my scattered observations had already indicated some factors in the physiological basis for the condition.

My plan was to make respiration experiments every two months throughout the year; but the conditions on the Expedition, journeys etc., made me alter my plan a little; instead of 6 series I obtained only 5, as I twice had an interval of 3 months. The observations fall, however, to some extent at the periods of the year, when the most marked differences were to be expected.

It was originally HASSELBALCH's discovery of the effect of light on the frequency of respiration and the blood-pressure, which led me to the idea, that these functions must also undergo change in the arctic regions according to the time of year; I expected especially to find during the dark period a change in the opposite direction to that discovered by HASSELBALCH. I also thought it possible that I might be able to contribute something towards the solution of the vexed question of the influence of light on metabolism. And lastly, it was my intention to test whether HALDANE and PRIESTLEY's hypothesis, that the CO_2-tension in the alveoli is an approximately constant quantity under varying conditions of life, would also hold good under the very different conditions met with on an arctic expedition.

During the preliminary experiments, which seemed to support my supposition with regard to the effect on the respiration-frequency and blood-pressure, I became aware of a relation between the frequency and the air-pressure. Any definite effect of the temperature I did not find in the respiration experiments, but as its influence on the blood and circulation was undoubted, I resolved to follow up any possible effect of these meteorological factors through all the series of experiments.

The respirational functions which were to be made the object of close investigation were therefore, frequency of the respiration, metabolism, carbonic acid tension in the alveoli and also the total amount of air respired. As I had to expect that the conditions for work would be difficult, my aim was to get along with the fewest possible and the least complicated apparatus. I therefore counted the respiration frequency myself during the experiments, which also had the advantage of giving me suitable "occupation". With regard to the metabolism, the oxygen analysis was given up and only the amount of CO_2 given off determined. I measured the expired air by means of a gas-measurer and analysed both the inspired and expired air. As I was unable to determine the amount of CO_2 given off for very long periods, I was obliged, in order to obtain comparable data, to take care that the experiments were carried out under as uniform conditions as possible; that is, I was obliged to make the experiments on fasting individuals and in the most comfortable positions possible, so that the influence of the muscles should be reduced to a minimum, and especially, in order that there should be no appreciable difference in this regard from the one experiment to the other. I was also obliged to take care that the composition of the inspired air was to some extent constant; I had to breathe directly from the outer air, as the air in the cabins often contained such a large amount of CO_2 that it had a distinctly appreciable effect, both on the respiration and on the cerebral functions. This led, on the other hand, to the misfortune, that I was obliged to give up the respiration experiments on days, when the weather was very bad, because the "breathing pipe" could not be kept open.

As already mentioned, it was necessary to restrict the number of instruments as much as possible and I resolved therefore to determine the CO_2-tension in the alveoli by calculation, for which indeed the experiments contained the necessary data; all the more, as the uncertainty arising from the "dead-space" in the calculations seemed no greater than the uncertainty, which for several reasons accompanies the direct determination according to the method of HALDANE and PRIESTLEY.

ARRANGEMENT OF THE EXPERIMENTS.

The methods of experiment were essentially the same as those employed by Hasselbalch[1] in his light-bath investigations.

I respired into a *metal funnel*, which by the help of Stent's composition fitted exactly on to the face, and thus, when smeared with lanoline-vaseline, closed air-tight round the nose and mouth. The funnel was connected by means of quite a short rubber-tube with the *valve-apparatus*, a Geissler apparatus with valve of thin guttapercha. The edge of the valves was fixed to the glass by small pieces of sealing-wax. The inspiration was made through a metal pipe, which was led up through the deck of the ship and ended free; it was connected with the valve-apparatus by a rubber-tube. From the upper bulb of the apparatus passed a short, ┌┘-shaped tube to a *mixing-vessel* surrounded by cold water. It may be mentioned, that whilst the tube of the funnel, as also the valve and rubber-tubes were 2 cm. in internal diameter, the metal tubes were only 1·6 cm. No irregularities were noticed however in the stream of air during the experiments, both the inspiration and expiration proceeding quite without force. From the receiver a short rubber-tube led to an ordinary, dry *gas-meter*, which indicated litres. This gas-meter was controlled before leaving and was found to indicate as much as 2 litres too little. Repeated experiments on returning gave the same result, but it also proved that the slower the air passed through it, the smaller was the error, and that the meter, in the first minute after the stream of air had stopped, itself partially adjusted the error, as the pointer moved a little on. As the stream of air during the respiration-experiments flows even slower than during the control-tests, there is reason to believe, that the error in the given volumes of air is of no account. As

[1] K. A. Hasselbalch: Det kemiske Lysbads Virkninger etc. Hospitalstidende, No. 45, 46, 47. 1905.

only whole litres are indicated an error of $\pm$ 0·5 litres is introduced as the maximum.

The thermometer was placed outside the gas-meter, at the side where the air passed in, in such a way that the receiver of the thermometer was in close contact with the metal, about half-way up the receiver. As it was impossible to maintain a constant temperature in the room, the thermometer was read immediately before and after the experiment, and the average of the two readings was taken. So far as I could determine, however, the rise and fall of the temperature proceeded quite evenly, and the error produced by the uncertainty of the temperature readings in the calculated volumes of air, where it occurs, is scarcely appreciable when whole litres are considered. The thermometer was divided into whole degrees, but tenths could certainly be read with some practice.

The height of the barometer was read from a pocket-aneroid, which was found to be trustworthy by comparison with a normal mercury barometer. As I very soon detected a possible connection between respirational frequency and air-pressure, the barometer was always read *after* the experiment. The nearness of the readings could not be placed closer than to 0·5 mm.

The time or duration of the experiment was determined as a rule by means of a stop-watch; when this was not working rightly (it could not stand exposure to extreme cold), one of the ship's chronometers or a reliable watch was used.

The number of respirations was counted by myself. Every time I came to 20, I moved 2 balls on a so-called "counting-machine", a wooden frame with 2 rows of 10 wooden balls moving on iron wires. On 100 being reached, I shoved this row back and moved on one ball on the second wire. The moving of the balls proceeded quite mechanically with small movements of the right arm.

Samples of the air expired were taken continuously during the experiment. In the side of the tube leading from the mixing-vessel was inserted a short, narrow brass-tube which connected by means of a rubber-tube with a lead-tube 0·5 mm. in diameter. This led to a sampling-receiver, which was arranged for my special purpose by Dr. KROGH. The air-receiver was provided at both ends with a tap with a double bore; the uppermost tap was connected by rubber-tubes with two lead-tubes, one of which, as mentioned, was connected with the mixing-vessel, the other served as connection with the apparatus for analysis. From the lower tap one rubber-tube led to a mercury-tank, from which the air-receiver could be filled, another was connected with the outlet-tube, which was drawn out to a capillary-tube of such a diameter that a suitable quantity of mercury could run out during the experiment. On the basis of experiments made by HASSELBALCH[1] I assumed that the mercury would in this way run our sufficiently regularly.

The analysing apparatus was a transportable apparatus for the determination of carbonic acid, as described by KROGH[2]. In accordance with advice from Krogh the apparatus was modified a little, so that it could be conveniently employed for the analysis both of atmospheric and expired air. The changes were, that the burette, instead of at once narrowing below to a capillary tube, first passed into a somewhat wider tube than the narrow one and with different divisions. Similarly, the absorption pipette was continued upwards into a somewhat wide tube before being drawn out into a millimeter tube; a circular mark was made both on the wide and on the narrow part of the tube.

The wide tube of the burette was divided into divisions of about 1 cc., marked from above downwards with the numbers 6 to 1, each of these being again divided into 10 divisions. Tenths of these could with certainty by read, twentieths are included in the calculation of the analyses; but owing to possible differences in the shape of the mercury meniscus, parallax etc. we must allow for a possible error of $\pm$ 0·1 of a division. Such an error, however, has not a disturbing influence on the results, when these are given to 2 decimal places. The error was reduced and the work made easier, when I had gradually accustomed myself to place the mercury column at once at a definite mark; this can be done with great accuracy. The narrow tube was divided into 20 parts and each of these was again halved by a partial line; tenths of such an interval could be read with certainty.

The calibration of the burette at the Physiological Laboratory of the University before leaving gave the following result:

Total volume (tap.—0)	60·060	cc.
6—5	1·005	-
5—1	4·0231	-
1—20 (between wide and narrow parts)	0·7585	-
20—0	0·19702	-
A division of the wide part of the tube	0·1005	-
- — - - narrow - - - —	0·009851	-

Analysis of the expired air was then carried out by leading so large a volume of air from the sampling-receiver over into the

[1] l. c. p. 7.
[2] Meddelelser om Grønland. Vol. XXVI. Kjøbenhavn 1904, p. 345.

burette, that there was an excess of pressure in this when the mercury stood in the narrow tube. Thereafter I brought the mercury column to one of the marks on the wide tube, opened the upper tap of the burette for a moment, to equalize the pressure, and then brought the absorption-liquid to the mark in the wide tube of the absorption-pipette by means of the movable receiver connected with the pipette. After absorption of the carbonic acid the absorption-liquid was again brought to this mark by means of the mercury. The manometer was thus not used in these analyses.

A source of error is introduced when the analysing apparatus is used in this way, since the temperature of the water-bath is of importance for the volume of air noted. This water-bath always contained plenty of glycerine or cooking-salt, owing to the cold at nights in the cabin, and its temperature was practically always lower than the temperature of the air. During the analysis, when the warmer air of the room was constantly being pumped through the bath, the temperature of the latter rose, usually by some few tenths of a degree. The temperature of the bath had therefore to be read directly after each reading of the amount of the air-sample. In calculating the results of the analyses a correction is made for these temperature changes and thus no uncertainty worth mentioning is introduced by these into the experiments.

The fact, that the absorption-liquid, owing to great changes in the air-pressure, may absorb or give off a measurable amount of other gases than carbonic acid, might occasionally introduce an uncertainty in the carbonic acid determination, which cannot be calculated but which may nevertheless be approximately estimated. Some examples of this will be shown in the discussion of the separate experiments. Apart from such errors, the importance of which must be judged in each separate case, we may most probably conclude, that the total error in the determination does not exceed $\pm$ 0·05 % CO_2. With regard to the ordinary use of the analysing apparatus, I may refer to Krogh's paper cited above.

The respiration experiment, which as a rule lasted 30 minutes, was carried out in a kneeling position, on account of the space available, with both arms resting and the face leaning against the respiration-mask. This position, after I had learnt to know it in the preliminary experiments, was convenient and easy to take up, so that it was almost exactly the same from day to day. Without changing the position of the body my right hand was able to move the balls on the "counting-machine", as also to reach the watch and a small glass which showed me whether the valves in the respiration-apparatus were working properly; with the left hand I could reach the tap on the outlet-tube from the sampling-receiver. After placing myself comfortably in my usual position, I began the experiment with an inspiration; where halves are noted in the number of respirations, this means that I also ended with an inspiration. The sampling of the respired air began first after 1 to 2 minutes, when the whole of the apparatus had been thoroughly permeated with expired air and after I was quite certain that I had settled down after the change of position. The sampling was concluded about 1 minute before the experiment came to an end, as in the last minute some attention had to be paid to the counting of the respirations and the watch.

I respired chiefly through the mouth during the experiment, which came quite naturally to me though I was usually accustomed to breathe through the nose.

As the time from my appointment to the Expedition until setting out was short, and the preparations for the voyage many, I was not able to find time and quiet to make the comparable respiration experiments beforehand. It has only been since my return that I have obtained the opportunity to make the experiments necessary for controlling the results from Greenland. As my health is now, as before and during the Expedition, invariably good, and my weight is the same as before leaving and my mode of life very much the same, I may assume that respiration experiments before the journey would have given the same results as those I shall now describe.

The control experiments, which were carried out at the Laboratory of the Finsen Institute, were made in a sitting position but otherwise in the same manner as described above, with the same apparatus in quite a similar arrangement.

In connection with these control experiments, I have made a series of direct carbonic acid determinations of my alveolar air according to Haldane's method. The procedure was as follows. I expired through a mouth-piece of glass into a cycle-tube, which, by means of a glass-tube inserted near the mouth-piece and a piece of rubber-tubing, could be connected with a glass-receiver holding ca. 80 cc. The receiver was provided above with a tap with wide bore and ended below in a rubber-tube, the other end of which was in connection with a mercury tank; this could be lifted or lowered and thus fill or empty the receiver. I had two of these receivers and could thus take the two air-samples shortly after one another, without requiring to move except to change the air-tube. Before the sampling began, the mercury was led up right through the tap of the receiver, which was then closed; the mercury tank was hung lower than the receiver. When the tap was opened on taking the sample, the mercury ran out of the receiver with great rapidity.

The air-sample was led over to the analysing apparatus by means of a lead-pipe. The "dead space", formed by the glass-tube above the tap, the lead-pipe and the connecting tube, was done away with by causing excess of pressure in the receiver and opening the tap a moment before the connection with the analysing apparatus was completed.

The analysis proceeded as described for the expired air; only it proved necessary, owing to the greater amount of carbonic acid contained in the alveolar air, to lead over the air-sample several times. I always made sure that the absorption was complete by taking over one extra sample for control.

EXPERIMENTS.

In the following pages I shall describe the separate series of experiments, beginning with the control experiments and then taking the Greenland experiments in chronological order. These experiments are shown in tabular form and the tables are collected later into a summary-table.

The tables give the following information. Serial number of the experiment, date, height of the barometer, temperature (gas-meter), duration of the experiment in minutes, number of respirations during this time, frequency of the respirations per minute calculated from the two previous factors; further, the air expired during the experiment in litres. As mentioned, the gas-meter may show as much as 2 litres too little; but it is extremely improbable that this error in the experiments amounts at any time to 1 litre; the error in reading amounts at most to $\pm$ 0·5 litres. Further, the total amount of air respired per hour at 0° and 760 mm. (dry). As the last is calculated from the previous factors, the possibility of error mentioned will also be present here; the possible error may indeed be multiplied by 2, but even in the most unfavourable cases these errors cannot amount to 1 % of the values noted. The error in the barometer and thermometer readings may have some influence on the figures for the total amount of air respired, but these errors are also of quite subordinate importance; in a chance experiment at approximately average pressure and temperature a change in barometric pressure of 0·5 mm. gives a correction of 0·4 litres in the reduced volume of air, whilst 0·1° on the thermometer gives 0·2 litres.

In the next column is shown the amount of an expiration at the given temperature and then the volume of an expiration at 37° saturated, calculated from the preceding. The error of the barometer and thermometer readings could here amount to 1 cc. and 0·5 cc. respectively. Then follows the amount of carbonic acid in the dry, expired air, calculated from the analyses. The error of reading might give a difference of at most 0·02 %; the total error in the analysis can scarcely, as mentioned, be taken at more than at the highest 0·05 % CO_2. Then the percentage of carbonic acid in the dry alveolar air is given calculated from Bohr's formula[1]. As the uncertainty is very small with the amounts composing the calculation, the errors in these numbers will be determined by the possible error, which is introduced by estimating the "dead space" and its variations due to temperature changes. A change in this value of 10 cc. gives in a chance case an error of 0·07 % or 1·5 % of the number noted. Errors of this kind will however chiefly affect the whole experimental series, they will scarcely have any influence on the form of the variation-curve.

The next column gives the amount of carbonic acid expired per kilo. and per hour in cc. If we assume that the maximum errors in the elements composing the calculation all tend in the same direction, we must here reckon with a possible error of ± 2 % of the number noted.

Errors in the analysis of the inspired air have no importance in this connection.

The bodily weight was determined by means of a steel-yard; the exactness may be placed at ± 0·5 kilo. On days when the weight was not taken, it was determined by graphic interpolation, on the supposition that the rise and fall proceeded regularly from day to day.

In the last columns are noted the pulse and in many cases the mouth or rectal temperature.

As a measure of the physiological variations I have used the "standard deviation", calculated from the formula $\mu = \sqrt{\frac{\Sigma \nu^2}{n}}$, whilst the uncertainty in the average is expressed by the probable error derived from the formula $\pm\, 0{\cdot}6745\, \frac{\mu}{\sqrt{n}}$.

The reasons why the same standards have not been used everywhere in the statistics of the variations, is that I wished to emphasize the difference between the physiological fluctuations of the organism and the errors, or uncertainty, which arise in the constants of the variation-series owing to imperfections in the statistical material. Naturally, the mutual differences between the separate experiments in a series are not all biological variations; as mentioned in the foregoing, there is a possibility of experimental errors up to a certain, given limit, but these errors are in most of the series so small

[1] Bohr: Respiratorischer Gaswechsel ect. Braunschweig 1905, p. 139.

in comparison with the variations, that we can reasonably exclude them in this connection.

It is admitted, that the formulae for large numbers can only be used with caution in cases of small numbers; I have nevertheless carried out the calculations also for the series with quite few numbers. It seems to me quite possible, that the calculated constants, even if they are not fully valid, mathematical expressions for the variation or uncertainty in the results in question, yet give a picture of — if I may so call it — the density of the series, which is easily grasped and which renders unnecessary a repetition of the series. And an average without such more complete determination appears to me quite without value. For example, 200 is the average of 199, 200 and 201 as well as of 150, 200 and 250, but the number is of very different value in the two cases; any addition therefore which indicates, even if only with an approximation, which of the two series is in question, must be regarded as a necessity.

It will often be difficult to determine, whether an experiment, which differs very greatly from the others, is due to chance errors or unusual circumstances. Even if the strongly deviating variants are relatively seldom, there is still the possibility of finding them in the smallest series; the apparently quite improbable number may also, when the number of chances is increased, prove to fall in quite naturally into the greater whole.

Leaving aside that such an irregular case may thus be quite a valid expression of the function investigated, it may however be necessary and right to exclude it from the calculation of the constants of the series, because it may by chance completely upset the average and alter the standard deviation, so that these factors come to give quite an erroneous impression of the actual appearance of the series. For this reason, after calculating an experimental series in the usual manner, I have determined its probable limits according to a formula given later; I have then recalculated the constants after exclusion of the values which fell outside the calculated limits. In all cases I have noted both the directly found and the corrected values for the average and μ.

The formulae used are given in Davenport's Statistical Methods (New York, 1904).

In the principal table 75 morning experiments are noted, distributed as follows:

Copenhagen,	February	1909,	13	experiments
N. E. Greenland,	April	1907,	14	—
	June	—	13	—
	August	—	10	—

November	1907,	10	experiments
January	1908,	12	—
May	—	3	—

In addition to the above, I have also made some experiments at different times of the day, chiefly in connection with temperature investigations made at the same time. These, in all 28, were carried out in November-December 1907 and in May 1908 and are noted in a special table along with some experiments from a third series in December 1906.

Altogether 107 experiments are noted in the tables. These are all self experiments; owing to the lack of space it was impossible for me to make experiments on others. There is nothing which would lead one to believe, however, that the characteristics appearing in my respiration and which are discussed more particularly below, are due to individual conditions; on the contrary, numerous analogies from the results of earlier investigators distinctly indicate that I have been quite a regular and normal subject.

The following table embraces 10 complete and 3 incomplete experiments. The agreement in the columns of the different, observed or calculated elements is very great, as the respiration in the various experiments has been perfectly regular and my condition, with exception perhaps of one day (No. 132), uniform; it is allowable, therefore, to regard the results as a sufficiently trustworthy expression of my respiration at the period mentioned. As I had become familiar with the respiration-mask and with the arrangements on the whole, and as the different parts of the apparatus with hardly any exception were working properly, no preliminary experiments were necessary.

On the second day of the experiments, a leak was discovered at the connection between the sampling-receiver and the analysing apparatus, and the first two analyses of the expired air had to be omitted as unreliable. In experiment No. 136 the minute hand of the stop-watch stopped at 28; though I took note of the ordinary works of the watch for the sake of control, I could not feel sure, whether the time was 30 or 31 minutes and I have therefore omitted from the comparison the elements of the experiment which are functions of the time. Experiment No. 138 has been omitted, as the watch stopped. The temperature in the laboratory was comparatively low on most days, but it is only for a few days that I have noted any great feeling of cold (experiments Nos. 128, 131 and 133). The low temperature during experiment No. 132 was less felt; my condition on that day was abnormal owing to lack of sleep. Defæ-

Series of Experiments, February 1909

No.	Date	Barometer	Temperature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour 0°, 760 mm., (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. air (dry)	CO_2 in cc. per kilo & per hour	Weight in kilos.	Pulse	Remarks
127	6	754	11.6 12.0	30	267	8.90	208	390	778	. .	. .	0.036	. .	. .	. .	64	
128	7	761	11.8 12.2	30	251	8.37	192	363	765	. .	. .	. .	. .	. .	. .	60 66	
129	8	766	12.1 13.1	30	245	8.17	203	386	828	943	3.20	. .	4.69	193	. .	58 59	
130	9	764	11.3 11.7	30	251	8.37	207	394	823	945	3.18	. .	4.65	195	63.5	60 60	
131	10	754	11.0 11.6	30	263	8.77	206	387	783	898	3.16	. .	4.73	191	. .	65 61	
132	11	762	9.0 10.0	30	269	8.97	214	410	796	919	3.21	0.035	4.76	205	. .	59 58	Slept less than usual. Extra meal at 11 p. m.
133	12	767	10.0 10.8	30	234.5	7.82	193	371	824	948	3.34	. .	4.88	193	. .	56 61	
134	13	765	11.1 11.9	30	251	8.37	197	376	784	898	3.20	. .	4.80	188	. .	57 59	
135	14	763	12.0 13.0	30	240	8.00	198	375	825	941	3.33	. .	4.88	195	. .	58 59	
136	15	744	12.3 13.4	(30)	246.5	(8.22)	202	(372)	820	935	3.31	. .	4.91	(194)	. .	57 59	Minute hand of stop-watch stopped at 28 min.
137	16	747.5	13.3 14.7	30	261.5	8.72	207	381	793	899	3.29	. .	4.92	195	. .	59 61	
139	18	759	12.7 13.7	30	257	8.57	200	375	778	884	3.22	. .	4.86	188	. .	58 60	
140	19	762	12.6 13.2	30	269	8.97	211	398	784	892	3.15	. .	4.74	195	63.75	59 60	

cation took place once in the day during the period of the experiments, in the evening, as a rule between 8 and 9.

The pulse was noted immediately before and after the experiment. The temperature of the body was not read.

It will be useful to note briefly the principal points in each column of the experimental series and later discuss the results all together; I shall thus deal with the respiration frequency, the total amount respired, the alveolar carbonic acid tension and the metabolism in the order mentioned.

The frequency of the respiration varies somewhat from day to day. The average is $8{\cdot}5 \pm 0{\cdot}072$, $\mu = 0{\cdot}37$ or $4{\cdot}36\ \%$ of the average. No influence of the outer temperature can be noticed. In the first part of the period, when my mode of life was quite regular, the frequency showed a tendency in the opposite direction to the air-pressure; in the later experiments, when my mode of life as well as the weather was less uniform, the tendency is less distinct but still on the whole recognizable.

In the separate experiments the frequency is very uniform. I have repeatedly counted it without the mask and convinced myself that it was the same as during the experiments.

No.	Number of respirations in			
	10 min.	15 min.	20 min.	30 min.
127	89	..	177	267
128	86.5	..	171.5	251
129	81.5	..	168	245
130	84	..	169	251
131	87.5	..	173.5	263
132	..	133	..	269
133	..	119	..	234.5
134	83.5	125	..	251
135	..	120	..	240
137	87	..	174	261.5
139	..	127.5	..	257
140	87	134.5	..	269

In the last case the number 87 is too low; I had the distinct feeling that a few of the respirations were longer at this time; on this day the respiration was on the whole not so regular as on the other days, owing most probably to a slight enteritis, which became apparent later in the day.

It was possibly due to the increasing feeling of cold during the experiment, that the frequency of the respirations several times decreases towards the end of the experiment. This is the case in most of the experiments in the first half of the series, whilst it is not present in the later, when the temperature in the room was higher and the cold less felt after the colder days preceding.

The total amount of air respired varies but little. The average of 12 experiments was $384 \pm 2{\cdot}4$, $\mu = 12{\cdot}46$ or $3{\cdot}25\,\%$ of the average. Experiment No. 132 gave a large positive deviation, experiment No. 128 the lowest; but neither of these deviations seems in itself improbable.

The amount of carbonic acid in the alveolar air is, as already mentioned, calculated from Bohr's formula $X = \frac{AE - aJ}{A - a}$. A is the volume of an expiration, a of the "dead space"; E and I the percentage of carbonic acid in the expired and inspired air respectively. The average amount of an expiration is given in the table. The "dead space" I have judged to give 300 cc., 150 cc. for the mask, connecting tube and valvular apparatus, and 150 cc. for my own person; the first of these was determined by means of water[1]. The calculated amounts for the carbonic acid are given in the table; as the carbonic acid in the inspired air varies very little and has here but little influence on the estimated alveolar air, I have been content with two determinations for it.

According to Bohr's reasoning, the carbonic acid in the alveolar air is found from the expiration phase. It will be less during inspiration and the difference will be all the greater, the greater the volume of the single breaths drawn. The accuracy of the determination will depend here, when the average volume is considered, only on the nearness with which we are able to determine the amount of the "dead space". As I have increased this by the whole arrangement of my apparatus, and as the increase can be measured, provided that all the air in the mouth and valvular apparatus takes part in the process, the error introduced in the conclusions will be comparatively small. The amount of the personal "dead space" is variously given; I have taken the average of the values.

Loewy[2] has endeavoured to determine the "dead space" in

[1] A correction for temperature has not been made, as the error thus introduced is of no importance worth mentioning and because the exact temperature could not be determined.

[2] Pflügers Archiv. Bd. 58, 1894, p. 416.

various ways. By taking casts on dead bodies he found 144 cc., a number he believes to lie very near the truth. ZUNTZ (according to information sent to LOEWY) found 140 cc. by a similar method. According to LOEWY, however, this is the anatomical, not the physiological "dead space". He has endeavoured without success to ascertain the latter directly by breathing into a tube with divisions, the separate parts of which could be isolated, so that each of the different portions of the expired air could be analysed separately. This method of procedure led, however, to quite improbable results; but the experiments showed quite a definite difference according to the mode of expiration. The "explosive" expiration gave the lowest numbers for the "dead space", "long drawn out" expiration the highest values. Even when LOEWY increased the "dead space" considerably, but small expirations were required to be able to detect the alveolar CO_2 which owing to vortices became mixed with the air from the "dead space". He is of the opinion also that these vortices arise in the trachea and bronchi, and that some physiological importance is attached to them there.

As the direct determination was unavailing, LOEWY endeavoured to calculate the extent of the "dead space". He knew the amount of an expiration, as also of oxygen used and the height of the barometer. By introducing now a supposed value for the "dead space" he could work out the alveolar tension. If he obtained a negative value for this, then the supposed value for the "dead space" was too high, etc. In this way he found that the "dead space" in the person investigated must lie between 100 and 140—150 cc.

HALDANE and PRIESTLEY[1] have endeavoured to determine the alveolar carbonic acid tension by direct analysis of the alveolar air. They thus avoid introducing any uncertainty with the "dead space", which, if their reasoning is correct, they are able to calculate, when they know the composition of the expired air and of the alveolar air, as well as the amount of the expiration.

HALDANE and PRIESTLEY'S method of procedure is, after a normal inspiration, to expire quickly and deeply into a rubber-tube provided with a mouth-piece; a sampling receiver is inserted close to the mouth-piece. As soon as the expiration is concluded, the mouth-piece is closed with the tongue, and the air-sample then taken. A corresponding sample is taken, by exspiring as deeply as possible after a normal expiration. These two air-samples, according to HALDANE and PRIESTLEY, should represent the alveolar air at the termination of the inspiration and at the termination of the expira-

[1] The Journal of Physiology. Vol. 32.

tion, and should thus give the minimum and maximum respectively for the percentage of carbonic acid. They take the average of these two values, and it is this factor they regard as constant for the individual.

It seems to me that the reasoning of HALDANE and PRIESTLEY is unsound, or rather perhaps, that it cannot be made to agree with the practice recommended by them.

If we maintain, as HALDANE and PRIESTLEY occasionally point out, that the production of carbonic acid proceeds continuously in the pulmonary alveoli, as also that the inspiration is not even throughout but from a fairly sudden and energetic beginning ebbs gradually before the expiration begins, as the pneumographic curves show, though the condition is less distinct than in the other phase, then the minimum of carbonic acid should be found immediately before the termination of the inspiration. And HALDANE and PRIESTLEY take their samples after the inspiration; certainly immediately after and with rapid expiration; but, in the first place, there must be time to open the mouth over the mouth-piece, and in the second, it is not possible to expire through such a momentary point of time, especially when the expiration has to be made more than usually deep. HALDANE and PRIESTLEY'S own results show this. As HASSELBALCH[1] has already remarked, their inspirational percentage of carbonic acid is in several cases higher than the expirational; and this appears even more strikingly in a later paper of HALDANE and FITZGERALD[2], in which experiments are made on quite untrained individuals. Here there can only be talk of experimental errors; that is, to make sure that their "dead space" was completely emptied, HALDANE and PRIESTLEY rightly made their expirations very deep and have therefore taken too long a time over them. I cannot help thinking, that HALDANE and PRIESTLEY'S inspiration-sample should come to agree to some extent with the value calculated from BOHR'S formula, when the expiration is made so great that one is certain that the "dead space" has been emptied. Some difference may well arise with regard to the rapidity of the expiration; HALDANE and PRIESTLEY have made experiments here, to show, that the percentage of carbonic acid may become constant, when the expiration has reached a certain stage; but by the side of so many disappointing samples which they show, these samples are very little convincing.

With regard to the second sample taken by HALDANE and

[1] l. c. p. 33.
[2] The Journal of Physiology. Vol. 32.

PRIESTLEY, it is at once obvious, that this is not taken from the tidal air but simply and solely from the reserve air, and as the maximum of the carbonic acid under a normal respiration occurs at the end of the expiration, this sample must necessarily give too high percentages for the carbonic acid. Expiration of the reserve air, as pure and simple active movement, can even less than the ordinary expiration occur momentarily; it cannot do so when one breathes out into the air, still less when the breathing takes place through a tube, which, as it is to be easily closed by means of the tongue, must be of comparatively small bore.

The objections, which I believe we may raise against HALDANE and PRIESTLEY'S absolute numbers, do not necessarily affect the results, however, so far as the variations are concerned. That an experimental series is affected by a systematic error, does not make it unusable, if we only know the direction and size of the error.

There is, however, still another source of error, the effect of which is much more difficult to estimate; as pointed out also by HASSELBALCH, the method presupposes, that the depth of the single respirations is quite the same, especially that no superfluous or unusually deep expiration immediately precedes the taking of the sample. The uncertainty which may arise in this way, is not small; in HALDANE and FITZGERALD'S paper above-cited we several times find differences of up to 1 °/o CO_2 in twin-determinations. This uncertainty becomes obvious when we calculate the "dead space" from the directly determined values. In one series given by HALDANE and PRIESTLEY the highest value for this is twice as great as the lowest. And even if the "dead space" is not an invariable quantity, not even in the same individual, yet such a series as mine given below, where the "dead space" enters into the calculation as a constant, will hardly be thinkable, if variations only approximately as large as those noted by HALDANE and PRIESTLEY were possible.

As too high a percentage of carbonic acid gives on calculation too high values for the "dead space", and as this is always estimated considerably higher by HALDANE and PRIESTLEY than by other investigators, who for the rest work upon different methods, I see in this a support for my view that the method of HALDANE must give too high values for the percentage of alveolar carbonic acid.

To test the agreement between the two methods as also to obtain a basis for the comparison of my investigations with those made by HALDANE and his co-workers, I have undertaken, as mentioned, a series of investigations of my alveolar air according to HALDANE'S method; the results of these observations are contained in the tabular summary given below.

Date	Barom.	CO_2 in alveol. air after Bohr		CO_2 in alveol. air after Haldane				Remarks
		%	mm. Hg.	inspir.	expir.	mean	mm. Hg.	
Febr. 09								
8	766	4.69	33.7					
9	764	4.65	33.3					
10	754	4.73	33.4					
11	762	4.76	34.1					
12	767	4.88	35.2					
13	765	4.80	34.5					
14	763	4.88	35.0	4.88	5.47	5.2	37.2	
15	744	4.91	34.2	4.86	5.87	5.4	37.6	
16	747.5	4.92	34.5	5.28	5.88	5.6	39.2	
17	754	..	..	4.86	5.57	5.2	36.8	
18	759	4.86	34.6	4.99	5.71	5.35	38.1	
19	762	4.74	33.9	4.83	5.71	5.3	37.9	
20	(765)	..	..	..	(5.45)	..	..	Barometric reading inaccurate.
22	770	..	..	5.03	5.19	5.1	36.9	
23	771.5	..	..	4.58*	5.54	5.1	37.0	* Average of 6 determinations.
24	771	..	..	4.50**	5.34**	4.9	35.4	** Average of 2 determinations.

The calculated values for the percentage of carbonic acid give the average 4·80 $\pm$ 0·018, μ = 0·089 or 1·9 % of average, and the partial pressure in mm. Hg. an average of 34·2 $\pm$ 0·12, μ = 0·59 or 1·7 % of the average.

The averages of the direct determinations give: average = 5·24 $\pm$ 0·043, μ = 0·19 or 3·6 % of the average; the partial pressure in mm. Hg.: average = 37·3 $\pm$ 0·21, μ = 0·95 or 2·9 % of the average.

Both series are thus very homogeneous. With the exercise in breathing quietly and evenly, which the respiration experiments afford, it was not difficult for me to carry out Haldane and Priestley's experiments afterwards.

I made some preliminary expiration experiments, not included here as the analyses were not as a rule carried out, in order to accustom myself to the arrangement, and then took some samples in immediate connection with the respiration experiments. In these cases the expiration was certainly rapid but yet quite under control and only a little greater than an ordinary expiration. The table shows that in the 5 successful, twin-determinations I have come to the same result by the two methods, but, it is to be remarked, only when I use the directly taken "inspiration-sample" for comparison. This however is just what we should expect from what has been said above. The table further shows, that the "expiration-samples" are the least variable, probably because they are easiest to make. On the last two days I tried to alter the mode of respiration, chiefly in the direction of breathing out more quickly or more explosively. The number 4·58 is the average of 6 analyses; it contains a good deal of meaning, taken with the remaining results; but the single determinations vary from a little over 4 to a little over 5, thus much more than the whole series from day to day. The last value is the average of 2 analyses, 4·6 and 4·4. It is noted for the last sample, as for some of the samples on the previous day, that the expiration was very short and explosive. I am thus of the same opinion as LOEWY, that the expiration should take place quietly and controlled; otherwise, vortices will easily be caused in the expiration-air, which mix this with the air in the "dead space" and thus affect the results.

If we carefully observe this rule, we should obtain values from HALDANE's "inspiration-sample", which agree with those calculated for the average of the expiration. HALDANE and PRIESTLEY's second sample is taken, as mentioned, from the reserve-air and will therefore, as my results also show, always give too high values.

As the standard deviation in my series of the averages is only 0·19, I venture to conclude, that I had complete control over the technicalities and presuppositions connected with the taking of the samples.

There seems to be a certain connection between the height of the barometer and the alveolar CO_2 tension, the variations of the latter proceeding on the whole parallel with the changes in the air-pressure. Looking at the matter broadly, when the barometer $\leqq$ 762, the CO_2 tension = 34·1, whilst when the barometer is > 762 the CO_2 tension = 34·3. Any influence from the temperature cannot be detected.

The production of carbonic acid varies inconsiderably from day to day. Experiment No. 132 is the only one that differs in this as in several other regards; the amount of carbonic acid given off on that day was as much greater than the next highest value, as the difference between the latter and the lowest value of the series. Calculating the limits for the series from the formula μk, where k is found from the integration table for the factor $\frac{2n-1}{4n}$ (n being as usual the number of variants in the series), this experiment falls outside the series; I have included it, however, as it does not exceed more than 3μ from the average in any of the columns calculated.

For the whole series we have: average = 194 $\pm$ 0·88, μ = 4·35 or 2·2 % of A. Omitting Exper. 132 we have: average = 193 $\pm$ 0·57, μ = 2·66 or 1·4 % of A.

The bodily weight remained practically unchanged during the

Series of experiments, April 1907

No.	Date	Barometer	Temperature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	Tp. Mouth	Remarks
29	13	774	10.5	31	302	9.74	267	500	884	1016	2.98	0.04	4.22	216	68	63	35.9	
30	14	771	9.0	30	300.5	10.02	276	536	920	1064	3.28	0.02	4.57	257	..	76	36.3	Gas-analysis not reliable.
31	15	771.5	7.5	30	258	8.60	225	440	872	1015	3.15	0.03	4.47	202	..	73	36.15	
32	16	770.5	8.25	30	261	8.70	231	450	885	1030	3.12	0.01	4.38	205	(67.5)	74	36.2	
33	17	769	6.6	30	293.5	9.78	232	454	792	926	2.99	0.03	4.41	199	..	70	36.15	
34	18	764	4.45	20	212.5	10.62	155	456	731	863	..	..	..	..	67.5	72	36.1	Exper. interrupted and begun again.
35	19	756	1.45	30	300	10.00	238	468	793	949	2.86	0.04	4.16	196	..	73	36.0	do. do.
36	20	752	11.9	30	327	10.90	258	483	789	903	2.80	0.01	4.17	198	..	70	36.2	
37	21	752	12.0	30	330	11.00	262	490	794	908	2.97	0.03	4.42	215	(67)	75	36.15	Gas-analysis hardly reliable.
38	22	750	11.6	30	303	10.10	266	497	878	1006	2.96	0.03	4.20	217	..	67	36.15	
39	24	760	9.3	30	294.5	9.82	257	491	874	1011	2.91	..	4.12	211	67	58	36.15	
40	25	766	12.5	35	327	9.34	288	469	881	1004	3.06	0.05	4.35	211	..	66	36.2	Exper. interrupted and begun again.
41	26	770	9.95	30	283.5	9.45	241	466	852	982	3.15	0.04	4.51	216	..	69	36.2	Samples not taken quite regularly.
42	27	758	7.5	30	272.5	9.08	233	448	857	999	3.05	0.04	4.35	203	(66.5)	64	36.0	

period of experimentation. The pulse was a rule steady. My pulse is for the rest very sensitive, any slight trouble with the putting together of the apparatus would be sufficient to produce the variations shown here.

We have in the series for April 1907 (p. 101) altogether 13 complete experiments, all from the mornings, made immediately after dressing; further 1 experiment, where the gas-analysis was unsuccessful. The gas-analysis is, however, also doubtful in two other experiments (30 and 36); in the first case, the temperature of the bath showed a rise of 0·5° between the first two readings, a difference strikingly great, even when allowance is made for the difficult conditions in the room. This experiment is in several regards so divergent, that I have thought it best to exclude it entirely from the series, the reasons for which will be discussed more fully below under the separate elements of the series. As my notes on the experiments at this time are scarce, I cannot give any reason for the divergence beyond what has just been said. In the second case (36) the control reading showed the height of the mercury in the analysing apparatus to be 0·3 of a division lower, with a simultaneous rise in the temperature of the bath of 0·1°; this is also a difference which is probably due to greater errors (that the absorption-liquid has given off gas, for example), but the experiment is otherwise quite normal. Two experiments (34 and 35) were interrupted, as the stop-watch was not going; both were completed immediately afterwards by the help of another watch. Experiment No. 40 was also interrupted and begun again, as the outlet-tube from the sampling receiver was stopped up by some dirt in the mercury. The sampling in this and the following experiment was not quite uniform. These small disturbances have, however, scarcely any influence on the results of the experiments.

The bodily weight has decreased evenly during the period of experimentation. The mouth temperature was measured daily before the beginning of the experiment; it gives, however, scarcely any information regarding the physiological condition of the organism, as it appears to follow very closely the movements of temperature in the room.

The pulse is somewhat high and varies not a little; on the first day it was taken (like the temperature) when recumbent before dressing, on the following days when sitting up after dressing. The causes for the variation in the pulse, especially in a downward direction, will be discussed under later experiments.

On several days during the experiments the temperature in the room was very low, as the petroleum-stove, which was lit while making the preparations, went out before the experiment was made; at other times it was only burning at half its height during the experiment. The low temperature was appreciable, on two days at any rate (exper. 34 and 35) it was undoubtedly 0° in the layer of air round about me; but I was not freezing, nor did I notice any "chattering" or "goose-flesh", nor any muscular stiffness beyond what always follows from taking up a definite position for some length of time.

Bringing together the experiments which were made at a temperature of less than 8·5°, the temperature which seems to form a boundary between the "ordinary room-temperature" and cold room, this series of experiments, as will be shown below, falls into two well-defined groups; the one made at temperatures with an average of 5·96° (group I) the other at an average of 11·11° (group II).

The frequency of respiration is fairly uniform within the separate experiments; in 3 cases where it was noted during the experiments it was:

102		96	95·5	for periods of 10 minutes
89		84	88	
	143·5		140	for periods of 15 minutes

On the other hand, the numbers vary not a little from day to day. In the 13 experiments (omitting 30), the average was $9{\cdot}78 \pm 0{\cdot}14$, $\mu = 0{\cdot}73$ or 7·5 % of the average.

The influence of the air-pressure is recognizable, especially on the series as a whole; but there are several divergences from the rule in details, partly arising from the influence of the temperature. Using the above-mentioned grouping we find, for group I, an average of $9{\cdot}46 \pm 0{\cdot}20$, $\mu = 0{\cdot}73$; for group II, the average is $10{\cdot}07 \pm 0{\cdot}15$, $\mu = 0{\cdot}61$. It appears therefore as if the frequency decreases with the outer temperature. There is however no conclusive evidence in these results; on the one hand, the difference is comparatively small, on the other the air-pressure works in the same direction, as the average height of the barometer for the two groups is respectively 765 and 760·5 mm., a difference, which by itself would be sufficient to explain the difference in the frequency. The significance of the "cold days" appears very distinctly, however, from a direct consideration of the curves; but this will later be made the subject of more detailed discussion.

Defæcation occurred once daily during the whole period of the experiments, as a rule in the evening, though several times also in the forenoon. There was never any desire to defæcate during the experiments, and there is therefore no reason to believe, that this factor has had any influence on the frequency.

Total volume of air respired. The total amount respired per hour is on an average 470 $\pm$ 3·63 litres, μ = 19·41 or 4·1 % of the average. The variation is not strikingly great; but the distribution of the deviations is of such a nature, that we may suspect some factors other than pure chance to have influenced the results. Experiment No. 30 is omitted; it falls here outside the limit calculated according to the method described above. The series becomes:

+ 30	
— 30	
— 20	
— 16	$\frac{\mu}{2} > 3 = 23·0$ %
— 14	
— 2	$2\frac{\mu}{2} > 6 = 46·1$ -
+ 13	
+ 20	$3\frac{\mu}{2} > 11 = 84·5$ -
+ 27	
+ 21	$4\frac{\mu}{2} > 13 = 100·0$ -
— 1	
— 4	
— 22	

This corresponds to a curve with two maxima. It appears already from direct observation that the maxima must lie at 450—455 and 480—490, as also that the negative variants fall on days with low temperature (cf. the table). If we separate the two groups, we obtain, for group I, the average 453 $\pm$ 2·34, μ = 8·50; and for group II, the average 485 $\pm$ 3·12, μ = 12·25. The distribution of the deviations in the first group is of some interest:

— 13
— 3
+ 1
+ 2
+ 15
— 5

There is a well-marked fall and then a regular rise in the 5 succeeding "cold days"; and again a fall on the last, isolated cold day. This is the same sequence as appeared for the respiration frequency, and it will reappear, but in a reverse order, in the alveolar carbonic acid tension.

On the other hand, there does not seem to be any constant relation between the variations in the total volume respired and the fluctuations in the air-pressure.

The alveolar carbonic acid tension does not show such simple and uniform conditions in this series as in the control series.

If we take the 12 experiments together (experiment 30 being excluded, as also 34, in which the gas-analysis is wanting), the percentage of carbonic acid is on the average $4{\cdot}32 \pm 0{\cdot}025$, $\mu = 0{\cdot}129$ or $3{\cdot}0\,\%$ of the average. Expressed in mm. of mercury, the average is $30{\cdot}9 \pm 0{\cdot}22$, $\mu = 1{\cdot}15$ or $3{\cdot}7\,\%$ of the average.

Group I, which here only embraces 5 cases, gives for the average $4{\cdot}36 \pm 0{\cdot}033$, $\mu = 0{\cdot}109$ or in mm. of mercury, the average is $31{\cdot}3 \pm 0{\cdot}309$, $\mu = 1{\cdot}03$. For group II, the average is $4{\cdot}28 \pm 0{\cdot}034$, $\mu = 0{\cdot}13$; in mm. of mercury $30{\cdot}6 \pm 0{\cdot}29$ and $\mu = 1{\cdot}14$.

We thus find a higher alveolar carbonic acid tension on the cold days than on the days with ordinary temperatures; but the two groups are not sharply separated. Just as a gradual adaptation to the cold could be noticed in the total volume of air respired, we can see the same thing here, when we consider the deviations in group I. The carbonic acid tension falls in the first four experiments, then rises again in the isolated experiment No. 42. The deviations from the average are.

+ 1·1
+ 0·4
+ 0·7
— 1·8
— 0·4

This series is however obviously disturbed by the variations in the air-pressure. Thus, if we consider the deviations in the separate experiments of the whole series, we find:

— 0·2	Bar. 774
+ 1·5	771·5
+ 0·8	770·5
+ 1·1	769·0
— 1·4	756
— 1·5	752
+ 0·3	752
— 1·3	750
— 1·5	760
+ 0·4	766
+ 1·8	770
± 0	758

We see here, that, with a single exception, for all the negative deviations the barometer is 760 or below; for all positive deviations it is 766 or more. Dividing the series into two groups on this basis, we have, when the barometer is $\leq$ 760, the average in mm. of mercury $= 30{\cdot}0 \pm 0{\cdot}21$, $\mu = 0{\cdot}75$; when the barometer is above 760, the average is $31{\cdot}8 \pm 0{\cdot}18$, $\mu = 0{\cdot}67$; the distinction is thus very clear.

The amount of the carbonic acid per kilo. and per hour, for the 12 experiments, is on the average $207 \pm 1{\cdot}45$, $\mu = 7{\cdot}47$ or $3{\cdot}6\,\%$ of the average. Including experiment No. 30 the average is $211 \pm 2{\cdot}8$, $\mu = 15{\cdot}0$ or $7{\cdot}1\,\%$ of the average. Experiment 30 has the value 257; its deviation from the average is thus $+ 46$ or $3{\cdot}06\,\mu$. The probability for a chance deviation of this order is 222:100000 or about 1 in 450. Calculating the highest probable limit of the series according to the method already shown we have 242·5. It seems justifiable, therefore, to exclude experiment No. 30 entirely. If we also exclude No. 36, we have for average $208 \pm 1{\cdot}46$, $\mu = 7{\cdot}20$ or $3{\cdot}5\,\%$ of the average. The deviations from the average in the 12 experiments are distributed as follows.

+ 9				
— 5				
— 2				
— 8	$\frac{\mu}{2}$	> 1	=	8·3 %
— 11				
— 9	$2\frac{\mu}{2}$	> 5	=	41·7 -
+ 8				
+ 10	$3\frac{\mu}{2}$	> 12	=	100 -
+ 4				
+ 4				
+ 9				
— 4				

This again leads to the above-noted divisions and gives for group I an average of $201 \pm 0{\cdot}95$, $\mu = 3{\cdot}16$; group II, average $212 \pm 1{\cdot}56$, $\mu = 6{\cdot}14$; In this last group, however, experiment No. 36 falls outside the calculated limit; excluding it we obtain for the average $214 \pm 0{\cdot}67$, $\mu = 2{\cdot}45$; the standard deviation thus decreases by more than half of its previous value, and the two groups thus show themselves to be very distinctly separate.

The following table includes 12 complete and one incomplete experiment. This last was interrupted by the inspiration-valve of the valvular

Series of Experiments, June—July 1907

No.	Date	Barometer	Temperature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour 0°, 760 mm., (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. air (dry)	CO_2 in cc. per kilo & per hour	Weight in kilos.	Pulse	Tp. Mouth	Remarks
44	20	753	8.55	35	277	7.91	314	512	1133	1315	3.15	0.04	4.06	237	..	62	35.7	
45	21	756	12.05	20	145	7.25	172	485	1186	1355	..	..	..	..	67	62	35.9	Valvular apparatus out of order.
46	22	758	11.55	30	234.5	7.82	272	514	1162	1331	3.16	0.04	4.06	239	..	59	35.8	
47	24	762.5	10.4	30	227	7.57	280	535	1233	1419	3.02	0.05	3.82	239	(66.5)	63	35.75	
48	26	760	12.7	30	216	7.20	263	495	1218	1388	3.23	0.03	4.14	238	..	68	35.9	
49	27	764	11.6	30	227	7.57	249	474	1097	1256	3.18	0.04	4.17	224	..	61	35.8	
50	28	764	11.6	30	218	7.27	260	495	1193	1365	3 23	0.03	4.12	240	(66)	57	36.05	
51	29	766	10.9	30	211	7.03	236	452	1118	1283	3.41	0.04	4.44	231	..	61	35.95	Slight uncertainty about the watch.
52	30	761	11.65	30	218	7.27	272	515	1248	1428	3.14	0.03	3.98	243	..	68	..	
53	1	760.5	11.05	30	214.5	7.15	258	490	1206	1384	3.32	0.03	4.22	246	65.5	70 66	36.0	
54	2	764	10.65	30	226.5	7.55	279	533	1235	1419	3.12	0.03	3.96	251	..	59	36.2	Heated and perspiring on waking up.
55	3	766	10.9	30	199.5	6.65	229	438	1151	1321	3.37	0.03	4.34	225	(65)	61 52	35.95	
56	4	764	12.7	30	208	6.93	264	500	1269	1446	3.18	0.03	4.00	243	..	58	35.8	

apparatus becoming loose and thus not working properly. The gas-sample obtained was too small for analysis. The stop-watch again went wrong in this series (No. 51), but the experiment was continued and the time taken from the ordinary works of the watch. This means an uncertainty of some seconds, but, even if we take a possible error of 15 seconds into account, which is certainly too high an estimate, the uncertainty is still less than 1 %, so that there is no occasion to exclude the experiment. In a second case (experiment No. 54), I woke up sweating and uncomfortably warm. The temperature reading also shows the highest number of the series, in spite of a relatively low temperature in the room. Though I have no doubt that special conditions prevailed during this experiment, in which all the elements investigated are strikingly high, there is no sufficient reason here either for excluding the experiment from the calculations. It is noted for the first two experiments, that I felt myself slightly uncomfortable, without being able to find any other reason for this than — contrary to the usual — a purely psychical irritation towards the mask.

The pulse during this period was likewise very variable, but on the whole distinctly less frequent than in the previous series. The pulse was counted in a recumbent position before dressing (the temperature was measured during dressing); where two frequencies are noted for the pulse, these were counted respectively before and after micturition. At this time I began to take note of the desire to micturate and the micturition as the possible causes of the relatively great fluctuations in the frequency of the pulse in the mornings. Defæcation occurred as a rule once daily in the forenoon. The bodily weight is on the decrease during the whole period; considering my regular mode of life, I venture to conclude that the decrease has proceeded evenly.

The respiration frequency is considerably lower than during the previous period of experimentation and it is also much less variable, partly perhaps because the fluctuations of both the barometer and temperature are less. For the rest, the influence of the air-pressure on the variations from day to day is not distinctly recognizable.

With regard to the frequency during the separate experiments, its regularity was throughout irreproachable. I have only noted it in one case; experiment 46 gave 116·5—118 respirations for every 15 minutes.

The average for the frequency is $7{\cdot}32 \pm 0{\cdot}067$, $\mu = 0{\cdot}36$ or 4·9 % of the average. The distribution of the deviations is quite regular.

The total volume respired has increased. The average is $495 \pm 5{\cdot}1$, $\mu = 27{\cdot}4$ or $5{\cdot}5\,\%$ of the average. Some very great deviations occur in the series, a very low value especially in experiment No. 55; calculating the several times mentioned limits, we obtain for the lowest value 438·3, which thus affects just the doubtful case. As the distance from the limit lies however within the error of calculation, and as the distribution of the deviations is perfectly regular, I do not think it justifiable to exclude this experiment. The relatively great variability in this series must certainly be regarded as an expression of the physiological state of the organism during this period, the arctic spring.

As the total volume respired, in spite of the decreasing frequency, has increased, the depth of the single respirations must have increased to quite a considerable extent.

The alveolar carbonic acid tension is not very variable, when we except a single large variation in a downward direction and 2 somewhat too high values. Whilst the two latter accompany the two highest barometer readings, the opposite condition does not hold good for the low value; no connection can be noticed in this series between air-pressure and the percentage of carbonic acid. Translating the numbers into mm. of mercury pressure does not alter the appearance of the series.

The temperature has been very regular, and I have not noted any feeling of cold, though it seems as if the effect of the cold was apparent in the experiments 51 and 55, as all the elements in these are in agreement with group I in the April series. Such a condition would explain the two large, positive deviations, the first of which falls approximately at the calculated limit of the series ($\pm$ 2.038 μ). All the experiments must, however, be included in the calculations for the series. The average is $4{\cdot}11 \pm 0{\cdot}032$, $\mu = 0{\cdot}16$ or $4{\cdot}0\,\%$ of the average. In mm. mercury pressure the average is $29{\cdot}4 \pm 0{\cdot}24$, $\mu = 1{\cdot}2$ or $4{\cdot}1\,\%$ of the average.

The three divergent cases mentioned produce a relatively large standard deviation, from which again it follows that a relatively large number of cases deviate less than μ.

The amount of carbonic acid expired shows throughout slightly higher values for the latter half of the series than for the first, yet not so much as to justify a separation into special groups. The high number in experiment No. 54 is certainly due to unusual conditions, as already mentioned; but I am quite unconscious of any special conditions on any of the days. Nor can I explain the few large negative variants; on two days (experiments 49 and 55) I have noted that I was very sleepy during the experiment, but this has also occurred on other days without causing any apparent deviations in the results. The agreement of these experiments with those made on the cold days in April has already been mentioned.

The calculated limiting values of the series are 222·3 and 253·7. The average is 238 $\pm$ 1·5, μ = 7·7 or 3·2 % of the average.

The series for August 1907 is so well-defined and uniform, that in spite of its small number of experiments I must consider my respiration as sufficiently well characterised at this time.

Of the 10 experiments only 8 are complete, the gas-analyses in the first two experiments being unsuccessful; the gas-sample was too small, giving rise to a negative pressure in the burette, which rendered the analysis impossible. The outlet-tube was changed, but it appeared that the mercury ran out too quickly from the new tube; the taking of the samples in experiment No. 59 was therefore not continuous but piecemeal. This had no influence, however, on the progress of the experiment, as I was able without difficulty to reach the outlet stop-cock; and as the respiration was regular, I believe that the air-sample taken gives a correct average value.

Defæcation occurred once in the day during the period of experimentation, as a rule in the evening; but there were two exceptions, on the 16th it occurred immediately after the experiment and apparently affected the respiration during this, on the 19th immediately before the experiment.

The pulse was throughout lower than in the previous series; but the source of error mentioned above has distinctly made its influence felt.

The rectal temperature was taken immediately after waking up, thus before dressing; it varies very little, but is on an average slightly less than the average value of my morning temperatures, probably because I got up earlier than usual during the period of experimentation and was therefore very sleepy, so soon after being wakened.

The bodily weight was only determined once during the period. From weighings taken outside the range of the table, however, I can conclude, that the weight has been the same during the whole period.

The respiration frequency was regular in the separate experiments. In one case only (No. 62) I have noted that the frequency was not so regular as usual, and that I twice felt myself uncertain as to the number; I consider the number given as reliable, however. The abnormally high frequency in experiment 60 is certainly due to the desire to defæcate which arose during the experi-

Series of experiments, August 1907

No.	Date	Barometer	Temper-ature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	Rectal temp.	Remarks
57	13	756	12.1	30	213	7.10	274	515	1286	1469	..	..	..	..	..	64	36.2	
58	14	761	11.3	30	215	7.17	269	511	1251	1434	..	..	..	..	..	60	36.2	
59	15	761	10.75	30	208	6.93	265	504	1274	1464	3.19	0.01	3.99	243	..	55	..	Samples not taken regularly.
60	16	756	12.0	30	234	7.80	299	562	1278	1460	2.83	0.03	3.55	240	65.5	60	36.2	Troublesome desire to defæcate during experiment.
61	17	758	11.05	31	228	7.35	297	544	1303	1496	2.89	0.01	3.60	237	..	53	36.3	
62	18	750	10.3	30	203	6.77	279	524	1374	1583	3.11	0.01	3.83	246	..	63	36.4	
63	19	754	9.9	30	214	7.13	275	520	1285	1483	3.07	0.03	3.83	241	..	54	36.2	
64	20	754.5	10.9	30	211.5	7.05	273	515	1294	1487	3.10	0.01	3.88	241	..	68 50	36.4	
65	21	758.5	10.4	30	209.5	6.98	283	537	1354	1558	2.99	0.01	3.68	242	..	57	..	
66	22	761	8.5	30	197.5	6.58	251	482	1274	1478	3.32	0.01	4.15	241	.	54	..	

ment. The number falls a little outside the calculated limiting value, which is 7·70. The low number in experiment 66, taken in conjunction with the other elements of the experiment, must be considered as due to the influence of the low temperature in the room. No relation with the air-pressure can be detected. The average of the series is $7{\cdot}09 \pm 0{\cdot}066$, $\mu = 0{\cdot}31$ or 4·4 % of the average; if experiment 60 is omitted, the average is $7{\cdot}01 \pm 0{\cdot}048$, $\mu = 0{\cdot}22$ or 3·1 % of the average.

The total volume respired reaches its maximum in this series. The average is $521 \pm 4{\cdot}2$, $\mu = 19{\cdot}5$ or 3·7 % of the average. The limiting values are $521 \pm 38{\cdot}2$; two numbers fall outside these limits, namely, experiment 60, where the high frequency was accompanied by an unusually large volume respired; it must be remembered, however, that the higher the frequency is, the greater will be the share of the "dead space" in the total volume respired, so that the change in the effective total volume respired is much less marked. The opposite is the case with experiment 66, where the frequency is strikingly low. Excluding these two experiments, we obtain for the remaining 8 an average of $521 \pm 3{\cdot}0$, $\mu = 12{\cdot}5$ or 2·4 % of the average. As the frequency is low, the volume of the single expiration is very large. This deep, slow respiration was not of a forced character; it was connected as a rule with distinctly good health. My immediate impression was, that at the end of the expiration or rather before the inspiration there was a distinct apnoic pause; but I was unable to undertake any further investigation of this condition.

The alveolar carbonic acid tension was small. For the percentage of carbonic acid the average was $3{\cdot}81 \pm 0{\cdot}046$, $\mu = 0{\cdot}19$ or 4·9 % of the average; estimated in terms of mercury pressure the average is $27{\cdot}1 \pm 0{\cdot}33$, $\mu = 1{\cdot}37$ or 5·1 % of the average. The last value is obviously high, apparently influenced by the air temperature; it falls just on the calculated limit of the series. Excluding this, the average becomes $3{\cdot}77 \pm 0{\cdot}038$, $\mu = 0{\cdot}15$ or 3·9 % of the average. In mercury pressure the average is $26{\cdot}7 \pm 0{\cdot}27$, $\mu = 1{\cdot}05$ or 3·9 % of the average.

The amount of carbonic acid given off is very little greater than in previous series. The average is $241 \pm 0{\cdot}58$, $\mu = 2{\cdot}4$ or 1·0 % of the average. The most striking thing about the series is, that experiment No. 66 does not, as might be expected, give a value under the average.

As a contrast both to the foregoing and to the later series, I have a series of experiments for November in which the numbers vary far beyond the limits I had previously considered as "normal", so far as I was concerned.

On November 2nd I returned to the ship after a sledge-journey of 6 weeks, which had been very fatiguing. Such a journey has in several respects quite a considerable effect physiologically; I would take the occasion, therefore, with this experimental series in mind, to refer to some conditions which may be of importance in judging it.

During the journey metabolism was greatly increased; we ate enormously twice a day and worked vigorously and unceasingly in the interval. The sleep at night was often uneasy and insufficient, and no inconsiderable amount of heat was required to thaw the frozen sleeping bag. As an example it may be mentioned, that the sleeping bag of one of my comrades on the journey proved to contain on our return 7 kilos. of ice in the inner, hairy layer. As a rule, therefore, one loses not a little weight on the first part of the journey; if the journey lasts some time, one learns to eat so enormously, that the loss is for the most part made up. After the return, the habit of eating a great deal is maintained for a time and one thus regains in weight, until the appetite again accomodates itself to the changed conditions. My weight before the journey, on $^{20}/_{9}$, was 66·5 kilos., on the return, $^{3}/_{11}$, 64·5 kilos., a week later, on $^{12}/_{11}$, I weighed 68·5 kilos. and had thus increased about 0·5 kilo. per day; then the increase in weight became less; on the $^{1}/_{12}$, I reached 70 kilos. Then the weight began to decline quite slowly, as is usual under conditions of sedentary work.

The great increase in weight during the first days was, however, perhaps only partially real; a few days after the return, namely, a very considerable œdematous swelling developed on the feet and lowermost part of the crura. This phenomenon is said to be fairly well-known among soldiers after a fatiguing march; I have not observed such a thing on myself at home, though I have gone very long distances on foot; on the other hand, I saw the œdema appearing on another occasion during the Expedition, after a 19 hours' continuous march in somewhat trackless territory. It was then less obstinate perhaps than after the sledge-journey, occurring at a time of year when one could get about, but had for the rest quite the same character. The characteristic thing about this œdema was, that it was very little affected by lying down; in the morning it was just as pronounced as in the evening; on the other hand, it completely disappeared after a quick walk of an hour and again reached its maximum after sitting still for a couple of hours. The urine contained no albumen or sugar, and investigation of the heart showed nothing striking whatsoever. The physical strength was on the whole unexceptionable and the health excellent; the sleep was good. The œdematous swellings disappeared of their own accord in the course of a couple of weeks. The reason for their occurrence may well be sought in the greatly changed conditions of circulation, especially for the lower extremities; sedentary brain-work instead of from morning to night following after a sledge in heavy clothes, an occupation which makes special demands on the muscles of the lower extremities. The greatly increased muscular work means increased flow of blood, and the circulation again is facilitated by the movements. In changing suddenly therefore from the active movement to a continuous rest, an unnatural condition arises, as the circulation requires some time to adapt itself to the new conditions.

The constant running after the sledge also makes demands, however, on the organs of the breast, the lungs and heart. I have made investigations on two strong, well-trained individuals before and after a long sledge-journey; I have no measurements of myself; I have had to be content with stethoscopic examination of the heart and found nothing unusual. As I was in the same condition as the two others, however, and as these in other regards, e. g. weight-curve, were as mentioned above, I may conclude that the changes in the breast measurement, noted for them, would also have been found in myself. In both the vegetative functions were in good order both before and after the journey. A sick condition occurred in the one case during the journey, characterized chiefly by constipation and passing tenderness of the gums. This state seems to have come as the result of bad nourishment, raw meat and lack of fat; the symptoms disappeared spontaneously with change of food.

I may give here the results of the two examinations mentioned above.

I. P. K. 37 years old. 26/3 07.

Stethoscopia pulmonum: thorax well-formed, resp. movements strong, equal; boundary of lung in front at intercostal-space VI, movable; respiration everywhere vesicular without secondary sounds. Boundary of lung on the back at intercostal-space X, with deep inspiration at XI; vesicular breathing without secondary sounds.

Stethoscopia cordis: cardiac dulness from C III and left margin of sternum to a finger's breadth from the mammillary line where the ictus was felt indistinctly in intercostal-space IV. Sounds at apex clean, strong. At base the first sound somewhat soft, indefinite. Pulse 62, strong, regular.

Circumference of chest at height of angle of scapula and papilla

with deepest inspiration	100	cm.
— deepest expiration	88·5	-
— middle position	93	-
Circumference of belly (umbilicus)	85	-

25/6 07, immediately after return. Weight increased by 0·5 kilo.

Stethoscopia pulmonum: as above.

Stethoscopia cordis: cardiac dulness from intercostal-space III and 1 cm. outside left margin of sternum to 1 cm. from left mammillary line, where the ictus is felt in intercostal-space IV. Ictus perceptible when action of heart forced. Sounds clean, strong; the 2nd sound, however, indistinctly divided in intercostal-spaces III and IV immediately to the left of the sternum. — Pulse 76, strong, regular (pulse counted after the action of the heart had been voluntarily somewhat strengthened).

Circumference of chest with deepest inspiration	102·5	cm.
— deepest expiration	93	-
— middle position	97	-
Circumference of belly	85	-

G. Th. 29 years old. 25/3 07.

Stethoscopia pulmonum: right shoulder a little hanging as the result of an old, badly healed fracture of the clavicle. Thorax for the rest well-formed. Boundary of lung in front at intercostal VI, movable with the respiration; on the back at C X, with deep inspiration at C XI. Resonant percussion-note except at right apex, where it is slightly suppressed (fracture of clavicle, more strongly developed musculature). Respiration everywhere distinct, vesicular, without secondary sounds.

Stethoscopia cordis: cardiac dulness from C III and left margin of sternum to 1 cm. inside the left mammillary line, where the ictus was felt distinctly in intercostal-space V. Sounds strong, clean; second sound somewhat hard. Pulse 62, regular, strong.

Circumference of chest with deepest inspiration	99	cm.
— deepest expiration	91	-
— middle position	92·5	-
Circumference of belly	88·5	-

2/6 07. Two days after the return. Weight decreased by 1·0 kilo.

Stethoscopia pulmonum: as above.

Stethoscopia cordis: cardiac dulness from C III and a little inside the left margin of sternum to about a finger's breadth inside the left mammillary line. Action of heart regular; 1st sound clean; 2nd sound at apex, divided along the left margin of the sternum and over the pulmonary orifice, somewhat hard and noisy. Pulse 60, strong, not quite regular in rhythm during the examination (later the pulse was regular).

Circumference of chest	with deepest inspiration	100·5	cm.
	— deepest expiration	92	-
	— middle position	96	-
Circumference of belly		85	-

It seems clear from these investigations that the capacity of the lungs has increased in both cases. The increase in the circumference of the chest is 2·5 and 1·5 cm., and this change cannot be considered as due to alteration in the walls of the thorax; neither of the men had become fat, in both previously the musculature was well-developed, and there is no reason to believe that just the muscles in question here have developed further during the journey. Again, the other measurements have also altered and not to the same extent. I consider it as certain, therefore, that the change in the measurements corresponds to an alteration in the capacity of the thorax. In one of the two men the difference between the maximum and the minimum circumference has been reduced, in the other a little increased; in both the circumference in the intermediate position has been considerably increased.

In both cases the heart after the journey is a little more covered by the lungs, most in the first case. The heart and the action of the heart have likewise been affected, most in the second case, where symptoms of heart insufficiency are apparent.

I have endeavoured in the above to outline the basis from which the November experiments should be estimated and may now pass over to a closer account of the experiments themselves.

This series also includes but 10 experiments; partly owing to the fact, that contrary to usual it was difficult to get started, as large errors occurred in the first experiments, which had therefore to be omitted, partly because I undertook a double series of experiments, in conjunction with the morning series, at intervals of 2 hours during the day, thus considerably lengthening the period of experimentation; and other tasks also claimed my attention.

On the first day of the experiments, 12/XI, the œdema had practically disappeared; the appetite was still large. On 17/XI, the first day of the complete and usable experiments, the œdema had completely disappeared. The four introductory experiments are not included in the table.

Series of experiments, November 1907

No.	Date	Barometer	Temper-ature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour 0°, 760 mm. (dry)	Vol. of an expiration at		% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	Remarks
									given temp.	37° C. (saturated)							
71	17	742	14.8	30	247.5	8.25	299	545	1210	1367	3.10	0.04	3.96	242	(69)	70 66	
72	18	747	12.2	30	232.5	7.75	250	464	1078	1232	3.56	0.05	4.70	236	..	64	
73	19	748	11.3	30	243	8.10	261	487	1074	1232	3.34	0.04	4.40	233	..	63	
75	21	748	14.1	30	247	8.23	256	472	1036	1173	3.35	0.05	4.48	225	..	57	Gas-analysis hardly trustworthy. Uncertainty with watch.
76	22	759	11.4	30	245	8.17	274	518	1118	1283	3.27	0.04	4.26	243	..	63	
78	24	758	12.0	30	277	9.23	319	601	1152	1318	3.01	0.05	3.88	256	(69.5)	56	
79	25	762.5	11.95	30	283	9.43	265	502	936	1069	3.10	0.04	4.29	221	..	65	Expiration a little constrained at the beginning of the experiment.
80	26	760.5	10.0	30	239	7.97	286	545	1197	1380	3.15	0.05	4.00	243	..	66 52	
81	27	756	11.55	30	251	8.37	286	538	1139	1304	3.15	0.06	4.07	239	..	63	
82	28	762.5	9.9	30	248	8.27	286	547	1155	1332	2.98	0.04	3.84	231	..	63	

With exception of No. 75 no irregularity of importance occurs in any of the experiments included. In the case of No. 75 the stop-watch only went for some time, and the time of ending the experiment was taken from the ordinary works of the watch. The error of some seconds thus introduced has however but very small importance. In addition to this, however, there is some uncertainty about the analysis of the expired gas, as on that day the temperature of the bath rose unusually high and was only read after the gas-sample had been led over for the first time. The temperature is therefore possibly too high, which means that the percentage of carbonic acid given is too low in this case. As the error thus introduced cannot be taken to reach 1 % of the value for the carbonic acid and as this is of quite subordinate importance for the alveolar carbonic acid tension, the experiment has been included.

The pulse is as usual variable; most of the values fall about 63.

The bodily weight was determined on the 12/XI and 1/XII, and the weights given have been interpolated from these.

The carbonic acid percentage in the inspired air is distinctly higher than during any of the previous experiments; the difference is however scarcely so great as to have any influence on the results.

The frequency of respiration has again risen; on an average it is $8{\cdot}38 \pm 0{\cdot}11$, $\mu = 0{\cdot}50$ or $4{\cdot}2$ % of the average. In the 10 experiments, however, there are only two deviations in a positive direction, experiments 78 and 79, and the average given is thus an incorrect picture of the conditions. On the other hand, I cannot find any reasonable ground for the greatly increased frequency on these days. There was nothing unusual in the temperature or height of the barometer. Defæcation occurred as usual once in the day, in the evening. I had permitted myself one irregularity, being up most of the night of the 22nd—23rd to take part in astronomical observations, but I had again slept normally on the night of the 24th. Nor can I believe that the slightly heavy expiration at the beginning of experiment 79 could work in the direction mentioned, it rather means that I was extremely sleepy during the experiment and also somewhat hungry. If we exclude these 2 experiments, of which for the rest only the last falls outside the calculated limit, the average becomes $8{\cdot}14 \pm 0{\cdot}044$, $\mu = 0{\cdot}18$ or $1{\cdot}5$ % of the average. Any dependence on the air-pressure and temperature cannot be detected.

Within the separate experiments the frequency has been unexceptionably even; I may give the following results from a couple of preliminary experiments.

Number of respirations	50	100	150	200	227	242
Time taken 12/XI	6·08 m	12·17 m	18·00 m	24·50 m		30·00 m
13/XI		13·20 m		26·42 m	30·00 m	

No count was taken in the later experiments; but, as was my custom, I have looked at the watch from time to time to be able to determine that it was going rightly.

The total volume of air respired is strikingly high and the values vary more than in any other series of experiments. The average is 522 $\pm$ 8·4, $\mu = 39{\cdot}6$ or 7·6 % of the average. One value, No. 78, falls outside the calculated limit; omitting it the average becomes 513 $\pm$ 7·0, $\mu = 31{\cdot}1$ or 6·1 % of the average. The total volume is thus as great as in August, though the other respiratory functions investigated have altered in such a way, that we might have expected a decrease. So far as I can see, this can be explained by the probability that the total capacity of the lungs has increased as a result of the sledge-journey. I have naturally only the external measurements; but as already mentioned, there is no reason to believe that these do not correspond to changes in the cubic capacity of the lungs, all the less since the changes described agree in all respects with what we should expect after BOHR's[1] and HASSELBALCH's[2] investigations. The changes found by these authors have certainly been fleeting; but DURIG[3] has shown, that after a tiring journey of 19 hours these altered conditions of lung capacity may remain for several days, and there is therefore scarcely anything improbable in concluding that in my case they have persisted for a month. In our journey of 6 weeks we passed over ca. 900 kilometers, of which about 800 km. in walking and running behind the sledge, usually at the same time with heavy work in clearing the heavily laden sledges, when they turned over or stuck fast. The measurements given above were made before these respiration experiments; but as I did not know of the papers by BOHR and HASSELBALCH, which were published during my absence on the Expedition, it did not occur to me to repeat the measurements, and I do not know, therefore, how long the changes persisted.

The carbonic acid percentage in the alveolar air is on an average 4·19 $\pm$ 0·057, $\mu = 0{\cdot}269$ or 6·4 % of the average. In terms of mercury pressure the average is 29·6 $\pm$ 0·38, $\mu = 1{\cdot}80$ or

[1] Deutsch. Archiv f. klin. Medizin. Bd. 88, p. 385.
[2] Festskrift fra Finseninstitutet 1908.
[3] Zentralblatt f. Physiologie 17, 1903. cit. BOHR l. c. p. 430.

$6 \cdot 1\,^0/_0$ of the average. The distribution of the deviations is very irregular; there are no specially great deviations, nor do any fall outside the calculated limit; but the values for the carbonic acid tension are so distributed that there are no variants between 28·9 and 30·4; on both sides of this interval there are 5 cases. What such a separation can arise from, is obscure; neither the temperature nor the air-pressure seem to exercise any apparent influence.

Lastly, the irregularity is again found in the results for the amount of carbonic acid expired. The average here is $237 \pm 2 \cdot 0$, $\mu = 9 \cdot 5$ or $4 \cdot 0\,^0/_0$ of the average; but the distribution of the deviations is more regular. The calculated, upper limit for the series is 255·7, thus the same as the value for the experiment No. 78. Compared with the other results for this experiment, it would seem that the respiration was forced on this day; I have noted nothing regarding the experiment, however, which could support this view. Nor can any disturbing influence be detected on the amount of carbonic acid expired from the side of the meteorological factors, temperature and air-pressure. Here as for the other elements the irregularities must be referred back to the fact, that the organism has been moved from its ordinary, regular physiological equlibrium by the considerable attacks made upon it as described above.

A more frequent determination of the bodily weight would certainly have been useful; but even if we assume that the weight-curve would have been less regular than that given, such could not be imagined as having any marked levelling effect on the greatly fluctuating values for the amount of expired carbonic acid.

As I was obliged to give up a projected series of experiments in October, owing to the sledge-journey, I was also compelled to alter the last part of the programme. I decided therefore, to take the projected experiments for December and February as one series in the end of January. This gave me no experiments during the winter solstice, but I obtained a series both at the beginning of the dark period and towards its end, and the distance from the "summer experiments" was very nearly the same at both sides. We should expect, that any peculiarities due to the dark period would appear distinctly in January, even though the maximum could only be expected to occur a month later. The sun reappeared, as mentioned, in the middle of February; but indoors, it still continued to be the "dark period" for some weeks afterwards.

The January series includes 12 experiments; one experiment (104) had to be given up, as it proved impossible to keep the breathing-tube open owing to a snow-storm. During the whole period, with excep-

Series of experiments, January 1908

No.	Date	Barometer	Temper-ature (C.)	Duration of experiment	Number of respiration	Frequency	Litres expired	Litres per hour 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	Remarks
96	17	754	12.9	30	287	9.57	242	452	843	960	3.24	..	4.70	211	68.5	65	
97	18	748.5	13.0	30	306	10.20	252	467	824	938	3.09	0.04	4.53	208	..	64	
98	20	746	11.3	30	318	10.60	268	499	843	968	3.04	..	4.39	220	(68)	65	Uncertainty in the analysis of the expired air.
99	21	747	14.7	30	324	10.80	271	497	837	946	2.94	..	4.30	212	..	67 55	66 S. after dressing.
100	22	745.5	16.2	30	330.5	11.02	259	471	785	881	2.99	0.04	4.52	204	..	61	
101	23	733.5	16.0	30	319	10.63	261	467	818	920	3.03	..	4.49	207	(67.5)	58	
102	24	737	14.65	30	415	13.83	278	503	670	758	2.77	..	4.57	204	67.5	60	
103	25	742	15.0	30	342	11.40	257	468	752	849	3.12	..	4.81	213	..	58	Uncertainty in the analysis of the expired air.
105	27	754	12.5	30	302	10.07	237	443	785	893	3.17	..	4.77	205	..	60	Do.
106	28	760	16.5	30	313	10.43	248	459	793	888	3.03	..	4.56	203	..	63	
107	29	770	13.5	30	280	9.33	248	472	886	1005	3.07	0.04	4.37	211	..	58	
108	30	759.5	14.5	30	298	9.93	252	471	846	956	2.97	..	4.31	204	..	60	

tion of the last three days, the weather was uncertain, stormy, with snow and relatively high air-temperature. The phenomena of the "dark period" became very apparent; the general feeling was one of instability, irritability and depression; the sleep at nights was uneasy; I was often sleepy and indisposed during the experiments. The gas-analyses especially were difficult, as the air-pressure constantly varied and the temperature of the water-bath fluctuated very greatly. In three analyses (experiments No. 98, 103, 105) there is some uncertainty in the results, but as errors of this kind, as previously mentioned, cannot be considered to have any influence worth mentioning on the results, they are included. The analysis of the inspired air on 29/I can only be regarded as approximate.

I have twice noted, that I have felt vortices in the breathing-tube during the experiment, owing to the storm; they did not trouble me, and I do not think that they can have influenced the results of the experiments. On 23/I (experiment 101) I was extremely sleepy and found it difficult to count; I consider the number given as correct, in spite of the fact, that the count in the morning in the berth gave a somewhat higher frequency.

The bodily weight decreased a little in the first half of the period, but remained constant during the latter half; weighed on 2/II.

The pulse was counted in the berth shortly after waking up; the series is very low, but exact information regarding the pulse is lacking.

The respiration-frequency in this series shows a slight peculiarity, as it several times proved to be either a little higher or a little lower than the average during the first minutes; this was a result of my whole "nervous" condition. In experiment No. 96 the frequency was about 10 in the first three minutes; introducing this correction, the frequency in the remaining 27 minutes would be 9·52. As no greater irregularities than this occur, so far as known to me, I have not thought it necessary to make any correction in the values given.

The frequency in this series shows a distinct tendency to vary in an opposite direction to the air-pressure. The highest value falls on the 24/I, the day after the lowest barometric condition read; it should be noted here, however, that the barometer on the 23/I was steadily falling and only rose a little on the following night. The strikingly low frequency on the 23/I, when regard is taken to the amount of the single respirations, which for the rest form a very regular series, might give rise to the suspicion that I had counted wrongly, for example, that I had forgotten to move the balls back and had thus counted 20 too few. Even though I was admittedly sleepy and indisposed, however, there is nothing to support this suspicion. The 12 experiments give an average of 10·65 $\pm$ 0·22, μ = 1·11 or 10·4 % of the average, thus a considerable variability, corresponding to the great fluctuations in the air-pressure. The very great standard deviation is however due mainly to the high value in experiment No. 102, in which the deviation is over $\frac{5\mu}{2}$, and falls outside the calculated upper limit, here 2·038 μ. Omitting this, the average becomes 10·36 $\pm$ 0·12, μ = 0·59 or 5·7 % of the average. In this case the deviations, which are still considerable, are distributed somewhat evenly, with 6 in a positive and 5 in a negative direction.

The total volume of respired air is in this series greatly reduced, but for the rest shows no special deviations. The average is 472 $\pm$ 3·5, μ = 17·8 or 3·8 % of the average.

The average for the alveolar carbonic acid tension is 4·55 $\pm$ 0·29, μ = 0·149 or 3·3 % of the average. In mercury pressure the average is 31·8 $\pm$ 0·22, μ = 1·12 or 3·5 % of the average.

This last series shows the following distribution:

No.	CO_2 in mm. of mercury	Deviations from the average	Barom.	Remarks
96	33.3	+ 1.5	754	
97	31.8	± 0.0	748.5	
98	30.8	— 1.0	746	
99	30.1	— 1.7	747	
100	31.6	— 0.2	745.5	
101	30.8	— 1.0	733.5	Falling barometer until a short time before experiment.
102	31.5	— 0.3	737	
103	33.4	+ 1.6	742	
105	33.7	+ 1.9	754	
106	32.5	+ 0.7	760	
107	31.6	— 0.2	770	
108	30.8	— 1.0	759.5	

There seems here a certain connection between air-pressure and the alveolar carbonic acid tension, low air-pressures giving low values for the alveolar carbonic acid tension and conversely, or perhaps it would be better to say, that falling air-pressure gives decreasing carbonic acid tension, rising air-pressure increasing tension. For the barometer > 750 we get an average of 32·4, for < 750, 31·4. The same phenomenon appears, though less distinctly, when we take the percentage of carbonic acid.

The amount of carbonic acid expired is on an average 208·5 ± 0·9; μ = 4·9 or 2·3 % of the average. The calculated limits of the series are ± 2·038 μ, and thus the experiment No. 98 gives too high a value, probably due to the uncertainty in the air analyses. This experiment omitted the average will be: 207·5 ± 0·7, μ = 3·6 = 1·7 % of average.

In addition to the experiments discussed in the foregoing, I may also mention here 3 morning experiments from May 1908. At this period I again made a double series of experiments at different times of the day, for comparison with the series from November—December 1907. I thus made morning experiments on 3 days, and these are of interest in several ways, partly because they fall in the interval between two of the foregoing series, partly because they were taken during relatively low, outer temperatures, which have a very marked effect in all three cases, perhaps for the reason that the experiments were not made on 3 successive days. As the separate values are almost identical in the three experiments, I believe that a certain importance may be attached to them in spite of the small number.

Morning experiments, May 1908

No.	Date	Barometer	Temperature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour, 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo & per hour	Weight in kilos.	Pulse	Tp. Rectum	Tp. Mouth	Remarks
115	21	759.5	7.7	30	222	7.40	214	412	963	1121	3.36	0.03	4.58	202	(68)	57	36.6	35.85	
119	23	754.5	7.1	30	224.5	7.48	221	423	983	1148	3.36	. .	4.54	205	(69)	66	36.55	36.0	
123	26	761.5	8.1	30	219	7.30	212	408	968	1125	3.46	. .	4.71	202	69.5	53	36.35	35.82	

All three experiments have proceeded normally. The pulse and temperature were taken in a recumbent position before dressing. The bodily weight was determined as 68 kilos on 25/V.

Analysis of the inspired air was also made on the 25/V and 27/V as well as the 21/V, both times with the same result as above given.

The respiration-frequency is influenced by the low temperature and approximates to the values for June. The average is 7·39. The frequency in the single experiments is regular; an afternoon experiment on the 26/V gave the following values:

10 min.	20 min.	30 min.
85	169	254

The total volume respired is comparatively low, the average is 414, which again is in connection with the high alveolar carbonic acid tension. We also see again the effect of the low outer temperature as in the April series; only it is even more strongly marked at this more advanced time of year. The percentage of carbonic acid in the alveolar air is 4·61, the tension in mm. of mercury 32·8.

The amount of carbonic acid given off is small, in good agreement with what has been said above; the average is 203.

The mutual differences between the 3 experiments are so small that we may well pay no attention to them.

RESULTS OF THE EXPERIMENTS.

In the foregoing I have discussed the elements of the separate experimental series in the order in which, for practical reasons, they occur in the tables; in now giving an account of the results of the experiments it will be more suited to the purpose if I choose a different order. And since there is general agreement, that the function of the respiratory centre is regulated by the carbonic acid tension of the blood, it seems most natural to begin this section with a discussion of the alveolar tension and the light thrown upon it by the experiments; I assume here provisionally with HALDANE that the variations in the alveolar carbonic acid tension may be considered as an "index" of the changes in the tension occurring in the centre.

The alveolar carbonic acid tension.

To facilitate the summary the results from the single experimental series are brought together here, only the equivalents in mm. of mercury being given however, not the percentage of carbonic acid, concerning which reference may be made to the tables.

To be able to compare my results with the values given in the English papers, which were obtained by direct determination of the alveolar air by HALDANE'S method, it has to be remembered that these last will always be somewhat higher, ca. 3 mm., than the corresponding results in my series. In February 1909 my alveolar carbonic acid tension was in Copenhagen:

calculated from BOHR'S formula	34·2 mm.
determined by HALDANE'S method	37·3 —

The last is the average of a series of mean values, obtained from twin-determinations according to the method already described.

Before going on to a close analysis of the experimental results, I may briefly mention some of the more recent literature, which is of interest in this connection.

February 1909			April 1907			June 1907			August 1907			November 1907			January 1908			May 1908			April 1907 group II		
CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.	CO_2 mm. Hg	Bar.	Temp.
33.7	766	12.6	30.7	774	10.5	28.7	753	8.55	28.5	761	10.75	27.4	742	14.8	33.3	754	12.9	32.7	759.5	7.7	30.7	774	10.5
33.3	764	11.5	32.4	771.5	7.5	28.9	758	11.55	25.2	756	12.0	32.9	747	12.2	31.8	748.5	13.0	32.1	754.5	7.1	29.4	752	11.9
33.4	754	11.3	31.7	770.5	8.25	27.4	762.5	10.4	25.6	758	11.05	30.9	748	11.3	30.8	746	11.3	33.7	761.5	8.1	31.2	752	12.0
34.1	762	9.5	32.0	769	6.6	29.5	760	12.7	26.9	750	10.3	31.4	748	14.1	30.1	747	14.7	32.8	758.5	7.6	29.6	750	11.6
35.2	767	10.4	29.5	756	1.45	29.9	764	11.6	27.1	754	9.9	30.4	759	11.4	31.6	745.5	16.2				29.4	760	9.3
34.5	765	11.5	29.4	752	11.9	29.5	764	11.6	27.5	754.5	10.9	27.6	758	12.0	30.8	733.5	16.0				31.3	766	12.5
35.0	763	12.5	31.2	752	12.0	32.0	766	10.9	26.2	758.5	10.4	30.7	762.5	11.95	31.5	737	14.65	April 1907 group I			32.7	770	9.95
34.2	744	12.85	29.6	750	11.6	28.5	761	11.65	29.6	761	8.5	28.6	760.5	10.0	33.4	742	15.0	32.4	771.5	7.5	..	..	..
34.5	747.5	14.0	29.4	760	9.3	30.1	760.5	11.05	..	..	..	28.9	756	11.55	33.7	754	12.5	31.7	770.5	8.25	..	..	..
34.6	759	13.2	31.3	766	12.5	28.4	764	10.65	..	..	..	27.5	762.5	9.9	32.5	760	16.5	32.0	769	6.6	..	..	..
33.9	762	12.9	32.7	770	9.95	31.2	766	10.9	..	..	..	..	..	..	31.6	770	13.5	29.5	756	1.45	..	..	..
..	..	..	30.9	758	7.5	28.7	764	12.7	..	..	..	..	..	..	30.8	759.5	14.5	30.9	758	7.5	..	..	..
34.2	759	12.0	30.9	762	9.1	29.4	762	11.2	27.1	757	10.5	29.6	754	11.9	31.8	750	14.2	31.3	765	6.3	30.6	760.5	11.1

In quite recent years an extensive and interesting work has been carried out by English physiologists, especially Haldane and his collaborators, in order to show the alveolar carbonic acid tension's decisive importance as the regulator of the ventilation of the lungs.

To be able to carry out a large number of analyses under all possible conditions, without being bound to the laboratory, Haldane and Priestley[1] have indicated and developed a rapid and apparently convenient method of directly determining the alveolar air. In a later work by Haldane and Fitzgerald[2] the mode of procedure is still further simplified. My objections to this method, as already shown, are, in the first place, that it is doubtful whether we obtain normal alveolar air by this method, and secondly, that even in the hands of an experienced investigator considerable inaccuracies may be introduced, as can readily be shown, and lastly, that there will always be greater uncertainty in the single determinations on this method than by calculation according to Bohr's formula of an average sample. In spite of these objections, however, I quite admit that with some experience and with a sufficiently large number of analyses, we might be able to demonstrate the variations in the composition of the alveolar air.

After their investigations on their own alveolar air, Haldane and Priestley began with ascertaining, that the alveolar carbonic acid tension may well vary individually but is nevertheless a constant magnitude for each individual, independent of practically all physiological encroachments, with the exception of excessive changes in the air-pressure. The total volume of air respired is regulated by the action of the carbonic acid on the respiratory centre, in such a way, that the carbonic acid tension, when its state of equilibrium is altered, tends to return again to its individual standard value.

In the above-mentioned work of Haldane and Fitzgerald it is shown that the alveolar carbonic acid tension lies at a somewhat different level or in different zones, according to age and sex.

For men the following values are given:

Maximum	44·5 mm.
Minimum	32·6 —
Average	39·2 —

From later investigations Haldane has seen, however, that the constancy is not absolute, that the carbonic acid tension like other

[1] The Journal of Physiology. Vol. XXXII, 1905.
[2] ibid.

physiological "constants" varies according to definite laws under the influence of physiological and climatological factors.

Haldane and Boycott[1] find that the alveolar carbonic acid tension falls and rises with transition from cold to warmth and conversely. They believe, in opposition to Scott[2], that there is no connection between this phenomenon and the fluctuations in the rectal temperature, but that it is due to a reflex action on the centre from the skin of the face and hands. In this connection they speak of the possibility of an annual periodicity with the lowest values in summer; the observations are however but few and scattered. The authors do not believe, that the fall in the tension begins at a definite temperature, but rather, that it occurs when the warmth or cold is felt as somewhat unpleasant.

It is already mentioned in Haldane and Priestley's work, that very great changes in the air-pressure are the cause of variations in the composition of the alveolar air, whereas this is not affected by ordinary, so-called normal fluctuations of the barometer. Haldane and Boycott have now investigated this condition more closely in a pneumatic room and have come to the result, that the carbonic acid tension remains constant until a total pressure of ca. 550 mm. during experiments of short duration. When the pressure further decreases, the carbonic acid tension also falls. If a sufficient amount of pure oxygen is introduced the pressure can go down to 307 mm. without the carbonic acid tension changing. In experiments of longer duration, on the other hand, the carbonic acid tension falls a comparatively large amount and the effect lasts longer; after 20 hours the tension had fallen 7 mm. against 1—1·5 mm. in an experiment of short duration, and the effect only disappeared after some days.

Ward took part in these experiments and undertook a journey later to Monte Rosa, in order to test, if the results obtained experimentally would hold good.

Ward came to the following results[3]:

	Approx. height in feet	Bar.	CO_2 tension	
London before the journey		769	37·7	(20 exp., range 40·5—35·8).
Zermatt, 25/7 07	5315	633	34·2	stay of a week.
Cap. Regina Margherita	14965	443·5	28·5	stay of a week.
Zermatt		633	28·7—32·5	stay of a week.
Oxford, November 07		752	37·7	

[1] The Journal of Physiology. Vol. XXXVII, 1908.

[2] ibid.

[3] ibid.

It is worth noting that the changes in the carbonic acid tension on the ascent occurred so to speak on the way; a couple of hours afterwards, the organism already showed itself adapted to the new conditions. On the descent, on the other hand, it took a week for the carbonic acid tension to return to its former level, and the rise was irregular. It is further worth noticing, that the fall in the tension on ascending the mountain is out of all proportion much greater than in the experiments under corresponding changes of pressure at the Lister Institute in London.

	London	Zermatt	London	Monte Rosa
Barom.	628	633	494	443·5
CO_2-tension	37·1	34·2	35·9	28·5

When we remember, that the great change on ascending to the high level happens almost as quickly as the small change in the pneumatic steel-room, and that it remains much longer, one gets the impression that other and stronger forces than the reduced air-pressure have played some part in these results.

The effect of reduced oxygen tension was also studied by Haldane and Poulton[1], who found that hyperpnoe occurs on a rapid fall of the proportion of oxygen. This hyperpnoe is due, however, not directly to the lack of oxygen but to the already present carbonic acid, the "threshold value" of which is reduced, owing to the lactic acid or other substances produced as the result of the lack of oxygen. Removing the preformed carbonic acid by forced respiration, we can even produce apnoe in spite of distinct lack of oxygen. Thus, the oxygen or lack of oxygen cannot have a direct effect on the respiratory centre, and just as little the abnormal products of metabolism, which owing to the lack of oxygen appear in the blood and tissues, especially lactic acid. They affect the centre indirectly by reducing the threshold value of the carbonic acid tension. The formation of these abnormal products of metabolism is a very gradual process; if the fall in the oxygen tension proceeds with a suitable slowness, so that the preformed CO_2 obtains time to be eliminated, no hyperpnoe occurs; a fall of still longer duration will, on the other hand, give the abnormal metabolic products time to accumulate, so that the centre is stimulated and the threatening symptoms of lack of oxygen are averted.

The paramount importance of the carbonic acid tension for the respiratory centre's activity is also shown by the investigations of Pembrey and Allen[2] on the respiration of Cheyne-Stokes type.

[1] The Journal of Physiology. Vol. XXXVII, 1908.

[2] Proceedings of the Physiological Society, 21. Jan. 05. Journal of Physiology. Vol. XXXII.

They found *inter alia*, that a quiet, regular respiration could be produced by varying the composition of the inspired air.

In sharp contrast to the standpoint taken up by the authors mentioned, we have a series of investigations by ZUNTZ, LOEWY and others[1].

The German investigators maintain, that whilst the oxygen tension in the alveolar air regularly falls with the air-pressure, this is not the case with the carbonic acid tension, which *inter alia* is likewise dependent on the total volume of air respired. When the latter is increased, owing to the lack of oxygen, the alveolar carbonic acid is reduced secondarily; similarly, the carbonic acid tension is reduced in the blood when lactic acid is formed. For these two reasons we should as a rule expect reduced carbonic acid tension at high elevations. If the fall in the carbonic acid were primary, we should expect a reduced total volume of air respired; since the opposite is the case, it must be because the sum of the excitements ("Reize"), which are characteristic for the mountain climate, has been increased; we cannot imagine, namely, that the centre alters its excitability. As a rule it is certainly the carbonic acid tension which determines the volume of air respired; but it might well be imagined, that vicarious excitements arose under the special climatic conditions. The authors would not have found it necessary to refer to the alveolar carbonic acid tension had it not been that Mosso had put forward his *akapnia* as the cause of mountain-sickness[2]. It may well be admitted now, that these two phenomena have nothing whatsoever to do with one another; since it was just in the two members of the German Expedition who suffered from mountain sickness, that no fall occurred in the carbonic acid tension, though the latter was found very distinctly in all the other members.

For comparison with the above-cited values of WARD I may give here two series from the results of the German Expedition.

		Barom.	CO_2 tension
N. ZUNTZ (1895)	Berlin	758	33·8
	Zermatt	625	30·9
	Bétemps hut	533	25·1
N. ZUNTZ (1901)	Berlin	758	36·1
	Brienz	715	38·5
	Rothorn	585	32·6
	Monte Rosa	435	27·2

[1] ZUNTZ, LOEWY, MÜLLER, CASPARI: Höhenklima und Bergwanderungen in ihrer Wirkung auf den Menschen. Berlin 1906.

[2] MOSSO: Der Mensch auf den Hochalpen. Leipzig 1899.

As will be seen, these results agree well with the English.

In this country Hasselbalch has briefly discussed the alveolar carbonic acid tension in his work on the chemical light-bath[1]. He puts forward the hypothesis, that the chemical light-bath may increase the percentage of carbonic acid in the alveolar air, and the results given are apparently in favour of this view; but only apparently. If we study his Table II closely, it is seen that the series is irregular, but not that it shows an increase in the percentage of carbonic acid after the light-baths. The average of the series is 4·47. The first 9 values, thus about the first third of the series, all have negative deviations from the average; on the 10th day, in the morning before a light-bath, the carbonic acid percentage suddenly rises simultaneously with a rise of 2 degrees in the temperature of the room. In the following period, almost the second third of the series, the distribution seems more according to chance, the fluctuations being small and distributed on both sides of the average (calculating with 2 decimals instead of with one would certainly give a more regular series); but after 14′ light-bath the value suddenly rises and we now have a series of large, positive deviations; at the same time the temperature in the room again rises. When we consider that no rise can be seen after the first light-baths, and just as little after the light-baths in Table III, we may well conclude that special conditions have been operative after the 14′ light-bath[2]. What these conditions may be does not appear from the table. The connection indicated with the air-temperature cannot be the determinating influence; both in Haldane's and my own investigations temperature variations such as those in question here produce quite the opposite movement in the carbonic acid contents of the alveolar air. But it is not excluded that changes in the air-pressure may occur at the same time, which would explain the phenomenon. A second factor, which must be taken into consideration after Haldane's investigations, is the blood-pressure; but there is no information about this point in these last experiments. A remark to experiment 45, noting a painful reaction on the face, leads one to think of the last-named possibility. Lastly, it must be remembered, that it is the percentage of carbonic acid, not the tension, which is given; the recalculation of the series in terms of mercury pressure would certainly make some change.

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[1] Hospitalstidende Nos. 45, 46, 47, 1905.

[2] According to conversation with Hasselbalch, two longer experimental series on a different individual have not been successful in showing any support for the view, that the alveolar carbonic tension rises after the light-bath. It seems quite uninfluenced by the latter.

The irregular distribution of the deviations in relation to μ is due partly to the fact, that there are some few large deviations in a relatively uniform series, partly to the fact that only one decimal has been taken account of in the calculations; a series of numbers, however, which just vary as much as μ, will undoubtedly prove to fall partly outside, partly within this limit on making closer calculations.

There is still one thing more in this paper to which I would refer. Hasselbalch has judged the increase in the amount of the "dead space", produced by the mask and the valvular apparatus, to be 50 cc., which is too low a value in my opinion. I have calculated with 150 cc., with the same valves and on the whole the same arrangement, after measuring the space with water. Certainly, there may be some difference in the "dead space" in the mask and this is just difficult to measure; but the difference can never be so great as 100 cc. The "dead space" enters into the calculations of the percentage of the alveolar carbonic acid; but as these two quantities are not simply proportional but vary together according to a more complicated function, the effect of any change in the amount of the "dead space" on the percentage of carbonic acid cannot be overlooked.

Turning to my own investigations, these give the following results with regard to the climatic factors, air-pressure and temperature.

In contrast to Haldane and Priestley's results, that the ordinary fluctuations of the barometer are without influence on the alveolar carbonic acid tension, I find both from control experiments as well as from the Greenland series a distinct tendency on the part of the two quantities mentioned to vary in the same direction. To express this tendency in numbers is difficult from the available material, as the experiments were not arranged with that object in view and are thus not "clean" in this regard; thus, the influence of the air-pressure interferes with the effect of the air-temperature and both produce independently or in conjunction comparatively superficial irregularities on the great annual variation; if we consider the separate series a definite tendency will, as said before, be unmistakable.

It is natural to assume beforehand, that there is no difference in principle in the effect of small and large fluctuations of the barometer; and the movement here also goes in the same direction as that found in mountain experiments. The reason why Haldane and Priestley have not taken this condition into consideration is, partly, that their values for the alveolar carbonic acid tension vary for other reasons much more than mine, and partly, that the fluctuations of the barometer in my experimental series are considerably greater than are ordinarily met with under our conditions.

In the experiments of February 1909 the carbonic acid tension in the first half of the series follows very exactly the rise and fall of the barometer; this part of the series also shows greater regularity in other regards than the remaining part; but the same feature is also recognizable in the latter in spite of a couple of inversions, and if we split up the series by grouping together the determinations at a barometric height of 762[1] or less into one group, the remainder into another, we obtain an average for the first group of 34·1 and for the second 34·3, a difference which is certainly not considerable but is nevertheless when taken in conjunction with the remaining series worthy of note. The temperature in the room was comparatively uniform during the whole period of experimentation.

The series of experiments for April 07 is different. In these experiments we see an interplay of opposing forces, which make the results difficult of interpretation; but as shown by the distribution of the deviations indicated on p. 105, the variations here also move on the whole parallel with the fluctuations of the barometer; and if we divide up the series, so that the experiments where the barometer is $>$ 760 mm. are taken in one group, we obtain a very sharp separation, which even considering the temperature conditions can scarcely be explained without assuming some influence of the air-pressure. For the barometer $\leqq$ 760 the average is 30·0, when the barometer is $>$ 760 the average is 31·8.

In the series for June, on the other hand, there is no parallelism in the variations from day to day; dividing up the series, for a barometric pressure $>$ 764 the average is 29·65, when the barometer is $<$ 764 the average is 28·65. It is to be remarked here, however, that as mentioned earlier some large deviations occur, which on dividing up the series tend to place a disproportionate weight on the side on which they fall.

The August experiments, excluding the last experiment in which there is a decided effect of the cold, show no tendency to group themselves according to the air-pressure. And this holds good likewise for the November experiments, where the daily variations sometimes go in the one direction, sometimes in the other. A grouping of the experiments here, however, gives a slightly lower value for the carbonic acid tension in the portion where the air-pressure is highest. The difference is however quite inconsiderable.

In the winter experiments of January 1908 the carbonic acid

[1] The value chosen so as to have the 2 groups somewhat of the same size.

tension again follows the fluctuations of the barometer; the exceptions are but few. The variations in the air-pressure are very considerable in this series; but the temperature is high and uniform. Dividing the series according to the average height of the barometer we obtain an average of 31·4 when the barometer is < 750, and 32·4 when the barometer is > 750, thus a decided difference, even though not so well-marked as in the April experiments.

It may be added, that the three experiments for May show the same features.

We can thus show a connection between the alveolar carbonic acid tension and the air-pressure, both in the Danish and in the Greenland experiments, most marked in the last, yet only for the winter and spring experiments. This connection is undoubtedly the rule. That it is specially distinct in the arctic winter harmonizes well with the direct observation of its influence on the state of health. These variations, never very considerable, are obscured in the arctic summer; it will be shown below, that the latter also exerts a levelling influence on the phenomena of respiration in other ways, and that this influence, just as is the case here, persists until the beginning of winter.

The influence of changes in the air-temperature can still less be calculated, partly because such an influence does not appear at a definite temperature limit, partly because, as mentioned, there never was the same temperature at the roof and on the floor of the cabin; the difference in temperature might even be very large. I can therefore indicate the temperature of the gas-meter but not that of the layer of air round about me during the experiment; regarding the latter I only know that it was always lower than the gas-meter's and as a rule varied in the same direction as this. Nor can I give the temperature of the inspired air; it passed through a metal tube and was thus to a certain degree heated up before it was inspired; but now and then I noticed that it was cold.

The mean temperature of the place does not seem to have any influence. If so, it should give higher carbonic acid tension in winter than in summer in my Greenland experiments; but my control experiments in Copenhagen, which give a still higher carbonic acid tension than the Greenland winter experiments, quite throw cold water on such a view. It is obviously the temperature in one's immediate surroundings during the experiment, which is of importance. And in this regard I must acknowledge the correctness of HALDANE's opinion, that it is not the temperature read on the thermometer which is of importance, but on the contrary the purely subjective feeling of discomfort under an unaccustomed temperature.

When the organism has adjusted itself to a certain temperature, and such an adjustment, at any rate in arctic regions, is very readily felt, fluctuations from this when they reach a certain amount will be felt as more or less unpleasant. A cold day is felt all the more and as more disagreeable the further we get on into the spring, and conversely, in winter, a day or a period with relative high temperatures will have a great effect on the state of health. How far the effect of such a change in the temperature extends, will depend naturally, not only on the intensity of the change, but also on individual idiosyncrasies.

It will be seen from the tables, that there is a change in the carbonic acid tension in the direction indicated by Haldane, whenever the experiment was made at a lower temperature than usual. The condition is most distinct in the April experiments. We find here, experiment No. 31, simultaneously with a slight fall of the barometer a considerable fall in the temperature and a sharp rise in the tension of the alveolar carbonic acid. As already mentioned, this rise is straightened out in the course of the following days, irregularly owing to the simultaneous, considerable fall of the barometer, but undoubtedly in the first place owing to an adjustment to the low temperature. That this last factor is the most important will appear from the values for the total volume of air respired, about which more will be said below. The last experiment of the series shows a well-marked fall in the air-pressure simultaneously with the temperature fall, whilst on the previous day there was a high carbonic acid tension with the high barometer; from this comes the relatively low value for the carbonic acid tension in the last experiment in spite of the fall of temperature. A distinct sign of the influence of the cold is also shown in the last experiment for August, experiment No. 66, as also in the three morning experiments for May. The influence is here very distinct owing to the fact, that the experiments do not fall on three successive days; there is no "adjustment", and each of these experiments thus comes to correspond to the first "cold day" in April.

Haldane believes that the effect produced on the carbonic acid tension is due to a reflex action on the centre from the skin of the face and hands[1].

This view falls in line with my experiments. Whilst the respiratory centre normally reacts to an increased carbonic acid tension with increased volume of air respired, the opposite is the case on my "cold days", and this can only mean that the irritability of the

[1] The Journal of Physiology. Vol. XXXVII, 1908, p. 360.

organ is reduced. Zuntz and Loewy's contention[1], that the increased total volume of air respired at high elevations is due to certain "excitations" peculiar to the mountain climate, since we cannot assume that the centre changes in sensibility, is therefore scarcely maintainable. It may be maintained where there is a question of reduced carbonic acid tension in conjunction with increased total volume of air respired; but when, as on my "cold days", we have increased carbonic acid tension and reduced total volume of air respired, this explanation is of no use to us.

As mentioned, the changes in the alveolar carbonic acid tension due to fluctuations of the barometer do not seem to influence the total volume of air respired. In other words, it appears as if variations in the carbonic acid tension as the result of changes in the total pressure, are without influence on the respiratory centre. My material is admittedly not suited for wide-reaching conclusions in this direction, at the most I can only give suggestions; but it is very fortunate, that the investigations of the English physiologists also support these views.

As mentioned on p. 129 Haldane and Boycott have found, that the carbonic acid tension remains constant until a pressure of 550 mm. in experiments of short duration, as also that, if sufficient oxygen is present, the pressure can be still further reduced without reducing the carbonic acid tension. There is indeed nothing remarkable here; the organism must have time to accomodate itself to the altered conditions. And the accomodation shows itself as soon as the experiment is prolonged. Haldane and Poulton have therefore come to the result that, given a suitably slow change in the pressure, the carbonic acid tension may be very greatly reduced without the total volume of air respired being changed. It is only with a still slower fall that there is time for the accumulation of abnormal metabolic products, which by reducing the threshold value of the carbonic acid exercise some influence on the centre; but it seems to me undoubted from the foregoing, that the carbonic acid tension may be reduced very considerably without affecting the respiratory centre, when the fall in the tension is a result of reduced total pressure. It is quite possible, therefore, that the low alveolar carbonic acid tension at high altitudes may, in part at any rate, be due to the low air-pressure; but the accompanying, increased total volume of air respired can certainly not be due to this. It might however be a result of want of oxygen, by which the threshold value of the carbonic acid tension is altered. But is there any lack of oxygen

[1] Höhenklima und Bergwanderungen Kap. XI.

on Monte Rosa? ZUNTZ and LOEWY give an oxygen tension of 53·6 mm. for the Alpine guide Bianchetti and add, that he was thus not more favourably placed than any of the others. But LOEWY has himself shown[1], that an oxygen tension of 40—45 mm. would be sufficient. This does not point to the lack of oxygen as the cause of the hyperpnoë.

GALEOTTI[2] has now shown, that there is a reduction in the alkalinity of the blood by 36—47 % on Monte Rosa, and this may explain the hyperpnoë. But is the reduced oxygen tension the cause of the reduced alkalinity of the blood? It may be so under certain circumstances; but on Monte Rosa it seems to me from what has been shown above that there is some doubt about the matter.

Whilst the variations in the temperature and air-pressure in my experiments have on the whole only slight influence on the alveolar carbonic acid tension, the latter for other reasons is subject to considerable fluctuations in the course of the year.

Omitting all the experiments with undoubted increase in the carbonic acid tension owing to the effect of cold, we obtain the following series:

		CO_2 in mm. of Hg	
Copenhagen,	February 09	$34·2 \pm 0·12,\ \mu = 0·59$	
"Danmark's Harbour", (76° 46′ N.)	April 07	$30·6 \pm 0·29,\ \mu = 1·14$	(group II)
— —	June 07	$29·4 \pm 0·24,\ \bar{\mu} = 1·22$	
— —	August 07	$26·7 \pm 0·27,\ \mu = 1·05$	(exp. 66 omitted)
— —	November 07	$29·6 \pm 0·38,\ \mu = 1·80$	
— —	January 08	$31·8 \pm 0·22,\ \mu = 1·12$	

There are two things we notice here: the annual period with a maximum in January, minimum in August, and the obvious fact, that the carbonic acid tension in the Greenland experiments, even at its maximum value, is lower than in the Copenhagen experiments.

If we undertook a spring and summer voyage from Copenhagen to North-East-Greenland, we should undoubtedly obtain a series of values for the alveolar carbonic acid tension, which would correspond to the first four values in the above series; we are therefore entitled to use the control experiments as the starting-point, in order in the end to seek for an explanation, why the January experiments show lower values for the carbonic acid tension than these.

As already mentioned, it is very improbable that the annual period stands in any connection with the temperature conditions,

[1] Pflügers Archiv. Bd. 58, p. 409, 1894.
[2] cit. Höhenkl. u. Bergw. Kap. X.

and the air-pressure can just as little play any part in the movement as a whole. For the first four averages the temperature is respectively 12·0°, 11·1°, 11·2° and 10·5° and the corresponding barometric heights 759, 761, 762 and 757 mm. And during this period represented by the months named the carbonic acid varies by 7·5 mm., i. e. a reduction of 21·9 % reckoned from the value for February.

The explanation must therefore be sought for elsewhere and it is found — I believe — when we bring together my series of annual variations with the results of the English and German investigations in the High Alps. We then see, that there is a very great agreement between the change occurring in the alveolar carbonic acid tension on going from Copenhagen to North-East Greenland and that which occurs on journeying from London or Berlin to the top of Monte Rosa.

	Zuntz (1895) CO_2 tension in mm. Hg		Ward (1907) CO_2 tension in mm. Hg			Author CO_2 tension in mm. Hg
Berlin	33·8	London	37·7	Copenhagen	Febr.	34·2
Zermatt	30·9	Zermatt	34·2	N. E. Greenl.	April	30·6
				—	June	29·4
Bétemps Hut	25·1	Monte Rosa	28·5	—	August	26·7

It may be added, that the same parallelism applies also to all the other respiratory functions investigated, frequency, total volume of air respired and metabolism. The agreement is so striking, that it seems to be more than due to chance, when Ward on his return from Zermatt found irregularly fluctuating values for his carbonic acid tension, just as I had a much greater standard deviation in my experimental group for November than in any of the remaining series. I imagine that the greater deviation in my November experiments was due to the lack of physiological equilibrium after the preceding, fatiguing sledge-journey; but it is not excluded, that the change in the carbonic acid tension may proceed uniformly in the one direction, less regularly in the other.

If the agreement in the series of values given above is more than due to chance, as I believe, the hitherto prevailing explanation of the results of the mountain experiments cannot be the right one; since the air-pressure in North-East Greenland is not lower than in Copenhagen, and there can therefore be no talk of lack of oxygen. The changes must be due, at least in the main, to a factor common to the two climates, and of such there is certainly only one which can come into consideration, namely, the strong light rich in ultraviolet rays, which is further strengthened by reflection from the ice and snow.

The effect of the light in the arctic regions is so striking in many ways, that it is a matter for general consideration. Snow-spectacles are necessary, and if these are forgotten the carelessness is very quickly punished with a very troublesome conjunctivitis. On journeys the skin on face and hands soon takes on a dark, copper-red colour, which gradually changes over to lighter or darker brown; on the nose and ears the epidermis peels off in large flakes, and excoriations or blisters occur, which only heal after a long time. As already mentioned, we can also note the effect of the light psychically. In summer in contrast to in winter there is a certain, restless eagerness to work, a reduced feeling of tiredness, which quite agrees with ZUNTZ and LOEWY's descriptions[1] and also with HASSELBALCH's experimental results[2].

It is well-known, that the Eskimos like the high-arctic peoples in general have almost as much skin-pigment as the tropical races, a phenomenon which is generally placed in connection with the intensity of the light.

With regard to the light reflected from the snow, I have only measurements from a single day; but these show quite distinctly — even if they naturally can only give an approximate picture of the conditions — that the reflected light is a factor of no small importance. The measurements were taken by Dr. WEGENER and myself one day towards the end of April about midday; they showed that the sensitive paper became black

under as far as possible perpendicular rays in	2·5 sec.
when the actinometer was held horizontally with the plate upwards in	3·0 sec.
when the actinometer was held horizontally with the plate downwards in	5·0 sec.

The maximum intensity of the light is indeed not so great at 77° N. L. as in the Alps, but as compensation the summer days last from the end of April to the end of August. It agrees well, therefore, with the explanation put forward, that the result which is attained in some few weeks in the Alps requires just as many months to appear in Greenland. And when regard is taken of the reduced total pressure at high altitudes, which, so far as the carbonic acid tension as isolated phenomenon is concerned, undoubtedly acts in the same direction as the light, as also to the comparative abrupt transition from the lowland to the high land, there is nothing remarkable in the fact, that the fall in the carbonic acid tension proceeds quickly in the beginning and thereafter more slowly, whilst in North Greenland it proceeds comparatively uniformly.

[1] l. c. Kap. XVII. [2] l. c.

As the sunlight is a factor, the action of which does not display itself at once in its fullest extent, but on the other hand, once manifest persists even a long time after the cessation of the light, it is a mistake to attempt to show the influence of the light by making experiments on the same day, partly in the sunshine, partly in the shade, as ZUNTZ and LOEWY have done. It agrees well with my experiments, however, that the German investigators have found a rise in the intensity of all the changes in the respiration on protracted stay in high altitudes, in spite of the fact, that the supposed lack of oxygen does not increase but must rather be considered to become less, on becoming accustomed to the high climate, as pointed out by themselves.

The minimum value of the carbonic acid tension in my experiments does not fall in the summer solstice either; the movement downwards is continued, at any rate so long as the sun is constantly above the horizon.

The second question which arises is this: why is the alveolar carbonic acid tension less in Greenland in January than in Copenhagen in February?

This question I am unable to answer satisfactorily; but various causes seem to play some part in this regard. In the first place it is doubtful, whether I have hit upon the maximum of the annual period in the Greenland experiments; a good deal indicates that this first occurs a month later. The sun appears in the middle of February; but on the ship all is covered over and the lamps lit till far on in March. The minimum period hardly occurs before the end of August and it is just towards the end of this time that its influence is most marked.

It is not excluded, further, that the after-effects from the previous light period are still present in the January experiments, as it was just in the preceding autumn that I was out on the sledge-journey, right until the sun disappeared and thus took as much advantage of the diminishing light as possible. It must also be remarked that the barometric pressure during this period of experimentation was very low, almost 10 mm. lower than during the experiments in Copenhagen; there can be no doubt that this is of some importance in this connection. And lastly, the temperature may also play some part; in the January experiments it was distinctly higher than at the other periods when experiments were made. The weather at this time was very broken and stormy, with abnormally high air-temperatures for the time of year. I am unable here, as is the case for the low temperature, to show definite experiments displaying the influence of the temperature, but there is some probability, to judge from the first-mentioned observations of HALDANE, that such an influence has been at work. If so, it would also contribute to the reduction of the alveolar carbonic acid tension.

In the work of HALDANE and FITZGERALD cited above, the authors have sought to find a possible daily period in the alveolar carbonic acid tension, but without being able to find it or make it seem probable. Nor is there any indication in this direction in my day experiments. As the table shows, the results are very uniform throughout the day and in general a little higher than in the morning, at any rate in the November experiments. In the May experiments this feature is more difficult to detect, as the cold made its influence felt distinctly in the morning experiments. That the numbers for May are a little higher than for November, is possibly connected with the generally lower air-temperature in the period first-mentioned. For the 14 November experiments the average temperature was 14·7°, for the 11 May experiments 11·8°. For the November experiments the carbonic acid tension was on an average $30{\cdot}4 \pm 0{\cdot}24$, $\mu = 1{\cdot}35$ or 4·4 % of the average. For May the corresponding values were $31{\cdot}9 \pm 0{\cdot}17$, $\mu = 0{\cdot}84$ or 2·6 % of the average. For the 4 experiments in December 1906 the average was 33·2.

Total volume of air respired ("Ventilation").

It may be regarded as certain, especially after the frequently mentioned English experiments, that it is the carbonic acid tension in the respiratory centre and this alone, which stimulates the centre and thus in so far determines its activity. The receptivity of the centre may change; but the stimulation is always the same, other vicarious forces do not seem present.

On the other hand, HALDANE and PRIESTLEY's supposition, that the alveolar carbonic acid tension is a constant magnitude for the same individual and that the total volume of air respired is so regulated as to maintain this constancy, does not find confirmation in later investigations.

It will have been seen from the foregoing, that increased carbonic acid tension as the result of a feeling of cold is accompanied by reduced total volume of air respired, that the reduced alveolar carbonic acid tension in the light occurs together with a greatly increased total volume of air respired and that the changes in the carbonic acid tension due to variations in the total pressure do not seem to have any effect on the total volume of air respired.

We are unable to reason, therefore, from changes in the alveolar carbonic acid tension to changes in the total volume of air passing in and out of the lungs.

Thus, whilst there is nothing in my experiments to indicate that the variations in the air-pressure influence the total volume of air respired, the effect of the cold is very clearly seen. For the April experiments we have

Temperature	Litres per hour (reduced)
6·0°	453
11·1°	485

The standard deviation in the two groups, which include 6 and 7 experiments, is 8·5 and 12·25 respectively. The difference is thus significant. It appears further from the short series given on p. 104 that the total volume of air respired decreases considerably on the first cold day and then rises uniformly on the following days. On the cold days in May such an "adaptation" does not occur, as there is a few days' interval between the three experiments. Quite a corresponding diminution in the total volume of air respired occurs in experiment No. 66, as also in No. 55, but here the temperature is not lower than at many other times, and I have noted nothing to show that I felt the cold during the experiment.

Lastly, it will be seen from the table on the day experiments, that the total volume of air respired is on an average lower in May than in November. Now the total volume of air respired was admittedly abnormally high during the period last-mentioned, but a difference can be distinctly seen in the table corresponding to the temperature. It is very marked, for example, in the groups for 9 a. m. and 3 p. m., where the difference of temperature is considerable, whereas we find the same total volume of air respired in the two series in the group for 5 p. m., where the temperature is very nearly the same.

The annual fluctuations in the total volume of the air respired will be seen from the following summary.

		Litres per hour (reduced)
Copenhagen,	February	$384 \pm 2{\cdot}4,\ \mu = 12{\cdot}5$
N. E. Greenland,	April (group II)	$485 \pm 3{\cdot}1,\ \mu = 12{\cdot}25$
—	June	$495 \pm 5{\cdot}1,\ \mu = 27{\cdot}4$
—	August	$521 \pm 4{\cdot}2,\ \mu = 19{\cdot}5$
—	November	$522 \pm 8{\cdot}4,\ \mu = 39{\cdot}6$
—	January	$472 \pm 3{\cdot}5,\ \mu = 17{\cdot}8$

We thus have an annual period with its maximum at the time when the alveolar carbonic acid tension has its minimum and *vice* *versa.* When we take into consideration the different influence due to the "dead space", when the frequency of respiration varies, the series will appear much more characteristic, since the highest frequency falls at the time of year when the total volume of air respired is least.

Two of the results apparently fall outside the series, the experiments for November and January. In November the total volume of air respired is just as great as in August, though both the alveolar carbonic acid tension and the frequency have increased as much as could be expected according to the time of year. In January the total volume of air respired is nearly as much as that for April, and even when we remember that the frequency of respiration for January is very high, this result will nevertheless appear remarkable, especially in connection with the Copenhagen experiments.

It has to be remembered here, however, that the November experiments were made immediately after a fatiguing journey, which means that the total capacity of the lungs was increased; but from this it again follows, that the total volume of air respired must remain large, if a given alveolar carbonic acid tension is to be maintained. Lastly, it is not at all improbable that the after-effects of this may still be felt in January. Jaquet and Stähelin[1] have experienced the after-effects from residence on a mountain for an even longer time.

The annual period appears most distinctly when we consider the average depth of the single expiration, since, as mentioned, the total volume of air respired and the frequency of the respirations vary in an opposite direction. The two, otherwise well-separated groups in the April experiments here fall together (with an average respectively of 964 and 976), as the reduced total volume of air respired owing to the cold is accompanied by a decreased frequency of respiration.

The volume of an expiration at 37° (saturated) is calculated in the tables.

The average values for these series may be given here.

	Vol. of an expiration at 37°, (saturated) in cc.
February 1909	$918 \pm 4{\cdot}8$, $\mu = 23{\cdot}5$
April 1907 (group II)	$976 \pm 11{\cdot}6$, $\mu = 45{\cdot}5$
June 1907	$1362 \pm 10{\cdot}6$, $\mu = 56{\cdot}5$
August —	$1491 \pm 9{\cdot}4$, $\mu = 43{\cdot}3$
November —	$1269 \pm 19{\cdot}3$, $\mu = 90{\cdot}4$
January 1908	$913{\cdot}5 \pm 12{\cdot}2$, $\mu = 62{\cdot}5$

[1] Archiv f. Exper. Pathologi u. Pharmacologi. Bd. 46, 1901; p. 300.

We again find here the analogy with Zuntz and Loewy's mountain-experiments. In "Höhenklima und Bergwanderungen", for example, the following series of numbers are given.

	Müller	Zuntz
Berlin	447	
Brienz	502	648
Rothorn	420	782
Col d'Olen	585	
Monte Rosa	1079	1495

The irregularity in the first column is due to the varying frequency of respiration; in both cases the total volume of air respired increases. Of the 22 persons examined by Zuntz and Loewy, only 2 showed reduced total volume of air respired on Monte Rosa; in all the others it was increased and remained so, even rising further during the stay at the top. We may believe, however, that a kind of adaptation took place; it is mentioned, that Zuntz on his first Alpine climb to the top of Gnifetti had a total ventilation of 8431 cc., on a later visit 7613 cc. per minute; but it is likewise mentioned, that Zuntz stayed for 6 days on Col d'Olen on his first journey before the climb.

If the increased volume of air respired is due to the light, "adaptation" in this connection is a superfluous supposition. The effect on the respiration will then be a function of the time passed in the strong light, and it will not be, or only secondarily, dependent on the altitude.

Jaquet and Stähelin[1] on Chasseral (1600 m.) found quite a small decrease in the total volume of air respired, but a small increase in the depth of the single respiration; but their results do not give any definite basis for the changes found and their experiments were of very short duration.

It is worth mentioning that Chasseral was chosen, because it has a damp climate with relatively few chemical light-rays.

Hasselbalch[2] found unchanged total volume of air respired after the light-bath but an increase in the depth of the single respiration, as the frequency was reduced; but this again means that the effective air respired was increased.

Nor do these investigations come into conflict with my view, that the light is the cause of the changes in the respiration in Greenland. Granted that it is the carbonic acid tension, which is determinative of the activity of the respiratory centre, then the increased

[1] l. c. [2] l. c.

total volume respired following on the low carbonic acid tension can be explained in one of two ways. Either substances are formed in the blood under the influence of the light, which reduce the threshold value of the carbonic acid tension, or the excitability of the centre is increased by reflex action. The question as to which of these two alternatives is correct, can scarcely be settled on the basis of the material available at present. There is no doubt that the light has some influence on the functions of the central nervous system; various observations speak definitely in favour of this. On the other hand, a considerable reduction in the alkalinity of the blood was found on Monte Rosa. Whether this is due to the light, I do not know; there are most probably no investigations on this point. And the question of the alkalinity of the blood seems on the whole to be at a very uncertain stage.

Frequency of the respiration.

If we study a text-book such as VIERORDT'S[1], we get the impression that the respiration-frequency was one of the best investigated and most accurately determined, physiological functions. We find it stated for example:

A rise in the air-temperature of 1° C. reduces the frequency by 0·054 per min.

A rise in the barometric pressure of 1¼ cm. increases the frequency by 0·74 per min. etc.

By far the most of these statements are taken from a work of K. VIERORDT from 1845; this has not been accessible to me; but in a later work of the same author[2] the experiments in question are briefly discussed.

VIERORDT gives the normal respiration-frequency to be 12 per minute, but adds that it may rise under certain circumstances. With increasing cold the frequency rises and at the same time the respiration increases in depth; the frequency rises and falls with the air-pressure.

These results are in direct opposition to what appears from my material on almost every point; but the methods pursued by VIERORDT are so full of shortcomings, that we hardly require to seek for other causes for the disagreement. VIERORDT inspires through the nose and expires through a mouth-piece; this is not a natural mode of respiration. Again he expires into a glass-receiver full of salt solution, and must therefore expire against a greater or less

[1] HERMANN VIERORDT: Anatom. Physiol. u. Physikal. Daten u. Tabellen. Jena 1906.
[2] KARL VIERORDT: Grundriss d. Physiologie d. Menschen. Tübingen 1862.

pressure, which makes the respiration further unnatural. Lastly, the receiver could only hold some few litres, so that the whole experiment could only last quite a few minutes. It may certainly be regarded as impossible to obtain in this manner information regarding the respiration-frequency of a quietly breathing person.

In his oft-cited work Hasselbalch[1] has devoted some space to the detailed discussion of the respiration-frequency. It may be considered as certain from his investigations, that the chemical light-bath reduces the frequency, since it lowers the tone of the capillaries of the skin. Hasselbalch further concludes, that the state of contraction of these skin blood-vessels must also have some influence on the frequency under ordinary conditions, as he proceeds to show in several regards. Thus, there is a distinct decrease in the frequency after defæcation; in agreement with Potain's investigations[2] there is higher frequency immediately after waking up than later, when one has become completely awake, as the result of the contraction of the peripheral vessels during sleep. It is likewise shown, that there is a dependence on the air-temperature, rising frequency with falling temperature. The material is however very imperfect on this point.

Zuntz and Loewy[3] find that the respiration-frequency at high altitudes behaves in different ways, sometimes decreasing, sometimes increasing. They found, as a rule, first increased then reduced frequency, as they gradually changed their station. In 2 out of the 6 individuals the frequency increased on the whole, in the other 4 it decreased. In one of the two with increasing frequency an increase in the depth of the single respiration also occurred however at the same time; as already mentioned, the total volume of air respired always increased at the high altitudes; in the other person the volume of the expiration remained unchanged. It might be thought now, that it was difficult or impossible for the individuals concerned to show the increase in the respiratory movements, which must be the consequence of a reduction in the frequency accompanied by increased total volume of air respired, and that the frequency must therefore have been unusual or abnormal. It is also possible, again, that the opposing forces, which are operative in this case, may result in a different way according to the personal peculiarities of the individual.

The rule is that the frequency decreases, as shown by the following table:

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[1] Det kemiske Lysbads Virkninger. Hospitalstidende 1905.

[2] cit. Hasselbalch. l. c.

[3] Höhenklima und Bergwanderungen. Kap. XI.

	MÜLLER	ZUNTZ
Berlin	13·0	—
Brienz	9·9	7·3
Rathorn	13·0	7·0
Col d'Olen	9·7	—
Monte Rosa	8·2	6·0

Whilst the majority of the members of the German expedition thus showed decreasing frequency with increasing altitude, the condition was different on one of Mosso's expeditions up Monte Rosa[1]. Of the 7 persons investigated only 2 showed a diminished frequency, in 4 it was increased:

		Frequency
U. Mosso	Gressony	12
	Margherita hut	13
B. Bizzozzero	—	11
		15
Camozzi	—	8
		9
Sarteur	—	10
		10
Solferino	—	10
		14
Chamois	Turin	18·5
	Margherita hut	15·5
Oberhoffer	—	20
		19

The two last-mentioned came directly from Turin, the others had been a week among the mountains before ascending Monte Rosa. It will be remarked that whilst the last two have a respiration-frequency of 18·5 and 20, all the others have considerably lower values to start with, and on Monte Rosa the highest value among the latter is scarcely as great as the lowest value in the two coming from Turin.

It will be shown below, that the frequency of respiration in my experiments rises when the air-pressure declines and *vice versa*; further, that my respiration-frequency is high in winter, low in summer. Connecting this with the view I have already set forth, that the changes undergone by the respiration in the high-arctic summer, which precisely agree with the changes experienced in high mountains, are due to the chemical rays of the sun, then the

[1] A. Mosso: Der Mensch auf d. Hochalpen. Leipzig 1899.

apparent contradiction in the results of the two expeditions mentioned will be explained.

When we very suddenly ascend into the strong light, the respiration-frequency will decrease, in the beginning a great deal, later more slowly, and this effect is in the great majority of cases much stronger than the effect of the decreasing air-pressure; this is seen, for example, in the majority of the Germans, as also in the two Italians from Turin. In the case of the Italians who had been some time among the mountains before the ascent of Monte Rosa, the respiration-frequency is already reduced to a minimum; as a result the decreasing air-pressure could show its influence during the last, considerable ascent, and the frequency rises.

There are exceptions from the rule; but the view put forth here, which so far as the effect of light is concerned has the advantage of being in agreement with the experimental results, gives at any rate a more satisfactory explanation of the observations than that which considers each change in the respiration in the mountains as a result of the rarification of the air at high altitudes. And this last explanation is quite useless, when we consider the respiration-frequency in my experiments.

February 09	$8{\cdot}50 \pm 0{\cdot}072,\ \mu = 0{\cdot}37$
April 07 (group II)	$10{\cdot}07 \pm 0{\cdot}15,\ \mu = 0{\cdot}61$
June 07	$7{\cdot}32 \pm 0{\cdot}067,\ \mu = 0{\cdot}36$
August 07	$7{\cdot}09 \pm 0{\cdot}066,\ \mu = 0{\cdot}31$
November 07	$8{\cdot}38 \pm 0{\cdot}11,\ \mu = 0{\cdot}50$
January 08	$10{\cdot}65 \pm 0{\cdot}22,\ \mu = 1{\cdot}11$

For the reasons already mentioned, it will perhaps be best to exclude the experiments 78 and 79 from the November series and experiment 102 from the January one; we then have:

November 07	$8{\cdot}14 \pm 0{\cdot}044,\ \mu = 0{\cdot}18$
January 08	$10{\cdot}36 \pm 0{\cdot}12,\ \mu = 0{\cdot}59$

We see again here the same annual periodicity as for the alveolar carbonic acid tension and the total volume of air respired; the turning-points of the curve lie in late-summer (minimum) and at the end of winter (maximum). And we see likewise, that the variation in the respiration-frequency from winter to summer is the same as occurs on passing from low to high levels, and that it is just this change we should expect, if the light is the cause of the variations.

As my experiments, unlike those made among the mountains, do not suffer under abrupt transitions in the light conditions, accompanied by great changes in the air-pressure, my series are much more uniform than the corresponding ones for the Alps. The difference between the extreme points is less in my experiments than in the latter, which is most probably due in the main to difference in the intensity of the light; but the fact that my respiration-frequency is on the whole relatively low certainly also comes into consideration. The relative reduction in my experiments is almost as great as in the mountain experiments.

The frequency in the Greenland winter and spring experiments is considerably higher than in Copenhagen. According to HASSELBALCH's investigations cited above, the respiration-frequency is a function of the state of contraction of the peripheral vessels, and this would mean, therefore, contraction of the peripheral vessels in the Greenland winter. This is also very probable. Direct experience shows, that one can much more easily stand severe cold in the winter than later in the spring, when the sun begins to make its influence felt, and the organism's protection against cold is mainly the contraction of the peripheral vessels. Measurement of the blood-pressure in the peripheral arteries points in the same direction. The average of 14 measurements of the blood-pressure in the art. rad. sin. in July 07 was 148·5 mm., and of 13 measurements in October 06 156 mm. The measurements were made with OLIVER's gauge. But as the measurements of blood-pressure are on the whole not so very accurate and self-observations especially difficult in this direction, I shall not lay too great weight on these results here.

Low temperature may however also influence the respiration-frequency in another manner. Here it is not the absolute temperature that is considered, but the feeling of cold, perhaps, as HALDANE has remarked in another connection, of an effect on the respiratory centre by reflexes, especially from the skin of the face and the hands. The feeling of cold reduces the frequency. The decrease cannot be expressed in numbers from my experiments; but the result is nevertheless obvious at several places. The few experiments which come in question here have already been discussed and need not been repeated here.

The air-pressure, as already mentioned, has a distinct influence on the respiration-frequency in several of my experimental series. To give a clearer view of my material I have represented the barometric height and the respiration-frequency graphically on the accompanying diagram; the scale is: 1 cm. = 2 respirations = 20 mm. barometric pressure; the numbers at the side indicate the level of the curves.

I have included a series of preliminary experiments from the winter of 1906; these experiments, which were made in the forenoon, thus after one of the principal meals, and which therefore cannot be directly compared with the later, morning experiments, cannot claim to have the same accuracy and uniformity as the latter; but the results are so considerable owing to the great fluctuations in the barometric height, that the curves may contribute, in spite of smaller irregularities in several of the experiments, to illustrate the conditions dealt with here.

We notice at once, that the two curves for the respiration and barometric height lie in the main symmetrically; looking closer, it is seen that the same movement is repeated in the small, in the variations from day to day; a rise in the barometer corresponds to a fall in the respiration-frequency and conversely. The exceptions are few, in the 22 experiments of the preliminary series the variations are 3 times in the same, in all the other cases in the opposite direction. Of the 3 divergent cases, only one is remarkable, the low frequency for $^{16}/_{11}$; the temperature (gas-meter) was on this day 8·15° against 15·4° on the preceding and 15·9° on the next day of the experiments. The experiment only lasted 20 minutes in contrast to the usual 30, and the air of the room was inspired, as it proved impossible to keep the breathing-pipe open owing to bad weather. This was likewise the case during the experiment on $^{7}/_{12}$, which was made a couple of hours later in the day than the other experiments and only an hour after a light meal. In this case, however, the temperature was 9·4° against 13° and 11° on the nearest days, and whereas the barometer fell evenly until the $^{16}/_{11}$, it fell 25 mm. in less than 24 hours on the $^{6}/_{12}$—$^{7}/_{12}$, which caused a sharp rise in the blood-pressure and various irregularities in the contraction of the peripheral arteries during the following days, both in myself and in 2 other individuals examined.

In the next series the symmetry of the two curves is also readily seen, and there are but few fluctuations from day to day. The respiration-curve is however somewhat irregular, several strikingly low values occurring, which are certainly due to the very low temperature in the room on these days. The fluctuations are less than in the previously discussed series and the amount of the reduction is approximately the same in both curves.

In June the symmetry is still present, when we consider the curves as a whole; but in details there is no longer agreement; the course of both is much more even than in the preceding. A single divergent value has already been mentioned.

In August the respiration-frequency may be regarded as almost constant, with exception of a couple of experiments. The relatively high frequency on $^{16}/_{8}$ was due, as mentioned, to a desire to defæcate during the experiment and this should perhaps be omitted. The low value on the last day is due probably to the influence of the cold. The two curves only show an indication of symmetry towards the end; for the rest, the course of the curves, as also the single rises and falls are apparently without connection with the barometric curve.

The even respiration-curve is still found in November; but the regularity is broken by a couple of strongly divergent values. I am not clear as to the reason for these deviations. The barometric curve in this series again shows somewhat greater variations but these have obviously no connection with the deviations mentioned in the frequency. Nor are the curves symmetrical here, they seem rather to indicate parallelism.

In the winter experiments, January 1908, we again meet with symmetrical curves, which are the same as the first-mentioned not only in the main but even in details. The variations are greater than in any other series of morning experiments and this applies to both curves. The striking deviation from the rule, which occurs in the respiration-frequency of $^{23}/_{1}$, can only be imagined as arising from the possibility of an error in counting, due to extreme sleepiness on the day in question. With regard to the high frequency on $^{24}/_{1}$ it may be remarked, that the barometric fall on $^{23}/_{1}$ lasted till late in the night, and that the rise thus occurred only a few hours before the experiment. This experiment in many respects resembles the experiment on $^{7}/_{12}$ in the preparatory series.

The features of the Copenhagen experiments are quite the same as those of the Greenland experiments at the same time of year. The symmetry between the two curves is unmistakable. It is best expressed in the first half of the period, as my mode of living was just as regular as in Greenland. In the latter half my bed-time especially was less regular; but it is only in one case that imperfect sleep had anything to do with the result, in experiment No. 132, which is divergent in several regards.

It results from the experiments, therefore, that the respiration-frequency and the air-pressure vary in an opposite direction[1]. This relation is distinct in the Greenland winter and spring experiments, as also in the Copenhagen experiments, more difficult, in part impossible to recognize in the Greenland summer experiments. Now the barometric fluctuations are less in summer than in winter; but

[1] The same was found by Mosso in sleeping marmots: Der Mensch a. d. Hochalpen. Leipzig 1889. Chap. XIX.

in the series, where the mutual dependence of the two functions is clear, it can also be seen in the small variations. As it is probable, that the influence of the respiration-frequency is felt throughout the vasomotor system, even when a distinct parallelism between blood-pressure and respiration-frequency cannot be detected (for this the factors, which affect the respiration and especially the blood-pressure, are too many), the result is only what we should expect and agrees with the experimental investigations of HASSELBALCH, namely, that the peripheral vessels, which are more or less paralysed in the summer owing to the light, react less readily than under other conditions. HASSELBALCH also found, that the respiration-frequency became more regular after the light-bath in addition to being less.

The respiration-frequency is subject to various influences in the course of the day. It is relatively high soon after waking up and decreases appreciably until one is fully awake; this morning value is the "standard value" for the individual at the time mentioned, the value which is found again at the quiet periods of the day, when no foreign influence makes itself felt. The frequency is hastened by muscular work, it rises likewise after meals, as will be seen from the accompanying table of the day experiments; but a regular day-curve cannot be shown.

The respiratory metabolism.

The determination of the respiratory metabolism and its variations suffers in my experiments from the very appreciable shortcoming, that only the amount of carbonic acid produced can be calculated. The oxygen absorbed is unknown.

I believe, however, that the respiration-quotient has altered so little under my regular mode of living with the same kind of food at all times of the year, that I can consider the carbonic acid given off as a measure of the amount of the metabolism, and consequently, that variations in the production of the carbonic acid are essentially due to quantitative and not qualitative changes in the respiratory metabolism.

If this is the case, then we find for the metabolism as for the respiratory functions already mentioned a distinct annual period, the curve of which agrees with the curve for the total amount of air respired; but its values are so great that they cannot be explained as a result of the increased work of respiration. ZUNTZ and LOEWY reckon 5 cc. increase of oxygen to 1 litre increase of total amount of air respired per minute. For a respiration-quotient of 0·8 this gives 4 cc. of CO_2. The total amount of air respired by me was in January 472, in August 521 litres per hour. The difference is 49 litres or almost 0·8 litre per minute, which gives 3·2 cc. CO_2 or 192 cc. per hour. Taking the bodily weight as 68 kilos. we have 2·3 cc. per kilo. and per hour; it will at once be seen that this quantity is in this connection quite without importance.

The series for the year becomes:

		CO_2 cc. per kilo. & hour
Copenhagen, February		$194 \pm 0{\cdot}9,\ \mu = 4{\cdot}4$
N. E. Greenland, April (group II)		$212 \pm 1{\cdot}6,\ \mu = 6{\cdot}1$
—	June	$238 \pm 1{\cdot}5,\ \mu = 7{\cdot}7$
—	August	$241 \pm 0{\cdot}6,\ \mu = 2{\cdot}4$
—	November	$237 \pm 2{\cdot}0,\ \mu = 9{\cdot}5$
—	January	$208 \pm 0{\cdot}9,\ \mu = 4{\cdot}9$

After the explanation given above, it is permissible to exclude experiment No. 132 from the first series, the average of which would then become $193 \pm 0{\cdot}6$, $\mu = 2{\cdot}7$. Further, it will certainly be most correct to exclude experiment No. 36 from the second group of the April series, as there is some uncertainty about the gas-analysis in this, otherwise normal experiment (see p. 102). It will be best, therefore, in this connection to take the following values.

$$\text{April}\quad 214 \pm 0{\cdot}7,\ \mu = 2{\cdot}4$$

The experiments made at a low temperature will be discussed more in detail below. If further experiment No. 98 is omitted, we have for the series of January:

$$207{\cdot}5 \pm 0{\cdot}7,\ \mu = 3{\cdot}6$$

With regard to the November experiments, it has already been mentioned several times, that they were made shortly after a journey, which made continuous demands on increased metabolism, in the day-time owing to considerable muscular work, at nights in order to thaw the frozen sleeping-bags. At the same time the declining sunlight was used to the very utmost, much more than usual when staying at a fixed station. After the journey I continued to eat more than under ordinary conditions, resulting in an increase of weight, which rose a little during these experiments, whereas it decreased in the other experimental series.

It is possible that the large value for January in comparison with the results of the control experiments, is due to the after-effects of the journey; it is likewise possible that the temperature has something to do with it. These experiments were made at a higher temperature than any other experimental series. Further, it is also possible that the value for February would have been lower than that for January. But there are two circumstances which must be remembered, especially as regards the metabolism. The one is, that the food was different in Copenhagen from that on the Expedition; the second, that the control experiments were made in a sitting position, whilst all the Greenland experiments were carried out in a kneeling position. How much the change of food means I cannot venture to say, though I do not believe that the difference is so very great. The different position has certainly very little influence on the results. After I had become accustomed to it, the kneeling position was very comfortable and only occasionally tired me, which the sitting position could also do if it were maintained without change during the experiment. The most probable thing is, that none of the reasons mentioned is the single cause, but that the difference is due to the interplay and conjunction of more or fewer of them.

There is thus an increase in Greenland of the respiratory metabolism during the light period in contrast to what occurs in winter. The increase is evident, amounting to 16 % of the minimum value or ca. 7 times the standard deviation of the lowest series, and in no case, therefore, can it be due to the contemporaneously increased total volume of air respired.

Just as in the other cases mentioned we have here also analogies from the mountain experiments.

Zuntz[1] gives for self-observations in 1901:

	CO_2 cc per minute	
Brienz	183·0	average of 3 experiments
Rothorn	192·7	— - 4 —
Monte Rosa	255·9	— - 3 —

The rarification of the air is given as the cause of the phenomenon, in spite of the fact, that no support is obtained in this case either from the experiments carried out in the pneumatic room, where the amount of oxygen used up, even with a considerable rarification of air, is less than in the mountains.

Jaquet and Stähelin[2] repeatedly point out this disagreement between experiment and observation and mention as other causes of an increase of metabolism: light, heat-radiation, dry air, temperature. Their own experiments for the rest do not explain very much;

[1] Höhenklima u. Bergwanderungen. Chap. VIII.
[2] l. c.

they chose a mountain station (Chasseral), where the importance of these other factors might be considered as relatively slight, in order to obtain the main influence pure; but as the height was but small (ca. 1600 m.), the experiments are not illustrative of possible changes owing to low air-pressure.

In opposition to other physiologists who have made mountain experiments, MOSSO[1] maintains that during rest there is no increase in the production of carbonic acid at high altitudes. His results are however too few and vary too much to be able to form a basis for his conclusions.

HELLER, MAGER & SCHRÖTTER[2] also consider the lack of oxygen as the cause of changes in the respiration at high altitudes. In addition to an *anoxyhaemia absoluta* (*anoxyhaemia barometrica*, JOURDANET), they also postulate an *anoxyhaemia relativa*, which is only active during work, and not during rest; the latter can be promoted by the climatic conditions: temperature, wind, absolute dryness, "gewiss auch das Licht".

SPECK[3] cites a number of the older authors with regard to the effect of light on metabolism, especially MOLESCHOTT, who found a no slight increase of metabolism in frogs under the action of light; the increase is proportional to the strength of the light and is due partly to its action on the skin, partly through the eye. SPECK points out that MOLESCHOTT's results are far too variable to form a basis for conclusions, and maintains especially, that MOLESCHOTT like other authors has not paid attention to the more active muscular movements in the light. SPECK shows from his own experiments, that the light cannot be credited with any importance in increasing the metabolism, but perhaps that it has a stimulating effect on the total volume of air respired with the resulting increased absorption of oxygen and separation of carbonic acid.

SPECK has most probably not estimated the last-named condition at its true value; but it plays a very subordinate part in this connection. The arrangement of his experiments is not satisfactory on the whole, and especially as regards the action of light it is hardly to be expected that this would come into consideration. SPECK has not exposed himself at all to the chemical rays of light; he remained in his working-room and there carried out a series of experiments of quite short duration, alternately with open and with blind-folded eyes. It is not to be expected that he would come to any result by this method; when he nevertheless believes that he has found in-

[1] l. c., Chap. XV.
[2] Zeitschr. f. klin. Medizin. Bd. 34, 1878.
[3] Physiologie des mensch. Athmens. Leipzig 1892, Chap. XI.

creased total volume of air respired in "the light-experiments", this must certainly be due to purely psychical factors.

It appears, therefore, that light is not credited with much, and yet, when we consider my Greenland experiments, this must be the main thing. On this point I am again in agreement with HASSELBALCH's experimental investigations. He found, both in himself and in Dr. J., an increase in metabolism of ca. 5 % in the morning after a light-bath. The effect was of short duration, but the time of exposure was only 1 hour. It is not a contradiction, that a less powerful but longer exposure can give both a stronger and more lasting result.

Although ZUNTZ and LOEWY, as mentioned, do not attach very much importance to the light, there are various things in their work, which point in the same direction as the series noted as analogous to my experiments. Thus, with regard to the effect on the metabolism, there is constantly a recognizable relation to the time.

CASPARI[1]	O_2 per kg. &	CO_2 per min.
Brienz 6—7 Aug.	3·46	2·78
Monte Rosa 9 Sept.	4·05	3·17

ZUNTZ	O_2 cc. per min.	
	259·2	aver. of 16 experiments in bed
	272·0	aver. of 5 experiments on the train

The last is of less importance; but it is included just to show that the light plays a small part — if any at all:

In bed	27/8	267·3
—	4/9	272·9

thus, again a rise on the last day; on the train, same day, 272·0.

It seems to me that ZUNTZ and LOEWY have in some way misunderstood their problem with regard to the investigations on the effect of light. The light is a climatic factor in the mountains as in the Arctic climate, and its influence cannot therefore be studied by making experiments in the sun and in the shade on the same day and at the same place. If we shorten the time of the experiment we must to the same extent strengthen the excitation, if we wish to obtain an appreciable result. When we desire to have a strong, momentary effect of light, we must, as HASSELBALCH has

[1] Höhenklima u. Bergwanderungen. Chap. VIII.

done, use a very powerful light rich in ultra-violet rays. And we must remember, that when we once have a recognizable effect of the light on the respiration, it takes some time before this effect disappears. An immediately following "dark experiment" will be quite illusory.

What we find in the literature seems to me, therefore, rather to be in favour of my view, that the light is a climatic factor of importance. It may be added further, that the same effect, which is caused by the Arctic and the mountain climate, is also found, though less marked, in the sea-climate[1]. And lastly, various investigators, among others also ZUNTZ, have shown that similar phenomena are to be observed on balloon-journeys.

What is common to all these different localities is the strong light, but not the rarification, nor the lack of oxygen, nor the amount of moisture, nor the temperature.

It still remains to mention some experiments made at low temperatures, especially group I of the April 07 experiments and the morning experiments of May 08, in all 8 experiments. To these are joined some few of the experiments mentioned above, where a similar "effect of cold" may with more or less probability be considered to have been present, as also some few of the day experiments of May 08, which are given in the accompanying table.

It has long been regarded as certain, that warm-blooded animals react towards the cold with increased metabolism. Experiments of LIEBERMEISTER, VOIT, RUBNER, PRINCE CARL THEODOR are cited as proof of this; only a few experiments of SENATOR point like SPECK's self experiments in an opposite direction. Later, it has been found, that warm-blooded animals under the influence of curare lost this property and remained like cold-blooded. Through the investigations of LOEWY[2] and JOHANSSON[3] it has been determined, that the rise under the cold will not appear when the individual experimented on is able to keep his muscles at rest. SANDERS-EZN[4] was of the opinion, that the metabolism in homoiotherms was different in relation to the outer temperature, according as the latter had some influence on the body-temperature or not. If the body-temperature is altered, the metabolism rises and falls with the outer temperature as in poikilotherms. This seems to be based on a confusion of

[1] Ide: Zeitschr. f. physical. u. diaet. Therapie. IX, p. 189.
[2] Pflügers Archiv. XLVI, 1890, p. 189.
[3] Skand. Archiv. f. Physiologie. 7, 1897.
[4] cit. JOHANSSON. ibid.

cause and effect. Detailed investigations of TIGERSTEDT and SONDÉN[1] have given the result, that the variations in the temperature of the body are probably entirely determined by the fluctuations of the metabolism. And my experiments at different times of the day point quite in the same direction. A decrease in the metabolism under the cold, as in poikilotherms or in hibernating animals in winter, is thus not proved, as far as is known to me. My "cold experiments" mentioned above point however in this direction.

As already shown, the April experiments may be divided into two well-separated groups according to the air-temperature:

	Air-temperature	CO_2 cc. per kilo. & hour
group I	6°·3 C.	$201 \pm 0{\cdot}95,\ \mu = 3{\cdot}16$
group II	11·0 -	$214 \pm 0{\cdot}67,\ \mu = 2{\cdot}45$

So far as known to me, there is no other difference whatsoever in the experimental conditions for these two groups than the different outer temperatures; but on the other hand, the difference in this regard is distinct. The morning experiments of May join on to the first of these groups.

Air-temperature	CO_2 cc. per kilo. & per hour.	
7°·6 C.	203	$\mu = 1{\cdot}4$

The temperature was read from the gas-meter; the air surrounding my body during the experiments was considerably colder, especially in the April experiments. I felt the cold, often a great deal, but without real discomfort, and I did not react with chattering or muscular movement of any kind. There might have been an inconsiderable muscular stiffness in the lower limbs when I rose up after the experiment; but I am unaware of any appreciable difference between these and the other days of the experiments. And the experiments, as mentioned, were not planned with this object in view, to investigate the effect of the low temperature.

Whilst there is a decreasing effect on the total volume of air respired and the frequency, as also most probably on the alveolar carbonic acid tension, on the 4 or 5[2] successive "cold days" in April, such a progressive "adaptation" cannot be detected in the metabolism; on the contrary, there is here such a marked parallelism with the temperature that it appears more than a mere chance.

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[1] Skandinav. Archiv. f. Physiologie. 6, 1895.

[2] Experiment No. 34, where the gas-analysis is wanting, is also included so far as regards the frequency and the total volume of air respired.

Exper. No.	Temp.	CO_2 per kilo. & per hour
31	7°·5 C.	202
32	8·25 -	205
33	6·6 -	199
35	1·45 -	196
42	7·5 -	203

In the May experiments there is no definite order; the three values must be considered as identical, as the difference between the highest and the lowest is only 1 % of the average.

When the average for the cold days is the same in May as in April, in spite of the more advanced time of year and in spite of the slightly higher temperature for the former, it must be remembered, that there is no definite temperature at which the effect begins, and that the low temperature is felt the more, the further we are on in the spring. It is only in summer, when the epidermis has become sufficiently thickened and pigmented, that a change occurs in this regard.

Bringing together what these 8 experiments show with regard to the influence of the air-temperature on the respiration, we obtain:

Frequency of the respiration	decreases
the alveolar CO_2-tension	increases
the total volume of air respired	diminishes
the respiratory metabolism	diminishes

The increased carbonic acid tension in conjunction with the diminished total volume of air respired indicates, that the excitability of the respiratory centre is reduced, thus that the point of attack of the "cold's influence" is the central nervous system itself. It is not to be wondered at, therefore, that the "cold's influence" indicated here is displayed in a manner which recalls the respiration characteristic for hibernation.

The daily period of the metabolism will appear from the accompanying table, which embraces a double-series from November-December and one from May. The experiments were carried out on different days; it was only exceptionally that two fell on the same day, never more.

Owing to the uniformity which marks my series of experiments, I consider such an arrangement as permissible. It then appears that the four day-curves have the same form and only differ slightly from one another. TIGERSTEDT[1] has also maintained, that in the same individual under the same outer conditions the amount of carbonic acid given off only varies slightly from day to day, even for months.

It will be seen that there is a distinct rise after the meals and then the metabolism sinks again regularly towards the value given in the morning experiments at the corresponding time of year.

The morning experiments in May are however, like the "cold experiments", under the value which corresponds to the time of year, and several of the day-experiments for May are probably influenced by the relatively low temperature.

The Annual Period.

As result of the foregoing it may be said, that there is a distinct annual fluctuation in the respiratory functions in the Arctic regions. The turning-points of the curve lie towards the end of the summer, presumably at the time when the sun "begins to go down", and in the beginning of spring, probably shortly after the sun has again appeared above the horizon. Considering the changes from winter to summer, we see that

the respiration-frequency	decreases by 32·4 %
the alveolar carbonic acid tension	decreases by 16·0 %
the total volume of air respired	increases by 10·4 %
the respiratory metabolism	increases by 15·9 %

Excluding experiment No. 102 of the January series the decrease in the respiration-frequency becomes 31·6 %.

When we remember that in January there may still be some after-effects from the journey in autumn, especially as regards the total volume of air respired, as also that January is probably not the month for the turning-point of the curve but that this falls a month later, then it is probable, that the results given here are less than the annual variation will prove to be on further investigation.

Taking the control-experiments as starting-point and remembering that special conditions have prevailed during my January experiments, we may imagine a journey beginning in Copenhagen in February and ending in North-East Greenland in August; we then obtain the following results:

The respiration-frequency	decreases by 16·5 %
the alveolar carbonic acid tension	decreases by 21·9 %
the total volume of air respired	increases by 35·7 %
the respiratory metabolism	increases by 24·2 %

[1] l. c.

The last three results are thus greater than those given above, especially as regards the total volume of air respired; the reason for the last has been mentioned above. On the other hand, the difference in the results for the respiration-frequency is remarkable, as it goes in the opposite direction to the others. This is obviously connected with the fact, that the frequency is a function of the innervation of the vessels and is thus somewhat influenced by the winter.

In consequence of the foregoing it is extremely probable, that the annual period is due in the main to the variations in the intensity of the light. We should therefore be entitled to expect a corresponding though less distinct period in lower degrees of latitude. So far as the available, sparse material goes, this seems also to be the case.

The experiments in the following table were all made in the morning while fasting[1]. Each experiment occupied 30 minutes and the arrangement was essentially the same as in my experiments. The individual experimented on was well-trained to the work, as a large series of experiments preceded those reported on here[2]; there is reason to believe, therefore, that the results are a reliable expression of the respiration of the person concerned within the period mentioned.

Calculating the values for the separate columns, we obtain

	Frequency	Vol. of 1 expir. at 37°, saturated	CO_2 in alveoli mm. Hg
26/3-8/4 (12 exper.)	$15{\cdot}2 \pm 0{\cdot}19, \mu = 0{\cdot}78$;	$759 \pm 8{\cdot}4, \mu = 42{\cdot}9$;	$38{\cdot}4 \pm 0{\cdot}46, \mu \doteq 2{\cdot}37$
13/5-15/6 (25 exper.)	$14{\cdot}3 \pm 0{\cdot}07, \mu = 0{\cdot}54$;	$801 \pm 4{\cdot}2, \mu = 30{\cdot}9$;	$35{\cdot}3 \pm 0{\cdot}24, \mu = 1{\cdot}75$

In spite of the relatively short space of time which lies between the two series, the results are distinct, which may be considered to rest partly on the fact, that the greatest changes in the intensity of the light occur just at this time of year, partly also on the fact, that the individual experimented on reacts very strongly to outer influences, a phenomenon confirmed by other experiments on the same individual. The single values also fluctuate much more than in my

[1] From material not yet published from the Laboratory of the Finsen Institute, kindly placed at my disposal by Dr. HASSELBALCH.

[2] These experiments are not included here, as they were made after the morning meal.

Date	Respir. frequency	Vol. of 1 Expir. at 37° saturated	CO_2 in alveol. mm.	Remarks
26/3	16.0	803	39.0	(3rd light-bath 4/3).
27/3	15.9	649	43.1	
28/3	15.3	760	40.1	
29/3	17.0	751	35.0	
30/3	15.1	754	39.0	
1/4	15.5	783	37.0	Valve jammed in the last 15 mins.
2/4	15.9	748	35.4	4th light-bath.
3/4	12.9	807	37.2	Slept soundly. Moderate erythema on back.
4/4	14.3	779	36.2	Erythema almost disappeared. Uncomfortable position on chair. Respiration-frequency very fluctuating.
5/4	14.9	745	41.3	Difficulty in breathing during the last part of the experiment.
6/4	15.1	716	40.1	
8/4	14.6	809	37.1	Tired and discomfort in arms from work on previous day.
13/5	13.9	785	35.2	After cold weather for some time, strongly rising temperatures in the last 4 days.
14/5	12.9	812	37.1	
15/5	13.8	821	35.5	
16/5	13.9	836	33.3	
17/5	14.3	838	32.1	
18/5	14.4	776	36.8	
20/5	14.6	861	32.0	
21/5	14.1	813	33.8	Air-temperature 10° to-day, 6° previous day.
22/5	14.5	846	32.0	Valve jammed almost during whole experiment.
23/5	14.4	782	36.7	
24/5	14.6	763	37.4	
25/5	13.9	785	36.2	
27/5	14.4	786	35.8	
28/5	14.1	818	32.8	
29/5	14.3	763	37.3	5th light-bath.
30/5	14.0	808	36.0	Light-bath almost without effect.
31/5	14.9	770	36.2	Still no erythema.
6/6	14.6	815	34.2	
7/6	14.9	786	36.4	
8/6	15.4	762	36.6	
11/6	14.6	761	36.9	6th light-bath.
12/6	13.2	809	36.5	Awake several times in the night. Skin discomfort. Erythema discoloured.
13/6	13.5	838	35.0	
14/6	14.8	746	36.7	Erythema practically gone. Experiment failed the first time.
15/6	14.3	834	33.1	

self-experiments, a difference that is partly counterbalanced by the relatively long series of experiments.

The material is in so far not "pure", as light-baths have been included on a few days. The effect of these on the functions in question here, seems however to be restricted to a few days and affects the two series practically to the same extent, as there was one light-bath in the short, two in the long series. Since, further, the single elements of the experiment are subject from day to day to influences of various kinds, temperature, air-pressure, individual disposition, no inconsiderable fluctuations will also occur on this account within the single columns. To unravel these in details would, however, be too difficult, especially for one who has not made or directed the experiments.

As previously mentioned, HALDANE has also indicated the existence of such a periodicity with respect to the alveolar carbonic acid tension[1]. He gives here:

	CO_2 pressure in mm. of mercury	
May 06	41·7	(6 cases)
June 06	39·7	(7 —)
November 06	41·3	(3 —)
July 07	39·8	(7 —)
August 07	39·6	(3 —)

Thus the movement here also has the same direction as in my experiments; the difference, as we should expect, is somewhat less. It must be remarked, however, that the cases are relatively few, which means so much the more as the values on which the averages are based, vary much more than mine.

Apart from this there is only one extensive series of experiments which concerns the present subject.

E. SMITH fifty years ago carried out a very large number of respiration experiments, partly to determine the amount of carbonic acid produced in 24 hours, partly to investigate the yearly variation. He came to the following results[2].

The respiration has a maximum in the spring and a minimum in the autumn; it decreases in June, increases in October. The reduction in summer amounts to according to the author:

in the total volume of air respired	30 %
in the frequency	32 -
in the production of carbonic acid	17 -

[1] The Journal of Physiology. Vol. XXXVII, Nos. 5—6, Decb. 1908.

[2] Proceedings of the Royal Society of London. Vol. IX, 1859, p. 611. Philosophical Transactions. Vol. 149. II. 1859.

Smith's results, which are in decided opposition to mine, are everywhere cited where these questions are discussed and without criticism; they thus seem to have obtained general recognition. It seems to me, however, that various objections can be raised against these experiments.

In the first place the person investigated on did not keep still. The author mentions at several places, that he was indisposed and uncomfortable, often changing his position; he would partly sit, partly stand during the experiments. He mentions, further, that he held on the respiratory mask with the hands, when he had no other use for them. This muscular activity brings an element of uncertainty into the results, which cannot be calculated and which, especially in the case of quite few experiments in one month, may have an influence out of proportion to its value. In the second place it is not known, whether Smith lived on the same kind of food the whole year round; but it is most probable that he did not do so. He himself mentions, that higher "carbonic acid values" were obtained as a rule on the Monday, as one does less and eats better on the Sunday. And in his curve for the amount of carbonic acid given off, there is a very marked rise from the $^{20}/_{12}$ to the $^{10}/_{1}$, which makes one think in this connection of the intervening Christmas festivities. To this must be added that the bodily weight is not given; the production of carbonic acid is given in absolute values, not per kilo. of body weight.

Lastly, it is not quite clear to me, where Smith has obtained the above-mentioned values for the annual period. In his main work we find the series given below, which if we except the frequency is far from giving the results quoted above. The value agrees in the case of the frequency, if we calculate the percentage from the lowest value, which is not quite correct in this case. In the short summary from which the numbers cited are taken, no experiments are given.

It appears from the summary below, that the months of April and May take up a special position; considering the last column, the frequency, it strikes one at once, that there is quite a remarkable jump from March to April. From this series we might just as well conclude, that the frequency falls throughout the year and then suddenly rises from March to April. To judge from the numbers the respiration in April and May has undoubtedly been forced. And the reason for this forced respiration is not far to seek; it lies in the imperfect training of the individual experimented on, who is not quite accustomed to his apparatus and does not feel comfortable during the experiments. Whilst the CO_2-curve, excluding the rise

Month	E. Smith			Mr. Moule
	CO_2 grains per min.	Total volume of air respired in cubic inches	Frequency	CO_2 grains per min.
April	8.58	498	14.3	7.18
May	8.89	451	12.4	6.63
June	8.19	426	11.64	6.34
July	7.62	393	11.0	
August	7.15	392	10.9	
September	7.13	402	10.94	
October	7.67	395	10.93	
November	7.86	414	10.87	
December	8.27	429	11.15	
January	8.35	447	11.73	
February	8.20	..	11.35	
March	8.25	..	11.38	

due to Christmas, is very regular in the last 4 months, it varies very greatly in the first 2 months. In another individual, Mr. Moule, who has obviously found himself more at home with the apparatus, the curve also has a different appearance; instead of the pronounced rise in May in the case of E. S. we find here just as marked a fall; unfortunately these last experiments were not carried on throughout the whole year.

An experiment of longer duration, which is described in detail, resembles a performance in modern sport. One of the participants, Professor F., was obliged to give up the experiment for a time owing to exhaustion; Smith says with regard to himself: "arteries throb; hands swollen". And the third person experimented on was no better.

The results, that may be taken from such experiments, can in no case refer to the normal respiration.

With regard to the annual period it may further be remarked, that the months are very unequally represented. For example, there were 26 experiments in April, 4 in October. August, September and October together embrace no more experiments than one of the spring months. This may easily affect the results where the numbers are so variable. In August there are some few experiments which all fall before the 10th; but at another place the author gives two long, in his opinion more complete experiments, one for the

22/3 which gave 10,644 gr. CO_2 and one for 10/8 with 11,713; this does not suggest less metabolism in the summer.

If we exclude April and May, which fall quite outside the rest of the series, we find that the frequency varies in the same direction as in my experiments: the summer and autumn have slightly lower values than the winter and spring. The difference is however only 7—7·5 %, not the 32 % given in his paper.

Similar conditions seem to prevail in the case of the amount of carbonic acid given off; but here, as mentioned, we lack information regarding the food and body weight, quite apart from the fact, that the numbers are more or less affected by errors arising from uncontrolled muscular movements.

For the rest, the author mentions, that the carbonic acid curve is symmetrical with the curve for the air temperature, which further shows, according to the above-cited investigations of JOHANSSON and others, that he had not maintained the necessary tranquillity during the experiments.

The author is of a similar opinion with regard to the relation between the production of carbonic acid and the air-pressure. None of these factors can in his opinion, however, be the cause of the annual period. He mentions the light as a possibility.

Altogether, it seems to me that the conclusions E. SMITH has drawn from his material are not justified, as also that this material is on the whole not suited to form a basis for conclusions with regard to the annual periodicity of the respiration.

RÉSUMÉ.

The contents of the preceding pages may be summarized under the following heads.

I. In Arctic regions there is a distinct annual period in the respiration with turning-points at the end of the summer and the end of the winter.

For the functions investigated: respiration-frequency, total volume of air respired, alveolar carbonic acid tension and metabolism, the changes from February to August are as follows, taking the control experiments as the starting-point.

The respiration-frequency	decreases by 16·5 %
the alveolar CO_2 tension	decreases by 21·9 %
the total volume of air respired	increases by 35·7 %
the respiratory metabolism	increases by 24·2 %

The periodicity is due first and foremost to the variations in the intensity of the sunlight.

II. The respiration characteristic for the Arctic summer is even in details the same as has been found in experiments during rest in the High Alps.

It is therefore extremely probable, that this last is likewise due to the sunlight, and not, as hitherto supposed, to low air-pressure and presumed lack of oxygen.

III. Experiments made at low temperatures show a peculiar change in the respiration suggestive of the respiration during hibernation.

IV. The opinion expressed by Haldane and Priestley, that the alveolar carbonic acid tension is a constant magnitude for each individual, and that the total volume of air respired is so regulated that the *status quo* is maintained in this regard, is in all probability unmaintainable.

This probably holds good for short periods under quiet conditions; but the alveolar carbonic acid tension varies very considerably under the influence of climatic factors: light, air-pressure and temperature.

Much indicates, however, that these changes may be due to changes in the excitability of the respiratory centre.

9—4—1910.

Summary of the morning experiments

No.	Date	Barometer	Temperature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour, 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	Temp.	Remarks
	Febr. 09																	
127	6	754	11.6 12.0	30	267	8.90	208	390	778	..	..	0.036	..	..	..	64		
128	7	761	11.8 12.2	30	251	8.37	192	363	765	..	..	..	..	..	..	60 66		
129	8	766	12.1 13.1	30	245	8.17	203	386	828	943	3.20	..	4.69	193	..	58 59		
130	9	764	11.3 11.7	30	251	8.37	207	394	823	945	3.18	..	4.65	195	63.5	60 60		
131	10	754	11.0 11.6	30	263	8.77	206	387	783	898	3.16	0.035	4.73	191	..	65 61		
132	11	762	9.0 10.0	30	269	8.97	214	410	796	919	3.21	..	4.76	205	..	59 58		Slept less than usual. Extra meal-time 11 p. m.
133	12	767	10.0 10.8	30	234.5	7.82	193	371	824	948	3.31	..	4.88	193	..	56 61		
134	13	765	11.1 11.9	30	251	8.37	197	376	784	898	3.20	..	4.80	188	..	57 59		
135	14	763	12.0 13.0	30	240	8.00	198	375	825	941	3.33	..	4.88	195	..	58 59		
136	15	744	12.3 13.1	(30)	246.5	(8.22)	202	(372)	820	935	3.34	..	4.91	(194)	..	57 59		Minute hand of stop-watch stopped at 28 mins.
137	16	747.5	13.3 14.7	30	261.5	8.72	207	381	793	899	3.29	..	4.92	195	..	59 61		
139	18	759	12.7 13.7	30	257	8.57	200	375	778	884	3.22	..	4.86	188	..	58 60		
140	19	762	12.6 13.2	30	269	8.97	211	398	784	892	3.15	..	4.74	195	63.75	59 60		
	April 07																Mouth	
29	13	774	10.5	31	302	9.74	267	500	884	1016	2.98	0.01	4.22	216	68	63	35.9	
30	14	771	9.0	30	300.5	10.02	276	536	920	1064	3.28	0.02	4.57	257	..	76	36.3	Gas-analysis not reliable.

31	15	771.5	7.5	30	258	8.60	225	440	872	1015	3.15	0.03	4.47	202	..	73	36.15	
32	16	770.5	8.25	30	261	8.70	231	450	885	1030	3.12	0.04	4.38	205	(67.5)	74	36.2	
33	17	769	6.6	30	293.5	9.78	232	454	792	926	2.99	0.03	4.44	199	..	70	36.15	
34	18	764	4.45	20	212.5	10.62	155	456	731	863	..	..	..	..	67.5	72	36.1	Experiment interrupted and begun again.
35	19	756	1.45	30	300	10.00	238	468	793	949	2.86	0.04	4.16	196	..	73	36.0	Do.
36	20	752	11.9	30	327	10.90	258	483	789	903	2.80	0.04	4.17	198	..	70	36.2	Gas-analysis scarcely reliable.
37	21	752	12.0	30	330	11.00	262	490	794	908	2.97	0.03	4.42	215	(67)	75	36.15	
38	22	750	11.6	30	303	10.10	266	497	878	1006	2.96	0.03	4.20	217	..	67	36.15	
39	24	760	9.3	30	294.5	9.82	257	491	874	1011	2.91	..	4.12	211	67	58	36.15	
40	25	766	12.5	35	327	9.34	288	469	881	1004	3.06	0.05	4.35	211	..	66	36.2	Experiment interrupted and begun again.
41	26	770	9.95	30	283.5	9.45	241	466	852	982	3.15	0.04	4.51	216	..	69	36.2	Sampling not quite regular.
42	27	758	7.5	30	272.5	9.08	233	448	857	999	3.05	0.04	4.35	203	(66.5)	64	36.0	
	June—July 07																	
44	20	753	8.55	35	277	7.91	314	512	1133	1315	3.15	0.04	4.06	237	..	62	35.7	
45	21	756	12.05	20	145	7.25	172	485	1186	1355	..	..	..	..	67	62	35.9	Valve-apparatus out of order.
46	22	758	11.55	30	234.5	7.82	272	514	1162	1331	3.16	0.04	4.06	239	..	59	35.8	
47	24	762.5	10.4	30	227	7.57	280	535	1233	1419	3.02	0.05	3.82	239	(66.5)	63	35.75	
48	26	760	12.7	30	216	7.20	263	495	1218	1388	3.23	0.03	4.14	238	..	68	35.9	
49	27	764	11.6	30	227	7.57	249	474	1097	1256	3.18	0.04	4.17	224	..	61	35.8	
50	28	764	11.6	30	218	7.27	260	495	1193	1365	3.23	0.03	4.12	240	(66)	57	36.05	
51	29	766	10.9	30	211	7.03	236	452	1118	1283	3.41	0.04	4.44	231	..	61	39.95	Some uncertainty about watch.
52	30	761	11.65	30	218	7.27	272	515	1248	1428	3.14	0.03	3.98	243	..	68		
53	1	760.5	11.05	30	214.5	7.15	258	490	1206	1384	3.32	0.03	4.22	246	65.5	70 66	36.0	
54	2	764	10.65	30	226.5	7.55	279	533	1235	1419	3.12	0.03	3.96	251	..	59	36.2	Warm and perspiring on waking up.
55	3	766	10.9	30	199.5	6.65	229	438	1151	1321	3.37	0.03	4.34	225	(65)	61 52	35.95	
56	4	764	12.7	30	208	6.93	264	500	1269	1446	3.18	0.03	4.00	243	..	58	35.8	

Summary of the morning experiments (continued)

No.	Date	Barometer	Temperature (C.)	Duration of experiment	Number of respirations	Frequency	Litres expired	Litres per hour, 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	Temp.	Remarks
57	Aug. 07 13	756	12.1	30	213	7.10	274	515	1286	1469	..	..	..	..	..	64	Rect. 36.2	
58	14	761	11.3	30	215	7.17	269	511	1251	1434	..	..	..	..	..	60	36.2	
59	15	761	10.75	30	208	6.93	265	504	1274	1464	3.19	0.04	3.99	243	..	55		Sampling not regular.
60	16	756	12.0	30	234	7.80	299	562	1278	1460	2.83	0.03	3.55	240	65.5	60	36.2	Troublesome desire to defæcate during experiment.
61	17	758	11.05	31	228	7.35	297	544	1303	1496	2.89	0.04	3.60	237	..	53	36.3	
62	18	750	10.3	30	203	6.77	279	524	1374	1583	3.11	0.04	3.83	246	..	63	36.4	
63	19	754	9.9	30	214	7.13	275	520	1285	1483	3.07	0.03	3.83	241	..	54	36.2	
64	20	754.5	10.9	30	211.5	7.05	273	515	1294	1487	3.10	0.04	3.88	241	..	68 50	36.4	
65	21	758.5	10.4	30	209.5	6.98	283	537	1354	1558	2.99	0.04	3.68	242	..	57		
66	22	761	8.5	30	197.5	6.58	251	482	1274	1478	3.32	0.04	4.15	241	..	54		
71	Nov. 07 17	742	14.8	30	247.5	8.25	299	545	1210	1367	3.10	0.04	3.96	242	(69	70 66		
72	18	747	12.2	30	232.5	7.25	250	464	1078	1232	3.56	0.05	4.70	236	..	64		
73	19	748	11.3	30	243	8.10	261	487	1074	1232	3.34	0.04	4.40	233	..	63		
75	21	748	14.1	30	247	8.23	256	472	1036	1173	3.35	0.05	4.48	225	..	57		Gas-analysis scarcely quite reliable. Uncertainty about watch.
76	22	759	11.4	30	245	8.17	274	518	1118	1283	3.27	0.04	4.26	243	..	63		
78	24	758	12.0	30	277	9.23	319	601	1152	1318	3.01	0.05	3.88	256	(69.5)	56		

79	25	762.5	11.95	30	283	9.43	265	502	936	1069	3.10	0.04	4.29	221	..	65		Expiration not quite free at the beginning of experiment.
80	26	760.5	10.0	30	239	7.97	286	545	1197	1380	3.15	0.05	4.00	243	..	66 52		
81	27	756	11.55	30	251	8.37	286	538	1139	1304	3.15	0.06	4.07	239	..	63		
82	28	762.5	9.9	30	248	8.27	286	547	1155	1332	2.98	0.04	3.84	231	..	63		
96	Jan. 08 17	754	12.9	30	287	9.57	244	452	843	960	3.24	..	4.70	211	68.5	65		
97	18	748.5	13.0	30	306	10.20	252	467	824	938	3.09	0.04	4.53	208	..	64		
98	20	746	11.3	30	318	10.60	268	499	843	968	3.04	..	4.39	220	(68)	65		Uncertainty about gas-analysis.
99	21	747	14.7	30	324	10.80	271	497	837	946	2.94	..	4.30	212	..	67 55		Pulse 66 S. after dressing.
100	22	745.5	16.2	30	330.5	11.02	259	471	785	881	2.99	0.04	4.52	204	..	61		
101	23	733.5	16.0	30	319	10.63	261	467	818	920	3.03	..	4.49	207	(67.5)	58		
102	24	737	14.65	30	415	13.83	278	503	670	758	2.77	..	4.57	204	67.5	60		
103	25	742	15.0	30	342	11.40	257	468	758	849	3.12	..	4.81	213	..	58		Uncertainty in gas-analysis.
105	27	754	12.5	30	302	10.07	237	443	785	893	3.17	..	4.77	205	..	60		Do.
106	28	760	16.5	30	313	10.43	248	459	793	888	3.03	..	4.56	203	..	63		
107	29	770	13.5	30	280	9.33	248	472	886	1005	3.07	0.04	4.37	211	..	58		
108	30	759.5	14.5	30	298	9.93	252	471	846	956	2.97	..	4.31	204	..	60		
115	May 08 21	759.5	7.7	30	222	7.40	214	412	963	1121	3.36	0.03	4.58	202	(68)	57	36.6	
119	23	754.5	7.1	30	224.5	7.48	221	423	983	1148	3.36	..	4.54	205	(69)	66	36.55	
123	26	761.5	8.1	30	219	7.30	212	408	968	1125	3.46	..	4.71	202	69.5	53	36.35	

Summary of day experiments
November—December 1907

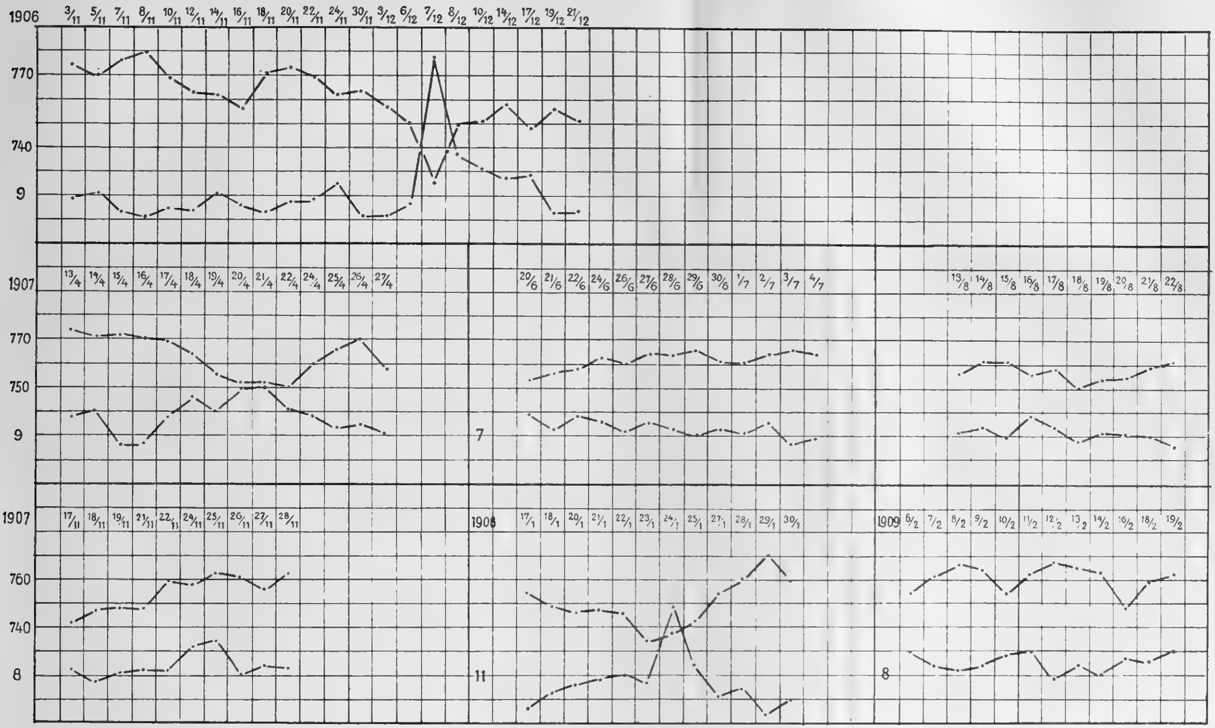
No.	Date	Time	Barometer	Temperature (C.)	Duration of experiment	Number of respiration	Frequency	Litres expired	Litres per hour, 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)[1]	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	CO_2 average for time of day	Rect. temp. average for time of day	Mouth temp. average	Remarks
83	29/11	9^{a}	762	12.55	30	298	9.93	331	626	1111	1266	3.13	0.05	4.09	275	(70)	82				
95	11/12	9^{a}	757	13.95	30	264	8.80	306	570	1159	1311	3.48	0.03	4.50	291	(67.5)	104 86	283	37.05	36.4	Experiment unreliable.
74	29/11	$11^{a}27$	740	13.45	30	273	9.10	319	583	1168	1331	3.48	0.04	4.48	291	(69)	76				Experiment made too near breakfast (late rising).
92	8/12	$11^{a}33$	756	18.0	30	258.5	8.62	268	490	1039	1155	3.34	0.04	4.49	238	(68)	78 76	(238)	37.27	36.4	
84	30/11	$1^{p}05$	747.5	11.3	30	317	10.57	337	628	1063	1220	3.22	0.04	4.25	286	(70)	81				
91	7/12	$1^{p}15$	760	16.5	30	287	9.57	318	589	1108	1241	3.11	. .	4.09	264	(68.5)	74 78	275	37.29	36.53	
86	2/12	$3^{p}10$	753.5	13.75	30	300	10.00	303	563	1010	1146	2.85	0.05	3.85	225	(70)	68 76				
90	6/12	$3^{p}25$	762.5	15.4	30	267	8.90	274	512	1026	1155	3.10	0.04	4.18	228	(68.5)	69 78	226.5	37.22	36.45	
89	5/12	$5^{p}33$	761.5	15.2	30	281.5	9.38	266	496	947	1067	3.26	0.04	4.53	231	(69)	79				
93	9/12	$5^{p}05$	761.5	16.0	30	285	9.50	252	469	884	992	3.11	. .	4.45	212	(68)	76 80	221.5	37.25	36.68	
85	1/12	$7^{p}05$	750	14.8	30	358.5	11.95	364	670	1017	1149	3.15	0.04	4.25	298	70	96				
87	3/12	$7^{p}05$	756	14.5	30	313	10.43	326	600	1032	1167	3.19	. .	4.28	272	(69.5)	93 87	285	37.54	36.55	
88	4/12	$9^{p}25$	759	13.8	30	300.5	10.02	301	563	1003	1137	3.23	0.04	4.38	261	(69)	76 85				
94	10/12	$8^{p}52$	758.5	16.2	30	306	10.20	300	555	980	1099	3.08	. .	4.22	250	67.5	76 76	255.5	37.47		

December 1906

No.	Date	Time	Barometer	Temperature (C.)	Duration of experiment	Number of respiration	Frequency	Litres expired	Litres per hour, 0°, 760 mm. (dry)	Vol. of an expiration at given temp.	Vol. of an expiration at 37° C. (saturated)	% CO_2 in expir. (dry)[1]	% CO_2 in inspir.	% CO_2 in alveol. (dry)	CO_2 in cc. per kilo. & per hour	Weight in kilos.	Pulse	CO_2 average for time of day	Rect. temp. average for time of day	Mouth temp. average	Remarks
25	14/12	$11^{a}25$	758	17.45	30	292	9.73	276	507	945	1054	3.37	0.04	4.71	254	(66.5)	73 76				Interrupted and begun again, breathing-pipe stopped up.
26	17/12	$11^{a}15$	748	12.7	30	296	9.87	256	475	865	986	3.33	. .	4.78	237	(66)	62				

27	19/12	11a 05	756	14.3	30.5	252	8.25	258	473	1024	1161	3.41	0.03	4.62	245	..	60 60				
28	21/12	11a 05	750.5	15.3	30	250	8.33	254	467	1016	1145	3.47	..	4.69	243	..	70 71	245		36.36	
May 1908																					
117	22/5	9a	752.5	6.2	30	246	8.20	270	517	1098	1286	3.52	0.03	4.58	263	(68.5)	96 82				
121	25/5	9a 18	761	8.9	30	261.5	8.72	279	535	1069	1238	3.32	..	4.37	253	69.5	92 83	258	37.3	36.38	
125	27/5	11a 25	754	14.05	30	253.5	8.45	253	470	1000	1136	3.41	0.03	4.63	229	69.5	78 79				
126	28/5	11a 20	757	14.5	30	245.5	8.18	273	508	1114	1259	3.33	..	4.36	246	(69.5)	76 82	237.5	37.2	36.5	Stood and walked a little before experiment.
110	16/5	12p 55	756	11.2	29	264	9.10	264	515	1000	1150	×	..	..	..	..	65 74				Experiment stopped too soon, error in reading the watch. Gas-analysis failed.
112	18/5	1p 10	750.5	9.6	30	285	9.50	274	517	962	1112	3.29	0.03	4.19	248	(68)	80 80	248	37.31	36.36	
111	17/5	2p 55	754.5	10.6	30	257.5	8.58	253	478	985	1133	×	0.03	..	..	..	71 76				Gas-analysis failed.
113	19/5	..	755.5	11.0	30	254.5	8.48	256	483	1008	1157	3.49	..	4.70	246	(68)	72 72	(246)	(37.39)	36.55	Walking for a couple of hours before the experiment.
114	20/5	4p 40	760	14.05	30	244	8.13	264	494	1082	1225	3.31	0.03	4.37	238	68	71 74				
116	21/5	4p 35	756	15.3	30	240.5	8.02	257	476	1071	1206	3.41	..	4.56	235	(68.5)	72 79	236.5	37.27	36.56	
122	25/5	6p 30	762	14.1	30	241.5	8.05	286	537	1187	1344	3.31	0.03	4.29	256	(69.5)	69 75				
124	26/5	..	759	10.6	30	254	8.47	275	522	1083	1245	3.52	..	4.62	262	..	66 77	259	37.30	36.5	
118	22/5	8p 35	754	11.0	30	237	7.90	255	480	1076	1236	3.41	0.03	4.49	235	(69)	75 74				
120	24/5	8p 30	760	15.0	30	233	7.77	277	516	1189	1341	×	..	..	..	..	72 75	235	37.50	36.8	No gas-sample. Stop-cock in wrong position.

[1] Owing to a small systematic error in the CO_2 figures, which was not detected until the earlier sheets were printed, some of the figures in the table on p. 53 are not quite correct and must be altered according to the numbers given here.


1906
3/11 5/11 7/11 8/11 10/11 12/11 14/11 16/11 18/11 20/11 22/11 24/11 30/11 3/12 6/12 7/12 8/12 10/12 14/12 17/12 19/12 21/12
770
740
9
1907
13/4 14/4 15/4 16/4 17/4 18/4 19/4 20/4 21/4 22/4 24/4 25/4 26/4 27/4
20/6 21/6 22/6 24/6 26/6 27/6 28/6 29/6 30/6 1/7 2/7 3/7 4/7
13/8 14/8 15/8 16/8 17/8 18/8 19/8 20/8 21/8 22/8
770
750
9
7
1907
17/11 18/11 19/11 21/11 22/11 24/11 25/11 26/11 27/11 28/11
1908
17/1 18/1 20/1 21/1 22/1 23/1 24/1 25/1 27/1 28/1 29/1 30/1
1909
6/2 7/2 8/2 9/2 10/2 11/2 12/2 13/2 14/2 16/2 18/2 19/2
760
740
8
11
8