REPORT
OF
A VIST
TO
FOREIGN
LABORATORIES

AUG. 1932
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P. G. BENEDICT

CARNEGIE INSTITUTION
OF WASHINGTON

NUTRITION LABORATORY
BOSTON

CONFIDENTIAL

B. N. L.

# REPORT OF A VISIT TO FOREIGN LABORATORIES

# AUGUST 1932 through FEBRUARY 1933

By

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Nutrition Laboratory of the Carnegie Institution of Washington

Boston, Massachusetts

1932-1933

ITINERARY AND INDEX

## INTRODUCTION.

### XIVth International Congress of Physiology.

It was planned this year to begin the foreign tour with the International Congress of Physiology which was held in Rome, Italy, August 29 to September 3. Since I purposed visiting practically all the European centers where work of direct interest to the Laboratory was being carried out and at these centers to give lectures representing results of unpublished work for the most part, it seemed best not to prepare a paper for the Congress. On the other hand, the Nutrition Laboratory was well represented by an interesting paper given by Dr. Carpenter. Both Dr. Carpenter and myself presided at sessions of the Congress, but for me the Congress was primarily a period of orientation, for there I could see many of the men I was to see later and get specific information with regard to the programs, the periods of their vacation, and their return to active work, etc. Much of this we had endeavored to secure beforehand by correspondence, but the inevitable overlapping of semesters in the various universities and the fact that the more northern universities begin their work earlier in the season made it necessary to rearrange a program that had been fairly well established before leaving America.

At the Congress there were a very large number of old friends and research workers in our special lines. Although I did not attend by any means all of the sessions, those which were of particular interest to the work of the Nutrition Laboratory were attended. I purposely entered into no discussions, realizing that I would see these individual workers at their own laboratories later and there take up points that needed clarifying. (See figures 1, 2, and 3.)

It is a fact that at a Congress the "Discussion" is divided into two different classes; first, complimentary statements with regard to research carried out. This goes without saying and it is quite unnecessary to take up the time of the Congress to make such statements. It is not helpful. The second method is, I think, to ask questions or to express differences of opinion with regard to the speaker and this is invariably interpreted on the part of the audience as a whole as adverse criticism. No matter if the paper as a whole is approved, if one point is raised in the line of discussion it is almost always considered a sign of disagreement with the entire paper. This of course is unjust. I was particularly interested in the communication of Professor Noyons on the insensible perspiration as being affected by different degrees of humidity, but left practically all discussion or criticism until subsequently arriving in Utrecht.

Figure / . Rome, Italy. Photograph of group at International Congress of Physiology in Rome.

Figure 2 . Rome, Italy. Group at International Congress of Physiology in Rome. In the center in white suit, Professor Asher. Many others can be recognized by lens.

Figure 3 . Rome, Italy. Professor J. E. Johannson. Photograph taken during International Congress of Physiology.

Dr. Carpenter presided at one of the most important sessions where there was a good deal, perhaps the most, discussion and I heard nothing but words of approval of his handling of the situation as chairman.

During the several days of the Congress a large number of appointments were made and definite plans outlined for visiting the different laboratories and rearrangement of the general itinerary, which had to be completely upset, thus almost making it apparent that there could be nothing done in the southern universities, that is, nothing south of Berlin. So, the first major change in the program was to plan to jump from Rome to Stockholm, a very long jump.

## Exhibits at Rome.

There was a fairly large exhibit of instruments and perhaps a larger proportion than usual dealing with metabolism.

Rat apparatus. A rat apparatus of Italian design, although made by Castagna of Vienna, attracted attention at first sight for it was extremely pretty, but singularly enough there was no recognition of the water vapor given off by soda lime or potassium hydroxide and yet the exhibitor claimed to measure both the oxygen and the carbon dioxide. The pump was very elaborate and probably would not stand up. There was a beautiful model of the small spirometer of the Krogh type. My impression was that the exhibit was beautiful as an exhibit but wholly impracticable. (This apparatus is shown in figures 4, 5, and 6 herewith.)

Benedict respiration apparatus. There was a grotesque model of the so-called "Benedict respiration apparatus", with a giant soda-lime can closed with seven thumb screws. It might work but was very cumbersome. (See figures 7 and 8.)

Knipping type of apparatus. A large model, designed I think by Melli, of the Knipping type of apparatus for man included the absorption of $CO_2$ in potassium hydroxide which was mechanically agitated, and then released later by sulphuric acid. It was an extremely complicated device and I do not know whether it would work or not.

Scheme for respiration apparatus with three spirometers. The Nutrition Laboratory has been working for years to try to find some apparatus that would write graphically both the oxygen consumption and the carbon-dioxide production. Our experience in investigating the Hagedorn apparatus covers many months. Mr. Coropatchinsky and I spent fruitless months trying to devise such

5.)

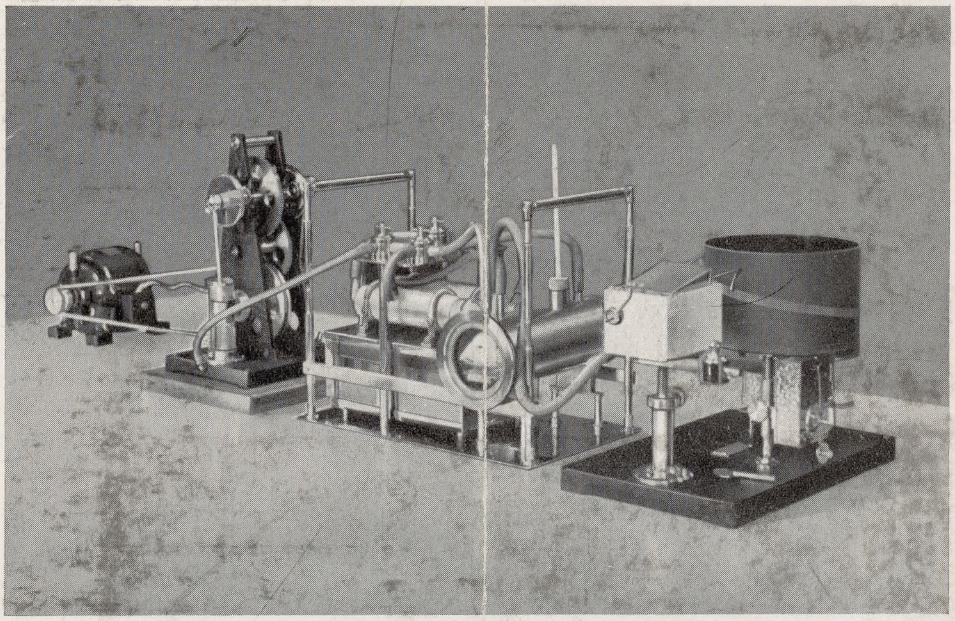


Figure 4. Rome, Italy. Rat apparatus of Italian design, made by Castagna of Vienna and exhibited at International Congress of Physiology at Rome.

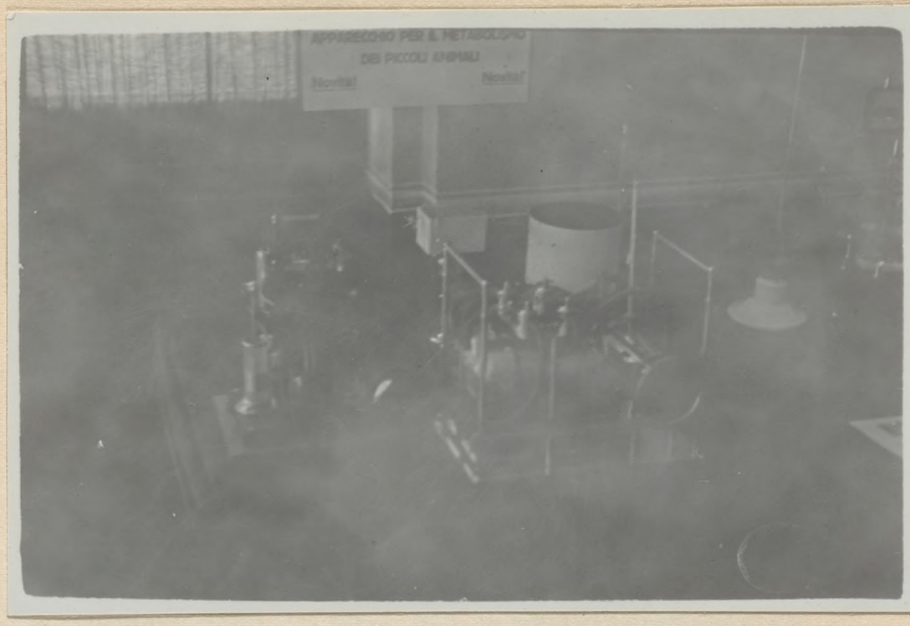



Figure 5 . Rome, Italy. Rat apparatus of Italian design, made by Castagna of Vienna and exhibited at International Congress of Physiology at Rome. Very poorly constructed.

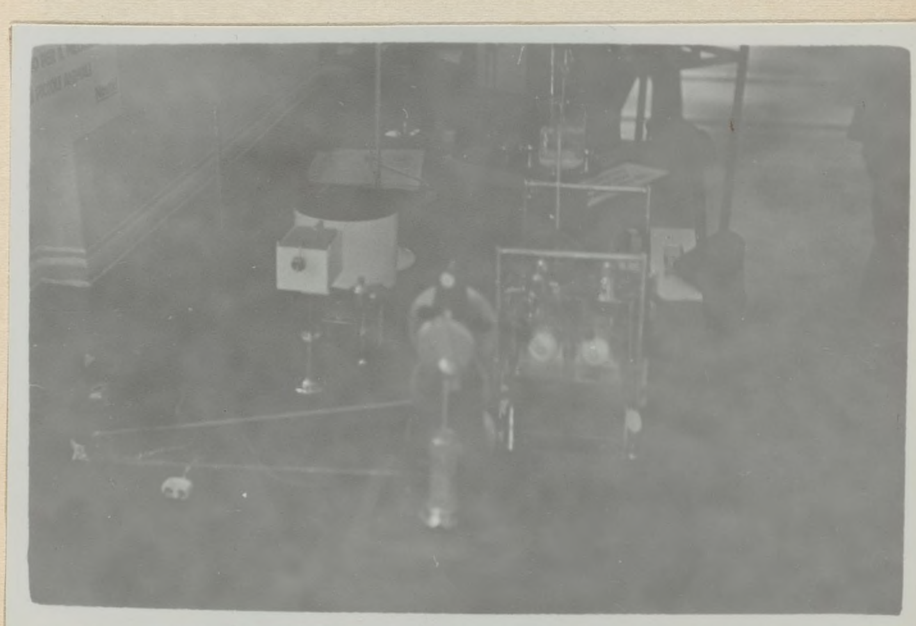


Figure 6 . Rome, Italy. Another view of rat apparatus of Italian design, made by Castagna of Vienna and exhibited at International Congress of Physiology at Rome.

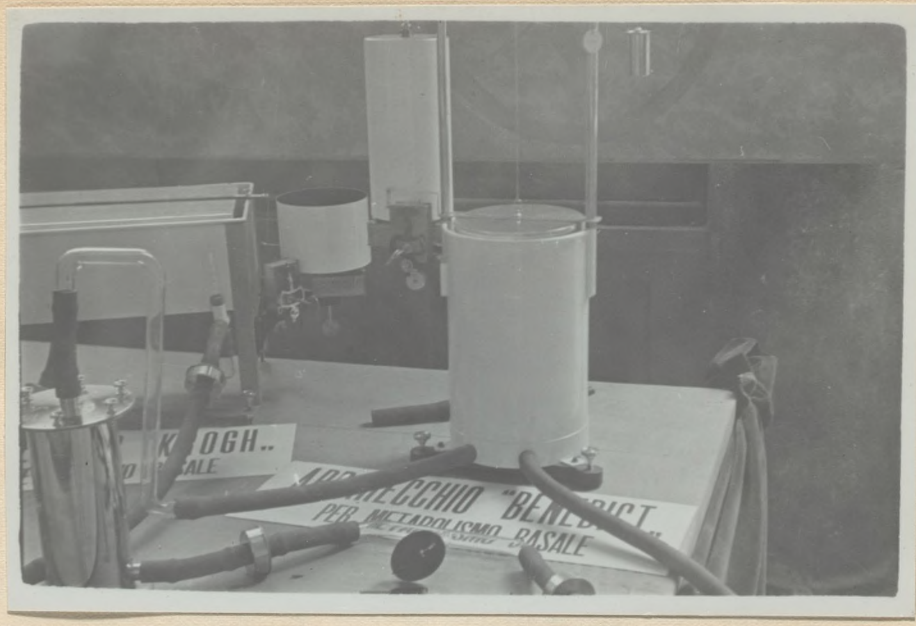



Figure 7 . Rome, Italy. View of part of exhibit at International Congress of Physiology at Rome, showing Krogh respiration apparatus at left and Benedict respiration apparatus at right. This view shows the large valves and the massive soda-lime container at the lower left-hand corner.

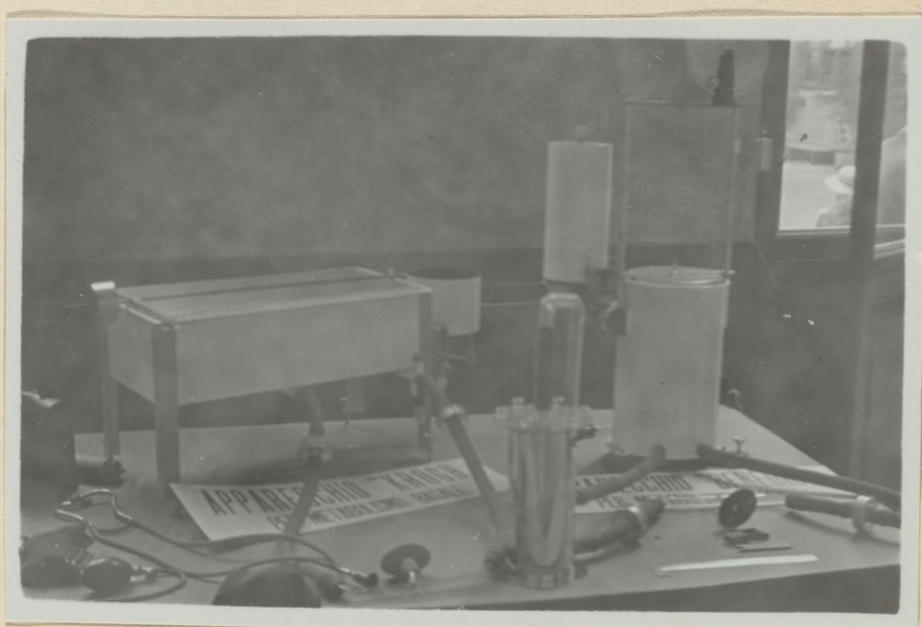


Figure 8 . Rome, Italy. Another view of part of exhibit at International Congress of Physiology at Rome, showing Krogh respiration apparatus at left and Benedict respiration apparatus at right with metal soda-lime can.

an apparatus. Here I saw what at first sight seemed to be a solution of the problem. The apparatus was devised by Professor Trimarchi of Milan. As a matter of fact there was no apparatus at the exhibit, only the scheme, but Professor Trimarchi stated that he had the apparatus at Milan. There were three spirometers but in this case, as I recall, he weighed the carbon dioxide, but again, as in the small rat apparatus mentioned above, he apparently forgot about the water vapor given off from soda lime. On careful examination of the scheme I came to the conclusion it was wholly impracticable. Since this time I have looked for a description of it but fail to see it. I hardly think it can materialize.

Respiration calorimeter at Genoa. Shortly before the Congress closed and after all of my travel arrangements had been made, I met a Professor Viale of Genoa, who told me he had an apparatus for direct calorimetry at Genoa and that he had succeeded in having a constant temperature chamber hewn out of solid rock, for equal temperature. I tried to find further information about this on my tour but no one had actually seen it.

Pipette for pyro. A Dr. Marchelli had what appeared to be an extremely ingenious pipette for absorbing fluids in gas analysis. I called Dr. Carpenter's attention to it as he had not seen it and suggested that he study it very carefully. Dr. Carpenter brought back with him one or more of these pipettes and finds that while they are ingenious they are impracticable and in no sense a betterment over the present form that he has on his apparatus. (See comment by Krogh, page 64.)

## Congress communications.

Of the numerous Congress communications, a relatively small proportion were of vital interest to us. Dr. Carpenter visited most of these and I took in only a relatively few. Of these I have already mentioned the paper of Noyons on the insensible perspiration. At the time I was there my notes read definitely that Noyons maintains that the insensible perspiration varies with the humidity, that I must see him in Utrecht, and that I felt there was a great deal of water vapor in the bed clothing, etc., and not enough time allowed for equalization. This recalled the same difficulty that had been written about so voluminously by Heller. This point of humidity will be touched upon several times in this report.

Comments on the helmet. Following a discussion of the Chinese metabolism, Necheles made the comment that he had found that the helmet was useless. I think I pointed out that the metabolism fell 10 per cent during sleep when measured by the helmet, and Necheles got up and stated that he thought the helmet was no good. It then transpired that Necheles had obtained the first helmet made outside the Nutrition Laboratory and made on order by Collins, who had no business to make it for him. We had not published the material and had not perfected the closures, nor had Collins been authorized to make it. This is an extraordinary representation of the damaging of the success of technique by imperfect preliminary use. It is needless to say that the thousands of experiments with the helmet have proved its unusual success.

## Diplomatic mission.

Visiting as I do so many different laboratories, and with the same interest in problems shown in nearly every laboratory, it has been frequently possible for me to correct misunderstandings between different research men, to carry greetings from one to the other, and to discuss results in a very general way, bringing in the work of the various individuals. Thus I have obtained, often unsolicited, the very adverse criticism of one investigator of another, and also not infrequently very favorable comment. I have felt it one of my duties to carry forward with me all references to other workers that may be helpful and stimulating and encouraging, with, of course, minimum emphasis upon adverse criticism other than what can be brought out in a personal discussion of points at issue. Thus I have frequently taken an adverse comment of another investigator, put it forth as a comment of my own, and tried to straighten the thing out, and then have not infrequently written back to the other man either that he in my judgment was right or that he was wrong. This has all been done in a very quiet, unpretentious sort of way, so that I have been able to straighten out not a few such difficulties. It was a matter of very great personal pleasure to me when I have been, on several occasions, introduced by the speakers as a "liaison officer" between the various laboratories.

Such a phase of the European travel I have designated under the head of "Diplomatic missions". It has called for not a little thought and at times not a little correspondence. If one is favorably impressed with an institute or with a research worker it is a relatively small matter to write, after the visit, a letter to the senior of the laboratory (in case one is writing with regard to an assistant), or to the rector of the university or even to the Minister of Education, when one is commenting upon the work of an institute as a whole. These letters undoubtedly do good that can not be measured in terms of material welfare, perhaps. They are always appreciated and it is one graceful method of showing the appreciation of the Nutrition Laboratory for the innumerable courtesies that have been extended to us.

As a result of the unprecedented success of the Congress in Rome, I felt it was simple justice to write to the Minister of Education regarding the monumental task undertaken by Professor Bottazzi in organizing the Congress, and this letter is here appended as an example of the type of letter that I had the great pleasure of writing on frequent occasions.

21st October, 1932.

S. E. Professor Ercole,
Ministre Educazione Nationale,
R O M E,
Italy.

My dear Sir,

Since I am writing while en tour, I have not official stationery, but I am writing officially as Director of the Nutrition Laboratory of the Carnegie Institution of Washington, located in Boston, Mass., U.S.A.

In common with a large number of Americans, Mrs. Benedict and I had the unusual good fortune to be present at the recent International Congress of Physiology held in Rome under the direction of Professor F. Bottazzi.

During the week of the Congress all of us were extremely impressed by the extraordinarily successful arrangements for both the scientific and the social functions. We feel deeply grateful to the Italian Government and particularly to Professor Bottazzi for all their efforts in arrangements for our comfort and pleasure during this time.

At one time the possibility of holding a Congress seemed somewhat doubtful, but with characteristic energy Professor Bottazzi carried the thing through to a splendid climax.

In a letter like this it is impossible to make a complete assessment of the scientific value, which was very great, but I would like to emphasize a side of the Congress that was probably not so definitely realized by the Members until after departure from Rome with certain time for reflection.

I am referring to the tremendous educational value of this Congress in aiding a large number of foreigners to better understand the true idealism of not only Italian scientists, but of the Italian Government. Most of us went to Rome, I am sure, either inadequately or wholly falsely informed as to the Italian situation. First hand observation shows us how erroneous our ideas were and hence I feel personally that the Congress has accomplished a great end in a step towards mutual international understanding. It is needless to say that one of the most potent factors in accomplishing this was the wholly unique and sympathetic character of Professor Bottazzi. May I congratulate you and the Italian Government in having had the details of this Congress in the hands of so worthy a representative as Professor Bottazzi.

I am,

Very truly yours,

(signed) Francis G. Benedict

FRANCIS G. BENEDICT.
Director.

## Foreign lectures.

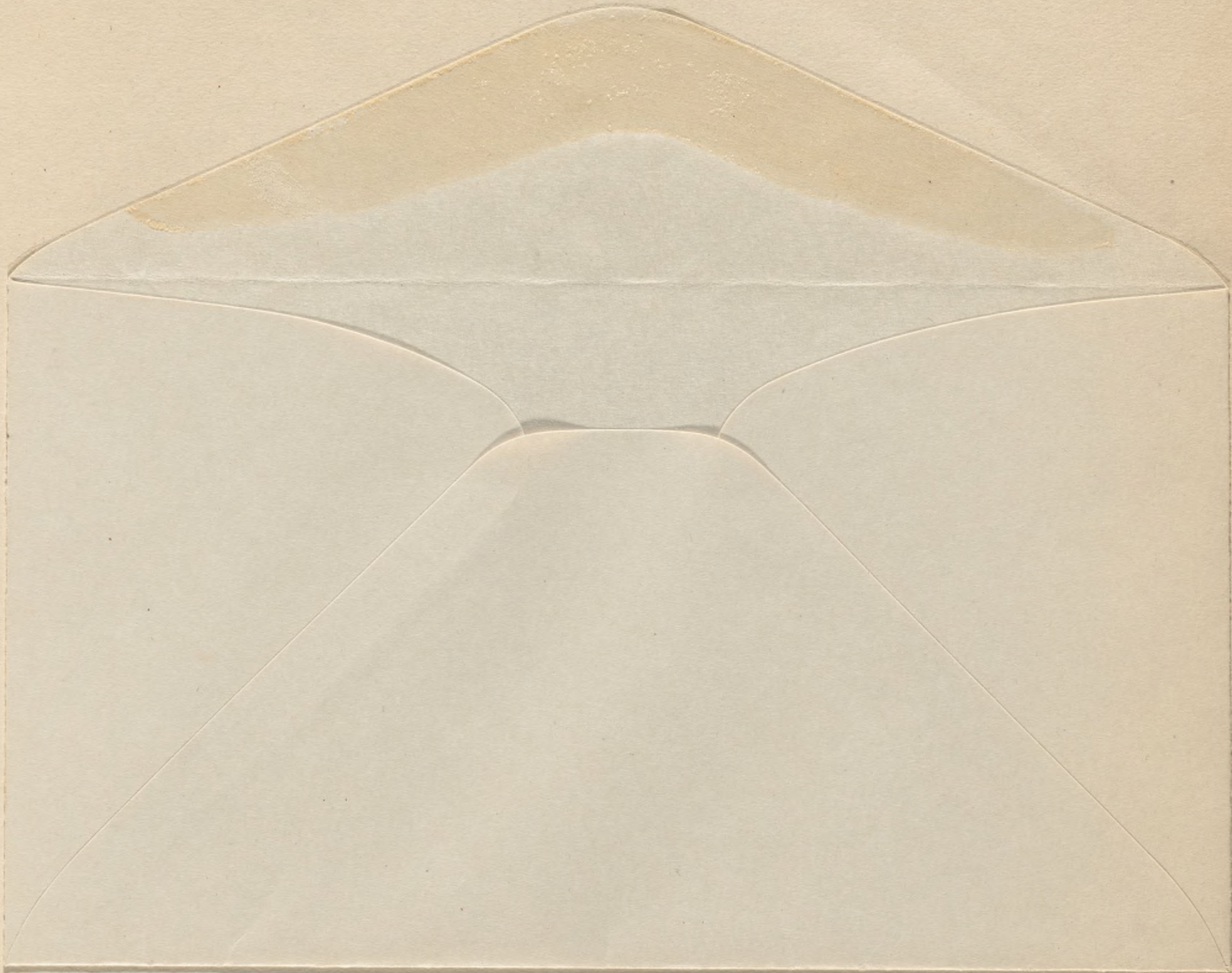
On the general plan, first inaugurated by Professor Miles of the Laboratory, that representatives of the laboratory could secure much more cooperation in studying the results of unpublished foreign work if they brought something with them to the various laboratories, I planned to give lectures as in the past few tours.

Several rather important innovations were introduced on this trip. First, instead of having one lecture I prepared three. Second, while formerly the lecture that I gave consisted exclusively of unpublished material, this year because of the fact that the work on cold-blooded animals had been published only in monograph form and hence was practically unknown to the large body of younger physiologists, it was decided to make the first lecture a semi-popular presentation or digest of the snake book. The second lecture summarized our new, unpublished for the most part, results on comparative physiology with special reference to comparative metabolism, bringing in the discussion of the surface-area law and the differences in metabolism of different species of animals, especially warm-blooded. This lecture is to be the basis of a comprehensive article, discussing the question as a whole, to be published I hope ere long. Since very frequently I would be invited to address various medical societies, hospital centers, etc., the third lecture dealt with human metabolism in the light of most recent investigations, laying emphasis upon its practical application in the hospitals.

These three lectures were carefully written in English and then they were translated into excellent German by Dr. Strieck of Würzburg and into French by Miss Vuilleumier of the Laboratory Staff. Thus I had nine manuscripts, three English, three German, and three French.

To aid in presenting to friends I was about to visit specific details in regard to the lectures a pamphlet was prepared and printed, giving certain fundamental information with regard to the lectures as a whole and giving a table of contents, so to speak, of each lecture. This was sent on, usually several weeks in advance, to the various centers I purposed visiting. This enabled the selection of the lecture by the medical society, etc., and gave a much wider choice than the single lecture I had usually carried with me. (A copy of this program is shown herewith.) The lectures were illustrated by lantern slides, of which I had 75 in all. Those in the snake lecture were obviously copies of material already printed. The other two lectures represented, as usual, for the most part unpublished material.

There was no occasion or no necessity for giving any of these in Rome, for the Congress was devoted almost exclusively to series of short papers.



# Synopses of three lectures available for presentation by

# Dr. Francis G. Benedict

Director of the Nutrition Laboratory of the Carnegie Institution
of Washington, Boston, Massachusetts, U. S. A.

After attending the XIVth International Physiological Congress at Rome, Dr. and Mrs. Francis G. Benedict will start on their triennial tour of European institutions. On this tour, at the time of the visits to various research centers, lectures, if desired, will be given.

These lectures have been translated and will be given in either French or German, in addition to English. Projection slides (standard American size, 101 by 82 millimeters) will be used in all three lectures; in the first lecture, 23 slides; in the second, 13 slides; and in the third, 38 slides. Owing to the liberality of the Carnegie Institution of Washington, Dr. Benedict is privileged to give these lectures at certain university and hospital centers without honorarium or any other expense. The delivery of each lecture occupies approximately one hour or slightly less. Each lecture is independent of the other lectures. In general there will hardly be time for more than one of the lectures in any one institution.

Permanent European address until March 1, 1933

Dr. Francis G. Benedict,
Care Brown, Shipley & Co.,
123 Pall Mall,
London, England.

## The physiology of great tortoises and snakes and its relation to human physiology

Introduction
- Use of studies with cold-blooded animals to illuminate problems in human physiology

Outline of research by Nutrition Laboratory on cold-blooded animals
- Special objects
- Animals studied

Body temperature of snake
- Mouth
- Rectal
  - Influence of handling, agitation, digestion
  - Reaction to intense cold
  - Comparison of rectal and environmental temperatures
- Skin, rectal, and environmental temperatures compared

## *(Continuation of First Synopsis)*

Incubating python
- Historical survey
- Skin temperature compared with environmental temperature

Paths for heat loss by cold-blooded animals
- Water vaporized by the snake
- Direct calorimetric measurements with snakes

Digestion experiments with snakes and tortoises
- Protein, fat, and carbohydrate

Respiratory quotients
- High with tortoises after food
- Low with snakes at low temperatures

Economy in energy expenditure of cold-blooded animals

Metabolism of fasting cold-blooded animals
- Temperature coefficient
    - Rattlesnakes at 15° to 44° C.
- Comparison of cold-blooded animals of different sizes
    - Surface-area measurements
        - Stretching of skin of snake
        - Factor of coiling
        - What is true surface area of tortoise?
    - Per kilogram
    - Per square meter
        - Surface-area conception insufficient
            - Should consider heat *loss* rather than heat *production*

Comparison of cold- and warm-blooded animals at same cell temperature
- Hibernating animals compared with cold-blooded
- Cold-blooded warmed to 37°
    - 37° not a damaging temperature for snake
    - Cold-blooded even at 37° can produce only 1/8th the heat of warm-blooded

Probable explanation of difference in metabolism of cold- and warm-blooded animals
- Distribution of the blood

Intermediary stages between cold- and warm-blooded
- Incubating python
- Tortoise
- Hibernating animal

## Basal metabolism in comparative physiology

Introduction
- Character of Nutrition Laboratory investigations
    - Human and animal metabolism
    - Comparative physiology

Development and simplification of techniques
- Closed and open circuits
- Carpenter gas-analysis apparatus
    - Constancy in composition of outdoor air

Bases of comparing metabolism of different animals
- Per kilogram
- Per square meter
    - Values of surface area constant (K) in Meeh formula $S = K \times w^{2/3}$

Prerequisites for comparable basal metabolism measurements

Mouse, extremely labile metabolism

Rat
- Huddling
- Giant rats—influence of size and of fasting

Small birds—canaries, parrakeets, sparrows

Doves and pigeons
- Influence of temperature, season, growth; wild versus domesticated doves

Normal and Frizzle fowl
- Influence of time of day, temperature, sex, moulting, defective plumage
- Rectal temperatures
- Surface temperatures of head appendages, legs, and feet

Goose

Marmot, awake and during hibernation

Monkey (Macacus Rhesus)

Large domestic animals—steer, cow, horse, sow
- Techniques employed
    - Feces and urine separating device for cows
- Results

Water vapor and insensible perspiration of humans and animals compared

Comparison of basal metabolism of all animals studied by Nutrition Laboratory
- Heat per kilogram
- Heat per square meter

## Human basal metabolism in the light of recent studies

- Introduction
  - Factors affecting basal metabolism
    - Age, weight, height, sex
    - Surface area conception
    - Sleep
    - Race
  - Techniques well established
- Age factor
  - Series of observations on 3 men and 1 woman, over a period of 17 to 24 years
  - Observations on elderly women, 66 to 86 years old
- Racial factor
  - Chinese and Japanese women
  - South Indian women (Tamils and Malayalis)
  - Australian aboriginals (Kokata men and women)
  - Maya, male, Indians in Yucatan (3 expeditions)
  - American-born Chinese girls in Boston
  - Racial factor with pigeons and doves
- Constancy in metabolism from day to day
  - Observations on a man over a 3-week period
- Sleep factor
  - Observations on South Indian women during sleep
  - Observations during "hypnotic sleep"
- Helmet apparatus in its various forms
  - Helmet as a breathing appliance
  - Closed circuit for $O_2$ only
    - Spirometer tracings
  - Closed circuit for both $CO_2$ and $O_2$
    - Rest experiments
    - Work experiments
  - Open circuit, both $CO_2$ and $O_2$
    - Rest experiments
    - Work experiments
- Mental effort experiments
- Muscular work and high oxygen experiments
- Muscular work and alcohol experiments
- Observations in recovery period following muscular work
- Value in the clinic of measurements of
  - Insensible perspiration
  - Basal metabolism; simple helmet-rotamesser apparatus enables rapid measurements of basal metabolism as a hospital routine
  - Temperature of expired air; its potentiality as a clinical measure

## BERLIN, GERMANY.

En route to Stockholm a day was spent in Berlin, in which time opportunity was taken to try to straighten out the problem of securing translations of articles for Pflüger's Archiv in the hands of Fräulein Steuber. On a visit to the laboratory of Professor Mangold I was told that she (Steuber) was away in Switzerland. They told me that she has no assistants working with her, she works alone and nobody knows anything of her goings and comings. I found she was in Davos with Professor Loewy and hence nothing could be done about the translations. I also learned that she had tuberculosis of many years' standing and had to be extremely careful.

Rat treadmill. Inasmuch as we were preparing and had practically finished the articles on rats, I wanted to find the place where I thought the description of the "rat treadmill" I had seen years before in Berlin had been published. (See figure 9.) Letters to Goldberg, manufacturer of the treadmill, had been without result, so I visited Goldberg's establishment and found that he was sick. He had written me on August 10, but I had not yet received the letter. I inferred finally that the apparatus I saw there had not been made there but that he had brought it there to be copied. I got no satisfactory returns from my inquiries and hence it was impossible to cite the apparatus in our current rat publications.

Figure 9 . Berlin, Germany. Rat treadmill manufactured by M. J. Goldberg & Son.

## STOCKHOLM, SWEDEN.

### Carolin Institute, Laboratory of Physiology.

### J. E. Johansson, Professor emeritus, Professor H. V. Gertz, and Dr. E. Abramson.

In the laboratory there were many respiration chambers of various sizes, but the entire laboratory is built around the thesis that the determination of carbon dioxide alone is sufficient. Professor Johansson is strongly obsessed with this idea. There were several respiration chambers, one large enough to hold the Johansson ergostat. He apparently used the chamber as a closed chamber without ventilation. Instead of having a spirometer outside in connection with the chamber, he had inside a large rubber bag which could expand and contract as air was taken into the lungs. (See figure 10.)

In connection with gas analysis I found a clever stopcock device. At the end of the stopcock there had been extended a sort of glass loop with a hole in it. In the loop was a small spiral spring, the other end of which was attached to a metal support. Thus there was almost a spring tension holding the stopcock into the seat and preventing the stopcock dropping out, and thus securing a good seat and closure.

Simple bicycle ergometer. Abramson had a very simple bicycle ergometer with a front handle. The wheel was covered with heavy lead cable wire threaded in and out the spokes to give weight to the rear wheel. There was a prony brake with suitable tension which could be taken up by handles and tension made on a small spring balance such as we would call a "fish scale". It was very cheap and apparently very serviceable. (See figure 11.)

Apparatus for studying the heart impulse. Among the numerous papers and exhibits at Rome, that which greatly attracted attention was the apparatus of Atzler and his associates at Dortmund in which the dielectric capacity of the heart was studied and the amount of blood passing through the heart at its various phases recorded photographically. Abramson had a heart impulse apparatus based upon the fundamental idea of Yandell Henderson. He (Y. Henderson) used a cot bed or bench on which the man lay and each impulse beat of the heart was recorded. This was first, I believe, used about the time of the Pike's Peak expedition.

Abramson used the idea only that the subject was sitting in a chair and the chair is clamped by very stiff flat springs so

Figure 10 . Stockholm, Sweden. Carolin Institute, Laboratory of Physiology. Interior of Johansson respiration chambers, showing in the rear the temperature pipes for cooling and a rubber bag connected to the outside with a hole through the wall, allowing expansion and contraction with each respiration. It is a closed circuit and closed chamber with spirometer.

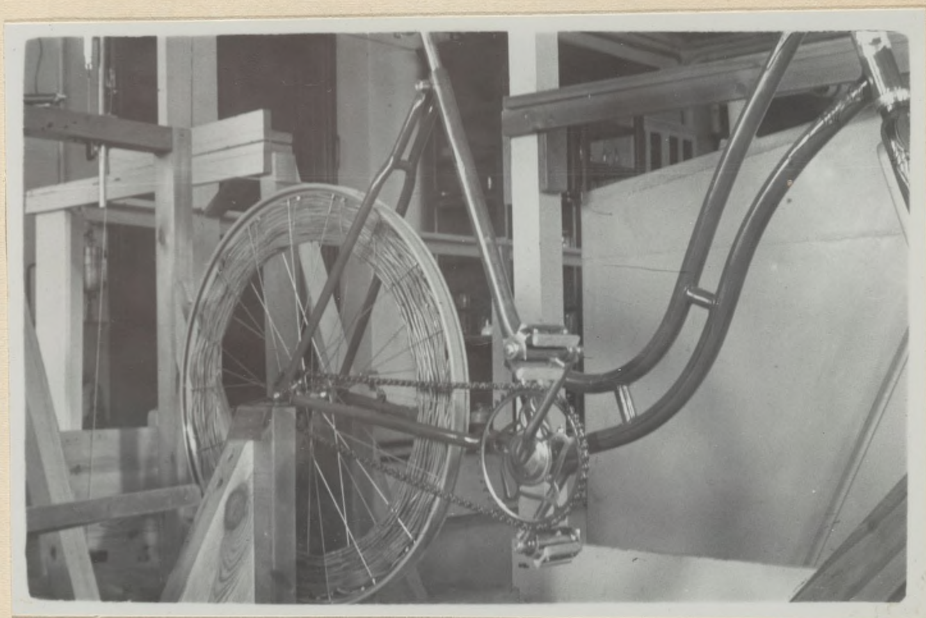


Figure 11 . Stockholm, Sweden. Carolin Institute, Laboratory of Physiology. Bicycle ergometer with front handle, used by Dr. Abramson. Wheel is covered with heavy lead cable wire threaded in and out the spokes to give weight to the rear wheel.

there is almost no give or slack to the springs. The extremely slight moves produced by the heart impulses are magnified by a twist or torsion on a flat ribbon carrying a mirror and reflections from this in turn are photographed. From these photographs he obtains heart curves characteristic for each person after work. The changes in heart rate with inspiration or expiration could be shown. He had a great many curves. From the technical side of the thing it is impracticable to hold the breath during the measurement, for the tension of the muscles as the breath is held gives a confusing picture. He had a number of pictures in which adrenalin was given but he got pronounced tremors in some cases. They had already made considerable application in certain pathological cases in the Serafim Hospital. Judging solely and simply from the form of curves obtained and carrying in mind the form of curves obtained by Atzler, it seemed to me there might be a correlation between Atzler's dielectric heart pictures and Abramson's material. If there was a relationship it would enormously enhance the value of each. Abramson thought he would follow up this matter carefully. His idea is to turn the matter over to the clinic as there is much promise of it in pathological cases. He also felt that it should show the volume output of the heart per beat.

As a result of my visit to Abramson it struck me that there was a possibility of making use perhaps in a minor way of our upright Chatillon balance at the laboratory for studying the heart impulse. We have often noted when standing on this balance the heart impulse. It occurred to me that by extending out through the glass face the rod holding the pointer one could attach a mirror to this and photographically record the heart impulses. It would be perfectly simple to build a chair on the balance. I thought this apparatus could be used in the basement where there would be no tremors. There could be a photograph on a paper about 6 to 8 cm. long for each complete cycle. The problems that occurred to me were (1) Does subject A at rest but after food give the same curve (a) 15 minutes to 15 minutes (b) day to day considering food? (2) How do A, B, and C differ (a) with food, without food? (3) Can we duplicate curves on the same individual at different times or different days? (4) Is there any "over-shooting" in the curves? I would predict, No. Of course all the precautions found necessary by Abramson and Atzler should be followed. It would seem as if this was a simple apparatus that might still furnish some important evidence for the hospital.

As on my former trip I was extremely impressed by Abramson as a serious, clever-minded, mechanically and mathematically inclined man. He was the only man in the Department of Physiology who is now interested in metabolism lines. The successor of Johansson, Professor Gertz, is interested primarily in mathematical optics and has little or no interest in metabolism. Here again we have one of these astonishing illustrations so frequently found in European laboratories where the former professor of physiology has developed a splendid school, and has developed magnificent equipment in the laboratory, and when he retires it seems as if the university authorities went just as far as they could go in the opposite direction to find a man with distinctly different interests to act as his successor. Only too frequently this means that a very expensive and elaborate equipment is left unused or at least later becomes antiquated. When one thinks of what used to be the output of the Tigerstedt-Sondén-Johansson school in which so many contributions to metabolism have been made, it seems sad to find this entire tradition and splendid equipment resting solely for its future in the hands of a young, although very capable, chap with his chief practically without interest in the problem.

Professor Gertz. Although I have never known Professor Gertz intimately I saw more of him on this occasion. I was profoundly impressed by his fortitude throughout the entire time of my stay. Professor Gertz was laboring under the frightful tragedy of the approaching imminent death of his wife from cancer. In fact it was a very great consideration that he appeared at my lecture.

## STOCKHOLM, SWEDEN.

### Carolin Institute, Laboratory of Biochemistry.

### Professor v. Euler.

This laboratory is new, elegantly arranged, and it has apparently the same fine attitude toward biochemistry that one finds in the laboratory of Sørensen; that is, biochemical organic studies can be carried out in a clean, neat laboratory. He is very much interested in problems dealing with vitamin E of Evans, colloid solutions and carotodin, and dealing with what he called chemical genetics. One special problem was the study of chlorophyl versus no chlorophyl. He used corn as a test plant. I was much impressed by the beautiful statuette of Carl Wilhelm Scheele done by a modern sculptor, which stood in the portico of the new laboratory. It was a very fine conception. I tried to get a photograph but was unable to do so.

We visited at the home of Professor v. Euler, where I took a photograph. (See figure 12.)

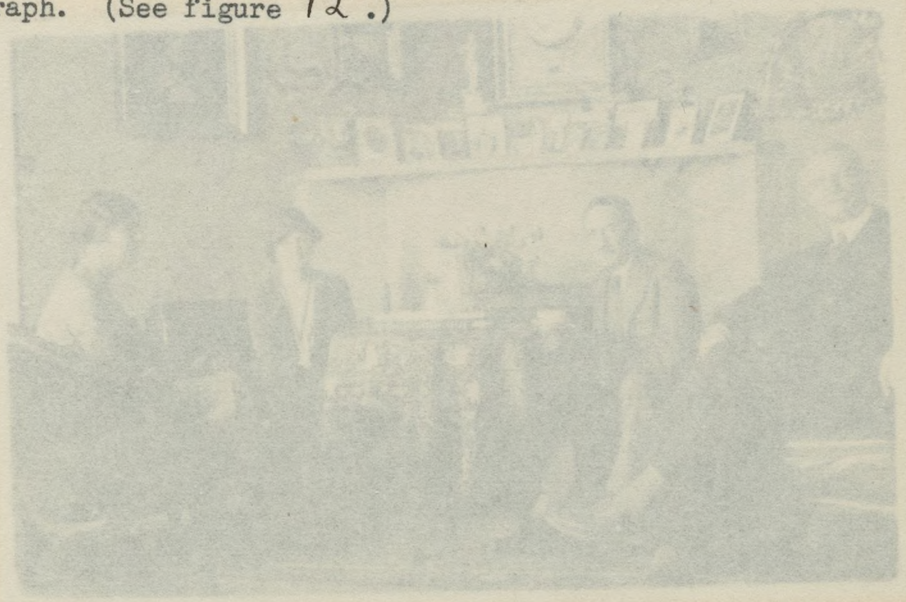


Figure 12. Stockholm, Sweden. Home of Professor H. von Euler. Left to right: Mrs. von Euler, Mrs. Benedict, Professor von Euler, and Dr. Benedict.

Figure 12. Stockholm, Sweden. Home of Professor H. von Euler. Left to right: Mrs. von Euler, Mrs. Benedict, Professor von Euler, and Dr. Benedict.

STOCKHOLM, SWEDEN.

Carolin Institute, Department of Pharmacology.

Professor G. Liljestrand.

Since the retirement of Professor Johansson, my interest in Stockholm has centered in large part about my dear good friend, Professor Liljestrand. The fact that he had been but a relatively short time before in Boston and we had been in intimate touch with him made a visit to his laboratory most important. We were extremely fortunate in being with him a good part of our time in Stockholm. As a member of the Nobel commission he talked rather freely in relation to Rubner and the Nobel prize, stating that Rubner never had a chance at the Nobel prize as his work was old when the Nobel prize was founded and in spite of Lusk's statement in the necrology of Rubner he, Rubner, did not step on Professor "X's" toes. He also said that it was quite impossible for him to agree that Rubner was anything like the greatest man he ever met. He considered that his isodynamic replacement of the nutrients was Rubner's most important work. Liljestrand is as stimulating as ever, with a great variety of interests, with a wide personal acquaintance with European scientists, and one of the finest minds I have ever met. He is at present very much interested in alcohol and the rate of loss by ventilation when the subject breathes 5 per cent carbon dioxide and 95 per cent oxygen. He was interested in the problem as to whether you can sweep out methyl alcohol by overventilation. If so, one may possibly off-set its cumulative tendency.

One problem in connection with the snake investigation was interesting to Liljestrand in that he thought perhaps one should keep cold-blooded animals at a high temperature and see if there was any tendency to develop an increased blood distribution or does it take ages. My feeling is that it would take a very long time. He thinks that the theory that the absence of blood circulation is the difference between cold-blooded and warm-blooded is very plausible. While Liljestrand is primarily a pharmacologist he can select his own problems and probably they are fully as much of a physiological as a pharmacological nature. My own feeling is that Liljestrand is the keenest among men in physiology in Europe. I know of no one who has the breadth of view that he has, with remarkably good powers of assessment and analysis. It is no wonder that I purposely arranged to be in contact with him as much as possible during my stay in Stockholm.

My admiration for Scheele and the fact that I have often stated that he is the patron saint of our laboratory found a ready response in Liljestrand, and he very kindly showed me a recently discovered portrait of Scheele and after I left Stockholm presented a copy of this portrait to the Laboratory. Sometime later I was able to secure from him a little of the history of Scheele.

The statue that I saw in the laboratory of v. Euler was made by a celebrated Swedish artist, Milles. The original is at Scheele's old home, Köping, and it is of full size. The well-known statue of Scheele in Stockholm is always worth while visiting. We have a photograph of it at the Nutrition Laboratory. It is of course idealized and certainly not a portrait, but his indomitable spirit is admirably brought out both in this and in the more modern statue. The Laboratory is especially fortunate in having the newer, apparently authentic copy of the minature of Scheele. The following description is from a letter from Professor Liljestrand dated June 19, 1933.

"The portrait of Scheele that I sent to you is a reproduction of a miniature painting on ivory made during his life-time, probably when Scheele was about 25 years old. It has been in the possession of relatives to Scheele since then; in 1929 a Swedish apothecary, Mr. Gullström, had the opportunity of seeing a reproduction from the original made in 1886. He interested the Swedish Society of Apothecaries, and after a long search he traced the original back to some relative of Scheele living in Berlin, from whom the minature was bought by the society. A detailed description of the hunting for the painting has been given by Gullström (Norges Apotekerforenings Tidsskrift 1931/1932) but only in Swedish. .....

"The modern statue of Scheele that you are referring to was made by Milles - who is now in U.S.A. - for the city of Köping, where Scheele lived for a long time and ultimately died. There is a reproduction of it in the notes on Scheele to the Physiological Congress - I enclose a copy - and another one in the little pamphlet on Milles which I am sending to you. As you will find from p. 49 the original statue is about life-size or a little more. The replica you saw at Euler's laboratory is 50 cm. high and costs 900 Sw. crowns. It is the only size obtainable and can be had from Bergmans Konstgjuteri AB., Roslagsgatan 31, Stockholm."

## STOCKHOLM, SWEDEN.

### Carolin Institute, Laboratory of Histology.

### Professor G. P. E. Häggqvist.

In Professor Häggqvist's laboratory they were working on temperatures in the liver and in the peritoneum. They had thermal junctions placed in the body of the living, quiet rabbit and they wrote curves 24 hours long. The temperature in the liver was always about a half a degree higher than that in the peritoneum. They had given adrenalin, insulin, and other materials. The thing seemed to me very ingenious, but only just started, and what will be the outcome I do not know. (See figures 13, 14, and 15.)

Figure 13 . Stockholm, Sweden. Madame Häggqvist and Professor Häggqvist in their library.

Figure 13 . Stockholm, Sweden. Madame Häggqvist and Professor Häggqvist in their library.

Figure 14 . Stockholm, Sweden. Left to right: Mrs. Häggqvist, Mrs. Benedict, and Dr. Benedict.

Figure 15 . Stockholm, Sweden. Left to right: Mrs. Häggqvist, Mrs. Benedict, and Professor Häggqvist.

## STOCKHOLM, SWEDEN.

### Serafim Lazarett, Clinic of Professor H. C. Jacobaeus.

### Dr. Nylin.

Dr. Nylin was much interested in the work and output of the heart, using a Krogh apparatus for oxygen, and then he got the curves one minute after work to six minutes after work and then rested four minutes. The kind of work that he did seemed to me rather unfortunate. He had a semi-circular staircase. The patient walked up six or seven steps and then down. (See figure 16-a.) This gave a very irregular rhythm to the metabolism, I thought, and not suitable for his particular problem. This same thing I have seen in other places. Nylin impressed me as an interesting and capable man, much interested in pathological problems.

Figure 16a. Stockholm, Sweden. Serafim Lazarett, Laboratory of Dr. Nylin. This photograph shows the semi-circular staircase with the exercising steps up and down, also a Krogh spirometer and bed for rest experiments.

Figure 16a. Stockholm, Sweden. Serafim Lazarett, Laboratory of Dr. Nylin. This photograph shows the semi-circular staircase with the exercising steps up and down, also a Krogh spirometer and bed for rest experiments.

## STOCKHOLM, SWEDEN.

### Veterinary High School.

### Professor A. W. Sahlstedt.

I had heard from Professor Liljestrand about Professor Sahlstedt and I was very anxious to see his laboratory. It was set up with a good many very ingenious features, and one of them was, for example, that all of the stopcocks on the desk were below the desk so it would not be possible to brush open a stopcock when passing by or hit them with bottles, etc. There was an especially ingenious method for handling lecture charts and hanging them. There was a special non-spattering water faucet.

He had also a scheme for counterpoising the Krogh spirometer bell at all positions with a back angle clamp to hold his counterpoise.

To prevent dogs which had had operations such as fistulas from lapping their wounds or pulling off bandages, etc., Sahlstedt had each dog wear a very large collar. This collar was made of thin compo-board and might be two or three feet in diameter. In this way it would prevent the dog from getting his head to any parts of his body and yet at the same time allow freedom of the head without muzzling. This same neck collar I saw subsequently in the laboratory of Karl Thomas in Leipzig.

They apparently had done some work on the inspired air and expired air of a horse and showed me a mask with two holes for the thermometers, wet and dry bulb. There were a number of masks and connections for getting the temperature of ingoing and outgoing air. No metabolism was done at all but one found traces of researches on the temperature of expired air and the humidity carried out in connection with Liljestrand. Altogether it was a very interesting laboratory to see, chiefly from the standpoint of equipment. Sahlstedt is now dean of the Veterinary High School and hence too occupied with administration to do much in research.

I took two photographs in Sahlstedt's laboratory. (See figures 16-b and 16-c.)

Figure 16-b. Stockholm, Sweden. Veterinary High School. Photograph of Professor A. W. Sahlstedt, showing his method of preventing spatter of a vacuum pump inserted in the top of a tube.

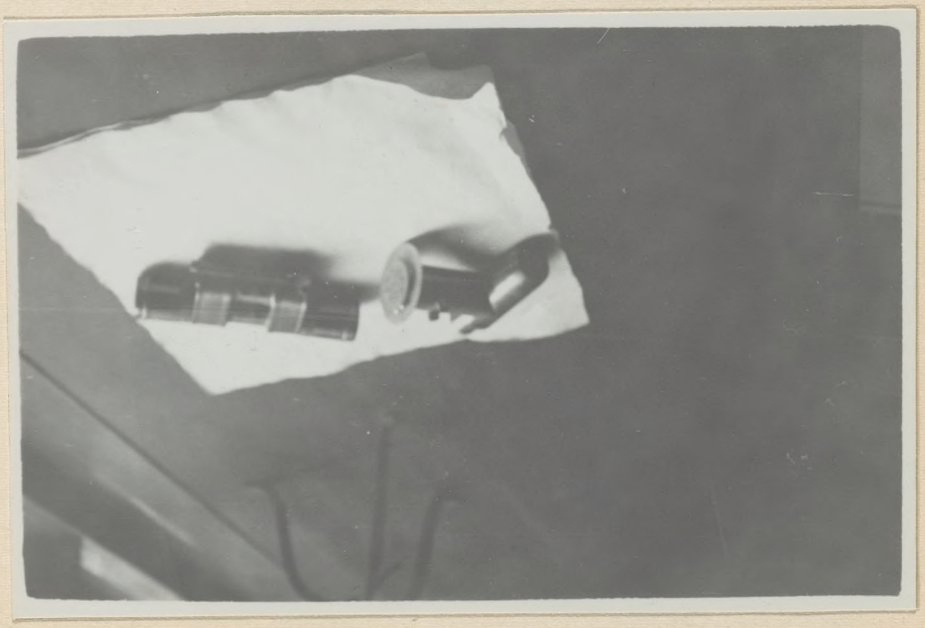


Figure 16-c. Stockholm, Sweden. Veterinary High School, Professor A. W. Sahlstedt. Inspiratory and expiratory valves in Sahlstedt's laboratory.

## Lectures in Stockholm.

The first lecture was given September 13 in the lecture hall of the Serafim Hospital under the auspices of Professor Holmgren, in the morning, to an audience of circa 150. I gave them the "third" lecture. Both in his introduction and in his concluding remarks Professor Holmgren emphasized the importance of such lectures in cementing international relationships. The second lecture was given on September 16, in the evening, in the lecture room of the Physiological Laboratory, and was, I think, before the Stockholm Biological Society. I gave the snake lecture. There was a very good audience and very considerable interest in the material. (There was an announcement of this lecture.) There was also an account of my visit published in the newspaper, "Dagens Nyhetter", for September 16, 1932, and the clipping is appended here. See page 44. In connection with the Holmgren Clinic lecture I profited by Professor Liljestrand's comments, who suggested that I cut out the details of the rotamesser apparatus as he found it very difficult to follow. Dr. Berglund who was present suggested that I cut out the insensible perspiration, but later considered I should cut out the details of the open versus the closed circuit, with the simple statement that it could be used. These comments were very helpful and suggested very wise practical cuts which were made.

Of special interest to me was the discussion of Zotterman and Liljestrand, commenting upon the enormous "oxygen debt" of the snake. It would appear as if someone simply must do the lactic acid in the snake.

Among those attending the lectures was Professor Rehberg of Copenhagen, who wanted for Copenhagen the lecture he heard at the Holmgren Clinic. He is apparently arranging the lectures at Copenhagen as Krogh is away. Abelin of Berne was also at the lecture in Stockholm.

## Fysiologföreningen

sammanträder å Karolinska institutets fysiologiska institution fredagen 16 septemberm1932. kl. 19:30.

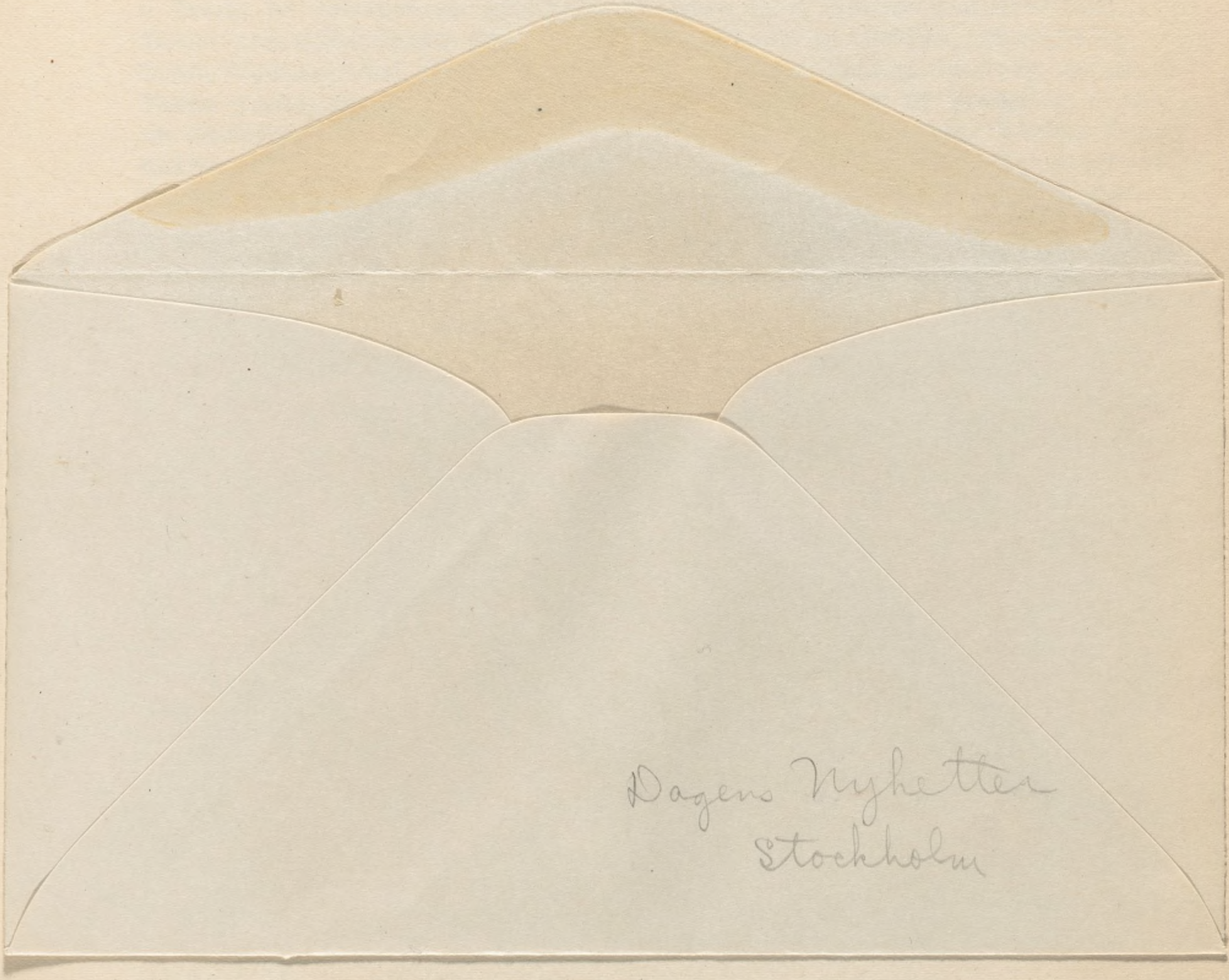
Program:

Dr. F.G.Benedict: The physiology of great tortoises and snakes and its relation to human physiology.

Ev. smärre meddelanden.

Nästa sammanträde förväntas äga rum i början av oktober. De som då önska lämna något meddelande torde benäget före den 30 september härom underrätta sekr. (adr. Karolinska institutet K.33818).

Sekr.


Dagens Nyheter
Stockholm

DAGENS NYHETER.

Fredagen den 16 September 1932

# Amerikansk fysiolog på europeisk turné, prisar svenska maten.

## SJUNDE GÅNGEN I STOCKHOLM

*Professor Francis G. Benedict med sin närmaste medarbetare, mrs Benedict.*

Den stockholmska vetenskapsvärlden har för närvarande besök av en världsberömd amerikansk forskare, professor Francis G. Benedict, vilken är här för att för sina svenska kolleger framlägga resultatet av sina forskningar på det näringsfysiologiska området under de tre senaste åren.

— Det är sjunde gången jag besöker Stockholm, och om inte kriget kommit emellan skulle det ha varit den nionde, säger professor Benedict, då Dagens Nyheter får en kort intervju med honom på Grand Hôtel. Tack vare Carnegieinstitutets generositet kan jag få göra en tur till Europa vart tredje år. Jag började 1907, var här 1910 och 1913, kom så fjärde gången 1923 och börjar faktiskt känna mig som stockholmare snart.

Professor Benedict har skapat sig ett namn över hela världen genom sina undersökningar över ämnesomsättningen hos normala människor, och med ledning av sitt stora material har han kunnat upprätta tabeller över ”normalmänniskans” basala ämnesomsättning, vilken är beroende av individens kroppsvikt, längd, ålder och kön och är ett uttryck för kroppens värmeproduktion i vila efter tolv timmars fasta.

— Carnegiestiftelsen är sålunda hygglig nog att låta mig få göra dessa europeiska rundresor utan att det behöver kosta vederbörande åhörare av mina föreläsningar någonting, fortsätter professor Benedict. Jag höll en föreläsning i förrgår, och i morgon håller jag ännu en. Efter Stockholm kommer turen till Lund, Köpenhamn, England och Skottland. Så går färden tillbaka till Paris och kontinenten i övrigt. Den 1 mars nästa år vänder jag efter sex månaders Europavistelse tillbaka till Amerika och mina forskningar i Boston. Det kan ju tyckas

Forts. å sidan sjutton.



## Amerikansk fysiolog på europeisk turné.

Forts. från sidan 1.

att jag slösar bort en massa tid på dessa resor, tid som jag skulle kunna använda bättre för mitt arbete i laboratoriet. Allt vad jag nu far omkring och föreläser om är visserligen sådant som inte är publicerat förut, men man skulle kunna trycka föreläsningarna, och ett sådant arrangemang skulle kanske till andra fördelar också bli billigare.

Det där resonemanget vill jag dock inte skriva under på. Det är inte bara för att framlägga forskningsresultaten som jag flackar omkring, utan samtidigt får jag tillfälle att återställa internationella förbindelser på det vetenskapliga området, särskilt naturligtvis mellan biologer i olika länder. På det kommersiella området är konkurrens nyttig, men när det gäller vetenskapen måste vi vara överens och samarbeta i stället för att söka slå ut varandra.

Detaljer från sina forskningar är professor Benedict inte beredd på att framlägga för en större allmänhet då det är svårt att göra det i tillräckligt populär form. Han framhåller dock att vad han kallar ”overhead cost”, d. v. s. vad individen genomsnittligt konsumerar, är mycket olika för olika raser. Medan denna ”overhead cost” är mycket låg i Indien, Kina och Japan, är den mycket hög hos t. ex. det primitiva mayafolket i Yucatan.

Professorn framhåller hur förspänt vi ha det i Sverge med vårt fiske och vår stora spannmålsodling. I det sammanhanget passar han också på att ge oss en eloge för vår matlagning och förklarar att ett sådant hotell som Grand i Stockholm, det finns inte i hela världen, inte ens i New York. En lunch på verandan utåt Strömmen är någonting som man inte kan få i någon annan av de städer mr Benedict besökt, och de äro många.

— Jag förstår att åtminstone de flesta som komma till Stockholm resa härifrån igen med saknad, och vi göra det kanske mer än de flesta, slutar mr Benedict. Men jag lever på hoppet att få komma hit om tre år igen för att hälsa på mina vänner bland edra vetenskapsmän. Och jag kan inte underlåta att säga att den starkaste magneten bland dem är professor Liljestrand, kanske den märkligaste vetenskapsman jag lärt känna under mina resor och i mitt arbete.

Swedish physiology and the outlet for young Swedish physiologists.

I was impressed on this trip as never before with the extraordinarily clever group of young Swedish physiologists. When one considers men like Zotterman, Abramson, young v. Euler, young Sonden, Malmros, Odin, and really a large group of others like them, it is a pathetic fact that there is no adequate scientific outlet for them. International feeling is such that it is impossible for a Swede to receive an appointment even in a Danish institution. There are only three physiological centers in Sweden, Stockholm, Lund, Upsala, and the number of posts to be filled is very small. These young men have, therefore, the option only of dragging along as assistant or instructor in a lyceum or high school, hoping for a university position, or forsaking research and going into practice for personal gain. I think it is certain that there are a larger proportion of unusually keen biological men in Scandanavia than in any other country.

It seemed to me it would have been better if Nobel, instead of founding his prize, had founded four or five great institutions of research with men of first calibre as directors but with a compensation that would allow this large group of young men scientific outlet for their energies and ingenuity. That at the same time would be a great benefit to science as a whole and enable them to have a compensation or livelihood. Although Sweden and Denmark are much better off economically than other countries, the fact that the positions are so few and their men so unusually brilliant makes an unfortunate combination.

LUND, SWEDEN.

University of Lund.

Physiological Institute.

Professor Torald Thunberg and Dr. J. Lehmann.

In Lund we were fortunate in being at the home of Professor Torald Thunberg (see figure 17), whose multiplicity of interests stamps him again as a man of foremost intelligence and capacity. Most of his laboratory was devoted as previously to methylene blue measurements of oxidative activity, but I was especially interested in the great barospirator and particularly their efforts to use it as an artificial respiration apparatus for cases of respiratory paralysis.

My earlier reports have shown photographs in detail of the instrument, one for man and one for very small animals, but the use at this time of a rubber diaphragm over the chest with the head and arms outside to produce normal respiration, and the use of the great pump was a new feature. Thunberg's assistant, Dr. Sahlin, stated that all chests varying in circumference from 120 cm. to 63 cm. could be fitted with one size rubber packing; larger or smaller chests must have other sizes. He maintained that leaks played a very small role, since the pump of the barospirator was of such enormous capacity. This is a different application of the instrument than the original Thunberg scheme in which the alternate production of vacuum and pressure was supposed to force air into the passive lungs. A young assistant was working on this newer modification with a rubber diaphragm over the end or door of the barospirator and with pneumograph tracings showing apparently a very definite lung ventilation without difficulty (see figures 18, 19, 20, 21, 22). Of course the apparatus appeared to me extremely cumbersome and noisy when compared to the more or less modern forms of respirators devised by Drinker and Emerson, but it was a usage of an apparatus already existing, i.e., the Barospirator. They had reports of clinical cases that had been greatly helped by it, but I imagine there was nothing new to be added to the extensive clinical experience in America.

There was much use of the Enghoff valves, also the Loven valves and mouthpieces.

Dr. Jorgen Lehmann. Dr. Lehmann is working both at the Thunberg laboratory and in the hospital where Professor Petrén formerly was. They were all rather excited over a new blood sugar method that Lehmann had developed, using resorcin. Briefly, Lehmann precipitates the protein in blood with 5 per cent tri-chlor acetic acid.

Figure 17. Lund, Sweden. The family Thunberg. Sitting, from left to right: Mrs. Thunberg, Mrs. Benedict; standing, Professor Thunberg, his daughter, and Dr. Benedict.

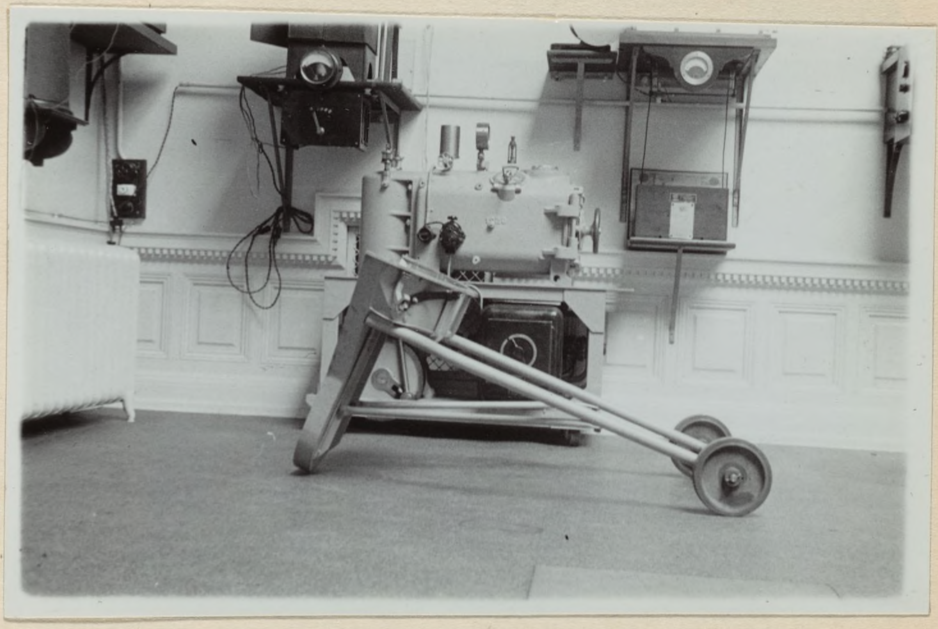


Figure 18 . Lund, Sweden. University of Lund, Physiological Institute, Laboratory of Professor Thunberg. In the rear of the photograph, in white, is a small barospirator for rabbits and small animals. The truck at the front is for attaching the special openings for chest breathing to the large barospirator, which is not shown but is at the immediate right.

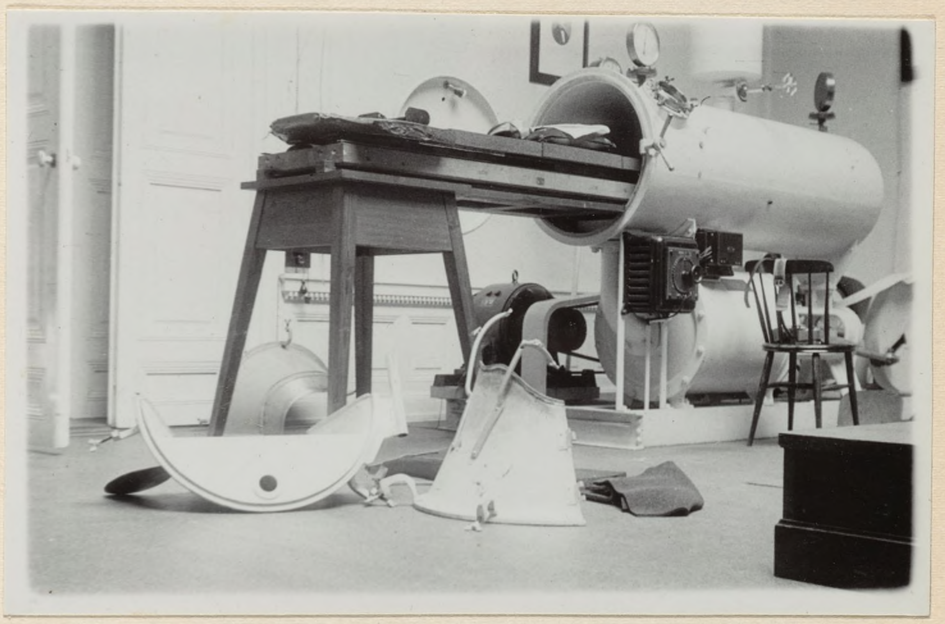


Figure 19 . Lund, Sweden. University of Lund, Physiological Institute, Laboratory of Professor Thunberg. Barospirator showing neck closure, bed pulled out, and on the floor attachments for the chest when the apparatus is to be used as an artificial respiration apparatus.

Figure 20. Lund, Sweden. University of Lund, Physiological Institute, Laboratory of Professor Thunberg. Use of the barospirator for respiration movements. A special door at the entrance end of the barospirator is provided with a rubber collar going about the chest of the patient just below the arm-pits. This is made tight by partial inflation and the respiratory movements written on the kymograph at the left. In the rear is standing one of Thunberg's assistants.

Figure 21 . Lund, Sweden. University of Lund, Physiological Institute, Laboratory of Professor Thunberg. View of Professor Thunberg's barospirator.

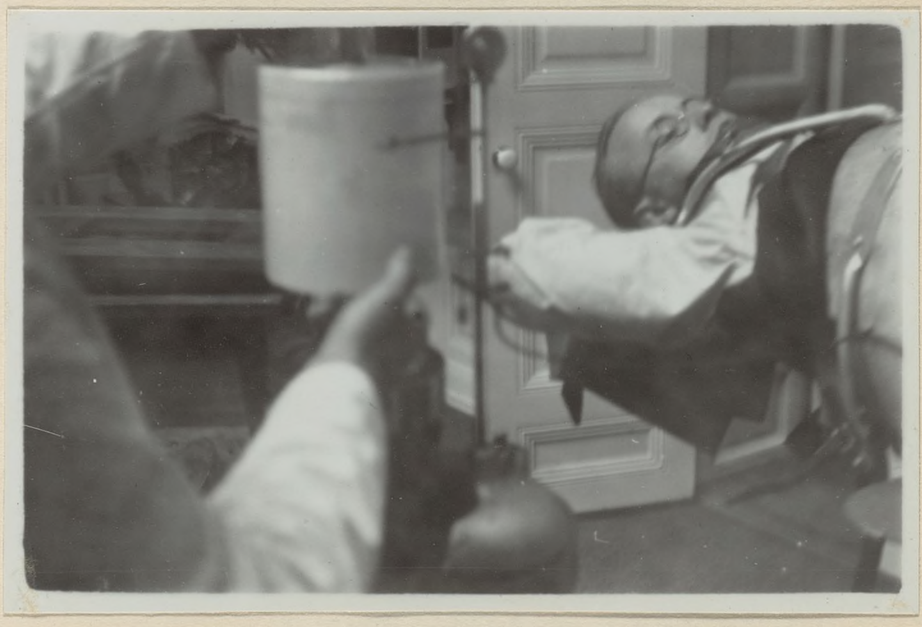


Figure 22. Lund, Sweden. University of Lund, Physiological Institute, Laboratory of Professor Thunberg. Another view of Professor Thunberg's barospirator.

This was done in a very small amount of blood in the cold without standing and without heating. It was then filtered through asbestos into a brown glass test tube containing 1 c.c. of a solution of resorcin, the pipette to be rinsed twice with the solution. He added 4 c.c. of concentrated sulphuric acid of a special purity and on shaking the solution it got very hot, up to 110° C. Four minutes after this it was cooled under the tap. By the colorimetric method the sugar was determined. The apparatus seems to be very simple and accurate, and especially gave the differences between sugar and other reducing substances. It had been used that summer in a clinic at Ramlosa and they were very enthusiastic about it. Realizing this was one of the important problems at Durham, i.e., the determination of the blood sugar of cows especially in the period following feeding, and since this was prior to my visit to Aberdeen (see Aberdeen) I ordered an equipment of this apparatus for the Nutrition Laboratory.

The questionable feature seemed to be the matter of the purity of the sulphuric acid. Lehmann had had a lot of trouble with sulphuric acid and only with certain lots could he get satisfactory results. He expected to have the apparatus entirely completed and ready to deliver to me on the boat in February. As time went on I heard nothing from him and in February (1933) he felt obliged to write me that he was not satisfied with the apparatus and was not ready to put one out as yet, so this whole thing fell to the ground.

## LUND, SWEDEN.

### University of Lund, Department of Pharmacology.

### Professor G. Ahlgren.

Although Professor Ahlgren's work (pharmacology) is entirely outside of the Nutrition Laboratory's problems, I have personally for many years hoped to spend a day with him and to visit his laboratory. The laboratory was in the same building with Professor Widmark, the floor above, as a matter of fact. Badly planned by his predecessor, Dr. Overton, Ahlgren with his dynamic personality and enthusiasm is trying to get it into condition for practical use. Overton's ideas were very curious and the laboratory was very improperly equipped. Unfortunately Overton had "turned back" a lot of money, stating that they "needed no more equipment." Hence this made it difficult for Ahlgren to convince the authorities that more money was needed.

Ahlgren is a splendid "live wire", and it is a real inspiration to be with him. (See figures 23, 24, 25, and 26.)

Figure 23. Lund, Sweden. Left to right: Mrs. Benedict, Mrs. Ahlgren, and Dr. Benedict.

Figure 24. Lund, Sweden. Left to right: Mrs. Benedict, Mrs. Ahlgren, and Dr. Benedict.

Figure 25. Lund, Sweden. Left to right: Professor Ahlgren, Mrs. Benedict, and Mrs. Ahlgren.

Figure 26 . Lund, Sweden. Left to right: Mrs. Ahlgren, Mrs. Benedict, and Professor Ahlgren, at luncheon.

## LUND, SWEDEN.

### University of Lund, Medical Clinic.

### Dr. H. Malmros.

In the hospital which was formerly directed by Professor Petrén things for the time being were under the charge of Dr. Malmros, while Professor Petrén's successor was away doing some writing. There was an interesting oxygen chamber in which they used a Cambridge Instrument Company's apparatus for determining the carbon dioxide and the oxygen. The chamber has thus far been used mostly for 5 per cent carbon dioxide and 95 per cent oxygen or 5 per cent carbon dioxide and 95 per cent air. They had not made very much progress with it.

Malmros has a Krogh apparatus and also some of the steps or circular staircase for muscular work such as I saw at Nylin's in Stockholm, but only half of it. Apparently it was to be used for muscular work but the man using it, Gibson (?), seemed to me very poorly suited for this kind of work. Malmros had had bad luck comparing the Krogh apparatus with the open circuit. On the other hand, they compared the gas meter and spirometer for volumes and the volumes agreed well.

The saddest feature of the Lund visit was missing the optimistic personality of our dear, good friend, Professor Petrén. It is perfectly clear that his successor (name I didn't hear) can in no way compare to him and that the clinic has undergone a very great loss. On the other hand, the clinic appears to be carried on with the same degree of precision and excellent organization that he left it. In spite of the northern latitude it has the appearance of one of the cheeriest clinics and hospitals that one has ever seen.

As before, I felt impressed by the sincerity of purpose of Dr. Malmros. I feel that if he could have had a few more years with a master such as Petrén it would have been greatly to his profit, but he apparently is carrying out his work with great success. Being a non-medical observer I properly can not pass upon his work, but he was holding up his own by carrying out the Petrén tradition to the best of his capacity. Certainly he makes a splendid impression upon anyone visiting Lund.

In front of the Medical Clinic at Lund is a bust of Professor Petrén, of which I took a photograph. (See figure 27 .)

Figure 27. Lund, Sweden. Bust of Professor Karl Petrén in front of the Medical Clinic.

## LUND, SWEDEN.

### University of Lund, Department of Biochemistry.

### Professor E. M. Widmark.

This laboratory has been very active under its dynamic leader for a number of years, and we were fortunate in finding Professor Widmark busily engaged in studies of alcohol on body tissues, especially alcohol in urine and blood in connection with police regulation of automobile accidents. He had worked out a technique whereby each local, provincial physician could draw a sample of blood, and with preservatives, etc., send it to Widmark, who was the head of the central station, and immediately the alcohol percentage was determined and this finding played a role in the police courts.

Professor Widmark showed us a micro-burette for titration, in which a screw plunger in the small syringe played an important role, and the volume discharged was read on the plunger and dial handle of the plunger. I imagine it was somewhat like the micro-pipette in the laboratory of Krogh. (See figures 28 and 29.)

### Lecture.

No lecture was arranged for in Lund. Not a word was said about it, although we were showered with courtesies. I do not know why they did not want one. No one had heard the Stockholm lectures to report they were bad. Another unsolved mystery!

A newspaper clipping from the Lunds Dagblad, of September 21, regarding my visit in Lund, is appended herewith. (See page 62.)

Figure 28. Lund, Sweden. University of Lund, Department of Biochemistry. Professor E. M. Widmark in his laboratory.

Figure 29 . Lund, Sweden. University of Lund, Department of Biochemistry. Professor E. M. Widmark and his micro-pipette.

en

lördag,

är!

# "Bättre ...

dag för

som hobby.

— Det är nionde gången jag är i Sverige, så ni kan väl förstå, att jag tycker om att komma hit, säger en känd amerikansk fysiolog, som under ett par dagar gästar Lund. Det är professor Francis G. Benedict från Boston, som låter hejda sig ett par minuter, innan han skall in på middag hos professor Thunberg. Professor Benedict är anställd som institutsföreståndare vid Carnegie-stiftelsen, och det är på denna stiftelses uppdrag han reser så ofta till Europa för att uppehålla den vetenskapliga kontakten och återknyta förbindelser, som av ett eller annat skäl slappnat. En sådan resa omfattar 3 år och sträcker sig över så gott som alla europeiska länder.

— I Lund har jag varit fyra gånger förut, fortsätter professorn. Jag är synnerligen intresserad av edra institutioner här. Framförallt är det professorerna Thunbergs, Widmarks och Ahlgrens forskningar, som draga mig till sig och lämna mig det största utbytet. Jag har ju också flera personliga vänner i Lund. Så länge Karl Petrén levde, sökte jag alltid upp honom. Vi voro synnerligen goda vänner, och vi gjorde också en resa tillsammans i Jämtland.

Jag har i dag varit ute på en biltur i ert vackra landskap. Det är verkligen underbart. I morgon far jag vidare till Köpenhamn, men inte förrän i mars kan jag anträda återfärden till Boston.

Vad som intresserar mig? Trollkonster bl. a.

Den något förvånade intervjuaren får ett kort i handen, som upplyser att professor Benedict är medlem av »Society of American Magicians», och »magicians» det är just vad trollkonstnärer betyder.

— Ja, det är min lilla hobby. Jag är själv en ganska god trollkonstnär, om jag får säga det själv.

Nu kommer professor Thunberg till hjälp och säger, att professor Benedict även har musik till hobby.

— Men fysiologien då?

— Ah, har ni glömt den, fortsätter professor Thunberg. Ja, då kan jag förråda, att professor Benedict är en av de förnämsta forskarna på ämnesomsättningens område. De undersökningar han utfört under sin kvartssekellånga vetenskapliga bana ha varit epokgörande, och på de fysiologiska institutionerna runt om i världen användas de standardvärden, som professor Benedict kommit till. Hans undersökningar ha sträckt sig över alla åldrar och raser och vidare genom hela djurserien, så den fysiologiska forskningen har honom att tacka för mycket, oavsett nu den gärning han utför som förmedlare mellan olika länders vetenskapliga institutioner.

LUNDS DAGBLAD
Onsdagen den 21 september 1932 158:e årg.

## *Intervju i förbifarten.*

**Amerikansk fysiolog med trollkonster som hobby.**

— Det är nionde gången jag är i Sverige, så ni kan väl förstå, att jag tycker om att komma hit, säger en känd amerikansk fysiolog, som under ett par dagar gästar Lund. Det är professor Francis G. Benedict från Boston, som låter hejda sig ett par minuter, innan han skall in på middag hos professor Thunberg. Professor Benedict är anställd som institutsföreståndare vid Carnegie-stiftelsen, och det är på denna stiftelses uppdrag han reser så ofta till Europa för att uppehålla den vetenskapliga kontakten och återknyta förbindelser, som av ett eller annat skäl slappnat. En sådan resa omfattar 3 år och sträcker sig över så gott som alla europeiska länder.

— I Lund har jag varit fyra gånger förut, fortsätter professorn. Jag är synnerligen intresserad av edra institutioner här. Framförallt är det professorerna Thunbergs, Widmarks och Ahlgrens forskningar, som draga mig till sig och lämna mig det största utbytet. Jag har ju också flera personliga vänner i Lund. Så länge Karl Petrén levde, sökte jag alltid upp honom. Vi voro synnerligen goda vänner, och vi gjorde också en resa tillsammans i Jämtland.

Jag har i dag varit ute på en biltur i ert vackra landskap. Det är verkligen underbart. I morgon far jag vidare till Köpenhamn, men inte förrän i mars kan jag anträda återfärden till Boston.

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## SWEDEN.

### General comments.

One can not help but feel very deeply impressed by the high scientific accuracy and general intelligent level of <u>all the Swedish workers</u>. I am surprised that more American students do not go to Sweden, where it strikes me that the men are all of an unusually high order. They do go in large numbers to Copenhagen to Professor Krogh, who attracts them by virtue of his extraordinary skill, but there are in Sweden men likewise of very great capacity and marvelous technique whom American students might well visit.

## COPENHAGEN, DENMARK.

### University of Copenhagen, Laboratory of Zoophysiology.

### Professor August Krogh, Dr. J. Lindhard, and Mr. Christensen.

The laboratory as usual is extremely active along many lines. One of the first things Krogh showed us was a micro combustion method for carbon and hydrogen employing a closed circuit and spirometer, a quartz tube, copper and asbestos, that is, a very thin layer of copper on asbestos. When he gets through there is no unreduced copper on the asbestos. It is of special value in determining small amounts of soluble carbon in solution. Thus in sea water he could get the plankton- and diatom-free water and thus get the soluble carbon. For example, one of the most interesting studies was that in which the head and gills of an eel were perfused. It called for an extremely complicated technique and it was beyond me to comprehend the significance of it.

Krogh, in commenting upon the Rome exhibit, mentioned the pipette of Marchelli which permitted very fine bubbles to come through, a thing to which I called Dr. Carpenter's attention and one or two of which he brought to Boston. Krogh maintains that this scheme is no good. I told him I thought that the sub-division of the bubbles would make up for the speed, but he thought it was not to be compared at all to the slow process of large bubbles along a Pettenkofer tube.

I saw an extremely interesting small rotary blower being used in the closed circuit, which was made at the laboratory. The packing about the shaft made it tight. I did not get any specifications with regard to it. Krogh agreed also that the Bohr meter was very good but altogether too expensive.

Krogh gas-analysis apparatus. I was interested to find this apparatus at last functioning. (See figures 30 and 31.) Four years ago Parsons, an Englishman, was playing with it, but Krogh said that Parsons was no good. It is functioning now and they use it every day in connection with muscular work experiments, of which I shall write more later on.

Although there were a number of other experiments going on that were not in connection with our work, the most important thing from the standpoint of our work was in the basement.

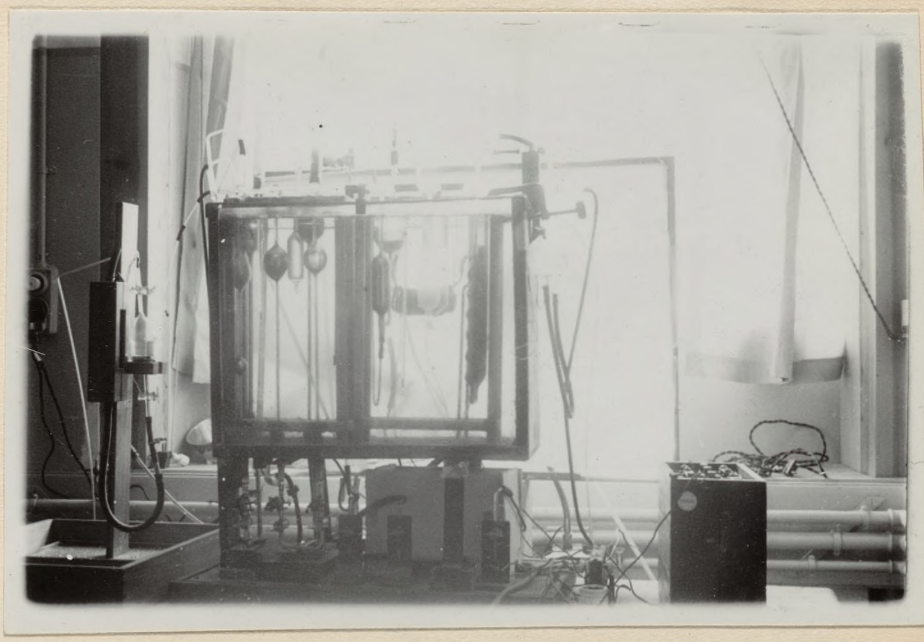

Figure 30 . Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Details of Krogh's gas-analysis apparatus used at Copenhagen.

Figure 31 . Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Part of Krogh gas-analysis apparatus.

Muscular work experiment. I was extremely interested to find a muscular work experiment going on in the basement, in which a helmet was employed. (See figure 32.) Krogh used, as was commonly used in Europe, the wet gas meter and employed it as a pump or aspirator. There was a 15-minute preliminary period and ventilation was about 20 liters per minute. That was so as to have about 1 per cent carbon dioxide coming out. For sampling the air he used the old "Eskimo device" that he and his wife described many years ago, that is, the mercury dropping into the bottle under constant head. ~~(See figure .)~~ There were 10-minute periods to collect samples.

The Eskimo apparatus for sampling is apparently very satisfactory, maintaining the usual constant level of mercury. I feel this should be used more generally. If we at the Nutrition Laboratory were not continually inclined to get away from mercury and glass, I should strongly advocate its use. It may still be just the thing for sheep experiments, and indeed all experiments where periods longer than 10 minutes are needed. For 10-minute experiments the best thing is the Fox bag. At the present time we are using the Fox bag for half of the time, and mercury for the other half. If one is going in for half-hour periods or more and wants to have a more perfect aliquoting I think the Krogh apparatus is the proper thing.

The Krogh helmet was constructed of pieces of copper at top and bottom, the helmet having a diameter of 37 cm. There was a celluloid strip going clear around the helmet, fastening together the upper and lower copper portions. At the rear the entire height over all was 20 cm., the celluloid strip being 12 cm. wide. In the front or face portion the height over all was 45 cm. and the celluloid portion 21 cm. (See figures 33, 34, 35, 36, 37, and 38.) The neck hole undistended was 8 cm. in diameter and the rubber was bolted on with a large number of bolts. In the upper part there was a very flat fan actuated by a small motor outside, to provide stirring up of air inside the helmet itself. The whole thing seemed very cumbersome, and I thought the helmet was very large for rest experiments, but since he was pushing the work up to 2700 c.c. of oxygen per minute, almost double what we have done thus far, it is possible that this type of construction was essential.

The device as it was arranged by Krogh was as follows. First, incoming air came from out-of-doors and was cooled by a Ford radiator with tap water running through it. At this time of year this water was very cold, 10° C. The air after leaving the radiator, which was encased in a wooden box, passed through an ingoing pipe (wrapped in cotton batting to keep the temperature low) into the helmet. Then there was an air pipe from the helmet, 6 cm. in diameter, leading to a wet gas meter in an air-tight box. The Eskimo apparatus for sampling was placed just before the wet meter. (See figures 39, 40, and 41.)

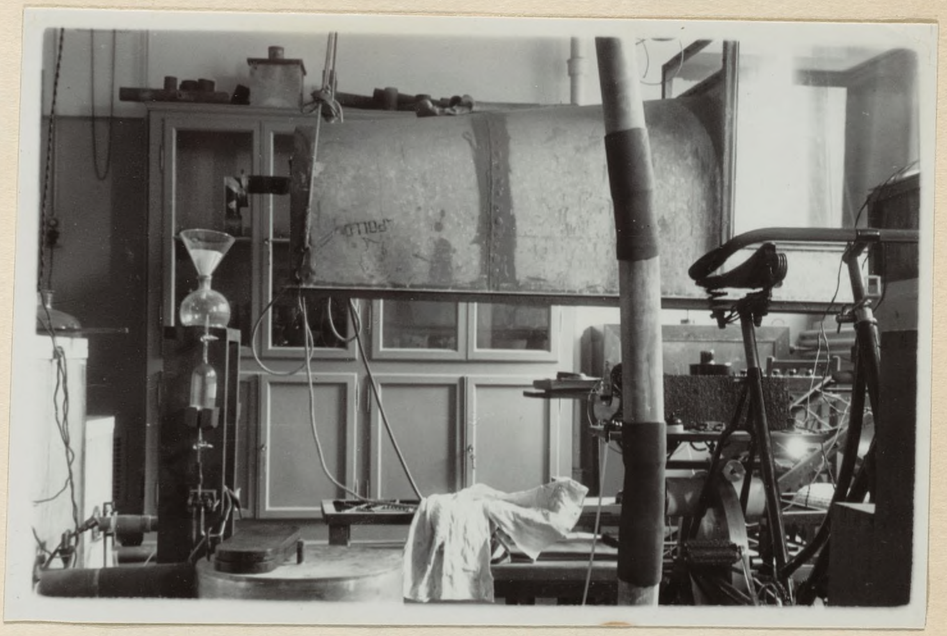

Figure 32. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Krogh's laboratory for muscular work. Bicycle ergometer at right; outgoing air tube with two large rubber connections vertically in the immediate foreground. Cot chamber with cover raised in the rear and a part of the Eskimo gas sampling device at the left.

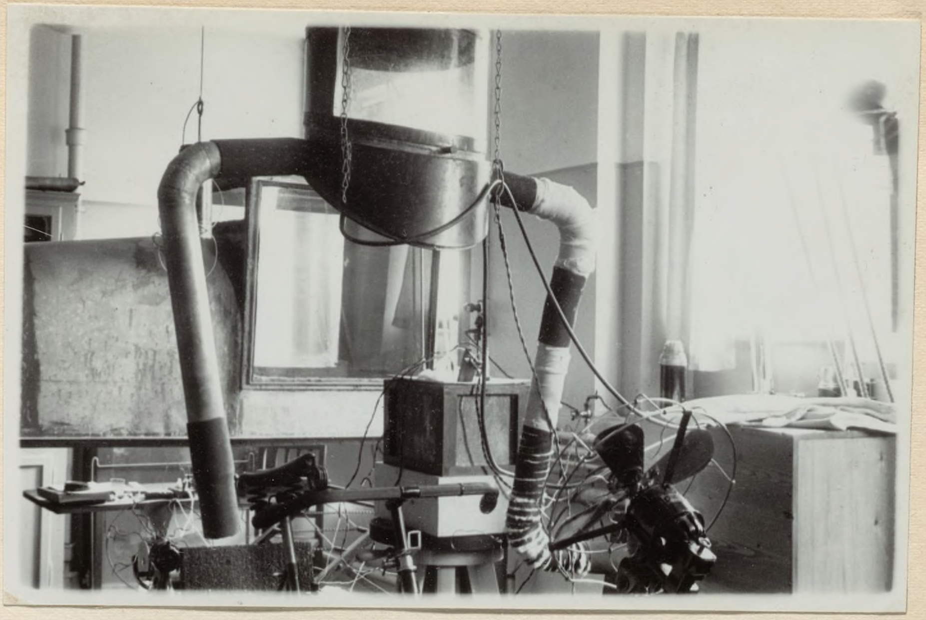

Figure 33. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Front view of Krogh helmet showing large angle of vision through the celluloid, the ingoing air pipe wrapped with cloth to keep the air cool. In the rear one sees a part of the cover of a cot respiration chamber.

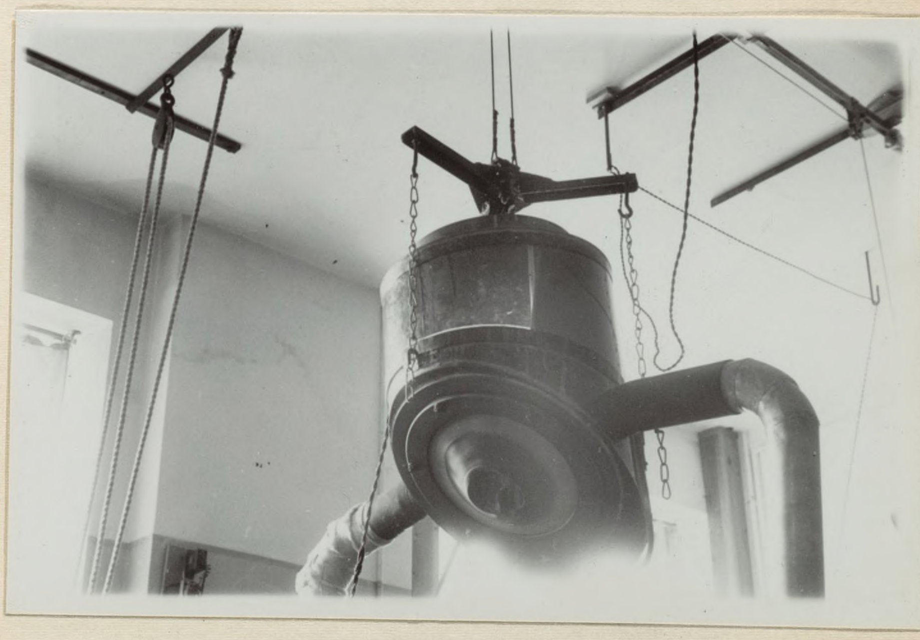

Figure 34. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Rear view of Krogh's helmet showing celluloid band clear around it and method of suspending from wall. The incline shows the base with rubber collar and very large air pipes leading to and out of it.

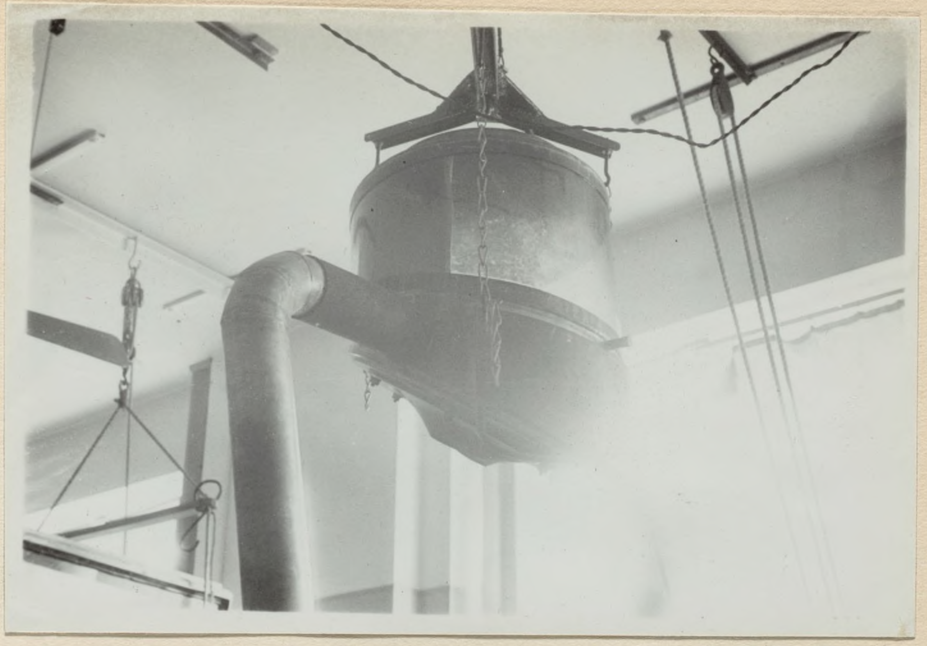

Figure 35. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. View of Krogh helmet suspended in the air, showing outgoing air pipe.

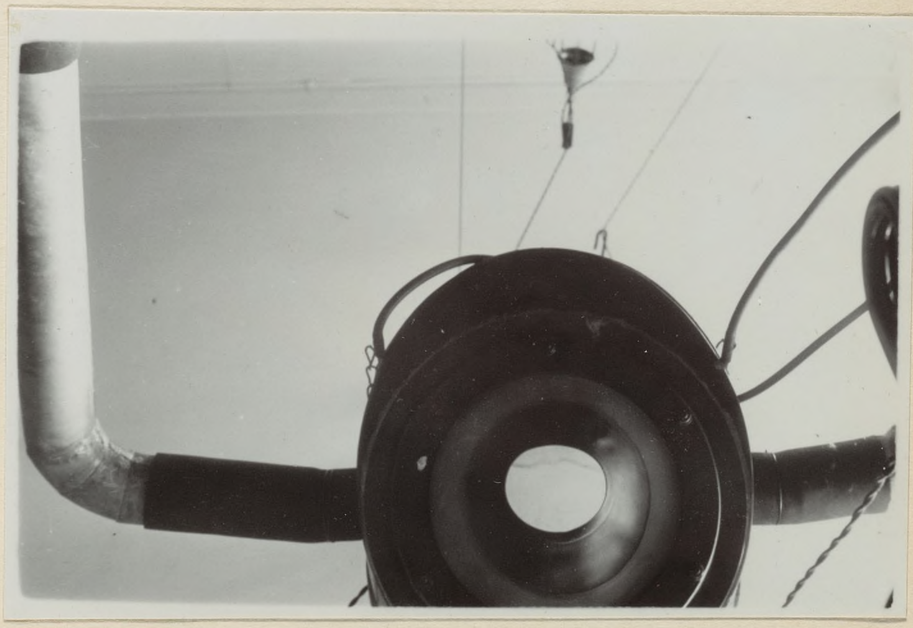

Figure 36. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. View looking up at the suspended Krogh helmet. Ingoing air in direction of arrow.

Figure 37 . Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Suspension of helmet or mouthpiece, showing small tracks from ceiling used in Krogh's suspension of the helmet.

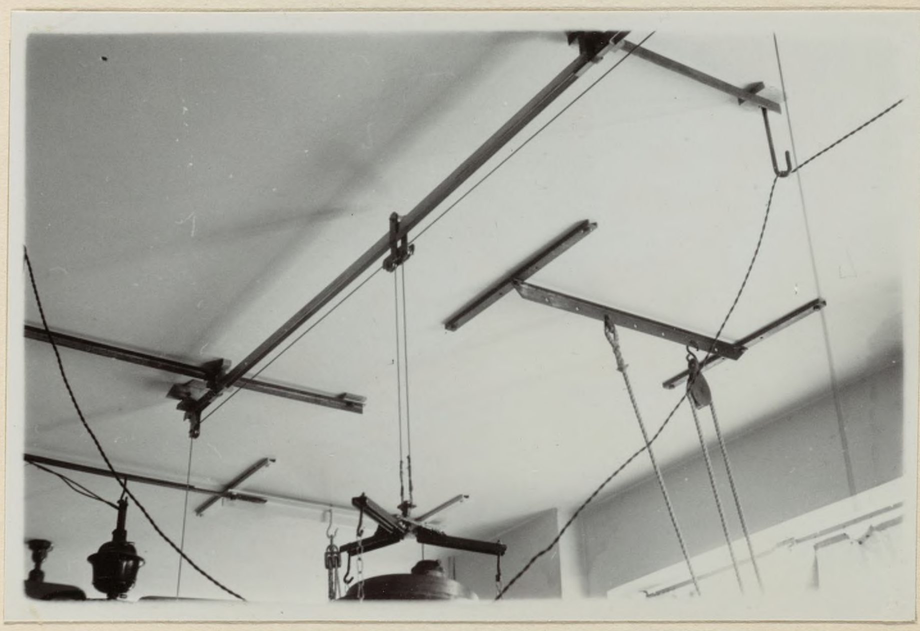

Figure 38 . Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Ceiling network of trolleys and tracks to suspend helmet and move it to any desired position in Krogh's laboratory.

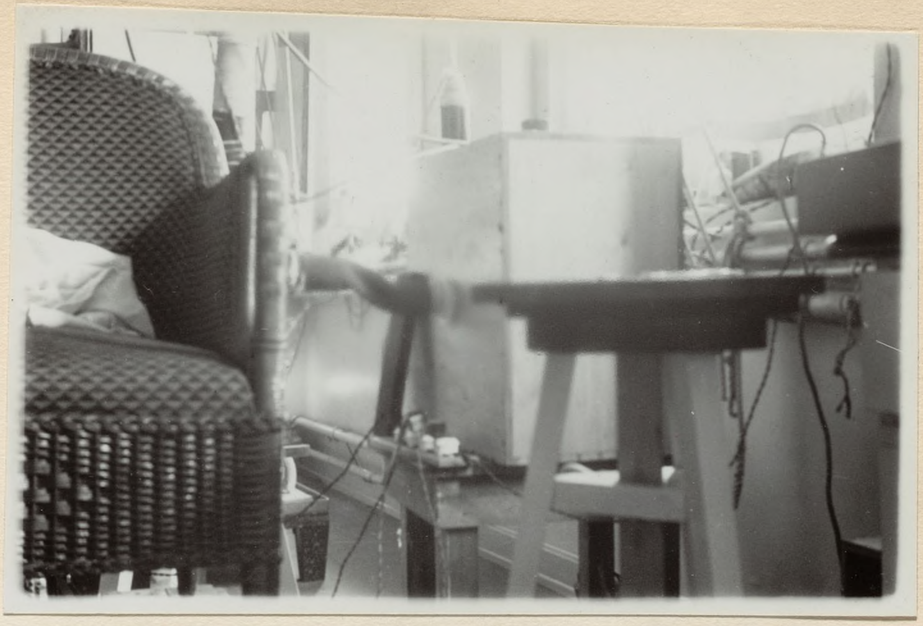

Figure 39. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Krogh apparatus. Chair for subject to be seated in while studying repose. In the center rear, box containing Ford radiator for cooling air.

Figure 40. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Krogh apparatus. Chair used by subject while resting before riding. In the background one sees the seat of the bicycle ergometer, and at the left the cover of a cot calorimeter; in the foreground at the lower left, the air pipe with the mercury sampler for the Eskimo sampling device.

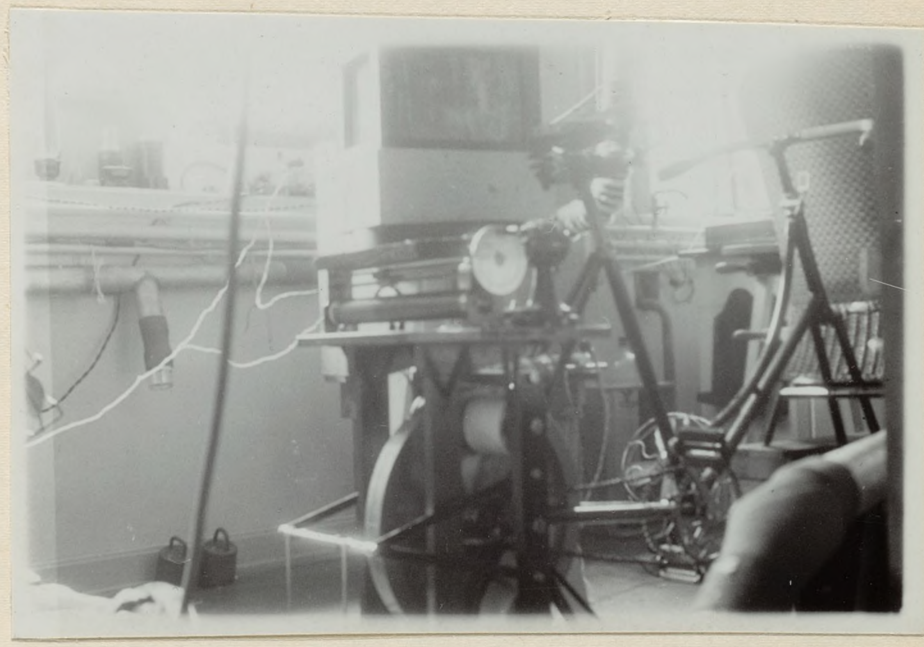

Figure 41. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Closer view of Krogh's ergometer from the rear. Dimly in the background, center, one sees the housing about the Ford radiator used to cool the ingoing air.

He counts the number of revolutions of the meter, which can not be read except in complete revolutions. Each revolution is marked on a drum. The mercury in the sampler is started when there is a "click" for a complete revolution of the drum. There are about 74 seconds between these revolutions. A special device was arranged to keep the water level in the meter (see figures 42 and 43), and the meter was calibrated with a large spirometer. The air was passed over a wet and dry bulb thermometer but the temperatures were alike, so could be considered saturated. The electric motor to run the gas meter was controlled with an old phonograph 3-ball governor from the extension of the armature shaft to make and break a contact and hold constant speed. This is connected with a worm gear and pulley to the axis or drum shaft. An electric contact on the extension of the shaft of the drum made a contact for each revolution of the drum and marked it on a kymograph. To adjust various speeds there were three pulleys on the meter shaft, the largest pulley being 40 cm., the next 29 cm., and the smallest 14 cm. in diameter.

At the beginning of the experiment the large meter was started and then the helmet was put on. There was an extensive use of rubber tubing. Personally I objected to this because there were altogether too many kinks in the rubber tubing. Rubber tubing should be reinforced with wire spirals.

After the real period begins the subject rides about five minutes; then the helmet is off and blood samples are taken. ~~(See figures - , , and .)~~

The subject rides about 60 revolutions per minute. Each work period for measuring the total metabolism, that is, for measurement of the gases, is from 6 to 8 minutes long. The ventilation is kept constant, for they use a meter as a pump and have an electrical control on the meter. The mercury in the Krogh sampling apparatus flows constantly so that the sample is truly proportional. Although the helmet is removed after each period the subject keeps riding at a remarkably constant tempo.

In the control period (i.e., second period) of the experiment the second period is 6 minutes instead of 12 minutes, which is the length of the first period. By having a larger capillary on the Krogh apparatus he has the mercury drop out twice as fast but the analyses agree well; that is, he uses no <u>double</u> samples but one sample of each work period, the second being taken twice as fast as the first, but usually both agree well for both carbon dioxide and oxygen.

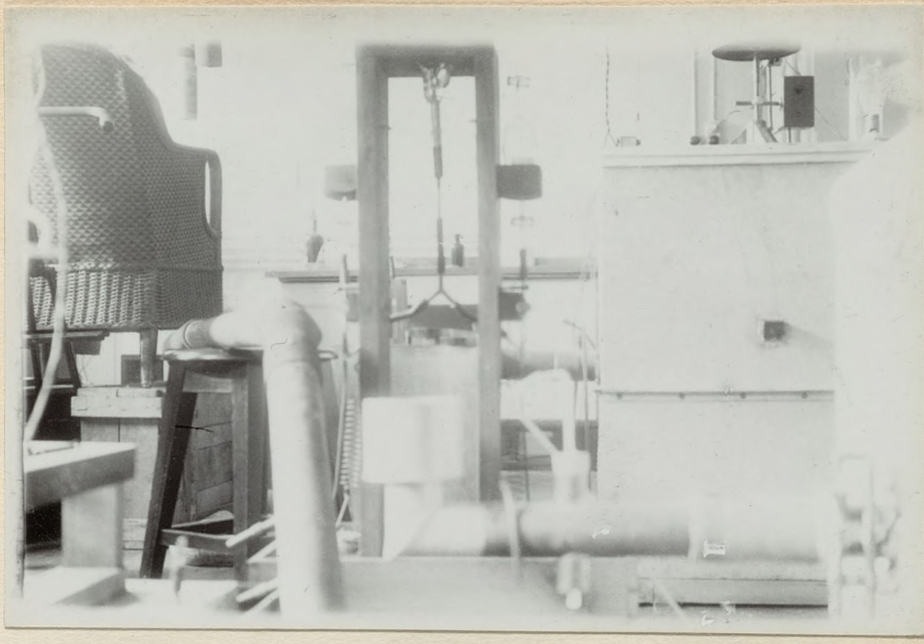

Figure 42. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. View of Krogh's respiration apparatus, showing large housing for gas meter at the right with little black level index showing that the water is at a constant level. Chair for subject to sit in when resting in front of handle bar of bicycle ergometer.

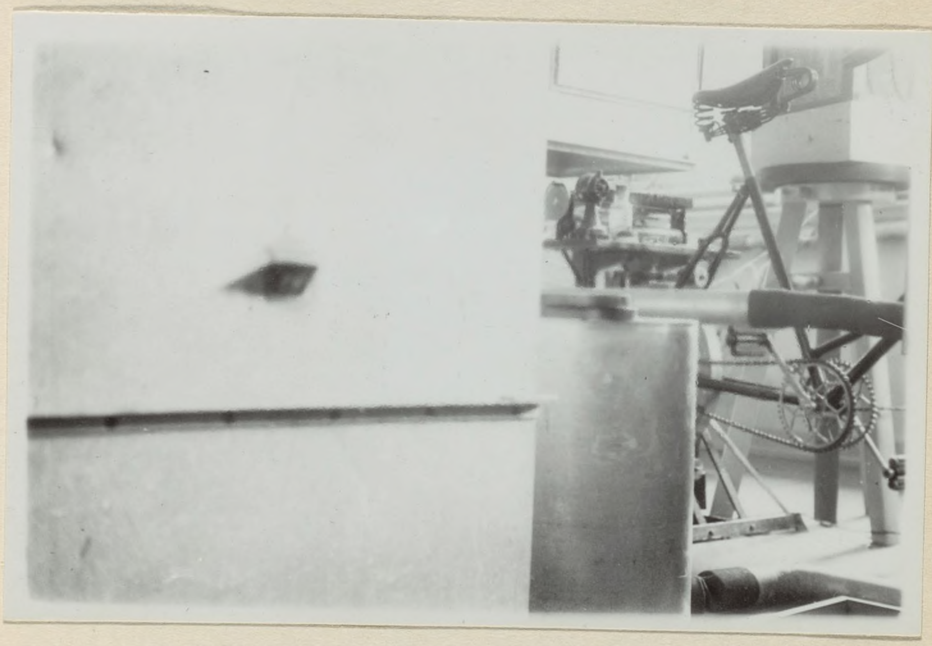

Figure 43. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Photograph shows Krogh ergometer at the right. At the left is the box housing one of the large gas meters, showing the constant level device on the side of the box.

On this particular day they were having about 300 liters per minute ventilation of the helmet with carbon dioxide of about 0.7. In the calculation of results Krogh uses not an <u>average</u> carbon dioxide and oxygen of outdoor air but, he says, that which is actually determined on these days and finds changes in the third decimal place only, but does not use 0.032 and 20.940 as constant. The pulse is taken by placing the finger on the carotid and they count for a fraction of a minute with a watch, reading the revolutions per 15 seconds. They find the pulse very, very constant all during riding and indeed from day to day. They also take it before the respiration experiments with the helmet off.

The subject was able to talk regularly in spite of the heavy work and there was no excessive panting but mouth breathing and sweat on the forehead. In fact, I noticed mouth breathing after the subject had been riding about five minutes. The control of the blood carbon dioxide was taken so as to control the respiratory quotient and this showed little or no "aspumpung".

For the rectal temperatures there was a thermo-junction, small with wires going out backward from buttocks instead of from the front. (See figure 44.) The constant temperature junction was held at 30° and they used a Hartmann and Braun voltmeter, each millimeter corresponding to 0.3°. They felt that reading this needle with a mirror back of it as with the ordinary voltmeter one could read it to 0.1 mm., which should correspond to 0.03°.

The subject wore only running trunks with the torso and legs bare. Two electric fans played over the chest and back all the time. (See figures 45, 46, 47, 48, and 49.) On this particular day something was wrong with the rectal thermometer. They seemed to think it was due to insulating, and as they had drawn two wires through a rubber tube the insulating was probably injured. Their rubber tube was a little smaller than ours but not much.

Along with the metabolism measurements there were many other observations made on this subject, a most intelligent man, who was one of the collaborators in the research and likewise acted as subject. Before knowing that this subject was one of the collaborators I put him down as an extremely intelligent subject, as he seemed to be running the whole show himself. Blood samples were taken by pricking the finger and squeezing it very hard. The hand was placed in warm water previously to have a greater flow of blood. There were also samples taken for fat soluble acids. Everything was on the micro basis. To take the sample a small celluloid tube 4 cm. long and 2 to 3 mm. in diameter with a flare at the lower end was attached by collodion to the finger prick. The blood was thus given a minimum exposure to air and then the blood was syringed out of this celluloid tube or little standpipe. They first tried taking it over paraffin but it did not go well. I wondered why they did not take blood from a vein as Dr. Root does. As it is now they make many tests on the finger by squeezing it. They claim that acid goes up at the beginning of work but there is no control on the corpuscle content. I wonder

Figure 44 . Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Krogh's subject on bicycle ergometer, stripped to the waist. Thermometer leads for rectal temperature seen coming out of trousers. Electrical method for controlling ergometer load at the lower left-hand corner.

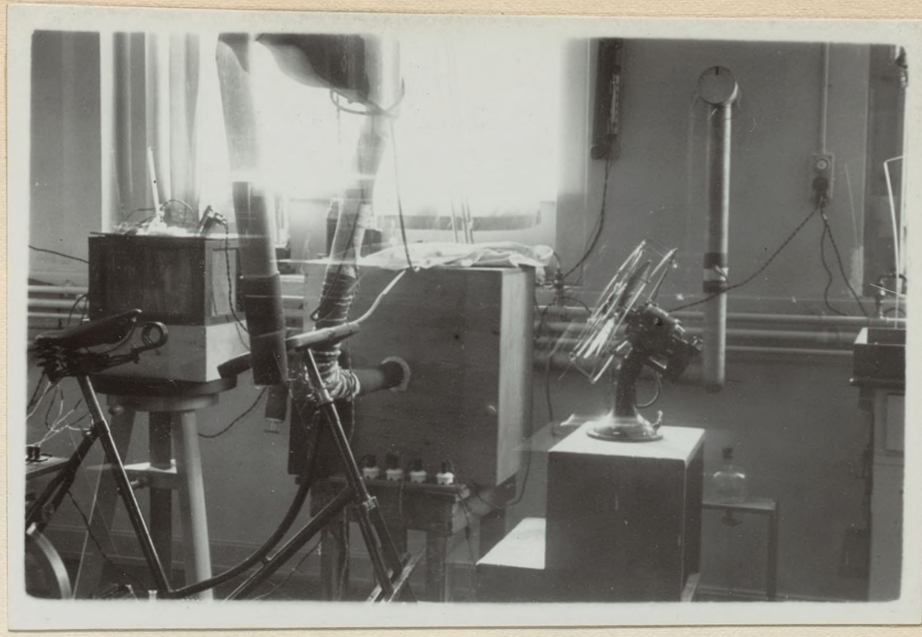

Figure 45. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Krogh bicycle ergometer with lower edge of helmet showing and electric fan in front to blow on subject's chest. Box for housing Ford radiator for cooling air is seen in center rear.

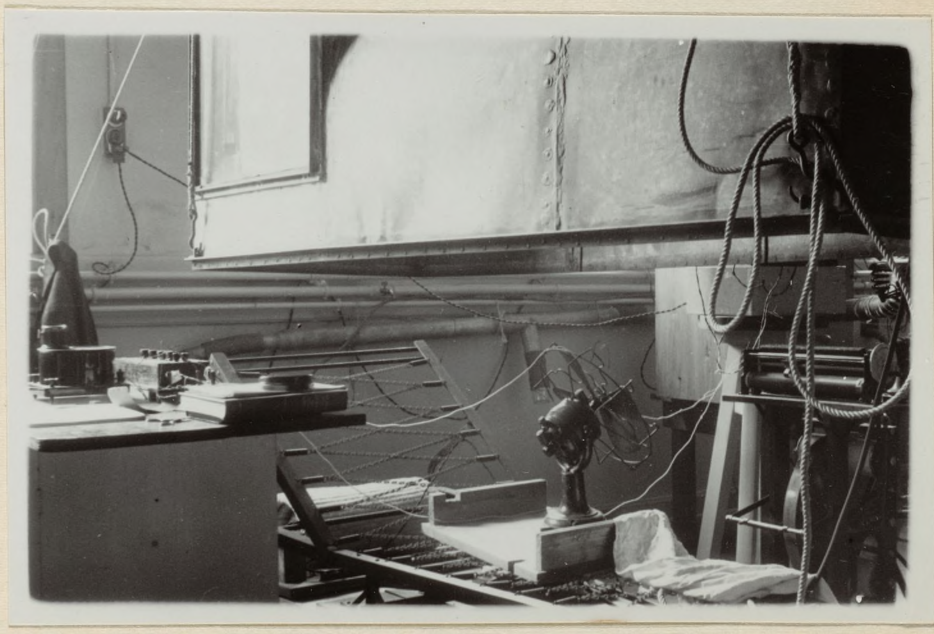

Figure 46. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Bottom of the cot chamber showing bed with head elevated and upper part of the chamber raised in air. An electric fan with blade inclined to the upper right blows air on subject when riding ergometer, the rear wheel of which is seen at the lower right.

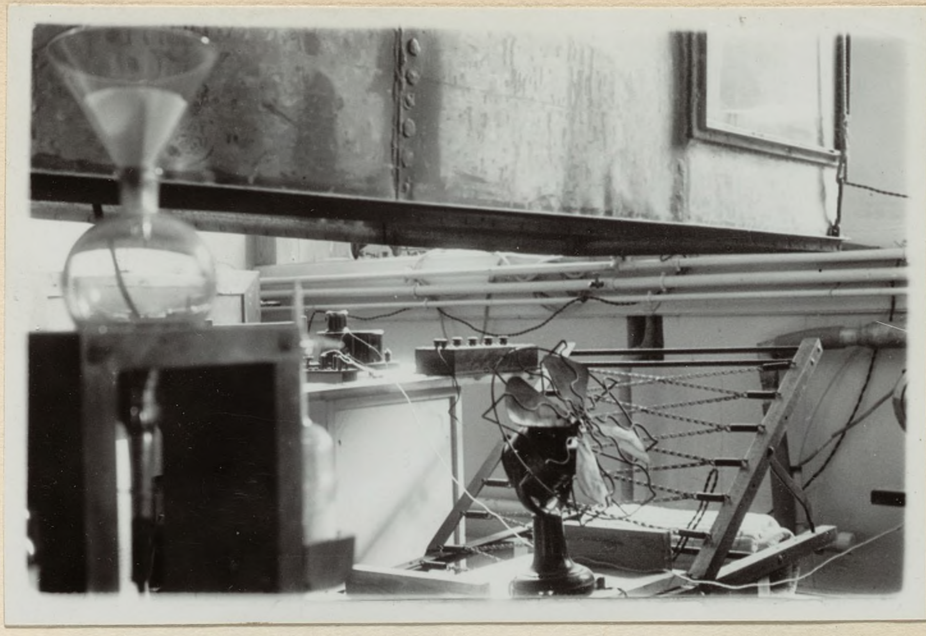

Figure 47. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Closer view of Krogh chamber, showing inclined head of bed, also fan turned up to blow on subject.

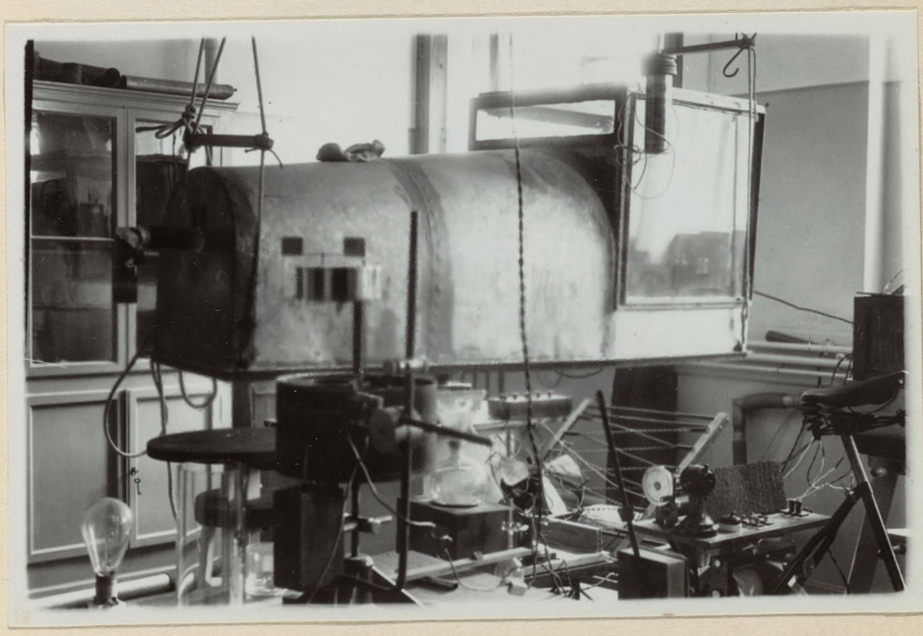

Figure 48. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. View from side of Krogh cot chamber showing cover raised. Seat of ergometer at extreme lower right.

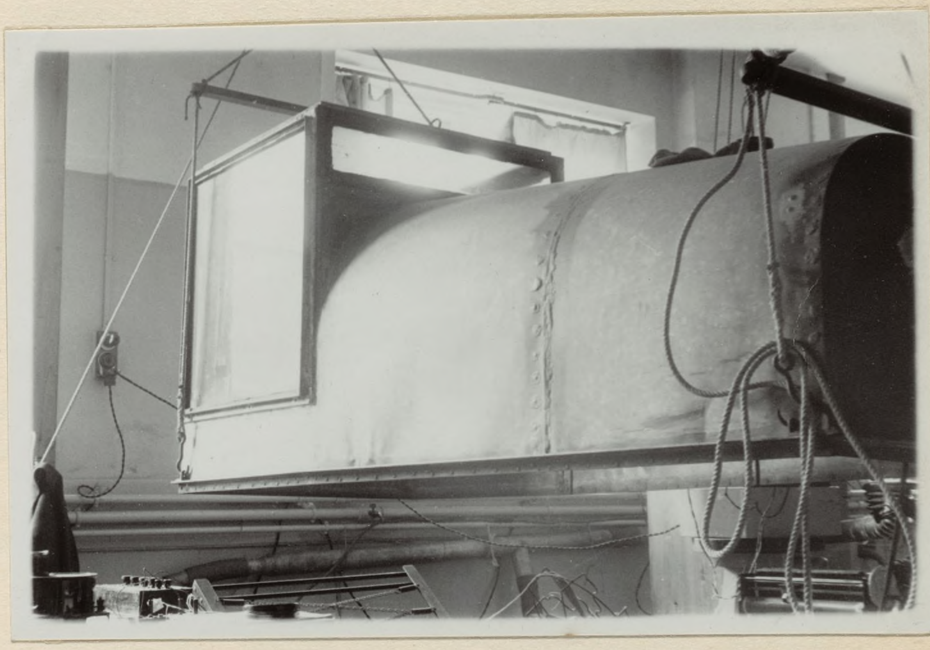

Figure 49 . Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Cover of Krogh cot chamber. The head end is glassed.

if the blood thickens. Mr. Christensen stated he thought thickening of the blood is too little to play any role whatsoever. The blood samples were taken while riding the ergometer with the helmet off at the start and the rider kept up the tempo by a resistance and by a loud bell, one stroke for each movement of the metronome. They used the electric control method of the ergometer with a small motor and rheostat for automatic adjustment.

From my notes I infer that the subject was eating from 4000 to 5000 calories per day but not all at one time, for he ate 1000 calories at 5 p.m. and 100 calories at 8 p.m.

The room itself had an interesting thermostat control of the room temperature. For example, there was a box over the steam radiator with a flap on it and a fan. As the fan accelerated in speed the flap opened and let out warm air. (See figure 50.)

Body weight balance. I saw a standing balance something like our Chatillon but the so-called "Lindell system" which was glycerin damped, so of course there was no heart impulse as we notice it. (See figure 51.) The advantage was that one could get a very rapid indication of the body weight. It was taken at the end of the respiration experiment. I see no particular advantage in it.

Dr. Lindhard, who is now domiciled in Krogh's laboratory, as before is carrying out experiments on exercise with much the same equipment as four years ago, and besides his interest in the muscular work I saw little to indicate activity. He was extremely agreeable and pleasant but there was nothing promising there. Apparently he is completely dominated by Krogh. I have been unable thus far to find out how much individuality there is in Lindhard and how much of a "yes, yes" man he is for Krogh.

The Krogh laboratory was, as usual, extremely stimulating. There is a very prize group of young men and women gathered there and the activities of both Professor and Mrs. Krogh are always of greatest interest. I regret that I did not have even more time for the study of the researches but I felt that the muscular work research was so greatly connected with our work that it was best to study it carefully. Mr. Christensen was one of the brightest men I met in Europe. Apparently the research is likewise not of little interest to both Krogh and Lindhard. Christensen impressed me greatly. I feel that he is an exceptionally capable man and I only wish we could have him in Boston for a year.

There is no question of Krogh's extraordinary capacity. Probably no man who has received the Nobel prize would be more qualified than Krogh. On the other hand I think he is definitely of the opinion that he is right on everything. He maintains a pontifical attitude in a discussion that probably is not always justified. On the other hand, he dominates the entire laboratory, and has developed a "school" of high skill, and hence is fully justified in his attitude of seeming infallibility.

Figure 50. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. In the center rear is the cover over the radiator for temperature regulation of the respiration room in Krogh's laboratory. Fan at bottom blows air out. The temperature is actuated by a thermostat.

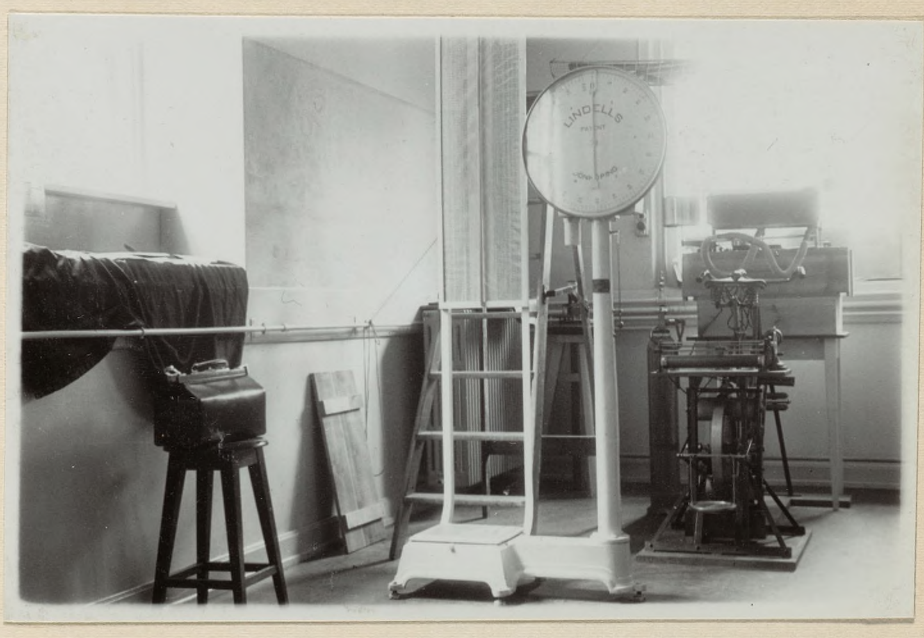


Figure 51. Copenhagen, Denmark. University of Copenhagen, Laboratory of Zoophysiology. Krogh respiration room for individual, showing Krogh ergometer and the Lindell balance for body weights. This balance is glycerin damped.

Citation of literature. I had a most interesting or extraordinary experience that I feel should go on record other than in the correspondence files of the Laboratory. Christensen and one of his associates had prepared a rather extensive Handbuch article upon body temperature during muscular work and I was astonished to find that nowhere was Professor H. M. Smith's monograph mentioned. Unfortunately they had duplicated a lot of his work in the Copenhagen laboratory and certainly they did it no better if as well. I then asked if they had seen the monograph. Neither had seen it or heard of it. I was sure there were copies somewhere in the library. I went to the library and was told that the books of Lindhard and Krogh were likewise deposited there and they had cards giving the books in Henriques' library. No trace of the monograph could be found in the library anywhere.

I did not want to start anything with Christensen as regards his seniors, but it so happened that at a tea at Professor Krogh's I saw practically all of the Carnegie monographs, among them Smith's, in his home library. Then I felt constrained to do something and later on my tour wrote to Krogh, asking him what "could be done about it." The Institution had printed this book, had given it free distribution, and had meant of course that it be placed where it could be obtained by those desiring it. Krogh wrote an appreciative letter, stating that it was an unfortunate situation, etc., and that the confusion of moving, etc., had probably caused the trouble.

It brought out a very important point, that is, that each monograph should be abstracted or digested in some of the standard abstract journals. This therefore has led us to adopt the plan of preparing a rather extensive digest of each monograph, which will be translated into French and German and sent by the Nutrition Laboratory to the various well-known abstract journals. At least in this way it will be called to the attention of readers who can not get access to the volume itself on the shelves.

<u>Distribution of Carnegie books.</u> The experience with Lindhard, Krogh, Christensen, and others in connection with the monograph of H. M. Smith was to me very disheartening. I asked myself: what is the trouble? Here the Carnegie Institution has paid for the collection of a lot of important data, calculations, tabulation, editing, and printing, and then has gratuitously deposited 400 copies in all the great libraries of the world and yet three men who have had it under their noses had not been aware of it and did not cite it. How can we get our books <u>into the hands of workers</u>? It is true that chiefs of libraries will <u>not</u> always pass along the books to their associates.

One point raised at Copenhagen was that the Smith book had not been abstracted or referred to in any abstract journals. That is a serious thing and I think now that abstracts must be prepared, preferably by Miss Wilson, and give if nothing more a <u>list</u> of material and subjects studied if they do not give the actual results. This certainly must be done in the case of long monographs. I ask: were there no notes of H. M. Smith's book in the National Academy Proceedings and no abstracts by Miss Wilson? Christensen tells me he had no reference to it in any place. Of course we can not get everything into a title and while the heart studies of Smith could not get into a title they could have been put into an abstract. Perhaps we can get, for example, one half of the book into a French abstract and put the emphasis of the last half of the book into a German abstract. Thus we would give a general summary of the first half of the book in detail in French and a general summary of the last half of the book in detail in German. Perhaps rather than try to stagger details throughout the entire abstract it would be better to emphasize the details in the first or second half or emphasize those points, or we might even get in an English abstract, and thus in the three abstracts get the book pretty well abstracted.

## COPENHAGEN, DENMARK.

### University of Copenhagen, Laboratory of Physiological Chemistry.

### Professor V. Henriques and Dr. R. Ege.

There was nothing of special significance going on in this laboratory for the Nutrition Laboratory's interest, but during our stay in Copenhagen we had most delightful social associations with both families and both men were active in arranging for my lecture and attended. Dr. Ege especially is a typical Copenhagen type of young scientist, full of enthusiasm and kindnesses and one can understand why students go to Copenhagen to this center.

COPENHAGEN, DENMARK.

University of Copenhagen, Laboratory of Hygiene.

Professor L. S. Fridericia.

I had never visited this laboratory before and although its activities are not along the line of Krogh and there is relatively little physiology, it was a great delight to come into contact with Professor Fridericia as intimately as I did. He is a very stimulating man with a group of associates who are actively engaged in problems chiefly of hygienic nature. There was, however, little if anything of direct value to the Nutrition Laboratory and hence no special comments can be made on the visit to this laboratory.

COPENHAGEN, DENMARK.

Carlsberg Laboratory.

Professor S. P. L. Sørensen.

A visit to this laboratory is always a delight. One finds here researches in physiological chemistry being carried out with the same degree of precision, exactly the same neatness and cleanliness as one would find in a laboratory devoted to atomic weight determinations. One found a number of volunteer associates from other countries, many from Paris. Professor Sørensen's activities are along the lines he has followed for so many years. Aside from a real insight into how biological researches should be carried out with special attention to details, there was little of definite value to the Nutrition Laboratory. However, a visit to this laboratory should be an important item in the program of anybody visiting Copenhagen.

At Professor Sørensen's home, where we were entertained, we took some photographs. (See figures 52, 53, and 54.)

Figure 52. Copenhagen, Denmark. Group at the home of Professor S. P. L. Sørensen. From left to right: Professor Sørensen, Mrs. Benedict, Dr. Benedict, and Mrs. Sørensen.

Figure 53. Copenhagen, Denmark. Group at the home of Professor S. P. L. Sørensen. From left to right: Professor Sørensen, Mrs. Benedict, Dr. Benedict, and Mrs. Sørensen.

Figure 54. Copenhagen, Denmark. Group at the home of Professor S. P. L. Sørensen. From left to right: Professor Sorensen, Mrs. Benedict, Dr. Benedict, and Mrs. Sørensen.

COPENHAGEN, DENMARK.

Agricultural Experiment Station, Department of Physiology.

Professor H. Møllgaard.

This laboratory as always was very interesting to visit, although at this time practically nothing was being done in respiratory metabolism. Professor Møllgaard told me that he had planned a large respiratory metabolism program with pigs but found that everything was very much mixed up with rickets. The pigs were fed with South American corn and had very little calcium. In spite of the large dairy industry in Denmark they had very little skim milk per pig, so Møllgaard felt he must first clear up the problem of calcium metabolism and his whole thought now is calcium and phosphorus versus the vitamins. He feels that calcium is the important thing; all others are incidental.

A respiratory chamber for pigs has been constructed since I was here before, indeed a very large chamber for pigs. (See figures 55, 56, 57, and 58.) He used a rotary pump which drove the gas through a gas meter at 20 liters per minute. A subsidiary gas meter, 3-liter size, and the usual complicated Møllgaard mercury burette is used to draw the sample. The mercury is lowered by a cord wound about a pulley on the extended axle of a wet gas meter.

There was also a respiration chamber for rabbits on the same principle. (See figures 59 and 60.) It had a zinc base at the bottom and an inverted glass aquarium resting in the water seal. There was also a cooling pipe for water inside the chamber. It impressed me as being a very large chamber for a rabbit, and the pig chamber was too large for a pig. The usual mercury lowering device is used.

Control tests. Møllgaard always sticks to hydrogen for gas checks. I do not know but what this is a good idea. We ought to use alcohol but at Durham we use carbon dioxide. It is harder to get a burner for a big alcohol check but we could burn hydrogen. Møllgaard plans to have 1 per cent oxygen deficit but of course no carbon dioxide is determined in these check tests. In all the cow experiments he draws a sample of ingoing air and analyzes it. He finds if there is not enough water above the mercury he often gets a variation in composition of the ingoing air.

His associate, Mr. Lund, is I think a very good man. He took us about the laboratory and showed us a large number of skulls of pigs showing that when there was a deficit of calcium they had a shorter upper jaw. They conclude that all vitamin A and D theories are out of the question with pig rachitis. They had also made experiments with pigs with the outdoor light, with ultra-violet light, and a light through double windows to filter out the ultra-violet.

I took a photograph of the gas-analysis apparatus. (See figure 61.)

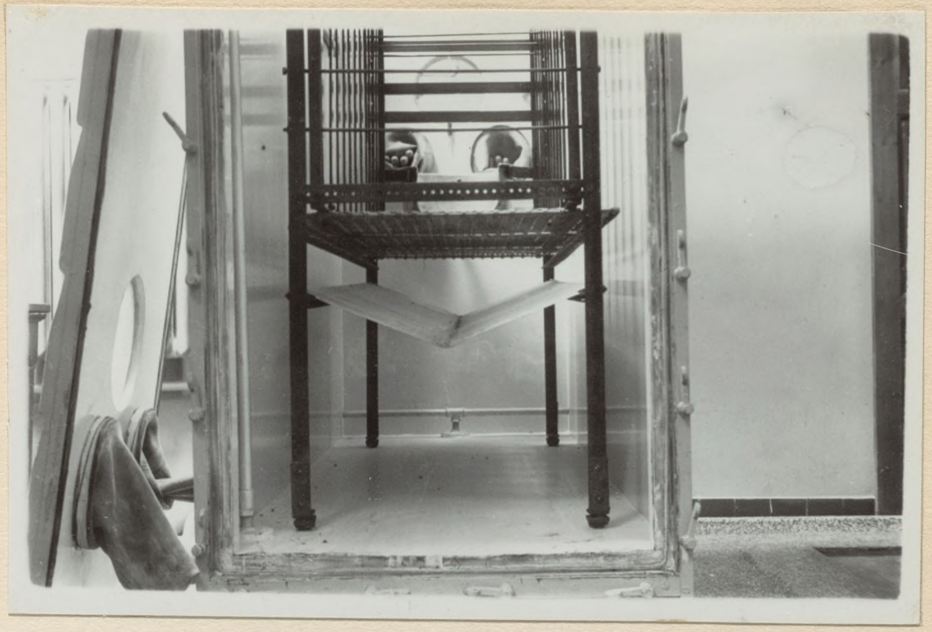

Figure 55. Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology. Møllgaard's respiration chamber for swine, showing long rubber gloves at front and rear for handling feces, excreta, and feed.

Figure 56. Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology. Another view of the Møllgaard hog respiration chamber.

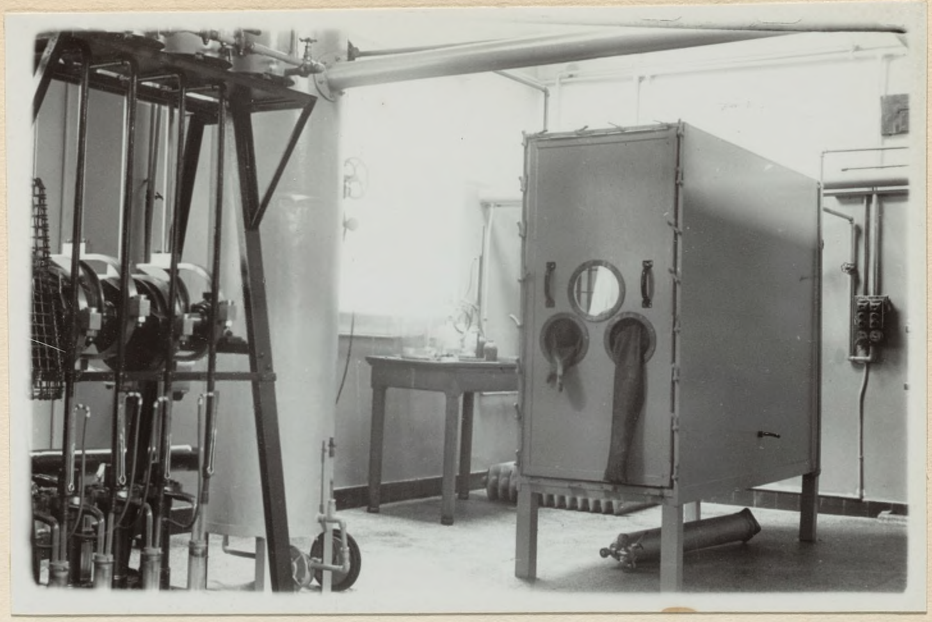

Figure 57. Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology. Møllgaard's pig chamber with rubber sleeve hanging out. At the left are the aliquot pumps for the large chamber.

Figure 58. Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology. Closer view of rubber sleeves and window of Møllgaard's pig chamber.

Figure 59. Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology, Laboratory of Professor Møllgaard. Rabbit chamber showing glass cover and cooling pipes.

Figure 60. Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology, Laboratory of Professor Møllgaard. Rabbit chamber with cooling device partially removed. At the right, part of the mechanical aliquoting pumps for the large chamber.

Figure 61 . Copenhagen, Denmark. Agricultural Experiment Station, Department of Physiology. Corner of Møllgaard's laboratory where gas-analysis apparatus is installed.

COPENHAGEN, DENMARK.

Lectures.

On September 27 I gave the "third" lecture before the Medical Society. No students were present but there was an attendance of about 225. It was a fine room and a good audience.

On September 28 I lectured in Krogh's lecture room before the Biological Society, giving here the "first" lecture, to an audience of about 85.

Announcements of both lectures are appended herewith.

## Det medicinske Selskab i København.

Ekstraordinært Møde afholdes Tirsdag den 27. September Kl. 8 pr. i Domus medica.

DAGSORDEN:

*Francis G. Benedict:* Human Basal Metabolism in the Light of Recent Studies.

Copenhagen

## Biologisk Selskab

afholder Møde Onsdag den 28. September Kl. 8 i Fysiologisk Instituts store Auditorium. Indgang Henrik Harpestrengs Vej 5.

DAGSORDEN:

Dr. *Francis G. Benedict*, Carnegie Institution, Washington: The physiology of great tortoises and snakes and its relation to human physiology.

*Bestyrelsen.*

Mærk den forandrede Mødedag og det forandrede Mødested.

## ABERDEEN, SCOTLAND.

### The Rowett Research Institute.

### Professor J. J. R. Macleod, Professor J. B. Orr, and Dr. H. E. Magee.

I was very sorry to find that Dr. Magee, who had made such an excellent start on the metabolism of ruminants, especially the goat, had stopped all this work and he and his collaborators were for the most part concentrating on blood sugar in fasting fowl and cows. There was a great deal of discussion and some excitement over the course of the total blood sugar, both with fasting hens and with fasting cocks. Magee found an extraordinary thing, that there was a rise in the blood sugar with the fasting fowl to about the 72nd hour, followed by a decline. Two groups fell during the first 24 hours and one group rose during the first 24 hours. This picture, complicated as it is, has given rise to a great deal of live discussion. This finding seemed significant until he said that the total blood sugar in the entire body of the hen is but 0.2 gm., so that one would have to be very careful in making statements as to an actual absolute rise in blood sugar. In getting the blood sample with chickens they lay the animal on the side and get a wing vein, and then pluck a few feathers. A needle is used but no syringe. There is no struggle.

Blood sugar in cows. Tremendous interest in the blood sugar of cows is being shown and a great deal of work has been done on that point. They had an ingenious method of drawing samples. The cow was tied up to a stanchion, the head drawn to one side and held tight, and the carotid or jugular vein punctured with a fairly large hypodermic needle. (See figures 62 and 63.) By thrusting the needle into the vein and massaging the vein they secured in a bottle as much blood as they desired. The same method is used with sheep. They do not even shave the neck. I found that they do not approve of taking the blood from the tail or ear. They have bled cows every hour for 24 hours.

They use the Hagedorn-Jensen method and also the yeast-fermentation method to get the real sugar, as distinguished from total reducing substances which are given by the Hagedorn-Jensen method.

One of the great problems in this institute is the study of the progress of food through the alimentary tract. They had done much also with the lactic acid determinations, specifically in a silo, and found that at about 40° temperature there was a great drop in the lactic acid formation, but the acetic acid and lower fatty acids predominated. They argued that the temperature of the paunch of the ox was probably above the rectal temperature and probably above the lactic acid level, and that it should be the body temperature plus the fermentation temperature. Possibly the lactic acid production

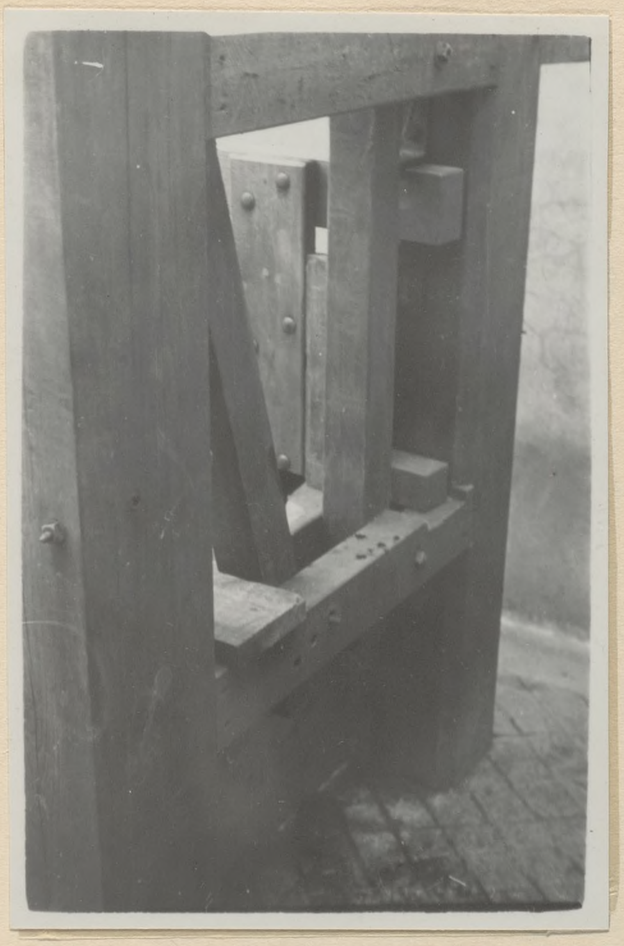

Figure 62. Aberdeen, Scotland. The Rowett Research Institute. Yoke for holding head of cattle when drawing blood samples in the neck.

Figure 63. Aberdeen, Scotland. The Rowett Research Institute. Animal in position for drawing a blood sample.

is all stopped by the high paunch fermentation temperature so that there will be no rise in lactic acid expected. Other acids of course should be there. Thus it occurred to me that we (i.e., Ritzman and the Nutrition Laboratory) should get (1) the temperature of the paunch, (2) I should look up the method employed by Scheunert and (3) of course there should be a chemistry of the paunch contents, but this should be done by an extremely capable person in biochemistry and, as Professor Macleod stated, it needs the best of men, men like Barger to do it. Orr maintained that Barger is the only man who could do it and probably he will not.

Macleod states that he has made no distinction between free sugar and combined sugar in the blood in all the studies as yet. But if the Hagedorn-Jensen and Lehmann methods give wide differences with different food levels, as was reported to me by Lehmann of Lund, then Macleod may be off in holding the carbohydrate metabolism studies by the Hagedorn-Jensen method alone. Of course since these notes were made Lehmann has admitted that his method is not sufficiently proved to be put out. It may be that the striking difference that he found by the Hagedorn-Jensen and his own method will disappear. Macleod sticks to the H.-J. method. Probably for a baseline this is correct, but Macleod assumes that any increase afterward as a result of digestion is only pure glucose and no other reducing substance. This applies to nearly all that Macleod is doing now.

An interesting point I did not know was that the blood sugar of ruminants, cows, etc., is very low, about one half that of man. I ask, is it because the carbohydrate is converted to fatty acids and not absorbed as glucose? This is worth looking into. How is the blood sugar in the horse? The horse is not a ruminant and yet we have found that it has the highest metabolism of all per square meter. I feel that here at Rowett one should settle the course of the blood sugar and the acids after food in the ruminant.

Ritzman must chase up the blood sugar in the horse. I think it might be well to look up the Vineland, New Jersey, company, the Kimball Company, who manufacture the rotamesser and manufacture these little ampoules of Mr. Felix Meyer of Aachen, and perhaps we can get the ampoules evacuated and ready for blood and by breaking off the tip under the blood quickly fill them. I think there is a large opportunity here to straighten out the theory (Grouven) that I feel very strongly holds. Perhaps these previously evacuated ampoules are just the things to get samples, preserve them, and send them to Boston to determine the sugar. If we know more about the course of the blood sugar and acids after digestion it should stimulate the whole question of the striking effect of food in ruminants and contribute to a solution of the Grouven hypothesis.

Magee was very much interested in Carpenter's apparatus. He should have all of our material along this line as he is a very good man and keen as a whip.

Professor Orr. While a number of years ago I wrote that if one wished to have a good anti-vitamin day one should spend a day with Orr, he tells me now that he is partially converted to the vitamins. He is very much occupied in various nutritional surveys, and I find on the whole much more apparent activity than six years ago. For example, they are at present very much interested in the question of cows and iodine and by giving 100 mg. of iodine per day they find it in the milk, but as yet there is no account of it in the urine and feces. Orr is most interested in committees and surveys, as formerly, but apparently there is a very serious and intense interest in scientific research, in no little part furnished by the great interest shown in this institute by Macleod since he has been there.

One of the healthiest things I noted in this laboratory was the keen laboratory discussion of all these points, partaking of the nature almost of a seminar. Rowett seems well worth keeping in touch with, perhaps the best and most stimulating place in the whole of Great Britain.

ABERDEEN, SCOTLAND.

University of Aberdeen, Faculty of Science, Laboratory of Physiology.

Professor J. J. R. Macleod.

Macleod is in my judgment a very remarkable man. He is as keen as a razor. His Linacre Lecture reprint, which was given me, was most stimulating. In his laboratory there was little if any metabolism, but I saw several Haldanes in the laboratory, long-stemmed, graduated, but as usual they looked bad, very unkempt and dirty. This laboratory is the University laboratory, not at Rowett.

I enjoyed very much every moment with him and considered it a great privilege that we could enjoy the hospitality of Professor and Mrs. Macleod. (See figures 64, 65, and 66.) The hours in his study, discussing many problems and questions, were really a treat. I was able to get in a wholly simple way the whole story of the discovery, development, and progress of insulin at Toronto. There was no question but what Macleod's personality is strongly felt in Aberdeen, not only in the University but likewise in the Rowett Institute.

Lecture. On October 3rd I gave the "second" lecture in Professor Macleod's lecture room, before a group of about 65.

Figure 64. Aberdeen, Scotland. Professor J. J. R. Macleod on the terrace of his rock garden, Aberdeen.

Figure 65. Aberdeen, Scotland. Photograph of Professor Macleod on the terrace at his home in Aberdeen.

Figure 66. Aberdeen, Scotland. Home of Professor and Mrs. J. J. R. Macleod.

ABERDEEN, SCOTLAND.

From the standpoint of nutrition I had most stimulating talks with both Orr and Macleod. Orr especially is on some government commission for alimentation in Great Britain, and I believe that MacLeod is very closely allied with the same commission. A point they both brought out very intelligently was that in spite of the great depression in England and the use of the dole, unemployment, etc., there was no reason why anybody should starve in England. There was the usual problem of the distribution of food, with a plethora at one point and a deficiency at the other point, but in general starvation should not take place.

The dominant factor of food selection in food likes or dislikes is illustrated by the fact that every morning in Aberdeen there were thrown into the river six thousand quarts of skim milk, simply because the people would not use it. The children will not drink it and people will not use it for cooking. It is looked upon as a waste product and thrown away. A proportionate amount is thrown away in practically all of the other Scottish cities. In the south of England there is undoubtedly a great scarcity of milk, certainly not an adequate supply, and here in Scotland is the wasteful rejection of this important food material. It should be said, however, that the great cost of transportation of skim milk (92 per cent water) from Aberdeen some five hundred or more miles to London or south offsets the food value, but it is still worthy of note that many of the poor in Scotland are unwilling to use this food material. It reminds one of the experience of the American Relief Commission in Belgium, when the Belgians refused absolutely to eat American rice sent over to them.

EDINBURGH, SCOTLAND.

University of Edinburgh, Department of Physiology.

Sir Edward Sharpey-Schafer.

Schafer's age (83 years) and illness have both contributed to his lessened activities. (See figures 67 & 68.) There was no metabolism of humans going on, but he had a chamber for rabbits and cats. Schafer is strong on the use of cats and makes many experiments with them. He says they are fine experimental animals. Of special interest was the fact that they had begun work on a monkey.

He was much interested in showing me the respiration apparatus for a cat, which I thought grotesquely bad. He was even more interested in showing me the apparatus for a rat which, if anything, was worse than the cat apparatus. (See figures 69 and 70.) He used a Kendrick spirometer, so-called "Benedict" type, but everything was very badly constructed and extremely large for a rat. The blower sucked air out of the chamber (a large glass bell jar), through soda-lime by closed circuit, and then for some unaccountable reason there was placed a Sadd valve between the ventilation and spirometer. One point of interest was that the soda-lime was protected by an indicator which showed when carbon dioxide passed. This was the so-called "Sofnolite" which turned red when carbon dioxide was present. This is manufactured by Sofnol, Ltd., Greenwich, London. This Sofnolite that he used in a number of places to indicate the efficient carbon-dioxide absorption is all right, but I think that we are always as well protected with our various systems, as by the unusually large soda-lime bottle or else by using a pair of bottles, or with the closed circuit, having the air continually passing over the soda-lime, and that this control of unabsorbed carbon dioxide is practically unnecessary. These two apparatus in Schafer's laboratory are excellent illustrations of how badly respiratory metabolism may be attempted in a laboratory supposed to be one of the most important in Europe. It was sad to see such a "set-up" as this.

Lecture. On October 5th I lectured in Schafer's lecture room for the Edinburgh Physiological Society, before an audience of 135. I gave the "third" lecture, which was followed by a lively discussion.

Figure 67. Edinburgh, Scotland. Sir Edward Sharpey-Schafer.

Figure 68 . Edinburgh, Scotland. Photograph taken at the home of Sir Edward Schafer. Left to right: Mrs. Benedict, Lady Schafer, and Sir Edward Schafer.

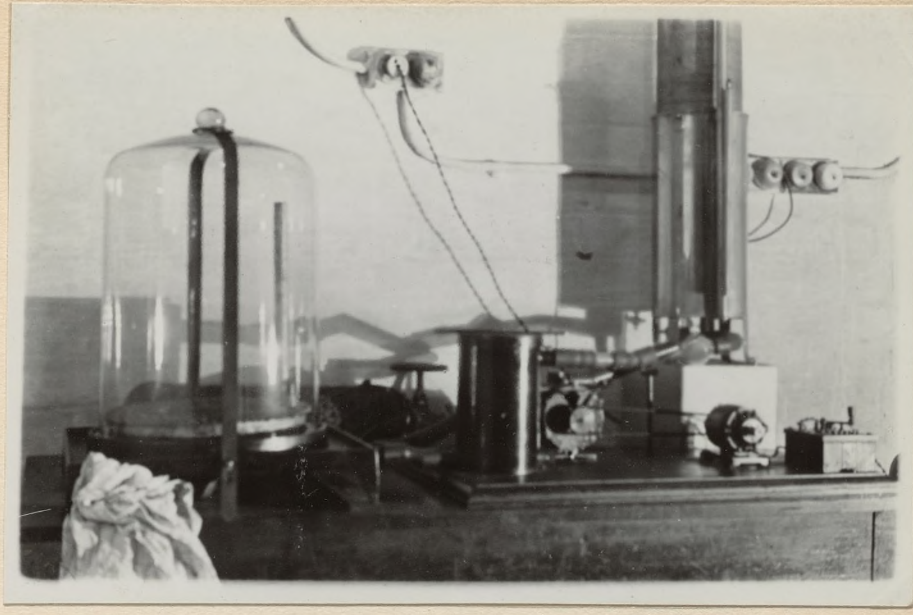

Figure 69. Edinburgh, Scotland. University of Edinburgh, Department of Physiology, Laboratory of Sir Edward Sharpey-Schafer. Cat (and small monkey) respiration apparatus of Professor Schafer. A bell jar is used with the chamber. Soda-lime can of metal with waxed glass top and part of the spirometer. In the circuit is placed a Sadd valve.

Figure 70. Edinburgh, Scotland. University of Edinburgh, Department of Physiology, Laboratory of Sir Edward Sharpey-Schafer. Schafer's metabolism apparatus for rats. Small bell jar with essentially the same equipment for soda-lime as the cat apparatus. The Sadd valve leads off to the right to the spirometer.

EDINBURGH, SCOTLAND.

University of Edinburgh, Department of Zoology.

Professor J. H. Ashworth.

This new institution, built with funds given by the Rockefeller Foundation and designed by the architect, Latimer, who designed the Scottish War Memorial, is a beautiful building. The work is essentially entirely zoological with practically nothing in the building in the line of metabolism, but they are much interested in cold-blooded physiology, so the snake monograph was sent to them. They have an excellent library and a remarkably fine museum.

Ashworth is a very fine man and has a splendid group of associates about him there. (See figures 71, 72, and 73.)

Lecture. I was very glad to give the "first" lecture, on snakes, on October 7th to a group of about fifty most interested people in Ashworth's institute, and there was a very stimulating discussion following the lecture. Contacts were made with two men formerly on snake (venom) research in India.

Figure 71 . Edinburgh, Scotland. Left to right: Dr. Benedict, Professor J. H. Ashworth, and Mrs. Ashworth.

Figure 72. Edinburgh, Scotland. Left to right: Mrs. Benedict, Professor J. H. Ashworth, and Mrs. Ashworth.

Figure 73. Edinburgh, Scotland. Left to right: Mrs. Benedict, Professor J. H. Ashworth, and Mrs. Ashworth.

EDINBURGH, SCOTLAND.

Royal (Dick) Veterinary College, Biochemical Laboratories.

Professor Henry Dryerre and Dr. J. Y. Bogue.

I found Dryerre very eager to have me visit his laboratory, which I did, and afterward we went out to the Experiment Station at Moredun. The most interesting thing in the laboratory was a young man, a very fine intelligent chap, Bogue. He is extremely interested in the direct calorimetry of the rat, he told me. He was designing and constructing an apparatus for rats, and I told him he should certainly make an alcohol check experiment before he publishes. I entered into a friendly conversation with him and told him to let me know his progress. He had most ingenious ideas and seemed to be a man well worthy of encouragement, and I feel that we must keep in touch with him, as he sounded like a good man.

Moredun I found to be only in the problematical stage so far as metabolism is concerned. They had no apparatus. They had a room in which Dryerre told me he <u>might</u> install a respiration chamber for sheep. The room was cold, with no heat and no temperature control. It was an interesting place to visit, and the director, J. Russell Greig, was a very pleasant man, but everything metabolic seemed to be in the future, with nothing actually accomplished.

Dryerre is always fussing about, is always <u>going</u> <u>to</u> <u>do</u> something, and is always <u>nearly</u> ready. Much to my surprise, I found that he had a <u>very</u> <u>elaborate</u> set of notes on Ritzman's apparatus. He seems heavy, serious enough, but I do not believe he will go very far. I hope he will but I don't think he is especially promising. Bogue, on the contrary, I think has great potentialities.

## CAMBRIDGE, ENGLAND.

### Addenbrooke's Hospital.

### Dr. C. G. L. Wolf.

Realizing the difficulty that Professor Eleanor D. Mason of Madras and I have had in securing basal metabolism measurements on people coming from India to England, it was a pleasure to note the interest in metabolism sustained by Dr. Wolf in Cambridge. He had recently fixed up a very small, to be sure, but very perfect metabolism laboratory including the Benedict apparatus, and the Nutrition Laboratory had presented the Addenbrooke's Hospital with a helmet. Dr. Wolf had had a certain amount of difficulty in attaching the helmet but in five minutes I showed him the trouble and he was on the apparatus for some time and was comfortable and enthusiastic as to its possibilities. His principal difficulty with the helmet seemed to be that the circulation was not running properly and he had no effect of the dry air current cooling the forehead. I straightened this out for him.

Apparently Dr. Wolf is trying very hard to introduce basal metabolism into the hospital as a regular measure and is making progress, although slowly. The laboratory itself is a model of neatness and should inspire respect for the apparatus on the part of the patients and the doctors in the hospital. It is in striking contrast to most rooms that one sees in many places. (See figures 74, 75, and 76.)

I was very much disappointed in finding that Dr. Wolf had laid aside perhaps permanently his preparation of a book on metabolism. After a very bad automobile accident in which both Dr. and Mrs. Wolf were badly shaken up and injured he felt it necessary to drop the task.

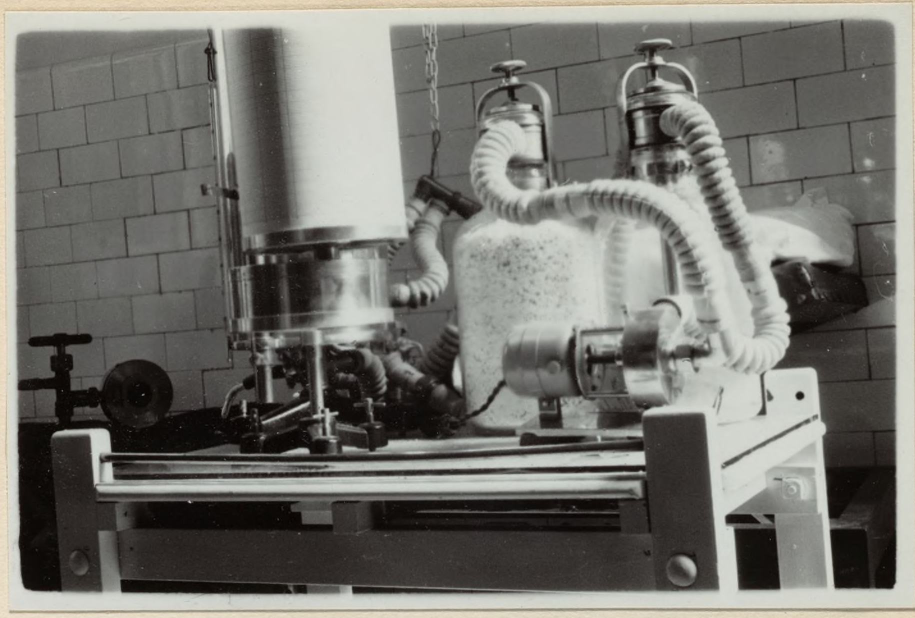

Figure 74. Cambridge, England. Addenbrooke's Hospital. Metabolism room of Dr. C. G. L. Wolf. Respiration apparatus showing Collins blower, spirometer, two soda-lime bottles, and mouthpiece.

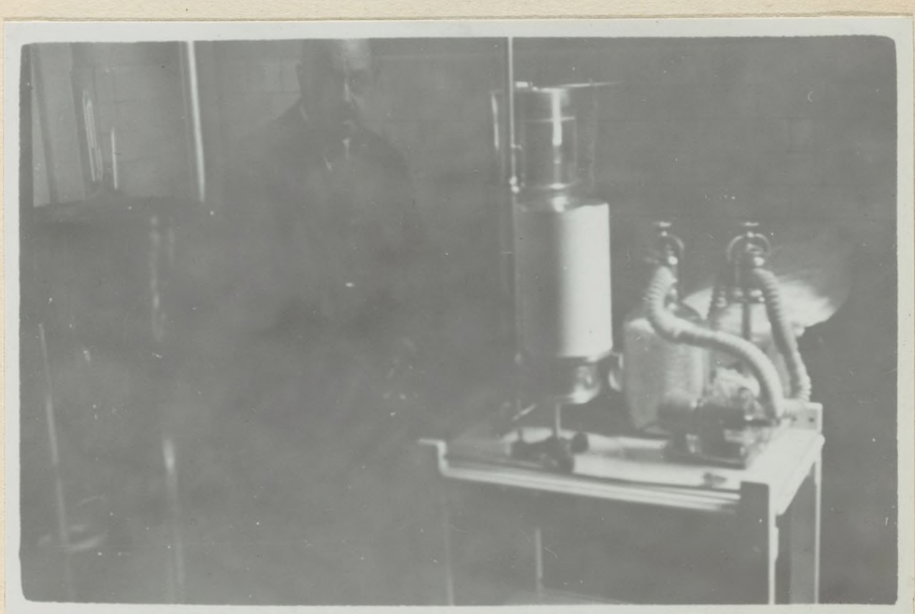

Figure 75. Cambridge, England. Addenbrooke's Hospital. Metabolism room of Dr. C. G. L. Wolf. A very bad view of respiration apparatus with Dr. Wolf dimly in the background at the left.

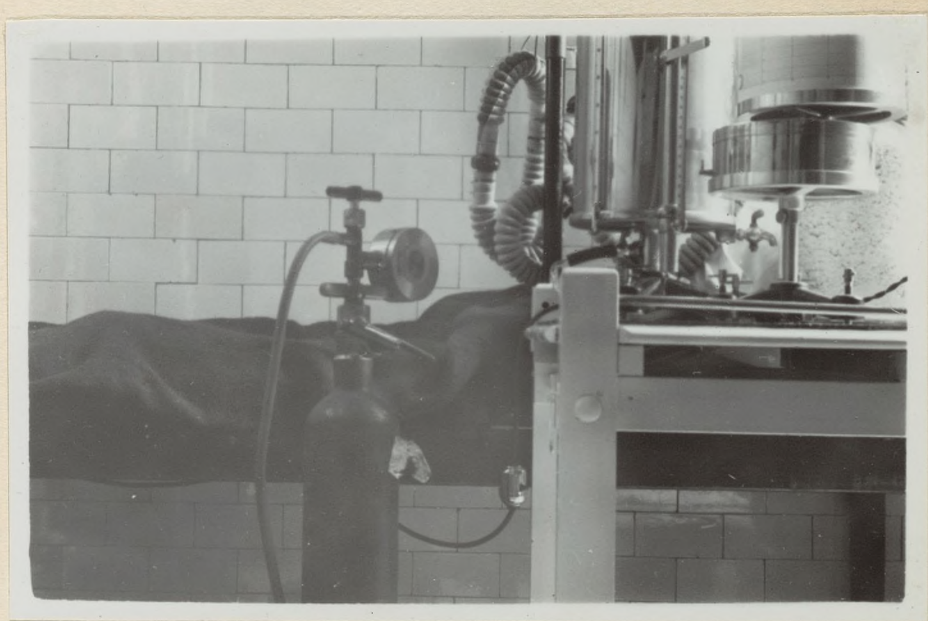

Figure 76. Cambridge, England. Addenbrooke's Hospital. Another view of respiration apparatus at Dr. Wolf's laboratory.

CAMBRIDGE, ENGLAND.

University of Cambridge, School of Agriculture.

Institute of Animal Nutrition.

Dr. Thomas Deighton.

Now Deighton is running his own show. T. B. Wood has died and Capstick is very seriously ill and I believe never comes to the laboratory. Consequently Deighton is running around laying most emphasis upon problems of an economic nature. The most unfortunate thing is that Deighton has no adverse critic. Since my last visit here he had been empowered by the authorities to take a trip to the Continent and I found that he had visited Fingerling, Møllgaard, and Lefèvre, and I am not sure about Zürich and Berlin. He had had no experience in indirect calorimetry but had made this visit to European laboratories and presented a very confidential report to the British Ministry, including a rather harsh criticism of Lefèvre, and he was most chagrined to think that the British Ministry permitted Lefèvre to see a copy of this. He tried to get a copy for me but the successor to T. B. Woods, F. W. H. Marshall, whom I had not met personally, told Deighton that I could not have it. At present he was distressed by the fact that the Minister of Agriculture had lost interest or had "let down" in appropriations.

In a recent article Deighton had touched upon a theory of Rubner on the question of length of life, death, and the potentialities of life, much upon the basis that a man or animal had only a certain time to live anyway and that with excessive activity he would be burned up more quickly. This is interesting in connection with work proposed by the Nutrition Laboratory in 1933 on Professor Sherman's rats.

Deighton is still obsessed with the idea of direct calorimetry and he had made experiments on the cat. He is at present constructing a compensation chamber for poultry, in which he is using cellophane walls, their particular value being, according to him, that the poultry will live normally in light. (See figures 77, 78, and 79.) The apparatus had also an elaborate electrical heating system and temperature measuring system to maintain the walls at constant temperature. It seemed to me rather hopeless. Although he had direct calorimetric compensation chambers he wishes now to add the indirect to it. He felt that 24-hour periods were necessary for poultry, but he also admitted that poultry experiments were made primarily for political reasons, to keep the interest of their farmer constituents.

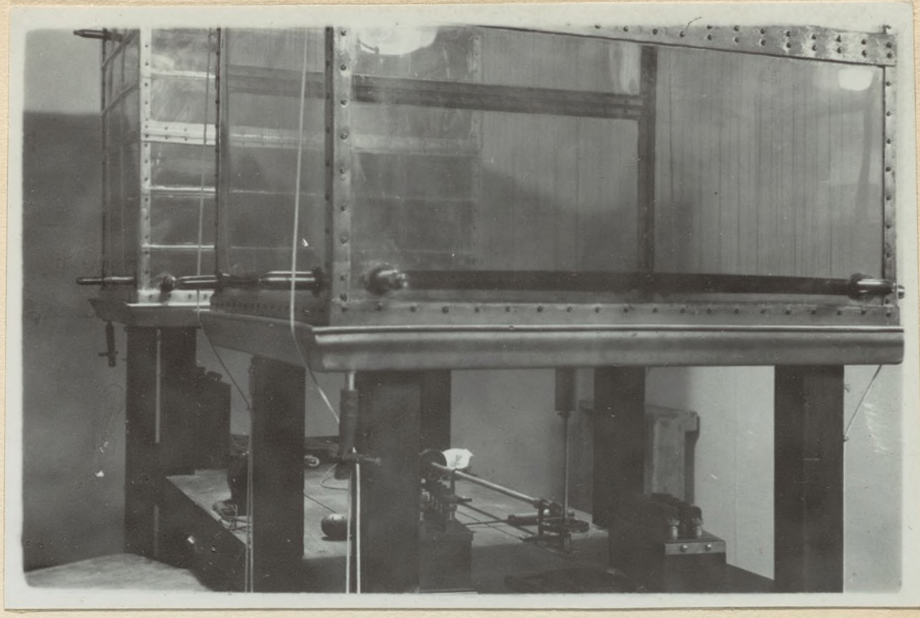

Figure 77. Cambridge, England. University of Cambridge, School of Agriculture, Institute of Animal Nutrition. Laboratory of Dr. Deighton. Double compensation respiration chamber with cellophane walls, to be used for poultry. Still under construction.

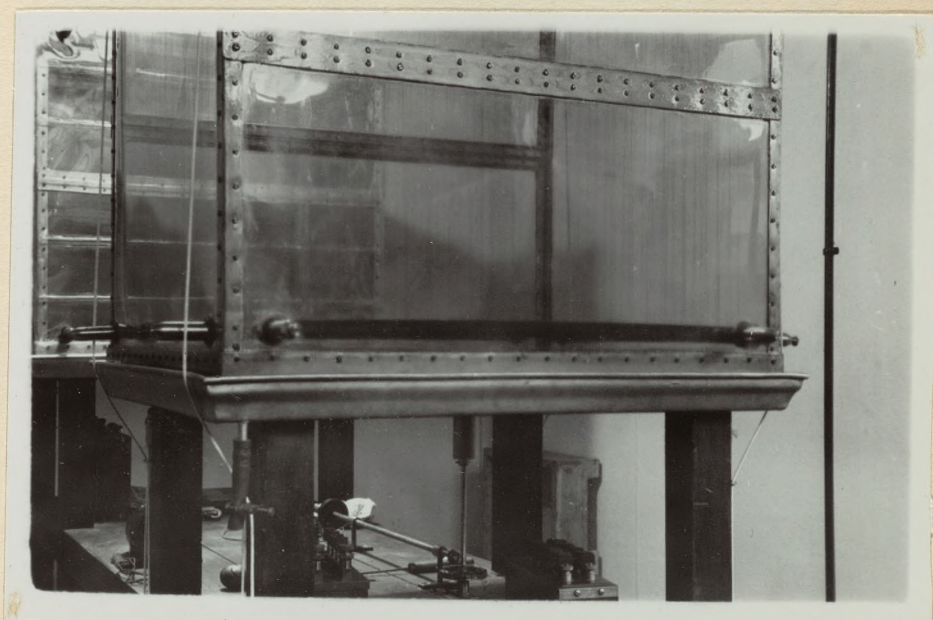

Figure 78. Cambridge, England. University of Cambridge, School of Agriculture, Institute of Animal Nutrition. Second view of Deighton's chamber for direct calorimetry of poultry.

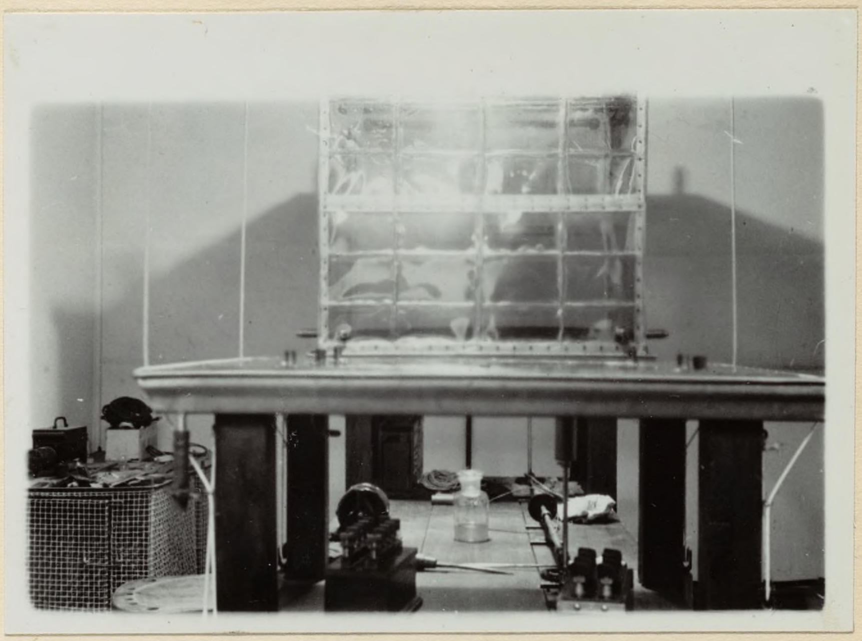

Figure 79 . Cambridge, England. University of Cambridge, School of Agriculture, Institute of Animal Nutrition. Another view of Deighton's calorimeter for poultry, showing the cellophane walls.

They were running a calorimeter 24 hours per day and five or six days at a time with the pig. The animal was allowed to move about as he would. The humidity was determined by a wet and dry bulb thermometer, with a very poor-looking wet bulb -- he had a "woolen blanket" effect over the mercury instead of a light tissue covering.

My feeling is that Deighton is very clever but is literally hobbled by Capstick's notions. Frankly, while I was formerly anxious to have Deighton have direct control of his researches, I feel now that in the absence of a critic he will not have the best planned researches. In spite of the fact that I question seriously Deighton's success in following out his present line of attack, since he is so dominated by the Capstick ideas, I felt justified in writing a letter to the Minister of Agriculture, Major Walter Elliot, with regard to Deighton, a copy of which letter is appended herewith, and I had a good reply from Elliot. My feeling is that Deighton is going to let them in for a great deal of money and then have a lot of uninterpretable results if they do not watch out. I also tried to get Deighton to come to America and visit institutions here. He asks intelligent questions and is a very good bet, and Cambridge will have a chance to have something worth while, but he certainly needs an adverse critic and needs to visit American institutions before going too far.

20th October, 1932.

Rt. Hon. Major Walter Elliott, M.P.,
Ministry of Agriculture,
WHITEHALL,
LONDON, S.W.1.

My dear Sir,

Writing as I am en tour I unfortunately have not with me official stationery, but I am writing officially as Director of the Nutrition Laboratory of the Carnegie Institution of Washington, the Laboratory being located in Boston, Mass.

Every three years I make a tour of European Institutions of research in the nutrition of man and animals. It is needless to state that the researches at Cambridge formerly under the direction of Professor T. B. Wood and carried out so admirably by Mr. Capstick and Dr. Deighton have always had a great interest for me.

On a recent visit to Cambridge, Dr. Deighton told me of his opportunity of seeing the European Institutes carrying out similar work. This study of these Institutes I have for years strongly urged and it is obvious that Dr. Deighton made most admirable use of such a visit.

May I emphasize what was perfectly clear to me in my recent visit to Cambridge that I think you have in Dr. Deighton a wholly unusual man for the researches to be carried out there. I have had an increasing impression of scholarliness and understanding of technique in all of Dr. Deighton's writings. Being occupied with very much the same line of research, I feel certain that a visit to certain American Institutions would be most productive of results, orient him in American work and give him what I think is a much needed perspective. Were I in a position to recommend to any Board the expenditure of funds for such research, it would surely include a modest appropriation to enable Dr. Deighton to make such a visit. Without any doubt after such a visit the progress of the Cambridge researches would be even more rapid and planned with a full knowledge of what is going on in all the important centres in both Europe and America.

This recommendation is based upon my own personal experience and indeed, practice in making a triennial tour of European Institutions a regular feature of our research program.

With such a man as Dr. Deighton to bank on one is very safe in making such recommendations and I do so with what I believe to be the joint interests and welfare of Dr. Deighton and the Cambridge Institute.

It is needless to say that Dr. Deighton visiting either the Nutrition Laboratory in Boston or the Laboratory for Animal Nutrition at Durham, New Hampshire, he would be received with the greatest friendliness and accorded the opportunity of seeing every detail of our work.

Trusting you will pardon my seeming intrusion which was prompted only by the fine impression I received last week at Cambridge,

I am,

Very sincerely yours,

(Signed) Francis G. Benedict.

Permanent European Address:-

Dr. Francis G. Benedict,

C/o Brown, Shipley & Co.,

123, Pall Mall,

LONDON, S.W.1.

## CAMBRIDGE, ENGLAND.

### Lectures.

On October 11th at Addenbrooke's Hospital I gave the "third" lecture, before the Cambridge Medical Society. This was very well attended, about 65 being present, with some discussion, especially regarding insensible perspiration, afterwards. (There is an announcement of this lecture.)

The "first" lecture, on the snakes, was given on October 13th at the Institute of Biochemistry (Professor Hopkins), where as usual I found an extraordinarily interested audience (about 55 people), with the usual aftermath of many questions and discussions. It is always a great stimulus to address this group.

# Cambridge Medical Society.

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A Special Meeting will be held at Addenbrooke's Hospital on Tuesday, October 11th, at 3 p.m., when a paper illustrated by Lantern Slides will be read by Dr. Francis G. Benedict, the Director of the Carnegie Nutrition Laboratory, Boston, U.S.A., on

BASAL METABOLISM IN ITS RELATION TO PHYSICAL FITNESS.

C. H. WHITTLE, *Hon. Sec.*

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*** By arrangement with the Cambs. and Hunts. Branch of the British Medical Assscation, all members of the Branch are entitled to attend the Meetings of the Cambridge Medical Society. These meetings, which are held monthly, excepting July and August, will count as clinical meetings of the Association.

LONDON, ENGLAND.

Middlesex Hospital.

Professor E. C. Dodds and Dr. J. D. Robertson.

This is about the only place in London where one can be sure of getting basal metabolism measurements or of a laboratory for such measurements, although I was told by Dr. Kenneth Tallerman that Dr. Gardner Hall at St. Thomas' Hospital had a very good equipment. Both Dodds and Hall carry out researches chiefly for the clinic, but Professor Dodds had been kind enough to determine the metabolism on at least one of our subjects returning from India.

Middlesex Hospital is being rather extensively reconstructed, and I found that Professor Dodds had a large room and four cubicles with one Collins and three Kendrick apparatus. The bells looked badly. There were dents in them and they looked as if they had been blown over and hit the floor. The impression was that of a "dirty Haldane". They used small oxygen cylinders, medical size, for each apparatus, and I suggested that with so many cubicles they might use a pipe line. They had a great many Haldane apparatus, which are made all in one piece of glass. They also had one large Haldane like Dr. Carpenter's, but with a capacity of 100 cm., and it was not functioning well. The hospital is most interested in thyroids and the follow-up after medication or operation. No experiments were actually in progress in the afternoon but apparently the laboratory was actively used in the morning.

LONDON, ENGLAND.

Guy's Hospital Medical School, Physiological Laboratory.

Professor M. S. Pembrey and Professor E. P. Poulton.

This extremely interesting laboratory is certainly a most depressing place to visit, badly lighted, dirty, and with an air of depression all about it. Of course Professor Pembrey is just about to retire so there was not the greatest experimental activity and enthusiasm; very little of immediate interest was obvious. Professor Poulton of the same department, but associated more with the hospital, told me he was still using his rather complicated apparatus for basal metabolism measurements with patients but I did not visit it.

Pembrey is just publishing a radiographic study of movements of the stomach after food followed by exercise. He is still hot after the low respiratory quotients and thinks the changes in the quotient may be both ways; that is, carbohydrate may go to fat and fat may go to carbohydrate. He thinks the respiratory quotient of 0.3 that he published on the marmot several years ago is correct but he still uses the weight method.

Pembrey banks absolutely on whatever Haldane says or does. For example, we were speaking about the variation in the carbon dioxide in outdoor air which was asserted to be diurnal and due to vegetation. Pembrey is sure of it. It is a biological necessity, but if you put one drop of ether on the floor of the lecture hall you can not find it at the top. Pembrey states that during a London fog the carbon dioxide may run to 0.14 per cent and one simply can not take air as of "constant composition". He agrees that the conditions for work are not good. He says he can not do the respiratory quotients by gas analysis as he can not accept constant composition of outdoor air. I feel that he might perfectly well analyze incoming outdoor air and still get his respiratory quotients. However, I felt his apparatus showed pronounced differences in outdoor air, so it is a fault of inaccurate technique. I had visited Haldane in Oxford and seen his private laboratory and his work upon the fermentation and free hydrogen (?) in silos and peat bogs. Pembrey is not surprised at this and believes it may be up as high as 2 per cent in silos. Certainly Pembrey is dominated by Haldane and his early experience with the gravimetric method for carbon dioxide with Haldane.

Commenting upon the hibernation of animals and former studies made on them, Pembrey stated that some continued observations were made in Europe by taking a sleeping marmot at 10° C. into a room at 20° C. to measure its metabolism!! At the time of my visit he emphasized that we must study the waking state of the animal. This was, of course, done at the Nutrition Laboratory during the winter of 1932-33. Pembrey told me that he has marmots on hand now and is checking the old respiratory quotient of 0.3, and considers of special importance the change from sleeping to awake. The question I wrote in my book was that we found that a snake in warming from 18° to 23° C. changed its respiratory quotient from 0.60 to 0.71. Will a marmot under any conditions go from 0.3 to 0.71?

Professor E. P. Poulton. There was a meeting of the British Physiological Society at about this time and at the request of Professor Poulton, who had a paper on direct versus indirect calorimetry, I participated in the discussion. Poulton's thesis is in my judgment wrong. He and his associate, Adams, have used the very old Middletown calorimeter series and have computed from the old respiratory quotients, in an effort to disprove the Zuntz-Schunberg caloric values for oxygen and carbon dioxide. Poulton of course uses many two-hour periods. He finds that the carbon dioxide heat is almost constant but the oxygen differs, and obviously all is based upon the determination of oxygen. He also told me that the respiratory quotients wandered all over the lot in a forenoon. There was a great deal of work there, a lot of fuss, and misuse of data, but he had caught me with a published respiratory quotient of 0.44 from the Nutrition Laboratory!!! This certainly was a great error. Poulton has disregarded completely all subsequent work on respiratory quotients.

He presented the matter in an antagonistic yet friendly way and I was called upon to discuss it. I pointed out that the material was based on experiments twenty-five years old and during that time there had certainly been some advancement in metabolism technique. Those respiratory quotients were admittedly difficultly determined and continued over two-hour periods and were certainly not to be taken as seriously and strictly as he had taken them. I referred him to the Nutrition Laboratory's recent work in which the respiratory quotients do not appreciably alter during the morning. I stated that I felt his whole thesis entirely unsound, although it had certainly raised a point.

There was a dinner following the Physiological Society meeting and several rather interesting incidents came up. I happened to be at Pembrey's left and at the right of Sir Henry Dale. There was rather a good deal of noise going on and Pembrey rose and stated in a most casual way that he wanted to speak of the death of two of their honorary members, Rubner and Lusk. Dale turned to me and said, "I feel as if I ought to say something about poor old Lusk, but you know we English people don't do that sort of thing over here." Personally I was rather shocked to think nothing was said about Rubner and Lusk and there was not even a fraction of a moment's silence. I doubt if five per cent of the crowd heard what Pembrey said.

As was common with many personal conversations that I had, Dale almost immediately began to talk on the subject of prohibition, into which I came only half-heartedly until I was able to break away from it. A little later on in the evening he said, "You know we Englishmen are fed up with the American attitude toward prohibition. Apparently you people can talk of nothing else." I felt constrained to ask exactly who had raised the question at the table that evening.

6. **L. E. Bayliss, P. M. T. Kerridge and D. S. Russell.**—" The excretion of protein by the mammalian kidney."

7. **A. F. Hurst.**—" Some observations on the physiology of the colon."

[illegible]

8. **E. P. Poulton and T. W. Adams.**—" The fallacies of indirect calorimetry—fasting values."

9. **J. Argyll Campbell.**—(1) " Gas tensions in the gall bladder." (2) " Hypertrophy of the heart in acclimatisation to chronic carbon monoxide poisoning."

10. **R. C. Garry.**—" The effect of stimulation of the sacral visceral outflow and of the central ends of the somatic nerves on the caudal end of the large bowel."

11. **I. de Burgh Daly and W. V. Thorpe.**—" An isolated mammalian heart preparation capable of performing work for prolonged periods."

12. **M. Hill and A. S. Parkes.**—" Effects of hypophysectomy on ovulation and spermatogenesis in the ferret."

13. **R. C. Brock** (introduced by **M. S. Pembrey**).—" Absorption by the plasma."

14. **P. M. F. Bishop** (introduced by **M. S. Pembrey**).—" Results obtained with the Friedman reaction for pregnancy."

15. **W. A. Bain.**—" On the mode of action of vaso-motor nerves."

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## DINNER.

Dinner (6/-) will be held in the Students' Club at 7 p.m. Will all those who wish to attend please notify Prof. Pembrey on the enclosed card before Friday, October 21st.

# THE PHYSIOLOGICAL SOCIETY

## SEMI-ANNUAL MEETING—OCTOBER 22nd, 1932.

The Semi-Annual Meeting of the Society will be held in the Department of Physiology, Guy's Hospital Medical School, on Saturday, October 22nd, at 3 p.m.

### DEMONSTRATIONS.

**B. S. Platt and George Winfield.**—" An apparatus employing thermoionic valves for the measurement of potentials in high resistance cell systems."

**L. E. Bayliss, P. M. T. Kerridge and D. S. Russell.**—" Histological preparations of normal and perfused kidneys of dogs showing the glomerular excretion of egg albumin."

### COMMUNICATIONS (3—4 and 5—7 p.m.)

1. **Sylvia Dickinson and B. S. Platt.**—" Observations on some factors involved in the accurate measurement of glass electrode potentials."

2. **B. M. Leiberman, B. A. McSwiney and W. R. Spurrell.**—" A quantitative study of the influence of fat on gastric motility."

3. **E. D. Adrian.**—" Potential changes in nerve ganglia."

4. **D. F. Fraser-Harris.**—" A simple method of appreciating muscular sense."

5. **W. H. Newton and G. F. Marrian.**—" The action of oestrin on the isolated uterus."

Titles of all Demonstrations and Communications are to be sent to L. E. Bayliss, Esq., Department of Physiology, University College, Gower Street, London, W.C. 1, and all Abstracts for the Proceedings to Prof. E. D. Adrian, St. Chad's, Grange Road, Cambridge, by Monday, November 7th.

In order that the title of a paper may appear in the printed programme, it must be sent to Mr. Bayliss, even if an Abstract has been sent to Prof. Adrian (see Rule 40).

H. E. Roaf
A. V. Hill } *Honorary Secretaries.*

L. E. Bayliss, *Assistant Honorary Secretary.*

LB

<u>Lectures</u>. I gave two lectures on two successive days at Guy's Hospital, giving the "first" lecture on October 24th, and the "third" lecture on October 25th. Both were extremely well attended and followed by a certain amount of discussion. The "first" lecture was only moderately well received but the "third" went very well with an unusually good crowd. (There was an announcement of these two lectures.) I am always very much impressed to see this existing situation of apathy in England, and in general in London, with regard to metabolism researches, and yet my lectures are always attended by an interested audience.

UNIVERSITY OF LONDON

ADVANCED LECTURES IN PHYSIOLOGY

TWO LECTURES ON

(1) THE PHYSIOLOGY OF GREAT TORTOISES AND SNAKES AND ITS RELATION TO HUMAN PHYSIOLOGY:

and

(2) HUMAN BASAL METABOLISM IN THE LIGHT OF RECENT STUDIES.

UNIVERSITY OF LONDON

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and

(2) HUMAN BASAL METABOLISM IN THE LIGHT OF RECENT STUDIES.

will be given in the

Physiological Theatre

GUY'S HOSPITAL
(London Bridge, S.E.1.)

by

DR FRANCIS G. BENEDICT

(Director of the Nutrition Laboratory of the Carnegie Institution of Washington, Boston, Massachusetts, U.S.A.)

at 5 p.m. on

MONDAY, OCTOBER 24th, and TUESDAY, OCTOBER 25th, 1932.

The lectures are addressed to students of the University and to others interested in the subject.

ADMISSION FREE, WITHOUT TICKET.

S. J. WORSLEY,
Academic Registrar.

PKW.350
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19.10.32

LONDON AND CAMBRIDGE, ENGLAND.

Whale investigation.

While the practicability of making any studies on the sea mammals to contribute toward the most interesting problem of respiratory activity of the Cetacea seems at the time rather remote, I had occasion when in London and again in Cambridge to discuss the matter of the whale, especially with Dr. Stanley Kemp at the Discovery Expedition Offices. Dr. Kemp gave me first-hand information that seems to me should replace a great many of the legends with regard to whales. For example, the stories with regard to the great depths to which they go are probably wrong. He believes they rarely go down more than 150 fathom. On the other hand, there is the legend that whales have been found with mud on the end of their noses and, on this basis, people believe there are shoals in the middle of the ocean. Kemp believes that if they are submerged 40 minutes it is a very long time.

In discussing the question of the amount of force required by the whale when swimming at the surface, I talked with Professor Joseph Barcroft who is writing a book in which this discussion is to appear. Kemp said they rarely break the water and swim just below the surface with only a small portion of the back showing above the water, 12 feet wide and 20 feet long. This plays a very important role with the harpooners who must aim at this small spot.

In my discussion with Barcroft at Cambridge I pointed out to him that I felt he had a number of very gross errors in his assumptions. As I recall it, there were about four pages of mathematics and calculations dealing with the resistance of the whale and the horse-power, based upon calculations made for him by a lieutenant in the navy who had to deal with submarines and torpedo boats and based upon the traction on a line required to draw these submarines or similar under-water boats at certain speeds. Apparently one must discount the fabulous stories, particularly with regard to the distance to which whales sound, the speed of their travelling, and the length of time they remain submerged.

Furthermore, the harpoon is in no sense a serious instrument for the whale. It is quite likely he hardly feels it and he is not in any sense "fighting the harpoon" when he goes down. Of course a bomb lance or bomb harpoon is a different thing.

Kemp very kindly sent to the Laboratory a number of publications dealing with the whale and especially choice is a copy of Hinton's report.

OXFORD, ENGLAND.

University of Oxford, Department of Physiology.

Sir Charles Sherrington, Dr. C. Gordon Douglas, and Dr. J. G. Priestley.

I went to Oxford just for the day and found Sir Charles so extremely occupied that I could hardly see him and he could not attend my lecture. Douglas and Priestley were hospitality personified. In the laboratory Douglas, owing to illness and change in laboratory administration, had been doing very little research and things had a rather depressing, dirty, unkempt appearance. Douglas is very much against Hill's notion of a respiratory quotient of 1.0 during work. He expects to go on to the problem himself but at present is not working. To talk with Douglas is most stimulating. I always find him one of the most interesting men to discuss matters with that I know of in Europe. (See figures 80 and 81.)

In the laboratory at Oxford no metabolism work to speak of is going on, although Priestley still teaches every student the use of the Haldane gas-analysis apparatus.

Lecture. Douglas and Priestley had made every arrangement for the lecture, which was given on October 18th, and the room was packed, 140 people attending. I gave the "first" lecture, which was extremely appreciated. Douglas was very much impressed by the snake work and its potentialities, and later on Sherrington on the train told me he thought it opened a new field. The classic story in America that the Englishman is slow-witted and slow on the "up-take" is completely negated by me, especially at Oxford. I have personally made a slight experiment with the Oxford group. I find that no matter what "asides" I make, how obvious or how subtile, this group of young Oxford men always react with as much spontaneity as any American group. It is a pleasure to speak to such a keen, interested body. On the other hand, I must say that in striking contrast is the large group to whom I speak in Guy's Hospital, London. Precisely the same remarks and asides will fall without any reaction whatsoever on this second group. It may possibly be explained that the second group is more in earnest and of a more thoughtful, scientific nature, and all stirred up on the scientific side and hence any lighter remarks pass unheeded.

Sherrington. There is a rather interesting story, possibly a bit of gossip, with regard to Sir Charles Sherrington and the Nobel Prize. The following story came to me in a very reliable way. It seems there is a certain Stockholm paper which has always taken upon itself to get in contact by long-distance telephone immediately with the various Nobel Prize winners and thus this paper announces to them before any other means their having received the Prize. The paper immediately asks

Figure 80 . Oxford, England. Photograph of group at St. John's College. Left to right: Dr. C. Gordon Douglas, Professor Haldane, and Dr. J. G. Priestley.

Figure 81 . Oxford, England. Another photograph of group at St. John's College. Left to right: Dr. C. Gordon Douglas, Professor Haldane, Dr. J. G. Priestley, and Dr. Benedict.

the person for comments and prints them in its columns. When Sir Charles was awarded the Nobel Prize the telephone connection was made with Oxford and the reply was given that "Sir Charles Sherrington was not to be seen", even after the messenger had been told that the Nobel Prize had been awarded him. Anyone who knows Sherrington knows he has a most lovable disposition and he certainly would not in any way offend such a courtesy. The fact was that it was the action of a Diener who is a classic at Oxford and who literally stands with a club before Sherrington's door and will not allow him to be disturbed under any condition whatsoever. Going back to London on the train with Sir Charles I had the most stimulating and interesting conversation. A more lovable man hardly lives.

OXFORD, ENGLAND.

Professor J. S. Haldane.

A visit to Haldane's home was interesting as I had not seen them for some time. He was busy as usual, at this time studying free hydrogen! which he had noticed in peat bogs and 2 per cent of it in silos. Likewise he is studying the temperatures of silos and found them, due to fermentation, as high as 170° F. Haldane is very sure he has got free hydrogen. I felt uncertain about it. It certainly is a problem that should be checked up.

<u>General impressions of metabolism interest in Great Britain.</u>

I was extremely disappointed by the generally very low interest in metabolism in Great Britain. There were practically no high spots that stood out as especially stimulating or of value. Indeed I have several times remarked that I doubt very much if it is worth while visiting again the laboratories of Great Britain. Why England has always been so negligent of metabolism measurements I do not know.

I remember A. V. Hill and one or two others who said that they felt metabolism measurements should be left to American laboratories, but even in the hospitals practically no use is made of the basal metabolism measurement. It is surprising when one realizes what a tremendous, perhaps too great use is made of it in American and German hospitals.

BRUSSELS, BELGIUM.

Military Laboratory for Physical Study.

Dr. A. Govaerts.

Going directly from London to Brussels I immediately got in touch with Dr. Govaerts, who was the particular magnet taking me to Brussels. Dr. Govaerts is especially interested in the metabolism of exercise in the army and employs a Douglas bag during work. There are a certain group of physical exercises given the recruits, usually of short duration, and his problem was to find the oxygen consumption or metabolism during these exercises. He studied oxygen during work and then the after-effect of exercise until the oxygen had come down to within 15 per cent of the basal. I found that most of this material is done in very short periods. Commenting upon his earlier report that his basal values were very low compared to the Harris-Benedict standards, he tells me that after we wrote him from Boston he revised his technique and found he had been entirely wrong. He is a very serious-minded man, very dynamic, keenly interested in research and in the progress of gymnastics.

One of his problems was the effect of training on basal metabolism. Some claim that it is increased; E. C. Schneider maintains that it is lowered. Thus far Govaerts finds it about evenly divided, but really there are all too few observations from which to draw conclusions and practically only one experiment, that is, a short period before training and one period after training. I emphasized to him the importance of concentrating, say, on three people and having them studied three days before the training and three days after the training and thus get the drift. Then confirm it later with ten people if he has a definite indication.

He has a Gauthier-Benedict apparatus but he complained of the heat developed inside the spirometer. I emphasized to him that the Benedict apparatus was chiefly suited for clinical purposes but that his problem is a fundamental physiological one and he had better use the helmet set-up. He used the Dautrebande mask but had no ingoing air tube, so if there happens to be a back leak, carbon dioxide is lost into the room. He should use outdoor air as room air has an unknown percentage of carbon dioxide. Govaerts states that his subjects prefer the mouthpiece and two Sadd valves as we use them.

Govaerts uses the gas-analysis apparatus of Haldane and has two equipments: (1) Douglas bag from which the air is passed through a dry meter; (2) he uses a wet English meter and samples one-third of it in the first two minutes, one-third in the fourth and fifth minutes, and one-third in the eighth and ninth minutes. He uses mercury samplers and shakes them to insure mixture of samples. He was most interested in the metallized fabric used for the Douglas bag, so I told him to get in touch with Goldberg of Berlin who manufactures it. This metallic fabric prevents or greatly retards the diffusion of carbon dioxide out of the bag. Govaerts finds the American Haldane apparatus very expensive. The 3-way stopcocks last only one year. The Belgian-made Haldane is very much less expensive for they use a German certified burette. This equipment looked as good as the usual Haldane equipment but I told him that oxygen only was good enough for his work.

Govaerts had a lot of high respiratory quotients, from which comparisons were drawn, for presumably post-absorptive values. Consequently I inferred that (1) his technique was bad, or (2) the subjects ate beforehand. I emphasized that the respiratory quotient well determined is a good control on eating but only if well determined. On the other hand, the control of the soldiers' eating should be good. Govaerts finds a change in the blood pressure after training so he concludes that there is a better blood usage, better circulation, and hence (and I do not see it) maintains that increased metabolism is due not to increased combustion of tissues but to a better blood circulation. Although he emphasized this a number of times I did not get it at all clear. Later he stated that endocrine activity may be increased as a result of training and perhaps this is a part of the differences that he noted.

Govaerts has an extremely good mind. The morning he spends at the laboratory on research and the afternoon on private practice. He has very good ideas and an accuracy unusual in metabolism work, but he reports that there is little interest in metabolism in Belgium and France. His laboratory is about the only place where basals are done. He has an unusual opportunity to do normal work. As I pointed out to him, today most physicians are interested in the abnormal and pathological cases and have very little interest in normal subjects. But with the great mass of new recruits from the army available to him, recruits from which the obviously unfit have been singled out, he has an opportunity to do a great deal toward some of the problems that deal only with the normal human. Physicians and experts spend much time on muscular exercise in heart cases and other infirmities, but seldom realize that the normal values are not well known. I also pointed out that while I was anything but an enthusiast for the Douglas bag, I was a tremendous enthusiast for Douglas himself.

Govaerts had had considerable correspondence with Lindhard but, as he emphasized to me, without special outcome. I emphasized strongly that he visit Atzler and Lindhard. Atzler particularly I thought was so near that it was not impossible for Govaerts to visit there.

BRUSSELS, BELGIUM.

University of Brussels, Faculty of Medicine.

Solvay Institute of Physiology.

Professor J. Demoor and Professor E. J. Bigwood.

Professor Bigwood, in the laboratory of biochemistry, had an oxy-calorimeter and was very much disturbed because he could not get it to work. I found that through some inexplicable stupidity Collins had sent him too large a wire and consequently he could not heat this wire to incandescence and light the substance. Bigwood was especially interested in burning foods such as milk rather than meals or daily samples. He was burning glucose as a control and I suggested that he use saccharose on account of the humidity and possible changes in weight and absorption of water by the glucose. The Collins apparatus looked well and I gave Bigwood a number of points for the control of combustion. It is very distressing to find an apparatus of this type, well constructed, good in appearance, and yet, through some stupid error, have it fail to function and hence have the whole apparatus more or less condemned.

This new building houses now the Solvay Institute of Physiology which was formerly at the Parc Leopold. There were several divisions, including biochemistry and physiology. In the corridor there was a tablet giving the names of the directors, beginning with Heger, then Dr. Slosse, and then the present director, Professor Demoor.

The laboratory itself is fitted up in a splendid way. It is a good building and one finds a mixture of new apparatus and equipment that one could find almost nowhere else. Some of the men were much interested in surface temperature and they had an ingenious method of detaching the thermo-junction, making it possible to change various forms of junction by putting hooks or elbows between hard rubber plates, then having a plate of copper and a plate of constantan, and bolting all together with two nuts (milled head). This seems to be a practical idea if the composition of the constantan and of the wire is homogeneous. They were interested in studying the problem of diathermy. Thus by heating a piece of meat they could coagulate it, but in these studies they were interested in only about one degree difference.

There was in this building one of the most interesting installations for biophysics I have ever seen. The machine shop was splendid and many things had been constructed, such as kymographs, etc.

Professor Demoor was extremely kind. We had met him at the Congress and were thrown with him in a social way quite a good deal.

Lecture. The "first" lecture was given before the Faculty of Medicine, on October 27th. This was my first lecture in French and admittedly I was agitated and disturbed by it, as I had not lectured in French for four years. Fortunately I had been lucky enough to get in contact with a Frenchman who came to the hotel in London one Sunday morning and listened to me read my lecture and corrected my most obvious errors. In spite of my stage fright and agitation everyone maintained that the lecture went very well. There was a very large crowd, about 125 attending.

## BRUSSELS, BELGIUM.

### Fondation Reine Elisabeth (Institut de Recherches Médicales).

### Professor P. Nolf and Dr. L. Dautrebande.

I have never been satisfied with the way Dautrebande criticized the Nutrition Laboratory apparatus, as I felt that our correspondence over it had left the matter in a weak stage and he had not made honorable amends for the adverse criticisms. I had hoped to talk the matter over with Nolf, for Dautrebande was an assistant of Nolf. Although we had a most delightful social evening with Nolf I did not feel justified in bringing in the discussion at that time, but I had hoped Dautrebande would be at the dinner, when I could have had a few moments with him. However, he was not there.

Apparently from recent letters from Nolf, he feels Dautrebande "has not departed from the rules of courtesy which apply between authors." I shall now drop the whole thing. I still feel that Dautrebande is unjust but it is not worth the horse-power to keep up the controversy. The fact is that the affair between Boulitte, Dautrebande, and this Laboratory is one of the very rare instances, in the whole of Europe, of misunderstandings that are not cleared up.

AACHEN, GERMANY.

Rotamesser factory of Deutsche Rotawerke.

Our continued use of the rotamesser as an instrument of precision, combined with the fact that it seemingly had a wholly unreasonable price, made it desirable to investigate somewhat more thoroughly the manufacture and particularly the business methods of this very peculiar concern. Our attempts to do business with them by correspondence and otherwise have been most unsatisfactory. Letters were not answered, unreasonable prices were given on the rotamesser itself and particularly on any parts.

I began corresponding with the concern some time before leaving Brussels. Although no answer had been received I felt I should go over and spend the day and run the risk of their not being able to see me, so I took the train, went over to Aachen in a rainstorm, found the factory, and met Mr. Felix Meyer, the responsible person at the factory. Instantly I saw that I had to deal with a gentleman. He was of course more or less on the defensive and maintained that the phraseology of my letter was such that he expected me anyway and did not feel it necessary to reply. (He was CORRECT. I was wrong.) I had a most satisfactory talk with him.

In the first place he explained to me why the rotamesser was such an expensive instrument. It is not made by selecting glass tubes of unequal bore at one end and the other. What is done is the following.

A steel mandrel is prepared with a definite, mathematically calculated taper and then over this mandrel is placed a glass tube smaller in diameter. An electric heating coil like a solenoid is slipped over the vertical glass tube, heated, and then the glass softens and falls down and "settles" on the mandrel. It is then cooled, removed, and now the internal bore is precisely that of the exterior part of the mandrel. The preparation of the mandrel is very difficult and technically an important procedure. For example, it is not solid but hollow, which presumably is to allow for shrinkage and to permit the removal of the glass from it after it is cooled. At first the mandrel is a straight steel tube, perfectly cylindrical. In order to insure the regular taper this is not done on a machine but by very slowly and at a perfectly regular rate lowering the steel tube into a dilute, very dilute acid. I did not find out what kind of acid, probably hydrochloric. This means of course that as the lower end of the tube is longer in contact with the acid there would be more metal eaten away and thus a perfectly conical form secured. I saw about four different sizes of glass tubes for the small apparatus, three of which we have at the Laboratory. The graduations are made by an engine as are burettes.

Two forms of apparatus of interest to the Laboratory are furnished. One is the rotamesser and the other the rotaregulator. Of these the first is calibrated. For example, the weight of the little hard rubber float is known to one-tenth of a milligram and its diameter known to a fraction of a millimeter. This brought up the discussion with Mr. Meyer as to why they charged the Nutrition Laboratory five dollars for one of these floats that would not weigh any more than a diamond that would cost five dollars. It seems we ordered a float to replace one that had been broken, and Mr. Meyer states that we ordered a float for the rotamesser and they assumed we wished a float the exact duplicate of that broken, assuming it was to be used with a perfectly calibrated tube and hence to use the old calibrations the float must be the "exact counterpart". This called for a great deal of precision work. For example, Mr. Meyer had them bring out the time card of many months ago of the workman on this particular float and we found he had spent three and one-half hours on it. Mr. Meyer maintained that if we had wished a float of only "approximately" that size it could have been made up and sold to us for one dollar. It was stupid of us not to have ordered more specifically, for we did not have the rotamesser but simply a rotaregulator, the difference being that the first is calibrated and the second is not calibrated. Thus, a small difference in the weight of the float would be no damage to us as we must calibrate anyway. Finally I had this all successfully ironed out. He said he would be glad to furnish people with rotaregulators at ten dollars and an extra float at one dollar. This means of course that they must be subsequently calibrated.

After clearing up this matter of the rotamesser we went over the factory, but I was disappointed that I could not see the rotamesser mandrel or the tubes or the process of fusing. The mandrels, he stated, are made by one firm in Europe, the only one who can grind them closely enough. This word "grind" implies something more than the acid treatment but seemed to be a secret, so I could ask no questions about it. The glass tubes for the rotamesser are not made in Aachen. I came to the conclusion that the rotamesser is of the same precision as a chemical balance and should be judged by that and not as 6 pennyworth of glass tubing. They also manufacture ampoules, test tubes, syringes, etc., with a most interesting automatic machine. It really was worth the trip to meet this man, an ingenious engineer and designer.

## AMSTERDAM, HOLLAND.

### Nederlandsch Instituut voor Volksvoeding.

### Professor E. C. Van Leersum.

This institute of Van Leersum's has completely petered out. He is now an old man at 70 and far from well. He was playing around with vitamins and was interested in the hydrogen-ion concentration in lemon juice, but probably will do no more real work. He says no one in Amsterdam is doing any metabolism research. Van Leersum still occupies a few rooms by courtesy in the Institute of Hygiene and has a few pieces of apparatus in the dark basement. It seems unfortunate that the library of his institute is in a cave and while I have always seen some person there cataloging or indexing, I imagine the library is hardly used at all.

It is rather pathetic to see the interest Van Leersum has and desire to do things and his utter failure to accomplish things owing to the impossibility of his working with other people. He is an idealist with very bad judgment.

UTRECHT, HOLLAND.

Physiological Institute.

Professor A. K. Noyons.

Of course one of the most interesting points in connection with my visit to Utrecht was to follow up the paper that Professor Noyons gave at the Congress in Rome on the question of determination of water vapor in the air by his electric hygrometer. It practically boils down to the use of an electric wet and dry bulb thermometer. He has 25 pairs of junctions and the air must pass over these junctions at the minimum rate of 1 meter per second, or better, 1 1/2 meters. He emphasized repeatedly that the velocity of ventilation passing the wet bulb had better be between 1 and 2 meters per second. This is correct for either electric or wet and dry bulb mercury thermometers. For his wet bulb thermometer covering he uses an old handkerchief. (See figures 82 and 83.)

For an adult cat Noyons had a circular chamber about 30 cm. in diameter and 50 cm. long, and passed 1000 liters per hour through it. He uses a double ventilating system, drawing the air over two rotamessers and over his electrical psychrometers, one for the ingoing air and one for the outcoming air. (See figures 84 and 85.) Hence in this case he has 1000 liters per hour passing through both arms of his psychrometer apparatus. Thus the humidity of the ingoing air is measured by passing it over the electrical psychrometer at exactly the same rate of flow as in the case of the outcoming air. He had a hygrometer and a rotamesser in line to measure the ventilation. He was interested in the possibility of using this method for studying the psychomotoric reflex in man, using the water vapor from the skin as a measure and keeping the lungs outside, that is, having a mouthpiece to go outside the chamber. The electric psychrometer is extraordinarily sensitive. Thus, when you have the room air passing through it and the electric system balanced, holding a moist finger in the air current for only a second will send the galvanometer off scale. Noyons uses a potentiometer to balance the thermo-electric force of the junctions. He makes much use of the formula $f = E - 1/2\ (T - t_c)\ \frac{B}{755}$, and he also makes much use of Jelineks' tables. (See figure 86.)

The electric hygrometer was very cleverly made. By using a 40 micro farad condenser, a 220-volt circuit, and constantin and manganin wires and a spark to jump across they fused the ends of the wires. In making these junctions they must select days when the humidity is not too high. It is a complicated and expensive equipment, but the whole thing looked very pretty and extremely well made, as is everything in Noyons' laboratory.

Thinking that the apparatus would have a definite use in our important study of water-vapor output of ruminants, horses, and pigs at Durham, and being much impressed by Noyons' report of it, I ordered two pairs of these thermo-junctions, two different sizes, from Noyons' mechanic and I ordered the Wheatstone bridge recommended by Noyons from

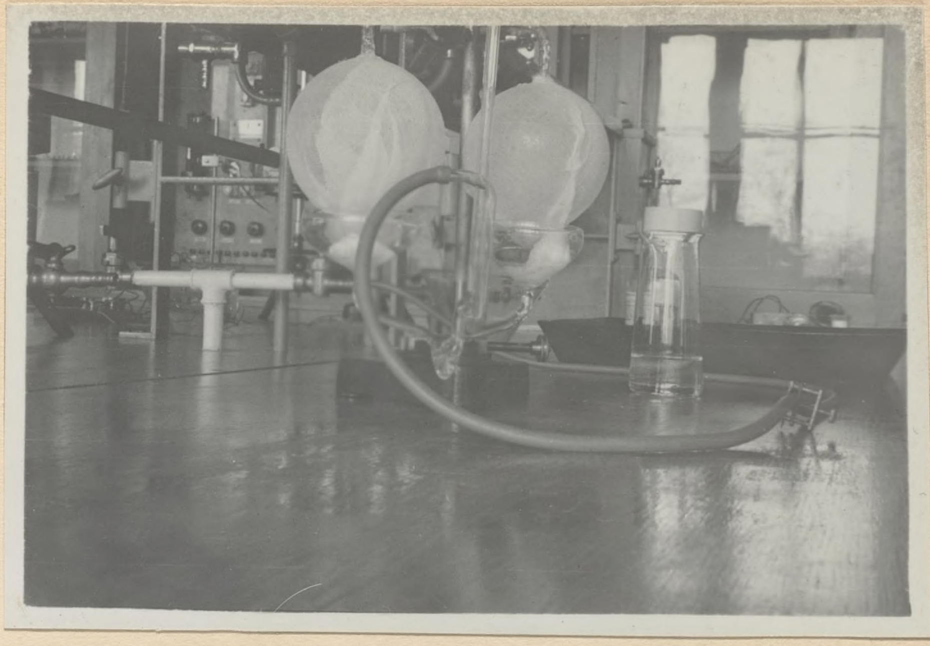


Figure 82. Utrecht, Holland. Physiological Institute. Noyons' apparatus for studying wet bulb thermometry, using large glass globes covered with fabric and completely moistened.

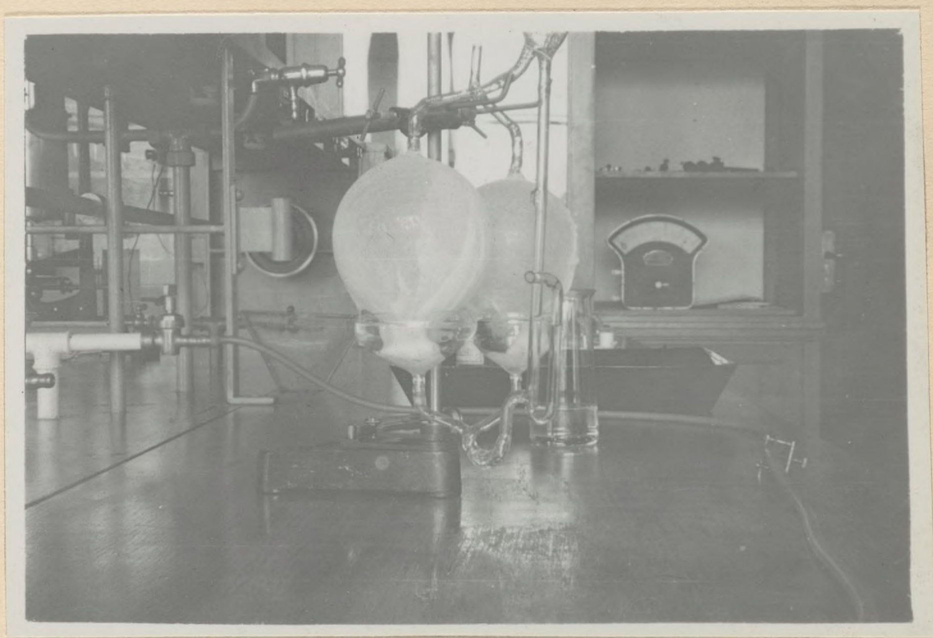


Figure 83. Utrecht, Holland. Physiological Institute. Another view of Noyons' apparatus for studying wet bulb thermometry. Glass bulbs covered with fabric and completely moistened.

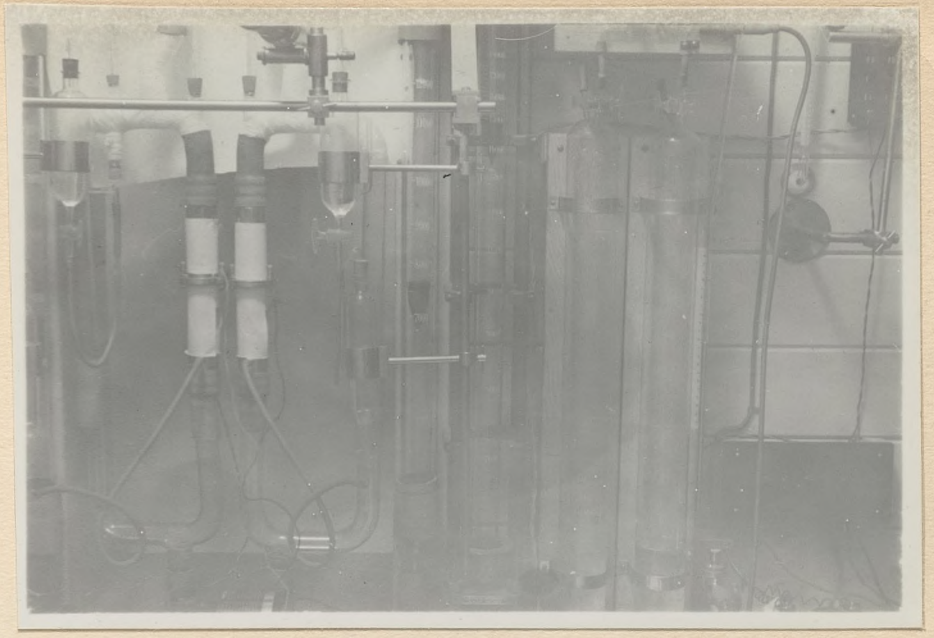


Figure 84. Utrecht, Holland. Physiological Institute, Laboratory of Professor Noyons. Part of the complicated system of measuring ingoing air, dimly shown by rotamesser. About the center of the picture is the electrical psychrometer covered with white paper tubes; a little to the left of the picture is the sampling device for air.

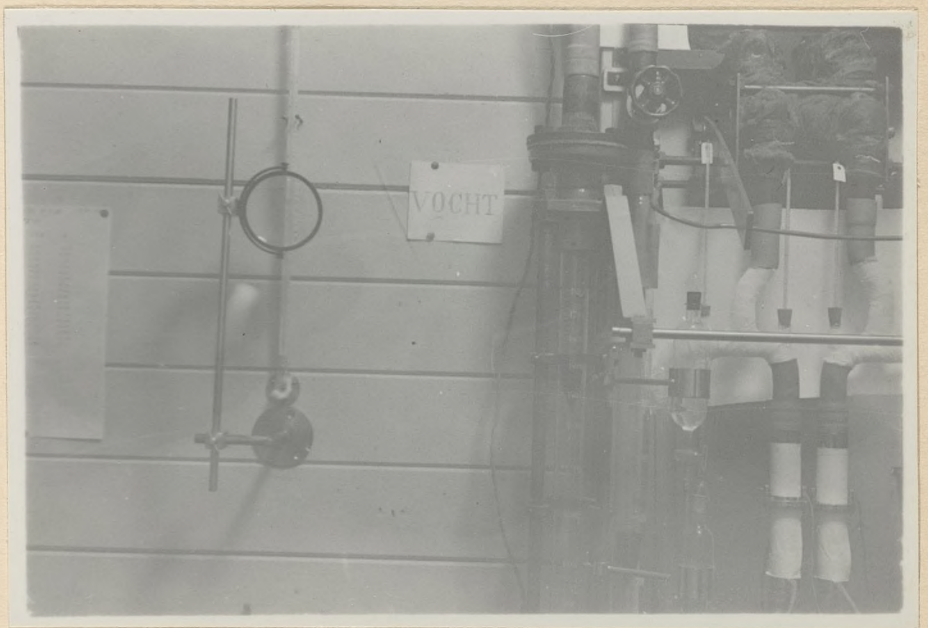



Figure 85. Utrecht, Holland. Physiological Institute. Another view of Noyons' air measuring and metering device, showing rotamesser dimly and a part of the electrical psychrometer.

Figure 86 . Utrecht, Holland. Physiological Institute, Laboratory of Professor A. K. Noyons. Metabolism apparatus for getting the hygrometry of an animal. The animal is placed in the chamber shown very darkly, and one rotamesser and one psychrometer of the Noyons type measures the water output of the animal.

Hartmann and Braun at Frankfurt. From the standpoint of construction it is a perfect piece of work. Actual tests at the Nutrition Laboratory (in the spring of 1933) have failed to give us, under the conditions under which we were working, the results we hoped for, and at the time of dictating this report (June 19, 1933) our Boston experience with this apparatus would make its immediate application to Durham seem very uncertain. While there could be conditions where comparisons between the electric psychrometer and wet and dry bulb are good, when we attempted to compare the electric psychrometer with the actual water vapor in the air by absorbing it in sulphuric acid and weighing it we found very great inconsistencies and only when the humidity was very high did the electric psychrometer agree at all with the absorbed and weighed water.

At the time of writing, therefore, it appears as if there were a tremendous difference between the Nutrition Laboratory and Noyons with regard to the use of the electric psychrometer in determining water vapor. The experiments made by Coropatchinsky and myself (spring of 1933) apparently were very unsatisfactory so far as the electric psychrometer is concerned. I was under tremendous pressure at the Laboratory at the time but I still feel that I followed the thing closely enough to avoid any obvious pitfalls. Before challenging Professor Noyons I have felt it best to let the results lie fallow until they can be digested and gone over again. At the moment of writing I feel that we are correct and that Noyons is wrong. I hope that I am wrong.

If Noyons is wrong, it is a striking illustration of a man who has too many things to do and too few associates upon whom to rely to discuss the problems. In this sense I feel there is a striking similarity between Noyons and myself. The criticism with the Nutrition Laboratory is that there are only two independent research workers in it. This is a very grave criticism. It was less marked when Dr. Smith and Dr. Miles were there, but certainly since their departure it is a fact, and it is a very grave error. Noyons has too many things to do. He has something of a shifting population in the matter of assistants, and outside of Jongbloed there are few important independent research workers.

Method for getting alveolar air sample. This seemed to me rather ingenious. The subject blows through a rubber tube about 10 to 12 mm. in diameter into the equivalent of a bathing cap can with no dead space. As this rubber cap expands due to the expired air it reaches a certain point of distention and trips a trigger that releases a weight attached to the handle of an inverted glass blood (Luer) syringe with about 200 c.c. capacity. This weight falls and a sample of the air from the tube leading from the mouth to the expansion chamber is instantly drawn into the syringe. The point was that there was practically no dead space, only that existing from the mouth to the deflated bag. The syringe was connected between the mouth and the bag so that when the bag was filled up and the thing tripped, the syringe could get only air from the mouth or pipe, thus getting alveolar air. This syringe can be purchased in Utrecht for about three dollars and should find considerable use with us. I think we should try it out for holding gases in place of the metal syringe, and in fact it should be able to take the place of the metal syringe and possibly the small Haldane glass mercury samplers. Noyons did not have a bathing cap such as we have but had a rather thin-walled, yellowish rubber bag. It seemed like a very pure grade of rubber and is used for the interior of water polo balls. These bags are of four different sizes and are made in Germany.

Noyons is very much interested in the Carpenter gas-analysis apparatus and I think we should furnish him with all the information we have. He is very strong on the use of 1 1/2 per cent tartaric acid solution over the mercury in the Haldane and states that Haldane himself recommends it. Noyons also had, as I noticed before, the A. V. Hill spiral on the top of the Haldane pyro-pipette. I have never seen it anywhere else.

<u>Noyons' automatic gas-analysis apparatus</u>. Noyons had an apparatus at the Boston Congress, in 1929, which was supposed to show carbon-dioxide determinations by shaking the sample and noting the decrease in pressure. He <u>now</u> has a very elaborate model of this with much glassware and nickel plating and an automatic shaking device, which he hopes to use to analyze the air coming out of the respiration chamber. (See figures 87 and 88) The apparatus is tilted back and forth and he has a water mantel about 10 cm. in diameter. He claims that theoretically there is a readable figure of 0.001, but it was interesting to note that he was checking or controlling this against an ordinary Haldane with hardly a certainty of 0.01 per cent. His idea was that if he made <u>many analyses</u> the <u>average</u> would be correct. He is using chamber air with about 1 per cent carbon dioxide, as at first.

The thing did not especially appeal to me, for even if successful it is frightfully complicated although rather fool-proof, as there are no levels to set, etc. Noyons himself felt that the absence of oxygen determinations made the apparatus of little value but he is now, he believes, right on the heels of successful oxygen determinations. I did not think this apparatus was any better or any more practicable than our double pump system. It may possibly give 0.002, as he claims, but it is not yet proved and there are no alcohol checks as yet. Personally I doubt very much if oxygen determinations are ever successful by this principle. Noyons of course hopes that they will be and I feel that if anybody can do it Noyons can.

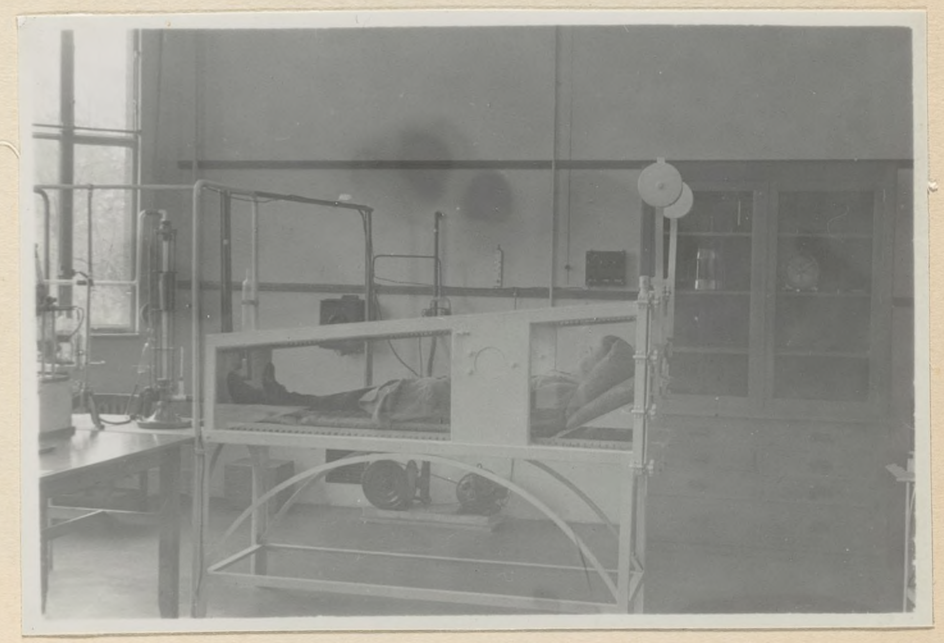


Figure 87 . Utrecht, Holland. Physiological Institute. Noyons' glass-walled cot chamber with subject inside. At the left one sees very faintly Noyons' new shaking apparatus for carbon dioxide.

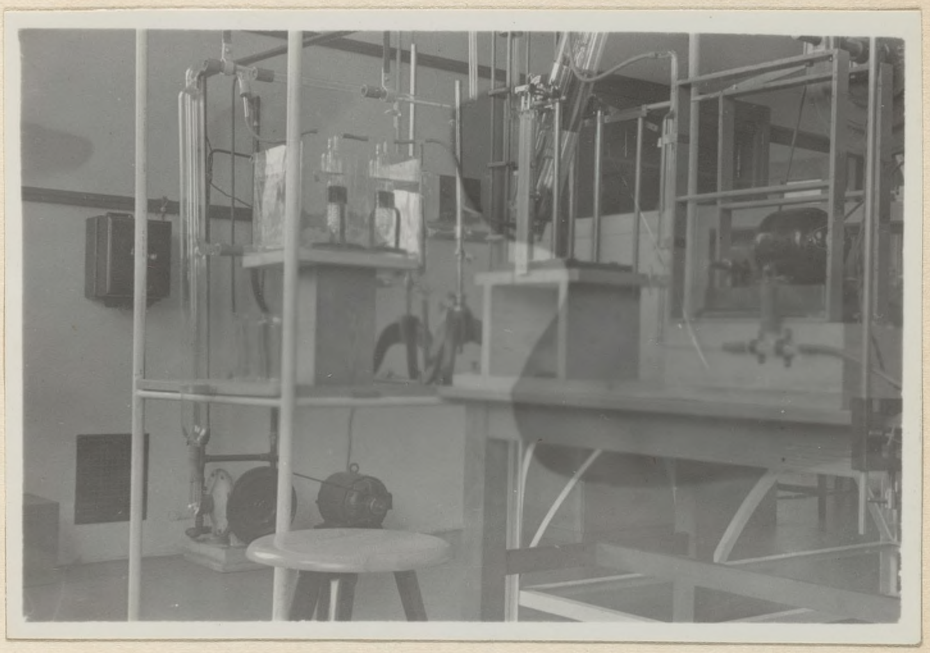


Figure 88. Utrecht, Holland. Physiological Institute, Professor A. K. Noyons. Tilting gas-analysis apparatus of Noyons, shown at the upper center. The gas for comparison is freed from carbon dioxide in the two bubbling tubes at the left, immersed in water. In the background at the right, difficultly seen, is the glass-walled respiration chamber.

Respiratory glass chamber. The large glass chamber (see figures 89 and 90) was still being used and samples are taken at the end of each hour, but the samples are not aliquoted. Noyons determines the carbon dioxide only and was developing an apparatus to determine carbon dioxide with very great accuracy, approximating that of the Carpenter or Krogh apparatus. This was extremely complicated (120 c.c. sample) and ingeniously constructed. (See description of this apparatus on page 150.)

Experiments on noise. Noyons emphasized the important relation of accoustic stimuli to increased metabolism. Thus, he was very particular to have his large respiration chamber of glass 1 cm. thick, that is, his chamber for man, practically "sound-proof", for he considers this very important. He spoke about the influence of noise on the "vorhorn" of the medulla and emphasized that complete quiet was most important for animals and humans.

Calorimeter. Noyons' calorimeter is a huge affair. (See figure 91.) It has double compensation chambers and I have described it on other trips, notably the calorimeter at Louvain and at Utrecht in the laboratory of Magnus, which I saw in 1929. I went inside the chamber. (See figure 92.) It seems rather forbidding with the subject facing the light, which would be rather uncomfortable for some patients. On the other hand, it is extremely quiet, with hardly any noise. A large, flat, circular fan in the ceiling gently stirs up the air in the chamber. No gas analyses were made as he was determining only heat given off, but he was taking into account the heat of water vapor. Formerly Noyons used to vaporize water artificially in the compensation chamber to correspond to the amount of water vapor given off by man, but now he determines the water vapor in both chambers and measures the difference. (See figures 93 and 94.) There were thermo-junctions all over the calorimeter, on the top and side walls but <u>not on the bottom</u>. Of course this applies likewise to the compensation chamber. I am not quite sure whether they were thermo-junctions or resistance thermometers, but in any case the bottom is not compensated. There had been added some niceties since I saw it the last time but the calorimeter had not been altered in any way so far as fundamental principle is concerned.

Specific dynamic action in the cat. The specific dynamic action in the cat was determined, and after cutting the splanchnic nerve all disappears but (1) there is a pronounced diarrhoea. The question is, does the cat absorb the protein and fat? (2) There is no control on the activity as I see it. This whole matter raised quite a question with Noyons and his associate who had done his work. The associate had left and was in another hospital but came over and spent several hours with us. Personally I was not convinced that there was anything of importance in his thesis. I did not think the thing was well done. It would be important if it were true. He had the respiration chamber in a water bath at 30° C. and employed the Knipping method of absorbing the carbon dioxide, liberating it later with sulphuric acid, and writing the oxygen on the kymograph as usual. (See figures 95, 96, 97, 98, and 99.)

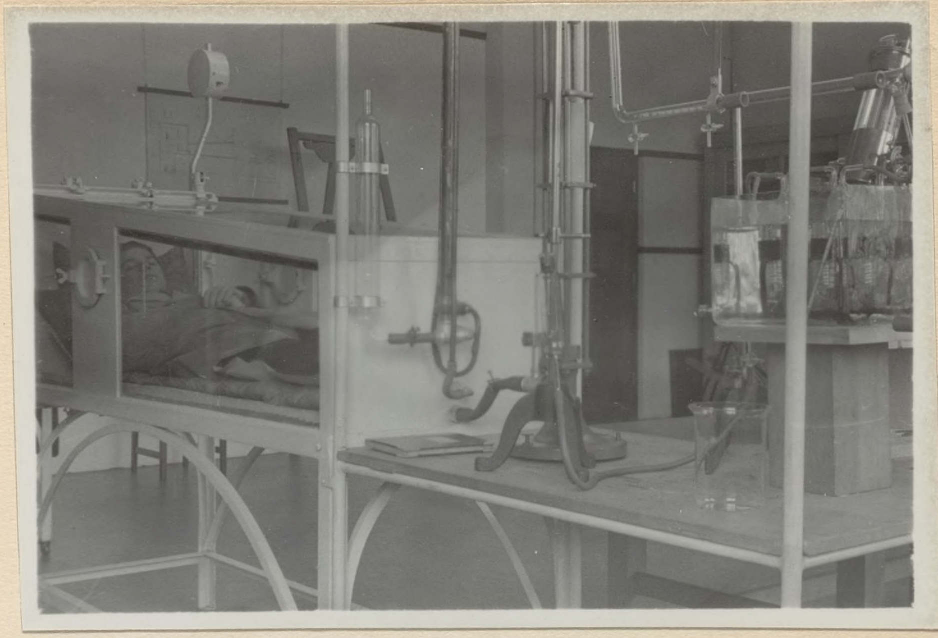


Figure 89 . Utrecht, Holland. Physiological Institute. Noyons' glass-walled cot chamber. Near the foot is a rotamesser.

Figure 90 . Utrecht, Holland. Physiological Institute. Another view of Noyons' glass-walled cot chamber.

Figure 91 . Utrecht, Holland. Physiological Institute. Double door entrance to Noyons' electric compensation calorimeter. The door at the right is where the patient is put in on the bed.

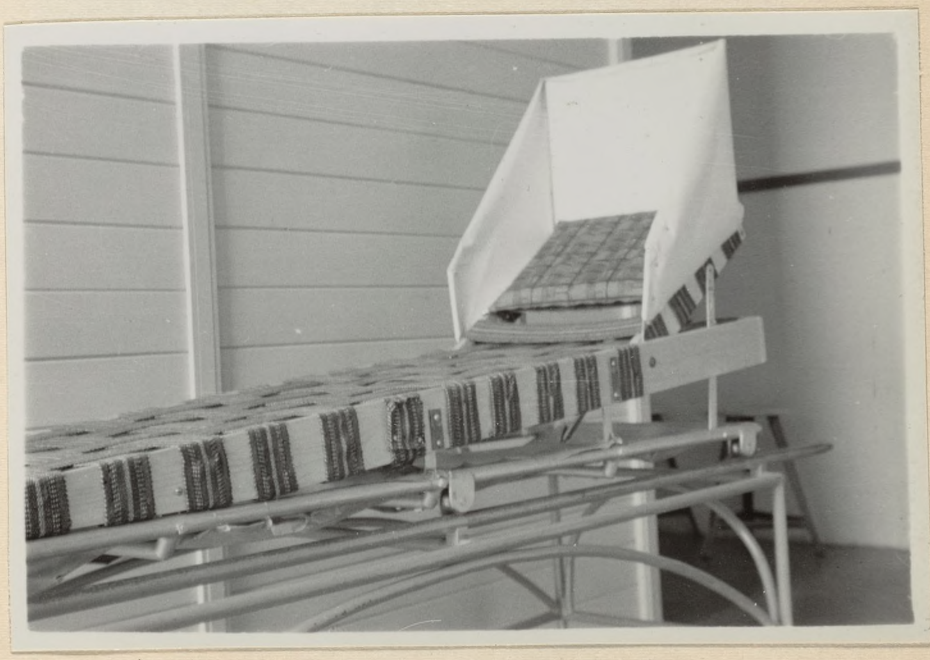


Figure 92. Utrecht, Holland. Physiological Institute. Bed in Noyons' calorimeter.

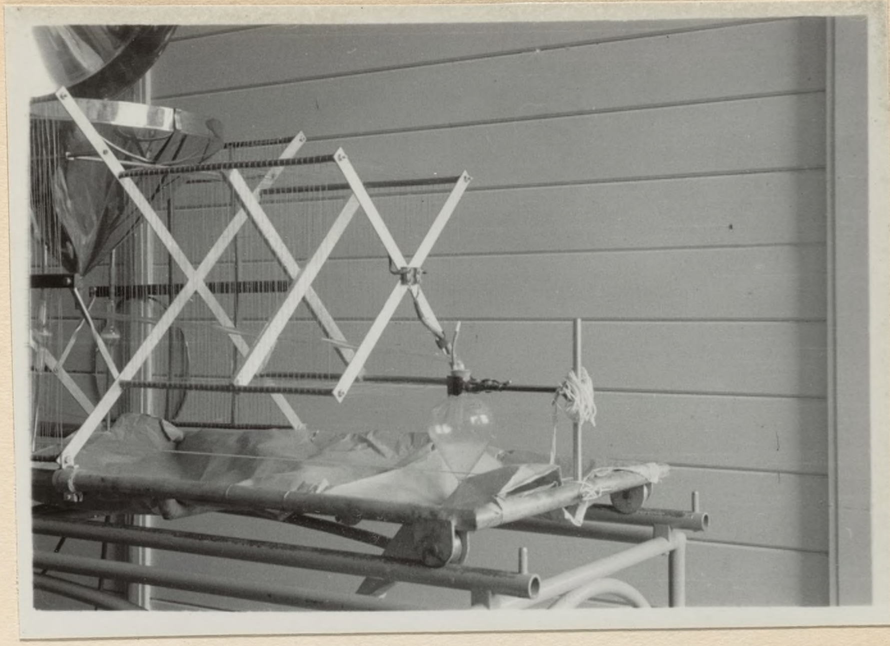


Figure 93 . Utrecht, Holland. Physiological Institute, Laboratory of Professor Noyons. Electric heater for compensation chamber and water vaporizing pan in the shape of a funnel. This is not so much used as formerly.

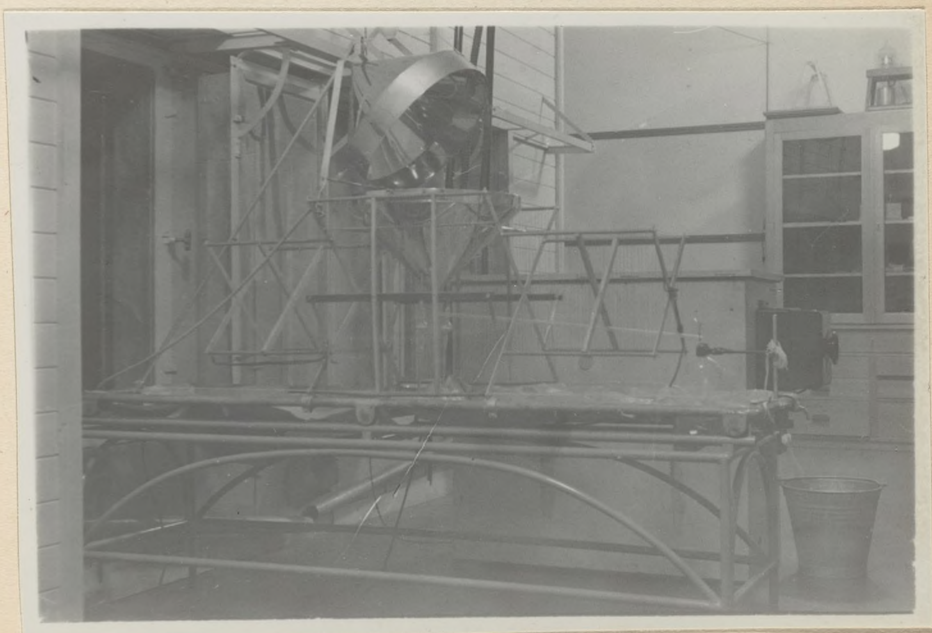


Figure 94 . Utrecht, Holland. Physiological Institute. Electric artificial man and water vaporizing apparatus for compensation chamber of Noyons' calorimeter.

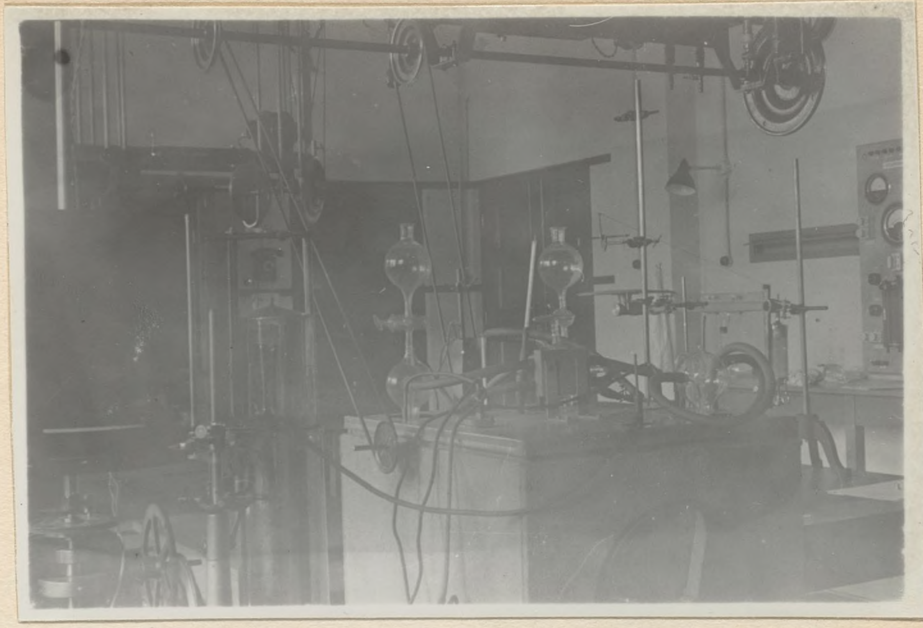


Figure 95 . Utrecht, Holland. Physiological Institute. Noyons' respiration chamber for cats, in which the oxygen is written on a spirometer and the carbon dioxide absorbed in caustic potash but subsequently released by acid, in accordance with the principle of Knipping.

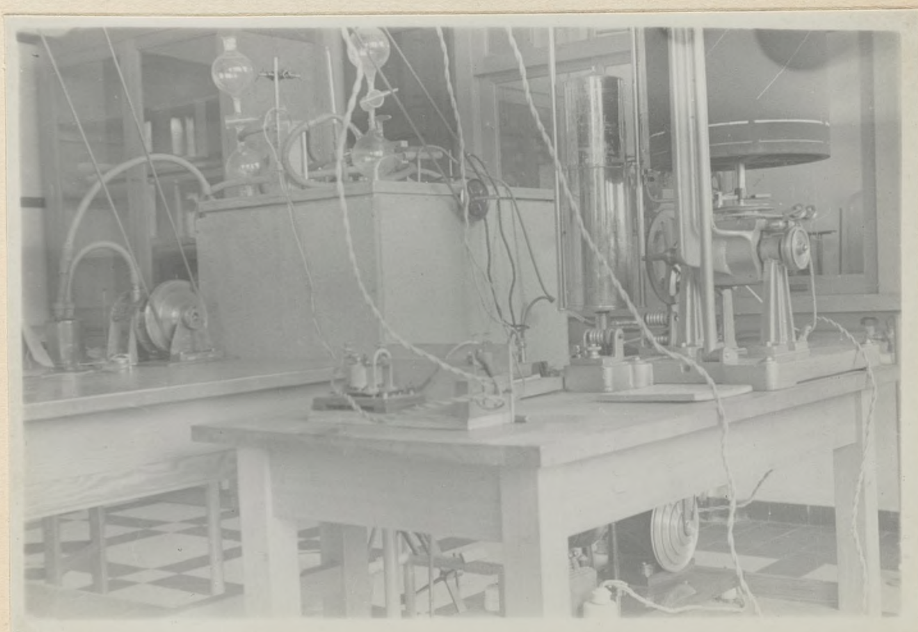


Figure 96 . Utrecht, Holland. Physiological Institute. Another view of Noyons' cat respiration chamber. In the background is the spirometer and kymograph. Also the activity is recorded here.

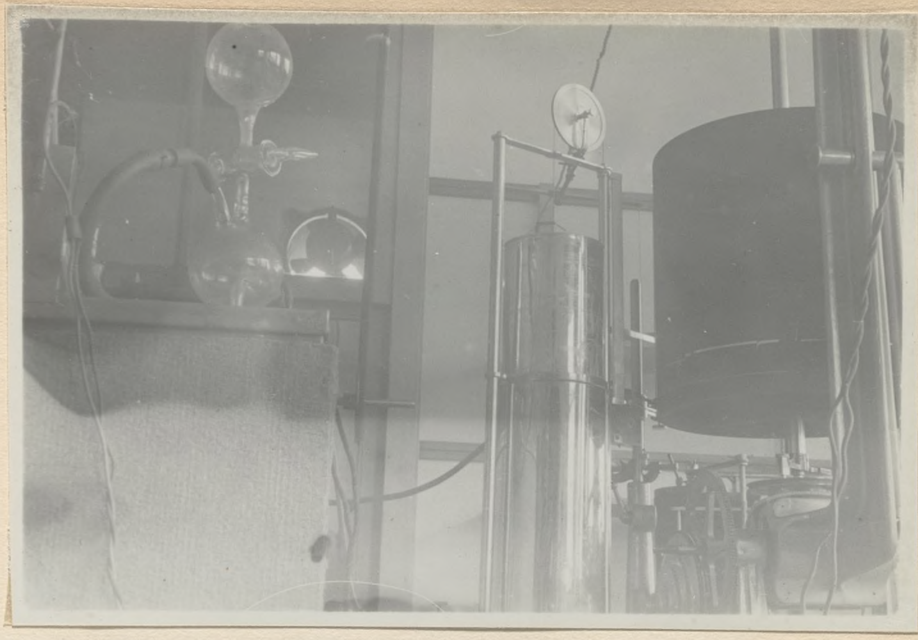


Figure 97. Utrecht, Holland. Physiological Institute. Another view of Noyons' cat respiration apparatus, showing spirometer, kymograph, and acid vessel for releasing carbon dioxide.

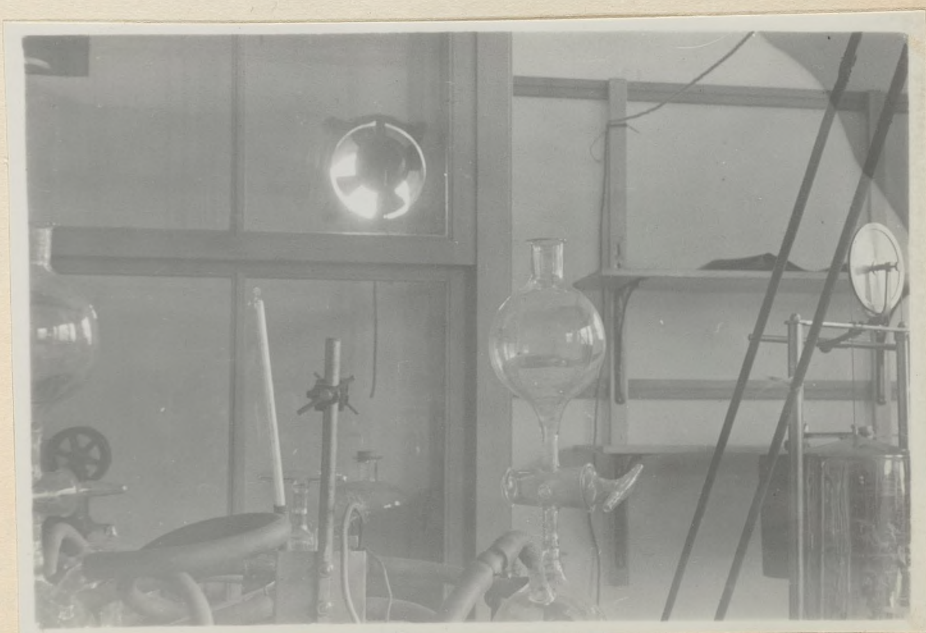


Figure 98. Utrecht, Holland. Physiological Institute. A closer view of acid vessel for releasing carbon dioxide in Noyons' cat respiration chamber.

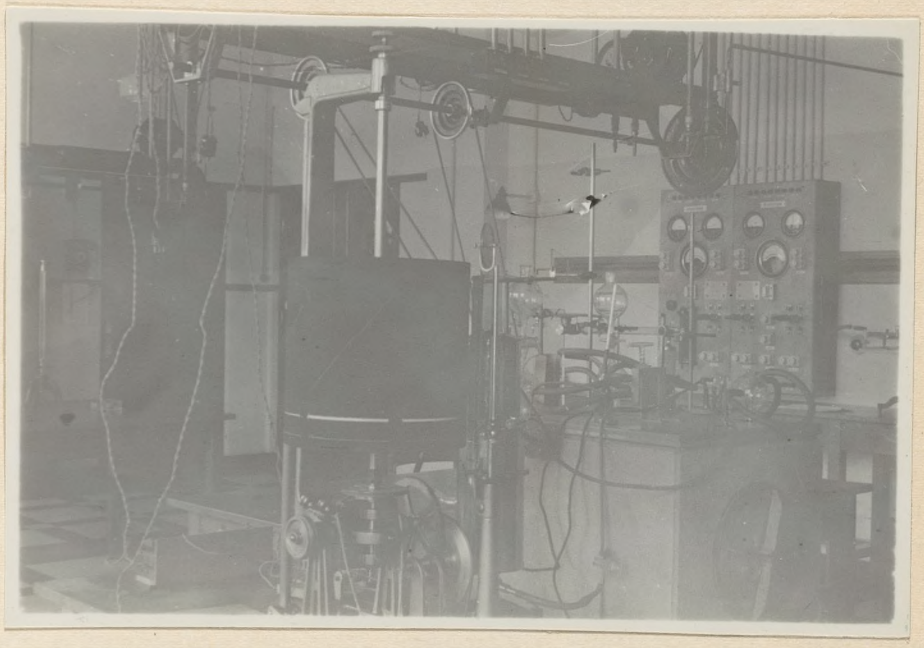


Figure 99 . Utrecht, Holland. Physiological Institute, Laboratory of Professor A. K. Noyons. Photograph showing kymograph tracing for Noyons' apparatus for studying the metabolism of a cat. The respiration chamber is shown at the right.

Noyons lays great stress on cats as animals but later evidence I saw led me to believe that cats were probably no better than any other animal. Noyons admitted that a well trained dog was the best and had a great deal to say about the dogs of Szarkall at Dortmund.

Insensible perspiration. After Noyons' discussion at Rome where he rather condemned the insensible perspiration as of any use for indicating the metabolism, owing to the fact that the insensible perspiration changed enormously with the humidity, I was rather keen to see just the conditions under which he worked. In many ways they were ideal. He had a specially constructed room in which the humidity and the temperature could be altered almost at will and very rapidly. Suspended from the arm of a very fine Sauter balance in this room was the bed upon which the subject lay, but I was literally astounded that the subject was lying upon a horsehair mattress!!! While in desert air we can easily assume a different vaporization than in saturated air, I do not think that Noyons has proved that there is a great change in the insensible perspiration of a human with very considerable changes in the humidity when every other factor is ruled out. It is true that in maximum humidity there would theoretically be no insensible perspiration and yet with maximum dryness it might be enormous. I pointed out that the variations in humidity, normal in America at least, ranged between 40 and 80 per cent. I also emphasized that there was a difference between sudden changes in humidity and slow.

Apparently realizing that the hair mattress was technically bad, Noyons had followed the suggestions of some German workers and attempted to make "corrections" for weight of bed and clothing itself. It is impossible, especially with the sudden changes with which he works, to assume that the bed and clothing attain equilibrium in a short time and of course one should wait until they are in equilibrium before beginning the real insensible perspiration measurements. Undoubtedly after such an experiment is begun at the end of the period the whole system is in equilibrium but the whole difficulty is what to deduct for the amount of absorption or amount given off, in case of dryness during the experiment. I feel that Noyons as well as a good many others, notably Heller in Berlin, has missed out badly on this point. As I have written one of them, they are trying to determine the insensible perspiration when they are weighing on the bed with the man the equivalent of 10,000 hair hygrometers.

I had a job to follow up the history of this upholstered mattress or horsehair mattress. Since I found precisely the same mattress in other laboratories it looked strongly as if Sauter had furnished mattresses with the balance and I had rather an acrimonious correspondence on this point, but he convinced me that he had never sold or recommended a horsehair mattress to anybody. Apparently what Noyons and others do is to take a mattress from a hospital bed without giving any thought whatsoever to its structure.

Physiology of aviation. Dr. Jongbloed, one of Noyons' associates, is extremely interested in aviation, and in the cellar of the building there is a large wheel on which a man or animal can be rotated, with the head out and the feet toward the center or with the head in and the feet out, and under these conditions measure the electro-cardiogram simultaneously with the blood pressure. (See figure 100.) A smaller wheel was used for dogs and rabbits, noting the effect of centrifugal force, etc. Jongbloed seemed to be a very keen, clever chap who felt his studies would make some important contribution to the physiology of aviation. Certainly there were many extremely clever devices in connection with this equipment.

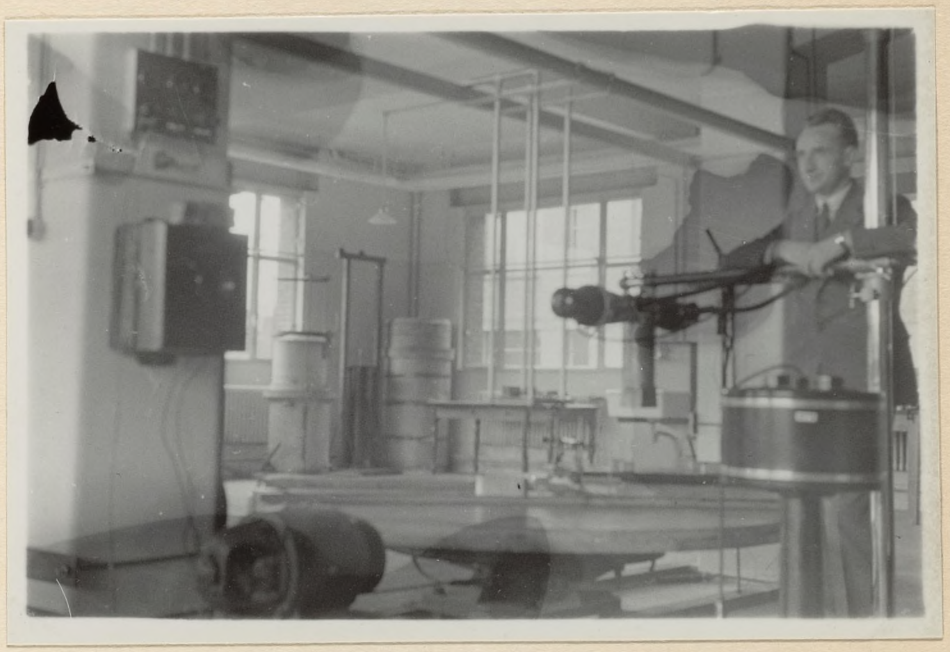


Figure 100. Utrecht, Holland. Physiological Institute. Basement of Noyons' laboratory. Dr. Jongbloed and the rotating table for studying problems in aviation.

General discussion of Noyons. Noyons' laboratory, as always, is an extremely interesting place to visit. A copy of the plan of his institute is appended herewith. (See page 164.) Noyons himself is a most charming personality, a good physicist, a good physiologist, a good physician -- rather an unusual combination. But he is certainly overworked and is himself well aware of the fact. He is one of those individuals who is chronically unhappy. I think he enjoys being unhappy.

Someone has said that Noyons spends all his time in constructing beautiful apparatus and then never uses it. I think there is something in that, although it is to be said that his large compensation calorimeter and the glass-walled respiration chamber are installed in the clinic of Heymans van den Bergh in Utrecht, and as a matter of fact in the calorimeter at Noyons' laboratory they were making direct heat measurements nearly every morning on patients from some of the clinics.

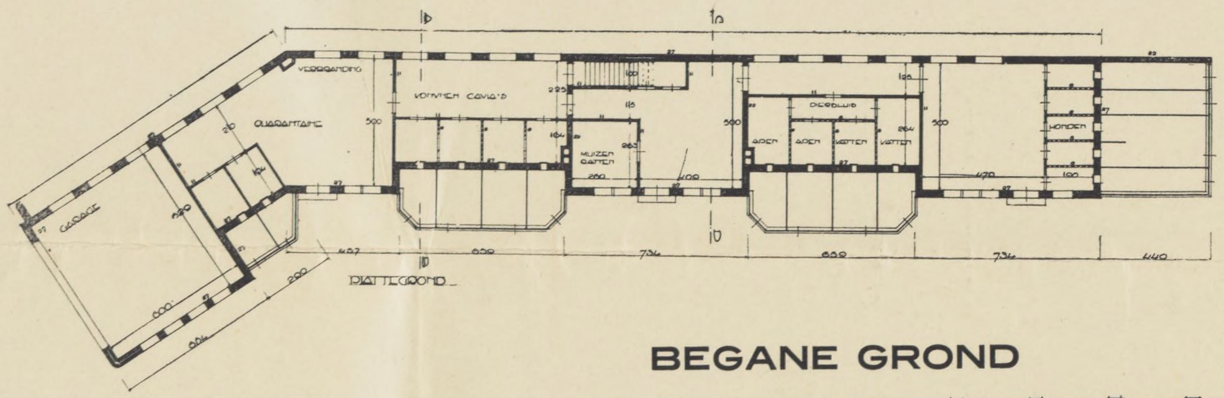
It is a great pleasure to me to find him established in his own laboratory, although the funds for his work are of course limited. I think from the standpoint of construction and ingenuity of design probably there are more things of interest in this laboratory than in any other in Europe, but from the standpoint of experiments in progress there is relatively little going on. (See figures 101, 102, 103, 104, 105, 106, and 107.)

Plan of Noyons' Inst

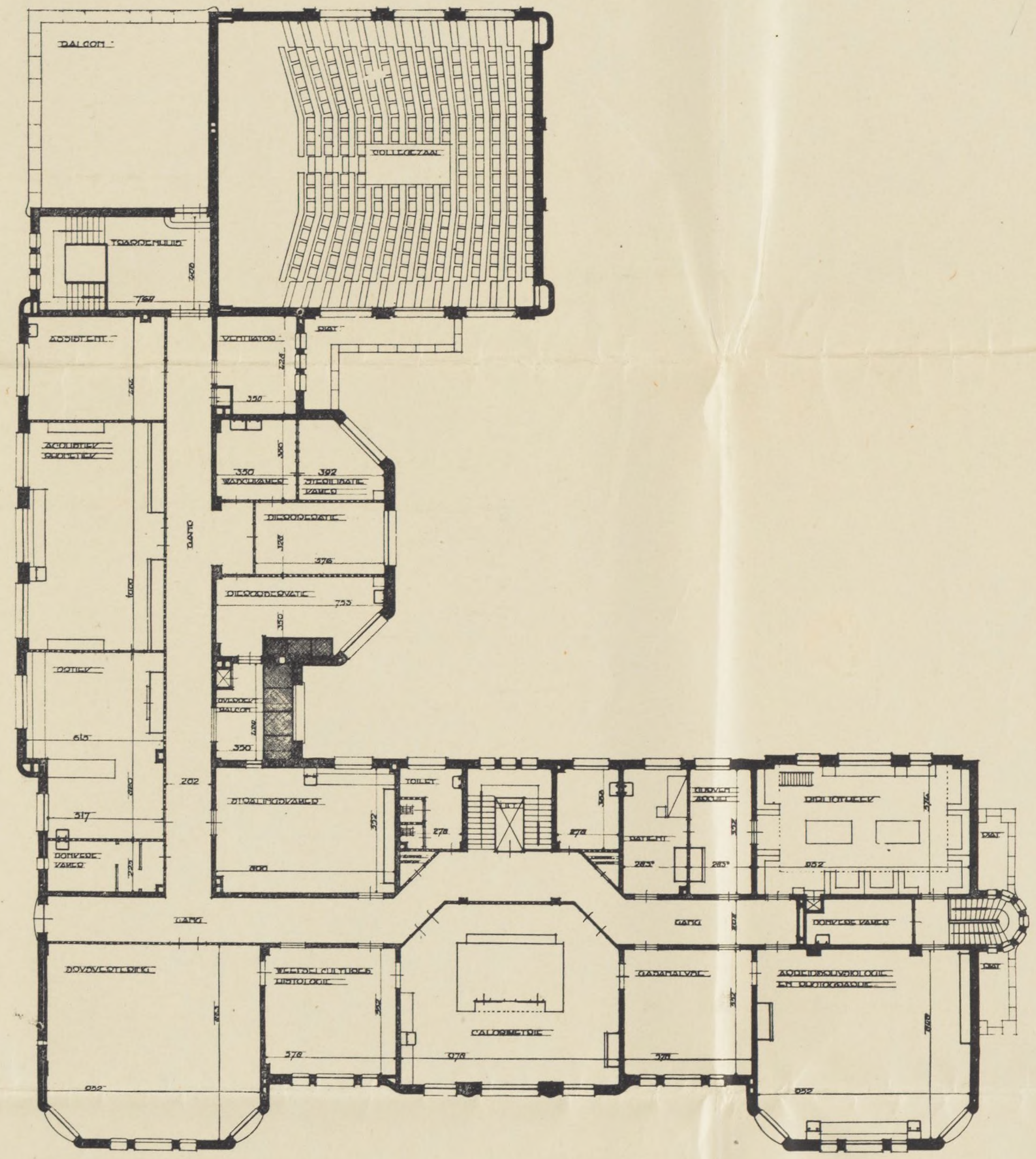
# PHYSIOLOGISCH LABORATORIUM
## DER RIJKSUNIVERSITEIT TE UTRECHT
6 NOVEMBER 1930

SCHAAL 1 A 300

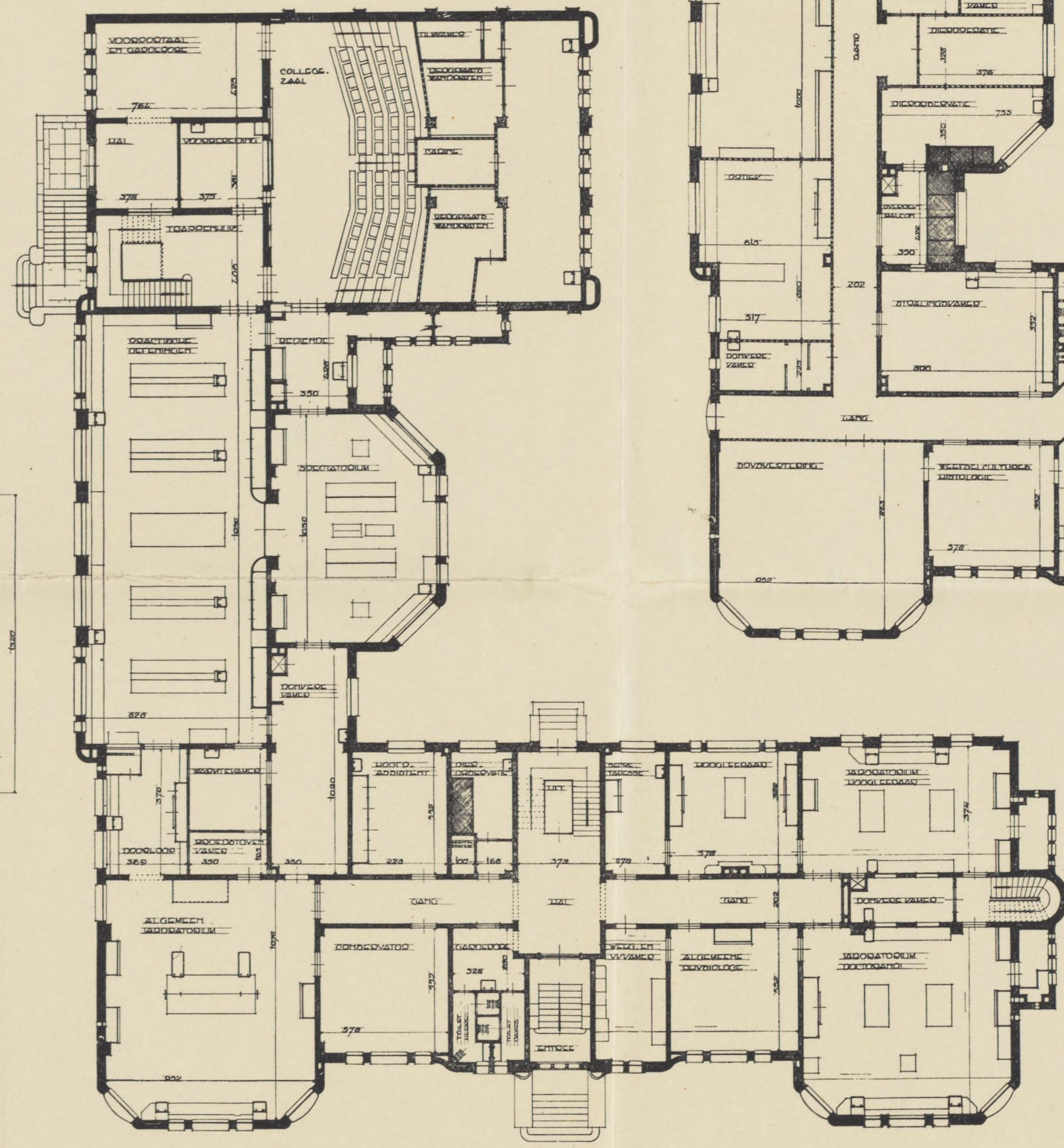
### DIERENSTALLEN



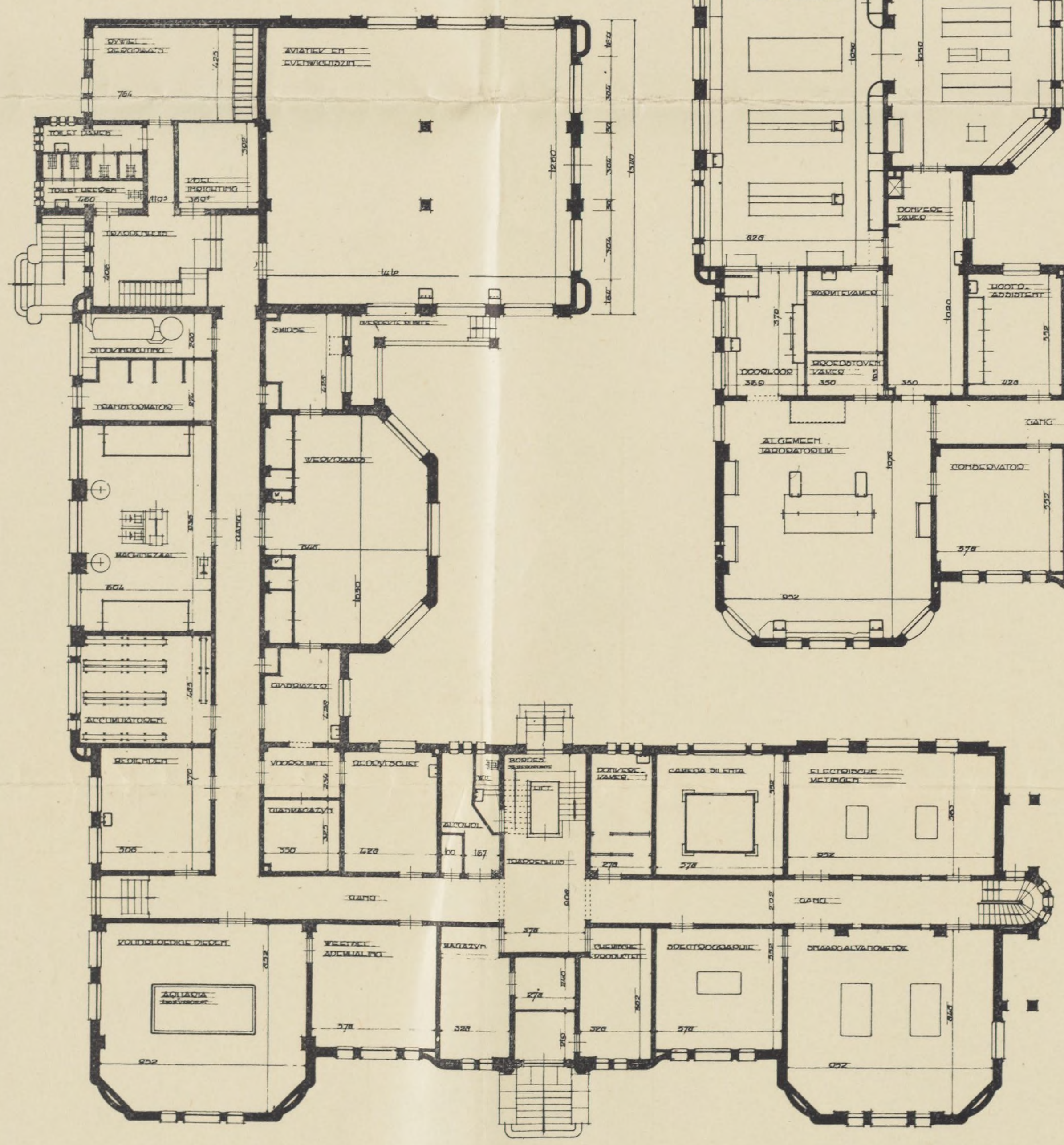
### VERDIEPING



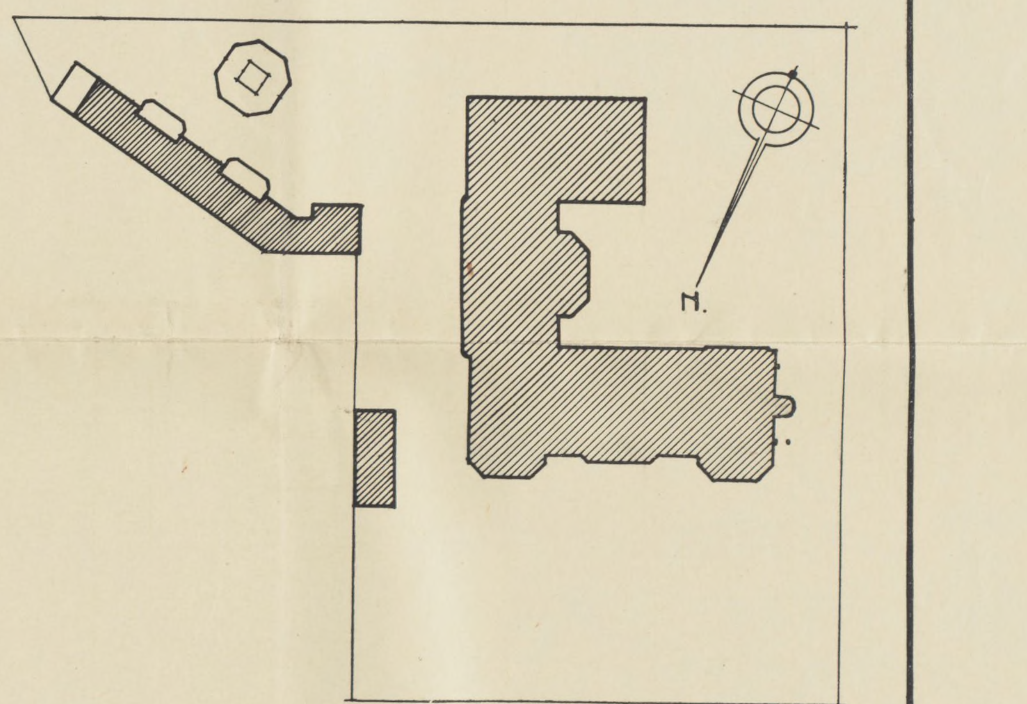
### BEGANE GROND



### KELDER



### SITUATIE SCHAAL 1 A 1000



A. A. KOK. ARCH.

Figure 101 . Utrecht, Holland. Physiological Institute, Laboratory of Professor A. K. Noyons. Nutrition Laboratory respiration apparatus for rats or pigeons, as employed by Noyons in Utrecht. In the center of the picture are seen the well known soda-lime and calcium chloride cans.

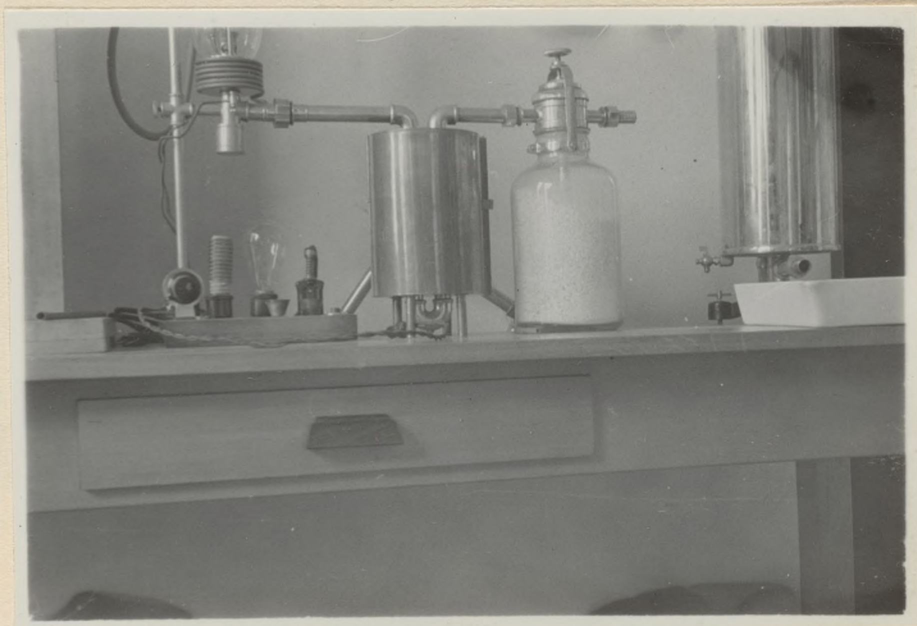


Figure 102. Utrecht, Holland. Physiological Institute. Oxy-calorimeter purchased by Noyons from Collins.

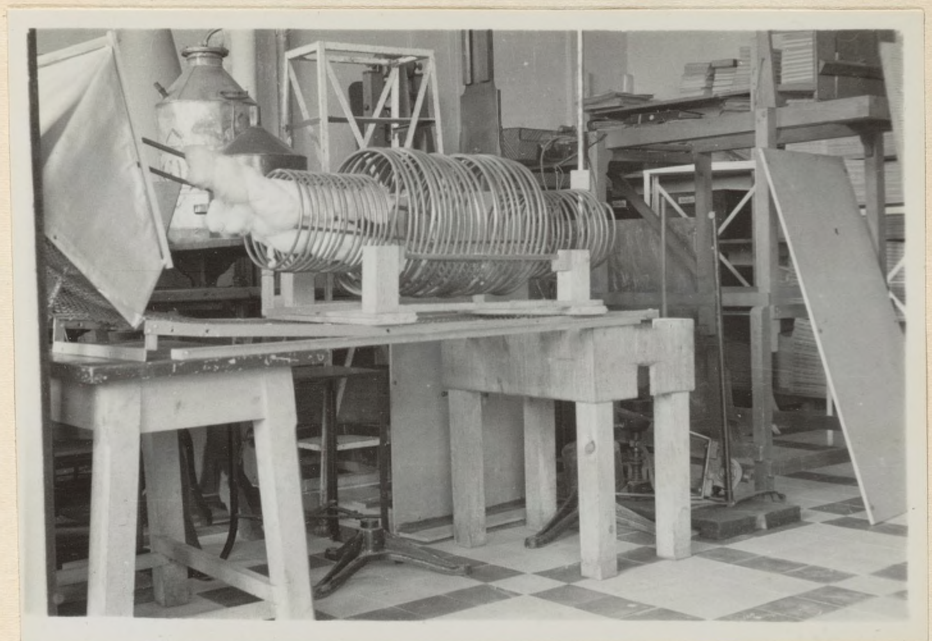


Figure 103. Utrecht, Holland. Physiological Institute, Laboratory of Professor A. K. Noyons. Storage room in Noyons' laboratory, where he has made use of various types of coils of wire to make artificial men for his respiration experiments.

Figure 104. Utrecht, Holland. Physiological Institute. Professor A. K. Noyons at his writing table.

Figure 105. Utrecht, Holland. Physiological Institute. Professor A. K. Noyons on the steps of his Institute.

Figure 106. Utrecht, Holland. Physiological Institute. Professor A. K. Noyons on the steps of his Institute.

Figure 107. Utrecht, Holland. Physiological Institute. Closer view of Noyons on the steps of his Institute.

UTRECHT, HOLLAND.

Veterinary High School.

Professor J. Roos.

A thing of rather considerable importance to me was the fact that Noyons pointed out that, commenting upon the special facilitations of the horse in standing, someone had found that in standing the "action currents" were low in the muscles of the horse's front legs, also with the cow they are very much less than normal, but in the hind legs the action is as pronounced as usual.

Noyons told me that Professor Roos had worked on this subject. I think subsequently Professor Roos told me that he was not at all the first to do it, but I have not yet been able to locate who did it originally.

UTRECHT, HOLLAND.

Lectures.

On October 31st the "third" lecture was given at Noyons' Institute, where there were about 170. (There was an announcement of this lecture.)

I gave the "second" lecture at the Veterinary High School, on the evening of November 1st. There was a very interested and attentive audience of about 125.

Clippings from the newspaper, "Utrechtsch Dagblad", regarding my two lectures in Utrecht, are appended herewith. (See pages 174 and 175.)

# MEDISCHE FACULTEIT

VAN HET

# UTRECHTSCH STUDENTEN-CORPS

## BUITENGEWONE VERGADERING

op MAANDAG 31OCTOBER 8 UUR

in COLLEGEZAAL VAN HET PHySIOLOGISCH INSTITUUT

SPREKER:

ONDERWERP:

# MEDISCHE FACULTEIT

VAN HET

# UTRECHTSCH STUDENTEN-CORPS

**BUITENGEWONE VERGADERING**

op MAANDAG 31OCTOBER 8 UUR

in COLLEGEZAAL VAN HET PHySIOLOGISCH INSTITUUT

**SPREKER:** DR.F.G.BENEDICT UIT BOSTON

**ONDERWERP:** BASALMETABOLISM AND RE CENT STUDIES

**Toegang vrij voor alle belangstellenden.**

**Namens het Faculteitsbestuur**

J. Schalij

Med. Fac. Ut. ab-actis

BASAAL METABOLISME BIJ DEN MENSCH NAAR AANLEIDING

VAN RECENTE ONDERZOEKINGEN

Inleiding.
- Factoren het basaal metabolisme beinvloedend.
  - Leeftijd,gewicht,lengte,geslacht.
  - Opvattingen omtrent oppervlakte.
  - Slaap
  - Ras
- Deugdelijk gebleken methoden.

Leeftijds factor.
- Serie waarnemingen bij 3 mannen en 1 vrouw gedurende een periode van 17-24 jaar.
- Waarnemingen bij oudere vrouwen tusschen 66 en 86 jaar.

Ras factor.
- Chineesche en Japansche vrouwen
- Zuid-Indische vrouwen(Tamils en Maleiers)
- Australische inboorlingen (mannen en vrouwen uit Kokata)
- Mannelijke Maya's,Indianen in Yucatan (3 expedities).
- In Amerika geboren Chineesche meisjes te Boston
- Ras invloed bij duiven.

Onveranderlijkheid van het metabolisme van dag tot dag.
- Waarnemingen bij een man gedurende een periode van 3 weken.

Slaap-invloed.
- ... gedurende slaap.

BASAAL METABOLISME BIJ DEN MENSCH NAAR AANLEIDING

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  Ras
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 In Amerika geboren Chineesche meisjes te Boston
 Ras invloed bij duiven.
Onveranderlijkheid van het metabolisme van dag tot dag.
 Waarnemingen bij een man gedurende een periode van 3 weken.
Slaap-invloed.
 Waarnemingen bij een Zuid-Indische vrouw gedurende slaap.
 Waarnemingen gedurende hypnotische slaap.
Helm-apparaat in verschillende vormen.
 Helm als ademhalingstoestel
 Gesloten systeem voor $O_2$ alleen
  Spirometer curven.
 Gesloten systeem zoowel voor $CO_2$ als $O_2$
  Proeven bij rust
  Proeven bij werk.
 Open systeem zoowel voor $CO_2$ als $O_2$
  Proeven bij rust
  Proeven bij werk.
 Proeven bij geestelijke inspanning
 Proeven bij spierarbeid en hoog zuurstofgehalte
 Proeven bij spierarbeid en alcohol
 Waarnemingen in de herstelperiode volgende op spierarbeid.
Waarde voor de kliniek van metingen van de
  Perspiratie (perspiratio insensibilis)
  Grondstofwisseling; het eenvoudige helmrotameter-apparaat stelt in staat snel metingen te verrichten van de grondstofwisseling ter aanwending in ziekenhuizen.
  Temperatuur van uitgeademde lucht; de mogelijke bruikbaarheid daarvan als een maatstaf in de kliniek.

Dagblad - Utrecht - Nov, 1.

AVONDBLAD
Dinsdag 1 November 1932

# UTRECHTSCH PROV. EN STED. DAGBLAD

136e Jaargang No. 257

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*HISTORISCHE FIGUREN.*

Maurits, moest, zoo er gevaar dreigde, aan de Staten-Generaal worden overhandigd. Dat gevaar echter broeide helaas in het verborgen. De dagen gingen voorbij. 's Prinsen

de voorzichtigheid, don Emmanuel naar Schiedam te laten vertrekken: men kon niet weten, waartoe Maurits' toorn zou leiden.
Den 20en November kwam Maurits terug

**WEERSGESTELDHEID**
BAROMETERSTAND.

# GRONDSTOFWISSELING VAN MENSCH EN DIER

## LEZING VAN Dr. BENEDICT

*Het vergelijkende onderzoek op physiologisch gebied*

## Belangrijke beschouwing

In aansluiting aan de belangrijke lezing, die Dr. F. G. Benedict uit Boston Maandagavond heeft gehouden voor de medische studenten over het vraagstuk der grondstofwisseling in het licht der jongste onderzoekingen (zie het verslag in het avondblad van Dinsdag), heeft deze stofwisselingsphysioloog Dinsdagavond in het loboratorium voor physiologie van de veeartsenijkundige faculteit (prof. dr. Roos) een zeer belangwekkende lezing gehouden voor de veeartsenijkundige studenten, over vergelijkende grondstofwisselingsonderzoekingen bij mensch en dier.

Spr. wil een overzicht geven van de uitkomsten van de nieuwere vergelijkende physiologische onderzoekingen in het Nuketroin Institute in Boston onder zijn leiding. Vele Europeesche collega's hebben medewerking verleend. In zijn inrichting staat het onderzoek bij den mensch op den voorgrond, maar wordt toch groote waarde gehecht aan het vergelijkende onderzoek, waarop men ook is ingericht. Vele groote lijnen hebben mensch en dier op physiologisch gebied gemeen.

Spr. begon met een kort overzicht van de ontwikkeling van de techniek (Pettenhofen, Voigt, Atwater en Benedict). De toestellen zijn hoe langer hoe eenvoudiger geworden.

De moeilijkheid bij het vergelijkend onderzoek is, dat de afmetingen der dieren zoo verschillend zijn en men dus voor muizen, konijnen, menschen en paarden toestellen van geheel verschillenden omvang moet hebben. Al die toestellen moeten zeer nauwkeurig geijkt worden, om vergelijkende uitkomsten te krijgen. Ook de gebruikte chemicaliën en de gebruikte lucht moet regelmatig worden onderzocht. Het zuurstofgehalte van de heete lucht van Boston bedroeg 20.94 %, evenals in Baltimore, het koolzuurgehalte bedroeg resp. 0.031 en 0.034. Van alle dieren wordt de hitteproductie in verhouding tot lichaamsgewicht en oppervlakte bepaald. Daarbij bleek dat er geen overeenstemming bestond tusschen de uit het lichaamsgewicht berekende oppervlakte (derde machtswortel uit het kwadraat van het gewicht vermenigvuldigd met een cemtanih) en de werkelijke oppervlakte. Ook bleek het noodig de dieren bij verschillende temperaturen te bestudeeren, speciaal bij de critische temperatuur, waarbij de activiteit het geringst is. Muizen zijn bijzonder actief ook bij lage temperaturen en daarom geen gemakkelijke proefdieren. De muis bleek per etmaal per M². 1200 caloriën te produceeren (dus meer dan de mensch). Het minimum schijnt bij de muis bij 400 caloriën te liggen. Dit dier blijkt subnormale temperaturen te hebben. Wanneer men meer dieren tegelijk in het toestel plaatst, treden „samenscholingen" op, waardoor het gemeenschappelijk oppervlak een wijziging ondergaat, die echter geen invloed op de grondstofwisseling bleek te hebben. Bij ratten bedraagt het minimum per M². per etmaal ongeveer 700 caloriën. Gedurende vasten ziet men eerst de warmteproductie sterk dalen, om later weer te stijgen (als minimum werd bij ratten een peil van ± 370 bereikt).

Zeer samengesteld zijn de verhoudingen bij vogels. Sommige bereiken enorme hooge waarden (tot 2500 caloriën per M². oppervlakte pet etmaal). Deze proeven gaan met bijzondere moeilijkheden gepaard en de uitkomsten zijn in hooge mate afhankelijk van de temperatuur. Ook bestaan er groote seizoen- en rasverschillen. De rui heeft grooten invloed en brengt belangrijke schommelingen te weeg. Kippen bleken zich goed voor het onderzoek te leenen; hier kreeg men cijfers van 650—750 caloriën per M². oppervlakte per dag, (bij de kip iets hooger dan bij de haan). De pluimage bleek van grooten invloed op de uitstraling; bij de eene soort is de perspiratis inscusibilis veel grooter dan bij den ander. De pooten vormen een zône van veel warmteverlies. Bij de gans kreeg men cijfers tusschen 700 en 800. Op groote schaal heeft men proeven met marmotten genomen (500—600) en met een groep apen, die moeilijk te onderzoeken bleken vanwege hun slimheid en beweeglijkheid. Voor paarden gebruikt men een toestel met een capaciteit van 9000 Liter! De lucht circuleert met een snelheid van eenige honderden Liters per minuut. Deze cijfers geven een denkbeeld van de technische verhoudingen bij dergelijke proeven, die zeer kostbaar zijn.

Spr. gaf beelden van de toestellen die men uitgewerkt heeft voor het doorloopend opvangen van urine en ontlasting van aan de proefnemingen onderworpen proefdieren. Deze uitscheidigs-producte moeten regelmatig kwantitatief en kwalitatief onderzocht worden. Een moeilijkheid is dat sommige groote dieren nu eens liggen en dan weer staan en de proeven in een van beide posities moeten worden uitgevoerd. Bij staande dieren is de grondstofwisseling 15 % hooger dan bij het liggende dier. Bij runderen krijgt men cijfers van 1400—1600 caloriën per M². per etmaal. Tal van factoren oefenen een grooten invloed op het peil dezer grondstofwisseling uit. Met name ook de voeding (specifieke dynaminale werking van bepaalde voedingsmiddelen). Bij het paard worden cijfers van 2000—2700 caloriën per M². per etmaal gevonden (gemiddeld 2300 in staande houding en vermoedelijk liggend 15 % lager, dus 1950). De perspiratis insensibilis kan gemakkelijk bij deze dieren worden vastgesteld. Ook het warmteverlies door uitstraling vertoont bij de dieren gelijksoortige verhoudingen als bij den mensch (de dieren verliezen op deze wijze ongeveer 15 %, tegen de mensch 25 %). Uit een en ander blijkt, dat de verhoudingen op dit gebied zeer samengesteld zijn. Spr. wees op de schitterende technische uitrusting, die hij bij prof. Noyons in Utrecht gevonden had en zijn technisch inzicht weer verdiept had. Vergelijkende onderzoekingen behooren met name bij gelijke temperatuur te worden verricht en bij minimale activiteit van de dieren. Bij voorkeur moeten dieren van beide geslachten worden gebruikt. Een van de belangrijkste factoren blijkt dus weer de lichaamsoppervlakte te zijn. Bij kleine dieren is het aantal caloriën per K.G. veel hooger dan bij grootere dieren (128 bij de muis, 20 bij varken en rund). Omgekeerd stijgt het aantal caloriën per M². per etmaal bij toenemende grootte van het dier. De uniformiteit die men hoopte te vinden, blijkt echter te ontbreken. Er bestaan groote verschillen: per K.G. van 1 : 10 per M². van 1 : 5. Deze onderzoekingen moeten verder worden voortgezet.

Dagblad- Utrecht
Nov. 2

AVONDBLAD
Woensdag 2 November 1932

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BUITENLANDSCH OVERZICHT



BULGAARSCHE MAATREGELEN

# OVER HET BASALE METABOLISME

## LEZING VAN PROF. BENEDICT

*Onderzoekingen op het gebied van stofwisselingsphysiologie*

### De Utrechtsche studie geprezen

De algemeen bekende stofwisselingsphysioloog Benedict uit Boston heeft gisteravond in de collegezaal van Prof. Noyons te Utrecht voor de leden van de medische faculteit van het Utrechtsch studentencorps een lezing gehouden over de grondstofwisseling (basaal metabolisme) en de uitkomsten van de jongste onderzoekingen dienaangaande.

Spr. wees op het feit, dat Utrecht een van de weinige plaatsen is, waar werkelijk aan grondstofwisseling op de juiste wetenschappelijke wijze wordt gedaan (prof. Noyons), zijn laboratorium was dan ook het doel van zijn bezoek aan Europa. Verzameling van n o r m a a l feitenmateriaal staat bij dit onderzoek op den voorgrond. Alle lichamelijke werkzaamheden worden daarbij geregistreerd en zoo nauwkeurig mogelijk gemeten en vormen waardevol materiaal voor diagnostiek en behandeling van verschillende aandoeningen.

Spr. wees op de verhouding tusschen grondstofwisseling (warteproductie bij rust) en lichaamsgewicht en op de pogingen gedaan tot standaardisatie van deze waarden. Ook de oppervlakte van het lichaam speelt een rol, zoodat bij kinderen de waarden grooter zijn dan bij volwassenen. Als enkele cijfers noemde spr.: 1 calorie per K.G. per uur en 1000 caloriën per M2 lichaamsoppervlak per dag. Lichaamsgewicht en geslacht zijn factoren die gewicht in de schaal leggen. Om zich rekenschap te geven van de leeftijdsfactor heeft men in Boston regelmatig een zelfde groep van 4 personen (3 mannen en 1 vrouw) onderzocht: deze onderzoekingen duren nu reeds 17 tot 24 jaar. Over de uitkomsten van deze serieonderzoekingen, die hun gelijke in de wereld niet kennen, gaf spr. een kort overzicht. Dit is de eenige methode om den invloed van den stijgenden leeftijd vast te stellen.

Het laatste jaar heeft men zich meer in het bijzonder gewijd aan de bestudeering van de grondstofwisseling van ouden van dagen, speciaal bij een groep vrouwen tusschen 66 en 86 jaar. Men koos normale gezonden ouden van dagen uit een oudevrouwenhuis. Het gemiddelde van de grondstofwisseling van al deze personen bleek slechts 0,9% van den norm af te wijken. Spr. wees in aansluiting daaraan op de behoefte aan physiologische waarnemingen bij normale ouderwordende proefdieren.

Spr. heeft ook nadere onderzoekingen naar de rasfactor ingesteld door onderzoekingen bij Chineesche en Japansche vrouwen, bij vrouwen in Engelsch-Indië (Tamils in Madras), inboorlingen in Australië, Yucatan en in Amerika geboren Chineesche meisjes. Verschillende groepen hadden een grondstofwisseling van 14—16% beneden de Amerikaansche standaarden.

Daarentegen hadden de Indianen in Yucatan bij herhaalde onderzoekingen bij gelegenheid van verschillende expedities een grondstofwisseling van 5—8% b o v e n de genoemde standaard. (het gemiddelde van die drie expedities onder leiding van resp. Williams Shattuck en Steggerda was 6,5% boven het gemiddelde. Bij onderzoekingen op verschillende dagen werden intusschen wel verschillende waarden gevonden. Merkwaardig was de geringe polsfrequentie (40—50), die bij deze Maya's in Yucatan deze hooge grondstofwisseling vergezelden. Vergelijkt met de Maya's in Yucatan en de Tamils in Engelsch-Indië dan komt men tot verschillen van 40%!

Bij een tiental Amerikaansch-Chineesche jonge vrouwen vond men waarden van gemiddeld 9.2 pCt. beneden de Amerikaansche standaardcijfers. Uit dit alles blijkt dat de rasfactor van belang is. Ook bij proeven op dieren is dit overtuigend gebleken. Met name zijn deze proeven bij duiven verricht. Een ander onderdeel had betrekking op de wisselingen van dag tot dag. Daartoe werd elken dag de grondstofwisseling bepaald bij een groep studenten gedurende een periode van 3 weken. Het zuurstofverbruik bleek vrijwel op één hoogte te blijven (per minuut gemiddeld 231 c.c.).

De veronderstelling was dat gedurende de slaap de grondstofwisseling gering zou zijn. Het was echter niet gemakkelijk daar over nauwkeurige uitkomsten te krijgen. Spr. heeft in Engelsch-Indië proeven bij een vrouw genomen ( 10 pCt. beneden den norm). Ook bij waarnemingen elders vond men tijdens slaap een daling van 10 pCt. De rasverschillen hebben dus op deze relatieve daling geen invloed. Vervolgens heeft men overeenkomstige proeven tijdens hypnotische slaap genomen.

Daarbij bleek een verlaging van de grondstofwisseling te bestaan, zoodat in physiologisch opzicht de hypnotische slaap zich duidelijk van de normale slaap onderscheidt.

Spr. ging vervolgens over tot de bespreking van de tegenwoordig door hem gebruikte apparatuur. Tegenwoordig wordt daartoe een afsluitend masker gebruikt in den vorm van een helm die het geheele hoofd omgeeft en om de hals hermetisch met rubber is afgesloten. Men onderscheidt een open en een gesloten stelsel, sommige alleen voor de bepaling van het zuurstofverbruik, andere ook voor de bepaling van de koolzuuruitscheiding. Als regel worden proeven bij rust en tijdens arbeid genomen, bij geestelijke inspanning, bij wisselend zuurstofgehalte, bij toediening van alcohol en gedurende de herstelperiode na uitputtende spierarbeid. Sedert spr. jaren geleden de helmrotameter van prof. Noyons heeft leeren kennen heeft hij dit toestel niet meer willen missen.

Al deze toestellen worden regelmatig gecontroleerd. Spr. beschreef de uitkomsten van zijn jongste onderzoeking.

Merkwaardig waren met name de bepalingen van den invloed van geestelijken arbeid op de grondstofwisseling. Hart- en longwerking waren vermeld maar een betrouwbare invloed op de grondstofwisseling kon niet worden vastgesteld. Ook als onderdeel van onderzoekingen naar de physiologische en psychologische werking van alcohol op het lichaam werden stofwisselingsbepalingen gedaan waarbij echter geen duidelijke verschillen werden vastgesteld. Tenslotte wees spr. op de waarde van een en ander voor de kliniek. Daartoe moet met name de perspiratio insensibilis worden gemeten en daarnaast de grondstofwisseling met de helmrotameter worden bepaald. Van beteekenis is daarbij gebleken de bepaling van de temperatuur van de uitgeademde lucht. De perspiratio insensibilis blijkt een waardevolle maatstaf voor de grondstofwisseling te zijn. Deze onderzoekingen behooren volgens spr. tot de routine onderzoekingen van alle ziekenhuizen.

# FUSAIN

**God Almighty first planted a garden and indeed it is the purest of human pleasure.**

De parken — in het late najaar — hebben zoovele bekoringen, dat het een lust is, een wijle te toeven te midden der Herfststemmingen. Niettegenstaande stormvlagen en gestriemd door regen is het de natuur bijkans onmogelijk van al haar schoons afstand te doen om — zij het voor een wijle — sterven te gaan.

Brons ligt het park Oog-in-Al met zijn prachtige tinten in goud en oranje en bruin vredig te pralen in de morgen herfst-zonneschijn; de tintelende ochtendkoelte streelt en brengt vreugde.

Zie den berkenheuvel, zooals die een boeiend middenpunt vormt van een wijd uitzicht op vijver en gazon, op perken met juichende bloemguirlandes in lila, rood en geel en wit, gerangschikt tegen een fond van groen in alle denkbare schakeering.

De blanke watervlakte, die eens vijver was, is thans door de stroomende regens tot een meer, zooals de slooten tot riviertjes zijn gezwollen. Het nú rimpellooze oppervlak weerkaatst felle dreigementen van wolkenreuzen met telkens zondoorgluringen.

Bontkleurige eenden duikelen kwakend en dartelend en zich toiletteerend; ze genieten volop van de momenten van het kortstondig zonnefeest, ze zijn de note gaie der omgeving. Trots drijven statig in de verte eenige zwanen, machtsbewust...

De anders zoo zonnig-jolige speelplaats met de zandbak gelijkt een grauw verlaten eiland midden in een oceaan. Triest staan er omheen de ledige banken in het watermeer. Bij de violenberg, die nog in paarsen gloed ligt, is het bruggetje onbegaanbaar, ligt midden in het water: nog bekronen margrieten-boeketten stralend de vier bruggehoofden. Eenige stamgasten, de oude heertjes, die dagelijks het park betippelen, bespreken — beraadslagend — den ernst van het niet alledaagsch gevalletje, hoe zij op de veiligste wijze de overzijde kunnen bereiken. En menige guitigheid wordt gezegd.

Als een opvroolijking van al die regendagen ligt het park gloriënd nú in den leutigen zonnegloed.

Een late herfstdag en toch...
Een dag als een lied.
31 October '32.

K. A. R.

**RIJKSTELEFOONKANTOOR UTRECHT.**

**Nieuwe aansluitingen.**

| | |
|---|---|
| 16656 | Fa. Brand en Zn., Nachtegaalstr. 65. |
| 16657 | Has Boekhandel en Leesbibliotheek, Mendelssohnstraat 37. |
| 16667 | Wm. C. v. Luyn, Nachtegaalstr. 42. |
| 16674 | C. A. Beukenkamp, 2de Daalschedijk 8, terrein N.S. |
| 16676 | Hypotheek- en Assurantiekantoor, Grietstraat 20. |
| 16677 | Maison „Mietze Linser", Oudegracht 319. |
| 16679 | D J. J. van Kempen, Nachtegaalstr. 39. |
| 16685 | Geref. Burgerschool voor L. O. en U. L. O., Plompetorengracht 25. |
| 16691 | R.K. Vrouwenbond, afd. Utrecht, Boothstraat 9. |
| 16692 | M. A. G. v. Isselt, Gildstr. 68. |
| 16693 | Mej. M. L. S. de Beaufort, Oudwijk 47 |
| 16696 | G. W. v. Tol, Nieuwe Gracht 62a. |
| 16703 | Café „de Kampioen", H. C. A. de Zeeuw, Jutph. weg 199. |
| 16801 | Huize Mohr, Leidscheweg 17b. |
| 26224 | Mevr. J. C. Drijber—Helfrich, Beetslaan 5, Bilthoven. |
| 26310 | J. Diederik, Zweerslaan 6, Bilthoven. |
| 26674 | E. G. Sauerland, Mozartlaan 20, Bilthoven. |
| 28300 | Piet Catoen, Waterweg 99, de Bilt. |
| 28301 | G. Rietveld, Waterweg 80, de Bilt. |

**ACADEMIENIEUWS.**

Bevorderd tot doctor in de geneeskunde aan de rijks-universiteit te Utrecht jhr. J. J. M. van der Does de Willebois, geboren te Arnhem, op proefschrift, getiteld: Over migraine, in het bizonder over hemicrania psychica.

GRONINGEN, HOLLAND.

University of Groningen, Physiological Institute.

Professor F. J. J. Buytendijk, Professor R. Brinkman, Dr. Mook,

Dr. R. J. Hamburger, and Mr. M. Dirken.

A visit to this laboratory brought recollections, of course, of the former director, Professor Hamburger, and it was rather surprising to find that Professor Buytendijk is practically without interest in metabolism matters and indeed many of the lines of research carried out by Professor Hamburger. In fact, Buytendijk had given up all attention to problems of metabolism that he had formerly studied in the Free Institute at Amsterdam. He was entirely absorbed now in studying the psychology of animals, particularly the dog, and was doing a great deal of work on this point. Personally I was so little interested in the particular things that he was doing that I made no notes with regard to the details.

Buytendijk's studies on animal psychology are based upon the fact that he thinks the best method of understanding the animal is not by reactions, reflexes, etc., but by behavior. He studies the dog's reaction to food under various conditions and the frog with the eyelid reflex.

I had quite a chat with Professor Brinkman, who is extremely interested in the relationship between hemoglobin in the blood and the $P_H$. He is also much occupied with the question of the carbohydrate anhydrase, referring to work done by Roughton of Cambridge, who thinks there is carbo-anhydrase in the blood corpuscles. Brinkman had done a great deal of work upon the relation between the carbon dioxide, the sodium acid carbonate, and the hemoglobin. He considers that the elimination of end products is hastened by the catalyst. He feels that the metabolism can be increased only by extending the output of the end products but not by increasing the production.

Brinkman had a very clever scheme to obtain the acidity and alkalinity at 0° of various solutions, employing colors as indicators and testing out the amount of catalase in known solutions. I found that he was too much of a biochemist for me although extremely interesting, and I was very grateful for the time he devoted to me.

Dr. Mook in the biochemical department, who was working upon blood, has published in the Biochemische Zeitschrift, 1931, 242, page 338, a micro method for blood oxygen and blood carbon dioxide. I think that his method is not as good or rather not as exact as Van Slyke's, but he can use any number of small graduated tubes and thus they use this micro method for rapid, repeated clinical measures. As I recall it he thought there might be a 5 per cent error between his results and Van Slyke's. He was very well spoken of by Noyons and all of the Groningen people.

Dr. R. J. Hamburger. It was a great pleasure to see young Hamburger in the hospital, where he is very much interested in observing the reaction of insulin, glucose, and calcium chloride, with the idea of studying permeability, and the whole thing is based on a blood sugar study. Again there was a little too much biochemistry for me personally but still he is not losing his father's interest in biochemistry, and it seems as if he is carrying on in good shape.

To me the most interesting man in Groningen in the laboratory was, as is always, Mr. Dirken. Dirken is an odd stick. He does not believe in standard, classical university training and refuses to take a doctor's degree, although I have been told that he has completed all the requirements but simply refuses to recognize or take it. He is not a man of unusual personality; he talks intelligently and is perfectly normal.

Dirken was very much interested in the reducing of the dead space to a minimum and he had devised a face mask or rather a mouth and nose piece with a small space. Of interest to me was the fact that he uses it in the clinic. The closure has a considerable amount of plaster around the tubes to the nose and mouth. He uses the Douglas bag, a Krogh spirometer, and makes much use of Enghoff-Loven valves. He finds that with room air, which he uses, there is a constant respiratory quotient with a person for at least two hours. As he has used the apparatus on students he believes there is normal respiration with it. The gas-analysis apparatus is much on the order of the Haldane with the ordinary degree of refinement but no closer than 0.01 per cent. Water pressure is used to raise or lower the mercury. (Figure 108 shows the brass nipple and collar used with the face mask.)

As on earlier trips I found Dirken very much interested in the composition of the air in different parts of a single expiration. He had a mouthpiece connected to a long brass cylinder with a piston rod and on it a series of plates or discs, and as he expires this rod cuts off samples between the mouthpiece and each little compartment between the discs on the piston rod, and thus he has a sample of air of different parts of the expiration. This can be pulled by at different velocities. (See figure 109.) He gave me no idea of results as yet.

Figure 108. Groningen, Holland. University of Groningen, Physiological Institute. Laboratory of Mr. M. Dirken. Brass nipple and thin collar used in Dirken's laboratory, for connecting with his mask. It is resting on top of a wooden box, the ring resting against a cork. At the right of this is the main stem of the mouthpiece which is approximately 1 1/2 inches high. At the extreme left is shown a part of the apparatus used by him for separating different sections of an expiration.

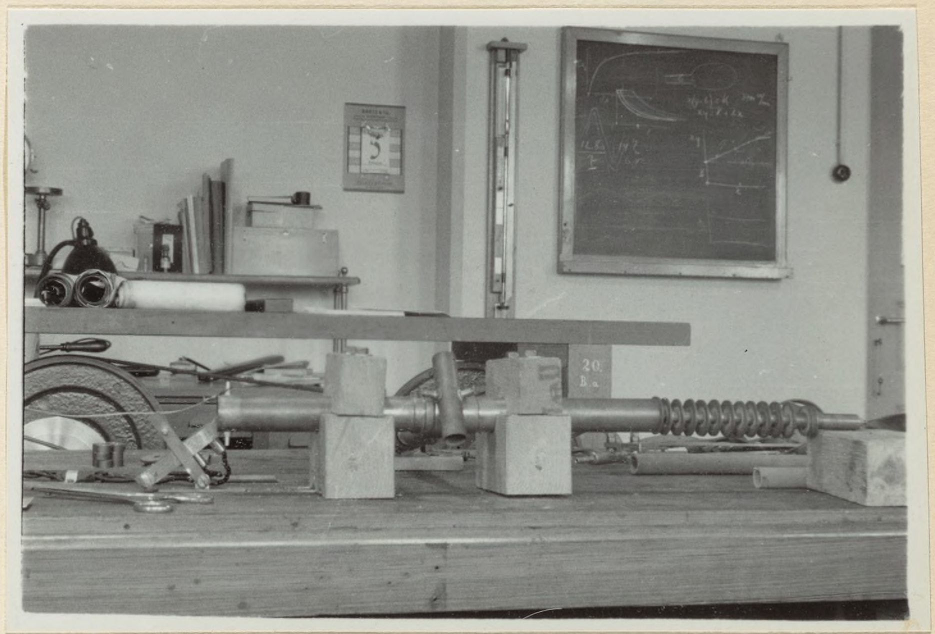


Figure 109. Groningen, Holland. University of Groningen, Physiological Institute, Mr. Dirken. Apparatus with plunger and discs to separate portions of a single expiration from the lungs.

We spent a good deal of time talking about the 3-meter Richet system of respiratory quotient analysis, and I told him somewhat about the sad experience of myself and Coropatchinsky in trying to get this and also the graphic tracings. Dirken is a very clever chap. I would like to see more of him and I think we could profit greatly by having him here for a year, but it did not seem advisable or practical now.

Lecture. On November 3rd I gave the "third" lecture for Professor Buytendijk, in the lecture hall of the Laboratory of Physiology, Faculty of Medicine, to an audience of 140. (There was an announcement of this lecture.)

Groningen

MEDISCHE FACULTEITS-VEREENIGING

*Het Bestuur der M. F.-V. heeft de eer U uit te noodigen tot het bijwonen van de lezing, welke* Dr Francis Benedict uit Boston *voor de Vereeniging zal houden, op* Donderdag 3 November*, des avonds te* 8 *uur, in de Collegezaal van Prof.* Buytendijk

*Onderwerp:* Human basal metabolism in the light of recent studies

## GRONINGEN, HOLLAND.

### Professor E. D. Wiersma.

It was a very great personal privilege to be the house guests of Professor and Mrs. Wiersma. (See figure 110.) Since our last visit he had retired as university professor and apparently there is a tremendous alteration in the status of financial affairs, for instead of having the rather luxurious professor's home they must now have a much smaller home. I was extremely impressed by the fact that while formerly as university professor he had a very large private practice in psychiatry, now no longer a university professor he was rarely called into consultation.

Although not at all conversant with his special field I found Professor Wiersma as usual a most interesting, intellectual man. He is a striking personality. I missed the laboratory, which of course he has nothing to do with now.

Figure 110. Groningen, Holland. Left to right: Mrs. Benedict, Mrs. Wiersma, and Professor E. D. Wiersma.

DORTMUND, GERMANY.

Kaiser-Wilhelm-Institut für Arbeitsphysiologie.

Professor E. Atzler, Dr. Krauss, Dr. Kraut, and Dr. Szarkall.

Remembering this magnificent institution from four years ago and the wide variety of problems studied by Atzler and his associates, I was particularly glad to come here again, although Atzler had told me in Rome that their activities had to be greatly curtailed, for the institution is subsidized by industries and the economic conditions were such this year that they could do little, if anything. For example, one thing that impressed me very much was that the building was cold, frightfully cold, most of the rooms seemed to be shut up, and the reason was that they could not have coal. The coal had formerly been given them by some of the large concerns. The coal mines are almost within a stone's throw of the institution and yet they had to buy the coal and could not get money to buy it. Indeed, only that day had sufficient coal arrived to start the heating apparatus.

Atzler, of whom I had formed a very good opinion on my former visit and who made a fine impression on me at Rome, is of course the center of the entire institution, and by having the extreme pleasure and great privilege of being his house guest while in Dortmund I had an opportunity to come more closely in contact with this most remarkable man. Atzler is a director first and foremost. He states frankly that he must travel almost constantly in the interest of the institute and nowadays to "pass the hat". He makes experiments himself only as a final control; that is, others make most of the experiments and he simply checks those up finally, so he considers his presence at the laboratory quite unnecessary; in fact he comes and goes as he pleases and is away a great deal.

There is much to be said for this type of direction. It is quite clear to me that when he is there he is thoroughly in touch with nearly all the researches, and I think probably he has worked out by far the best method of solving his administrative problems. If his time were entirely free for concentrated effort in the institution then quite a different plan would have to be worked out. He outlined his program for while he was there and on parts of each day he was to concentrate on several of the main divisions of the institution's activities, so that my first day was spent with his psychologist, Dr. Krauss (?). (I am ashamed that I have not the name more definitely established.)

This psychologist was much occupied with hand work, such as knitting, assembling pieces of metallic work such as Meccano, and the Erecto construction bridges, and things of that kind in which there is the idea of rapidity of assembling. (See figures 111, 112, and 113.) This is always followed by pause periods. One room was devoted to "Band Arbeit", that is, where parts travel around on an endless belt and each person does a certain small thing and there is "mass production", such as was possible for the Ford five-dollar-per-day job. (See figure 114.) The theory is that when the man in front of this belt can keep up with the belt then he gets five dollars a day. The old idea was to fit the man to the belt. Now here they hope to "fit the belt to the man". The room was frightfully cold and a woman was there doing some knitting. She had been on a long study of speed of knitting for several weeks, if not months. I thought it altogether too cold for hand work. When this has been well worked out on this small "piece" work they expect to go on to heavy steel workers' work. I found the psychologist a most interesting chap and right on to his job in every way.

The division of chemistry is in the hands of Dr. Kraut. There was much interest in the presence of silica, and in the case of miners they had a micro method for determining silica. The normal amount of silica is constant from year to year but they found the silica in the blood varied greatly with certain individuals. The whole problem of silica and dust in the body and blood was being very actively attacked and on similar lines was also phosphorus. Among others was the study of the fat of tuberculosis. They felt that the fat about the bacillus was very resistant and perhaps resistant to splitting ferments.

One most interesting problem was the use of the piezo tube. For example, in a shovel they had three of these tubes connected at different parts of the shovel so that the sudden movements of the hands at different parts produced movements that would be transferred to the pieces of quartz, hence electrical contact registered the "intensity" of movement of a definite part of the shovel. One problem was shoveling coal 1 meter high on the shelf with struts 6 feet apart, that is, a condition approximating working conditions in mines.

Cats on a treadmill. They find cats can not run. After 5 kilometers they are all in.

Carbon monoxide in automobiles. In this institute they were studying the influence of carbon monoxide on work on a treadmill. There were certain connections between inhalation of carbon monoxide and "decision" of a person either to move or sit still to prevent injury in an accident. Atzler was particularly interested in the carbon monoxide with special reference to the carbon monoxide in a limousine or closed car. They had found that the amount of carbon monoxide in the back part of a closed car was very great with the windows closed, and if this continues there is a rapid absorption by the hemoglobin of the blood, thus lowering its oxygen-carrying power.

Figure 111. Dortmund, Germany. Kaiser-Wilhelm-Institut für Arbeitsphysiologie. Methods of studying in a psychological laboratory hand work with various electric contact instruments.

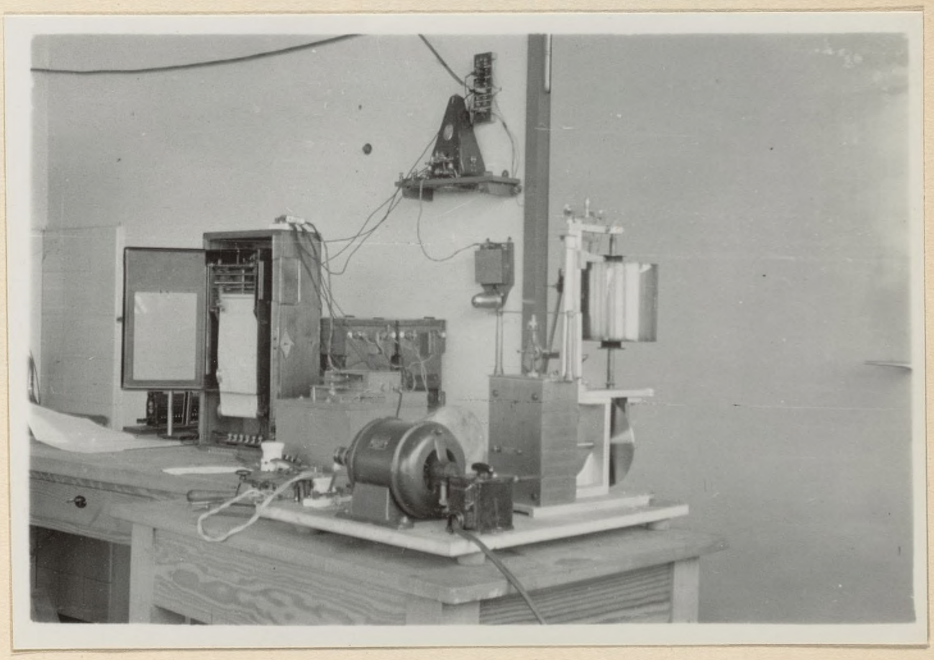


Figure 112. Dortmund, Germany. Kaiser-Wilhelm-Institut für Arbeitsphysiologie, Department of Psychology. Device for measuring reaction times.

Figure 113. Dortmund, Germany. Kaiser-Wilhelm-Institut für Arbeitsphysiologie. Pursuitmeter or eye reaction apparatus in psychological laboratory. Revolving cylinder is used and the black line travels back and forth through the slit.

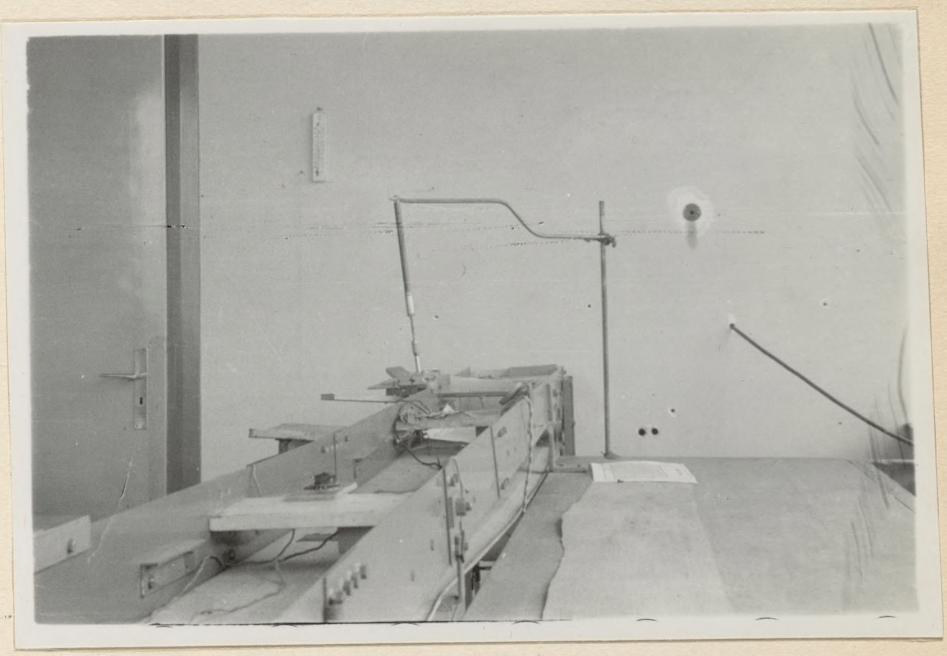


Figure 114. Dortmund, Germany. Kaiser-Wilhelm-Institut für Arbeitsphysiologie, Department of Psychology. Apparatus for studying work on a belt conveyor with timing contacts for the time relationships.

A considerable equipment was established to study the influence of effect of working using various hand wheels, auto steering wheels, and large machine wheels. The wheels were turned at different angles and different weights and resistances added, and fatigue curves written.

Stair climbing. There was a very elaborate steel fire escape and elevator so you could study walking up the stairs or down. I was a little surprised that they had no escalator.

One of the problems under study was that of pneumatic tools to measure the pressure against the hand or shoulder from the back pressure. Such tools were very different and no two firms had the same type of hammer. These hammers had from 900 to 1600 strokes per minute. The workers themselves have very bad subjective impressions. As a result of using these hammers the workers develop occupational diseases, such as shoulder muscles and nerves in the arms going to sleep. The noise of the hammer is an important factor. The noise of the hammer is measured with a microphone and they can analyze the noise in different parts of the hammer.

Dr. Szarkall. Dr. Szarkall, formerly at Budapest, is the man of whom Noyons spoke to me who had the wonderfully trained dogs. Szarkall showed us a female dog with tracheal cannula, who was trained to lie and to be catheterized as frequently as was desired. The animal was nearly as immobile as Noyons indicated. Szarkall maintained that while at the beginning there was a good deal of rigidity if not cruelty in subjecting the dog to his mastery, now the dog reacts without hesitation or obvious fear. He was interested in studying the ultra-violet ray for he thought it produced a selected combustion of carbohydrate. He used the Benedict apparatus but a large form.

I was particularly interested in the alcohol lamp that he used. The flame was such that it impinged on a hot platinum spiral extending over the wick so that the alcohol burned well even with the air leaving the flame at 3.4 per cent carbon dioxide. In other words he could have an alcohol check with the air leaving the apparatus essentially that of the man. A description of this apparatus had been published by Dr. Meyer previously. I am not sure just how much Szarkall has added to it.

General comments. One of the striking illustrations of economic disturbance was the fact that this enormous institution, so well equipped, was running at only a fraction of its capacity. Nevertheless, the men were enthusiastic and apparently Atzler has a firm hold on the whole picture. However, I think he was dominated in large part by the necessity of interesting the industrial men. For example, when I was to give my lecture great stress was laid upon the fact that eminent industrial men were coming and it was important that these men come to the house in the afternoon for refreshments, and Atzler begged me to put on some magic, which I was very glad indeed to do.

This is not the place to write of the personality of Atzler and his charming wife, an ideal host and hostess and a remarkable couple. I must record, however, an incident told me by O'Brien in the Rockefeller Institute, Paris. O'Brien was passing through Dortmund and dropped in to the laboratory to see Atzler. He was told that Atzler was at home with a cold but he could see him. He went to the house and was shown in, and, as O'Brien puts it, a young man came down and O'Brien expected him to state that his father would be down in a moment or two, but the young man proved to be Atzler. He is not as young as one would think, but his wife is very young and they make a charming couple.

Lecture. On November 8th I gave the "third" lecture for the Kaiser-Wilhelm-Institut für Arbeitsphysiologie, to an audience of about 75. (There was an announcement of this lecture.)

**KAISER-WILHELM-INSTITUT FÜR ARBEITSPHYSIOLOGIE**

Fernsprecher Dortmund Nr. 23705 u. 23706

Postscheckkonto: Dortmund Nr. 4847

DORTMUND, den 3.Novbr.1932.
Hindenburgdamm 201

Euer Hochwohlgeboren

beehren wir uns zu dem am Dienstag, den 8. ds.Mts. abends $8^h$ s.t. im Hörsaale des Instituts stattfindenden Vortrage des Direktors des Nutrition Laboratory der Carnegie-Stiftung in Washington, Herrn Professor Dr. Francis G.Benedict über das Thema:

"Der menschliche Stoffwechsel im Lichte neuerer Untersuchungen".

freundlichst einzuladen.

Damen und eingeführte Gäste sind willkommen. Nach dem Vortrage zwangloses geselliges Beisammensein im Institut mit Gelegenheit zu einem Glase Bier.

In vorzüglicher Hochachtung

Atzler

Atzler.

Um Antwort wird gebeten.

HAMBURG, GERMANY.

University of Hamburg, Allgemeines Krankenhaus Eppendorf.

Physiological Institute.

Professor Otto Kestner and Professor Franz Groebbels.

It was our great misfortune to find Professor Kestner just recovering from a long illness. Indeed, his first public appearance following his illness was made at my lecture, so of course he had no problems in progress. I saw quite a little of his associate, Dr. Groebbels. He is a pessimistic individual, complaining about everything and especially about the economic situation in Germany. I think they had no money to do anything with and they could make no progress.

Groebbels' own interest is most intensive with regard to birds and especially studying their instincts, the flying instinct, nesting, etc. He had done a good deal of work with the stomach fistula, studying digestion with the birds. He agreed with me that the goose is an extremely satisfactory and intelligent animal to work with. I told him I thought his canary work was all wrong and referred to our new article which had not then appeared. He maintained that a humming bird went to 4000 calories per square meter, quiet and asleep, and 50 per cent higher when the bird was active.

Groebbels was talking continually about his difficulties and troubles but I did not feel at all impressed by his conversation or his ideas. I had been told he was a man of considerable means and hence one should feel that a great deal of credit is due him for trying to do any scientific work. The other standbys or stalwarts I saw very little of. For example, Mrs. Rahel Plaut I did not see.

Lecture. On November 10th I gave the "second" lecture at the Physiological Institute in Hamburg, before an audience of 500.

## HAMBURG, GERMANY.

### University of Hamburg, Allgemeines Krankenhaus Eppendorf.

### Medical Clinic.

### Professor Ludolph Brauer, Dr. Wilhelm Knipping, and Dr. Arthur G. Vollmer.

Here I saw again the great cement respiration chambers which are now completed. One is for excessive pressure of about 1/3 of an atmosphere. In this they had a group of patients on whom they were studying the effect of the ultra-violet rays, especially in connection with asthma problems. The other chamber was for decreased pressure but no research was going on in that.

Knipping impressed me more than formerly. He is now engaged in clinical work and told me he had given up research in physiology. I had an interesting reaction to Knipping. When I was speaking with several men who had been in Hamburg they independently used the expression that Knipping was "Pfiffisch" and it has been rather difficult to secure an exact translation of this term.

In the hospital I came in contact with the gas-analysis apparatus of Vollmer, who tells me that this was developed because the very large respiration apparatus demanded an extremely accurate instrument to analyze the air in the chamber. (See figures 115, 116, 117, and 118.) Correspondence with Dr. Carpenter brought out the fact that Vollmer along with others, like Pickworth in England and more recent writers in America, have raised a lot of intricate theoretical questions on this gas analysis, questions which are only partly solved. Some of them such as "absolutely constant" temperature would call for the submerging of the entire outfit in a water bath, thus immediately making the apparatus impracticable from the standpoint of portability and general use. Dr. Carpenter definitely points out that until they can secure much better results than he is able to secure with his apparatus there is no reason for them to think that they have a better instrument. I am completely in accord with him on this point.

I spent a great deal of time with Vollmer and Knipping, going over the apparatus carefully in every detail and bringing up a number of points, many of them formerly brought up with Dr. Carpenter, but I likewise came to the conclusion that they had nothing that approximated the accuracy or usefulness of the Carpenter apparatus.

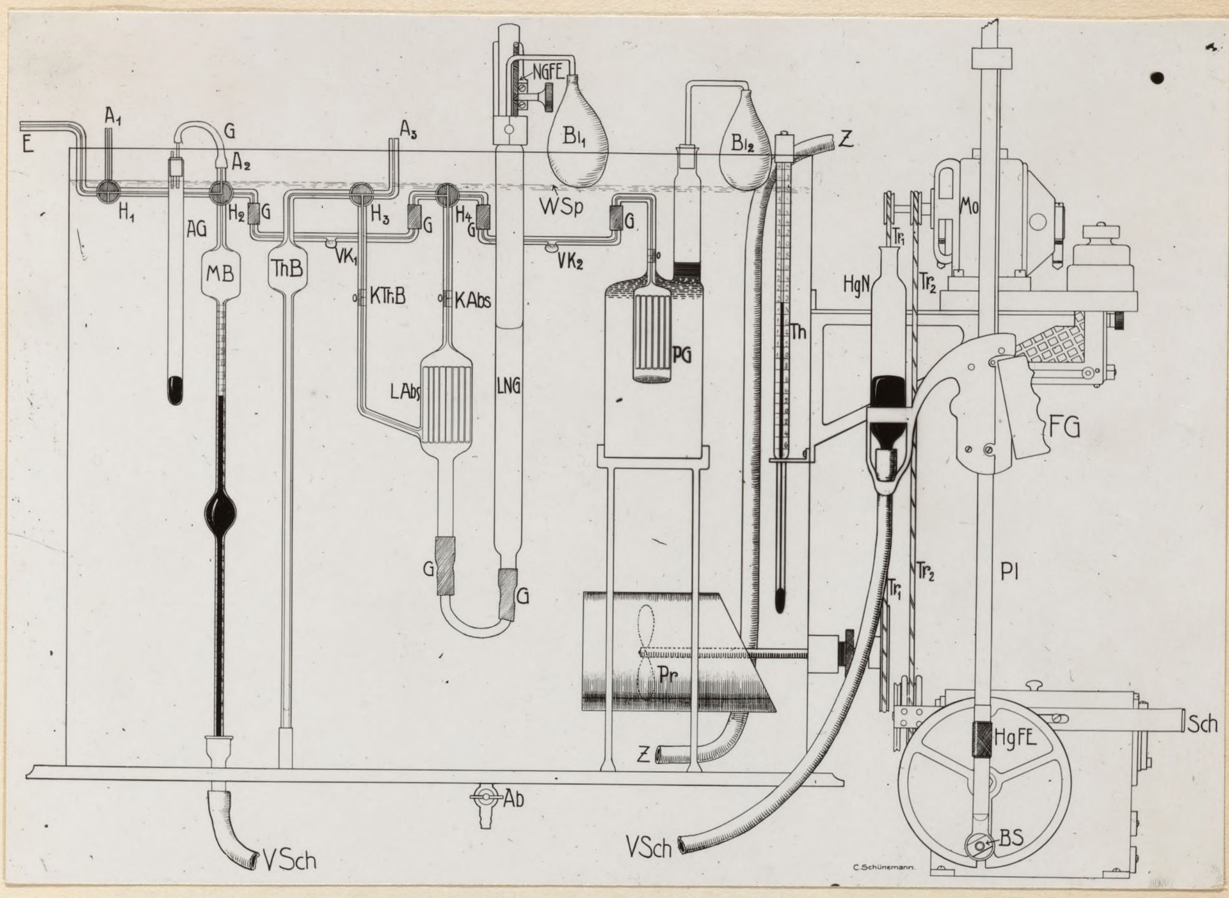



Figure 115. Hamburg, Germany. University of Hamburg, Allgemeines Krankenhaus Eppendorf, Medical Clinic. Sketch of gas-analysis apparatus of Dr. Arthur Vollmer.

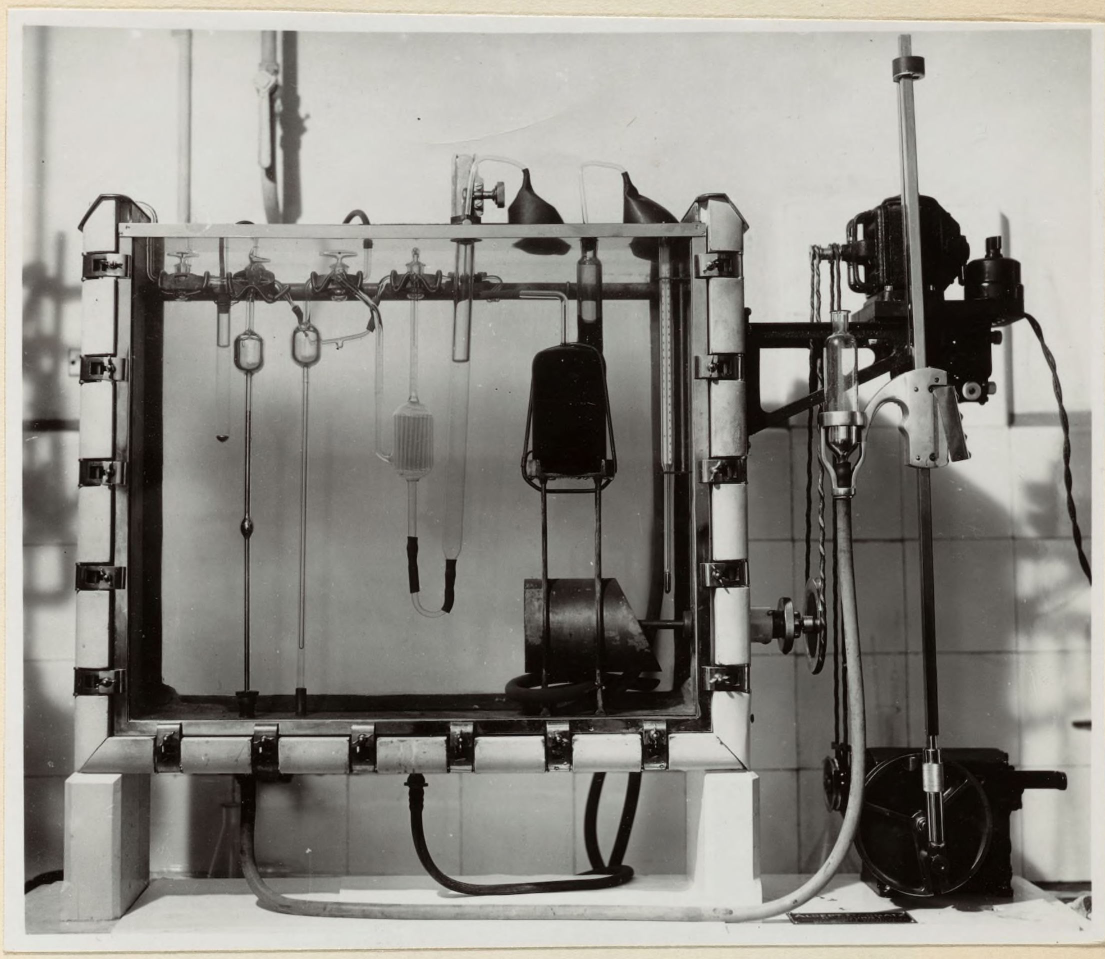


Figure 116 . Hamburg, Germany. University of Hamburg, Allgemeines Krankenhaus Eppendorf, Medical Clinic. Photograph of gas-analysis apparatus of Dr. Arthur Vollmer. This photograph and the preceding sketch are both reproduced in Dr. Vollmer's description of the apparatus entitled, "Beitrag zur Gasanalyse", in Zeitschr. f. d. ges. Exper. Med., <u>78</u>, pp. 93 to 133 (1931).

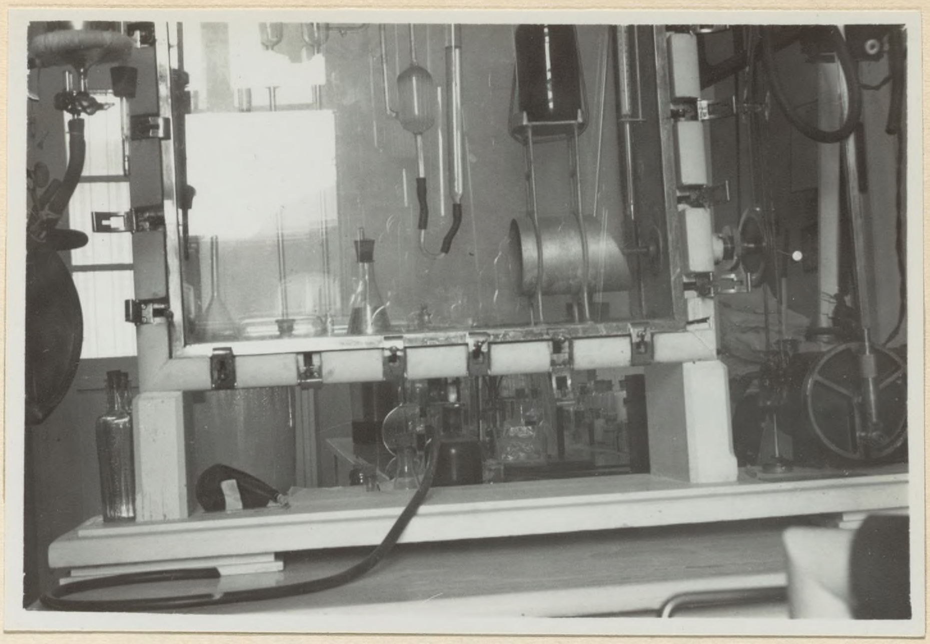


Figure 117. Hamburg, Germany. University of Hamburg, Allgemeines Krankenhaus Eppendorf, Medical Clinic. Photograph showing part of gas-analysis apparatus in the laboratory of Dr. Arthur Vollmer. Stirring device at the lower left-hand side, with the fan blades housed in a metallic tube. All this is under water. The support of the pyro is shown.

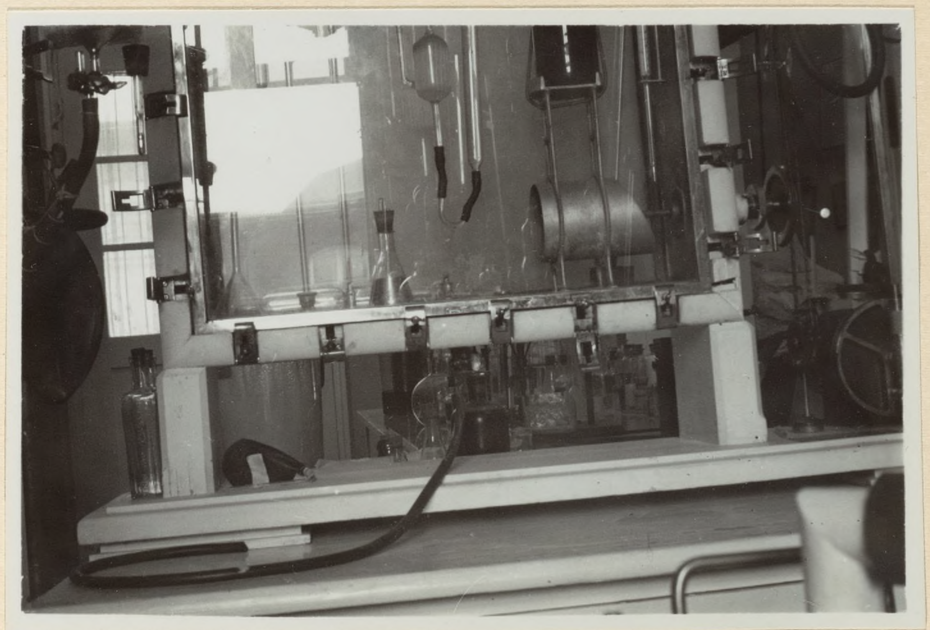


Figure 118. Hamburg, Germany. University of Hamburg, Allgemeines Krankenhaus Eppendorf, Medical Clinic. Another view of gas-analysis apparatus in the laboratory of Dr. Arthur Vollmer.

## HAMBURG (STELLINGEN), GERMANY.

### Hagenbeck Animal Park.

We had the great misfortune to find our friend, Dr. Zukowsky, away from Hamburg but we were given a great deal of time by the Hagenbeck brothers, spending more than an hour and a half in their office, the walls hung and the floor strewn with animal skins of every nature. There was one skin in which I was particularly interested and I was told that it was a hybrid between a lion and a tiger. The skin seemed larger than either that of a lion or a tiger.

The Hagenbecks were extremely interested in the snake book and also in the Nutrition Laboratory's program for comparative physiology and firmly expressed a great regret that the Nutrition Laboratory was not located in Hamburg, where they assured me the entire resources of the Park would be placed at our disposal. Certainly in their calling they are men of high scientific interest and appreciate scientific work. We were taken about the Park by Heinrich Hagenbeck, Jr., who was very courteous and kind. I was impressed by the fact that the Park had gone down both in variety and number of animals. This was explained by the fact that practically all private parks no longer existed, as those members of the royal families, dukes, etc., who had maintained private parks could no longer do so. On the other hand, they told me the Berlin Zoological Park was never in better shape. This we subsequently verified.

BERLIN, GERMANY.

Landwirtschaftliche Hochschule, Tierphysiologisches Institut.

Professor E. Mangold, Dr. Linzberg, and Fräulein Steuber.

The great Zuntz respiration apparatus is not used at all. The pressure chamber that was formerly installed in the court of the laboratory has been used occasionally by Fräulein Steuber, but she is ill and now is doing no research work. I visited her in her room on the third floor and the poor thing is rather pathetic. I am not sure how serious her tuberculosis infection is but apparently she has had it for some time and has to be very careful. We had to have rather close relations with her because she had undertaken to translate the two articles for Pflüger's Archiv. The first she had finally got through; the second was only half done when I was there but she later completed it.

Mangold is interested in entirely different problems than Zuntz. I found one of his associates, Dr. Linzberg, who was interested in rat metabolism and had worked out a rather clever scheme to have the rats run in a treadwheel and still have a ventilation through a rotating chamber with a diameter of 28 cm. The rats could run about 8000 meters a day. The important feature of his treadmill was that the respiration chamber, which consists of the assembly of two glass desiccator covers clamped together, was used as a treadmill. It was rotated and ventilated at the same time. In the stoppers of the desiccators were two bits of glass tubing with short pieces of rubber tubing serving as joints and the ends dipping in mercury. As the desiccators revolved this rubber tubing turned freely and the mercury served as a seal. The ventilating current was conducted through a long U-tube immersed in the mercury and leading up through the desiccator. The important point is that the desiccator could turn around, the rubber joints served as a flexible joint, and the mercury prevented any loss of air. It is very clever. (I am not sure what he is going to do with it although my recollection is that he gave me a reprint.)

There were no experiments going on except the work on rats with Linzberg, of which they gave me four papers. I thought they were very poorly done. Mangold confided to me that Fräulein Steuber was a very fine critic and she says Linzberg's material is no good.

BERLIN, GERMANY.

Veterinary High School, Physiological Institute.

Professor Max Cremer and Dr. R. W. Seuffert.

Cremer is doing nothing but simply continuing as his own jovial, unique self. I was much interested in Seuffert, who had done work on the surface area of the horse. He was there and I had a chance to talk with him. I also found that Fuchs, the man who had been working on cancer and whose name was coupled with Wolf of Cambridge (Wolf has never met him), was in this laboratory as a guest.

Cremer especially requested to have the snake book sent to his laboratory and it was sent later.

Commenting upon the high price of German books and wondering how Germans could buy them, Cremer stated that the prices were made for the United States and Japanese consumption.

## BERLIN, GERMANY.

### Krankenhaus Lankwitz.

### Professor Georg Zuelzer.

Zuelzer was still almost obsessed with the idea of the heart hormone which he calls "eutonon". I did not visit his hospital or laboratory but I was told about it all the time I was at Rome. I find that very few people take him seriously, at least so far as this work is concerned. It seems to be pretty well evident that he nearly discovered insulin, but most people think he is entirely wrong and full of false notions. He is very much excited about the racial metabolism and lays great stress upon the various hormones. Consequently he is most anxious to have studies made with regard to the effect of eutonon on metabolism. Lichtwitz' assistant stated that if one uses enough eutonon to open the coronary arteries it attacks the heart muscles, so that this material is really dangerous in effective doses.

Although I did not visit Zuelzer's clinic we had a most delightful social evening with him and his wife. He spoke to me about some lectures he had been giving, particularly on scarlet fever. I afterwards wrote him: "I have heard many times expressions of interest in your scientific activity, but I fear that with your heart hormone you are going to duplicate the experience of our good friend, Abderhalden, with his 'reaction' and the experience of Zwaardemaker with his 'activation of the heart by potassium and radium'. There is a general disinclination to be enthusiastic about eutonon, for many apparently have little results from it and frequently those who do maintain that to get a result you must give so much as to injure the heart muscle."

Since this was written I have reason to believe that Zuelzer has likewise come under the ban in Germany.

BERLIN, GERMANY.

Professor Leo Zuntz.

Although Professor Zuntz is no more actively engaged in research, he still retains his great interest in research, as would be expected of the son of his father, the great Professor Nathan Zuntz. As a gynecologist he was extremely anxious to see the helmet apparatus used during birth. There have been a very, very few metabolism experiments made during actual childbirth and they used the mouthpiece and of course the subject was uncomfortable. He thought it a cruel thing. By using the helmet he thought experiments would be possible and hoped this could be done.

BERLIN, GERMANY.

Rudolf Virchow-Krankenhaus.

Professor L. Lichtwitz.

It was a very great pleasure and antidote to the general depression of scientific research in German institutions to visit the laboratory of Lichtwitz. (See figures 119 and 120.) This brilliant man has come from Hamburg to the Rudolf Virchow-Krankenhaus in Berlin. It is a most up-to-date, modern laboratory with attention to practically all diagnostic aids and a certain amount of attention to respiratory studies. He had not brought the insensible perspiration balance with him from Hamburg because that belonged there and could not be moved, but there were a great many studies, especially on different forms of light, and blood chemistry in every detail.

Lichtwitz is an extremely active, hustling individual. In the laboratory I saw a Simonson apparatus with dry gas meter and the "four-branch candlestick" for sampling, also a part of a Knipping apparatus, but Lichtwitz said it was not good for carbon dioxide and uses it for oxygen only, and as a matter of fact uses it with valves. There was a splendid equipment for chemical work and electro-physiology, with a great deal of work on lactic acid and globulin, using nephelometry, of which more use is made. The instruments of Leitz and Zeiss were spread around the laboratory galore. No gaseous metabolism work is done other than on man.

I saw in Lichtwitz' laboratory a potentiometer that looked to me like a very good substitute for the one Noyons was using with the electric hygrometer, so I had an extensive correspondence with the manufacturer (Goldschmidt, Fennstrasse 1, Berlin, N. 65), but finally found it would involve complications to hook this up under the special conditions Noyons used, so I fortunately did not buy one.

It seems the irony of fate that not many weeks after our leaving, there was the political cataclysm and Professor Lichtwitz was practically compelled to leave Germany and this magnificent work he has so well instituted. One can not understand the thing at all.

Figure 119. Berlin, Germany. Rudolf Virchow-Krankenhaus. Left to right: Mrs. Benedict, Dr. Benedict, and Professor Litchtwitz. Photograph taken in the court of the Rudolf Virchow-Krankenhaus.

Figure 120. Berlin, German. Rudolf Virchow-Krankenhaus. Left to right: Mrs. Benedict, Dr. Benedict, and Professor Lichtwitz. Photograph taken in the court of the Rudolf Virchow-Krankenhaus.

BERLIN, GERMANY.

Krankenhaus im Friedrichsain.

Dr. H. Heller.

Just as I was finishing the lecture at the Rudolf Virchow-Krankenhaus and about to leave the auditorium a youngish man approached me and introduced himself as Heller, who had written so much on insensible perspiration. I don't know how many papers he has published on this point recently, on technique, etc. One of the great mistakes on my trip was not going to his laboratory and seeing just what he was doing, but we were about to leave Berlin and everything was booked up so that it was not convenient to visit his laboratory, but that was a real error on my part.

Based upon my observations at Noyons' laboratory, and subsequently at Würzburg, and my correspondence with Heller, I am sure that not a little of the difficulty that has been experienced has been the absurd use on the insensible perspiration balance of a horsehair mattress. Subsequently on my tour I saw more and more traces of this horsehair mattress, so I wrote to Heller from Geneva asking him for a description of the mattress, where he got it, etc. He replied in detail. It is covered over with linen and then wholly covered, i.e., sewed into a rubber cloth, I presume the purpose being to keep the humidity down to the lowest point. The mattress and pillow weighed 21 kg. Apparently this mattress has played a very large role. As in the case of Lichtwitz, since that time Heller unfortunately has had to leave Germany on account of the Jewish situation and is now somewhere in London.

BERLIN, GERMANY.

Lectures.

The "third" lecture was given on November 18th before the Berlin Physiological Society, to an audience of 250. Professor Rona presided and no one, even in the first row, heard one word he said. A poorer presiding speaker could not be found, but he is a very fine man. There were no questions, but I could sense from the audience that they were deeply impressed. (There was an announcement of this lecture.)

On November 19th I gave the "first" lecture at the Rudolf Virchow-Krankenhaus, for the Berliner Gesellschaft d. Naturforsch. Freunde. (See figure 121.) There was a very large crowd present, including a number of city dignitaries interested in medical education, for it was a novelty to have a lecture of this type in the hospital. I was glad to learn that Lichtwitz, who had engineered it, was very highly approved for his so doing. Magnus-Levy spoke afterwards, stressing the importance of animal physiology for human physiology. I was much interested to note that in his introductory speech Lichtwitz characterized me as a "general inspector of hospitals and laboratories" to see that everything was running well. One of the questions asked me was, if the snakes were kept at 38° would they ever go above the room temperature, that is, is there any point where the snake might have fever, so to speak? Of course we believed that the incubating python was in a febrile state.

A newspaper clipping, commenting on my Berlin lectures, is appended herewith. (See page 209.)

# Berliner Physiologische Gesellschaft

## Sitzung

am Freitag, den 18. November, 20 Uhr c. t.
im großen Hörsaal des physiologischen Instituts der Universität, Hessische Str. 3/4

Herr **F. G. Benedict,** Boston (als Gast)
„Der Grundumsatz des Menschen im Lichte neuerer Untersuchungen."

Gäste willkommen!

**E. Holzlöhner,** 1. Schriftführer.
Berlin N 4, Hessische Str. 3/4.

Figure 121. Berlin, Germany. Photograph of Dr. Benedict, standing at the lecture table. This was taken during the lecture at the Rudolf Virchow-Krankenhaus. Photograph taken by the Keystone View Company, Berlin.

Die schwarze Front
a radical sheet
Dec. 4. 1932

Berlin

arbeiten, ausgerichtet nach dem Ziel, das da heißt: sozialistisches Großdeutschland. Eins steht aber fest: alle diese Arbeit entbindet nicht von der revolutionären Entscheidung, befreit nicht von dem entscheidenden revolutionären Kampf. Denn die deutsche Revolution erst — und das muß immer wieder gesagt werden — macht die ganze Arbeit von heute sinnvoll und fruchtbar. Sie macht erst das Feld frei für die Front der Bünde, sie gibt erst Raum dem bündischen Menschen zur Wirkung und Erfüllung. Weil das aber so ist, heißt unsere Parole nach wie vor: **Bünde, geht nicht in dieses System, Bünde, bleibt draußen!** Bald kommt die Stunde, wo man euch dringend zum Aufbau bedarf, heute könnt ihr nur noch Abrißarbeiten leisten. Euer Weg kann immer nur heißen: die deutsche Revolution! Euer Ziel: das sozialistische Großdeutschland! In diesem Sinne wollen wir in der heimlichen Schwarzen Front arbeiten und uns vorbereiten. Wir können dankbar sein, daß wir diese Zeit noch haben.

**Bünde an die Front? — Ja! Aber nicht in die Front des Systems, sondern in die revolutionäre Front der schwarzen Fahne!**

# ...rnährung

halten und behandelt werden, nicht als Ware und Maschine.

Es handelt sich also gar nicht darum, wie man durch die gewissenlose Behandlung dieser Frage in einer gewissen Tagespresse meinen könnte, die Menschen zu **reinen** Rohköstlern zu machen. Es wird nur gefordert, daß die Rohkost in der Ernährung einen breiteren Raum einnimmt als bisher. Früchte und Salate ißt jedermann gern, der noch einen einigermaßen unverdorbenen Geschmack hat. Es handelt sich auch nicht darum, alle Deutschen zu Vegetariern zu machen, sondern um eine Zurückdrängung der Fleischkost, die unwirtschaftlich und im heutigen Umfang auch ungesund ist. Der Fleischverbrauch der Gegenwart ist etwa viermal so hoch als zur Zeit der Befreiungskriege, wo wir gewiß ein leistungs- und begeisterungsfähiges Volk waren. Von den Bauern der Mark Brandenburg, mit denen der alte Fritz seine Schlachten schlug, sagt Willibald Alexis, der größte Kulturschilderer der Mark: „Sie lebten

# Der Rassenfaktor beim Grundumsatz

**Eine wesentliche neue Entdeckung auf dem Gebiete der Rassenforschung.**

Am 18. November sprach im Rahmen der Berliner Physiologischen Gesellschaft der international berühmte amerikanische Professor Dr. Benedikt über Grundumsatzbestimmung.

Unter dem Grundumsatz eines Menschen versteht man seinen Verbrauch an Sauerstoff und seine Kohlensäureabgabe. Er ist weitgehend abhängig von den Bedingungen, unter denen die Versuchsperson steht. Im nüchternen Zustande ist er anders als nach einer reichlichen Mahlzeit, in der Ruhe anders als bei körperlicher Arbeit. (Geistige Arbeit, selbst in konzentriertester Form, ruft keine wesentlichen Schwankungen hervor. — So ein armer Geistesarbeiter kann unbeschadet seines Grundumsatzwertes 20 Stunden hintereinander tätig sein.) Auch nach Geschlecht und Alter sind die Werte verschieden. Dazu kommen noch wesentliche Schwankungen bei verschiedenen Krankheitsprozessen, für deren Diagnose diese Untersuchungsmethode ein wichtiges Hilfsmittel darstellt.

All diese Tatsachen waren bereits bekannt und wurden nur noch einmal durch zahlreiche Versuchsreihen mit der den Amerikanern dank ihrer günstigen wirtschaftlichen Bedingungen möglichen glänzenden modernen Technik bestätigt.

**Neu war nur die Feststellung des Rassenfaktors beim Grundumsatz.**

Professor Benedikt machte auf seinen Expeditionen nach Australien und Asien die Beobachtung, daß zwischen den Eingeborenen der anderen Kontinente und den untersuchten Europäern bei gleichen Bedingungen in bezug auf Alter, Geschlecht, Ernährungs- und Gesundheitszustand eine konstante Differenz der Grundumsatzwerte bestand. Diese Differenz fand er dann auch, als er Australier und Asiaten, die schon längere Zeit in Amerika, in den gleichen Verhältnissen wie die dort ansässigen Europäer, lebten, untersuchte. **Der Unterschied war demnach nur durch die Verschiedenheit der Rassen zu erklären.** Uns konservativen, völkischen Menschen ist diese Tatsache keineswegs etwas Befremdendes. Wir sind zutiefst davon überzeugt, daß jede Rasse, ja jedes Volk, von Anbeginn auf der körperlichen, geistigen und seelischen Ebene des Lebens seine charakteristischen, eigenen Gesetze hat. Nur die liberale Welt hätte gar zu gern landschaftliche und klimatische Verhältnisse und die dadurch bedingten verschiedenen Lebensgewohnheiten für die Verschiedenheiten der Menschenrassen verantwortlich gemacht. Leider wurde dieser schöne liberale Traum durch Herrn Professor Benedikt wieder einmal zerstört.

# Lebensreformer oder

BERLIN, GERMANY.

General comments.

One of the most depressing features of my European visit was the fact that there was so little going on in metabolism in Germany as a whole and particularly in Berlin. I found almost nothing in Berlin. There were a good many minor points to look up, such as to try to find the rat treadmill and things of that kind, but for concentrated research there was very little indeed and almost no metabolism work.

In one of the laboratories I took a photograph of a series of chains hanging by hooks. These chains are run through the arms of students' clothing to keep them from being stolen. This condition in Berlin is an aftermath of the war period. (See figure 122.)

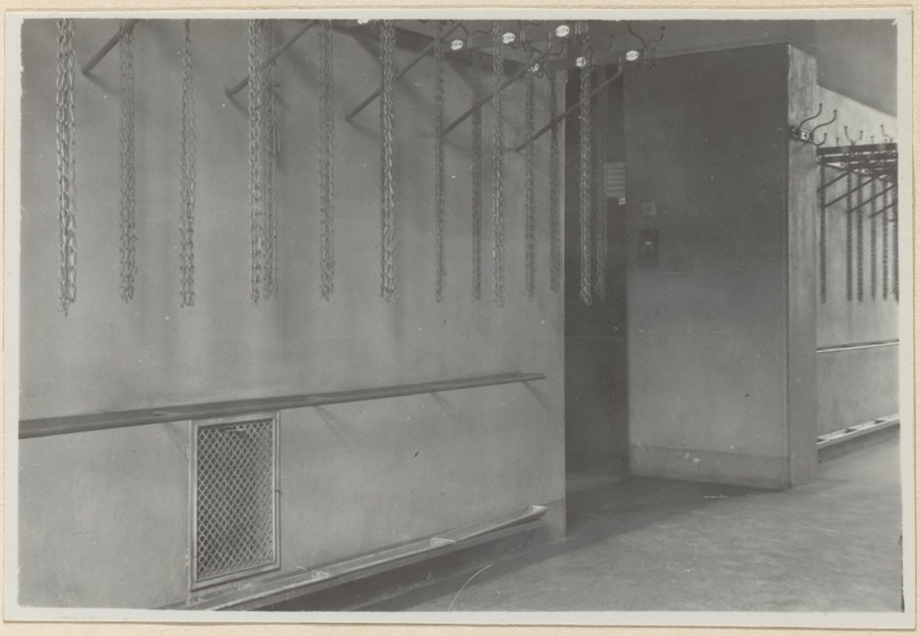


Figure 122. Berlin, Germany. An aftermath of the war period in Berlin. One of the laboratories, showing chains hanging by hooks. These chains are run through the arms of students' clothing to keep them from being stolen.

LEIPZIG, GERMANY.

University of Leipzig, Physiological Institute.

Professor M. Gildemeister.

Gildemeister is exceptionally interested in methods and he prefers men to come to him to learn the various methods, chiefly Chronaxie, etc. He is working on permeability problems and especially electrical physiology, but nothing goes on in metabolism. It is a very poor building and very poor equipment.

LEIPZIG, GERMANY.

University of Leipzig, Physiological-Chemical Institute.

Professor Karl Thomas.

Thomas is one of the most remarkable men in Germany. At the present time he is chiefly interested in studying the effect of a definite one-sided food or diet, such as water, glycerin, oleic acid, etc. The dogs had stomach fistulas and there was a metal cylinder or canister strapped to the back which forced the liquid food in small amounts intermittently for six hours, even when the animal was running around. Thus there is continual feeding of the animal for this period. This canister is supplied with liquid food and is forced in by the compressed air. The dog usually has a wide collar extending several centimeters each side of his neck, after the design of Sahlstedt of Stockholm, to prevent the dog from biting or interfering with the apparatus.

Thomas is also expanding. He has a great many students and the place is teeming with enthusiasm and much life. He has a respiration chamber for which he received some special grants and this naturally attracted a good deal of my attention. Alongside of the respiration chamber is a Universal table, but he used no spirometer, employing instead the automatic oxygen valve attached to an oxygen cylinder with the trade name "Andos", which I saw at Dortmund in 1929. They also used the alcohol lamp of Meyer of Dortmund, using the electrical ignition. After the lamp is lighted the heating wire can be turned one side out of contact of the flame. The lamp is on the pyknometer principle and can be weighed before and after. Essentially the same thing, designed by Meyer, formerly of Dortmund, is used by Szarkall of Dortmund.

Thomas's Williams bottles are ingenious although clumsy. It was a form of Williams bottle in which no rubber connection was used, but the bottles were "tied" to each other with large U-tubes containing mercury so as to have a mercury seal between each bottle. (See figures 123 and 124.) This called for extensions on the ingoing and outcoming air so that the ends turned down and enlarged or flared to fit into the mercury seals. There is a tremendous amount of mercury used everywhere in the laboratory.

The respiration chamber had a 180-liter capacity and the top had a celluloid window with clamps to hold it down. The chamber seemed to me extremely large for a dog. There was a tube to draw off the urine and the bath temperature was controlled by an electric thermostat and heater. Thomas determines the oxygen and carbon dioxide together and then uses the oxygen analyses in calculations. In going into some calculations with him on this point, I saw that he got a 90 c.c. error of oxygen. Personally I saw no advantage in analyzing

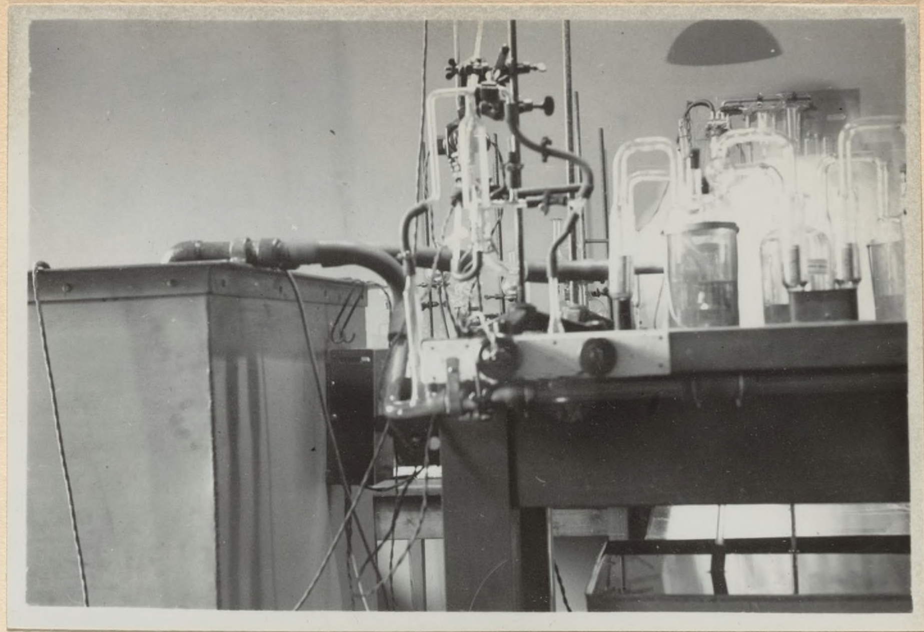


Figure 123. Leipzig, Germany. University of Leipzig, Physiological-Chemical Institute. Karl Thomas' dog respiration chamber with absorbing vessels for carbon dioxide and water at the right, showing their peculiar mercury seal. In the middle foreground is an alcohol check lamp developed at Dortmund.

Figure 124. Leipzig, Germany. University of Leipzig, Physiological-Chemical Institute. Absorbing train for Karl Thomas' respiration apparatus, showing bottles connected by mercury seals.

for oxygen in the residual sample. We do not and it seems to me an added complication, and with the degree of exactness he should determine it, a considerable error must enter into it.

The Leipzig laboratories both of Gildemeister and Thomas are very bad. They are crowded to the elbows and there are series of catacombs in the cellar, winding staircases, quaint corners here and there, and are wholly unsatisfactory for modern teaching. I don't see how students can have any respect for science or their "master" under such conditions but the fact remains that Thomas draws a great many students.

Lecture. On November 22nd I gave the "first" lecture at the combined lecture room of Gildemeister and Thomas, before an audience of 200. (There was an announcement of this lecture.)

**BIOLOGISCHE GESELLSCHAFT · LEIPZIG**

Am Dienstag, den 22. November 1932

wird im Anschluß an den schon angezeigten Vortrag von Prof. Achelis über Permeabilität und Stoffwechsel (im großen Hörsaal des Physiologischen Institutes, Liebigstr. 16, $18^{30}$ Uhr) noch ein weiterer Vortrag stattfinden: 1932

Prof. F. G. Benedict
(Direktor des Ernährungslaboratoriums in Boston, Carnegie Institut):

**Die Physiologie der großen Schildkröten und Schlangen und ihre Beziehungen zur menschlichen Physiologie.**

Lendle,
**Schriftführer.**

Gros,
**1. Vorsitzender.**

## LEIPZIG, GERMANY.

### University of Leipzig, Institute of Animal Physiology.

### Professor A. Scheunert.

I did not visit Scheunert's laboratory for the general impression was given me that he was now practically in "business", manufacturing vitamins and things of that kind. He had certain patented salts that he thought advantageous for agriculture, but this is no time for German scientists to sell foreigners anything.

Four years ago I learned that Scheunert was most interested in the synthesis of food. I got a rather poor impression of his rat colony.

## LEIPZIG, GERMANY.

### Agricultural Institute at Möckern.

### Professor Gustav Fingerling.

Although my time was rather limited at Leipzig, the one place I felt impelled to visit more than once was this laboratory of Fingerling's. Fingerling makes a very great impression upon me. I do realize his weak points. He is a meticulous experimenter and is very ingenious in many constructive details, but has been making too few experiments and getting too few results, partly due to the necessity of his having to "pass the hat".

The large respiration chambers are still employed on the Pettenkofer principle. (See figures 125, 126, 127, 128, 129, and 130.) In talking with him it is clear that the Kellner-Gustav Kuhn tradition dominates the situation and yet there are a lot of new and very clever things. His water aspirators to draw samples and his design to correct instantly for barometer and temperature changes are clever, but I think he works too much alone. There is no critic about to discuss matters with him. The surroundings are neat, the laboratory is kept very clean, and there is no impression of the so-called "cow chemistry". He insists on neatness even in the cleaning room where dishes are washed and foods weighed and things of that sort. The place is alive with animals and metabolism experiments are in progress. Thus, in the stalls were two heavy oxen with metabolism harnesses, two heavy horses, a large number of sheep, and double this number of squealing pigs. He has a very clever woman assistant who apparently maintains the accuracy and cleanliness of the laboratory.

Fingerling kindly gave me one of the typical protocols from one of his oxen, and this is appended herewith. (See page 222.) He states it takes a month to get an animal into equilibrium. He calculates from the respiration experiments the total energy per square meter and his value given on this sheet, 11021.2 calories, was determined from his respiration apparatus, but singularly enough, he uses the value 1086.0 calories as the basal value, taken from the literature. Apparently he had never determined it himself.

More than anything else Fingerling needs some collaborator to discuss his plans with and criticize him very seriously. However, as I stated above, I made two very advantageous visits and found him a most interesting, stimulating man. I feel all the more regret that it is impossible to get together at one conference Ritzman, Fingerling, Møllgaard, and Wiegner, with possibly Mitchell and Brody. That, however, apparently is ruled out.

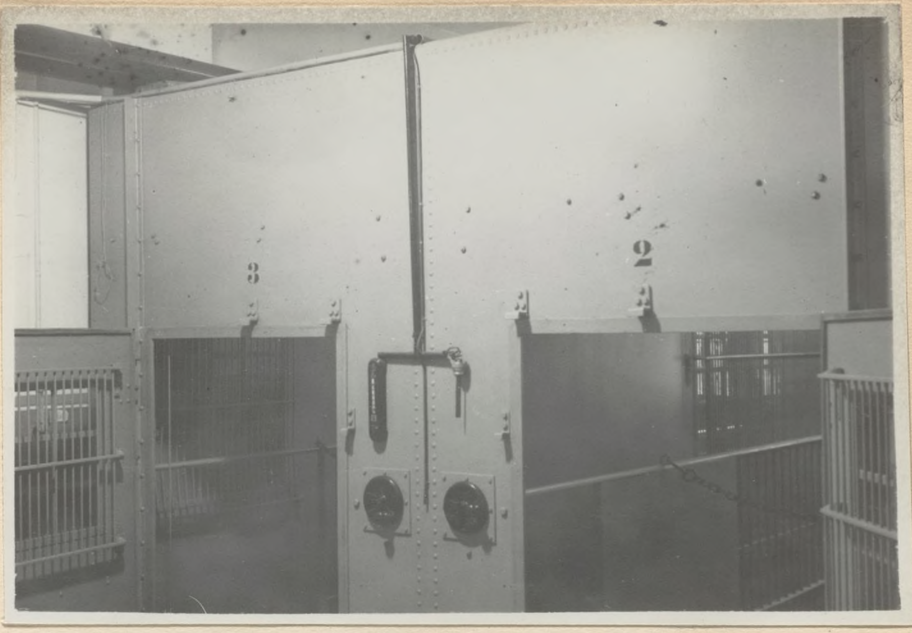



Figure 125. Leipzig-Möckern, Germany. Agricultural Institute, Professor G. Fingerling. Double respiration chamber for two animals.

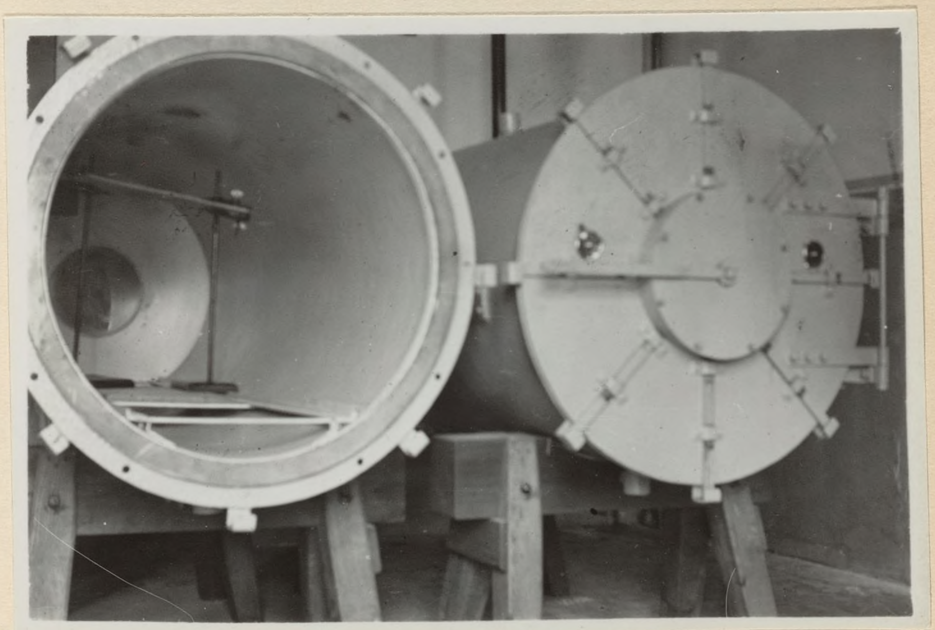


Figure 126. Leipzig-Möckern, Germany. Agricultural Institute, Professor G. Fingerling. Double respiration chamber for small animals, showing one chamber open.

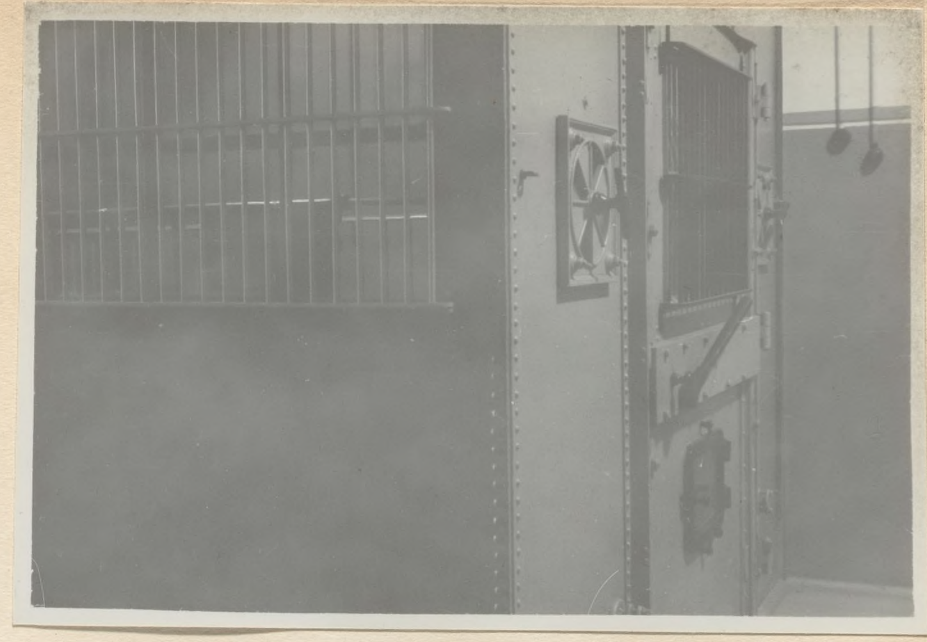


Figure 127. Leipzig-Möckern, Germany. Agricultural Institute, Professor G. Fingerling. View of respiration chamber for large animals.

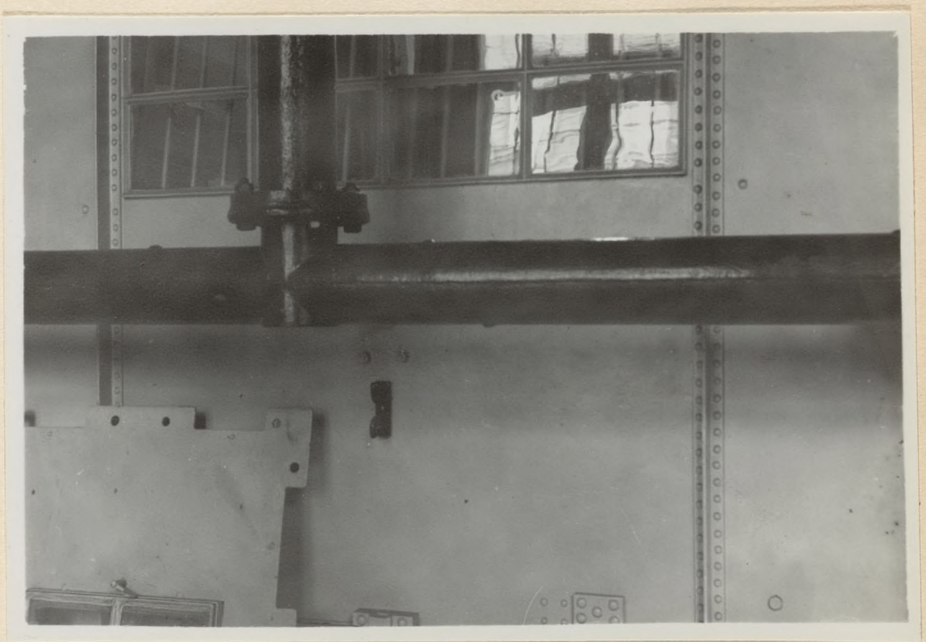


Figure 128. Leipzig-Möckern, Germany. Agricultural Institute, Professor G. Fingerling. Respiration chamber for oxen, showing massive construction.

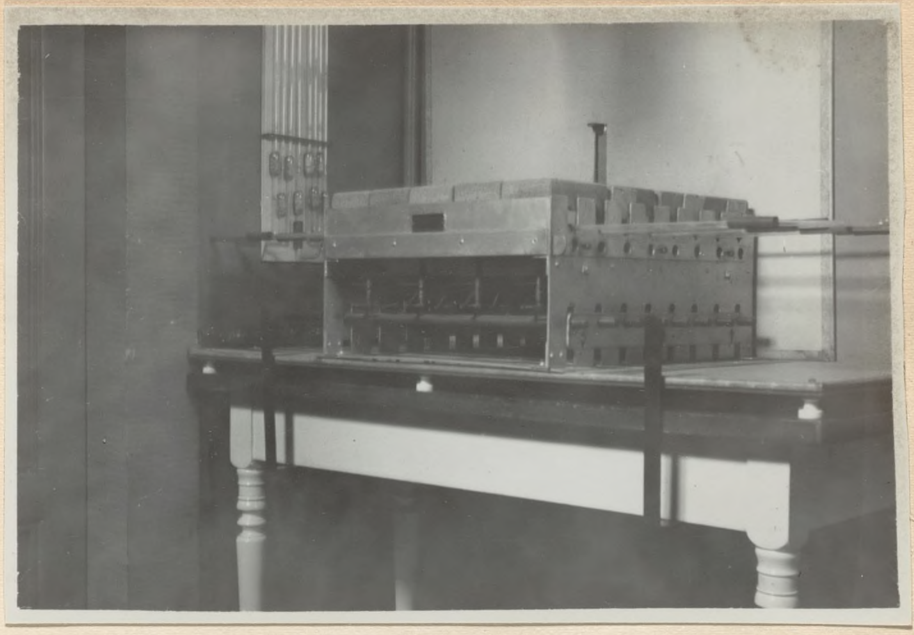


Figure 129. Leipzig-Möckern, Germany. Agricultural Institute. Professor Fingerling's furnaces for measuring the methane from his large respiration chambers.

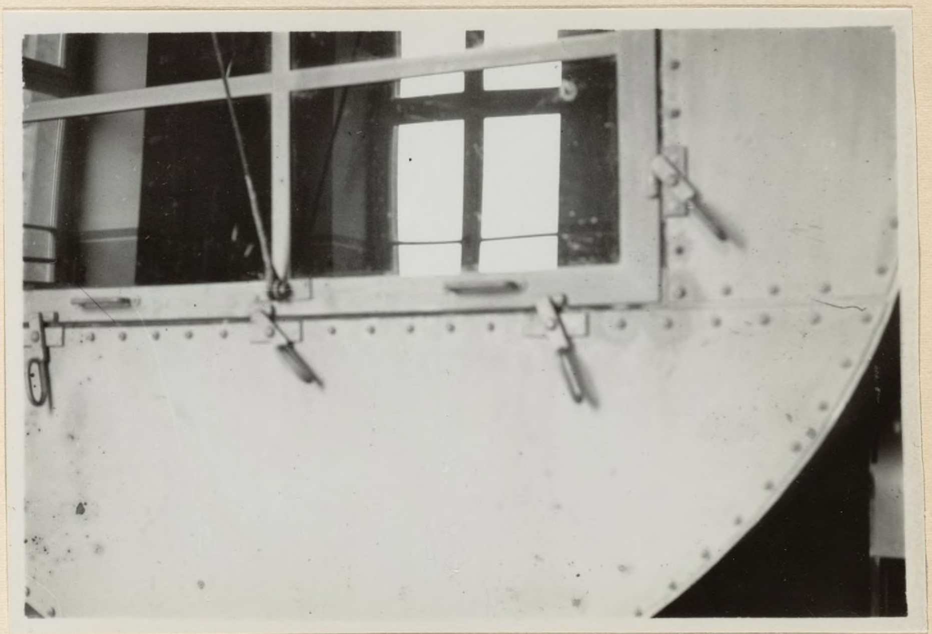
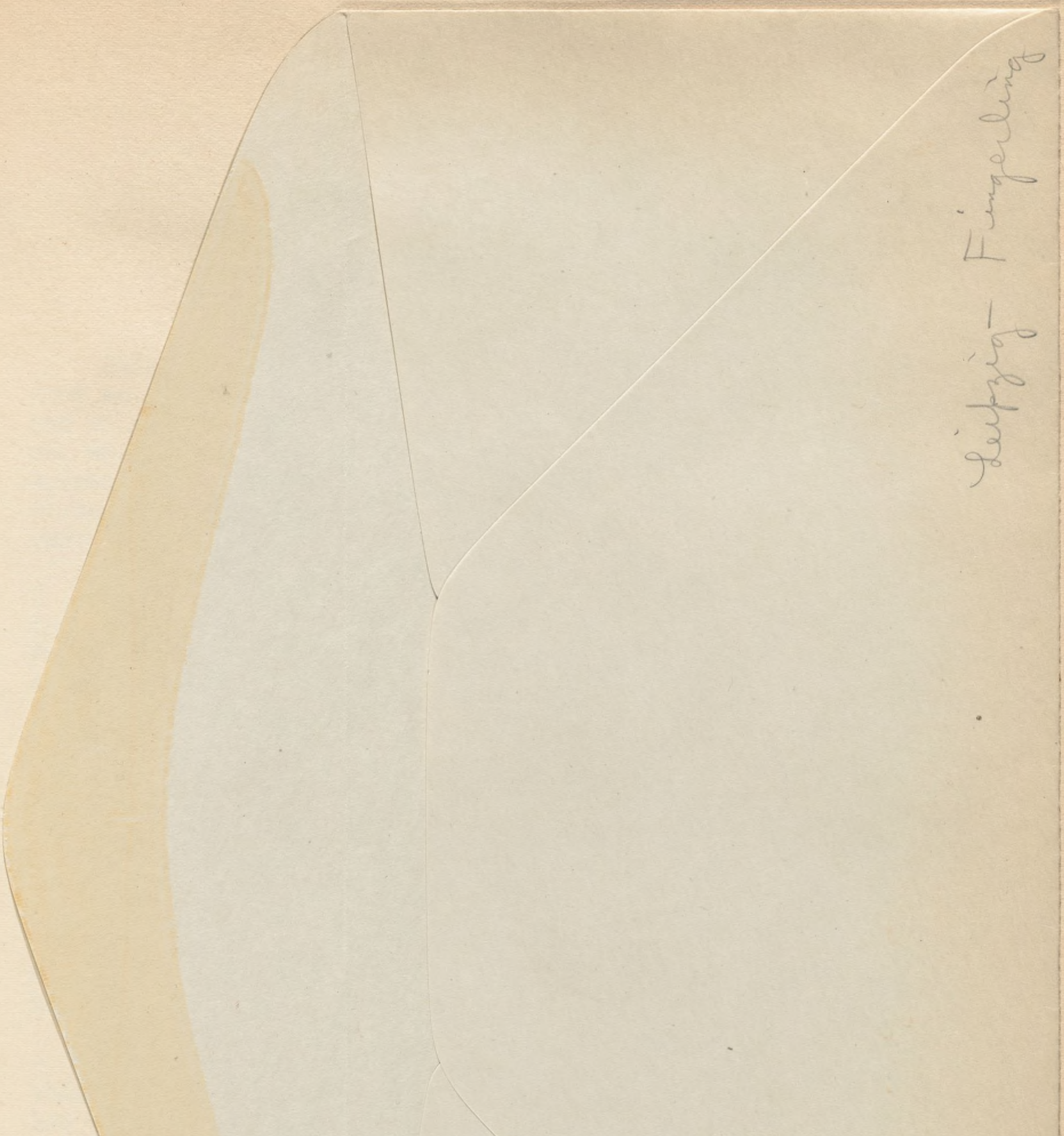


Figure 130. Leipzig-Möckern, Germany. Agricultural Institute, Professor G. Fingerling. Corner of circular chamber made of an old steam kettle. In my 1929 report, Figure 67, page 134, one can see this kettle out in the yard with Fingerling standing alongside.


Leipzig – Fingerling

Leipzig

Fingerling

the months fit to equilibrium.

## Hans, Periode 7.

Lebendgewicht : 554.0 kg  Oberfläche : 6.093 qm

30% under the [illegible] maintenance requirement

| Einnahmen: | | N | C | Cal. |
|---|---|---|---|---|
| 3574.8 g | Wiesenheu (Tr. S.) | 64.0 g | 1670.1 g | 16005.8 |
| 431.8 g | Hafer " | 8.6 g | 202.9 g | 1989.0 |
| 449.8 g | Leinmehl " | 26.7 g | 212.4 g | 2168.7 |
| 397.1 g | Melasse " | 8.8 g | 148.3 g | 1524.1 |
| | | 108.1 g | 2233.7 | 21687.6 |

| Ausgaben: | | N | C | Cal. |
|---|---|---|---|---|
| 8124.3 g | Kot | 25.8 g | 912.6 | 8911.1 |
| 8229.6 g | Harn | 78.9 g | 120.6 | 1264.9 |
| 48.9 g | Methan | - | 36.6 | 652.8 |
| Angesetzt | | 3.4 g | - 11.7 | - 162.4 |
| Umgesetzt | | - | 1175.6 | 11021.2 |
| | | 108.1 | 2233.7 | 21687.6 |

Metabolizable energy 11021.2 Cal.

From resp.-app.

pro 1 qm : 1808.8 Cal Brutto

" 1 " : 1086.0 " Netto

60.04 %.

from the literature

## HALLE, GERMANY.

### University of Halle, Physiological Institute.

### Professor Emil Abderhalden.

Abderhalden is always intensely busy with editorial work. He told me that in his Handbuch he had one thousand co-writers and had had only one bad experience in this lot. Freudenberg of Heidelberg, who wrote him about his American travels, very strongly criticized German books and Abderhalden's Handbuch especially. Abderhalden stated that Freudenberg demanded a "double honorarium" for his whole contribution if he were to write it at all.

Abderhalden discussed in detail the Halle Academy and he stated that the revivified "Nova Acta" are to publish conclusive articles. Thus v. Euler is preparing an article on vitamin A, and they had thought of publishing my "fourth article", etc., possibly the "K" article, but it was finally decided this would not be printed there. I agreed to send Abderhalden only articles that were translated, we to pay the expense of translating, and to use up the honorarium for buying separates. He is trying to renovate the entire Academy and get out much of the historical material. He has found an etching by Goethe, who was also a great natural scientist.

I saw nothing of the laboratory for Abderhalden was at the house all the time. He states he is the only Geheimrat in Halle, but that he being a Swiss and the nationalistic feeling being highly against him, they criticize him for his being a "foreigner" in his laboratory.

"Every idol has its feet of clay", and I am definitely assured that Abderhalden is working too hard to be happy. He does not know how to play and I fear he is losing his perspective in the home and family life, for he is so wrapped up in work and possible service outside. He seems to be completely lost in doing work and almost a stranger in the home.

<u>Lecture</u>. On November 23rd I gave the "first" lecture for the Halle Akademie, before an audience of 125. (There was an announcement of this lecture.)

Halle/Saale, 17. November 1932

Einladung

der Kaiserl. Leopoldin.-Carolin. Deutschen Akademie der Naturforscher zur außerordentlichen Sitzung, Mittwoch, den 23. November 1932, 20 h.

Ort: Physiologisches Institut.

Vortrag des Mitgliedes der Akademie Professor Dr. Francis G. Benedict, Direktor des Nutrition Laboratory in Boston/Mass. "Die Physiologie der großen Schildkröten und Schlangen und ihre Beziehung zur Physiologie des Menschen."

Gäste willkommen.

Der Präsident.
gez. Emil Abderhalden.

HEIDELBERG, GERMANY.

Kaiser-Wilhelm-Institut für medizinische Forschung.

Professor O. Meyerhof.

This is the first time I have seen Meyerhof since he left Berlin. I was anxious to see this equipment of which I had heard so much. Practically all work was done in a micro-chemical way. There is even a micro-bomb calorimeter and there were a great many forms of Warburg apparatus. They were studying the oxidation of slices of tissue of warm-blooded animals, also pieces of rat lungs, and everything was done in an extremely delicate, fine way. All the metabolism work was tissue metabolism.

The equipment is enormous. For example, I saw an oxygen cylinder marked "Abt. Meyerhof". There were several chambers of different constant temperature in which one can run anything from 30° to 37° if necessary, and there were several rooms for cooling. All the indications were of wonderfully fine biochemical work. Unfortunately most of it was outside my own particular lines and I did not get much of the details.

## HEIDELBERG, GERMANY.

### Kaiser-Wilhelm Institut für Pathologie.

### Professor L. Krehl.

I was astonished to find Professor Krehl in as good condition as he was. The evening before we had been at his home with an assistant, Dr. Schenk, and there he was just as full of enthusiasm as ever, and has great plans for the future. He is a man of 71 years or more and has had several grave illnesses, and I believe he is afflicted with stomach ulcers and has been through rather serious surgical operations.

There are at present no patients and all the work is on animal experiments except a certain amount of cancer tissue obtained from the hospital. There is a respiration apparatus for dogs, with water bath enabling one to raise the temperature to 44° C. (See figure 131 .)

There is what I think is the only existing one of its type, a Rubner calorimeter in its original form with small spirometers and every connection. (See figures 132, 133, 134, and 135.) Krehl was of course associated with Rubner in Marburg and hence has always been very keen about Rubner's ideas and plans. He had had this calorimeter a long time and, as I understand it, only recently got it in order. He and a number of others spent about a year putting it in order. To me it seemed like love's labor lost, for I can't imagine doing much with it. He had an interferometer of Zeiss and the 3-meter length tube which apparently he thought was going to function satisfactorily for extremely accurate gas analysis. (See figures 136 and 137.) No definite results thus far had been obtained with it.

The remainder of the building is reserved for an experimental hospital with 12 beds. In addition to the 12 beds, he must have nurses' rooms, kitchen, laboratory, etc. Already they are too crowded in the building since they have not used this section. This hospital section is not furnished, but they have applied for $120,000 from the Rockefeller for 12 beds. They have left a space for a Grafe-Jaquet chamber for man.

Here is a very fine group of men, very active, but one can not but think what will happen to a man starting in on this new project at 71 or more years. There is the problem of who will carry on for the next few years, for example.

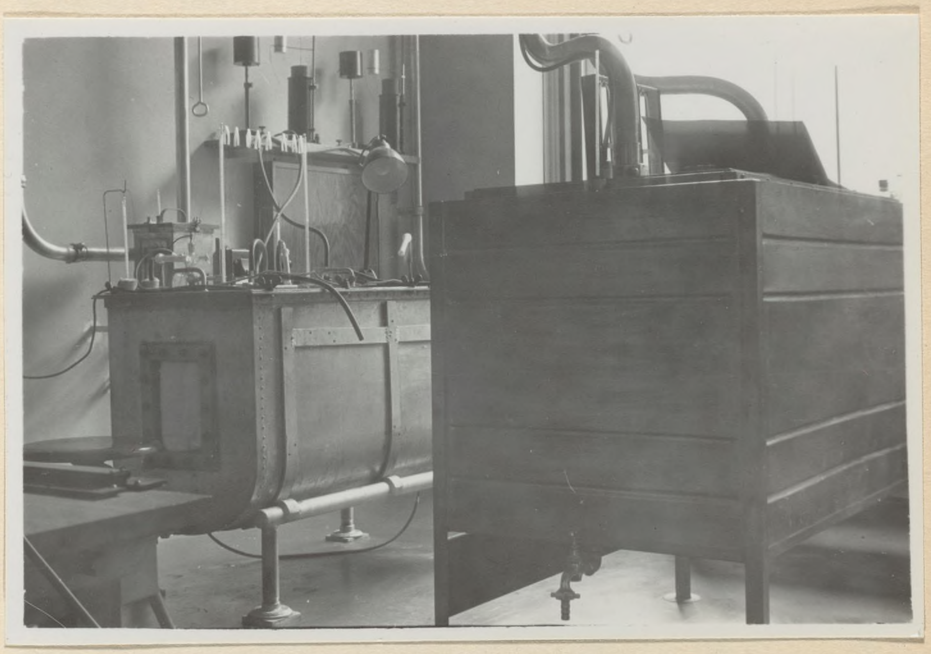


Figure 131. Heidelberg, Germany. Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. Respiration apparatus for dogs, right foreground. Rubner calorimeter, left, with small spirometers for the calorimeter across the wall on the left.

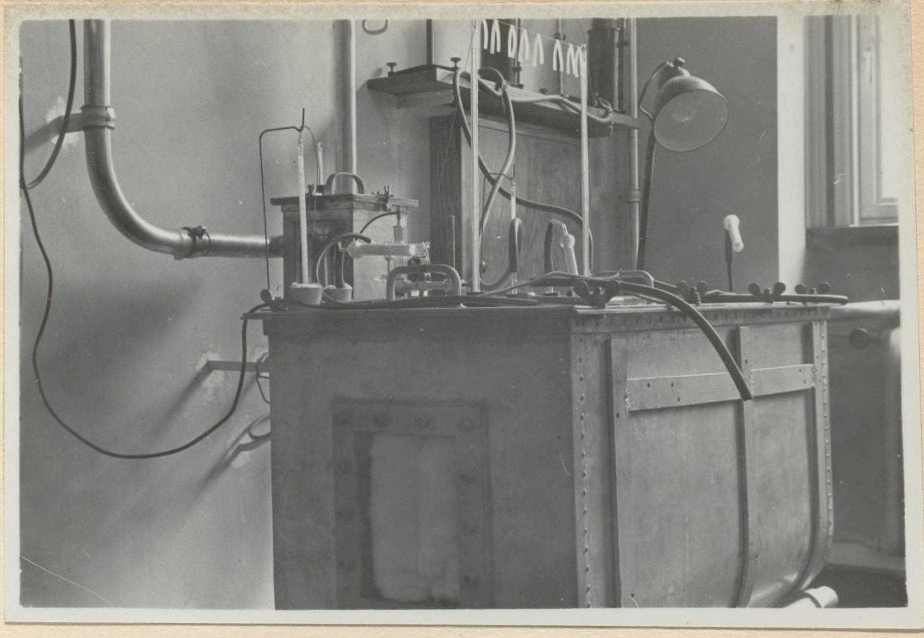


Figure 132. Heidelberg, Germany. Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. Rubner calorimeter in its original form.

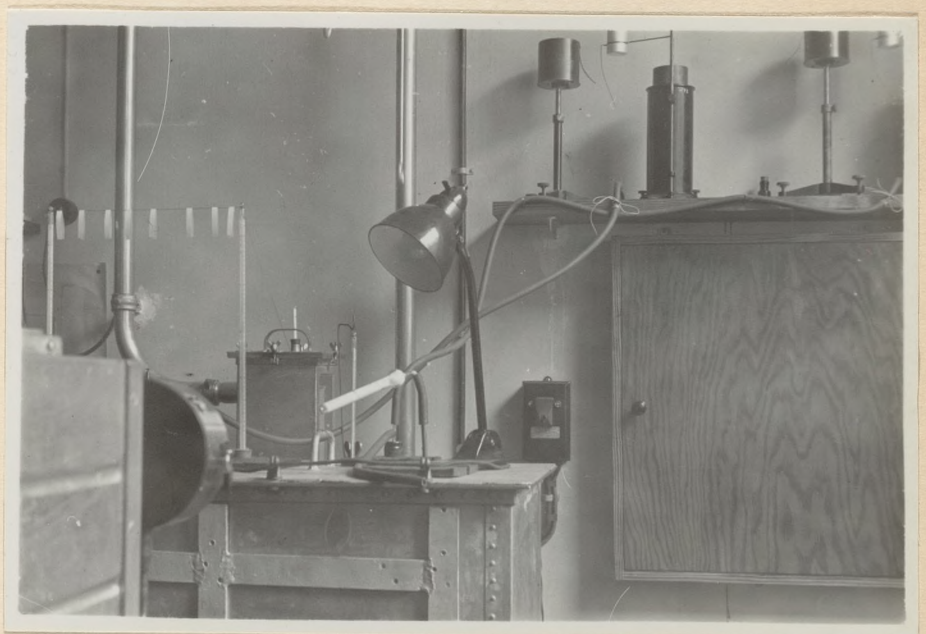


Figure 133. Heidelberg, Germany, Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. View showing part of Rubner calorimeter, showing one spirometer on shelf at right. Edge of dog respiration chamber at the left.

Figure 134. Heidelberg, Germany. Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. Top of Rubner calorimeter, extreme left lower corner. In the right lower corner is a respiration chamber. In the rear is a small respiration chamber (Desiccator) for small animals.

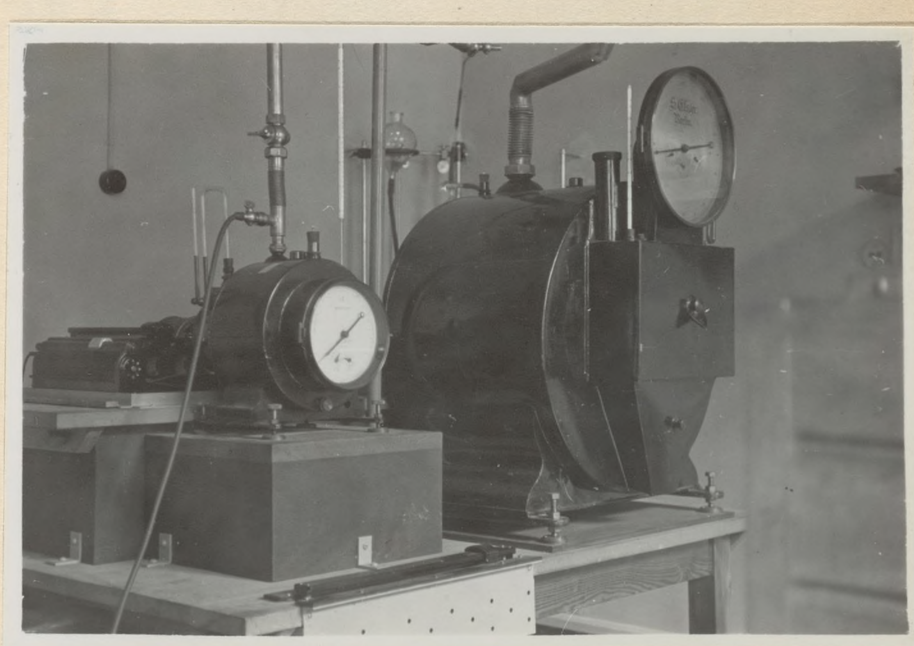


Figure 135. Heidelberg, Germany. Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. Photograph showing two gas meters for measuring air current and sampling air current coming out of respiration chamber.

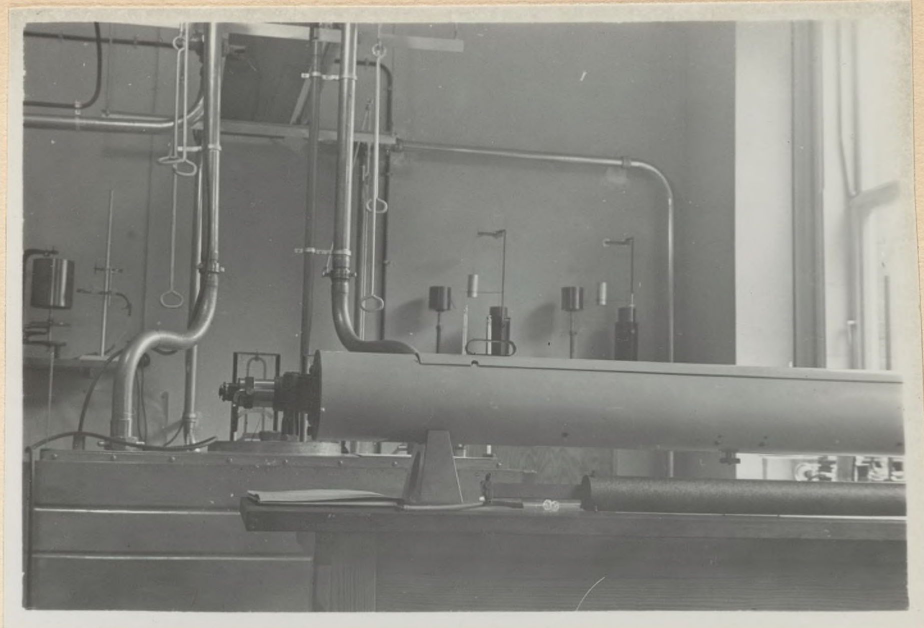


Figure 136. Heidelberg, Germany. Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. In the right foreground is the large Zeiss interferometer; in the lower background, a respiration chamber; and on the wall at the right rear, the two expansion chambers or spirometers for the Rubner calorimeter.

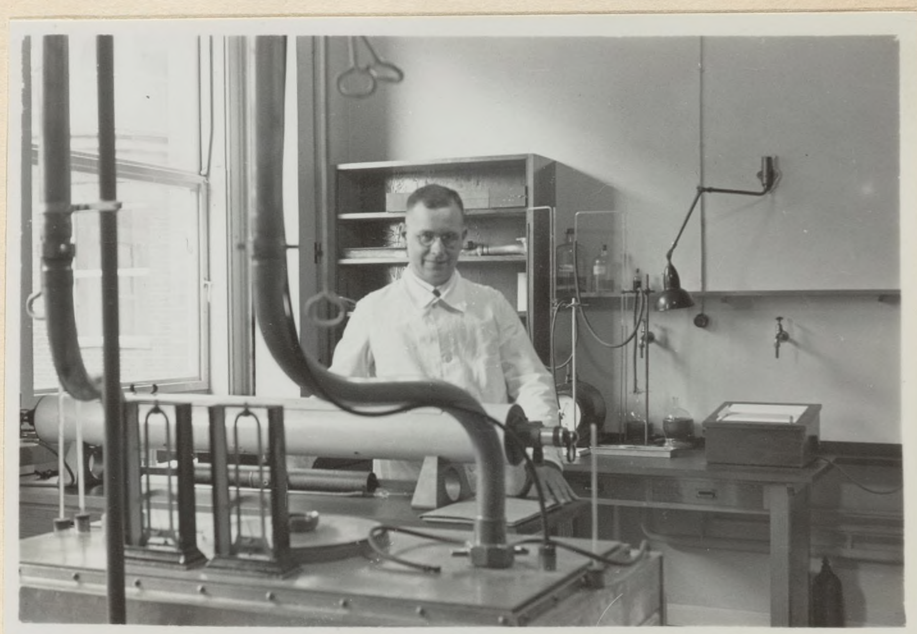


Figure 137. Heidelberg, Germany. Kaiser-Wilhelm Institut für Pathologie, Professor Krehl. Assistant standing back of Zeiss interferometer. Respiration chamber for dogs in the immediate foreground.

HEIDELBERG, GERMANY.

University of Heidelberg, Laboratory of Chemistry.

Professor Karl Freudenberg.

On a visit to America about a year before this time, Professor Freudenberg, who is the successor to Victor Meyer and Curtius, visited the Nutrition Laboratory and had made a special point while on his American tour of looking up former Heidelberg chemistry students. I told him that I had a great desire to give a lecture in my old chemical lecture room where I had attended lectures for two semesters, telling him that I was looking forward to getting quite a "kick" out of such an arrangement.

Freudenberg invited me to lecture in this lecture hall but it seems the clinical people, especially Krehl, advised him to give this up, saying that the medical people would not come over to the chemical laboratory and inasmuch as my lecture dealt with metabolism of man it would be far better to give it in one of the hospital lecture halls. I was much disappointed, much more so than I would have imagined. However, I was given every attention and after a delightful time with Freudenberg at his house it was a real treat to go over this building where I had been as a student so many years before and see the various changes that they had made.

While Freudenberg's problems were all of a highly chemical nature and hence of relatively little interest to me, I was impressed by the feverish activity of this laboratory.

The laboratory was shockingly poor and every corner was made use of. There was no sense of orderliness or cleanliness but they had the usual mess of the standard organic laboratory. Freudenberg is a man of great dynamic personality and has a wide variety of interests. He seemed to me a stimulating person and a wonderful successor to my old master, Victor Meyer.

HEIDELBERG, GERMANY.

Lecture.

On November 25th I gave the "third" lecture at the Medical Clinic, for the Naturhistorisch-Medizinischer Verein zu Heidelberg. There was an attendance of about 95 at this lecture.

# Naturhistorisch-Medizinischer Verein zu Heidelberg

## Medizinische Section

**Freitag,** den 25. November 1932, 20½ Uhr im Hörsaal der **Hautklinik** (Voßstraße)

Tagesordnung:

Herr Prof. **Francis G. Benedict**-Boston (als Gast): Über den menschlichen Grundstoffwechsel im Lichte neuer Untersuchungen.

## WÜRZBURG, GERMANY.

### University of Würzburg, Medical Clinic.

### Professor E. Grafe, Professor H. Bohnenkamp, Professor H. Rietschel, and Dr. F. Strieck.

I had looked forward particularly to the visit to Würzburg on account of the close relations with Dr. Strieck and also on account of the fact that in the spring of 1932 Würzburg very kindly conferred the honorary degree of Doctor of Medicine upon me. I had also read a great deal of a series of papers by Professor Bohnenkamp, papers that had provoked a great deal of discussion all along the route in Europe, and I was extremely anxious to see him because he laid great stress upon the importance of insensible perspiration. My chief center, really only center, at Würzburg was Grafe's clinic. (See figure 138.) Here one finds, as always, everything alive with activity. Grafe is a tremendously active man, interested in many problems and particularly in metabolism. Dr. Strieck had been influenced by his sojourn at the Nutrition Laboratory and was endeavoring to introduce American methods all along the line.

Respiration chamber. They use still the large Grafe chamber with a train of gears to shift speeds, like automobile gear shifts, changing the rate of ventilation of the chamber depending upon the size of the individual. The Krogh apparatus is used a good deal, especially in the experiments of Bohnenkamp where the person is standing. I was told that they had a Benedict-Roth apparatus but I did not see it. There was nothing new in apparatus as such but much work.

Dr. Strieck said they were much interested in the extracts of the hypophysis and were giving special attention to the heart centers and to the metabolism centers. A great deal of activity was going on but Dr. Strieck sounded and talked very depressed. He was very much interested in the so-called "negative phase" of basal metabolism as emphasized by Bernhard in the His Clinic in Berlin. I wrote him later that I felt there was nothing in it, that we had never made any special experiments to demonstrate it but in all our twenty-five years' experience we had never found any sub-normal conditions other than those explainable by drowsiness or possibly of course under-nutrition. Later he told me that they had been unable to confirm Bernhard's findings.

In the same hospital Professor Hans Rietschel in the children's department had two respiration chambers on the Grafe principle, one for a child and one for a baby. The small one was extremely well constructed and provided with apparatus for measuring the activity, and one found here also the duplicate of Dr. Carpenter's apparatus with Dr. Strieck's electrical mechanical device for raising and lowering

Figure 138. Würzburg, Germany. Left to right, Mrs. Grafe, Mrs. Benedict, and Professor Grafe.

the mercury. Apparently Professor Rietschel was also interested in the insensible perspiration of children, for he had one of the typical Sauter balances for the child's insensible perspiration, finished with very great elegance and refinement but equipped with the inevitable horsehair mattress. This seems to have clung to the whole insensible perspiration thesis.

Grafe related innumerable incidents with regard to Rubner and the older physiologists. He pointed out that Pflüger always combatted Voit and that Rubner told him that Voit had held up the publication of Rubner's paper on the isodynamic law. Rubner was only 25 years of age at the time and the paper lay in the drawer for two years. Rubner also told Grafe that there was not one idea in Voit's head. The whole story of Pflüger's ego and the dedication of Zuntz' books on high altitudes to Pflüger was repeated. Pflüger objected to the simple dedication "E. von Pflüger". Zuntz replied that on the Bismark "Denkmal" there was only "Otto von Bismark". Grafe also told me that Pflüger was always in controversy with Ludwig and that Pflüger was invariably right but Pflüger had no "school" and Ludwig had the great school, so his ideas and personality went much further than did Pflüger's. It was interesting to see the relationship between the different schools but Grafe, who had been so long at Heidelberg, said he thought that Krehl was a very much bigger man than F. v. Müller of Munich and he thought that the latter would be wholly forgotten in 100 years, but not Krehl.

Someone, not in Würzburg, commenting on Grafe's dog upon which he published his long discussion of the adjustment of the animal to the environment and to the different fasting levels, a criticism that I raised very strongly in the fasting man book, said that Grafe's dog had an enlarged thyroid and that dogs were especially liable to upset thyroid and he felt that this was the case with Grafe's dog. Of course there is no means of knowing now, but it is an interesting point in the argument against doing too much work with dogs.

Bohnenkamp. The newest things in Würzburg were the work of Bohnenkamp. I had not met him before or at least did not recall having met him. He is a most serious-minded, charming chap, a very good man to talk things over with, critical but perfectly willing to give and take, a most helpful and encouraging man for discussion. One felt perfectly free to talk over every point with him and criticize as sharply as one wished and nothing would be taken amiss. I was interested in his problem of studying the surface area of the body and getting the average temperature of the body and all the various steps in his series of papers. For the average temperature of the body he used the resistance thermometer. He had a "fish line affair" made up of 30 meters of fine copper wire on a reel. The subject first removed the clothing and he then wound the wire around and around the body from head to foot, in about 1 centimeter intervals. In about 30 minutes the temperature is constant with a room temperature of 25°. It is important now to emphasize that Bohnenkamp is strong for 24° to 25° C. One recalls that Rubner did everything at 16° C. (See figure 139 .)

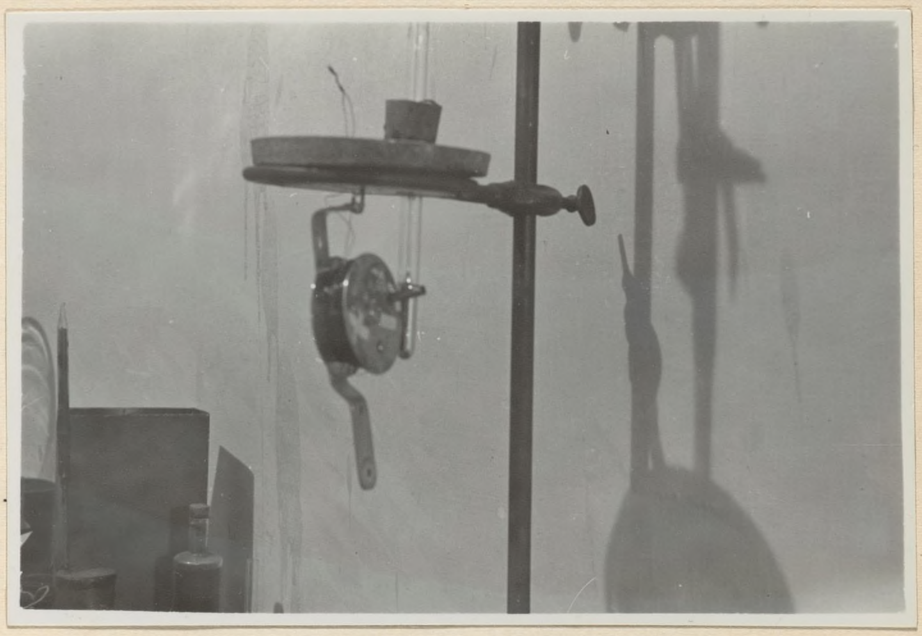


Figure 139 . Würzburg, Germany. University of Würzburg, Medical Clinic. Laboratory of Professor Bohnenkamp. Reel of wire used by Bohnenkamp to wind around the body of a man to measure the resistance and get the average room temperature. The thermometer bulb is shown near it, showing that the apparatus is at an initial temperature to measure its true resistance before winding in about the man.

Bohnenkamp determines the metabolism with the Krogh oxygen apparatus and he computes the average temperature of the body from the metabolism and the surface area itself, and states that the surface temperature must be a certain temperature which he finds agrees within 0.5° C. with what he finds with his resistance thermometer. He uses the old calculations by Boothby, Du Bois, and Harris and Benedict, and by calculating the metabolism he finds the direct is lower than the calculated.

Radiating surface area. To get the radiating surface area Bohnenkamp uses the condenser principle and maintains that by determining the electrical condensation he can get the surface. At about this time I received a letter from Wardlaw in Australia, who states that a physicist claims that Bohnenkamp is all wrong on his surface method by condensation principle. Later I had a letter from Wardlaw giving the details of this and I forwarded this letter to Bohnenkamp, but at the day of writing (June 23) I have not heard from him. Bohnenkamp is dominated by the surface temperature. This of course is obvious, for while Bohnenkamp recognizes the feathers ruffled in the bird, as affecting heat loss, he does not think of any change in the surface distribution of blood to vary the heat brought to the surface.

I think Bohnenkamp has a physical mind but Strieck says he is one of the best physicians in the clinic - a medical-physiological physicist. One thinks of H. B. Williams. On the whole Bohnenkamp is one of the keenest men I have ever met, but is especially deficient in comparative physiology. It would help him out a great deal, for he has accepted the surface area wholly. There is one important modification. He has little or no use for the calculated surface area for he recognizes, for example, that between the legs and under the arm-pits there is a large amount of surface that does not give off the heat that other parts do. He is not interested in the measured surface area as such but, as he calls it, the "radiating surface".

Bohnenkamp has direct calorimeters but he advises strongly against them. He accepts insensible perspiration *in toto* but I find Grafe does not accept it for fat people, as Grafe's assistant, Magendantz, finds that obese, at least certain types, do not lie anywhere near the Benedict-Root straight line and they consider there is something else in play here. Having found out that they did this work with the stuffed hair mattress I am quite inclined to question it. Bohnenkamp's laboratory is filled with physical apparatus of all kinds. (See figure 140.)

I obtained a very fine impression of Bohnenkamp. There is, I understand, a strong likelihood of his leaving Würzburg, and I am afraid this means that he may get into a place where clinical and administrative duties will take him away from research. I talked with a great many people about him, and most people thought he was crazy, meaning he was wrong. My impression is this. I don't care whether he is correct or not. He has rendered a very great service to physiology in making people think about these problems as they never have before.

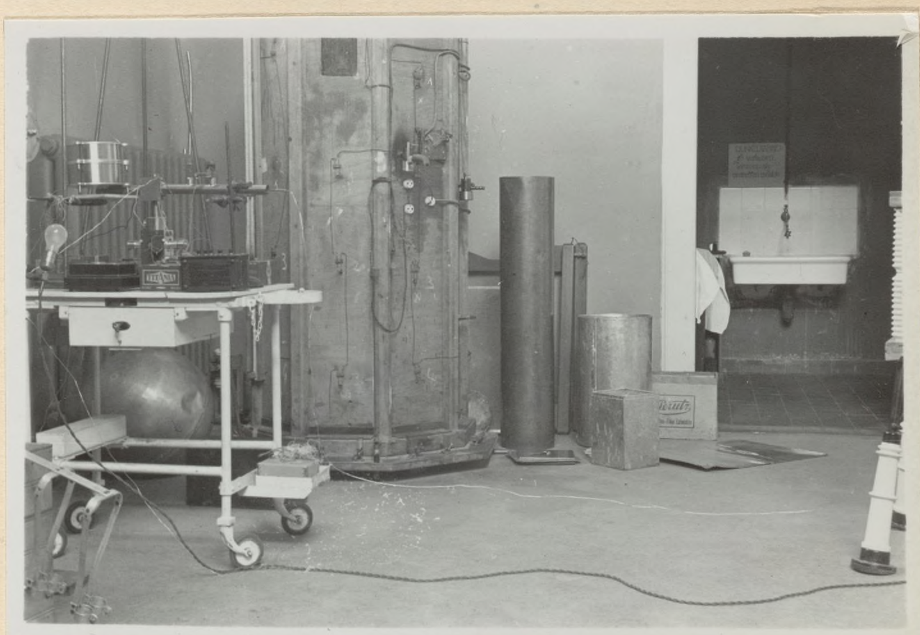


Figure 140. Würzburg, Germany. University of Würzburg, Medical Clinic. Part of Bohnenkamp's laboratory, showing a hexagonal, vertical chamber used in radiation measurements.

Dr. Strieck. It was a matter of disappointment to both Mrs. Benedict and myself to find Dr. Strieck so depressed. We had him with us every moment while we were in Würzburg and he was kindness personified in helping us in every way, but there seemed to be a definite note of depression about him. This was caused partly by the health of Mrs. Strieck. She had not returned to her usual health after the birth of a baby lately but was about and doing well. He felt the economic situation in Germany was very depressing and the whole thing had changed him pronouncedly. He was not the individual we saw in Boston.

Only one time did I find Dr. Strieck as he was formerly, that is, one evening when we had had a delightful dinner at Grafe's and afterward I put on some magic effects, and I brought Strieck more or less into the thing as assistant and co-worker. I noticed repeatedly that he was keenly interested and excited and I think regained his personality as he was with us in Boston. I feel he is an exceptionally good man, level-headed, a very careful worker, and it is a pity that he can not be in a position where he does not have to worry and can go ahead with characteristic drive on various questions that are of so great importance to him.

## WÜRZBURG, GERMANY.

### University of Würzburg, Physiological Institute.

### Professor Edgar Wöhlisch

One of the saddest features of our visit to Würzburg was the absence of our dear, good friend, Professor von Frey, who passed away at the age of 80 years only shortly before. His successor is Professor Wöhlisch and it was a great pleasure to realize that they had a good man to take his place. Wöhlisch made a fine impression on me. He is a professor of physiology, interested in psychology. Although carrying out nothing in the lines definitely associated with the Nutrition Laboratory work, he is an addition to a splendid group of men at Würzburg represented by Grafe, Bohnenkamp, and Strieck.

WÜRZBURG, GERMANY.

Dr. Hermann Rein.

In Würzburg I heard a good deal about an apparatus by a Dr. Hermann Rein who is now in Freiburg. This was a respiration apparatus in which the analyses are based on differences in the electrical conductivity of the air entering and leaving it. I had never seen the apparatus itself, although at Heidelberg in the laboratory of Krehl they showed me photographs of it.

It is manufactured by F. Hellige in Freiburg i. Baden, Germany, and I have written for a description of the apparatus. A letter received from the manufacturers in July, 1933, shows that the apparatus in all probability has not got the required sensitivity that we desire. I did not think it would do at all what the Heidelberg people said or thought it would do. It is more ingenious than practical.

## WÜRZBURG, GERMANY.

### German translation of Nutrition Laboratory manuscript.

The problem of getting our communications translated into German before submitting them to various journals and to Abderhalden's Handbuch had acutely arisen in connection with Fräulein Steuber's attempting to translate for us. After repeated, hectic correspondence with Abderhalden I agreed to try to find someone to do this for us, so all along the line I was on the lookout to get these translations made. Atzler had suggested someone in Berlin but it developed that this particular person was a philologist, and while knowing perfectly the mechanical construction of the English and German languages he had no experience in translation of scientific, much less medical and physiological material.

As a last resort I appealed to Strieck, realizing that he could not personally attend to these translations as he did so kindly to my lectures. Strieck introduced me to a man in the clinic, a Dr. Hohenrein, a man who seemed particularly an ideal man for this job. He spoke English perfectly (one parent was English) and he was a medical man, so I supposed our problem was solved. I had Abderhalden send to him the so-called "K article" and left Würzburg happy in the thought that we had at last a good translator. Subsequently, indeed before I left Europe, I had a wail from Abderhalden that he was hearing nothing from Würzburg and was getting very anxious about the translation. This was partly explained by the fact that there was a death in Hohenrein's family, but it finally comes about that we can not count on Hohenrein and the whole thing was unsuccessful.

WÜRZBURG, GERMANY.

Lectures.

In Würzburg there were three lectures. The "third" lecture, on human metabolism, was given on the 29th of November at Grafe's clinic, to an audience of 265.

On December 1st, at noon, I gave the "second" lecture, on comparative physiology, in the lecture hall of Professor Wöhlisch, before an audience of about 285, many of them students.

The "first" lecture was given on the evening of December 1st before the Physical Medical Society. There was an attendance of 115. (The announcement of this lecture, printed in German, is included in this report.)

# Physikalisch-medizinische Gesellschaft

## X. Sitzung

**am Donnerstag, den 1. Dezember 1932, abends $7^{3}/_{4}$ Uhr, im Hörsaal des physiologischen Instituts**

Herr **Prof. Dr. et h. c. Francis G. Benedict,** Nutrition Laboratory, Boston: Die Physiologie der grossen Schildkröten und Schlangen und ihre Beziehung zur menschlichen Physiologie.

**Studierende sind zum Besuch des Vortrages eingeladen.**

Würzburg

## MUNICH, GERMANY.

### University of Munich, Second Medical Clinic.

### Professor Friedrich v. Müller, Professor Kurt Felix, and Dr. Bauer.

In visiting Munich I was certain, of course, to find nothing of particular interest to us at the Laboratory of Physiology of the University, but Munich was en route to Vienna and Budapest and I wanted to see again K. Felix and Professor F. v. Müller and a number of others, so I felt it was worth while stopping off. I also had been invited to lecture at the University of Munich.

One of my first visits was to Müller's clinic, where I met Dr. Bauer, who was occupied with metabolism work in the clinic and has a great deal to say about the so-called "negative phase" of basal metabolism. Nothing that he showed me, none of his protocols, convinced me that he was dealing with anything corresponding to a "negative phase". I also met Dr. Dietrich Jahn, who had written a great deal and worked in New York with Lusk and Du Bois, but I don't even remember his special topic. The man did not impress me at all.

Professor Kurt Felix, who is associate professor in the University, is working as a biochemist and has charge of the biochemical work in Müller's clinic. Since everything was of a purely chemical nature there was little that interested me other than the personal and social relations with the Felixes.

MUNICH, GERMANY.

University of Munich, Institute of Hygiene.

Dr. H. Ilzhoefer.

I then went over to the Hygiene Laboratory and saw Dr. Ilzhoefer, who had formerly done work on metabolism during mental effort. It was very sad not to see my dear, good friend, Professor Max v. Gruber, who had died since I was here before. Ilzhoefer is no longer working on metabolism but in discussing the question of comparative metabolism and especially some recent protocols I had had from the Laboratory in Boston showing the great tendency for mice to show a sub-normal temperature, Ilzhoefer told me that he had a lot of sub-normal temperatures on mice that had not been reported.

Ilzhoefer had done work with a collaborator, a Dr. Kraut, who sent the laboratory a reprint. He had been doing some work on metabolism but was chiefly interested in humidity problems, water vapor given off from the skin. They had a chamber with a rubber diaphragm through which the head could go, in which they could study water vapor and carbon-dioxide. On the whole the laboratory presented little of interest to us.

MUNICH, GERMANY.

University of Munich, Physiological Institute.

Professor Otto Frank.

Frank lived up to his reputation as the greatest grouch and pessimist in Europe. He freely discussed and particularly cussed nearly everybody in Europe. For example, he is very much against Abderhalden, says he has a dirty laboratory and can not do good work. One must say with regard to Frank's laboratory that it is very clean.

Frank was in controversy with his former assistant. The man who stuck by him the longest was Bremser, formerly in Basel and now in Heidelberg. They have now split. Frank maintains that Bohnenkamp is no good. Then to cap the climax he said Professor Krehl was the most uncritical man in Germany. This went on all the time we were talking with him.

Lecture. On December 2nd I gave the "first" lecture in Professor Frank's lecture hall. This was very well attended, with about 250 present, and I was much pleased to note Friedrich v. Müller, Romberg, and others of the faculty on the front seats. It was a very stimulating crowd.

BUDAPEST, HUNGARY.

University of Budapest, Physiological-Chemical Institute.

Professor Paul Hári and Dr. Zoltán Aszódi.

The chief reason for going to Budapest was to make contacts again with Professor Hári and his associates, notably Dr. Aszódi and Dr. Ernst, so I went immediately to the laboratory and in fact spent the greater part of my time there. Professor Hári, unfortunately, was in very poor health, which at first we were inclined to ascribe to a neurosthenic condition but it evidently was something more serious, for we were greatly shocked to learn in the spring of 1933 that he died of heart failure. A more courteous, kind gentleman never lived. He was a great scientist and a keen critic. He will be very much missed. It was a great pity not to have him spend an academic year with us in Boston as we had hoped to have him do. In the laboratory he was not so active, being much occupied with writing, and his poor health had greatly reduced his capacity for work. (See figure 141.)

Studies on specific dynamic action with dogs. The experiments were usually in short periods and they spend many months hunting for a dog that will stay quiet. At the time I was there they had two dogs they were preparing for experimental work. The demonstration here was much like that shown me in Dortmund; that is, the dog minded but there was every evidence of being thoroughly cowed. One of the great problems was the comparison of the direct and indirect calorimetry. This should of course agree in 20-minute periods while it would be less likely to agree in periods of one hour or even two hours. Hári finds, however, that they do not agree in short periods of feed. He had for his thesis that the splitting of fat may take place some time prior to the actual oxidation of the end products, although I question whether or no this would not assume too much heat of cleavage, but no doubt the fact remains that they are serious workers. Both men are strongly of the belief that oxidation in the body takes place in stages and that the heat production is not parallel to oxygen consumption or at least not parallel to the cleavages.

Insensible perspiration. They had an insensible perspiration balance much like those one finds in Europe, furnished by Sauter for children, and used it to weigh animals to 0.01 gm., they said. There were female dogs that were catheterized to prevent urine being passed during experiments. The dogs were invariably 72 hours fasting. In the calorimeter they used from 1300 to 1800 liters per hour for ventilation. This seems a very large amount, for this is more than one would use with a man, but I presume that the reason for this is to prevent any possible condensation of water. The dog that was in the apparatus when I was there was in for the third time and apparently was remaining very quiet and proving to be a good experimental animal.

Figure 141 . Budapest, Hungary. University of Budapest, Physiological-Chemical Institute. Photograph of Professor Hári, standing near the control table of his calorimeter.

Although I had studied the Hári respiration calorimeter several times before, I had never realized there were so few thermo-junction points between the two chambers and I could not understand why the chamber was painted black on the inside. Fearing that this would contribute toward insulation rather than free loss of heat I asked Hári, and he explained it as being an attempt to direct the heat along the metal so as to have the thermo-junction points all as equally affected by the heat as possible.

The light was very poor as usual and my attempts to photograph things were most unsuccessful. (See figures 142 and 143.)

I had opportunity to talk with these two remarkable men a great deal. The conditions under which they work are very severe. I have the impression that the situation is a little better than it was four years ago, but the fact that several times an assistant brought in a bottle of urine that Hári had to analyze immediately or place one side for analysis later showed that he is still under the necessity of supplementing a very poor university salary by such analyses as he can make. On the other hand, one was lost in admiration of the fact that these men could do work at all under the conditions under which they had to work. As four years ago, I found Aszódi at the laboratory at early hours in the morning carrying out research and going home for private practice and consultation hours and then back again to do his reading and writing at night. There was one point brought to my attention when Hári was talking, and that was that Hari has had to do all of the writing. I can understand that would be the case with many of his younger assistants, but I directly got the impression that in the case of Aszódi the writing was done by Hári. If this is the case it is going to make it very difficult for Aszódi to carry on.

After conferring with Hári and Aszódi I decided it was not worth while to visit the Agricultural Institute presided over by Weiser.

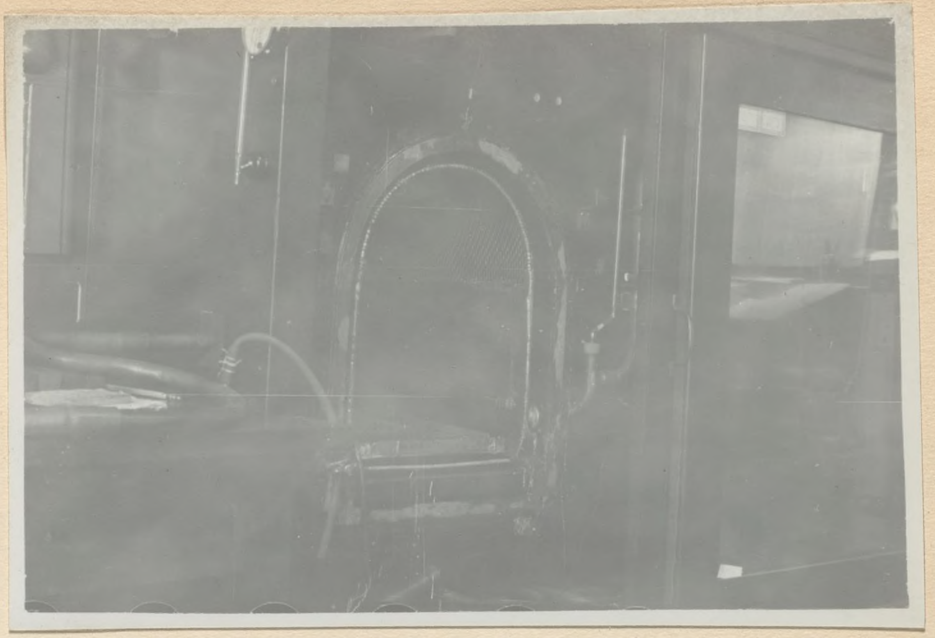


Figure 142. Budapest, Hungary. University of Budapest, Physiological-Chemical Institute. Entrance to dog or rabbit calorimeter used by Professor Hári.

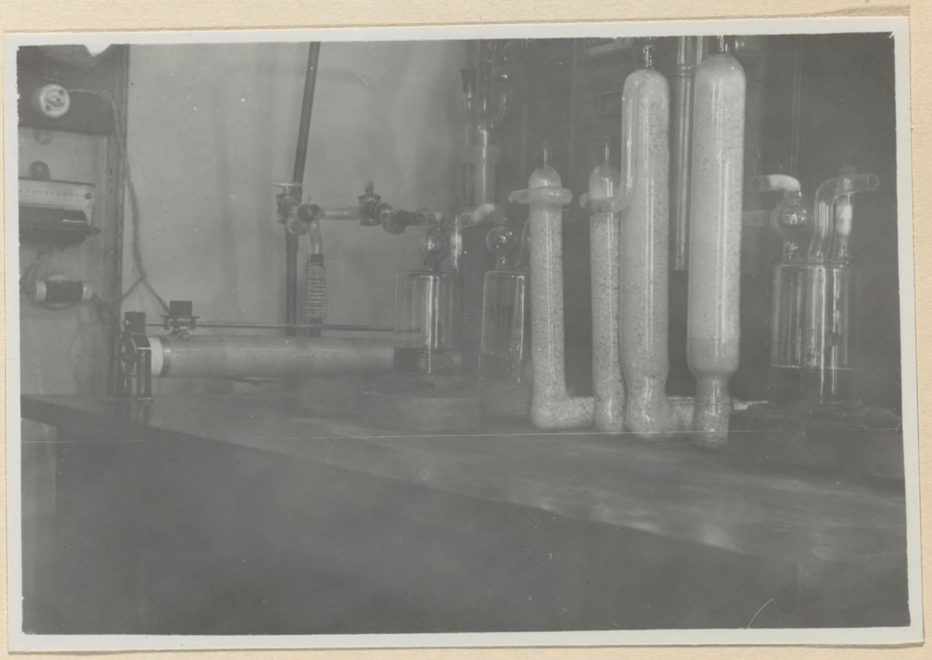


Figure 143. Budapest, Hungary. University of Budapest, Physiological-Chemical Institute. Soda-lime absorbing tubes of special construction so they can stand up on the table. Used by Professor Hári and Dr. Aszódi.

BUDAPEST, HUNGARY.

University of Budapest, First Medical Clinic.

Dr. Zoltán Ernst.

Ernst is a very serious worker but apparently not at all a self pusher, and his friends are much distressed by the fact that he has made no further progress in the University standing. For example, he has been seventeen years in chemistry but is not yet "private Dozent". His former chief, Balint, is dead and while Balint had great interest in Ernst and if he had lived undoubtedly Ernst would have been appointed "private Dozent", Balint's successor, Herzog, is interested in other things. He is a neurologist primarily, for example.

Ernst is much interested in the perfusion of the spleen and has demonstrated that bilirubin may be formed in the spleen and in the liver. His technique is perfect, his apparatus very clean and nicely set up and I could not help comparing Ernst's set-up for perfusion experiments with other set-ups I have seen in other laboratories and particularly the gruesome affair of poor Börnstein of Hamburg several years ago. Ernst showed us a very clever iodine reaction by demonstrating the presence of potassium sulpho-cyanate in the saliva.

One interesting talk with Ernst had to deal with endogenous obesity. His chief (I don't recall whether it is Balint formerly or Herzog at present) considered fat deposits like a tumor growth. You can have, therefore, a <u>tumor</u> of fat and lose weight in every way but this <u>fat deposit</u> is not disturbed, even if a person gets very thin. We must write him and ask for the elaboration of this idea. If so, then obese people <u>can</u> eat but little and not lose fat. It is to my mind one of the most interesting theories of obesity that I have heard.

As formerly, Ernst impressed me as being a serious-minded chap working a lone hand now, with apparently no special advice from his present chief. I was glad to have the privilege of talking with Herzog, at which time I emphasized strongly that my chief reason for coming to Budapest was not to see the laboratory, etc., but to see men like Hári, Aszódi, and Ernst, and I was told that that sort of talk could be a help to Ernst in the eyes of his chief.

BUDAPEST, HUNGARY.

University of Budapest, Department of Physiology.

Professor G. Farkas and Dr. H. Tangl.

Professor Farkas. This real genious had been also in poor health and of a neurasthenic disposition and he was quite occupied in his own subjective feelings. He is one of the most versatile men I think I have ever met, a very extraordinary linguist, with a great command of scientific literature, and has about him an enthusiastic group of young men.

The application of the Universal apparatus to the experiments with the dog showed again the apparent impossibility of people using a well worked out piece of apparatus and applying it to their own problems and at the same time giving too little attention to the various basic points necessary to have successful usage of such an apparatus. The whole set-up in Farkas' laboratory as applied to the dog seemed very definitely crude. With all the descriptions given of our apparatus in the various journals it is difficult to understand how they can make such a clumsy application of the apparatus to their specific problems. It is all the more surprising when you realize what a marvelous linguist Farkas is himself.

His interest in the metabolism of the Hungarian field worker continues unabated and undoubtedly he will carry it on with more care in subsequent years.

In Farkas' laboratory I saw a Benedict Universal table such as has been used by Atzler. The experiments were being made on a dog and the ventilation was simply tremendous. It was amusing to note that the empty Williams bottle which is put on as a trap to catch any acid to be backed up into it was put on "wrong end to" so it would not be any protection whatsoever.

One very ingenious device was that of a student of his, who had attached to the bathing cap on the Benedict and Benedict student apparatus an electric signal. This was so arranged with a lever in contact that when the bathing cap was in contact with the plate a light was lighted, but if the bathing cap extended beyond this point the light went out. I thought the idea was rather good but the operator might be confused by the light going out either because of lack of contact, that is, the cap not being high enough, or the cap being too high. I felt the bathing cap was very poorly adjusted with tremendous tension.

Dr. Tangl is as active as ever. He is still associated with Professor Farkas but working chiefly on biochemical work and I understand he has nothing whatsoever to do with the metabolism phases of Farkas' work. He is an extremely tense individual and apparently trying to follow out the brilliant example of his father.

## BUDAPEST, HUNGARY.

### Lectures.

I gave a number of lectures in Budapest, all extremely well received. The "third" lecture, given before the Medical Society on December 9th, was well attended, with an audience of about 215, and enthusiastically applauded. (There was an announcement of this lecture.) On the same day I gave the "first" lecture for Professor Farkas' students, and there was an attendance of 500. On December 10th I spoke before Farkas' associates, giving the "second" lecture, to an audience of about 80.

Newspaper clippings with regard to the lectures, from the "Pester Lloyd" of December 10th and the "Pesti Hirlap" of December 10th, are appended herewith. (See pages 257 and 258.)

During one of the lectures in Farkas' lecture hall a most amusing experience took place. When I began speaking I saw seated in front of me Hári and Farkas, and in the seat directly behind was seated Aszódi. I had hardly begun when I heard Farkas talking in loud whispers to Hári. At first it troubled me and then I said to myself, "Hári's eyes are very poor and Farkas is explaining to him the rather numerous slides." But a little later on I noticed he was carrying on the same buzzing sound with Aszódi and I knew <u>his</u> eyes were very good. As I recall, Farkas was talking the entire time of my lecture. Later on I said to Hári and Aszódi, "May I ask what was the chief topic of conversation between Farkas and yourselves? Was it some point that challenged his attention so much?" It then transpired that Farkas had invited Mrs. Benedict and me to lunch and during the entire hour he was very much disturbed as to <u>where</u> was the best place to take us. He was seeking this advice from Hári and when Hári clearly indicated that he wanted to hear my lecture he turned and continued the conversation with Aszódi. I have no idea that the man felt he was discourteous and probably he grasped more of what I was saying than did most of them. He is a man of keen, clear mind.

BUDAPESTI KIR. ORVOSEGYESÜLET, VIII. SZENTKIRÁLYI UTCA 21.

# MEGHIVÓ

az 1932. évi december hó 9-én, pénteken délután kivételesen 1/2 **7** órakor
az Egyesület Semmelweis-termében tartandó

## X. rendes tudományos ülésre.

NAPIREND:

*Francis G. Benedict* (a bostoni Nutrition Laboratory of the Carnegie Institution igazgatója):
**Az emberi alapanyagcsere az ujabb vizsgálatok megvilágitásában.**
Az előadás német nyelvü.

Budapest, 1932. december 2-án.

*Zalka Ödön*
*főtitkár.*

**PESTER LLOYD** Samstag, 10. Dezember 1932

**Stoffwechseländerungen im Alter und bei verschiedenen Rassen.** Professor Francis G. *Benedict*, Direktor des vom Carnegie-Institut erhaltenen Ernährungslaboratoriums in Boston U. S. A., der mit seiner Gemahlin und Mitarbeiterin aus Anlaß des Physiologenkongresses in Rom nach Europa gekommen war, besucht zurzeit die ungarischen wissenschaftlichen Anstalten und hat aus diesem Anlaß heute abend in der Budapester kön. Gesellschaft der Ärzte einen Vortrag über den *„Grundumsatz des Menschen im Lichte neuer Forschungen“* gehalten. Der dichtgefüllte Vortragssaal ehrte den hervorragenden Gelehrten und seine Gemahlin während der vom Präsidenten Professor Stefan v. *Tóth* gehaltenen Begrüßungsansprache mit stürmischem Applaus. In seinem deutschen Vortrag sprach der amerikanische Gelehrte über die hervorragende Bedeutung, die der Messung des Stoffwechsels für die Diagnose und die rationelle Behandlung von Krankheiten zukommt. Er stellte fest, daß im allgemeinen der erwachsene Mensch eine Kalorie pro Kilogramm Körpergewicht und Stunde produziert. Zur Messung des Stoffwechsels erwies sich die Menge des von der Lunge aufgenommenen Oxygens am entsprechendsten. Frauen produzieren im allgemeinen um 10 Prozent weniger Wärme als Männer. Die Wärmeproduktion und damit der Stoffwechsel nehmen mit vorrückendem Alter ab. Hochinteressant sind die Messungen, die ergaben, daß der Grundumsatz und damit natürlich auch der Stoffwechsel bei den verschiedenen Menschenrassen erhebliche Unterschiede aufweisen. So ergaben Messungen an Tamilen in Madras einen Grundumsatz von — 16 Prozent, an australischen Eingeborenen von — 14 Prozent, an australischen Eingeborenenfrauen — 16 Prozent, an Maya-Indianern in Yukatan + 8.4 Prozent. Daß hiebei nicht Ernährungs- oder klimatische Unterschiede maßgebend sind, beweisen die an in Boston geborenen und dort lebenden chinesischen Mädchen durchgeführte Versuche, die um 9 Prozent niedrigere Werte als bei amerikanischen Mädchen zeigten. Weitere Versuche stellten den Einfluß von Schlaf (— 10 Prozent) geistiger und körperlicher Arbeit fest. Schließlich führte der Vortragende die bei den Versuchen verwendeten Apparate vor. Die gedanken- und inhaltsreichen Ausführungen ernteten stürmischen Beifall.

Dr. B. A.

Budapest

Budapest

1932. december 10., szombat. PESTI HIRLAP



— **Egy amerikai tudós előadása Budapesten.** Érdekes ülése volt pénteken este a Budapesti Kir. Orvosegyesületnek. *Francis G. Benedict*, a bostoni Carnegie-alapitványos élettani intézet igazgatója beszámolt nagyhirü vizsgálatainak és kisérleteinek egyes eredményeiről. Az elnöklő *Tóth István* tanár meleg szavakkal üdvözölte a munkatárs-feleségével megjelent világhirü tudóst, aki német nyelven tartott előadást az emberi alapanyagcsere ujabb vizsgálatairól. Alapanyagcsere alatt — fejtegette — a testben végbemenő égési folyamatoknak azt a minimumát értjük, amelyet teljes nyugalomban is éhgyomorra és minden izommunka lehető kikapcsolásával, igénybe vesz az ember, amit tehát a létfőntartás legkisebb mértékének kell tekinteni. A Carnegie-alapitvány bőkezüsége rendkivül nagyarányu kisérleteket engedett meg, külön sisakszerü készüléket is szerkesztett, amely szellemes módon könnyiti az elhasznált oxigén és a képződött szénsavmennyiségeknek meghatározását. Kiderült, hogy ez a szám meglehetősen állandó, szellemi s testi megerőltetésnél nő, a haladó korral csökken. Álomban is kevesebb, átlag tiz százalékkal, hipnózis alatt azonban alig valamivel, ugyhogy ezt a két állapotot nem lehet élettanilag azonosnak tartani. Benszülött törzseknél, indusoknál — még a Bostonban szülötteknél is — alacsonyabb értékeket talált, amit eleinte arra akart visszavezetni, hogy ezek jobban tudják izomzatukat elernyeszteni. Minthogy azonban ezeknél is álom közben tiz százalék csökkenés állt be, ezt faji jellegzetességnek kell tartani. Külön figyelmet forditott még a kilehelt levegő hőmérsékének vizsgálatára, amiből több érdekes következtetés vonható. Az erre vonatkozó állatkisérletek még folyamatban vannak. A nagyszámu hallgatóság elismerő tapsa hálálta meg az érdekes fejtegetéseket.

— **Liptay Béla meghalt.** Liptay Béla felsőházi tag.

## BUDAPEST, HUNGARY.

### Considerations regarding the Budapest situation.

The Budapest group are as a whole very well worth visiting. To be sure the chief spirit, Hári, is gone, but the ideas of the Tangl-Hári school will still keep on. Just what political changes will involve and how much of a readjustment of the laboratory work will take place, whether there will be a coalition of the department of physiology and the department of biochemistry or whether possibly, as Aszódi wrote me he hopes will be the case, the institute will be maintained primarily as a research rather than a teaching institute, no one can tell. In any event a man of Aszódi's make-up certainly should be permitted expression of his scientific drive. It will be a great pity if he is not allowed to continue his work so well established. He has ideas and is extremely accurate and precise, which in biochemical research is often lacking.

BELGRADE, YUGOSLAVIA.

University of Belgrade, Department of Physiology.

Professor J. Giaja and Dr. S. Gelineo.

In Belgrade there are two physiological laboratories, one at the Faculty of Sciences of the University, under the direction of Professor Giaja, and one at the Medical School, Professor R. Burian. I have been particularly interested in the work of the laboratory of Giaja for, although it is very poorly equipped, shockingly poorly equipped, Giaja's enthusiasm and ingenuity are always well worth studying. (See figures 144, 145, and 146.)

I had read, of course, all of Giaja's articles that were published in French but many of the most important things have to be, by law, published in the perfectly unintelligible Serb. Thus, for example, I found a very large monograph on the metabolism and thermic reaction of rats to lower temperatures with an extensive discussion of the effect of acclimatization, which had been written and was about to be published in Serb, by a clever assistant, Gelineo. This dealt with rats studied all the way from 30° down to 0° C. I was interested to note, however, that with this complete range they had found only a 40 per cent rise in the metabolism instead of the 5 per cent per degree that had been generally found. Gelineo's special interest has been in the adaptation of the animal to environmental temperatures in the nature of acclimatization, in other words, keeping them at these different temperatures for some time and noting the reactions. Unfortunately the material is all prepared to be published in Serb. I emphasized to him very strongly the importance of having this published in some international language like French, German, or English, or at least some language that could be read by other people, and this brought about a rather interesting discussion. (See page 269.)

Giaja's whole thesis deals with the question of basal metabolism and what he calls "métabolisme du sommet", which means the metabolism when there is a pronounced cooling effect. He has laid great stress upon the fact that there seems to be a relationship between these two. He has a factor which is, as I recall it, 3.5 times greater than the basal.

Gelineo. Gelineo has a tremendous piece of work on the "métabolisme du sommet". He finds that rats adjust themselves to different temperatures. Thus, when they live at 0° and are measured at 30° C. the metabolism is high. If they live at 30° and are measured at 30° the metabolism is low. It does not drop immediately but stays up for some time, so there is a period of acclimatization. Later on I found out that his rats at 0° did

Figure 144. Belgrade, Yugoslavia. University of Belgrade, Department of Physiology. Professor J. Giaja in his laboratory.

Figure 145. Belgrade, Yugoslavia. University of Belgrade, Department of Physiology. Professor J. Giaja in his laboratory.

Figure 146. Belgrade, Yugoslavia. University of Belgrade, Department of Physiology. Professor J. Giaja in his laboratory.

not have a temperature coefficient less than 5 per cent per degree as they first told me. Now for example, cold rats, that is, rats living at 0°, when measured at 31° C. had 900 calories per square meter; when measured at -10° C.they had 2900 calories per square meter; that is, for a difference of 41 degrees in temperature they had an increase of 2000 calories or 50 calories per degree, and 50 calories referred to 900 calories would be 5.5 per cent. Rats living at 16° or 17° C. when measured at 32° C. had a basal metabolism of 670 calories per square meter. This same type of rat, that is, the animal living at 16° or 17°, when measured at 2.5° C. had 1800 calories. Thus, for a temperature difference of 29.5 degrees they had a rise of 1130 calories or approximately 35 calories per degree. 35 calories referred to 670, the baseline at 32° C., gives about 5 per cent.

Gelineo uses different sizes of chambers but I think they were all too large for a rat. (See figures 147 and 148.) He admitted that the rat remains most quiet in small chambers. He emphasized a good deal the fact that his animals would keep quiet even in an ice bath, but when he showed me one in an ice bath the animal was not quiet according to Nutrition Laboratory standards. As a matter of fact the animal had not fasted, so there were feces and urine in the chamber which contributed to his discomfort. I think the chamber should have a false bottom to keep the feces and urine clear.

<u>Rectal temperature of rats</u>. The rectal temperature of the rat is taken by holding the animal by the tail and giving it a little jerk or pull, and then inserting the thermometer. (See figure 149.)

One of the assistants, Mr. Markovitsch, was making a most interesting study on the heat loss from two different flasks filled with water with exactly the same area but covered with fur, studying the differences when the fur is turned inside versus fur turned outside. They found that when the fur is turned outside there is better protection than when it is turned inside.

<u>Fever</u>. Another assistant was working on a study of fever and fever-producing situations, studying different kinds of fever and metabolism with special reference to the thermo-regulation; that is, they had a chemical fever produced by beta-dinitrophenol and by tetra-hydro-naphthylamine and also with adrenaline. Under these conditions basal metabolism is elevated and thermo-regulation is very small or zero; that is, the caloric loss is increased. They are interested in trying to classify the different fever-producing agents.

They found that the temperature of the exterior has a great influence on the fever process. Some things produce fever at low temperatures but not at high temperatures, etc. Giaja finds with a low rectal or cell temperature an increased metabolism, which would

Figure 147. Belgrade, Yugoslavia. University of Belgrade, Department of Physiology. Laboratory of Professor Giaja. Corner of laboratory showing against the back wall two respiration chambers in which the "métabolisme du sommet" is studied.

Figure 148. Belgrade, Yugoslavia. University of Belgrade, Department of Physiology. Laboratory of Professor Giaja. This view shows Dr. Gelineo, associate of Professor Giaja. In the rear, respiration apparatus covered with ice for measuring "métabolisme du sommet".

Figure 149. Belgrade, Yugoslavia. University of Belgrade, Department of Physiology. Laboratory of Professor Giaja. Dr. Gelineo taking rectal temperature of rat. Rat is standing on piece of paper, tail is held in the hand, and the rat tries to pull away from the tail.

seem to be contrary to the law of van't Hoff. Lowering the temperature increased the heat loss but raising the temperature decreased the heat loss. If this is so it is a tremendously important thing. It would appear therefore as if it had bearing upon the source of fever and characteristics of fever production, since it would appear to depend on the exterior temperature. I have a special note in which I state that I must change my mind that heat loss is of little or no importance. If Giaja is correct it is of very great importance and we must study his papers closely. Throughout my entire trip I had been emphasizing more or less that stress should be placed more upon heat production than upon heat loss.

Growth. I recall very well Giaja's telling me that he was interested in "métabolisme du sommet" as a result of a practical problem in Yugoslavia where they have very cold winters and the animals are exposed. In the discussion I had with Giaja, he argued that plants are cold; they grow slowly but have a low metabolism. On the other hand, animals when in the cold have a high metabolism. Do they grow faster? He was thinking for example of his two rat curves. He said he is always thinking of things as they exist in nature. He is an individual full of speculative philosophy. Giaja is interested in the heat production, etc., from the chemical standpoint and not from the morphological.

In the laboratory he has a most interesting method of teaching. He is trying to keep his students, even elementary students, in touch with the researches in the laboratory. In other words, he shows the student elbow to elbow each advancement and thinks of teaching with books and theory as a cold and dead thing. His students can not use the stimulus from him for they go into high schools, teaching, and not into research fields. However, he must be a very stimulating teacher and his interesting viewpoint I think is well worth copying.

Lecture. I gave the "first" lecture in Belgrade on December 13, to an audience of about 125. A newspaper clipping regarding my visit to Belgrade is appended herewith. (See page 267.)

At a banquet which was tendered to Mrs. Benedict and myself, while we were in Belgrade, the Rector of the University of Belgrade, Professor Vlad. K. Petković, made a speech, a copy of which is appended herewith. (See page 268.)

КА 12 децембар

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# Убиства у Софији не престају

## ПРЕКСИНОЋ ЈЕ УБИЈЕН ПРОТОГЕРОВИСТА КИРОВ-ЦАНДИЛОВ

Цариброд, 11 децембра

Непрекидна крвопролића македонствујушчих нижу се без краја. У немогућности да сврше са вођама протогеровиста Крстаном Поп-Тодоровом, Пецом Трајковим и Христом Лиловим, које узалудно траже по свима софиским квартовима, михаиловисти без милости истребљују њихове и најбезначај-

Радничка партија не потпада под одредбу члана 20 закона о заштити државе, а да л је она легална или није, о томе суд није надлежан да расправља.

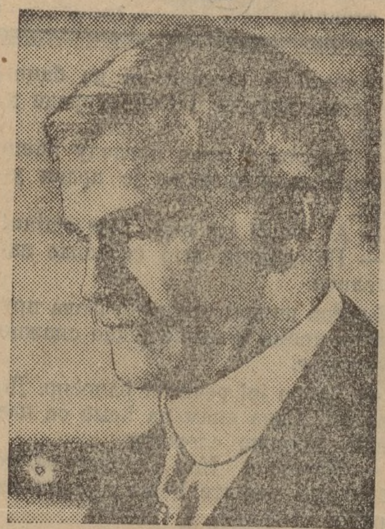
Решење суда изазвало је велико изненађење, јер се држало да ће мандати комуниста бити поништени.

Министар унутрашњих дела г. Гиргинов поводом одлуке суда изјавио је:

**— Ситуацијом коју је створила**

# Славни амерички научник г. Бенедикт у Београду

Belgrade



**Г. Франсис Бенедикт**

Данас стиже у Београд г. Ф. Бенедикт, чувени амерички професор, познат по својим важним радовима о метаболизму човека. Он ће сутра, 13 овог месеца, у 18 часова у амфитеатру Физиолошко-хистолошког института, Зрињскога број 92, одржати предавање о физиологији великих тропских змија и корњача у вези са људском физиологијом. Предавање ће држати на немачком језику.

Г. професор Бенедикт биће гост нашег Универзитета.

Поштарина плаћена у готову

# ПОЛИТИКА

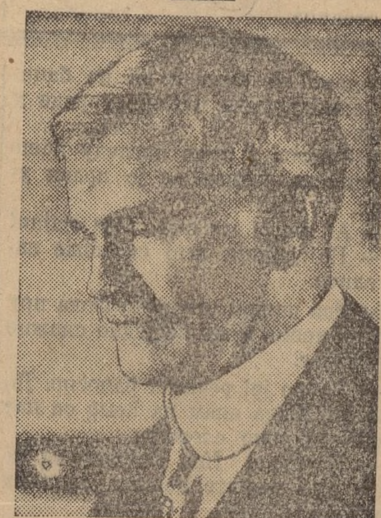
Чековни рачун број 53.002

ВЛАДИСЛАВ РИБНИКАР, ОСНИВАЧ
рез. капетан, погинуо 1 септембра 1914 год.
ДАРКО РИБНИКАР УРЕДНИК
рез. капетан, погинуо 31 августа 1914 год.
Д-р СЛОБОДАН РИБНИКАР, ДИРЕКТОР
рез. потпуковник, умро 24 септембра 1924 год.
Директор: ВЛАДИСЛАВ СЛ. РИБНИКАР
Уредници:
МИОМИР МИЛЕНОВИЋ и ЈОВАН ТАНОВИЋ

ПРИМЕРАК 1 ДИН. | ТЕЛЕФОНИ Администрација 25-007 Уредништво: 25-010 и 25-005 | Београд, понедељак 12 децембар 1932 Број 8833 — Година XXIX | ПРЕТПЛАТА ЗА ЈЕДАН МЕСЕЦ За нашу земљу 20. За стране земље 48 дин. РУКОПИСИ СЕ НЕ ВРАЋАЈУ | ПОЛИТИКА Поенкареова 31

Belgrade

## Славни амерички научник г. Бенедикт у Београду



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Г. професор Бенедикт биће гост нашег Универзитета.

Speech of Rector Univ of Belgrad at Banquet Dec Belgrad

Monsieur et très honoré collègue

C'est avec un réel plaisir que je vous salue au nom de notre Université, ainsi que Madame Benedict, qui associée à vos remarquables travaux partage avec vous la fatigue de vos voyages scientifiques. Il y a trois ans vous nous avez honoré de votre première visite. Votre visite actuelle signifie que vous avez trouvé de l'intérêt à échanger des vues avec vos collègues de l'Université de Belgrade, qui, j'en suis persuadé, profiteront largement de votre expérience et de votre autorité. Ces rapports intellectuels entre savants de différents pays sont vraiment ce qu'il y a de plus élevé et qui contribue le plus à la connaissance et au rapprochement des peuples. Votre grand pays nous donne à ce sujet un exemple que nous ferions bien de suivre, nous en Europe, en facilitant à nos savants des voyages d'étude, et de visites à leurs collègues de même spécialité dans les autres pays.

Au nom de l'Université de Belgrade, je vous remercie de votre visite, je lève mon verre à votre santé, Madame, et à la votre, cher collègue, ~~et~~ à la solidarité scientifique des peuples et à la prospérité de votre grande Patrie.

Speech of Professor Vlad. K. Petković, Rector of the University of Belgrade, at a banquet tendered to Dr. and Mrs. F. G. Benedict, December 14, 1932. These are the original notes of the Rector.

PUBLICATIONS IN YUGOSLAVIA.

I pointed out to Giaja that it does not help Yugoslavia, in other words, it is not for the "glory of Yugoslavia", to publish in Serb, for the number of people in Yugoslavia who read it are very few, extraordinarily few in this small country. It does not help other scientists for they can not read the Serb. Therefore, the nationalistic ideas are actually restricting the spread of important and expensively accumulated scientific facts. I thought they should publish in two forms, perhaps complete in Serb, which is of course obligatory since the researches are subsidized and expenses paid by the Government. They should also publish in French, but this of course increases the cost very much. That is the present ideal and what they would like to do.

The problem is very acute with the Serbian Academy of Sciences. It might publish now in Serb with the hope of publishing later in some other language. I suggested to him very strongly that they should publish in detail these researches in French, German, or English, and a very long summary in Serb, not merely a page or two but several pages. Of course the tables of Arabic figures will be internationally understood and the gist and essentials of the material could be given in a Serbian abstract. Giaja was on some committee of the Serbian Academy to take up this matter in detail in hopes of working out some system whereby a wider distribution of their publications and findings can be given and still retain the interest for Serbia.

<u>Scientific and psychological attitude of scientists in Budapest and Belgrade.</u>

Under the caption "optimism versus pessimism" I found in my notebook a number of comments stimulated by my contact with scientists in these two countries. Anyone can be a pessimist but a man who is optimistic is a little bit better and society as a whole benefits by his being optimistic. On the other hand, a man must not be like the ostrich sticking his head in the sand and not recognizing difficulties, but he should attack these difficulties with a belief in the possibilities for success.

Those men in Europe who spend their time complaining of conditions could, if they spent the same number of hours in productive work that they spend in complaining, get a great many things done. I was very much impressed by the attitude of Hári and Aszódi in Budapest working under very great disadvantages and the same way with Giaja in Belgrade. Think, for example, of a physiological laboratory in a university, the largest university in the country, without a galvanometer of any kind in its equipment.

Later in Vienna I heard the lecture of Saint Georgy of Zagreb. It was a splendid instance of a man driving around, as he stated it, in a sort of gypsy life, working in different laboratories, for example in Hamburger's laboratory and in London and finally coming back to his own country, where I believe he is in Zagreb (?), where he makes use of a great native product, paprika, as a source of vitamin C, from which he obtained a crystalline product which was only 1/1000 as potent as other vitamins. He argued that although it was crystal in form it may still be dimorphous, so he made an acetone derivative and crystallized it. This then was broken up by heating with water and he obtained a far more powerful product. Saint Georgy likened hypothesis to the scaffold of a building, to be removed when the building is completed. He stated that probably all his theories may be wrong but at least out of this he has left a crystalline product that is very potent, which can now be made by the kilogram.

This was a most stimulating lecture and showed the research drive in a small Hungarian college. Similarly Giaja is making bricks without straw, using his meager equipment, spending his time in productive work, and not complaining.

The attitude of scientists in Belgrade is discussed in a statement I made to the editor of the South Slav Herald, and which was published in that paper on January 16, 1933. A copy of the newspaper article is appended herewith. (See page 271.)

SOUTH SLAV HERALD, MONDAY JANUARY 16th, 1933

# BELGRADE'S KEEN SCIENTISTS

## Fine Results Despite Equipment Difficulties

By Francis G. Benedict

One of the recognised obligations of every educational institution, large or small, is that of not simply pouring out of the pitcher of knowledge, but making some substantial contributions thereto.

This is especially the case in the larger universities and technical schools and a visitor to Belgrade is obviously interested to see how this relatively new institution is living up to this obligation. Some people visit cities to see great buildings, works of art, bridges, transportation lines, etc., but the scientist is usually attracted by his fellow-workers. This was particularly the case in my two visits to Belgrade, for, without the presence of Professors Giaja of the Department of Physiology, Burian of the Medical Faculty, and Chahovitch in the Department of Pathological Anatomy, I would have hardly felt justified in taking the long trip to Belgrade. On both occasions I was more than impressed with what I found in the line of research. Men working with often altogether too inadequate facilities, especially with regard to equipment and assistance, are carrying out splendid research.

In perhaps no field of human endeavour is there any more complete absence of a strict nationalism than in the biological sciences, and we find the University of Belgrade actually contributing more to the world at large than perhaps to the restricted area of Yugoslavia. The number of readers of the various publications in Yugoslavia is relatively limited. Whenever the communications of the various Yugoslavian laboratories are published in either French, German or English, and this is unfortunately too seldom the case, the number of readers, and consequently those who profit by these splendid researches, becomes very great.

### FROG PHYSIOLOGY

On my recent visit I was made acquainted with three masterful pieces of research, one the splendid "Atlas" of Professor Chahovitch, second the extensive monograph of Dr. Melas, on the physiology of the frog, unfortunately not readable by most European readers, and third the splendid thesis, as yet only in manuscript form, of Dr Gelineo on the physiology and heat regulation of the rat. These last two monographs are of invaluable help in interpreting researches from the laboratory of the Carnegie Institution of Washington.

Passing through almost the entire Europe, one is impressed greatly by the fact that, despite the generally prevalent economic difficulties, it is precisely in those centres where least complaints of these difficulties are heard that one notes the greatest research activity. It is greatly to the credit of the Belgrade *savants* that they have profitably used time that is so often wasted by others, in complaints, in intensive and profitable scientific research. It is, of course, discouraging to the enthusiastic scientist to try to make scientific bricks without straw, and, for example to find a physiological laboratory that does not possess a galvanometer is probably unique among European institutions. Professor Giaja has, however, with characteristic force, selected important problems that for the most part can be carried out without this partucular instrument, and has wasted no time before accummulating a large amount of data.

### BELGRADE TRIBUTE

One of the most popular themes of discussion among scientific circles is the so-called tremendous material facilities possessed by American institutions. *Without going into this very debatable question, it can only be said that the Belgrade scientists are making progress and contributions that are challenging the attention of the scientific world.*

This should not be used as an argument against furnishing these conscientious workers with more adequate equipment, but the American visiting the University of Belgrade can only go away stimulated and inspired by the intensity of effort and successful accomplishment of men working with extraordinarily meagre equipment.

The lack of equipment does not seriously hamper the research worker with the research drive, and the sympathetic accord existing throughout the Faculty and under the wise leadership of the Rector, Professor Petkovic, explains fully the wholly unusual successes in the field of research.

Carnegie Institution of Washington
Nutrition Laboratory
Vila Street, Boston, Massachusetts
Francis G. Benedict,
Director.

# "SUN NEVER SETS..."

## On "Herald" Readers, Reveals Postbag

New Year messages and greetings continue to reach the "South Slav Herald" from readers both in Yugoslavia and abroad. A further selection of some of these letters to the Editor are published below, and provide evidence of the steadily-growing circle of "Herald" readers on whom — as was said of the British Empire — "the sun never sets". The "Herald" numbers its subscribers in Australasia and South Africa, South America and the U.S.A.

From Francis G. Benedict,

"Shortly before leaving Belgrade I purchased your Thursday, December 1st, issue of the *South Slav Herald*. I think I can safely say that I have read every word of it and thereby obtained my first clear picture of many Yugoslavian problems. May I congratulate you on the excellent make-up of your paper and particularly your article entitled "The 1st of December 1918". I found the paper so interesting that I am sending it to a friend in America."

"Congratulating you on the obvious strong service you are rendering to Anglo-American-Yugoslavian relations, I am.

## VIENNA, AUSTRIA.

### Professor Arnold Durig, Professor Hans Horst Meyer, Professor E. P. Pick, and Professor Wilhelm Falta.

The prime object of visiting Vienna was to see my good friend, Durig, and considerable time was spent with him although practically no researches in gaseous metabolism were going on in his laboratory. The semester had just closed so no lectures were arranged for, although I had the privilege of hearing the most interesting lecture by Saint Georgy, on which I have commented in the preceding pages.

We also had most interesting and social relations with Professor Hans Horst Meyer, Professor Pick, and Professor Falta. Apparently research as a whole in Vienna is very low, particularly in metabolism, and I should state that the Pirquet Clinic organization was completely demolished. There was little if anything of specific interest in Vienna other than the ever stimulating contact with my good friend, Durig.

## BERNE, SWITZERLAND.

### University of Berne, Physiological Institute.

### Professor Leon Asher and Professor I. Abelin.

The physiological laboratory at Berne under the leadership of Professor Asher has been always interested in the endocrines. Asher is one of the most enthusiastic of the endocrinologists. For example, Allan W. Rowe made him American representative of a Congress soon to be held.

Asher is an extremely dynamic individual, tremendously enthusiastic, full of anecdotes, and a friend of nearly everyone. To be sure, they laugh at his foibles. He is an unusual personality, full of humor, lots of experiences, and a good thinker. One can not help liking him. There was one discordant note which I think he himself regretted extremely. At a luncheon at Abelin's house there was an extremely disagreeable conversation or rather monologue, bringing out in a very acrimonious way the competition and jealousies between the Swiss universities. On this occasion Asher was bitterly unkind and I think horribly unjust to a colleague in another university in discussing him, his appointment, and his work. It was most disheartening and I was very sorry to have my good friend Asher explode in such a manner and talk about this matter as he did. Although he prefaced his statements by saying that it gave him a great deal of pain to make these remarks, I felt they never should have been made. I have reason to believe that he himself was rather ashamed of the outburst.

The equipment of this laboratory is excellent, with more new apparatus. His experimental work was much too elaborate for me, and many of the experiments demanded a tremendous amount of expensive apparatus. For example, they had a large room sheathed with polished copper to shield the apparatus. This was used for electrical measurements, and was given by the Rockefeller Foundation. They called it "The Rockefeller copper palace". There is a similar room in Lapicque's laboratory in Paris. Following Erlanger and Gasser's lead Asher was working with a very large cathode ray oscillograph and the entire apparatus was thoroughly grounded. He is doing very little in respiratory metabolism now and no Japanese are visible in the laboratory, a striking contrast to earlier years. Asher's exuberance and enthusiasm were evident as usual, but I had the distinct impression that he had a rather loose contact with the various researches and that they were not particularly well directed.

The respiration apparatus seemed to me very poor. He still uses the Haldane weighing method in which there seemed to be too little acid for absorption of water and enormous Wolff bottles, indeed two, for the carbon dioxide, which seemed entirely out of proportion. Some of his problems were making determinations on rats and I noticed that none of the measurements were made during the special hours that Dr. Horst and I recommended, that is, between 10 a.m. and 2 p.m. The rats were in a darkened room or at least a box darkened. The animal was placed in a small glass tube, which is a good idea, but it appeared to me that the air was blown against the rat's fur, that is, from the tail to the head, and I was surprised that he was not restless. The temperature of the air in the box was used and not the temperature of the air leaving the apparatus or the temperature of the air in which the rat was placed. Of course Asher will probably take the stand that he is after comparative values and hence as long as the conditions are maintained constant he is not interested in absolute values. The temperature of the chamber was 20° C.; that is, undoubtedly inside the glass tube the temperature was somewhat higher, but the tube containing the rat was inside a wooden box which had a temperature of 20° C. I was interested to find out that Asher's associate, Abelin, says that 22° to 24° is used in this box, but Abelin is not particularly enthusiastic over the method. Inside the wooden box containing the glass tubes was an electric lamp to keep the temperature up. I think probably it is a good idea to have the light inside the chamber, for this tends to keep the rats quiet. Probably it would be better to have an excess of light on the tube and have a glass-walled box.

Three rats in tubes with their three respiration apparatus were working at once. There was an assistant of de Quervain working in Asher's laboratory with rats, where the experiments were two hours or longer with no registration of activity and one can not see the rat at all. For ventilation they use a glass water suction pump with a special check valve made in Berne to prevent the water getting back into the bottles. They did not know, nor did they have the slightest idea, what the ventilation rate was. It seemed to me there were a great many weighings to be made for a single rat.

Among other apparatus Asher had bought a spectrograph, a very expensive instrument, for spectro analysis. They were photographing the work of a frog's heart, using a combination of mirror membranes of Frank so as to see if the hormone injected increased the work of the heart. I asked him if he expected to try out eutonon but he said, no, that he is not a pharmacologist. Asher was rather doubtful about eutonon. He thought Zeulzer was a very fine fellow but had gone too far with his hormone stuff. Asher succeeded in isolating a substance with repeated fractions for promoting growth of rats and had got it twenty times more potent. I suggested the idea I have so often raised, that the rats grow because they eat more and hence the hormone or whatever vitamin is being studied acts more in the nature of an appetizer or a cocktail. But singularly enough Asher thought that the controlled and the injected rats ate the same quantities. It was astonishing to see how shady these ideas are. Asher said that the injected rats had the same metabolism as normals, etc., and the question arose: Where does its extra growth and weight come from if the injected rats eat the same and have the same basal? Of course the injected rats probably are less active but as yet Asher has no idea as to their relative activity. I think their whole method of handling the experiments is quite other than we would do it.

One study, in which they were using thermo-couples for the temperature of the rabbit's neck, studying the temperature near the thyroid, interested me greatly. After denervating they cool the animal and get the temperature difference on the two sides, or determine whether one side cools off more than the other. This was rather too deep for me, but the animal was wonderfully quiet. I noticed another rabbit with a catheter in place, with which they were studying the indigotin, carmine output in the urine. The animal was strapped down but Asher says it does not attempt to struggle. It had been used many, many times. One remark that Asher made impressed me greatly. He stated that he believes rabbits in general are too stupid to fuss and that dogs are impossible for many delicate physiological tests, as they are too stimulated and always under tension.

Asher was much disturbed over a recent American paper on creatinin, published in the American Journal of Physiology. He could not understand what the man wrote and I am sure I could not, and Gordon Douglas, who was at the dinner at Asher's house that evening, was also nonplussed. In one breath the writers stated that the creatinin is a linear function and in the next they stated that it is independent of the other comparative factors. It is a good case of very bad writing. When will the writing students realize the value of learning English?

Abelin I had the pleasure of meeting in Stockholm, where he and his wife attended my lectures. He doesn't impress me as being well. I feel that he is a very serious worker and it is almost a pity that he must stay at Berne, where he is so dominated and overshadowed by Asher. I think he feels this strongly himself. Apparently there are the pleasantest social relations between the two families but Abelin is evidently dominated by Asher or he has found it necessary to become a "yes-yes man". My returns, however, were more profitable from my discussions with him than from what I had with Asher.

Abelin very kindly drew up for me what he considered his main problem with his sub-problems and this is included herewith. (See pp. 277-278) He is very much occupied in studying the influence of the thyroid upon metabolism, but is puzzled by the fact that while he can get an increase of 100 per cent in metabolism by feeding thyroid, he can produce an increase of only 30 per cent by changing the food. Abelin is much impressed by the respiration rate as a good indication of the basal metabolism and he finds that 4 heart beats for each respiration is a good relationship.

Abelin
For Foreign Report

Hauptproblem: Bekämpfung der experimentellen Hyperthyreose.

Mittel dazu:

1). Ernährungsart.

Durch Zufuhr von passendem Eiweiss(Casein),von passendemF Fett und Lipoid(Gehirn,Knochenmark,Eigelb etc.),durch reichliche Beigabe von Vitaminen kann die Schilddrüsenwirkung sehr stark abgeschwächt werden.Man sieht eine Erniedrigung des Grundumsatzes und eine Abschwächung der Vergiftungssymptome.

2) Dijodtyrosin und Dibromtyrosin.

Selbst bei üblicher Ernährung kann die Schilddrüsenwirkung durch Zufuhr von Dijodtyrosin oder von Dijod- und Dibromtyrosin abgeschwächt werden.

Zweites Problem: Einfluss der Ernährungsart auf den normalen Grundumsatz.

Bei Verfütterung einer hochwertigen Nahrung an Ratten sinkt der normale Grundumsatz im Durchschnitt um 10-12%.An einzelnen Tagen und bei einzelnen Tieren kann es zu einer Grundumsatzerniedrigung von 20% und etwas darüber kommen.

Darf in diesen Fällen an eine Abschwächung der normalen Funktion des Schilddrüsenhormons gedacht werden?

Durch welche andere Annahme könnte die Grundumsatzerniedrigung bei der beschriebenen Diät erklärt werden?

Perspiration insensibilis und Hyperthyreose.

Hyperthyreoidisierte Tiere haben eine erhöhte perspiratio insensibilis( bis zu + 70-90% und darüber). Den bisherigen Versuchen nach lässt sich aber dieselbe schwerer bekämpfen als die Grundumsatzerhöhung.

Welche Beziehungen bestehen zwischen der perspiratio insensibilis und der Grundumsatzerhöhung?

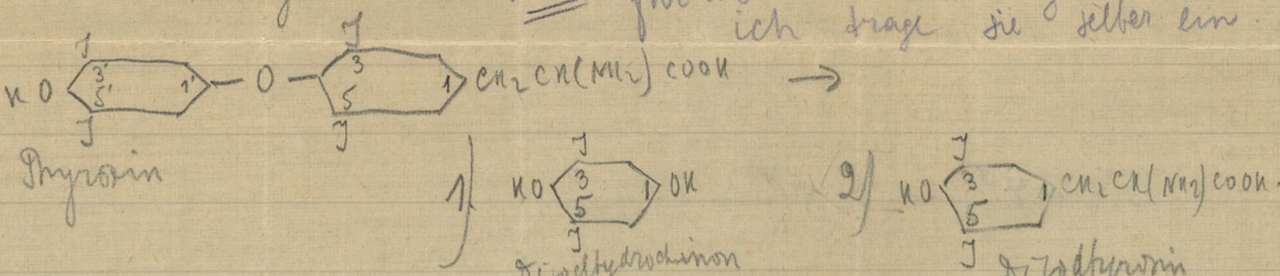
Bestehen Anzeichen für ein Parallelgehen vom Grundumsatz und perspiratio insensibilis?

abelin

Dijodtyrosin vertreten ist, so sind

die nahen chemischen Beziehungen zwischen diesen beiden Verbindungen klargelegt. Das Dijodtyrosin erscheint chemisch tatsächlich als eine gewisse Vorstufe des Thyroxins, allerdings als eine Vorstufe, von der ein noch ziemlich weiter Weg zum fertigen Thyroxin führt, denn die Bereitung des zweiten Thyroxinanteils, des Dijodhydrochinons und die Verknüpfung desselben mit dem Dijodtyrosin ist keine ganz einfache und scheinbar eine spezifische Leistung nur der Schilddrüsenzelle. NB! für die Formeln Platz lassen, ich trage sie selber ein

freier Platz!




neue Zeile

Kann das Dijodtyrosin auch in physiologischer Hinsicht als eine Vorstufe des Thyroxins betrachtet werden? Mit dieser Frage beschäftigen sich die nachfolgenden Versuche. Bei deren näherer Betrachtung wird man erkennen, dass der Begriff „Vorstufe" hier nicht ganz angebracht ist. Von „einer Vorstufe, etwa der eines Ferments, setzt man voraus, dass sie an sich unwirksam ist und dass sie durch eine relativ geringe Veränderung in die

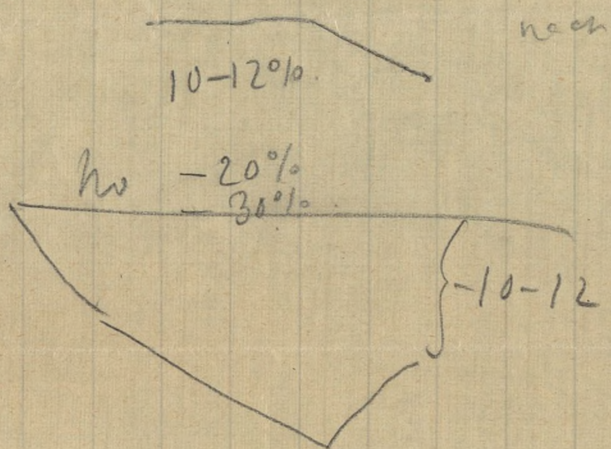
1/ Grundumsatz
bei normaler Ernährung
(Milch, Brot, Vegetabilien)

2/ Grundumsatz
bei der Diät
(Casein + Gehirn, Eigelb, Knochen-
mark, reichlich Vitamine,
etwas Lebertran)

Hafer
Gersten
Reismehl

Alkalin [illegible]

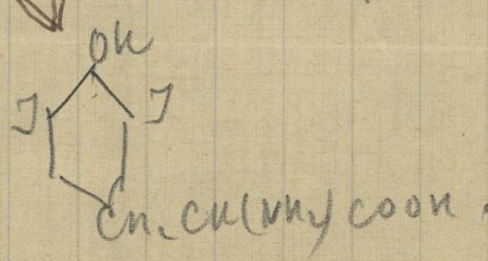
nach 2-3 Tagen




1) Wärmeabgabe durch die
Haut bei der Hyperthyreose

2) negative Phase

Dijodtyrosin
Thyroxin




24° — 28°

950
200

In the thyroid 50 per cent of the iodine is derived from thyroxine and 50 per cent from Di-Jod-Tyrosin. This is based upon the work of Harrington, an English chemist. Abelin, although a very serious worker, has a "hide-bound" technique, using the Haldane weighing method. He lays great stress upon diet and thinks that a great deal can be done in every phase of life with simply diet changes. Thus, by changing the diet he can lessen the action of thyroxine. Hence he thinks it can lessen cancer activity. I have rarely found a man who is so optimistic with regard to the possibilities, therapeutic and otherwise, of diet as Abelin. He was much interested in knowing whether the Journal of the American Dietetic Association is a popular or scientific journal. (Wrote to T.M.C. on July 3, 1933, concerning this.)

## BERNE, SWITZERLAND.

### University of Berne, Veterinary Department.

### Professor B. Huguenin.

Mrs. Benedict's cousin, Dr. Leon Boissonnas in Geneva, with whom I was discussing the question of length of life and metabolism, referred me to a classmate of his, Dr. Huguenin in Berne, who had published and had worked upon this problem considerably. I found him a most interesting man and enjoyed my talk with him very much.

Commenting upon the question of physiology and old age Dr. Huguenin stated that there was very little known. He said there had been a study of the teeth of dogs that split and are lost out, as with man, and the following senile changes in the jaws as with man, but in the case of swine and most other animals nothing is known. He believes in the general rule that the animal lives five times the length of time required for the disappearance of the epiphysen line, that is, they should live four, five, or six times. There is no carcinoma in swine. Huguenin has found only by accident one case. He had a chart, unpublished, showing that the age limit is about five times the zone when the epiphyses are filled out. This had an important bearing upon the cancer age which is usually between 40 and 60 years.

Apparently Huguenin believes that this absence of cancer in swine is well worth studying to note why they are immune, believing that such a study would throw light upon human cancer. This is again a great example of what light normal animal physiology can throw upon human pathology. Commenting on the question of horses lying down, he stated that horses lie down less frequently as they get older. That is, a horse over 15 years of age is not inclined to lie down at all but the younger they are the more they lie down. These points I jotted down as significant in the study of old age.

BERNE, SWITZERLAND.

International Alpine Physiological Station (Jungfraujoch).

During my stay in Berne there was a meeting of the Jungfraujoch committee and I saw them for a few moments, among them Douglas. I also had the pleasure of dining with Douglas at the home of the Ashers.

I had intended to visit this Station personally but Dr. Carpenter, who visited it before the Rome Congress, reported very little equipment and was not enthusiastic about the laboratory as such. A very large percentage of people who visit this laboratory, I was told, suffer from mountain sickness and since at the time we planned our visit Mrs. Benedict was ill, I did not go up.

I have heard a great many reports on this laboratory. I am told that Hess is almost invariably ill when he goes up and Asher told me that 70 per cent were ill. Loewy confirmed this and said that the Jungfraujoch was too high to be of sufficient physiological importance to make the difference between the level of Davos and the level of Jungfraujoch worth while. He thought practically every problem could be studied at a considerably lower level than Jungfraujoch. (See Davos vs. Aix-les-Bains discussion under Dr. Saidman, Paris, page 324.) The Verzárs had been up and they had been there for some time studying the bilirubin in the blood and the conditions where there was no blood formation.

DAVOS, SWITZERLAND.

Forschungs-Institut, Physiological Institute.

Professor A. Loewy.

I made a special trip to Davos to see Professor Loewy and the successor to Dorno, Mörikofer. In the Physiological Institute I found very little activity and yet quite a good deal of apparatus. (See figures 149 and 150.) Loewy was interested in some problems on muscular work and in environmental temperature and metabolism with rabbits. He made an interesting comment upon the hibernating woodchuck, stating that at Davos they never went into complete stupor and remained thus all winter, but they woke up every three or four days and, as he put it, "staggered" to one end of the cage where they urinated and defecated, picked up a few mouthfuls of hay or food and went back to the corner where they slept. In other words, they do not have a complete dormant state such as one finds with snakes and lower animals.

The day with Loewy was an enjoyable one. We had many things to chat over and many experiences and on the whole it was a day well spent. He is about to retire but his successor has not yet been appointed (Wertheimer of Abderhalden's laboratory is mentioned), so he may hold on for a while. Altogether he is a most interesting man and yet I think frightfully superficial. For example, one or two experiments uncritically made often served as the basis for a great deal of discussion and writing.

Figure 149. Davos, Switzerland. Forschungs-Institut, Professor Dr. Adolph Loewy.

Figure 150. Davos, Switzerland. Forschungs-Institut, Professor Dr. Adolph Loewy.

## DAVOS, SWITZERLAND.

### Forschungs-Institut, Physical-Meteorological Observatory.

### Professor W. Mörikofer.

Mörikofer impressed me greatly. His institute was as usual very interesting indeed, and I particularly wished to see the frigorimeter. While I had received an impression in former years that Loewy did not think much of the frigorimeter, on the contrary Mörikofer is very enthusiastic about it and had self-recording instruments for keeping records of this continually. There was a copper ball about 10 cm. in diameter with a heating wire inside. It was run on a 220-volt circuit with fuse to prevent burning out, and a relay with a clock for recording the time in minutes that it actually must work to keep up the heat necessary to maintain the ball at a certain temperature, that is, the greater the cooling effect of the air the longer the heater must be on. The equipment included a thermometer, and the ball was supposed to be heated to 37° C. inside; hence the outside of the ball corresponded to the skin.

Personally it impressed me as an empirical thing, and while it might have important correlations with meteorological studies and indeed subjective impressions of weather conditions, I think it can have little true physiological basis other than through the subjective impressions.

## ZÜRICH, SWITZERLAND.

### University of Zürich, Department of Veterinary Medicine.

### Professor Dr. W. Frei.

Dr. Frei had been recommended to me by Dr. Huguenin in Berne, but I found relatively little of importance there. A few reprints gave me practically all that he had published with some side lights on the question of normal length of life. Frei was most interested in Mrs. Mellanby's papers on teeth and the work of F. A. E. Crew and said he had been unable to get her papers on senescence, so I promised to write her in his behalf.

ZÜRICH, SWITZERLAND.

Agricultural Institute for Feeding Domestic Animals.

Professor G. Wiegner.

Although I left this institute until one of the last on my stay in Zürich, which was broken by my trip to Davos, I found Wiegner extraordinarily interesting. He is a man of independent thought and rather contentious, and apparently is opposed to both Fingerling and Møllgaard. Fingerling emphasizes, according to Wiegner, the flesh calories and fat calories but Wiegner says that Møllgaard says they are the same. For example, Møllgaard emphasizes always the nitrogen-free calories but Fingerling always has the nitrogen-containing calories in his calculations. The result is that it is very complicating. Wiegner points out that Physiological "Nutzwert" is the equivalent of the metabolizable energy. Wiegner's great point is that of milk formation. He says Møllgaard is very hard to understand and his writings are comparable in obscurity only to Rubner's.

I had a long talk with Wiegner and he argues that the calories from 1 gm. of protein deposited should first be reduced to fat calories before determining the value of the food. In his studies of feeding values he believes in using rabbits entirely and one food combination. I feel there are two sorts of physiologists who work on these things; first, the physiologist who is very poor in calculations, and second, the mathematician who has very poor physiology. Wiegner admits this latter type but considers there are more errors in the first group. I was astonished at Wiegner's keenness in these feeding experiments. On the last visit I thought Kleiber was the active worker in these experiments and that Wiegner was little interested, being more as he called himself a soil (colloid) chemist, but now seemingly Wiegner is very keen, very enthusiastic, and says that we need new institutes to try out (a) various foods and (b) various races of animals.

He is always in a polemic with Møllgaard and says he is very sorry Møllgaard makes it so personal, as he does not like to be savage himself. He regrets very much indeed the California-Kleiber-Møllgaard mix-up.

We went around the laboratory, which is much the same as before, with the usual apparatus, and with what he calls the "Pullman respiration chamber" for rabbits, using the Haldane method for weighing. He has very large absorber Wolff bottles for a rabbit and his periods are 12 hours long. No dummies are used and no activity recorder. I think the gas-analysis apparatus is useless and I suggested that he use the Carpenter apparatus. He is much interested in this and I asked Dr. Carpenter to send him information concerning it. Wiegner had said that his sheep chamber was useless because the gas analysis was useless and he was very much inclined to turn the entire sheep chamber into the Haldane principle, that is, to weigh chamber, sheep, etc., but this seemed to me profoundly fantastic. His associate, who worked hard, had lost heart in the work since this gas-analysis apparatus had been so poor. Sometimes he had found 22 per cent of oxygen and could not find the leak. They are using Shipley's reagent and test under pressure and under vacuum and the apparatus is perfectly tight and then suddenly it is of no use. The very complicated mercury apparatus in which the level of the mercury was to be photographed (as shown in my 1929 report) is not used.

My impression of his laboratory was that given a Carpenter apparatus Mr. Fox and Robert Lee could put the whole place on a good basis technically in six months.

Wiegner has now a rat apparatus immersed in water and this is used for vitamin-B work. He finds that with rats with vitamin-B free diet there is no change in basal metabolism up to the point of death and there is no change in the respiratory quotient. He expected the acidic condition of the body to "eat up the bones" as he put it. Formerly he used a mercury seal for his rat apparatus but found that the mercury killed the rats, as they are susceptible to mercury fumes, and now he uses an oil seal or salt solution.

All his experimental work is done on rabbits. I do not like it. I think he is assuming too much to draw conclusions on rabbits for other animals. From a statement of H. T. Karsner we found out that rabbits are very prone to be abnormal. You can never tell until they have been dissected whether you have done your work on abnormal animals or not.

Wiegner gave me a number of his papers. He is an important man to keep in touch with.

## ZÜRICH, SWITZERLAND.

### University of Zürich, Laboratory of Physiology.

### Professor W. R. Hess.

The liveliest wire in Swiss physiology is without any doubt Professor Hess of the Physiological Laboratory in Zürich. Unfortunately for us very few of his problems have direct bearing upon our own work, as he is most interested in nerve physiology. Practically no metabolism work is done. He has studied the temperature of marmots or hibernating animals but little else of direct importance to us. There are practically no notes made during my visit to his laboratory. On the other hand in personal conversation, in going about the laboratory, one is continually impressed by the extreme versatility of this man, his great and wide variety of interests, and his enthusiasm.

Fleisch, his former associate who had gone to Dorpat,had just been appointed to the University of Lausanne. But a number of other promising young men were working with Hess and apparently he attracts an extraordinary quality of associates. Just before our arrival in Zürich he had been awarded a prize of 40,000 Swiss Francs. Each year it is awarded to the person who renders Switzerland the most service and his activity in education and particularly in establishing the laboratory at Jungfraujoch have been much appreciated.

In spite of absence of researches bearing directly upon Nutrition Laboratory work in Hess's laboratory, it still remains a fact that Zürich with its nearby Davos is an important center for any Nutrition Laboratory representative.

ZÜRICH, SWITZERLAND.

Lectures.

My lectures, which were given in the lecture hall of the Laboratory of Physiology, were extremely well attended.

On January 9th, in the evening, I gave the "third" lecture, to an audience of 175. (There was an announcement of this lecture.)

On January 10th I lectured for Hess's class, and had an attendance of 135.

This year the lectures were given in German, in contradistinction to the English of four years ago.

Zur gefl. Orientierung

Sehr geehrte Kollegen !

Am 9. Januar 1933 wird der bekannte Stoffwechselphysiologe Prof.Dr. F.G. B e n e d i c t, Direktor des Nutrition Laboratory of the Carnegie Institution of Washington in Boston (Mass.) in Zürich sein. Er hat sich bereit erklärt, aus dem speziellen Arbeitsgebiet seines Institutes einen ungefähr einstündigen, mit Projektionen illustrierten Vortrag zu halten, und zwar über den "Grundumsatz des Menschen im Lichte neuerer Forschung". Im Einverständnis mit den Herren Präsidenten unserer beiden stadtzürcherischen Aerzte-Gesellschaften wäre der Rahmen dieses Vortrages eine kombinierte Sitzung der beiden Gesellschaften. In diesem Sinne gestatte ich mir, Sie zu diesem in deutscher Sprache gehaltenen Vortrag freundlichst einzuladen. - Derselbe findet Montag, den 9. Januar 1933, abends 8.1/4 Uhr im Physiologischen Institut Zürich, Rämistr. 69 statt.

In kollegialer Hochschätzung

ergebenst

Zürich, den 3. Januar 1933

sig. W. R. Hess.

BASEL, SWITZERLAND.

University of Basel, Medical Clinic.

Professor Rudolf Staehelin.

Staehelin has a pressure chamber and they can use both very high pressures and low pressures with a very elaborate pumping system. Now he wants to use it for high oxygen. For most of the clinical work they use a Krogh spirometer but they have a 100-liter spirometer that they use when they want to get the respiratory quotient with analysis on the Haldane. Staehelin emphasized that the air in the spirometer must be mixed. He does not believe simple diffusion is rapid enough. This is especially true in the Grollman method with acetylene. The technique for obtaining the total blood volume he finds very difficult to get.

I was much interested in an electro-cardiograph that writes the cardiogram in ink, but thus far have been unable to get the name of it. I thought the system would have some value for us.

Staehelin is much occupied in clinical work and hence has almost no time for research. He is a very serious man and it is a pity that he is lost to research work.

## BASEL, SWITZERLAND.

### University of Basel, Physiological Institute.

### Professor F. Verzár.

It was a great delight to see Verzár in Basel. (See figures 151, 152, and 153.) I have always had a profound respect for this man and his great ability and variety of interests ever since I met him many years ago at Tangl's laboratory. Now he is settled at Basel and apparently contented, and I inferred from the colleagues with whom I talked that he is highly esteemed in the community.

He is doing much work on rats and all measurements must be made at 28° C. Verzár states the animal must fast 20 hours or longer, but he uses 6-hour periods and claims that the animal remains quiet. The rats are in a covered box. Verzár uses the closed-circuit method but weighs the carbon dioxide. There was a rather ingenious arrangement of four spirometers with four different apparatus writing on one kymograph, the spirometers being placed around the kymograph at equal distances. (See figure 154.) He notices a great difference in the females during the oestrous cycle. This effect, too long neglected, is very pronounced.

I found a great many kinds of research in progress in Verzár's laboratory. While he is primarily a physiologist he insists on having a chemical laboratory in connection with his physiological researches, so he has a good chemical laboratory with a number of chemists working on problems. He is an extremely keen man but I did not think his human respiration apparatus was at all good. There seemed to be a very large dead space with Lowen valves and there was one modification of a Roth-Benedict spirometer.

I think Verzár is a great success. On a former occasion I heard very serious criticisms of Verzár which annoyed me a great deal, but I think he is a very stimulating man and I recommend everybody to visit his laboratory.

Figure 151. Basel, Switzerland. Left to right: Madame Verzár, Mrs. Benedict, and Professor Verzár, on the edge of the Rhine.

Figure 152. Basel, Switzerland. Left to right: Professor Verzár, Madame Verzár, and Dr. Benedict, on the banks of the Rhine.

Figure 153. Basel, Switzerland. Left to right: Madame Verzár, Mrs. Benedict, and Professor Verzár, on the banks of the Rhine.

Figure 154. Basel, Switzerland. University of Basel, Physiological Institute. Four chambers for rats at Verzár's laboratory. Photographed through glass wall of laboratory room.

## BASEL, SWITZERLAND.

### University of Basel, Medical Polyclinic.

### Professor Alfred Gigon.

Gigon is much interested in carbon and uses the ultra-red photographs. He cuts fine sections of tissue and lets different amounts of light through, but there are practically no respiration experiments. He is interesting, keen, and a good addition to the Basel coterie.

BASEL, SWITZERLAND.

Lectures.

On January 12th, in the evening, I gave the "third" lecture before the Medical Society, at Verzár's laboratory, to an audience of 105.

On January 13th I spoke before the students in the Physiological Institute, giving the "second" lecture, to an audience of 200.

STRASBOURG, FRANCE.

University of Strasbourg, Faculty of Medicine.

Laboratory of Physiology.

Professor Georges Schaeffer, Professor C. Kayser, and Mlle. Le Breton.

Schaeffer is of the opinion that in problems in pharmacology one must use direct calorimetry. Thus far there has been altogether too much work done with thermometry alone, that is, no direct or indirect calorimetry. In the study of fever one must have direct calorimetry.

I found an interesting mixture of soda and pumice. They use one-half soda, granulated, and one-half pumice. This soda is the same as I found first at the laboratory of Lefèvre in Paris four years ago, about the size of rice. This is mixed with an equal volume of pumice of about the same size and they find it a satisfactory absorber for carbon dioxide. Furthermore, when it is exchanged one has but to take it out of the container, leach it out with water and dry it and the pumice can be used over and over again. (See figures 155 and 156.)

Collins apparatus. They had bought one of Collins' rat apparatus and out of the eight cans shipped them, two leaked. There was a leak around the axle of the blower, the blower throwing much oil along the line, and they were quite disappointed with it, of course. (See figures 157, 158, 159, and 160.)

I was very much gratified and relieved to know that Kayser was associated with Schaeffer and not, as I had supposed, with Terroine. Kayser is a splendid fellow with a great deal of excellent research on pigeons. I was particularly glad to see him because in a recent paper published by Riddle and myself, through some unfortunate situation an exceptionally good paper of Kayser's was not mentioned and should have been, for it practically forestalled all our work. I felt rather chagrined, apologized to him, and as a matter of fact made a public statement at one of my lectures that he had forestalled our work.

Kayser is most interested in trying to cut out by brain operation the daily rhythm of metabolism. Schaeffer states that the clinicians do not recognize the existence of a rhythm. As the chief of the clinic takes daily temperatures at 10 a.m. this is the time for those students the rest of their entire lives. They have not thought that the temperature is high at 5 p.m. and low at 4 a.m.

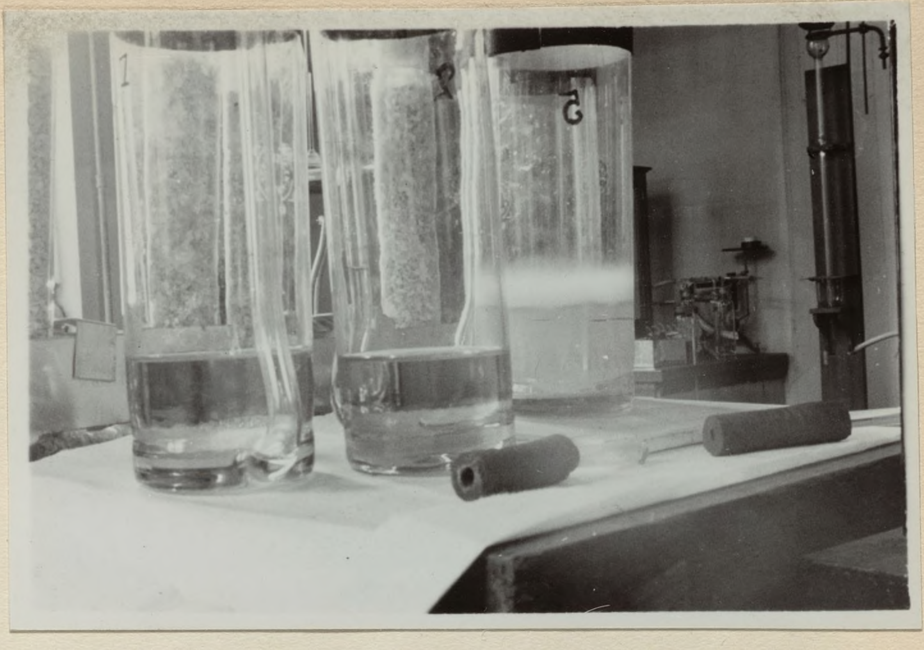


Figure 155. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Laboratory of Physiology, Professor Schaeffer. Absorbing apparatus for carbon dioxide and water vapor showing the introduction of gas to the bottom of the bottle, passing up through a porous gas disc which breaks up the bubbles into a very fine spray.

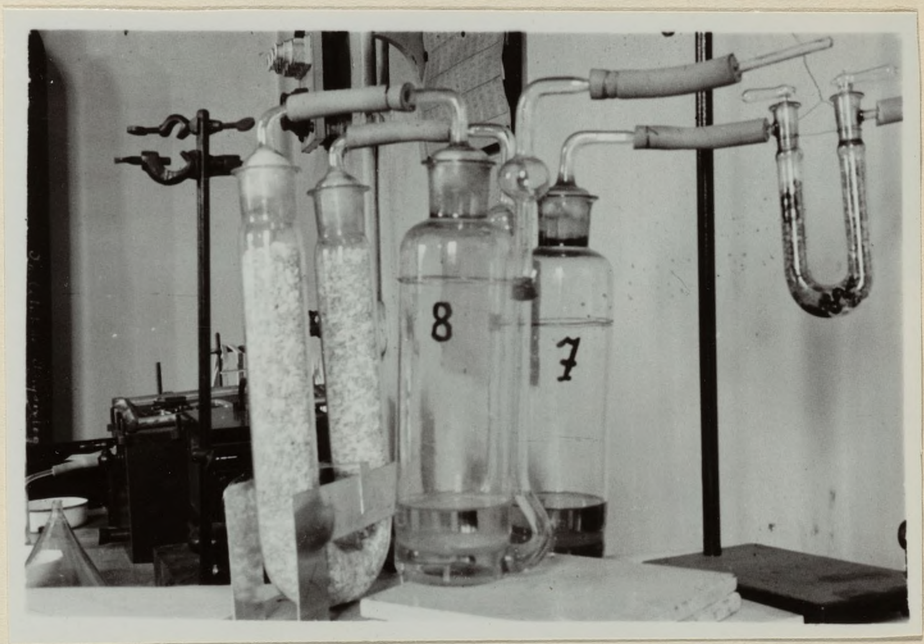



Figure 156. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Laboratory of Physiology, Professor Schaeffer. Another view of absorbing system used by Kayser and Schaeffer showing a U-tube with mixture of pumice and rice-formed sodium hydroxide, also sulphuric-acid bottles with entrance at side of bottle.

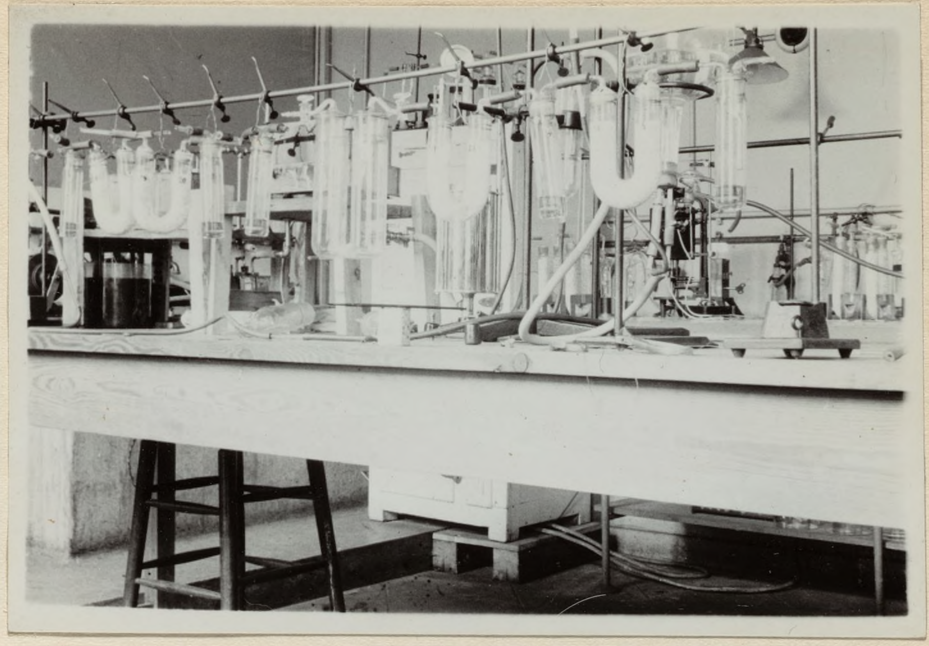


Figure 157. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Laboratory of Physiology, Professor Schaeffer. Photograph of individual rat apparatus, evidently made by Collins. They apparently have substituted for the cans a long series of U-tubes, shown particularly in this photograph. One sees rather dimly in the background the respiration chamber.

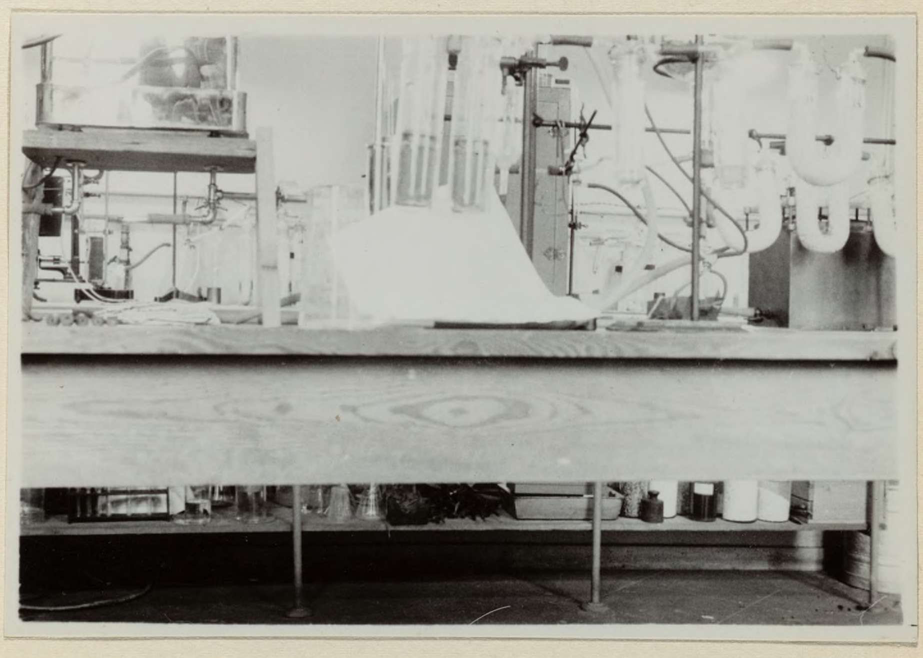


Figure 158. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Laboratory of Physiology, Professor Schaeffer. Another view of apparatus shown above, in Figure .

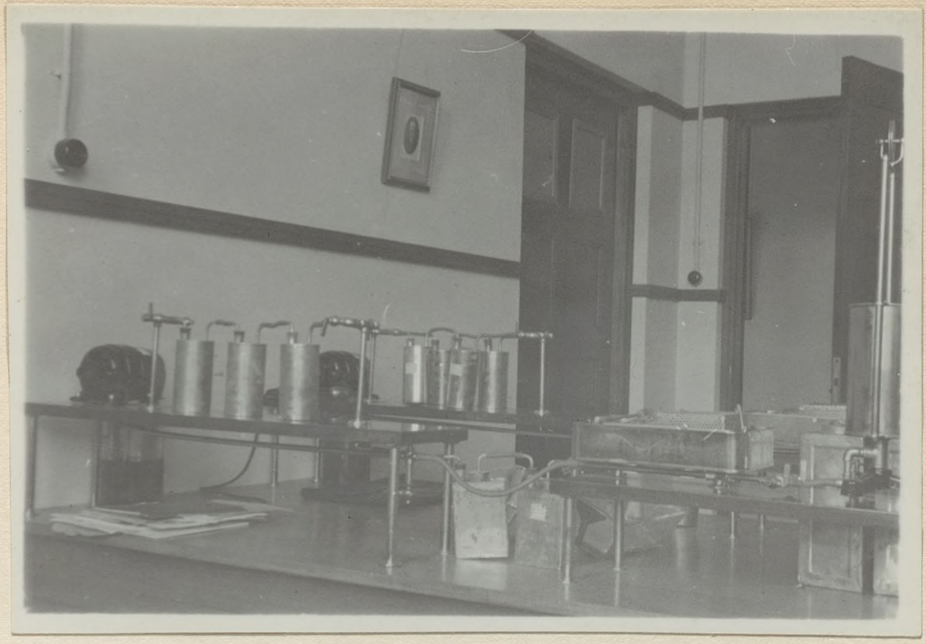


Figure 159. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Laboratory of Physiology, Professor Schaeffer. A Benedict rat apparatus, evidently supplied by Collins (see the glass baking dishes and occasional square cans). The apparatus also has round cans for calcium chloride and soda-lime. This is probably in Schaeffer's laboratory, although possibly it is in Berne, at Asher's laboratory.

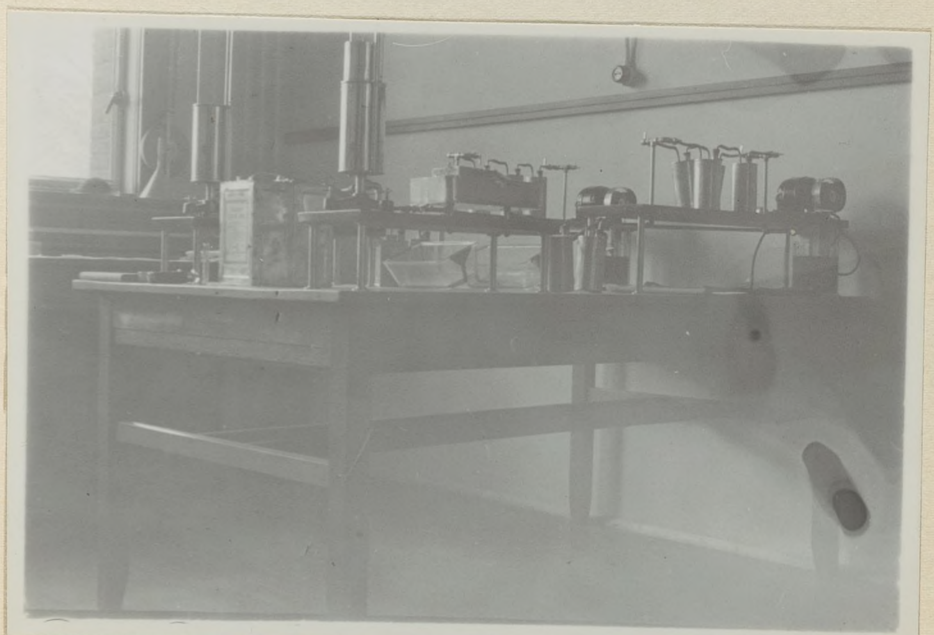


Figure 160. Another view of apparatus shown above, in Figure .

Kayser uses a one-chamber Benedict pigeon apparatus at 28° C. but his birds live at a low temperature. They keep them at about 8° C. for 24 hours. He finds if they are put at 10° for 2 hours and then measured at 28° the metabolism is 10 per cent higher. I checked up again with Dr. Kayser on the point that his pigeons live at 2°, are fed at 2°, then are kept one hour at 30°, a four-hour experiment is run at 30°, which really means a total of five hours living at 30° C., and this gave a 10 per cent increase in metabolism due to the existence at 2° C. previously. I was most interested in their technique and made a number of small photographs. (See figures 161 and 162.)

Another associate of Schaeffer was Mlle. Le Breton, a very clever woman working entirely with rabbits. She says they have no infections and I think they have had many of their animals for three years. They consider that the rabbit is perhaps the most quiet of all animals and find activity only between 3 a.m. and 5 a.m. They used the Haldane method with soda and half pumice. Their U-tubes were very large, 25 cm. high and approximately 2 cm. in diameter. One U-tube will thus take up 50 gm. of carbon dioxide. The tubes are all made out of pyrex and they use 500 gm. of the NaOH-pumice mixture in each.

Dealing with the question of the acclimatization of pigeons they stated that birds living at 0° and then kept for one hour at 28° C., in a four-hour period of measurement showed an increase of 10 per cent. Le Breton is studying the respiratory quotient of rabbits, and finds a respiratory quotient, immediately following food, of 0.95. The course of the respiratory quotient is then as follows: .95, .89, .86, .79, .77, .74, and .79, tending to mount again after 48 hours both with adult and young rabbits. With young rabbits after 24 hours of fasting it was .75, with adults .78. After 48 hours with young rabbits it was .78 and adult, .74. The next to the last period was .72 and the final period with adults is .75, showing a tendency to rise again.

All these were determined with the Haldane method. They had a very poor Carpenter apparatus made by Bleckmann and Berger, which they reported was no good, as it leaked badly. I suggested they put it in commission and test by an alcohol experiment for the respiratory quotient only. They are much troubled about the alcohol lamp but I told them there were two kinds, (1) respiratory quotient only that does not need a special lamp, and (2) absorbed carbon dioxide, when we must know the amount of alcohol burned. Schaeffer is troubled over the incomplete combustion, always fearing it. I told him we never found it and when we thought we had found it, it always turned out to be another cause. I told them that the people at Dortmund were able to make alcohol checks with 4 per cent carbon dioxide, something I could hardly believe.

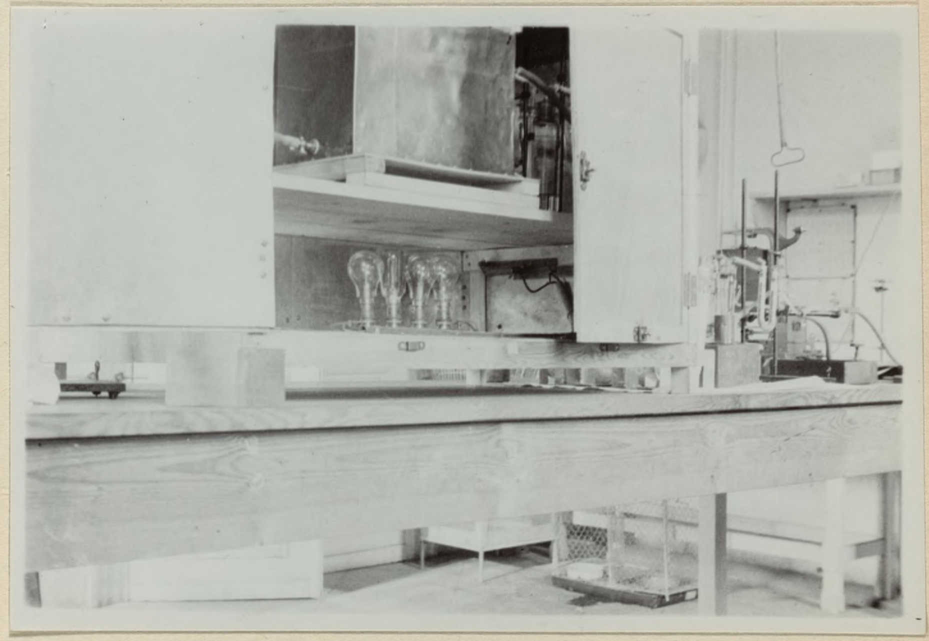


Figure 161. Strasbourg, France. University of Strasbourg, Laboratory of Physiology, Professor Kayser. Respiration chamber with electric lamps below for controlling temperature, studying rats and pigeons.

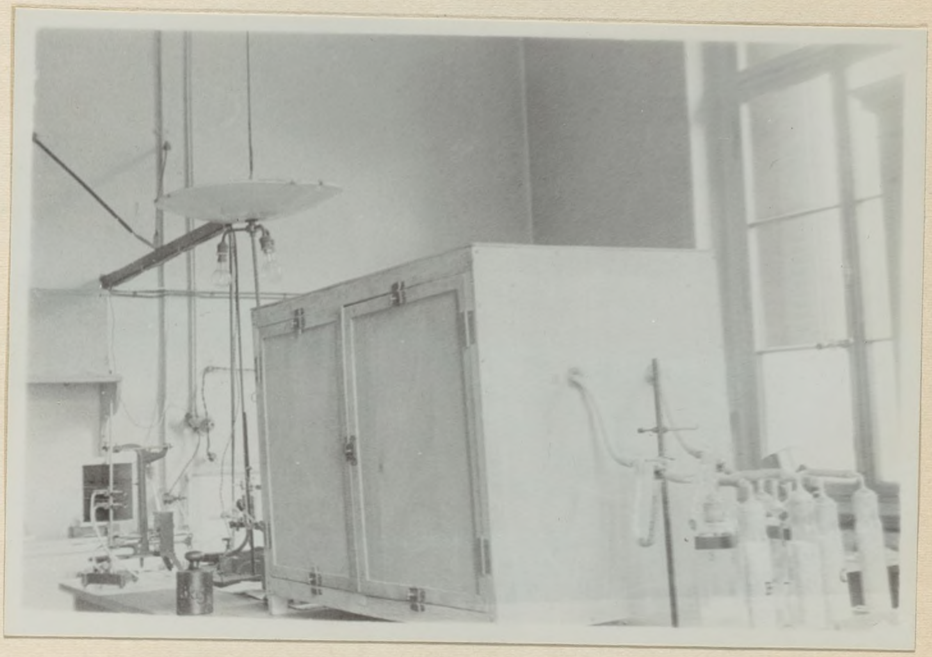


Figure 162. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Laboratory of Professor Kayser. Photograph showing absorption tubes and large chamber or thermostat for holding animals at a constant temperature.

Studying frogs they found that when the frog was not active the metabolism tapered off but if the frog moves then the curve stays up for several hours. Any physical activity seems to cause a pronounced increase over the already existing low basal. They found that frogs at <u>25° C. are always active</u>, even if put quietly in a black chamber. Le Breton states they are bad animals to work with unless one uses urethane, and the animal can not stand this for long experiments. In short 5-minute periods they find identical results, period after period, with the same animal. They found that frogs increased the combustion of alcohol with the temperature as per the law of van't Hoff. This was also reported by Nicloux.

Mlle. Le Breton believes that there are different protoplasms and the circulation is adapted to the animal and condition. For example, in the egg there is at first no air; then as the egg develops circulation is built up as needed. She thought the idea of blood and circulation as determining metabolism might be reversed. Really the circulation is built up to meet the needs of the metabolism, that is, going back to the differences in protoplasm. Does it have any relation to fat-ash-water-free body?

Schaeffer states that raw, uncooked starch is not digested by pigeons even if mixed in a ball and moistened. Apparently it must be cooked. On the other hand, the raw starch in the interior of a dry grain is digested. Dr. Pozarski of the Société Scientifique d'Hygiène Alimentaire did a lot of work on this point.

## STRASBOURG, FRANCE.

### University of Strasbourg, Faculty of Medicine.

### Institute of Biological Chemistry.

### Professor Maurice Nicloux.

The visit to Nicloux's laboratory was, as is always the case, very stimulating. (See figure 163.) As I stated at a dinner at Nicloux's house, I saw more and learned more in 11 minutes and 43 seconds in Nicloux's laboratory than I would learn in 11 hours in most others. Of course his forte is micro-chemical determinations and he is at the present time keenly occupied in the determination of very small amounts of alcohol. He has published researches on the use of alcohol, on the frog as a cold-blooded animal, and on the mouse, and I believe that he and Kayser have worked on the pigeon. The cold-blooded animal uses alcohol in accordance with the van't Hoff law, that is, the higher the temperature the greater the utilization of alcohol, but the pigeon has the same utilization independent of the concentration of the dose. (The work done on the mouse is reported in Bull. Soc. Chim. Biol., 1931, p. 856. Dr. Carpenter should check up on the utilization of alcohol by the mouse as I think it is very important.)

I had the pleasure of meeting in Professor Nicloux's laboratory Dr. Pierre Graber, his associate, who showed me about very extensively.

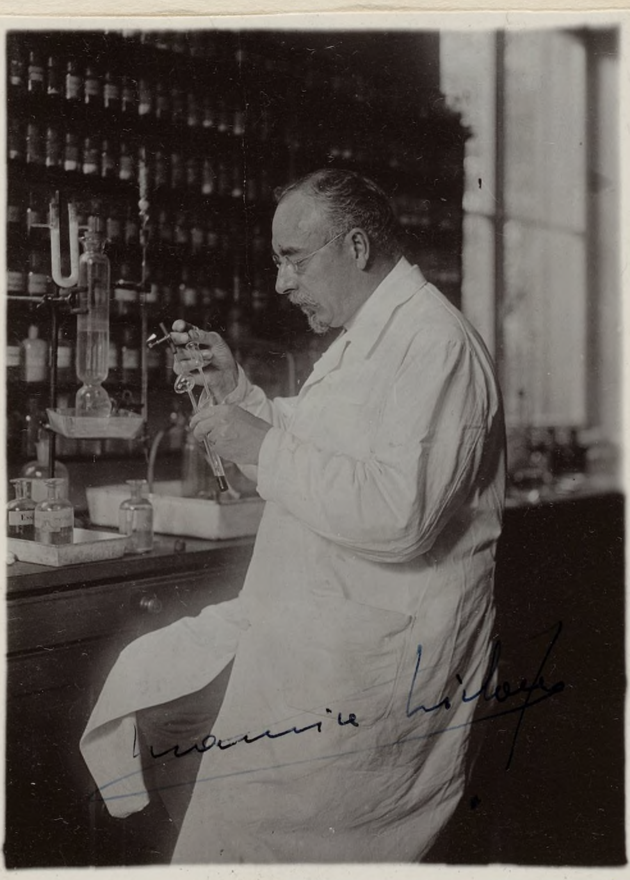



Figure 163. Strasbourg, France. University of Strasbourg, Faculty of Medicine, Institute of Biological Chemistry, Professor Maurice Nicloux in his laboratory.

STRASBOURG, FRANCE.

Lectures.

In Strasbourg I gave two lectures under the auspices of the Faculty of Medicine.

The "second" lecture was given on January 17th, to an audience of about 200.

On January 18th I gave the "third" lecture, which was attended by a group of about 110. (There was an announcement of these lectures.)

UNIVERSITE DE STRASBOURG

FACULTE DE MEDECINE

Strasbourg, le 13 janvier 1933

Mon cher collègue,

Le Professeur F.G. Benedict, directeur du laboratoire de la "Carnegie Institution" à Boston fera 2 conférences à la Faculté de Médecine, auxquelles il serait bon que le personnel de votre service assistât, vu la personnalité du conférencier. Elles auront lieu les mardi 17 et mercredi 18 janvier 1933 à 18 h. à l'Amphithéâtre de la Clinique médicale A.

1° Conférence: Le métabolisme de base en physiologie comparée

2° Conférence: Les Progrès récents de l'étude du métabolisme de base de l'homme

Ces conférences auront lieu en français.

Vous serez aimable de communiquer cette information au personnel de votre service.

Veuillez agréer, mon cher collègue, l'assurance de mes sentiments les meilleurs.

Le Doyen,

P. Merklen

## STRASBOURG, FRANCE.

### The strange case of Professor Emile F. Terroine,

### Institut de Physiologie, Faculty of Sciences, University of Strasbourg.

It has seemed impossible for me to get in personal touch with Professor Terroine. Year after year and tour after tour I have tried, but always unsuccessfully, to see him. He is a man who has been frankly antagonistic to the American writers in general and the Nutrition Laboratory in particular. He is very acrimonious and he is in constant discord with his colleagues. Thus I am told it is utterly impossible for Professor Schaeffer to have anything to do with him whatsoever. I wrote to him on July 2, 1932, prior to beginning my tour, and again on July 30, but heard nothing from him. I then wrote him on January 3, 1933, a paragraph of which letter is given herewith.

"Although I have never had any reply to my two letters of July 2nd and July 30th, 1932, you still see that I am making every move that I can to see you personally. Mrs. Benedict and I now plan to reach Strasbourg from Basle about January 14th or 15th and to leave Strasbourg for Paris the 18th or 19th. During my stay I shall make one effort to see you personally. If I am unsuccessful I am afraid I shall not have courage to carry it any further."

I do not understand Terroine at all. It is a rather singular thing that in the entire European field of physiology the only two points where there is anything but a very friendly relation with the Nutrition Laboratory happen to be with Frenchmen, (1) Terroine and (2) Dautrebande. I have considered it an important part of my mission to try to get in touch with these two men and straighten out seeming difficulties, but it simply can not be done and I have finally come to the conclusion that it is not worth the effort.

When I arrived in Strasbourg, although I had heard nothing whatsoever from Terroine, although the Carnegie Institution had sent him a copy of the snake monograph of which he had never acknowledged receipt, I tried to telephone him from the hotel but without success. I then took the chance of going to his laboratory. On my arrival there I was told he was not there, but was at home. Monsieur Bonnet, his first assistant, received me courteously but coolly and in a perfectly justifiable manner farmed me out to a very intelligent young woman (whose name I have forgotten). She showed me all over the place but I found practically nothing going on in gaseous metabolism. They were all occupied with nitrogen metabolism and chiefly with frogs. I left my card and expressed regret at not having seen Professor Terroine, and told them I would be at the Hôtel de France and I was to lecture on two afternoons at the Faculty of Medicine.

I sincerely hoped he would come and speak to me at one of these lectures, but although I remained in Strasbourg four days after this, at no time did I have one word, directly or indirectly, from Professor Terroine. I feel that as a representative of the Nutrition Laboratory I have done all that is legitimate to try to get in touch with this eccentric, although extremely intelligent, individual.

PARIS, FRANCE.

Laboratoire de la Société Scientifique d'Hygiène Alimentaire

et d'Alimentation Rationnelle de l'Homme.

Professor J. Lefèvre, Monsieur J. Alquier, and Mme. Radoin.

Professor Lefèvre's apparatus is as imposing, complicated, impracticable, and useless as ever. Now he is working with geese. He states that the apparatus is working, but I was interested to see that he had given up the frightfully expensive electric furnace he formerly had to burn the "methane" which he maintained was given up by man and geese. He had these furnaces but I found by conversation that he had not used them even in studying sheep, when he should have. The use of air current to bring away the heat makes possible a self-regulating humidity, but the volume of air needed to bring away all the heat is tremendous.

In his apparatus one-fifth (approximately) of the air passes through four trains of absorbers for carbon dioxide, which is weighed in a closed circuit and the heat brought out by the air current is calculated. By passing one-fifth of the air in this way he holds the carbon dioxide to a fairly constant level. In other words, there is five times more air, if not a greater amount, than is needed to hold the carbon dioxide constant. The regulation of the temperature is very perfect and it is a real feature of the apparatus. The hygrometers used, which were arranged to make electric contacts, thus turned dry cool or dry hot air into the chamber. There was a large cooling machine and brine tank which was expensive to run.

On the afternoon that we were first there at a special reception Lefèvre gave a description of his apparatus with his usual very clear, didactic manner. He was extremely pleased. We had tea in the laboratory with Senator David, who I found out was blind. On the afternoon of the tea the apparatus was first functioning with a man inside reading a paper. When we came back after the tea the man was riding a stationary bicycle which was constructed so as to have a vertical shift come out on top of the calorimeter and the heat of the ergometer was generated outside the calorimeter. The whole calorimeter is built upon a tremendous amount of theory, bulky, but full of impracticable conceptions.

I found on another day he was having geese experiments. He had no use for "stuffing". He maintained it was not normal, and it was not physiological. Lefèvre is very strong for the normal phases of life. I then suggested, based upon our experience with the Rhode Island Red roosters, that he let them fast and then let them eat all they will. It was amusing to see how pleased this old chap was at this suggestion, i.e., normal self-stuffing. In his experimental plan his whole experiment of 2 to 4 hours was based on (1) one combined weighing of one-fifth of the carbon dioxide, (2) calculations of the remaining four-fifths, and (3) oxygen admitted from a cylinder through a gas meter. But there was only one respiratory quotient determined for the whole experiment. This would do on a fasting day but certainly would not do over a four-hour feeding experiment. I don't think Lèfevre knows what he is about. He is a very pleasant chap but there is a lot of money tied up in this wholly impracticable equipment, and it remains a fact that his apparatus is as nearly useless as anything could be. If Lefèvre died tomorrow there is not a living man who could run the apparatus and hence the whole thing would have to be scrapped. Even his own associate does not understand the thing well enough to run it. It represents a gigantic amount of money and is quite comparable to that former great experiment which amounted to nothing, that is, the calorimeter of Mlle. Pompelian and Professor Letulle.

Monsieur Alquier is now extremely busy in committee work and as an organizer, as before. I believe he is doing practically nothing in his own laboratory. We saw a great deal of him and he is very active in many of the governmental things going on around Paris. Madame Randoin is working at rue de l'Estrapade 16, and as a matter of fact has a small respiration chamber for rats, using the closed-chamber principle, but I saw no experiments actually in progress. Alquier has stopped all his work on the large animals as I understand it, and when I asked him on several occasions to show me his laboratory he emphasized that he was doing nothing in research work now. He is an extraordinarily active man, a very good organizer, and apparently is counted upon by a good many people for directing different government projects.

PARIS, FRANCE.

Dr. J. M. Le Goff.

Dr. Le Goff to me is a rather pathetic picture of a man with a real intense research drive and no outlet. He has not a hospital appointment and has no laboratory. Four years ago he was making a few experiments at his country home. He is most interested in vaso-dilatation and had used cobalt solutions to produce it in rabbits' ears. Not infrequently he writes for the medical journals and has a deep, keen interest in America and particularly American medicine.

Of course he is too old now to secure an appointment of official position but it is a great pity that this man's initiative and interest could not have been better directed.

## PARIS, FRANCE.

### Hôpital des Enfants Malades.

### Professor P. Nobécourt, Dr. H. Janet, and Mlle. M. Bochet.

A visit to this hospital showed very considerable changes in the metabolism section of four years ago. Now they had apparently a division for basal metabolism in which they had measured both children and adults. Of course dominated by the technique of Stevenin, they were using the Plantefol gas analysis, face mask, dry gas meter, etc. For children much the same equipment was being used, and while there was an attempt here to secure metabolism measurements in a metabolism laboratory the impression on the whole was very poor. There was a great deal of active bustling and disturbance in the room where several patients, children, were supposed to be studied at the same time.

I think their whole technique and respiration studies were very bad. They had a Tissot mask and used room air (there were 10 people in the room) and found by analysis that the room air had from 0.1 to 0.3 per cent carbon dioxide, that is, in the latter case ten times normal. I do not think they analyzed it every day, but took an average value, which means of course a very large percentage of the total air must be deducted for ingoing air. It was interesting to see them apply a man-size Tissot mask to a child, using some large "C" clamps on the side to take up slack, and even then there was a large amount of dead space. (See figure 164.) They used a 50-liter rubber sack. The subjects remained seated nearby for one half hour. They then walked into the laboratory, sat down for 5 minutes, were measured for 6 minutes, and that was the end. With the Plantefol gas-analysis apparatus they report that they often found 2.0 per cent carbon dioxide in the bag. I wonder if undiluted expired air can be so low as this. I got a poor impression of it. Also I found that all the adults from the nearby Neckar Hospital are done there.

Mlle. Bochet is in the other room where she conducts basal metabolism experiments on children with her sack, glass globe, pump, and Tissot spirometer. (See figures 165 and 166.) She finds room air is 0.1 carbon dioxide but the air in the spirometer may be 1.0. Under these conditions one can see that the correction for the room air may be as high as 10 per cent of the total. I suggested that she ought to put in an air pipe from outdoors and work with air of a known composition. Her glass globe had at the upper part of it a bowl of ice leading through a metal spiral which ran down into the glass globe and made it possible to cool the air.

Figure 164. Paris, France. Hôpital des Enfants Malades. Clinic of Dr. Janet and Mlle. Bochet. Measuring the metabolism of a child. Note the Tissot mask collapsed with clamps to make dead space smaller. Note that ingoing air is room air. Expired air goes into a Douglas mask. This is a very bad equipment.

Figure 165. Paris, France. Hôpital des Enfants Malades. Bell jar respiration apparatus used by Mlle. Bochet.

Figure 166. Paris, France. Hôpital des Enfants Malades. View of respiration apparatus with glass bell jar and rubber sack of Mlle. Bochet. Tissot spirometer at the left.

I saw again the apparatus for using high oxygen with children, that is, the bell jar with the rubber blanket attached to it made of fabric used for balloons, and Mlle. Bochet was much interested in having found a new type of zipper that seemed to produce an air-tight closure. Oxygen was introduced at 5 liters per minute, so she thought she was getting 60 per cent oxygen and says it works very well. As near as I could make out they were not using the infant respiration chamber that we sent them, although they were making attempts to put it in condition. The trouble was to get a blower that was air tight. The closed circuit is out of the question and for the open circuit I don't think they have sufficiently mastered the technique of the Plantefol apparatus to justify its use for such a small carbon-dioxide increment, and worse than that I don't believe that the Plantefol apparatus at its <u>best</u> and with the <u>best</u> operator approximates the accuracy of the Carpenter apparatus. So the outlook is not particularly good as it appears to me.

Mlle. Bochet is certainly a very clever girl, but I don't know how far she is getting. I think if she could have a year with us she would be quite a different person but that is out of the question.

## PARIS, FRANCE.

### The pediatric section of the Salpetrière.

### Dr. Ribodeau-Dumas and Dr. Max Lévy.

I found in this laboratory most interesting work, or at least potentially interesting work being done on children living in high oxygen atmospheres. Thus, the assistant, Dr.Max Lévy, stated that when they had the children at 55 per cent oxygen they found disturbances of the acid base equilibrium. But the question is, what was the index of this disturbance? They found a shift in the ratio of chlorine in the corpuscles and the chlorine in the plasma, varying greatly with the acidity, also with the $P_H$. It is very hard to determine this. On further discussion it seemed to me also that the real oxygen percentage was very uncertain. I felt that the method of determining and sampling was very bad, although the problem had been farmed out more or less to Mlle. Bochet at the laboratory of Dr. Janet. I made a number of suggestions to them as regards supplementary ventilation. They also had found that it was a very good plan to have the child have high oxygen intermittently, that is, one-half hour at high and one-half hour at low. They thought this much better than continuous high oxygen but I really don't believe that they knew <u>what</u> percentage of oxygen they had when continuous, that is 24 hours a day.

Personally I can not believe that, everything else being equal, the presence of 55 per cent oxygen can alter the acid base equilibrium, and I am willing to predict that they have much more than 55 per cent. Their method of ventilating the chamber is very primitive; that is, they have a chamber with a small hole in one end and let the oxygen stream in at the rate of 1500 liters for 3 hours or 500 liters per hour, or about 9 liters per minute of pure oxygen. (See figures 167 and 168.) This equals about one-half the amount of the total ventilation used for man with the helmet apparatus, that is, about 20 liters per minute. Under these conditions it would appear as if they must have a very much higher oxygen content than they think they have. The whole question of the determination of this percentage demands very considerable revision. Before this I had talked with Mlle. Bochet and was not particularly impressed with her statement as to what they found and why they found it with their methods, etc. It is a good illustration of a very important problem being very poorly attacked while with a little care and thought it could be done very well indeed.

I talked with Max Lévy and was, as formerly, much impressed by him. He is a very clever, serious chap and I believe that he desires to do scientific work but has to use "clinical methods". It illustrates very well why clinical methods are held in disrepute.

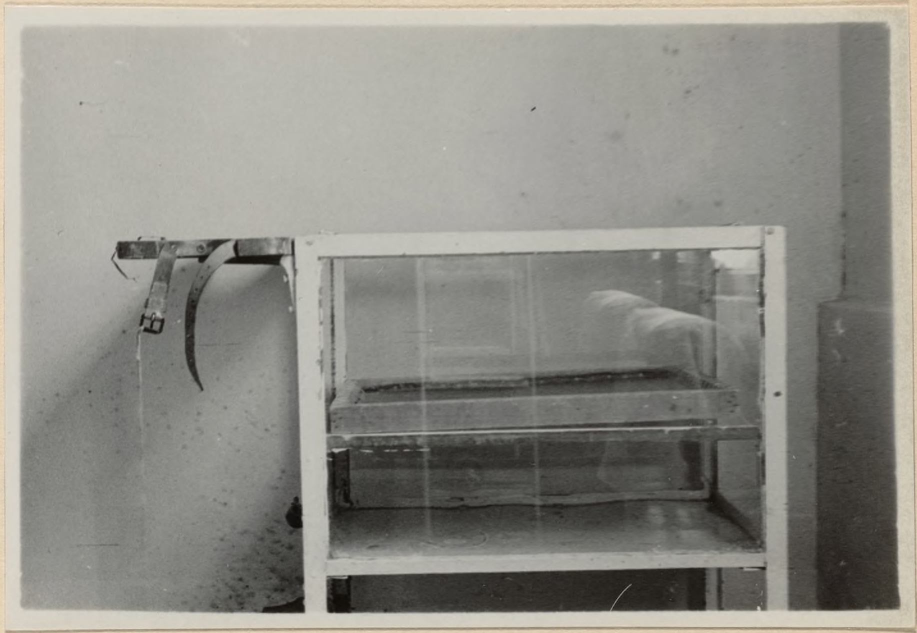


Figure 167. Paris, France. Salpetrière, Clinic of Professor Ribodeau-Dumas. Glass-walled respiration chamber for infants. In this chamber oxygen is admitted from a steel cylinder strapped to the upright at the left of the chamber, a part of the chamber. Oxygen escapes through a hole at the bottom; a very impracticable and unknown percentage of oxygen is thus secured.

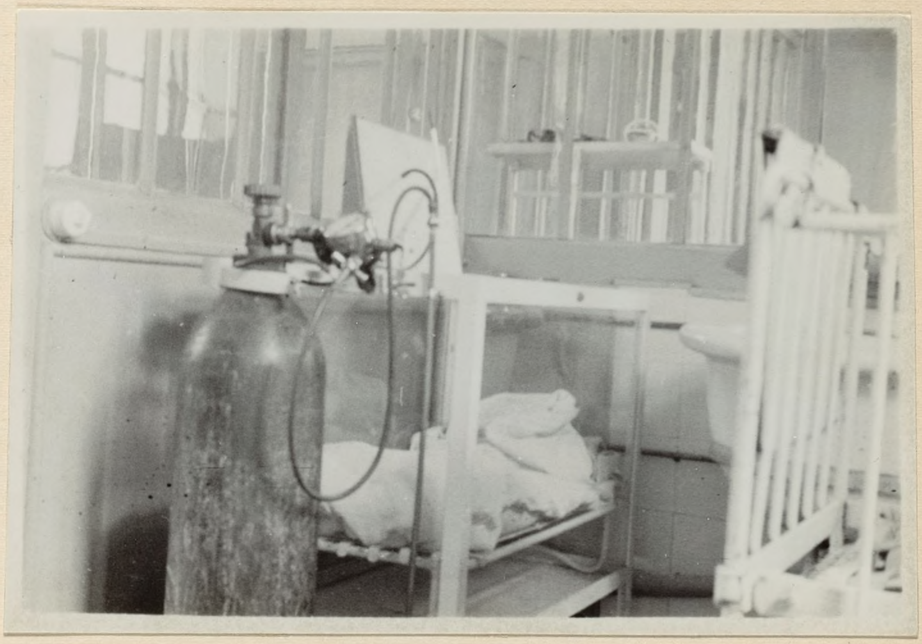


Figure 168. Paris, France. Salpetrière, Clinic of Professor Ribodeau-Dumas. Photograph of oxygen respiration apparatus. Oxygen is admitted from a cylinder into the glass-walled box, and simply seeps out of the box with no special provision for ventilation.

PARIS, FRANCE.

Institut Pasteur.

Professor G. Bertrand and Dr. Pierre Le Compt-du-Nouey.

I had the misfortune to visit the Pasteur Institute when Bertrand was away, but I had met previously at a dinner at Lapicque's Dr. Pierre Le Compt-du-Nouey who had charge of the new division of biophysics at the Pasteur Institute. Frankly, I was bewildered by the mass of new and obviously frightfully expensive apparatus of every conceivable form, spectrographs of every kind and, too, a great deal of work on blood serum, especially with the question of viscosity and the changes in the hydrogen-ion concentration. The whole thing was beyond me but I was astonished that they could get such an equipment together and yet there was the complaint that funds were obtained only with difficulty. It appears, however, that at least sometime rather recently a very considerable sum of money was available to set up this entirely new division.

Professor Bertrand I was very sorry not to see at the laboratory, although his micro-chemical work dealing with the minute qualities of the metals does not interest me especially. I did see him frequently on several social occasions and at my lectures and at a most delightful social affair at his house on Sunday afternoon. Altogether he is a most stimulating, charming personality and a complete refutation of the idea of sloppiness and carelessness in French scientific work. He is a man of whom it can be said, "He thinks in nothing less than the third and fourth decimal figure."

## PARIS, FRANCE.

### Institute of Actinology.

### Dr. Jean Saidman and Dr. Jean Meyer.

Before reaching Paris I had had most cordial letters from Dr. Jean Meyer, stating that the directors of the Institute of Actinology were very anxious to have me visit the Institute and that they had arranged for a banquet for Mrs. Benedict and myself. This occasion was a most delightful one and I met for the first time all of the directors and their wives. On a succeeding day I went to the Institute, which still is in its almost impossible quarters in a very old, ramshackle building, but packed from cellar to garret in every corner with innumerable forms of apparatus for heat and particularly light therapy. While the presiding genius is Saidman, he has a most ingenious corps of young men who are pushing this institute and its possibilities to the limit.

The whole institute is devoted to three major studies. First of course is diagnostics, second, therapeutics, and third, to me most interesting, the pure research with the waves of various lengths. A subsidy from the state and liberal concessions on the part of electrical companies have made it possible for them to continue even under the present economic conditions.

Aside from the battery of various types of electric lamps and source of radiation, all of which were reported upon in my 1929 report, the subject under investigation that interested me most was Saidman's work in skin temperature. He had prepared a rather crude, I thought, skin temperature measuring apparatus, a thermo-junction affair, and had made a number of observations, but I got the impression of its being rather superficial. One of the most astounding things he showed me was that he found there could be a local inflammation produced in which there could be a high surface temperature but a low radiation. He explained that he thought the fluid under the skin may absorb the rays and hold the heat back. As usual, as in every laboratory, the research must have an endocrine slant, and they found that the sensitivity of the skin ran along fairly parallel to increased thyroid action, so much so that Saidman thought <u>the sensitivity of the skin may be a test of basal metabolism</u>. Thus if the basal metabolism is affected solely by thyroid activity and the skin increases its sensitivity with this activity, it may be argued that the sensitivity of the skin is a test of basal metabolism.

It is worthy of note that Saidman and the associates in the room at the time had never heard of Bohnenkamp and Cobet. That brought up a discussion of preparation of papers and citing of literature. Jean Meyer said that a German won't cite a French paper and stated that it is very difficult for a Frenchman to get up a good literature, and therefore they have to be content with giving all of the French literature. This appeared to me as being very naive. They were loud in their praises of the American Journal of Dermatology but thought that the syphilitic journals were very poor.

Saidman is working on a change which corresponds more or less to the radiometer or pyranometer of Abbot or something like the instrument I recently saw in Dr. Du Bois' laboratory in New York, developed by Dr. Hardy. This involved the use of a thermo-pile a definite distance from the skin with a window that may be either left open or provided with a ray filter of some kind. Evidently he is trying hard to do something but it seemed to me very crude, although he had good quantitative ideas. He waited for and checked the baseline after each observation. He also tried to get the skin temperature at the same time to compare it. They emphasized to me that the most important work was by a doctor at a sanitarium at Cannes and Aix-les-Bains where the remarkable "solarium tournant" had been constructed.

Saidman judges of the solar activity of places by the time element in which a certain skin reaction could be obtained. Thus he found that at Jungfraujoch it took 13 minutes, at Davos 15 minutes, at Aix-les-Bains 15 minutes, and at Cannes 15 minutes. Thus there was hardly any change between the low altitude of the two French places and the high altitude of Davos, and the extremely high altitude of the Jungfraujoch. He felt that altitude *per se* was not so important for light therapy. He had visited Davos, Arosa, and Jungfraujoch and made tests on his own skin to see which was the best place.

The black body. Saidman had a black body for study which consisted of a tin can 10 cm. high and about 10 cm. in diameter. It was painted flat black inside and rested on a water surface in a can, the level of the water being just above that of the bottom of this black can. This had a ring so it could rest on the edge of the water-containing can. The water in this can was at 37° C. and the bottom of the can was 37° C. This was his standard black body.

The impression one gets on talking with Saidman and his associate Meyer, and the others, is that they are really trying very hard to do some good work but, as is so frequently the case, their work pronouncedly savors of clinical methods. I presume that the solarium is in very large part a commercial feature and probably the clinic at Aix-les-Bains is likewise commercial. But still at the Institute of Actinology at Paris there is no question but what a large amount of research as such is attempted. Saidman is a voluminous writer, has published a large number of books, edits a journal, and is very active in the various congresses for actinology, a great traveler and plans to come to America in the fall of 1933. While most of this work is definitely outside the sphere of the Nutrition Laboratory, surface temperature has direct bearing upon our work. The Institute is well worth while keeping in close touch with.

PARIS, FRANCE.

College of France, Department of Physiology.

Professor André Mayer, Monsieur L. Plantefol,

and Monsieur L. Chevillard.

This laboratory is like a hive of industry with a great variety of things being studied. There were many different researches in progress. One thing interesting me much was an electrical compensation calorimeter in the basement, in a room which is held at a constant temperature, 18.1° to 18.2° C., by a thermostat. For example, if an assistant comes into the room enough heat is cut off to equal the heat he gives off and when he goes out a similar amount of heat is automatically developed. The calorimeter was under the charge of Monsieur Gasnier. This calorimeter consists of two chambers and is definitely designed in every way for work on an adult rabbit. The thermo-junctions are installed on the Bohr principle and the heat is brought away in the air current. They use only one thermo-junction on each can, and that is about 10 cm. from the wall. The assistant who talked with me thought you could get one junction better placed symmetrically than five, and they had found one junction would work. In other words, when each of the two cans are at the same temperature there is no deflection on the galvanometer.

Using the Haldane principle they weighed the calorimeter chamber complete and the absorbed carbon dioxide is weighed. There is first inside a wire netting covering the animal something like a tunnel. This is followed by a copper sheet and this by the air-tight walls of the calorimeter. He compensates for changes in weight of the rabbit by using extra shields; thus by equalizing the heat inside the chamber there is what one might call an internal compensation. In the compensation chamber is a resistance wound on a triangle to approximate the form of the animal. They have an alcohol lamp of special construction that I could not very well sketch, and find the error is about ±3 per cent. Inside of the compensation cage there is an electric circuit of 110 to 120 volts for sudden heating, to use in compensating for heat developed by the animal and retained by the chamber during the process of weighing. (See below.) They have also in each cage a thermo-electric circuit and two couples of copper-constantin to establish the equality of temperature of the two cages, and there is a bolometric circuit giving the temperature of the cages which is registered.

The primary idea of this calorimeter is to keep the voltage constant, but change amperes by changing the resistance inside the calorimeter. This can be changed by the operator in a number of steps by having a switch outside the calorimeter. In this way the meter registers everything correctly. The ventilation is 7 liters per minute. One interesting thing is that the whole calorimeter can be disconnected from both the electric circuit and the air circuit by plugging the air tube and disconnecting it and weighing it. It takes about 7 minutes to disconnect, weigh, and reconnect, but the can heats up during this time so they heat the other can and start at about 2° higher, that is, 20° to 22° C., instead of 18° C. They have no use for alcohol checks, as they are dominated by the notion of incomplete combustion of alcohol; first, because of the smell, second, they accept Lefèvre's experience with his chamber, and third, they get bad respiratory quotients. Of course none of these points are justifiable.

The animal's temperature is taken after the experiment as it disturbs him and gives too much water in the first period if taken before. They make experiments two or three hours long but have 24 to 72 hours fasting. The animal is quiet. His nose is not against the glass front but he has a tendency to back up into a tunnel where he remains immobile. Mayer thinks it is a reflex with these animals and recommends it strongly as a measure for keeping them quiet. Thus far with our experiments in Boston we have not noted this. They tried to find all animals about the same size and about one year old. Their whole experience was that animals of different color had about the same metabolism. These experiments in Mayer's laboratory are not basal for all their work is done at 20° C. But since Mayer and Nichita found the temperature effect flattened out very strikingly between 18° and 30° C. they argue that if the work is done at 20° there would be no coefficient. Personally if their curve is correct I would think their values must be basal.

What interested me greatly was their use of an electric meter, a standard form of household meter for alternating current, using 120 volts, specially constructed at a very low price, with a calibration card furnished by the factory. Thus from 0 to .8 amperes the meter was 98 per cent, 0.8 to 1.2 amperes it was 100 per cent, and from 1.2 to 2.0 amperes it was 98 per cent. Subsequently in thinking over this matter I wondered whether the meter counted in the current to run it, as we found was the case with the American meters? The question arises, could it be used with direct current and to what limits? This meter is worth looking into further and I am sure we can call on any help from Mr. Bull. The electric meter is manufactured by Société des Compteurs, 12 Place des Etats Unis, Monteonge-en-Seine, France.

This equipment as such impressed me very much, although before understanding it completely I have to wait to read a reprint that was being held in proof. But the idea of having the calorimeter in a constant temperature room is a good one. It is a pity to make a calorimeter for only <u>one size of animal</u> but Mayer emphasized that that was their main problem. Many of the water relationships in the work are closely allied with our problems at the Nutrition Laboratory. Since the building is very archaic and their work is done in caves, one gets the impression that things are not up to date. This is not true; a more up-to-date man does not exist than Mayer or Plantefol.

<u>Research on mice</u>. I had a long talk with Monsieur Chevillard, assistant of Professor Mayer, who was working with mice. The mice were at 20° C. and feeding, but the respiratory quotient was very low, 0.74, and stayed at this level for 6 hours. They used the Haldane method of weighing the U-tubes and the chamber, but also claimed that they checked it for respiratory quotients by gas analysis. They were particularly interested in the ratio of oxygen to water. He finds the same slope for the two factors although much wider variations in special points with the water, but the general slope is the same. <u>With mice at 20° C. about 10 per cent of the total heat is given off by water vapor</u>. He believes that the smaller the animal the smaller the percentage in water vapor but he has no idea of what it would be if the animal were Nüchtern. The researches have been reported on the basis of one hour, but I think the periods are too long for basal results. They were using a special mercury rectal thermometer of the design of Nicolle,manufactured by Poulenc Frères, No. 12, rue Pelie, in Paris. I bought four of these for the Laboratory. I found Monsieur Chevillard a very patient, good worker.

<u>Plantefol</u>. One of the most interesting visits was that with Plantefol, as he showed me about the new Rothschild Institute, but particularly in his laboratory with A. Mayer, to discuss his gas-analysis apparatus. I found this pretty widely distributed over Paris, and finding Plantefol such an interesting, intelligent chap, I was only too glad to talk it over with him in detail. He had recently prepared a paper, published June, 1932, discussing eudiometers of precision in general and discribing his own apparatus in particular. This copy I marked with a good many comments which were subsequently the basis of discussions with him.

Of special interest to me was the old problem I have taken up so frequently with Dr. Carpenter, with regard to whether or no the compensation tube in the gas-analysis apparatus should have the same volume of gas in it as that being measured, or whether it can have a different volume. Dr. Carpenter maintains it can have a different volume and Plantefol agrees with him perfectly and almost convinced me. He commented upon the fact that rubber tubing always contained chloride of sulphur, which in contact with mercury gave a smut and dirt that caused so much trouble. Much discussion was given to the uncompensated dead space in the capillaries above the different tubes. Apparently Plantefol had worked on this problem very carefully indeed.

He uses the small oil manometer or what we call Sondén manometer, and has a drop of caproic acid in the capillary as an index bubble. I asked him why he preferred this, and he stated that caproic acid is very mobile, dissolves fat, and has no tension of vapor, in spite of its smell. In his analyses he waits a long time between each reading, because he considers that the gas cools when there is a partial vacuum and it is necessary to wait about ten minutes until it comes back to normal. On the other hand, he says he gains time by having three apparatus run by one operator, and thus no time is lost, as he can make three analyses in fifty minutes. His more exact form of apparatus costs about 3000 Francs, which at that time represented about $120 each. He uses water acidified very lightly with sulphuric acid. No special stopcock grease was used that I could find out, nor did he change the composition of it with the season. He told me that he had no trouble with fat in greasing stopcocks, although he has the most elaborate 4- or 5-way stopcock I have ever seen on a gas apparatus. I took up with him very much the question of diffusibility of gases, going over the story of the fact that Noyons thought carbon dioxide might diffuse out. Plantefol is very much inclined to think that in spirometers mixtures can not be assumed, and with the question of 5 per cent carbon dioxide and 95 per cent oxygen Plantefol states that I am all wrong and that Drinker and Krogh are correct. In his directions for making analyses one finds so frequently "wait ten minutes". This long time to make an analysis is, however, compensated, as stated above, by his having three apparatus for one observer. One point I thought rather practical is that he puts a wad of cotton batting above the mercury in the levelling bulb to keep out dust.

There is no question but what Plantefol understands the principles of gas analysis probably better than anybody I met in Europe. He has developed an apparatus based upon the early Laulanié tradition, but has bettered it and has got a very good apparatus. Plantefol is careful and concentrates on technical details, but he did not convince me for a moment that he had an apparatus that compared with the Carpenter apparatus. He has two models, one of which is no better than the Haldane but is more complicated. The other model he claims has a high degree of accuracy but has not enough accuracy to analyze outdoor air or alcohol checks with low carbon-dioxide production, such as are possible with the Carpenter apparatus. Madame Plantefol is likewise very much occupied in gas analysis and I should judge "pulls the laboring oar" in many of the studies in which gas analysis is an important factor. She is working in the basement in a sort of cave or sub-cellar, working on a micro-method for gases in vegetable cells.

I kept continually thinking what a fine thing it would be if Plantefol and Carpenter could be together in Boston for, say, two or three months to thrash out gas-analysis problems. I am perfectly certain that Plantefol would get a good deal out of it and modify his ideas. There is a reasonable probability that Dr. Carpenter would likewise find some points worth changing. As it stands, however, I am perfectly convinced that the Plantefol apparatus is not to be compared for one moment in (1) facility of operation, (2) accuracy of results, and (3) usability by others, with the Carpenter apparatus. I had hoped until the last that through some arrangements Plantefol could visit us, but Professor Mayer told me he would probably receive a very good university appointment shortly, and until the decision was made as to who was to have the appointment Plantefol must, in his own interests, "stand by". He deserves a good appointment. I think of all the men I met no one spoke as clearly in discussing matters as Plantefol. It seemed as if every word was weighed, clearly enunciated, and with no delay in arriving at a conclusion. He must make a splendid teacher. If it ever transpires that he can get to America, it will be a splendid thing for us to have him as a guest at the Laboratory for as long a time as he can give to us.

As usual I had a most delightful talk with André Mayer. I find him the most interesting and stimulating man in Paris and I took every opportunity, often too frequently I felt, to get in touch with him. I was most interested in Mayer's tremendous idealism. He says that he can not do much research work but he and his colleagues are hoping to prepare a good "<u>new generation</u>". The laboratory is extremely busy in I don't know how many departments and there are a great many Plantefol apparatus. He uses the Haldane weighing method and the reason is that he gets the water output of the animal under various conditions of humidity, etc. I do not know that I sensed his various problems but I do not think this principle is good for real basals.

<u>Rothschild Foundation</u>. Through Mayer, who is apparently the head of it, Plantefol showed me about. It is a splendid building on land belonging to the Sorbonne.

Plantefol I find is primarily a botanist and has charge of the department of botany. There are departments of biophysics (work on infra-red), a chemical division, and even a geological slant to the work.

The whole building savored much of the new division of the Pasteur Institute, with very fine equipment, wonderfully well lighted, splendid shop, fine workmen, and tremendous equipment throughout. Here I saw the extensive use of large sheets of cellophane to cover up apparatus not in use, in order to keep the dirt out. In this Rothschild Foundation there is no teaching as the library is very small, but they have a weekly colloquium. This was established by the donor, the idea being to bring together the research, clinical, and medical students in such a center.

To my mind André Mayer is the man of all men to engineer a project of this kind. I find myself quite incapable of adequately expressing my reaction to André Mayer. I had learned to regard him very highly indeed as a scientific man but the more I am thrown with him the more I realize he is a humanist, an international man, and I can see why he is called upon so much for different commissions and committees. The loyalty and enthusiasm of his co-workers is an illustration of the esteem in which he must be held by all in contact with him. The social evening at his home is unforgettable, because of the delightful chat I had with him.

PARIS, FRANCE.

University of Paris, Faculty of Sciences, Laboratory of Physiology.

Professor Louis Lapicque.

I called upon this charming gentleman on several occasions and we had a most delightful dinner at his home with a number of friends. His laboratory is practically all chronaxie, set after set. Some slight modifications of this measurement were found, but nothing else is there. If the Nutrition Laboratory is "narrow" and sticks too closely to metabolism ideas, what can one say of Lapicque's laboratory on chronaxie? We found, among others there, a very intelligent psychologist, Professor Jasper and his wife. He was one of Seashore's men.

Mrs. Benedict and I were particularly interested in seeing Lapicque's set-up of the student apparatus. It seemed he had found it unusually useful in teaching his students, only the apparatus was extraordinarily fitted up. The chamber was a large 2-gallon bell jar which rested on a plate through which two holes passed and through one of these holes was a pipe supporting on its end inside the bell jar a glass bottle with soda lime. Tied over the neck of the bottle was a sort of bathing cap with a spiral wire lamp shade ring under it to suspend the bathing cap in a horizontal position. Air went out of the bell jar through another tube. A third pipe, much smaller, entered the bell jar and this connected with a 4-liter bottle containing oxygen and graduated on the sides with marks on a strip of surgeon's plaster. A reservoir of water in a bottle was a little above the level of the oxygen bottle.

They used a Tissot mask with two valves and the experiments were made as with a helmet, standing resting, standing with the dumb-bells, and after work. Oxygen was added from the bottle by allowing water to flow in out of the residual. The bathing cap rose and fell as with the student apparatus and the index button touched a suspended plate. The student is impressed by the fact that he absorbs the oxygen, etc. We saw at least three of these set-ups in the laboratory and Lapicque makes much use of it with his students. It was interesting to us to see this because I have often stated that the only place I have found the Fredericq oxygenograph was here in this laboratory.

I had a long talk with Lapicque with regard to nutrition problems and particularly alcohol. He is very much opposed to having alcohol in public life and feels that the chauffeurs are dangerous with their continual sipping of alcohol throughout the entire day.

PARIS, FRANCE.

Hôpital de la Pitié.

Professor Marcel Labbé.

In one division of this hospital is Professor Marcel Labbé. He is one of the most remarkable men that I know, very quiet, very modest, but a great thinker. He is very much interested in Basedow and para-Basedow. The respiratory quotient only is used for the control of the oxygen and heat, but he had a lot of experiments on 50 gm. of glucose, 50 gm. of casein, and 50 gm. of olive oil and used the respiratory quotient as the index of the material burned. They have a Laulanié gas-analysis apparatus and deduct just for the carbon dioxide in the room air??? It is too crude. Really it is a pity. There were six different determinations ~~on~~ each morning on six different patients. They lie one-half hour on the bed, 10 minutes with mask on to get adjusted, and then 10 minutes for collecting the air and analyzing it. That is the actual final determination.

Labbe's most interesting comments on clinical matters were only in slight manner absorbed by me. It is a pity one more versed in clinical things could not have been there. To my mind he is one of the most fascinating men I have met in Europe. Of course he is directly influenced by the metabolism technique of Stevenin and it is a pity that this hospital could not have six months with two of our men from the Nutrition Laboratory to put them on their feet. They would have an altogether different idea of the potentialities of basal metabolism if they could have that experience. This applies likewise to a great many laboratories in Europe.

PARIS, FRANCE.

Institut Marey (Boulogne-sur-Seine).

Monsieur Lucien Bull.

The journey to Boulogne-sur-Seine is always a long one and I expected to find but little new out there. In this I was not in error but it is always a great pleasure to meet Bull, if only from the standpoint of construction. He is one of the most ingenious men I have ever known. Practically everything he does has a definitely optical slant. He is still very much interested in ultra-rapid photography and had developed a machine for taking 3000 exposures per second. Thus he has been able to photograph a rubber band as it contracts after stretching, and he finds it travels at the rate of 40 meters per second in snapping back when it is stretched ten times its initial length.

We both brought up his old notion, expressed on his visit to Boston, of an optical wattmeter and Bull suggested that I send him a statement of conditions under which I should be using such an instrument and he would work out details. His idea was to photograph a small beam of light falling on a mirror attached to the axis of a voltmeter, this light to be reflected in turn upon a similar mirror attached to the axis of a milli-ammeter. I think the idea is that the "<u>resulting</u>" direction of the beam of light would give a direct reading of the wattage. I expect in the fall to determine the conditions under which we would use such an instrument with the so-called baby calorimeter and also the large calorimeter, both emission calorimeters, and send him.

PARIS, FRANCE.

Station Physiologique (Boulogne-sur-Seine).

Dr. F. Caridroit and Dr. Pierre Gley.

At the Station Physiologique I met both Pierre Gley and Caridroit but only for a moment. Apparently no metabolism was being carried out there. I did take occasion, however, to refresh my memory of the remarkable afternoon that Mrs. Benedict and I spent at the Station a few years ago. It was a dull, drab, dreary afternoon and we were in the small laboratory at the Station gathered about a stove, Mrs. Benedict, E. Gley, Gary N. Calkins, and myself, and I am not sure whether both Caridroit and Pierre Gley were present. It became darker and darker as we sat chatting about the stove with no lamp. The stove door was open and the light streaming out fell upon the striking face of Gley, which was vivacious even under these conditions. It was a picture never to be forgotten, and savored greatly of the chiaroscuro of Rembrandt. There is no question but what the Station has lost a great mind in the loss of Gley.

Pierre Gley called upon me at the hotel and I had a most interesting chat with him, but his problems are quite different from those in which we are interested. I discussed with Pierre Gley the respiration experiment I heard of at the Station Physiologique four years ago, at which time his father had told me of the observations of Pézard showing striking changes in the metabolism of the cock almost immediately after castration, but I felt, and I think that Pierre Gley agreed with me, that the respiration apparatus was poorly constructed and probably the results were of no value. I was interested to hear him say that he thought the castration of the pigeon was not to be compared with the cock, for the pigeon has no secondary sex characteristics being subsequently developed as does the cock, so I felt strongly that Riddle and I should do the cock.

Based upon my visit to the Station Physiologique, the following problems came up in my mind, which I have already communicated to Dr. Riddle.

Pierre Gley felt that the castration of the pigeon was of a wholly different nature than that of the cock, probably less profound changes in body characteristics, since the pigeon would not develop any secondary sex characteristics. It was apparent, therefore, that in the study of castration one would a priori expect to find relatively little change in the metabolism of the pigeon. I felt that Riddle and I should do the cock before and after castration. This brings up very sharply to my mind the fact that Riddle's laboratory is the only place where first-class avian metabolism is being done and there are a certain number of problems Riddle should do although they may not contribute directly to his main endocrine and genetic studies. Among them of course is the castration study above.

<u>De-winging birds</u>. The influence of the extra protection afforded for heat loss by the heavy wing feathers and relatively large area of the wings, and the increased area of the body by including the relatively large wing area (both sides of the wing) make it important if possible to make studies on birds that have been de-winged. I know little if anything about the surgery of birds but I know that they are not at all liable to infection and hence it would not be impracticable to de-wing birds at a very early age, let them develop, and study the metabolism. It may be possible to de-wing adult birds and thus measure the metabolism before de-winging and then afterwards.

<u>Blinding of birds</u>. The reported influence of hooding birds, their quietness and the influence on their metabolism leads one to believe that perhaps very much information could be obtained on the subject of light and its effect on metabolism by blinding birds. This could, I think, be very simply done. Of course they would have to be fed in part by hand and yet a type of food may be developed that would allow them to feed themselves. The influence of blinding on the study of birds should be very significant. When one notes the peculiar head movements of birds in walking one wonders about the connection between eyesight and the semi-circular canals.

I really feel that Riddle should consider at least these three problems very seriously.

## PARIS, FRANCE.

### École Nationale Vétérinaire at Alfort.

### Professor F. Maignon.

We had seen Professor Maignon at Rome and although he was not actively engaged in metabolism studies at this time I felt I should visit his Institute, as I had never been out to Alfort.

As a matter of fact, the visit there showed relatively little of direct importance to the Nutrition Laboratory but led to my getting in contact with a veterinary in Versailles. The laboratory at Alfort is interesting historically as Chauveau worked there. There is a great deal of historical apparatus. None of Maignon's present problems interested me at all. He is enthusiastic and an interesting man to meet and we greatly enjoyed our social contacts with him and his family.

PARIS, FRANCE.

Professor Lafaye at Versailles.

Blood pressure of large domestic animals. While calling upon Professor Maignon he told me of a Professor Lafaye of Versailles who had developed as a part of his thesis an apparatus for determining the blood pressure of horses and cows. Realizing in our work at Durham the high metabolism of these animals and that for its explanation it needs a consideration of every physiological factor, I thought here perhaps we have the first satisfactory measurement of blood pressure without cannula in the artery. I made a special trip to Versailles and had a most interesting afternoon with Dr. Lafaye. The apparatus was a cuff that could be dilated by pressure and the pulsations of an oscillometer read. Dr. Lafaye was good enough to drive me some distance to a large breeding farm and there demonstrated on two horses and showed the practicability of this method. The apparatus was not quite ready for the market but I expect to hear from him before long.

Of considerable interest to me was the fact that the horses reacted differently. For example, one of the horses that he measured was the greatest race horse in France and had won for his owner something like 2,000,000 francs. This horse stood quietly all through the blood pressure measurement as if it were a part of the day's work and entirely indifferent to the whole proceedings. On the other hand, rather a scrub horse danced and pranced around and showed the necessity of at least training the animals into these measurements. Finally the measurements were made without the slightest difficulty on a cow that had just come in from the fields, had never seen the operator or had the cuff adjusted on the leg.

I was impressed more than ever before with the importance of the hydrostatic level in these blood pressure determinations. Thus, when the horse is standing and the point at which the measurement is made is on the leg right at the elbow, so to speak, there is considerable difference between the level and the heart at this particular point. Similarly they find large differences in blood pressure whether the animal is standing or lying. The same is true with regard to cows and hence measurements reported must include a record of the posture of the animal. The whole procedure seemed eminently satisfactory for Durham work and I hope to have it installed there in the near future.

PARIS, FRANCE.

Lectures.

In Paris I gave four lectures. The first of these was given on January 30th, before the Faculty of Medicine, and was entitled, "Etudes récentes sur le métabolisme basal de l'homme." There was an attendance of 800 at this lecture.

On February 6th I gave a lecture for the Société de Biotypologie, at the Conservatoire National des Arts-et-Métiers, before an audience of 115. This lecture was entitled, "Le métabolisme de base chez les races différentes."

February 7th I spoke before the Société de Chimie Biologique, at the Collège de France, to an audience of 155, giving the "second" lecture.

The "first" lecture was given on February 18th before the Faculty of Sciences at the Sorbonne, to an audience of 175. (There were announcements of all these lectures.)

Newspaper clippings from the New York Herald and the Chicago Daily Tribune, with regard to my lectures in Paris, are appended herewith. (See pages 345 and 346.)

Lecture - Paris - Faculty of medicine

# UNIVERSITÉ DE PARIS — FACULTÉ DE MÉDECINE

Année scolaire 1932-1933

M. le Docteur Francis **BENEDICT**, Directeur du Laboratoire de la Nutrition de l'Institut Carnegie de Washington, à Boston, fera, le *Lundi 30 Janvier 1933, à 16 heures,* au Petit Amphithéâtre de la Faculté, une conférence sur le sujet suivant :

## ÉTUDES RÉCENTES SUR

# LE METABOLISME BASAL DE L'HOMME

Cette conférence sera accompagnée de projections.

*Le Doyen de la Faculté :* **BALTHAZARD.**

47

A 33. — Paris, Imp. administrative Centrale, 8, rue de Furstenberg (6e)

SOCIÉTÉ DE BIOTYPOLOGIE

LE SECRÉTAIRE GÉNÉRAL

PARIS, le -3 FEV 1933

Monsieur et Cher Collègue,

Comme suite à la convocation que je vous ai adressée pour la réunion de travail de la Société de Biotypologie qui aura lieu le lundi 6 février (1) ,je vous informe que M. le Professeur Bénédict, Directeur de l'Institut de la Nutrition de l'Institution Carnegie (Boston U.S.A.) de passage à Paris, a bien voulu accepter de faire à notre réunion une communication sur

Le métabolisme de base chez les races différentes
(avec projections).

Je vous prie de croire, Monsieur et cher Collègue, à l'expression de mes sentiments les meilleurs.

Le Secrétaire général,
Henri LAUGIER.

(1) à 17 h.30, au Conservatoire National des Arts-et-Métiers , 292, rue Saint-Martin ; salle D.

# SOCIÉTÉ DE CHIMIE BIOLOGIQUE

INVITATION

## CONFÉRENCE

M. le Professeur F. G. BENEDICT, Directeur du Laboratoire de la Nutrition de l'Institut Carnegie, Boston, fera, à la séance du *Mardi 7 Février*, à 20 h. 30, au Collège de France (amphithéâtre n° 8), une Conférence avec projections sur le sujet suivant :

**Physiologie comparée du métabolisme.**

**SOCIÉTÉ**
**DE**
**CHIMIE BIOLOGIQUE**

*Paris, le 2 Février 1933*

*La prochaine Réunion de la* Société de Chimie Biologique *aura lieu le* **MARDI 7 FÉVRIER,** *à 20 heures et demie, au Collège de France, 7, Place Marcellin Berthelot. (Amphithéâtre nº 8).*

## ORDRE DU JOUR :

Allocution présidentielle.

**R. WOLFF** et **M. TRAIN.** - Recherches expérimentales sur le microdosage du magnésium dans les tissus animaux.

**R. WOLFF.** - Microdosage du calcium dans les tissus animaux.

### Conférence :

**F.-G. BENEDICT.** - Physiologie comparée du métabolisme.

NOTA. -- *Les Membres de la Société sont instamment invités à faire connaître, au Secrétaire général,* M. R. FABRE, 149, Rue de Sèvres, Paris (15e), *l'énoncé des communications qu'ils se proposent de faire à la Société. Pour arriver en temps utile, leur avis doit être parvenu au moins* **huit jours avant la séance.** *Cette mesure est indispensable pour la rédaction de l'ordre du jour ; il demeure entendu qu'elle n'atteint en rien le droit qu'ont les Membres de la Société de faire des communications non annoncées.*

*Les Membres de la Société qui communiquent ou prennent la parole en séance sont instamment priés de remettre* **LE SOIR MÊME,** *au Secrétaire général, une note pour servir à la rédaction du procès-verbal.*

**Munier, Paris - 2709**

Lecture - Paris - Faculty
of Sciences.

RÉPUBLIQUE FRANÇAISE

UNIVERSITÉ DE PARIS

# FACULTÉ DES SCIENCES

## CHAIRE DE PHYSIOLOGIE GÉNÉRALE

*Année scolaire 1932-1933*

M. le Professeur **F. G. BÉNÉDICT**, de Boston, Directeur du Laboratoire de la Nutrition de l'Institut Carnegie de Washington, traitera, en français, le sujet suivant :

**La Physiologie des grandes Tortues et des Serpents et ses relations avec la Physiologie Humaine**

Cette conférence, accompagnée de projections, aura lieu à l'Amphithéâtre de Chimie (Sorbonne), le *Samedi 18 Février, à 15 h. 30.*

**Vu et approuvé :**

*Le Président du Conseil de l'Université de Paris,*

**S. CHARLÉTY.**

*Le Doyen de la Faculté des Sciences,*

**CH. MAURAIN.**

H. B. — Paris, Imp. administrative Centrale, 8, rue de Furstenberg (6e)

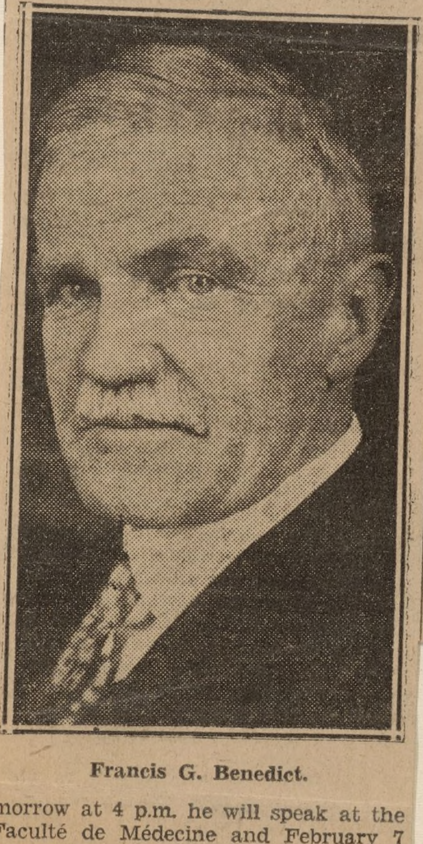
THE NEW YORK HERALD, PARIS, SUNDAY, JANUARY 29, 1933

# Carnegie Snake Authority Talks Tomorrow in Paris

Paris scientific authorities this week will hear results of 16 years of experiments by an American inquirer in a branch of medical science never studied by any other man—the pathological relations between snakes and humans.

This unique scientist is Francis G. Benedict, of the Carnegie Institute, who has concluded his researches with contributions regarded by medical men as being of fundamental value.

He has been lecturing on the subject in the chief European universities. To-



Francis G. Benedict.

morrow at 4 p.m. he will speak at the Faculté de Médecine and February 7 at 8.30 p.m. at the Collège de France.

A story of one of the most curious laboratories in existence was told by Professor Benedict when seen last evening. That is the Carnegie Institute's research establishment in Vila street, Boston, where the scientist and his assistants have conducted dangerous experiments on boa constrictors, pythons, rattlesnakes and other serpents for the advancement of medicine.

While remarking that "only exhaustive scientific dissertation" could explain results of the highly complex experiments, Professor Benedict stated that his long study disclosed "important indications for metabolism research," aiding medical science in its understanding of human pathology.

One of the most significant results of his researches was comparative experiments in which the temperatures of ordinarily cold-blooded snakes were raised to human fever degrees. With the aid of Dr. Raymond Ditmars, New York Zoo curator, rare specimens of serpents approximating human beings in weight were obtained for study. Frequently, Dr. Benedict and his assistants were in danger during their work. Speaking a layman's language, Professor Benedict pointed out:—

"There's no truth in the Bible's assertion as to the wisdom of the serpent. The snake is probably the dumbest of God's creatures. But study of its curious and obscure relationship to the internal changes of the human body has given us fundamental aid in our knowledge of man's physical being."

Snakes are good to eat—for some people, the scientist stated. "The Australian bushman, who has a hard time finding sustenance, eats snake-meat with relish. It agrees with him and provides him with almost the only protein in his diet."

PARIS, TUESDAY, JANUARY 31, 1933. CHICAGO DAILY TRIBUNE.

# DR. F.G. BENEDICT REVEALS SECRETS OF METABOLISM

## Scientist Of Carnegie Institute Heard By Student Group

Dr. Francis G. Benedict of the Carnegie Institute gave a lantern slide lecture dealing with his metabolism experiments over the past 16 years before a large group of students and scientific authorities at the Faculté de Médecine yesterday afternoon.

In great detail, the doctor told of the experiments held by the nutrition clinic with the aid of the most delicate instruments in registering the metabolistic changes over periods lasting a lifetime with all races, ages and sexes. Comparative charts were shown at the lecture giving an idea of the conclusions arrived at.

Dr. Benedict demonstrated that metabolism varies according to age, race, weight, sex and height, and even until this day medical authorities, he said, are at a loss to give any logical reasons for these differences.

In his lecture Dr. Benedict pointed out that metabolism is more active in men than women, and that the process 's retarded with aging. Dr. Benedict also stated metabolism was much more active among American men and women than among other races.

He will give two other lectures, one at the College de France on February 7 at 8:30 o'clock and a second at the Sorbonne on February 18 at 3:30 o'clock.

American medical students in Europe.

My attention was frequently called, while en tour, to the presence of a large number of American medical students in Europe, particularly in Germany and Switzerland. It seems that one of the steamship companies organized a special propaganda in America for students to study in Germany, pointing out that they could obtain their medical training much cheaper there than at home. The result was that a large number, I am told 1500, went over to Germany in the fall of 1932, most of them Jews. Most of them had been unable to enter class A medical schools in the United States, or were men who had entered only after the quota for each school had been filled. In Hamburg and Berlin I had heard of the influx of American students and rather disheartening stories with regard to their calibre, but in Leipzig I found the laboratories overcrowded with American students, practically all Jews. In Basel I saw another large group, and indeed I was asked to speak in English particularly to these groups before giving my lecture.

This experience led to a number of interesting points, one being that they seemed to infer that for the most part these American students were of poor quality. This was particularly true in German universities, but less so in Switzerland. In fact, there was no criticism of them at Basel. Secondly, it was pointed out that in Germany the fees paid by these students constituted only a small part of the amount spent by the university, and hence the government, for their education. Hence there was a tremendous draft upon German resources. I was told that it cost the German government 1200 Marks per year to educate these men and the students pay only 250 Marks. It was pointed out that the German universities had no limiting clause; that is, they must take all who apply. The students were allowed to train and could not be "kicked out" until they had been there for nearly a year and a half, at which time they were subjected to an approval or estimate, and if they were falling off they were not allowed to remain. Efforts are now being made to change this law. Another point is that so many students are out of work, and they would rather loaf in the laboratory than loaf on the street.

This whole matter was subsequently taken up by me and sent to the New England Journal of Medicine in a letter, a copy of which is appended herewith. (See page 348.)

## CORRESPONDENCE

### OBSERVATIONS ON MEDICAL EDUCATION IN EUROPE

Carnegie Institution of Washington
Nutrition Laboratory
29 Vila Street, Boston, Massachusetts

April 6, 1933.

*Managing Editor,*

The *New England Journal of Medicine,*

I have been extremely interested in reading the editorial in the March 30 number of The *New England Journal of Medicine,* on "Foreign Dumping on Massachusetts."

Having just spent six months in Europe, lecturing in practically all of the large European universities, I had occasion to see a great many American students. The situation is in many ways extraordinary. The countries where most American students have gathered have been Germany and to a certain extent Austria. I was told that one of the large transportation companies had made a campaign and had succeeded in bringing over I think about fifteen hundred American students. German professors reported that almost invariably they were of a quality below what they had been accustomed to see in American medical students. They told me also that many of these men were men who had failed to secure admission to the regular American schools. It was not made clear whether they had failed to pass the admission examinations and hence were not intellectually prepared for this work or whether they had not been able to be in the selected group that had been chosen for admission to any of our schools. Unfortunately they reported to me that they were on the whole not a particularly satisfactory group.

I tried to see as many as I could of them, and very many of them attended my lectures and I was able to speak to a good many afterwards. In some cases the professor asked me to address the American students in my audience in English before giving my lecture in either German or French. The general tenor of my remarks was to the effect that I was very much distressed to hear that the men had not, for the most part, come up to what had been expected of American students; that just at that time there was a controversy going on in New York with regard to the admission to practice of graduates of foreign universities, and I emphasized to these students that they must so comport themselves, both in the clinics and out, as to show what serious American students could be, and secondly, when they came back to America they must return so well trained that there could be no discussion as to the qualifications of graduates of certain European schools for practicing in America. My personal contacts with them were most pleasant; in fact, the groups that I met rather belied the impression that they were below par. I frankly discussed this with them and not a few admitted that they were over there purely as a matter of expediency.

There is one phase of your editorial that I think is perhaps misleading, that is, the economic side of it. As a matter of fact, each American student in a German university costs the Government close on to 800 marks per semester over and above what the student pays, which I believe is only about 200 marks. A number of professors bemoaned the fact that they had no means of disposing of these men until they had been at the university for nearly a year and a half, when their first examination took place. Only when they failed in that examination could they be sent out from the university. The result is that the situation is extremely complex and economically not to the advantage of German universities.

In striking contrast to the German situation is the American group at present studying in the Swiss universities. There they have made a very fine impression and are, I believe, very welcome.

In France the situation is much the same as in Germany, only the influx of students is not so much from America as from the eastern states, particularly Yugoslavia, Czechoslovakia, and Poland, but the French professors, while they all recognize that these students are a heavy drain financially upon the French Government, yet feel their definite obligation to medical education in making it possible for these men to partake of the advantages in the French schools.

With best regards, I am

Very truly yours,

FRANCIS G. BENEDICT, *Director.*

P. S.: You might possibly be interested in the type of work I was doing in Europe and the series of lectures I gave, and I am enclosing the printed outline of the three lectures.

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EDITORIAL NOTE: The work referred to by our esteemed correspondent is under the general title of "The Physiology of the Great Tortoises and Snakes and Its Relation to Human Physiology." His subheads indicate a broad conception of the problems involved.

tion. Something may be gained from the dissemination of knowledge of contraceptive measures, but unfortunately those who should not have children are least likely to employ voluntary contraception.

We should at least recognize this danger that threatens to replace our population with a race of feeble-minded; we must study its causes and the sources from which it springs. If we wait too long, this viper that we have nourished may prove our undoing.

## MEDICAL EDUCATION ABROAD AND AT HOME

The letter of Dr. Francis G. Benedict, appearing on page 857 of this issue, will relieve some of the apprehension expressed in the editorial of this *Journal* of March 30 last, but there are definite indications of a modicum of truth in the expectation of having physicians who are educated abroad apply for registration in Massachusetts in considerable numbers.

Although Dr. Benedict was impressed by the appearance and spirit of many of these students, there may be a lower stratum even among these groups, as is sometimes found in most medical schools. It is well known that hundreds of young persons desirous of practicing medicine are unable to secure admission to the medical schools of the United States and are studying abroad. It may happen that some, and it is a reasonable fear that there will be a considerable number, will not be able to qualify for examination in most of the states of this country and yet can meet the low requirements of the present Massachusetts law.

If rejected in other states, it would be reasonable to expect that such disappointed doctors will regard this Commonwealth as a possible resource. Under such circumstances Massachusetts will be the choice, for most graduates wish, and some will need, to get into practice as soon as possible.

Massachusetts will not be in a position to exercise discriminatory power unless her legislators have a change of heart. Her policy is to open her doors to many who cannot secure recognition in any other state.

Well-educated physicians will always be welcome here. There are more than enough of the indifferent class.

Even those members of the Legislature who naturally would vote for better statutory requirements respecting medical education are pessimistic respecting prospects for endorsement of the recommendations of the Board of Registration in Medicine.

There is no doubt but that the future will see changes for the better but this is not likely until the general public shall have come to realize the great importance of a body of practitioners comparatively well equipped to deal with the health of the people.

Our leaders are striving for this awakening but it will be deferred unless the nearly five thousand better equipped doctors unite in an educational campaign.

Active centers for metabolic research.

Making such a wide survey of institutions, it naturally follows that certain of these institutions challenge attention as especially active centers for metabolic research. In the first place one was rather disappointed by the fact that so few outstanding institutions or laboratories were actively engaged in metabolic research. A number of the places visited were hardly worth while from the standpoint solely of what one could get for the Laboratory. Every place I visited I felt that I had left considerable for science as a whole. Of these active centers those which at present are most worthy of visiting from the standpoint of current Nutrition Laboratory interests are Stockholm, Lund, Copenhagen, Aberdeen, Cambridge, Utrecht, Dortmund, Leipzig, Würzburg, Budapest, Belgrade, Zürich, Berne, Basel, Strasbourg, and Paris.

Now in the order of importance for the Nutrition Laboratory's activities I have attempted to arrange a few of these but it is very difficult, so this must be considered only a tentative, rough approximation. In the last analysis we must also realize that the importance of these centers is determined not by magnificent equipment and by tradition, but by the active personality and capacity of individual men working there, such as Dortmund. There, in a magnificently equipped laboratory, is a splendid man, Atzler. On the other hand, at Belgrade we have no equipment but a splendid man, Giaja. Bearing this in mind, to a person visiting Europe for metabolism purposes I should suggest approximately the following order.

Copenhagen, with the laboratories of Krogh, Lindhard, Møllgaard, Sörensen, Henriques, and Ege.

Paris, with the laboratories of André Mayer and his assistants, particularly Plantefol, the Pasteur Institute with Bertrand and Le Compt-du-Nouey, the hospital of Marcel Labbé, and for children the clinic of Nobécourt and his assistants.

Utrecht, with the magnificent institute of Noyons presided over by one of the greatest geniuses in Europe, Noyons himself.

Dortmund, with ~~an~~ the almost stupendous institute with equipment and staff of very fine men and directed by one of the strongest men in Europe, Atzler.

Strasbourg, with its much more modest equipment but the splendid school of Schaeffer and his associates.

Basel, with the personality and equipment of Verzár.

Aberdeen, with first and foremost the personality of Macleod, and secondly the Rowett Institute with its corps of men, Magee and others.

Finally I must include Belgrade with no equipment whatsoever, but on the other hand a very extraordinary group of men like Giaja and his associate, Gelineo, and the pathologist, Chahovitch, and Burian with the biochemical slant.

The interest in metabolic research varies enormously in the different countries. For the past two decades I have never failed to be surprised at the complete indifference to metabolism measurements exhibited in Great Britain so far as the clinic is concerned. In physiology Hill has done a great deal of work on respiratory exchange, much of it subsequently debated, but it has stimulated a great deal of research. Clinical basal metabolism is dead. There is no interest in it. The letter files of the Nutrition Laboratory will show frantic efforts on our part to secure the measurement of the basal metabolism of people returning from India to Great Britain in connection with a study of acclimatization. In spite of the past studies of Haldane and Pembrey and the remarkably ingenious methods of Douglas, there is still practically no use of these techniques in Great Britain.

In Germany on the other hand there is a tremendous interest in metabolism, although by no means approaching the hysterical wave passing over America with its nearly ten thousand different basal metabolism apparatus. Still I think it is fair to state that in German centers basal metabolism is seriously considered. In England they would be satisfied with blood pressure and heart count. Even in Switzerland, the home of the goiter, I did not find as great an interest in basal metabolism as I would expect. Certain clinics, notably that of de Quervain, had of course their metabolism sections to follow their operations.

<u>Basal metabolism as a hospital routine examination for all entering patients</u>. On my tour one of my missions was to awaken interest in basal metabolism not only in studying endocrine disturbances but more particularly to start the ball rolling toward the idea of introducing <u>basal metabolism measurements as a routine measurement in the admission of every patient to a hospital</u>. The technique for basal metabolism has been simplified to what I believe is the last step. The conditions important for basal measurements are for the most part easily obtained, that is, no food for twelve hours and the subject lying quietly for at least one half hour. Mental repose is not so easy to secure, and the influence of psychic disturbance and mental repose is very definite even with well-trained subjects. All technical details that contribute toward a psychic equanimity are moves in the right direction.

Thus the Nutrition Laboratory's introduction of the helmet with its free, untrammelled breathing, full vision, and light closure around the neck is a welcome replacement for the saliva-stimulating mouthpiece or the tightly bandaged face mask. This apparatus I described in one of my lectures. The helmet likewise does away with all work of the lungs in opening and closing valves, but what is more important from the standpoint of the operator, the latest modification of the apparatus has become a direct-reading instrument. By introducing a rotamesser in the oxygen supply line, one can read directly the oxygen consumption for each minute. I found that this apparatus attracted a good deal of attention but there was no great enthusiasm over the idea of introducing it as routine hospital procedure. If this is accomplished in ten years I shall feel satisfied. Personally I feel it must ultimately prove its true value.

It is believed that another important procedure can be introduced, in that the subject should not be lying. It is a psychological fact that a patient sitting up will seemingly react far less to mental disturbance than when lying completely flat. The metabolism of well-trained subjects, it is true, is slightly higher when sitting up than when lying down. On the other hand, if the subject is well supported by pillows, such as for example in an English "club" chair or a well arranged steamer chair or reclining chair, this difference is wholly insignificant. Consequently it has been my tendency to recommend that metabolism measurements in the clinic be made if possible when sitting or in a semi-reclining position with the helmet and not with the mouthpiece.

Above everything else it is imperative that each person attempting to interpret basal metabolism measurements must from previous experience, preferably on himself and a number of laboratory (not hospital) normals, know what in the first place is normal and secondly, what are the variations that can be expected with the normal individual. If these two important points can be bettered, first, better technique employed, i.e., the helmet and semi-reclining position, and second, that the interpreter have personal experience on the apparatus, it is fully believed that the basal metabolism will be one of the most important adjuncts in the clinic. It is the only measurement of the level of vital activity.

## The metabolism of animals.

In the survey of metabolism studies in Europe I found various laboratories with more or less activity with different groups of animals, and it may be of interest to classify them in the following way.

Cold-blooded animals. Practically nothing is being done in the gaseous metabolism of cold-blooded animals. One finds everywhere frogs, but only in the laboratory of Terroine are metabolism studies active and these in the line of nitrogen metabolism.

<u>Small animals</u>. Mice and white rats are extensively used in studies of vitamins and the endocrines, and in special studies, as for example the unusual researches of Giaja on the reaction of animals to temperatures of the environment, especially extreme cold. Rabbits are much used at the laboratory of Schaeffer of Strasbourg and Wiegner of Zürich is doing <u>all</u> his work on the nutritive value of certain foodstuffs on rabbits. Pigeons are being employed by Kayser in Strasbourg.

<u>Dogs</u>. I was astounded at the relatively small number of dogs I saw being used. Not infrequently one found a single dog being used in metabolism work, but nowhere was there an extensive study. Hári in Budapest, Szarkall in Dortmund, and likewise Karl Thomas, were studying dogs, but very few others.

<u>Large animals</u>. Pigs were being studied by Møllgaard, at present chiefly engaged in the mineral metabolism, but he has a respiration chamber for them. Deighton in Cambridge has studied the direct calorimetry of the pig. Cows and steers have been studied extensively in Copenhagen and likewise in Leipzig by Fingerling, who has a very complete equipment. I do not recall seeing anywhere sheep confined to a respiration chamber. There were a number in metabolism stalls in Fingerling's laboratory. I saw no place in Europe where the gaseous metabolism of the horse had been studied, certainly not in recent times.

<u>Metabolism of humans</u>.

<u>Rest experiments and normal basal metabolism</u>. I recall no place where such experiments for normal data were in progress.

<u>Work experiments</u>. These were being actively carried out at Dortmund and particularly at Copenhagen by Krogh, Lindhard, and Christensen.

<u>Pathology</u>. Usually I found the technique was very poor and too much emphasis was laid upon one or two experiments. Perhaps the best illustrations, certainly of different hospital cases, were those in the clinic of Labbé in Paris, although I was anything but satisfied with his technique.

Why is there so little attention given to normal basal metabolism in Europe?

I found myself asking this question frequently. Why is it? Then I ask, is it because the apparatus is badly constructed or is it because the results are poorly interpreted? As a result of the summation of the many laboratories I have come to the conclusion that the Douglas method, while ideal in many cases, is too complicated for successful introduction in the clinic.

The Knipping apparatus is likewise altogether too complicated, although it has the value of writing graphically the total carbon dioxide as well as the oxygen. The Krogh apparatus ought to be exactly as good as the Nutrition Laboratory apparatus. There are certain objections to the larger area of its spirometer and particularly its wedge-shaped form and the curved nature of the writing on the kymograph, calling for a special curved measuring rule for this, but there is no reason why the Krogh apparatus should not be as good as our own form.

There is one very definite outstanding point, that is, that the interpretation of the measurements is always loose. In the first place the technicians are too careless about the basal conditions of the subject. It may be that the measurements are made 12 hours after the last meal and the subjects may be lying down, but whether there is both muscular and psychic repose is very doubtful. With the mouthpiece or face mask, etc., there is not a little apprehension if not real discomfort, but on top of all this is the fact that the results when obtained are very poorly interpreted. In the first place they do not know from previous experience what normal values are and what is the variability of the normal. They are taking so-called "standards" as if they were as rigid as steel bands. One is always reminded of the small mark on the Fahrenheit thermometer at 98.6°, considered as normal temperature. This small mark has become a real fetish.

Special observations of immediate importance to the Nutrition Laboratory's activities.

As outlined in the early part of this report, I was disappointed in the relatively small amount of new, valuable material that could be brought back, so to speak, or of immediate practical use. The great economic depression in Europe has affected profoundly the activities of the various laboratories and there was an astounding dearth of work in most of the countries. Consequently the specific points of special value to the Nutrition Laboratory are relatively few.

Making intimate personal visits to the laboratories with photographic appliances, notebook, and sketch book, one is always on the lookout for betterments of technique that can be applied in Nutrition Laboratory procedures. Certain techniques were especially noted, among them the following.

Hydrogen for check tests of respiration chambers. It has been the custom of the Nutrition Laboratory to rely upon the combustion of alcohol in the respiration chambers and respiration calorimeters. It has furthermore been the custom to burn that amount of alcohol that will result in the production of carbon dioxide and absorption of oxygen approximating that of the particular animal to be studied in the chamber; that is, with the mouse there is a very small combustion of alcohol, so small that it is difficult to maintain a flame. With larger animals it is simpler and with man the technique has been thoroughly worked out. In the case of large ruminants and horses the amount of alcohol to be burned quantitatively to represent their total metabolism and the complete combustion of such amounts of alcohol is a great difficulty and we do not attempt at Durham to check the chamber by burning alcohol. In the large respiration chamber (the group chamber) in Boston we have constructed and used a multiple Argand burner for alcohol but it was not particularly successful. At Durham we confine ourselves to so-called gas checks, introducing known weights of carbon dioxide from a bottle of liquefied gas. Ideally, burned alcohol is the best substance, for the combustion per gram of alcohol equals the production of a definite amount of carbon dioxide and the absorption of a definite amount of oxygen.

Møllgaard at Copenhagen in his large chamber for cows has always used burning hydrogen. This is admitted in a very simple way from a cylinder of compressed gas, passing the gas through a calibrated wet gas meter, and burning the gas at a simple jet. By this process one obtains the oxygen deficit but no carbon dioxide. Møllgaard emphasizes the value of this strongly and it is quite likely that we should take this up at Durham.

Blood sugar in cows and ruminants. The course of the blood sugar with ruminants, indicating possibly the absorption of carbohydrate in the body, is of special interest to us, for we have noted at Durham a relatively high metabolism per square meter of surface area of cows and steers. One of the explanations offered for this is the old theory of Grouven that carbohydrates are absorbed into the intestinal tract not as carbohydrate but they are first reduced by fermentation in the paunch to the lower fatty acids. Thus the organism is flooded, so to speak, with fatty acids that probably stimulate cell activity or metabolism. Further study of this problem is imperative. After giving a very large supply of carbohydrate what is the course of the blood sugar curve? What is the course of the fatty acid curve? One of the problems is to decide what is the blood sugar as compared to the total reducing substances in the blood.

I found at Lund a potentially important method, developed by Dr. Lehmann, using small amounts of blood and capable of being carried out with great rapidity, which gave a lower sugar content than the Hagedorn-Jensen method, and it was assumed that the difference between these two represents the non-sugar reducing substances. I hoped this method could be applied to Durham but it has not been sufficiently perfected to be put out. At Aberdeen, Macleod and his associate, Magee, have been actively interested in the blood sugar of cows, sheep, and fowl, using the Hagedorn-Jensen method and making a study precisely of the problem outlined above, that is, the course of the blood sugar if carbohydrate is ingested. They are still actively engaged in this and I feel that Durham must keep closely in touch with them, as their ultimate findings should be of greatest value in interpreting the total metabolism results.

A quantitative study of the heart impulse. At the Congress at Rome Atzler of Dortmund showed a dielectric method for studying the capacity of the heart to show the minute volume of blood. Shortly after this I was in Stockholm and saw a special design of a chair used by Dr. Abramson, registering photographically the impulse beats of the heart, much like the impulse bed used in former years by Professor Yandell Henderson and I believe first reported about the time of the Pike's Peak Expedition. Abramson's curves "seemed" very closely related to those found by Atzler.

At the Nutrition Laboratory we have often noted when standing on the upright Chatillon balance the impulse of the heart. It occurred to me that we could extend the axis of the pointer and attach a small mirror and photograph these impulses, considerably magnified. When the subject is either standing or sitting on a chair one can obtain an extremely simple method for getting these heart impulses that might prove of clinical value. The study can be made very inexpensively and is worth while doing.

Cooling ingoing air to the helmet. During the summer season it is necessary to provide some cooling arrangement for the air entering the helmet, although when dry air is returned to the helmet the cooling power of the air in vaporizing water from the forehead and face is usually sufficient to keep the subject comfortable, especially in rest experiments. During work experiments artificial cooling is necessary. At the laboratory of Krogh in Copenhagen I saw an extremely simple device for this purpose. Krogh had enclosed in a wooden case a Ford automobile radiator which was connected with the water supply of the laboratory, which at that time (in the fall of 1932) was down to 10° C. This method, it seems to me, is perfectly plausible for us. One can very simply put a small ice reservoir on top and have even greater intensity of cooling.

The electric psychrometer. One of our largest problems at the present time is the relationship between the water of vaporization and the total heat production of various animals, including humans. Where the entire ventilating air circuit is passed through absorbing bottles it is possible to weigh the water vapor given off by a man or animal. With the chamber at Durham it has been necessary to rely upon wet and dry bulb psychrometers for this measurement. While at the University of Utrecht, in the laboratory of Professor Noyons, I saw an extremely clever electrical psychrometer made of thermo-junctions connected with a potentiometer and galvanometer. Technically it appeared perfect, as do all of Noyons' apparatus, so four electrical psychrometers were ordered along with a potentiometer, the idea being to introduce this electrical method at Durham if possible and take rapid, frequent readings of the difference between the wet and dry bulb. During the spring of 1933 a number of experiments were made in Boston under my direction by Mr. Coropatchinsky, in which, however, the comparison between the electrical psychrometer and the absolute weighing method of absorbing the air in sulphuric acid was very unsatisfactory.

Sampling alveolar air. A device has been developed by Noyons for sampling alveolar air and I think this can be of considerable use to us in connection with our proposed study of the relationship between the temperature of expired air and the body temperature. Noyons has a small copper can, something like our so-called "pipe of peace", covered with a bathing cap and a pipe leading to the mouth. The bathing cap when deflated lies flat; when it is fully distended it releases a trigger which in turn dislocates a weight directly connected to the handle of an inverted glass syringe, the so-called Luer syringe, of about 200 c.c. capacity. When this weight is released the piston falls immediately and draws in the sample of air. The piston is not released until the subject has given the maximum expiration.

At the Nutrition Laboratory we have made preliminary studies using the spirometer and graphic tracings of the expired air in connection with a curve for temperature of expired air to establish the relation between the volume of air and its temperature. I feel sure a simple device like Noyons' will serve to show to clinicians the point at which the temperature of the expired air should be read and indeed "cut in" the electric circuit and galvanometer at just the proper moment to meet the maximum temperature.

The rotamesser. This instrument, first seen by me a number of years ago at Noyons' laboratory and rarely found outside of that laboratory and practically never in America, is to my mind one of the most ingenious and important instruments that can be used in a laboratory devoted to respiration. I made a special trip to Aachen from Brussels to visit the factory where this instrument is made. Two forms are available. One is a non-calibrated form, the so-called "rota-regulator", and the other an instrument in which the relationships between the float, the calibrated, graduated tube, etc., are so exactly determined that one can use the instrument to know precisely the volume of air per minute passing through it at different levels of the little float.

Already since my return a number of experiments have been proposed and actually made in which the use of this instrument will simplify enormously certain laboratory procedures. For most of these procedures a rota-regulator, which is relatively inexpensive, can be used. For others the rotamesser, preferably calibrated by the man who is to use it, must be used. In any event no laboratory can afford to neglect the careful use of this most extraordinary instrument.

Blood pressure of large domestic animals. One of the factors in the interpretation of the relatively high metabolism noted with large domestic animals is naturally a consideration of the heart rate and especially the blood pressure. The heart rate we have frequently recorded. The blood pressure has been neglected, for it usually calls for the introduction of a cannula with its attendant dangers. At Versailles I found a Dr. Lafaye who had developed, when carrying out his thesis under Professor Maignon at Alfort, a blood pressure cuff that can be attached to the foreleg of either horses or cows and connected with an oscillometer. With this one could read the blood pressure rapidly and correctly. This apparatus was not yet perfected although Lafaye was expecting the manufacturer to give him the final form shortly. It is needless to say that in studies at Durham such measurements would greatly help and I am still hoping to secure such an instrument.